

PHILOSOPHICAL  
TRANSACTIONS.

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I. *The Croonian Lecture. On the Irritability of Nerves.* By  
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Read November 20, 1800.

THE nerves have been hitherto considered as chords that have no powers of contraction within themselves, but only serving as a medium, by means of which the influence of the brain may be communicated to the muscles, and the impressions made upon different parts of the body conveyed to the brain.

The difficulties which attend every attempt to investigate the real state of the nerves in the living body, and the impossibility of acquiring any information upon this subject after death, may be urged in excuse for this opinion having been so universally received, since it will be found, from the following experiments and observations, to be void of foundation.

The only means by which any knowledge respecting the irritability of nerves can be procured, must be from the operations in surgery performed upon nerves, either in a healthy state, or under the influence of disease; or from experiments

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made upon animal bodies before they are wholly deprived of life, and instituted for that particular purpose.

My attention was directed to this subject by the following case, which explains many circumstances respecting the actions of the nerves when under the influence of disease, and gave rise to the experiments and observations contained in this Paper.

A person thirty-six years of age, naturally eager and anxious in his disposition, whose stomach was peculiarly irritable and irregular in its action, in the winter of the year 1796, while riding in the country, was thrown from his seat by a sudden motion of the horse; and, in endeavouring to save himself, fell with his whole weight upon the end of his thumb, against the pommel of the saddle.

The part swelled, and became very painful. A few days after, he hurt it again, which prevented the swelling from subsiding, and it remained uneasy and enlarged for three or four months. It afterwards got well, but the motions of the thumb were not always under the command of the will; so that he was sensible, in the years 1797 and 1798, while writing, of finding a difficulty in forming particular letters.

On the evening of the 16th of October, 1799, which was cold and damp, he was travelling in a post-chaise with two other persons, and let down the window, to speak to the driver. A cold wind blew directly into the carriage, and he endeavoured to pull up the window; but, not seeing the glass rise, he looked down, and his hand, instead of pulling up the window, was lying upon his knee. The thumb was bent in towards the palm of the hand; a spasm came upon the muscles of the arm, making them bend the elbow; and immediately he became insensible: in a quarter of an hour he perfectly recovered himself. Some

hours after, upon bending his thumb, to shew what had happened to him in the carriage, there was a return of the same attack, which also rendered him insensible for a few minutes.

From this time, he had no return of these attacks for nine weeks; at the end of which period, on the 18th of December, 1799, he was waving his hand over his head, with a degree of eagerness, as a sign for some people to make haste and follow him; this exertion made the thumb contract towards the palm of the hand, and he fell upon the ground in a state of insensibility. This attack went off as the others had done; he had another in the evening; and, in the course of the next day, two more, equally violent. As the motion of the thumb was the first symptom in all these attacks, the assistants were led to contrive a glove, the front of which was strong enough to resist the motion of the thumb, and to keep it in its place: while this was kept on, the attacks were less frequent. A ligature was then applied round the fore-arm; when the thumb was beginning to be agitated, this was tightened, and the spasms were found to be arrested at the ligature, and of course deprived of their violence.

From this time, a tourniquet was kept constantly upon the fore-arm; and a person was always in readiness to tighten it, the moment the spasm was expected, which was always preceded by a general feel of uneasiness all over the body: as soon as the spasm went off, which it did instantaneously, the tourniquet was loosened. The spasms in the thumb and fore-arm returned frequently, and at irregular intervals, generally every three hours, sometimes oftener, and once did not come on for thirty-six hours.

On the third or fourth day, electricity was tried, with a view

to relieve them; sparks drawn from the thumb, produced tremors in the muscles, which were confined to the thumb. An electric shock through the ball of the thumb, brought on a very severe spasm in the arm; but neither sparks, nor a shock through the other thumb, produced any sensible effect.

On the 29th of December, I first saw the patient; and, after watching the symptoms for three days, made the following observations upon the complaint.

That the beginning of the attack was some involuntary motion of the thumb and fore-finger; and therefore, the disease appeared to be in the branch of the nerve which supplies these two parts, called by WINSLOW, the median nerve.

That the progress of the spasms was in the direct course of the trunks of the median nerve, up to the head.

That compressing the parts in the course of that nerve, when it was done before the spasms had reached them, always arrested their progress; but, when once the muscles had become convulsed, or agitated, the same compression had no effect in stopping the progress of the spasms.

The mode in which the spasms were propagated along the course of the nerves, was as follows.

Five or six tremors took place in the flexors of the thumb and fore-finger; then similar convulsive motions affected the muscles of the fore-arm; soon after, the muscles of the arm were thrown into the same kind of action; afterwards the pectoral muscle, and scalmi of the neck: the muscles of the lower jaw were probably in the same state, although their action was not within the notice of the by-standers. The head was pulled forcibly to that side, in quick successive motions, and in a second or two, the whole ceased; the parts became tranquil, the insen-

sibility went off, and the patient recovered himself: there was, however, a general feel of languor and distress over the whole body, before the recovery.

From these observations, the disease appeared to be decidedly in the inferior branches of the median nerve; and the irritation was conveyed along its course, from its terminations in the thumb and fore-finger, to the origin in the brain.

It was proposed to divide the nerve, as it passes from under the annular ligament of the wrist towards the thumb, to cut off the communication between the diseased extremities and the trunk of the nerve, and so put a stop to the progress of the irritation which constituted the disease.

That such an operation might be attended with success, was not only rendered probable from reasoning, but the performing it was fully justified by the success which had been experienced from a similar operation, in some cases of the *Tic douloureux*; a disease, in many respects, of the same nature with the present.

All these circumstances were explained to the patient, who, from a desire of obtaining relief, consented to have the nerve divided. This was done on the 1st of January, 1800, in the following manner: the nerve, as it passes from under the annular ligament, towards the thumb and fore-finger, was laid bare, for above an inch in length; it was then detached from its lateral connections, and, in this exposed state, a probe-pointed bistoury was passed behind it, and the nerve was raised upon the edge of the instrument, so as to be distinctly seen by the different medical gentlemen present, before it was cut through. As soon as it was divided, the two cut ends retracted from one another, to a considerable distance. This retraction was very unexpected, as the nerve was disengaged from the cellular membrane, and

no other part had been divided, whose action could make the portions of the nerve recede.

That nerves, when divided, do retract, is well known in the practice of surgery; but this effect has been usually attributed to the contraction of the neighbouring parts, as the cellular membrane and blood-vessels, with which the nerves are connected. As none of these causes could produce the effect in the present instance, it was natural to suppose, that an independant action existed in the nerve itself, which had been so much increased by the influence of disease, as to become unusually great; and, therefore, the retraction was more distinctly seen than in a healthy state of the body.

The moment the nerve was divided, there was a spasm over the whole body, and a momentary insensibility. The blood-vessels divided in the operation were not secured by ligature, but allowed to stop of themselves, to give the wound every chance of healing by the first intention. The edges of the skin were carefully brought together, and kept in that state by compress and bandage, to promote as much as possible the union.

For eight hours after the operation, the parts were perfectly quiet, and there was no spasm. The wound then began to feel hot, as if a red hot coal had been applied to it. To relieve this sensation, the outer bandage was loosened, and immediately there were twitches in the nerve, which soon went off. The patient felt himself generally unwell, extremely nervous, and irritable.

Fifteen hours after the operation, he had a violent spasm, which went along the arm to the head, but did not affect the brain. In an hour, there was a second attack, at which I was

present; the pulse was 105 in a minute, the tongue white, a great deal of general irritation, nervous twitches all over the body, but in the greatest degree in the arm and leg of that side. The stiff-fronted glove was now put on, to confine the thumb.

Twenty-four hours, or one day, after the operation, the first dressings were removed: the thumb was much swelled, and no union whatever had taken place; the spasms returned every five hours, but were less violent.

The second day, there was no abatement of the symptoms, but the spasms did not affect the brain; they were not now stopt by the pressure of the tourniquet, as they had been before the operation.

The third day, there were intervals of ten hours between the spasms; and, in the night, they did not extend beyond the elbow.

The fifth day, suppuration took place in the wound; the swelling in the hand was much abated; and the patient was able to dress and shave without spasm, having only twitches in the fingers, and tremors in the fore-arm.

The sixth day, there was a burning pain in the hand, and a numbed heavy feel in the thumb and fore-finger, similar to what the patient recollected to have felt four years before, when he hurt his thumb.

The seventh day, the patient awoke with great pain in the hand, succeeded by a violent spasm, which passed up to the head, although the tourniquet had been previously tightened: after this, he had no spasm for sixteen hours.

The eighth day, the hand was less swoln and less painful; and he had only two spasms in twenty-four hours.

The ninth day, the swelling had subsided, and the twitches

ceased; in thirty hours, there was only one slight spasm, which did not go beyond the wrist.

The sixteenth day, the wound was intirely healed; and, as there had been no return of spasms, the patient was considered as well.

On the twenty-fourth day, which was a fortnight after the spasms had ceased, at nine o'clock in the morning, he was awakened by a violent spasm, which passed directly up to the head, and affected the brain, producing insensibility; this was the only time the brain had been affected since the operation.

Two days previous to this attack, he had a violent diarrhoea; and, on the preceding day, had undergone unusual fatigue.

The tourniquet, which had been laid aside, was now applied; and, for the greater security, two were placed on the fore-arm, and one upon the arm itself. At six in the evening, there was another spasm, attended by insensibility, although the tourniquets had been tightened. The hand was found swelled, as well as the wrist, and the cicatrix formed a hard welt, tender to the touch. This hard state of the cicatrix, in which the end of the divided nerve was included, appeared to be a probable cause of the return of the spasmodic attacks.

The twenty-fifth day, the pulse was 100 in a minute; and, every two hours, there were slight spasms.

The twenty-sixth day, there were eleven spasms, at irregular intervals, in twenty-four hours, eight of which, went up as high as the head. As the spasms were not stopped by the tourniquet, as before, it was proposed to make the pressure directly upon the nerve: this was done by placing pieces of cork in the course of the nerve, and confining them there by the band of the tourniquet, so that, when the screw was tightened, the cork was



pressed down on the nerve. This pressure gave great pain, and, instead of arresting the progress of the spasms, seemed rather to increase their violence; it was therefore left off.

The twenty-seventh day, the pulse was only between 80 and 90 in a minute; there were seven spasms, all of which were arrested by the first or second tourniquet.

The spasms went on, with very little variation, till the 39th day at six o'clock in the morning, when he was seized in his sleep with a violent spasm, attended with insensibility, and convulsions over the whole body: these lasted for twenty minutes. After his recovery, the hand was found much swoln, and the welt formed by the cicatrix was painful. In the course of the forenoon he was well enough to bear going out in the carriage; the fresh air always proving very grateful to him.

From this time, the swelling of the hand and the hardness of the welt diminished; and the spasms were less violent, and seldomer. On the 45th day, there was only one slight spasm in twenty-six hours. In this state he went into the country; and, for the first fortnight, the spasms diminished, but afterwards became more violent.

The return of the spasms after the wound had been healed, made it evident, that the operation of dividing the nerve had not answered the purpose which was expected from it. The failure probably arose from the wound not healing by the first intention: the consequent inflammation rendered the cut end of the nerve uncommonly irritable; and, in this state, the confinement in the hard thickened cicatrix, rendered it liable to be stretched by every motion of the thumb, so as to bring on spasmodic contractions.

From this time, the patient was not under my direction; but

I understood, that he tried the effect of large doses of opium, which did not afford relief. He was then induced to employ electricity, which was also unsuccessful; and he died in a fit, which at the time was believed to be apoplexy, about five months after the operation had been performed; but, as the body was not examined, the nature of the fit could not be ascertained.

In this case, some of the branches of the median nerve had acquired, from disease, an unnatural power of contraction, which was made evident by the operation; and there is every reason to believe, that the spasmodic attacks which took place, were in reality convulsive motions in the nerves themselves, which excited corresponding contractions in those muscles that were under their influence.

This case naturally occupied my mind; and I could not avoid dwelling upon many of the extraordinary symptoms which made a part of it; but nothing so impressed itself upon me, as the retraction that took place in the cut ends of the nerve, at the time of the operation.

The first idea which suggested itself was, to endeavour to ascertain whether this retraction arose from an increase of a natural action in the nerve, or from one newly acquired, produced by disease.

With a view to ascertain this point, different experiments were instituted. The object of these was, to determine whether a similar contraction took place in nerves, when divided in a healthy state of the body; the extent of such contraction, if any occurred; and the circumstances by which it may be influenced.

For the first of these purposes, the following experiments were made.

*Exper. 1.* The cutaneus internus nerve of the fore leg of a young rabbit was laid bare, where it passes down before the biceps flexor cubiti muscle: the nerve was disengaged from its lateral attachments; and, while the limb was in a moderately extended state, a probe-pointed bistoury was passed behind it, by which means it was divided transversely. The two ends immediately receded from each other: the upper portion appeared to retract more than the other, and the end lay close to the muscle, in a straight line, while the end of the lower portion was a little bent to one side. The space between them, when measured by a pair of compasses, was found to be  $\frac{2}{8}$  of an inch.

The branch of the musculo-cutaneus nerve, which lies near to the cutaneus internus, was divided in the same manner; and the retraction of the cut ends was found to have been to the same extent.

In this experiment, the limb was extended, although by no means to its utmost limits; it therefore became a question, whether the same degree of retraction would take place in the bent state of the limb.

To determine this point, the experiment was repeated, after an interval of four days, upon the other fore leg of the same rabbit, with the limb in the bent state: the retraction, however, was found to have been exactly to the extent of  $\frac{2}{8}$  of an inch.

From this experiment, made under these different circumstances, a retraction of the cut ends of a divided nerve was ascertained to take place, which led to the further prosecution of the inquiry.

For this purpose, the phrenic nerve in the horse was selected, as being more favourable, in many respects, than most others

in the body, both from its superficial situation in the chest, and its great extent without giving off any branches.

In making experiments of this nature, it is an advantage that the animal should be of a large size; and the mode in which horses are killed in London, affords an opportunity of experiments being made on that animal, without giving the operator the painful sensations of having made any addition to its sufferings.

As horses are killed at stated times only, and these occur in a part of the day which is necessarily occupied by my professional engagements, the following experiments were made by Mr. CLIFT, the Conservator of the Hunterian Museum, whose accuracy may be relied upon, as well as his abilities in conducting them, having been early initiated, and long experienced, in inquiries of this nature.

*Exper. 2.* Immediately upon a horse having been knocked down, the thorax was laid open, and the phrenic nerve of the right side, passing round the pericardium, was exposed. It was nearly of the size of a crow-quill, and slightly connected with the pericardium. In this state, the point of one blade of a pair of scissars was passed under the nerve; and, by closing them, the nerve was transversely divided, without the smallest disturbance to its lateral connections. The two cut ends immediately retracted from each other, leaving the space of one inch between their extremities.

This experiment was repeated upon a second horse; and the retraction of the cut ends of the nerve was found to be exactly one inch.

It was repeated upon a third horse; and the retraction was found to be nearly two inches. In measuring the space between

the two ends of the nerve, the compasses accidentally touched the lower portion, and the diaphragm was immediately thrown into action.

The result of this experiment, not only confirmed the former, which had been made upon the rabbit, but it proved in the most satisfactory manner, that any action the nerves are capable of exciting, is nearly as strong after apparent death has taken place from a violence committed upon the brain, as while the animal is in perfect health.

Monsieur PORTAL, in a paper on a new mode of performing the operation of amputation, published in the Memoirs of the Academy of Sciences for the year 1773, mentions an experiment made on the sciatic nerve of a dog, in proof of nerves not having a power of retraction, at least none deserving of notice.\*

This experiment was repeated by Mr. CLIFT, on the sciatic nerve of a rabbit. Immediately on dividing the nerve, the cut ends receded from one another: but, that the result might be exactly ascertained, the rabbit was killed half an hour after the experiment was made; the parts were carefully dissected, and the space between the two cut ends measured; which was exactly  $\frac{6}{10}$  of an inch.

To ascertain whether this retraction was the consequence of a change taking place in the nerve itself, or arose from any other cause, the following experiment was made.

*Exper. 3.* As soon as a horse was knocked down, the chest was laid open, and the phrenic nerve of the right side was exposed: twelve inches in length were immediately measured by a pair

\* Memoire sur une nouvelle methode de pratiquer l'Amputation des Extrémités, par M. PORTAL. *Histoire de l'Academie des Sciences*, 1773. p. 542.

of compasses ; and the limits of this portion were marked by a small pin, passed transversely through the substance of the nerve. The part included between the two pins was then separated from the rest of the nerve, in the following manner. The person who was to divide the nerve had a pair of scissars in each hand ; and, having passed the point of one of the blades under the nerve, above the upper pin, and having done the same with a blade of the other pair of scissars, below the lower pin, the two pair of scissars were shut at the same moment, and the nerve at these two parts cut through.

This portion was again measured, and, instead of being twelve inches, was now only eleven and  $\frac{1}{8}$  ; so that the irritation produced by dividing it, had made it contract  $\frac{7}{8}$  of an inch.

This experiment was repeated upon several horses ; and in all of these repetitions there was a contraction produced : this varied in the different experiments, and in some of them was only  $\frac{3}{8}$ ths of an inch. When the nerve was divided very early after the animal had been knocked down, it was the greatest ; and, in proportion to the delay that took place, so was the diminution in the degree of the contraction.

In these experiments, the nerve, as well as the surrounding parts, was disturbed as little as possible, that the results might be the more readily and more accurately ascertained : this, however, makes them liable to an objection, which is, that the contraction might be produced by the cellular membrane surrounding the nerve ; an objection which certainly can have little weight in the peculiar situation of the phrenic nerve, as it lies between the pleura and pericardium, where the cellular membrane can have little influence over it, while the pericardium is left entire.

As, however, the opinion of the cellular membrane being the agent by which the retraction of divided nerves is produced, has been very generally received, it was highly proper to attend to that circumstance, and have the experiment made in such a way as to prevent any other surrounding part from acting upon the nerve; with this view, the following experiment was made.

*Exper. 4.* The pleura was removed from twelve inches of the phrenic nerve of a horse; and afterwards the attachments between the nerve and pericardium were completely divided: under these circumstances, this portion of nerve was separated, as in the last experiment. This portion was again measured, three hours after, in its detached state, and it was found to have lost  $\frac{6}{8}$ ths of an inch in length. The horse was twenty years old, and was killed on account of its age, which rendered it by no means a favourable subject for such an experiment.

With a view to determine whether the power of contraction in a nerve continued for any length of time after apparent death had taken place, and also to ascertain what proportion of elasticity a nerve possesses, (for every part of an animal body that is not rigid, appears to be endowed with it in a greater or less degree,) the following experiment was made.

*Exper. 5.* Eighteen inches in length of the phrenic nerve were measured, and separated by means of scissars: the contraction produced was only  $\frac{3}{8}$  of an inch; the experiment being made nearly an hour after the horse was knocked down. Upon being stretched with force, it elongated to  $18\frac{1}{2}$  inches; and, on being left to itself, retracted to  $17\frac{7}{8}$ . It was kept till next day, and again measured, when it was only  $17\frac{5}{8}$ : upon being stretched, it was elongated to  $18\frac{1}{2}$ ; but, immediately on being left to itself, it retracted to eighteen inches.

This experiment was repeated upon another horse; and the result was similar, both with respect to the contraction which took place after the nerve had been removed from the body, and the elongation which depended upon elasticity.

To ascertain if there was any difference in the appearance of a nerve when contracted, from one in a relaxed state, the following comparison was made.

*Exper. 6.* A portion of the phrenic nerve, about eight inches long, was removed immediately after the horse had been knocked down. This was allowed to contract; and, after it had remained quiet for twenty-four hours, its external surface was exposed by dissection, so that the appearance of its fibres could be distinctly seen. A portion of the same length was removed from another horse who died a natural death, and these were compared together.

The difference in the appearance of these two portions was very great: in the contracted nerve, the fibres were all serpentine; in the other, they were straight. The annexed plate, (see Plate I.) in which they are represented, shows very correctly, the great contrast which they exhibited.

The serpentine transverse lines described by MONRO, appear to be an effect of this contraction of the nerve; as they disappear when the nerve is relaxed or elongated.\* These serpentine lines in the phrenic nerve, in a man who died of a locked jaw, when examined twenty-four hours after death, were much more distinct and regular than in the phrenic nerve of a man who died of a mortification of his arm.

\* "When the nerve is fully relaxed, these serpentine transverse lines are best seen; when the nerve is moderately stretched, they are much less evident; when the nerve is greatly stretched, beyond what it ever is in a living sound animal, it appears



These experiments, upon so large an animal as the horse, made by a person well qualified for the purpose, and repeated sufficiently often to preclude any material fallacy, admit of the following conclusions being drawn from them.

1. That the nerves of an animal in health are capable of retracting themselves when divided; and that this effect is intirely independent of the parts by which they are surrounded.

2. That this contraction takes place in the nervous fibres themselves; and is independent of the brain, from which they originate, and of the muscles and other parts in which they terminate.

3. That the contracted nerve exhibits to the eye an appearance of contraction in its fibres, not to be seen when it is in a relaxed state.

As the nerves are so readily influenced by electricity, in exciting the muscles to action, it naturally suggested itself, that some further information might be obtained in the present investigation, by means of experiments made upon the nerves by the electric fluid. With this view, the following experiments were instituted; and Mr. CARPUE very obligingly assisted Mr. CLIFT in making them, and carried one of Mr. CUTHBERTSON'S large plate-glass electrical machines to the slaughter-house, for that purpose.

*Exper. 7.* A portion of the phrenic nerve, twelve inches long, was exposed, and divided at both ends, as in the former experiment uniform in its colour and consistence.—Hence these lines, are in the *first* place, to be considered as folds or joints in the nerve, and may be compared to the lines in the palm of the hand, serving to accommodate the nerve to the different states of flexion and extension.”—(In a note,) “By soaking in water, this appearance is lost.”  
MONRO on the Nervous System, p. 39.

riments. When it had contracted to  $11 \frac{1}{8}$ , a strong electric shock was passed along its substance, from one end to the other; but, when measured again, the length was exactly the same. The portion of nerve was then dissected out, and laid upon a piece of glass; in its detached state, it measured  $11 \frac{5}{8}$ . Several strong electric shocks were passed through it, in the direction of its fibres; but they did not produce the smallest effect upon it.

This experiment was repeated upon another horse, and the result was the same.

*Exper. 8.* Half an hour after a horse had been knocked down, 24 inches in length of the nerve called par vagum were laid bare, and a portion of it detached from its lateral connections, so that a piece of glass, 12 inches long, was admitted under it, without dividing the nerve from the trunk; in this state, electric sparks were drawn from it, and several strong electric shocks passed through it; but there was not the smallest change to be perceived, either in its length or appearance.

From these experiments it appeared, that when the nerve had contracted itself, in consequence of being divided, no increase of that contraction was produced by the electric fluid.

To ascertain whether electricity was capable of exciting contraction in a nerve that had not been previously irritated, the following experiment was made.

*Exper. 9.* Twelve inches of the phrenic nerve were measured; and the limits of that portion marked, by pins stuck through the nerve. This portion of nerve, in its relaxed undisturbed state, had electric shocks passed along its substance; but these were found, upon measuring the portion of nerve, to have produced no contraction in its length. When this portion was

separated, as in the former experiments, it contracted to  $11 \frac{3}{8}$  inches; a diminution of  $\frac{5}{8}$  of an inch.

The electric fluid, in this last experiment, excited the action of the diaphragm, but produced no evident or permanent contraction of the nerve; and, when the nature of the contraction of a nerve is considered, it is not to be expected that permanent contraction can be ascertained, in any other way than by separating intirely a portion of nerve from the rest of the system. For the action is continued in tremors along the nerve, in quick succession; and, when the muscle has been excited to contract, the complete action of the nerve is finished, and it immediately relaxes, or returns to that state which admits of a new action.

This appeared to be the case in several experiments made upon the nerves of frogs, and of quadrupeds of a higher order, by two different metals, as described by GALVANI. In all of them, there was a convulsion of the muscle, and a tremor in the nerve; but, such was the rapidity of the effect, that it could not be decided that any motion took place in the nerve, except what arose from the agitation produced by the action of the muscle.

The experiments and observations which have been related, appear to illustrate an action in the nervous chords, capable of producing the symptoms which occurred in the case related in the former part of this paper, and also those met with in many other diseases, the symptoms of which have never been satisfactorily explained.

The hypothesis of a nervous fluid, although it may explain every symptom which originates in the brain, and from thence pervades any part of the system, and every symptom which

begins in the extreme parts and goes to the brain, does not give a satisfactory solution of those nervous agitations brought upon an extreme part, which only proceed for some way in the course of a nerve, and are there arrested, without being allowed to proceed to the brain.

The circumstance of nerves having been divided, and their functions being restored twelve or twenty-four months after, when the two cut ends have been united by a new substance, is a strong argument against the circulation of a nervous fluid; since no such effect takes place in the pervious canals of the body.

In many diseases, there are symptoms so decidedly confined to the course of the nervous chords, that an impartial observer would be unable to account for them, in any other way than by supposing them to arise from some action in the nerves themselves.

This idea must have been strongly impressed upon the mind of Dr. MEAD, who, in treating of his third sort of Quinzy, says, all the nerves are convulsed, and the patient drops down dead suddenly.\*

The *Tic douleureux* is a remarkable instance of this kind, both in the circumstances under which the spasmodic tremors are brought on, and the manner in which they are propagated along the nerve.

In one case of this disease, in which the operation of dividing the nerve was performed, with a view to remove the complaint, union by the first intention did not take place; and, during the time the wound was open, the inflamed state of the cut end of the nerve, made the patient liable to several attacks of the disease, similar to those he experienced before the operation;

\* MEAD'S *Præcepta Medica*. Quarto, p. 434.

but there was no recurrence of them after the wound was completely healed.

This is a very important fact ; as it proves that inflammation on the cut end of a nerve, while in an irritable state, is capable of producing exactly the same symptoms as the original disease. This effect of inflammation upon the end of a nerve, explains the startings of the limb which occur too frequently after amputation.

These most commonly are met with when the limb is taken off above the knee, and the nerves and vessels have been previously inflamed higher than the part at which they were divided ; and where the nerve is confined by the thickened state of the surrounding parts.

The same fact also explains the cause of locked jaw, when it is produced by a wound or bruise upon a nerve, in a constitution either rendered irritable by climate, or naturally so ; also where the nerve itself becomes diseased, in consequence of the accident.

The following case of locked jaw, from an injury to the thumb, bears so great a resemblance to the case related in the beginning of this paper, as to show that the diseases must be nearly allied.

A lady of a very irritable habit was overturned in her carriage, and hurt her thumb, which swelled very much ; and the skin over the metacarpal bone of the fore-finger, about the size of a shilling, sloughed off. No symptoms came on for fourteen days after the accident, when, upon bending her fingers, violent spasms took place in the thumb, which proceeded up to the neck and lower jaw ; these were exceedingly painful, and the jaw was so much shut as hardly to admit a tea-spoon. In

fourteen days more, the jaw began to open; and, for a month longer, there were only two or three spasms daily in the thumb, attended with pain; these went up the arm to the jaw. At the end of that period, the sore on the back of the hand healed, and she recovered perfectly from the spasmodic affections.

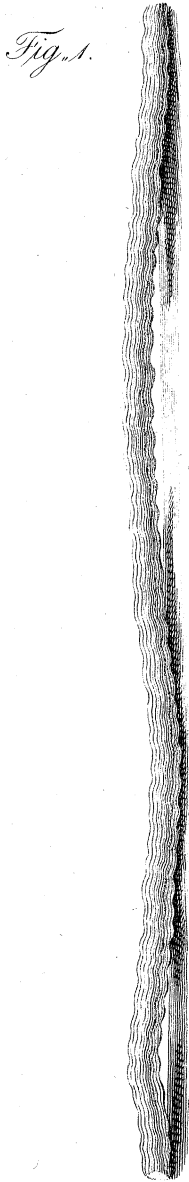
To enter further into the histories of cases which afford evidence of a morbid action in the nerves, would be trespassing too far upon this learned Society, and would render the present Paper an inquiry into medical facts, which is only intended to be an investigation of the natural actions of the nervous fibres, illustrated by the phænomena which occur while these chords are under the influence of disease.

EXPLANATION OF THE FIGURES. (See Plate I.)

Fig. 1. A portion of the phrenic nerve of the horse, as it appears when in a contracted state.

Fig. 2. The same, as it is seen when magnified.

Fig. 3. A portion of the phrenic nerve of the horse, as it appears when elongated, or in the state of complete relaxation.



*Fig. 2.*

