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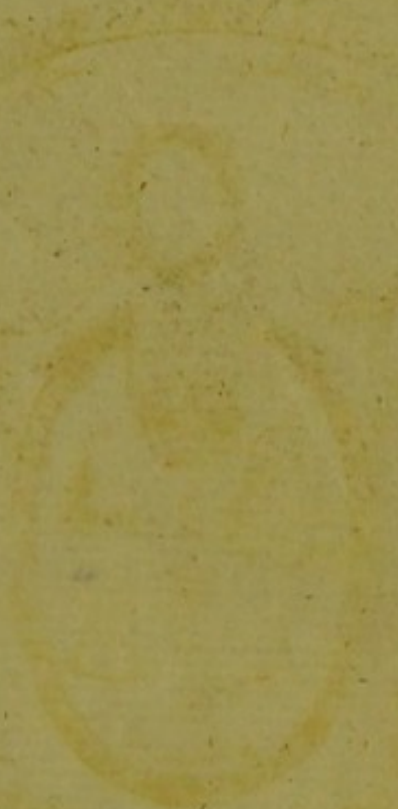
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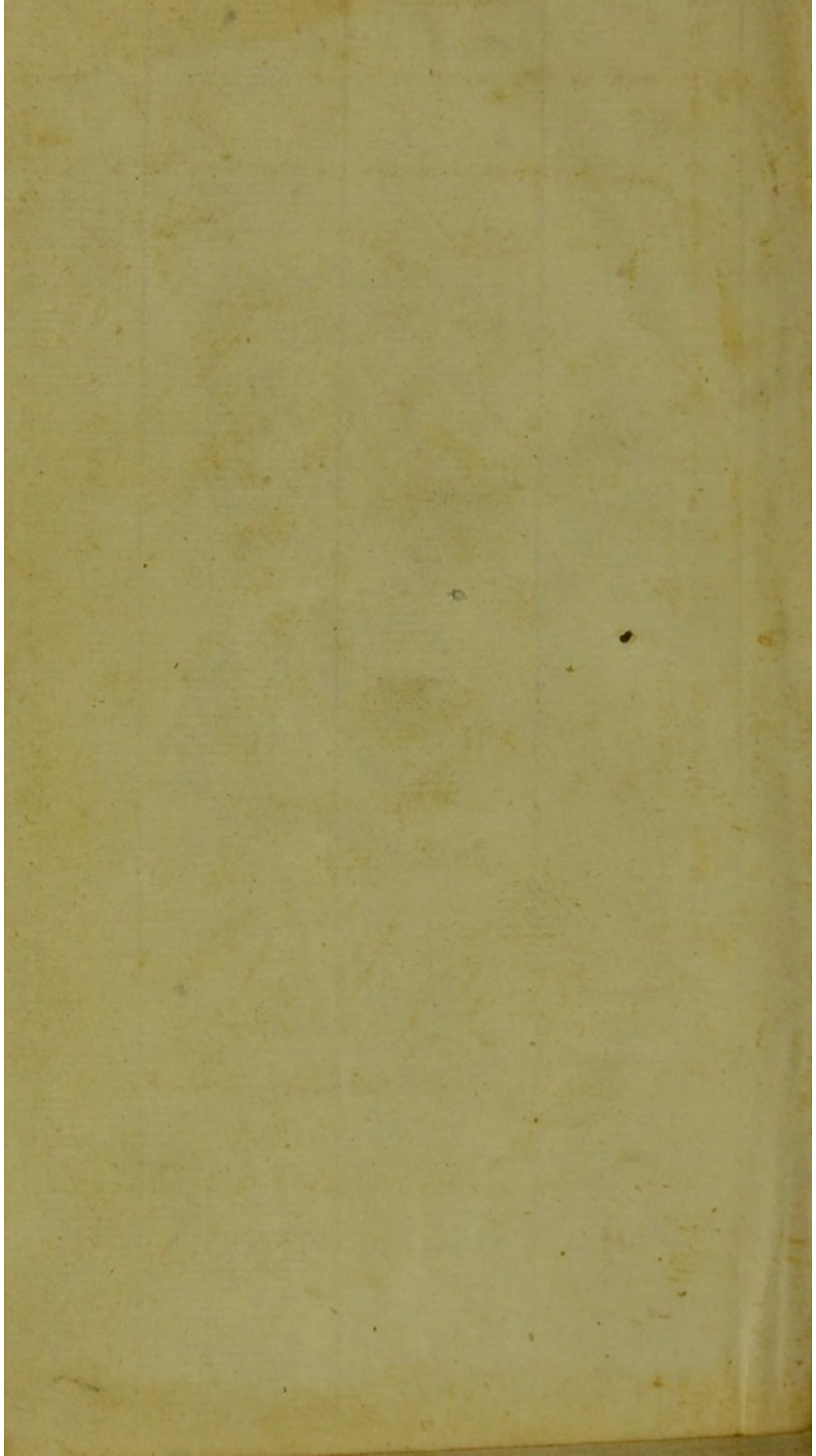
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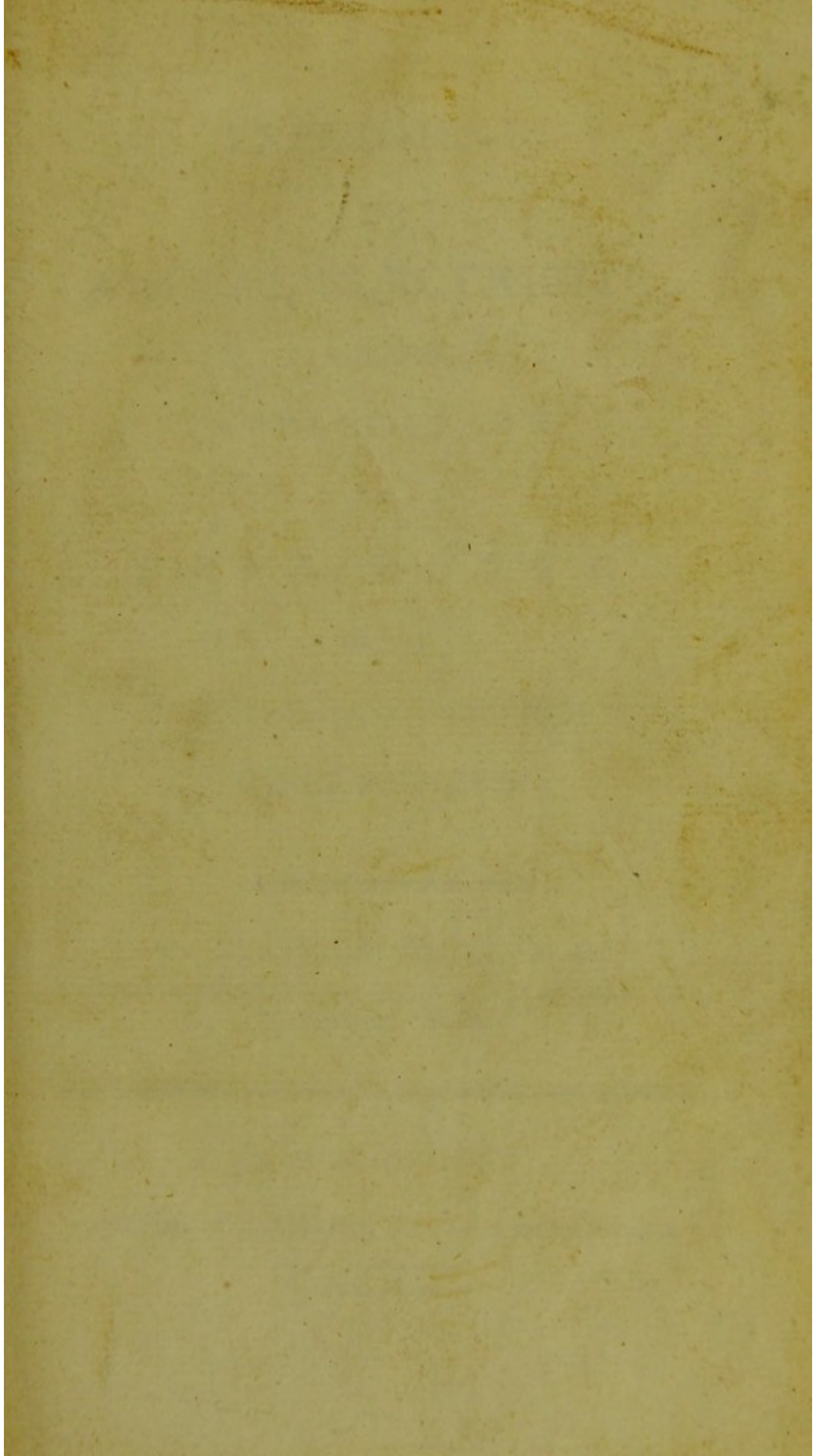
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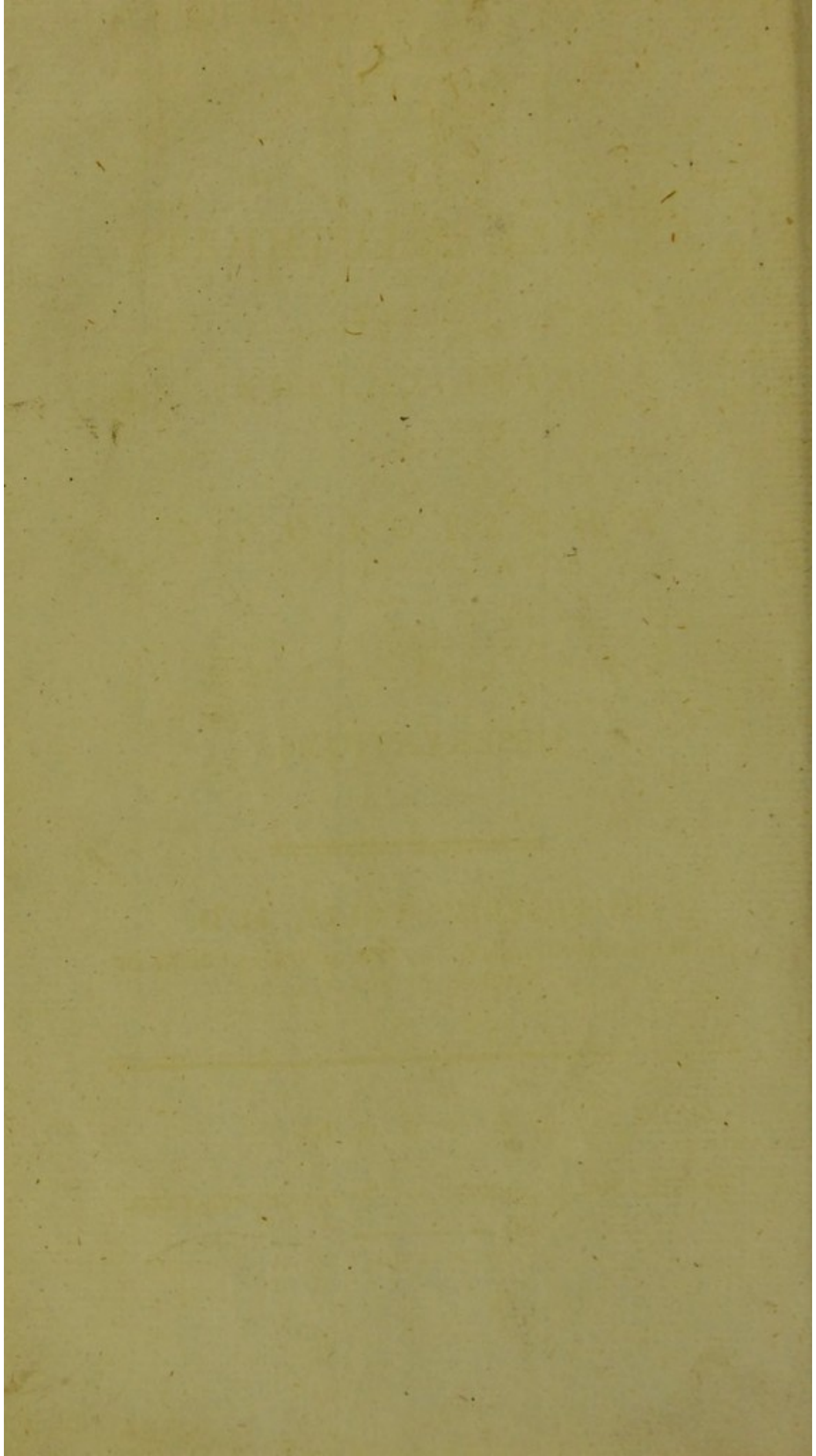
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EXPERIMENTS
ON
ANIMAL ELECTRICITY,
WITH THEIR
APPLICATION
TO
PHYSIOLOGY.
AND SOME
PATHOLOGICAL AND MEDICAL
OBSERVATIONS.

By EUSEBIUS VALLI, M. D.
CORRESPONDING MEMBER OF THE ROYAL ACADEMY OF
SCIENCES OF TURIN.

L O N D O N:

PRINTED FOR J. JOHNSON, ST. PAUL'S CHURCH-YARD.

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EXPERIMENTIS

ANIMAL ELECTRICITY

APPLICATION

PHYSIOLOGICAL

AND

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BY RUSSEIUS VALLI, M.D.
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1800

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
SAMUEL FORT SIMMONS,

M.D. V. R. S.

THESE
EXPERIMENTS

ON
ANIMAL ELECTRICITY,

AND

WITH VERY SINGULAR ESTEEM,

BY HIS MOST HONORABLE SERVANT,

E. KILLI.

T O
Mr. MOORCROFT,
VETERINARIAN.

S I R,

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.

P R E F A C E.

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

speēt 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
and

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

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 sensation 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

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

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 ra-
ther stand forwards in my defence,*

EXPERIMENTS

EXPERIMENTS

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 preceding ones; the Italian physicians endeavoured to impose upon the world with their *medicated tubes*.

Whilst this was going forwards, 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,

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.—*Elettricità 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

at

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

B 4

floor.

floor. Considerable movements were observed in the animals, whether of cold or warm blood, whenever it lightened. These preceded thunder, and corresponded with its intensity and repetition, and even when it did not lighten, the movements took place when any stormy cloud passed over the apparatus.

Professour Galvani one day suspended some frogs, perhaps with similar views, on metal hooks, fixed in the spine of the back, upon the iron railing of his garden; several times he remarked that these animals contracted, and appeared to receive shocks; at first he conceived the movements were owing to changes in the atmosphere, but a more scrupulous examination undeceived him. Having placed a prepared frog upon an iron plate in his room, and happening with his dissecting forceps to press it against the plate, he observed the movements to take place.

This

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

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.

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 com-
 motions,

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 element,

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 hypo 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.

EXPERI-

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 scissars passed underneath them served as a coating, and a French crown piece as a conductor; the movements were very smart. During these discharges, the animal appeared perfectly tranquil. For some moments, notwithstanding my exciter, the wing remained motionless; I then had recourse to a leaden coating and a copper conductor: this change did not answer my expectation, for the wing still continued without motion. To ascertain whether this was owing to a state of insensibility, or want of energy, or to the muscular fibres being fatigued, I pricked and irritated the nerves, the animal screamed 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 meantime, 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

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

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 repeating this experiment upon frogs, after having taken off, and re-applied several times, the coating of the muscles, to observe 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

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 ankle 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.

EXPERI-

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 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,

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 pur-

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pose

pose 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 likewise

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 scissars are insulated, no movements take place.

The small frogs have likewise presented me with a very singular phenomenon. As they enjoy much vitality, their legs are contracted, or drawn upwards with as much force as if they were still connected with the body. If, during this state of contraction, the spine or the nerves be touched with the scissars, the legs almost constantly become relaxed, and fall down.

I have likewise seen in two frogs the movements occur at the distance of half an inch from the scissars, and which ceased at the moment I insulated the scissars.

A still better manner of exciting the electricity is, to hold the feet of the frogs between the rings of the scissars, and incline them in such a way as to permit the medulla spinales to fall gently upon them.

Instead of two conductors, as in the first experiment, there is here only one, and

on

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

The discharges excited by these two means soon cease, and then the coating becomes necessary.

It constantly happens in this new circumstance, when the coating is of the same sort of metal with the conductor, that the electricity of the animal does not make its appearance.

Professour Galvani asserts, that he has sometimes obtained some very weak marks of it, although the plate which the coating touched, the coating itself, and the conductor were all of iron. There is, however, reason to believe, that all the iron was not of the same quality, or that this circumstance 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 movements,

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

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, bismuth, antimony, or zinc, very sharp movements were induced, which put the animal in a state to experience slight convulsions, when the two different leads were re-applied. When the electrical power of the animal was still nearer being exhausted, different metals excited convulsions, but coatings of glaziers and assay lead were incapable of commanding them.

The electrical action has been found to disappear in the following order; glaziers lead being constantly used for one of the coatings.

1st, Assay lead has ceased to induce movement.

2d, Tin,

3d Antimony,

4th, Zinc,

5th, Copper,

6th, Gold,

7th, Silver.

A fimi-

A fimilar 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 flightest 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

Water and other fluids, which give passage 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. Bibliothéque 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

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 hav-

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ing resource to the exciter, the movements took place.

EXPERIMENT XXIV.

Although the last experiment appeared decisive, yet I could not avoid repeating it on several more frogs. Whenever the ligature of the nerve was made near its insertion into the muscles, the electrical fluid did not show itself; but under opposite circumstances it was sufficiently evident. By this means, I could at pleasure either suspend the movements, or call them forth, by approximating or removing the ligature from the muscles.

EXPERIMENT XXV.

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

EXPERI-

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.

EXPERI-

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 $\frac{1}{8}$ 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 flipped 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 $\frac{3\frac{1}{2}}{100}$ 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 stimuli 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.

EXPERI-

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 pos-

ferred 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.

EXPERI-

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

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!

EXPERI-

EXPERIMENT LX.

Having opened the abdomen of a live frog and exposed the crural nerves, I passed under one of them the flat handle of my scalpel, and applied some opium upon it. In four minutes the animal lost the power of moving the leg. During this space of time, the exciter produced the ordinary effects. This experiment, which I have repeated more than forty times, has not, like many others, presented anomalies and want of uniformity. At every repetition, the frogs have constantly lost their power over the leg, and the exciter has produced motion.

In this case, it was necessary to coat either the nerves or the muscles.

Why opium should, under certain circumstances, act upon the nerves and not in others, is what I am altogether ignorant of, and perhaps the best informed physiologists are not much better instructed on this head.

Are

Are physicians acquainted why those medicines, to which we have improperly given the title of sedatives or antispasmodics, 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 adductor 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 employed

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, here-

after 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 these affections.

EXPERIMENT LXXVI.

The brain was wounded in a very considerable number of frogs. Some of them died immediately, others in a few hours, and some survived for some days.

It has been found, that some frogs only experience a slow lingering death from the destruction and laceration of this organ; all of them, however, when exposed to the test of the exciter, exhibited their ordinary vigour, except three, in which the brain had been lacerated, and which only gave momentary shocks. The muscles of these frogs had evidently undergone an alteration.

Several frogs killed by a discharge from a jar, presented the same signs with others which had not experienced this commotion.

EXPERI-

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 previously divided the blood-vessels. From time to time I excited shocks in one of them, and which, after death, continued to afford some tremblings. The other, which had been left to itself, was not capable after death, of being excited by the ordinary process.

EXPERIMENT LXXXI.

Whilst exciting shocks in the wing of a fowl, just upon the point of dying from loss of blood, the convulsions ceased; but when I omitted using the exciter, they again were renewed.

EXPERIMENT LXXXII.

A dog was made to swallow a quantity of arsenic, which caused his death. When submitted 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

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 previously prepared. The animal was restored to life.

EXPERIMENT LXXXVIII.

The same experiment was repeated upon another fowl, in the presence of M. de la Metherie, who examined it himself, and was convinced that it did not shew any signs of life. I then employed my exciter upon it, but many discharges were required to restore it to its natural state.

EXPERIMENT LXXXIX.

Encouraged by this result, I caused another fowl to be drowned, but what was
my

my surprize 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 refuscitate.

EXPERI-

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 glafs vessels filled fometimes with inflammable air, and at others with vitiated, or with phlogifticated 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 stiffness and tension, at which time the movements 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 became motionless after three or four shocks.

For a few minutes they were left to themselves. I afterwards endeavoured to excite them, but without success. Notwithstanding 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 inertia.

There exists in the animal economy a principle of life which prevents the equilibrium 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 exposed to the contact of nitrous air, continues to beat for some time.

EXPERIMENT C.

Frogs which have their heart exposed, do not perish more quickly in nitrous air, than others in which it is left under natural circumstances.

I have sometimes seen the movements of the heart cease, and notwithstanding the animal continue to exercise its voluntary motions. This fact leads me to presume, that nitrous air acts more upon the irritability of the heart and lungs, than upon the sentient 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.

Phlogificated air, or azotic gaz, is
 [equally noxious to frogs with the nitrous.
 After death, the heart still beats. Their
 flesh is of a beautiful purple colour, as well
 as their blood. With regard to the move-
 ments, we observe nearly the same acci-
 dents as occur in the nitrous gas, except
 that the azota does not produce any change
 in the muscles taken off by the animal.

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

The flesh of animals as well frugivorous
 as carnivorous, and even that of fishes,
 furnishes, on chemical analysis, a consider-
 able quantity of phlogificated air.

Phlogificated 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 antiseptic 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.

EXPERI-

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.

EXPERI-

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 particularly 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 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. Transf. vol. lxxv.

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

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 motionless. I then brought the cut ends of the nerves near the vessels, without suffering them to touch the coating, and the exciter produced very evident movements at several 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 ceased when the bone was divested of its periosteum.

EXPERIMENT CXXI.

In place of the bone, I substituted the nerves of another frog, and the movements still 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
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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

tioned 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

table differ in one very material circumstance. 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. Transf. 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 increasing

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, concentra-

centrating 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

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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.

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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,

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 $\frac{1}{2}$ 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.

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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 $\frac{1}{3}$

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

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. lxxiii. lxxv. from whence the preceding extracts have been taken.

If we were acquainted with the electrical organ of the filurus, which Mr. Broussouet calls "le Trembleur," and that of the fish Mr. Paterfon 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.

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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

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

charge 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 flight 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 muscles. To support this opinion still more, he observes, that strong shocks are produced by touching the coating of the brain, and that if the muscles are coated, the animal only moves a little, and that even but seldom. He likewise remarks, that the experiment succeeds on touching the coated nerves when separated from the muscles, but that there is not any effect when the muscles are coated and separated from the nerves. (Journal de Physiq. mois de Juiet 1792, p. 52, &c.)

The author by applying a positive or negative electricity to the nerves, believed he could direct it against a surface charged *plus* or *minus*, but this was a mistake.

The nerves communicate with all the points of the muscles, and consequently with several surfaces charged with opposite electricities. - Thus it is not possible by this means to judge of the nature of the electricity we may wish to discover.

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,
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are disposed in such a manner as to refuse a passage 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 itself, however strong it may be, produce the least effect, because it does not act as a stimulus, but by another law, as will be shewn in the fourth Section.

But that the agent, which calls these organs into action is electricity, is demonstrable both from analogy and facts. These organs possess irritability in common with muscles. The irritability of the muscles is most powerfully excited by the animal electrical fluid. It is therefore very natural to conceive, that these organs experience the same effect. If I am not mistaken, we have incontestible proofs of this in the history of diseases of the nerves.

A person seized with convulsions, one moment has very terrible shocks in his
whole

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,

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which

which would have come on, had they neglected this agitation.

The impressions made by objects, whether external or internal, upon the common sensory, are affected by the same cause which excites motion.

The force or energy of this fluid is always 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 discovery, 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 electricity under another point of view.

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

Previous to the discovery of Galvani, it was asserted, that animals contained electricity. Mr. Nicholson, as far as I know, was the first who set on foot any experiments to ascertain the quantity a man was capable of containing. "If we suppose," says he, "the bulk of a man to be only "three solid feet, or 5184 solid inches, the "natural electricity of this mass, will be "equal to the charge of a battery of up- "wards of 15,000 square feet."

It is presumable that this matter exists in different proportions, in different species of animated beings, and that each has its determined measure of it, in like manner as each possesses its measure of heat.

This electricity cannot be kept in a state of equilibrium. Muscular action, the secretions, evacuations, heat, and the emotions of the mind, give rise to constant changes in the whole constitution of the animal. The electricity follows these

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.

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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 flight, 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 advisable 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 furnished with a coating. It is upon these two facts, that the opponent of animal electricity rests his first position.

Are then the nerves upon which the experiment is made, not distributed upon all the muscular fibres? Is there any point of a muscle destitute of nerves? Are they not all so circumstanced as to communicate with each other? Does not the surface of muscles 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 surface of the flesh, can communicate with the constituent fibres of muscles, so as to excite their irritability; how happens it that he will not allow these nerves to serve as conductors of the native electricity?

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

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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
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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.

EXPERI-

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.

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 metallic 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

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

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

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 the electricity. They alone are conductors of it, and it is by them alone that this circumstance is effected.

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

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.

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

I propose treating of muscular motion, the secretions, sensations, and nutrition, both in the state of nature and that of disease.

From amongst the immensity of objects which this widely-extended field presents to my consideration, I shall only select those which appear to me most relevant to the subject, and most useful to the art of healing.

OF MUSCULAR MOTION.

Physiologists acknowledge the existence of three distinct powers in muscles, viz.

1st, 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.

The

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. (Vallifneri.)

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 dedicated 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

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 inferitur 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.

OF THE NERVOUS FORCE;

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

fect has a cause, and this cause we assert to be electricity. But in what manner, it may be asked, does electricity operate? Does it irritate the muscular fibre at the instant it passes from one to the other surface of the muscle, and that its equilibrium is restored?

For a time I believed this to be the case, and it was likewise the opinion of Professor Galvani; but on coming to consider it with more attention, it appeared to me to contain insuperable difficulties, viz.

1st, Each discharge ought to be succeeded by a new rupture of the equilibrium, which could not be effected with the rapidity we observe in the movements, nor perhaps without a constant loss of this fluid, and likewise a more complicated architecture.

2d, The attraction would be the effect of a stimulus, which does not accord with the idea philosophers have associated with the term.

For

For my own part, I conceive the process is carried on in the following manner: The attraction of the fibrils is owing to the different state or condition of the electricity in the muscles, that is to say, that in the state of contraction, the surfaces of the fibrils are differently electrified from what they are in a state of relaxation. Agreeably to this hypothesis, the equilibrium never takes place.

That electricity increases the cohesion of bodies, and that after a discharge it may be found in two different states, are facts demonstrated by experiments so decisive as to preclude every doubt. I shall not hesitate to insert them here, as they form the basis of my theory.

Mr. Symmer was the first who discovered the power which electricity possesses, of increasing the cohesion of bodies. This gentleman had observed, that upon putting off his silk stockings 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 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.

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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 lacrem 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 sef-
qui 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, inducibam extimam hujus faciem, attractando simul indusia junctorum vitrorum quatiebar, valida repente existebat ab eâ explosione vitrorum cohæsiō, eademque validior pro explosione vehementiore.

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

Unius superiorem, alterius inferiorem faciem denudabam: hoc illi imponebam: continuo cohæsiō obtinebat aliqua: attractatis extimis indusiis quatiebar, atque sub hac explosione cohæsiō invalſcebat.”

The author having detached the plates A and B before the explosion, found that the plate A was positively electrified on both sides, and the plate B negatively electrified on both its surfaces likewise. When he separated the same plates after the explosion had taken place, the electricities appeared quite opposite, that is to say, the plate A was electrified negatively on both sides, and the plate B positively. Phil. Transf.

In the experiment of Lane, the result was the same with regard to the electricity of the plates examined before, and after the explosion, as in the experiment of Beccaria.

But Lane placed the two surfaces 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 single plate, he placed one of them so as to touch the prime conductor, whilst he touched the other with his finger.

Mr.

Mr. Henley treating in the same manner the Nuremberg glasses, 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

electrified, the force of attraction is continually in action within them.

An opinion has been entertained by some, that irritability is only a transitory attraction, but this is a mistake. If we cut the muscle of an animal we observe in a moment, the antagonist muscle take on extraordinary movements, as there does not exist any longer a force capable of opposing any exertion to it, and preserve a counterpoise.

Let us examine an athletic, nervous man, and in certain attitudes, we shall observe the bodies of some muscles become turgid and elevated, whilst their antagonists become more flaccid than even during a state of rest.

Thus betwixt muscles which are in a state of contraction, and those which are not, the only difference is as to the intensity of attraction. This difference, however, is very great. Borelli demonstrated, 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, Languish, Pozzi, and several others. (See Haller, *Phyf. T. iii. lib. xi. f. 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

to

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 concurrence

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 contraction

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
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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 morbid 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 occurrence 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 impulses, 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 expla-

nation 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 physical cause which determines all these different movements, by acting upon the springs, which are already prepared for this purpose. The mind has not any concern with it; on the contrary, in general every thing is performed without its being conscious of it.

Cataleptics and Somnanbulists were very common during the reign of Mesmer. This celebrated impostor took advantage of the credulity of mankind, and made a most shameful and infamous abuse 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 spontaneous.

Different stimuli determine the action of these different organs. When I speak of stimulus, I understand an impression

made not upon the fibre, but upon the nerves of a given part, which in consequence throw the muscular fibre into the state of contraction.

It is in this sense that I call the blood the stimulant of the heart, the aliment, air, gas and excrement the stimuli of the intestinal canal, the urine that of the bladder, light the stimulus of the Iris, and so of the rest.

Whenever the nerves feel a stimulus, the electrical fluid is excited, and then its effects are made manifest. Under these circumstances that takes place, which we have noticed when speaking of voluntary motions, that is to say, the electricity passing from one state to another, and changing its mode of existence, produces the contraction. Its equilibrium is never established. A new cause 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 discharged by the nerves, is a cause sufficient to produce the effect sought for.

6th, Some experiments, as for instance, that of the learned Cigna with silken ribbons, prove that electricity may exist naturally in substances in the two opposite states.

One might establish a theory upon more solid grounds, were one acquainted with the ultimate structure of these organs, and all the laws of the electrical principle, but being destitute of these advantages, and proceeding with unassured steps, it is much to be doubted whether I have seized the truth.

It may be asked, whether an examination of the motions of every organ in particular, would conduct us farther in the discovery? It appears to me, that it would produce a contrary effect, and embarrass our imagination still more. We
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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 witnesses 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.

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We shall now descend to the consideration 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 lemen, under the circumstances just mentioned, occasion disagreeable accidents, which are not always unattended with danger.

2d, Extraneous matters, either introduced 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 render 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. Some-
times

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, Hysterical 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 Diff. 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

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

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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.

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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 mechanism. The quantity of blood which passes through it is enormous. It has been estimated at one third, and in the more moderate calculations, at one fourth part of the whole mass. This blood after having circulated through it, and afterwards descended by the jugular veins to the heart, is sent back again several times to the brain. The artifice, which nature has employed for this purpose, is amongst the most singular we have any knowledge of.

We shall take some notice of it in this place, both because it is not sufficiently known, and because it leads us to a research which is nearly connected with our subject.

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

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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 trans-

versely near the sinciput, and saw with surprize, 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
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of the heart, and that the motion of the jugular veins was synchronous to that of the arteries.

Being astonished at this unexpected phenomenon, 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 infe-

rior. 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 alternately, 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 turgid 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 different 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
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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 incision recover their powers; yet the nerves under the incision seemed at the end of that period, as large in the limb in which the experiment was made, as they were in the sound limb. (Loc. cit.) Monro observes also, that the substance of the nerves is not only medullary, but that it is mixed with a cineritious matter, which is furnished to them by the pia mater, and that it is from this membrane and from its vessels, that independently of the brain, they derive the energy and the principle of life which they possess.

Thus the great quantity of blood which goes to the brain, does not appear destined for the general secretion of the nervous fluid, but for the purpose of furnishing materials for the preservation of so large a mass as the brain.

It is however probable that the blood-vessels may have another office, viz. that of separating the electrical fluid in the brain
(by

(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 railery 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 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. (Tiffot 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 tranquillity. 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 disequilibrium. (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

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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

sufficiently manifest, as in the seventh and fifth pair (V. Haller, Prim. Len. Physicum 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

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 expo-
nere 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.

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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 likewise diminish it, and mitigate by this means the pains by which it is afflicted, but with much greater difficulty and exertion.

It acquires the same advantage by exercise and habit. A man born blind, for instance, who continually exercises his sense of touch for the purpose of becoming acquainted with the sensible qualities of bodies, brings it to such a degree of perfection, as even to distinguish colours by it.

The passions in general render the senses more delicate and exquisite, and in this situation the slightest causes may disconcert both the physical and moral parts.

Irritation of certain parts of the body do the same, particularly of those organs endued with much sensibility, and which sympathize with the rest of the system, as the stomach and uterus.

When I experience an indigestion, the
merest

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 unfocial. 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,

ness, became almost an object of detestation, and the innocent amusements of her children, on whom she lavished extreme affection and care, became tiresome and disgusting to her. Odours affect them strongly. Every sudden noise electrifies the whole machine. They lose all relish for the most exquisite viands. They desire what formerly they disliked or detested, and sometimes these desires are preposterous. In a word, an extraordinary change takes place in their constitution, which affects both the character and passions.

The mother of Doctor Mantovani of Pegognago, a country in the Province of Mantua, during pregnancy, conceived an aversion for wine, and retained it even after delivery. She became pregnant a second time, and from the instant of conception, she felt a return of desire for wine, and an insuperable aversion to water. This
 repugnance

repugnance for water was such, and continued so long, that when I saw her (many years after she had left off having children), she could not drink any without suffering an indisposition from it.

In the diseases we call nervous, and in acute fevers, the sensibility is such, that a slight current of air, whispering, a very weak light, and even entire darkness, irritate and disquiet them, brings on convulsions, phrenzy, and delirium. This happens particularly in hydrophobia and inflammation of the brain. Inflammation frequently increases the sensibility of the organ it affects, as in ophthalmia. A man with inflamed eyes could see clearly in the dark, but on his curement he was deprived of this privilege. (Nicolai Vom. Schmurze.—V. Haller, Phys. T. iv. L. x. sect. 7.

We know tolerably well the causes which may distinguish or increase, or charge

charge 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 indicate a 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

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 morbas attinet primario universales, et acutos quorum primum vel insultum comitantur vehementia symptomata: illi certé a fegni 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 fan-

guinis 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 corripitur 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 muscutorum maxillam inferiorem deprimentium, sæpius repetito laborat et tandem frigore subito per totos artus diffuso (quali symptomati hystericas etiam lipothymicas a mera spirituum ἀλαξία 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

causes capable of altering the mass of fluids, yet it is always true, that the animal spirits are the immediate principle which forms the disease. He is moreover confirmed in these sentiments on finding, that by the new theory he can explain phenomena which were impossible to be resolved by the obscure doctrines of the ancients. The ancients, for instance, explained by sympathy, the vomiting which follows a concussion of the brain, as well as that which attends nephritic affections; but Morton observes, that in order to explain these accidents, we must admit a continuity or connection of the whole body with the brain. With the same principle he explains the metastasis or translation of a disease from one part to another, which is observable not only in the crisis of fevers, where the disease quitting one region all of a sudden attacks the brain, but likewise in *peripneumony*, which frequently changes

changes into angine in the unfettled 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 morbid 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 mass of blood and humours which circulate through the body, and impress upon them a diseased character, and it is in this way that metastases take place. (De morb. univers. acut. apparatus. curat. morb. univers.)

Morton likewise makes many other judicious reflections in the course of the work we have just quoted, and subjoins several others in his *Dissertation de Cortice Peruviano atque virtute ejus febrifuga*. It is here that he observes, that poisons act immediately upon the animal spirits or principle of life, and that at the very instant the poison is swallowed, the patient is attacked with a fever of the malign kind, and other distressing complaints. And he likewise observes, and with propriety, that the action of topical applications throws much light upon his theory. “*Quantum enim cerebrum atque genus nervosum afficiuntur ab emplastro de Galbano*

bano

bano umbilicali et a variis odoramentis fœdis aut moschatis ut in hysteriis videre est? Quantum a cataplasmate ex allio plantis pedum applicato, paucarum horarum spatii, natura patitur, ut facile cum suo damno quivis in se ipso experiri potest? Quantum denique a suppedaneis, pericarpis, vesicatoriis, epithematis, cataplasmatibus, 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 these observations of Morton, with this difference, that they reject the depravation of the animal spirits which this physician admitted, and instead of it believe that miasmata affect the nerves alone, and not their fluid. This doctrine is more simple, more conformable with facts, and with the recent discovery of the nature of the nervous fluid.

All medical men, however, are far from being convinced that the impresson of miasmata is only made upon the nerves, and there are many who still retain the errors of the humoral pathology. If, notwithstanding, they would take the trouble of examining the history of diseases, they would soon lay aside their prejudices. It would be the greatest possible gratification to me to attempt a work with this view, but I must not flatter myself that a physician whose reputation is confined within very narrow limits, would be able to persuade

suade.

suade or make many profelytes 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. (Tiffot 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 Nimega attacked with the plague, immediately lost their strength, and became incapable of moving, whilst others retained their powers to the very moment of dissolution. (Diemberoeck 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 Bruffels 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

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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

of

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 factò,” 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 quotidi-

anis, et statis periodicis paroxyfmis jam tandem typum, ac genium suum palam prodeunte, suspicatus sum fermentum febrile venenatum delitescens, et spiritibus implicitum, symptomati huic dolorifico admodum molesto, ansam præbuisse. 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

ful 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 evacuation 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

diseases still more terrible. M. Ab. Elten discovering that he had a pestilential bubo, swallowed a decoction of tobacco, which threw him into an agony, and such loss of strength, as to bring on a swoon, from which it was thought he would never recover. Sometime after, however, he came a little to himself, vomited excessively, and had afterwards several very considerable evacuations by stool. He was put to bed in so weak a state, that he could not move, but his bubo had disappeared. He drank some hot wine with spice in it, slept well, and sweated much; on his awaking he drank more wine, fell asleep again, and in the morning found himself quite recovered. (Diemberbroeck de Peste, Hist. 40.)

Nappeltern finding himself attacked with the plague, went into a convivial company in hopes of dispelling his apprehensions, and getting rid of his complaint.

Having

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 miasmata 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,

tions, 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 awkward 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 effervescence 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 cathartorum usu circa declinationem morbi in tempestive præscriptorum parum alleviare videatur, imo nonnunquam apyrexia omni modo 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.)

If the effervescence had been carried almost to its term, how could it begin afresh, and go through the same course, 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

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 morbidic 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 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 symptom 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 fluids, 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 sollicit and augment the action of the kidneys. 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 foetid. (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, 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.

Hippocrates 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

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 Eru-
ditorum 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 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

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

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 dropfy 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

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

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 morbidic 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.—

“ Lenies

“Lienes tumidi si illis non veniant sanguinis eruptiones et oris graveolentia deficiunt in ulcera prava tibiaram 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

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 re-

collect 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,

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 excruciating pain, the bones have been known to become soft and flexible. (Gagliardi, Morgagni.) The enlargement and caries of the teeth in the scurvy, are
the

the effect of a depraved 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 re-
peat

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

of physiology, pathology, and the practice of medicine. If I have not been so happy as to have succeed in my design, I yet flatter myself this Essay will be found to contain some matters of use.

A P P E N D I X.

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

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, Differt. 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. Transf. 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. Transf. 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 animal 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.

Y

I shall

I shall make an assemblage of the most remarkable facts and observations with regard to the alterations of animal heat, which appear to militate against 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

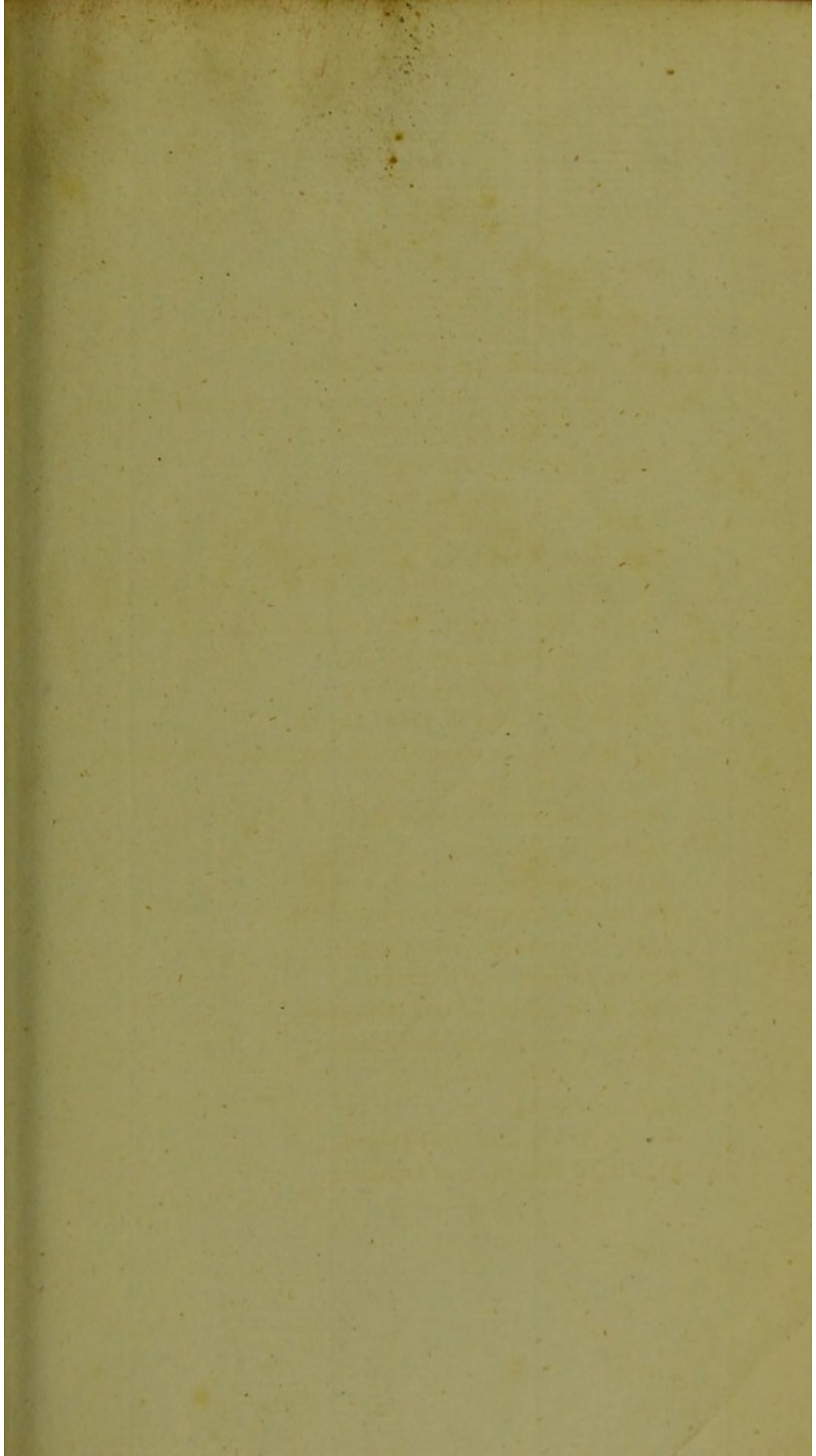
am in search of, I shall make myself the subject of many experiments.

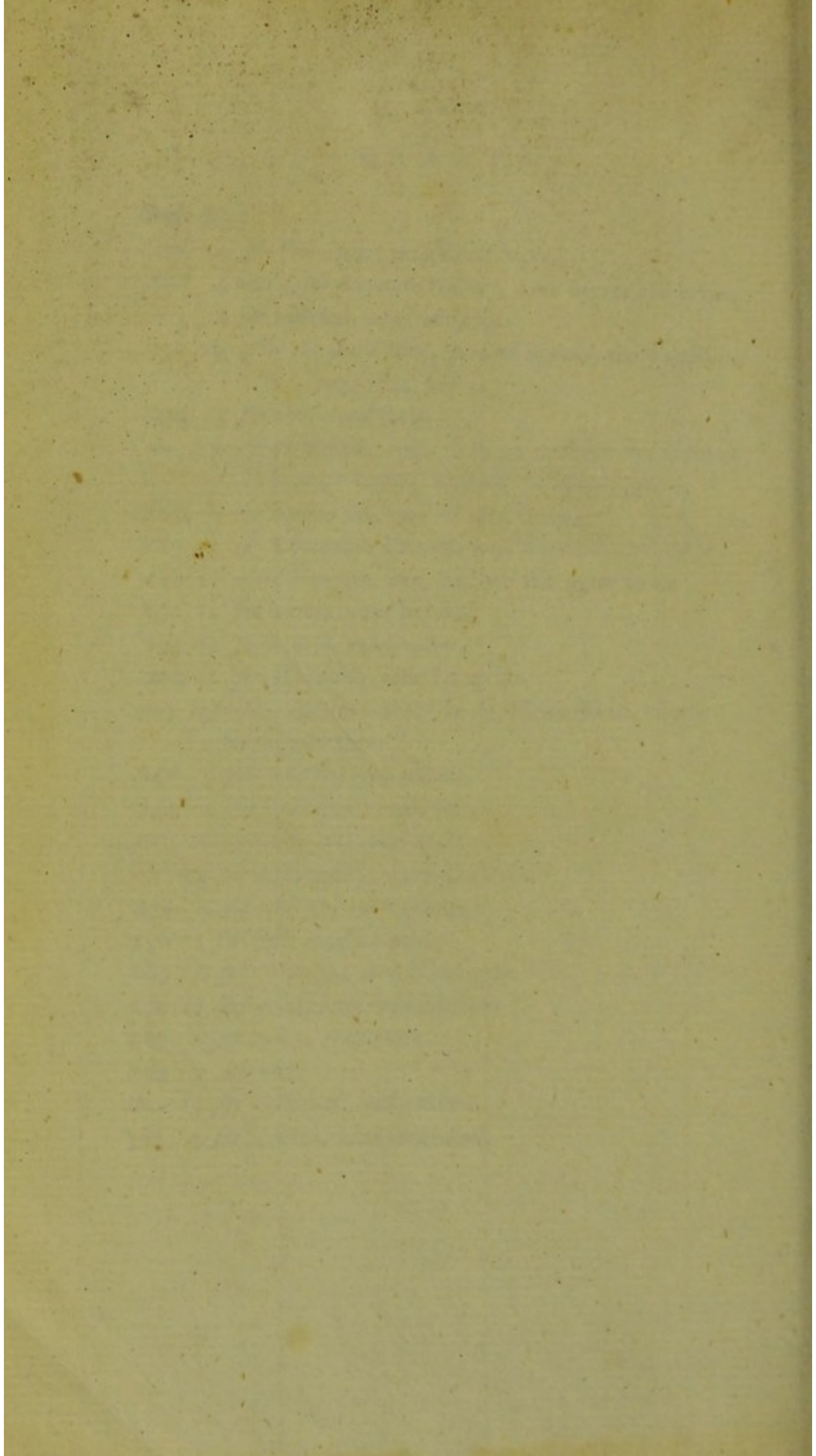
If there is a means of advancing in animal physics, it is certainly by that of experiment; but we must acknowledge that this field has, hitherto, been little cultivated by those who have devoted themselves to the study of the human body and of medicine.

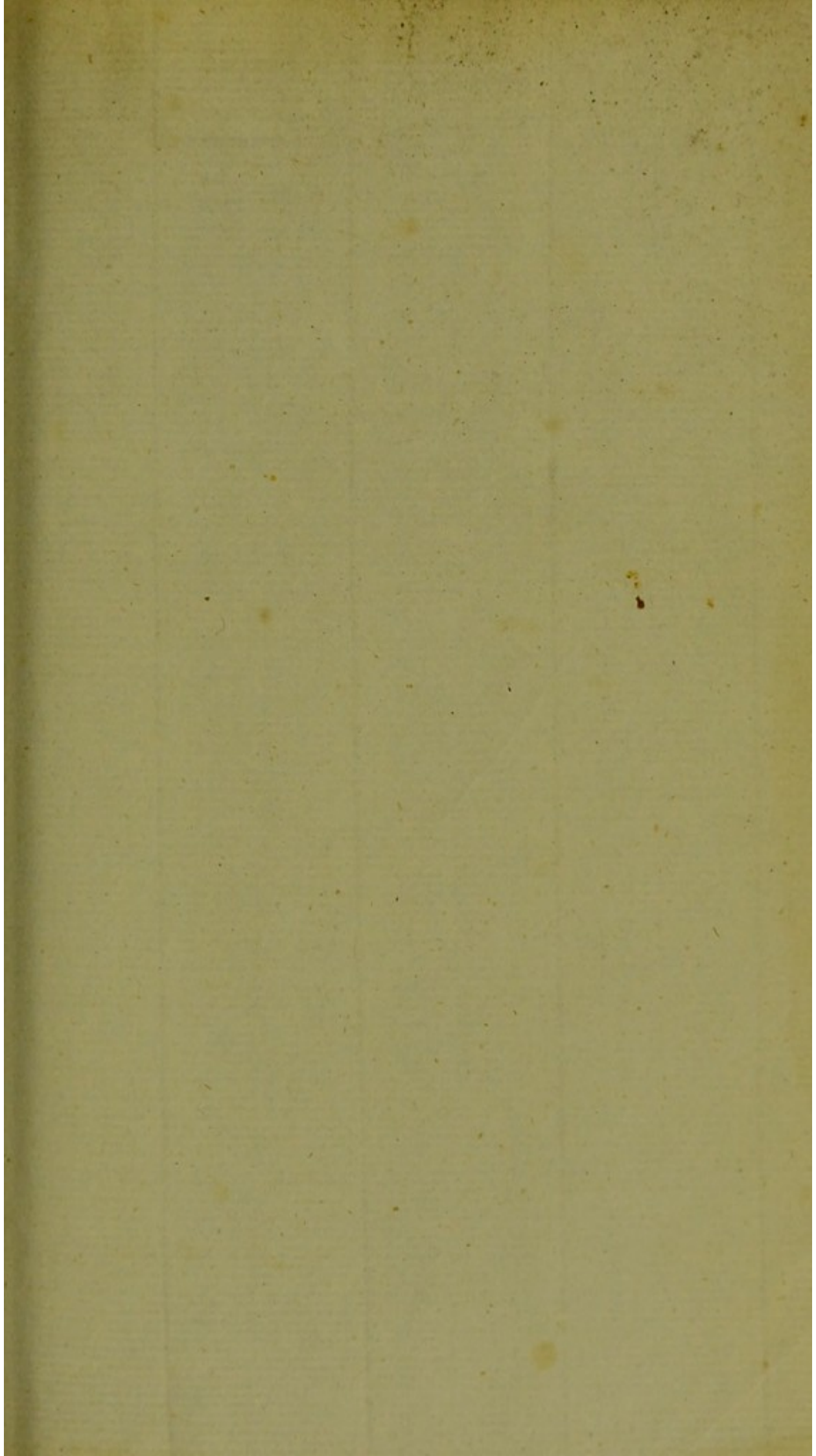
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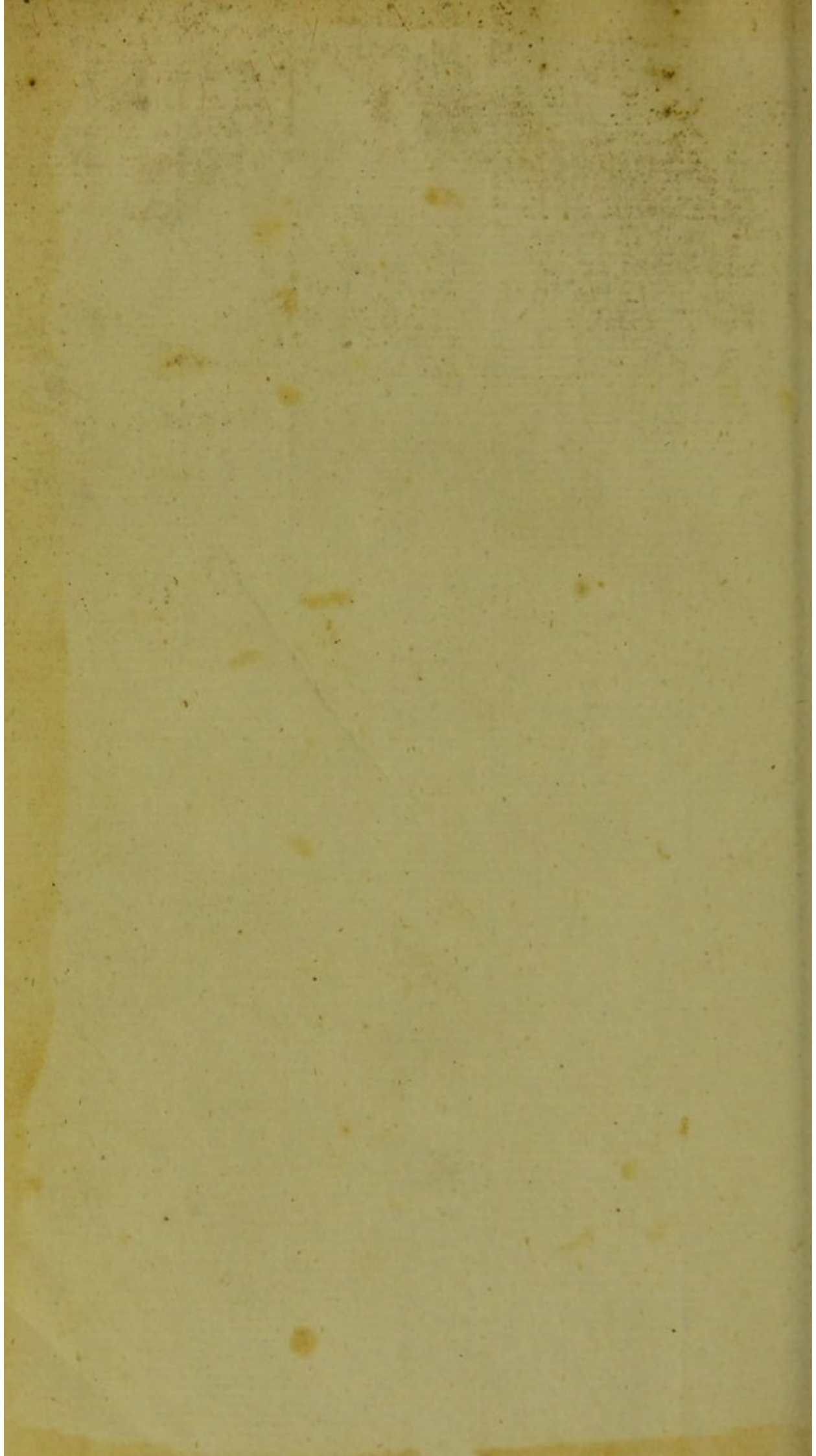
E R R A T A.

- Page Line*
- 135 9 for Brouffouet 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 Praelect.
- 188 18 after Newton, add, and we also agree to it.
- 194 14 for lacrem, read lœvem.
- 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.









S2

