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ESSAYS

ON

MUSCULAR ACTION,

WRITTEN IN

1843 AND 1844,

BY

LOUIS MACKALL, M. D.

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ADVERTISEMENT.

The subject of muscular action is one of the first magnitude in the science of physiology, and, as such, has been laboriously investigated for many years past by the Scientists of Europe. It is at this time exciting among them more than usual interest.

Since the discovery of electricity—from observing the effect of this agent on the muscles of recently killed animals—the impression has very generally prevailed that this difficult subject was destined to be fully elucidated by means of electrical experiments. Accordingly, all the investigations referred to above have been directed to this end.

The anticipations resulting from this general impression have not been realized; nor is this surprising, when it is considered that it has been attempted to establish, or to reason out a law of nature that belongs to one department of science, by making use of instances or phenomena that belong to another and very distinct department. The law of muscular action is *a vital law*, its operation is confined to the living body, and is observable nowhere else in nature. Electrical phenomena belong to *Physics*, and perhaps to Chemistry, *but have no place in Physiology*.

For the last twenty-six years I have been engaged in investigating this subject of muscular action, but have reasoned solely from instances derived from vital phenomena, or such as are observable in the living body. The following is a succinct account of my proceedings in this investigation:

From often observing, in the practice of medicine, the inadequacy of remedial means made use of, and from frequently experiencing disappointment in my anticipation of the results of the operation of such means, I became convinced that there was something wrong—some great error in the theory or in the principles of medicine in which I had been taught.

This conviction was brought forcibly home to my mind by the death of my wife, from uterine hemorrhage, notwithstanding the use of all the remedial means known to myself and a very skilful medical friend who was present. Under the influence of grief, consequent on this bereavement, I resolved that I would devote the residue of my life to the task of endeavoring to discover, if possible, the error in the theory of medicine that I had before suspected, and in which suspicion I was confirmed by my late experience.

While studying this theory in the books, for the above purpose, a case of whitlow (paronychia) was presented for treatment. Prompted by the resolve mentioned, I carefully noted the prominent facts of the case. I particularly noticed the pulsation of the arteries at the diseased point, and at the wrist; and my attention was forcibly arrested by observing the remarkable difference in that pulsation at the two points; that in the finger was full and strong, while the pulsation of the artery at the wrist was comparatively calm.

Reflecting on the above fact, I arrived at the conclusion that there must be some agency in the arteries of the finger, that were throbbing so violently, to produce this result, that was independent of that in the general circulation; and, in casting about in my mind for some suggestion as to what that agency could be, it occurred to me that the throbbing or distension of the arteries was occasioned by the *action* of their muscular fibres. The correctness of this explanation of the phenomenon in question, was confirmed by running over in my mind, as I did at the moment, a number of instances wherein irritation was attended with the distension or dilatation of the tubes or hollow organs, when such organs were supplied with muscular fibres—as in the œsophagus, in the stomach, intestines, in the uterus in pregnancy, &c.

I had now arrived at a definite proposition; and in the year 1834, in November, I wrote down that proposition in the following words: "All the tubes of the animal body which are supplied with muscular fibres, have their calibres increased by the *action* of those fibres." This was shown and explained to four medical gentlemen at the time mentioned, and signed by them, witnessing that it was so shown. Three of these gentlemen, namely, Drs. J. H. Skinner, B. B. Hodges, and Wm. Ghiselin, are now (1860) living. One of them, the late Dr. Henry Brooke, died a few years since.

In 1836 I forwarded a paper, setting forth the above idea by an application of it to a number of vital phenomena, to the Professor of Anatomy in the University of Maryland, requesting him to advise me as to the best mode of bringing the subject to the notice of the medical profession. My communication was treated with contempt, as were also several papers written on the same subject and shown to members of the medical profession.

Although I was fully convinced, from the period mentioned above, that the calibres of the tubes and hollow organs were increased by the action of their muscular fibres, I did not fully comprehend how this occurred until the spring of 1842. At that time, being in conversation with a gentleman who was fond of gesticulating, he, in derision of something that was said, thrust out his tongue over his under lip. This sudden elongation of the tongue instantly suggested that which I had been in search of for eight years—a rational explanation of the action of the fibres about the tubes. The truth flashed on my mind, *that the fibres of muscles are actively elongated by innervation.*"

In 1843, February 24th, I wrote the following "Essay on Muscular Action," embracing the idea just mentioned—applying it to the action of all the muscles in the animal body, and showing its application by a number of experiments on living domestic animals and on my own person. This essay is certified by me, under oath before a magistrate, as having been written at the time at which it bears date.

In 1844 I wrote another essay on the same subject, and illustrated my views by a reference to a large number of instances taken from Natural History. This essay is also included in this pamphlet.

In 1848 I published a small pamphlet, entitled "Outlines of a New System of Physiology," and at the same time a circular, to be enclosed with it, referring to instances in Natural History in corroboration of the correctness of my views of muscular action. This pamphlet and circular were gratuitously distributed the same year to all the most distinguished physiologists and physicians in Europe and in this country of whom I had any account.

In 1850, having carefully reflected on and retraced the mental process by which I had been enabled to arrive at and to apprehend the law of muscular action, I wrote and published an essay, entitled "An Account of the Reasoning Process," and instanced this process in the discovery of this law.

In 1852 was published my "Notes on Carpenter's Human Physiology;" and,

In 1857, the first forty pages of "Principles of General Physiology"—merely as a record of conclusions at which I had arrived, but which I had not had time to arrange in any order.

In 1859, in May or June, my Essay on the Law of Muscular Action was printed.

Most of the above publications have been distributed freely and gratuitously, wherever it was thought probable they might awaken an interest.

LOUIS MACKALL, M. D.

Georgetown Heights,
January, 1860.

AN ESSAY ON MUSCULAR ACTION.

"Non alius fere sit aditus ad regnum hominum ut fundatur in scientiis, quam ad regnum cœlorum; 'in quod, nisi sub persona infantis, intrare non datur.'"

It is assumed by physiologists that there is a certain subtle fluid, termed the nervous fluid, transmitted through the nerves to the muscles. This transmission of the nervous fluid to the muscular fibre is termed innervation.

It has always been thought, and is thought at the present time, by physiologists, that the effect of innervation is *contraction* of the muscular fibre. This is not considered an abstract proposition, laid upon the shelf as one of those the truth or the fallacy of which is immaterial. No; it is taken as one of the fundamental principles—one of the main axioms of the Science of physiology. In explaining locomotion, the circulation of the blood, digestion; in short, in explaining all the functions of animal life, there is a constant recurrence to this principle; and in any process of reasoning on physiological subjects, the mind must continually recur to this axiom. The proposition, that innervation causes muscular contraction, bears the same relation to the science of physiology, as the proposition, that "things which are equal to the same are equal to each other," bears to the science of geometry.

Having studiously examined and closely observed the phenomena of life connected with this subject, and having made some careful experiments on the same, I am prepared to assert that this axiom, adopted by physiologists, *is false—totally false.* I declare boldly, that innervation does not cause muscular contraction, but a state diametically the opposite of contraction—*elongation.*

We will take an instance—the most familiar—the tongue. For the sake of elucidation, we will consider the tongue as one muscle. Here, then, is the point at issue. We can elongate or shorten the tongue at will. Physiologists assert, that by determining a flow of nervous fluid to this organ or muscle, we cause its contraction, and by withdrawing or diverting elsewhere the nervous fluid, we cause its relaxation or elongation. I maintain that, by determining nervous fluid to this muscle, we cause its elongation; and that elongation is exactly in proportion to the power we possess of innervating this organ. The contraction of the tongue is caused, I contend, simply by a withdrawal of the nervous fluid. Man possesses the faculty of elongating or of shortening his tongue to a limited extent, but this faculty is possessed to a remarkable degree by some of the lower orders of animals, as lizards, serpents, &c. It may be suggested that the elongation or protrusion of the tongue may be attributed to the contraction of the muscles attached to it, but I confidently defy physiologists to show any muscles, the contraction of which could produce the state of the tongue referred to above, and particularly that observed in the lower animals.

I have said that the opinion that innervation causes muscular contraction was erroneous, and to show that it is, let us attend more particularly to the phenomenon alluded to above. A rattlesnake* is confined in a cage; we scrape harshly on the side of the cage; it becomes violently excited; it shakes its rattles, erects its head, and protrudes its tongue to the extent of several inches. The tongue of this animal, as that of every other animal, is muscular. It is evidently in a state of action. The cause of this action must be derived from its nerves, for there is no way of operating on the muscles except through the medium of the nerves. The operation of the nerves in the instance before us could not be that of withdrawing the nervous fluid from this muscle or organ. It would be absurd to say that the state of this organ which we are contemplating is that of relaxation. The only mode, then, in which the nerves could operate on the organ in question, is that of innervation.

Innervation, then, causes elongation in this instance, and I proceed to show that it produces the same effect in other instances. We will take the ciliary processes. This is one of those muscles which, like the tongue, is attached by one of its extremities, whilst the other extremity is free. Let us observe the muscle in the eye of this horse. He is withdrawn from the light into the stable; the ciliary processes are contracted; the pupil is enlarged. He is led slowly to the door of the stable; as he approaches the light, the ciliary processes are elongated; and now that he is in the full light of day, they are so much elongated as to have reduced the pupil to a small opening. The horse is again withdrawn slowly from the light and returned to his stall. As he recedes from the light, the ciliary processes become gradually shortened, until the pupil is enlarged to the

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* The instance here alluded to had been observed a day or two before writing this essay at the U. S. Patent Office; where, among other subjects of natural history, a rattlesnake confined in a wire cage, was exhibited, and where I made the experiment referred to.

size it was when we first noticed it. In this instance, again, the muscle is in a state of action or of innervation from the exciting cause—light—and the effect is *elongation*, and not contraction. I might adduce another familiar instance* of the same character as those mentioned, which would furnish arguments still more conclusive on this point, but I forbear. What has been written is sufficient to explain the position I have taken, and I make no doubt that, as we proceed, the truth of that position will be made apparent.

Having noticed the physiological action of some of those muscles having one of their extremities attached, we will proceed to notice briefly the action of some having both extremities attached. We will take the flexors and extensors of the fingers, and, to render the subject more simple, and to make our remarks more easily understood, we will assume that all the flexors of the fingers are one muscle, and all the extensors of the same, one. I grasp in my hand this rule lying on my table—the flexor muscle is contracted, that portion of the muscular fibres near the wrist, which, before the contraction, gave a rotundity and fulness to that part of my arm, is so contracted as to cause a hollowness there. Physiologists assert that this state of the muscle is caused by innervation; that is, by a determination of nervous fluid to the muscle. It was probably in observing this very phenomenon we are now contemplating, that the false inference was drawn, that innervation causes muscular contraction. There is no evidence before us that there is an *innervation* of this flexor muscle. We have no reason for such a supposition. It is true, my attention is turned to this muscle while in a state of contraction, and there is an effort to continue or increase the contraction; but may not that effort consist in *withdrawing* nervous fluid from this muscle instead of determining to it? I grasp the rule still more firmly. What do I observe? The flexor muscle is, indeed, still more contracted, but the extensor muscle is considerably developed. The portion of this muscle, which, before this effort of contracting the fingers, was not observable, is now palpably enlarged, and that, too, in proportion as I increase my present effort. Having shown in the instances before enumerated, that innervation causes elongation of the muscular fibre, I infer from this fact that the extensor muscle is innervated in the act of grasping, and that the nervous fluid is withdrawn from the flexor muscle during that operation. Again, my fingers are moderately flexed, and the back of them placed against a resisting body—the table. An effort is made to extend or straighten them. The extensor

* My allusion here was to the male organ of generation.

muscle is contracted, and evidently diminished in size, but the flexor muscle is becoming larger and more developed. If the effort is increased, instead of the hollowness remarked in the former experiment, there is considerable fulness and enlargement of the muscle near the wrist. From this experiment I draw the same inference as from the former, viz: that the nervous fluid is withdrawn from the contracted muscles, and determined to the muscle which was elongated or enlarged. Another inference from these experiments is, that where there are antagonizing muscles, motion is not the result of the action of either set of muscles exclusively, but the result of the motion of both sets. Thus, in grasping the rule, the action of the extensor muscle contributed to that act as well as the contraction of the flexor. Another inference may be, that the tendons are not, as laid down in all our works of physiology, mere ropes or pulleys, but are parts of muscles as important as the fleshy portion; that they are supplied with nerves, and are acted upon by the nervous fluid in the same manner as the fleshy parts of the muscles are. Thus, that the flexor muscle has its tendon shortened as much as its fleshy portion, and the extensor has its tendon elongated in the first of the above experiments, &c.

Let us dwell awhile on the phenomena before us. I have said that it was from probably observing the contraction of muscles, that the erroneous conclusion was adopted that *contraction* was caused by *innervation*. I say further, that from observing muscular contraction, physiologists draw this still more absurd inference, that muscular action and muscular contraction were one and the same thing, and the terms used to express those states, are synonymous and convertible terms. Whereas, those two states of a muscle, so far from being identical, are *opposite*.

Why is it that physiologists have not endowed animals with the power of producing muscular contraction by *withdrawing* nervous fluid from the muscle, when that muscle notoriously has its size diminished, or occupies a less space, in consequence of being contracted? Is it not more reasonable to suppose, that a body could be lessened by taking somewhat from it than by adding to it?

I have attempted to show, in a very brief and cursory manner, that the physiological axiom, *that innervation causes muscular contraction*, is false, when applied to the explanation of the action of two classes of muscles; and it remains for me to show that this axiom cannot be admitted in explaining the action of the muscles constituting the tubes or hollow organs. This principle is not only tacitly allowed by physiologists in explaining the functions of the hollow organs, but, at the very outset of such explanations, we are gravely informed that *the tendency of the* *action of the muscular fibres placed around such organs is, to obliterate their cavity.* But the truth is, that the tendency of such action *is exactly the reverse*, namely, *to enlarge the cavities of the tubes.* I repeat, that in every instance that has come under my observation, where there has been innervation or determination of nervous fluid to a living animal tube, it has caused *a dilatation or enlargement* of its cavity, and not an obliteration. It appears strange that the ancient physiologists should have thought the arteries did not contain blood, but only animal spirits, when they must have had so many opportunities of observing the arteries pouring out blood. But it is passing strange that physiologists even of the present day should shut their eyes against so glaring a fact as that we are now treating of.

I can account for so strange an oversight only by supposing that the axiom, that innervation causes muscular contraction, had been adopted before the muscular fibres about the tubes were observed or demonstrated. Some of those fibres are semi-circular and some longitudinal. If we admit that innervation contracts those fibres, we must admit the conclusion that innervation obliterates the cavity of tubes. But I assert that this conclusion is utterly false, and I am borne out in this assertion by the phenomena constantly before our eyes. If the conclusion is false, as I shall presently proceed to show, then the premise, the above axiom, must be false. We will take an instance of a muscular tube, the œsophagus. As this tube is very muscular and highly sensitive, it will be easy to cause innervation, by irritating it. I expose the œsophagus of this living rabbit by carefully dissecting the integuments so as not to injure the nerves. I introduce a feather into its mouth, and gently irritate the upper portion of the œsophagus. What do I observe? There is suddenly a dilatation of the part of the tube irritated—not a *contraction*—but a *palpable dilatation.* This dilatation is confined to a small portion of the tube at a time, say to the extent of four lines. After having taken place in one portion, it ceases there, and is transferred to the adjoining portion of the tube of the same extent, and thus proceeds alternately and regularly from one end of the tube to the other. This operation is repeated as often as I cause an act of deglutition by irritating the fauces.

We will make the experiment on another animal, in which innervation may be more readily effected. Let us take the game cock. We expose the œsophagus, as in the case of the rabbit. I make the same experiment with the same result. I have a solution of tartarized antimony by me, into which I dip the end of the feather, and again apply it to the fauces. The dilatation is greater than before, and passes from one extremity of the tube to the other. Since using the tartarized antimony, the whole tube is sensibly enlarged. Now, the dilatation commences at the other extremity of the œsophagus, that next the crop, and proceeds towards the fauces—the same action as before, but only its course reversed. It might be imagined that this dilatation was caused by something contained; but it was evident that the tube contained nothing. However, to make the thing certain, I pass two ligatures under the œsophagus at such distance from each other as just to include the portion which is dilated. A single tie is made on the ligatures, but they are left loose. An act of deglutition is caused. When the dilatation reaches the space between the two ligatures, these are suddenly tightened; the dilatation of the portion included by the ligatures ceases as before, and nothing is found in the tube. A grain of corn is introduced into the upper portion of the œsophagus—that tube is momentarily dilated—it then begins to contract; as the first portion of the tube contracts, the adjoining portion dilates, and when sufficiently dilated, the grain of corn passes into it. This operation is repeated until the grain has passed the whole length of the tube. I press the use of the tartarized antimony, and now the contents of the crop passes into the œsophagus and is ejected by the mouth—vomiting is produced.

I observe that in the passage of any body through this tube there never appears to be any pressure of its walls on the body that is passing; but the motion of the body passing, appears to be the effect of the dilatation as well as of the subsequent contraction of the tube. When contraction takes place after every act of dilatation, it is carried, momentarily, beyond the natural state, or its state of rest, and then immediately returns to that state.

The fallacy of the principle I have been combating, is, I conceive, clearly shown in the phenomena observed in the above experiment. Irritation or innervation does not cause contraction or an obliteration of the cavity of the œsophagus but manifestly an opposite state, dilatation. Nor is this a solitary instance. Nature is uniform in her operations. When the stomach is irritated or innervated by taking stimulating articles of food, &c., is there not a sensation of distension? When the intestines are innervated, is there not the same sensation? The uterus is innervated in pregnancy, and in consequence of that innervation, is dilated or enlarged. In connection with the latter remark I will mention a case: Shortly after adopting my present view of muscular action, I was called to visit a woman in labour. She was of about the middle age, was healthy and robust; there was considerable general excitement of the system, but the pulse not remarkably full. She had been in labour twenty-four hours or more. At first, parturient efforts were tolerably strong, but for twelve hours immediately preceding my visit the pains had subsided; and when I saw her there appeared to be no disposition in the uterus to contract. I determined to try if I could not cause uterine contraction by diminishing the general excitement and by lessening the powers of life. For this purpose a bowl of pounded ice was procured, and the patient made to swallow the ice freely; in a minute or two, or as soon as the system was brought under the influence of ice, the uterus contracted forcibly and the fetus was expelled. This occurred, I find by reference to my books, on the 26th day of May, in the year eighteen hundred and thirty-five.

Soon after this occurrence I relinquished the practice of medicine, in which I had been engaged in Maryland, but mentioned the circumstance to several of my medical acquaintances in the vicinity. I have been since informed by them that the practice has been very generally adopted in that neighborhood, and with great success.* I have in one case, recently, in Georgetown, very similar to the one mentioned above, advised the use of this agent, and with the same successful result.

But to return to our experiment. What is termed peristaltic action is here, I think, fairly explained. The first stage in that action is, as we have seen, dilatation; and the second stage, that of contraction, is merely the consequence of the expenditure of innervation in the first stage. What has been observed in the action of the œsophagus may be observed in all other animal tubes, differing only in the extent of their action; thus the stomach has an extent of action greater than the œsophagus; the intestines greater than the stomach; the arteries greater than the intestines; for the action of all the arteries in the animal body is synchronous, and alternates with that of the heart.

But let us apply the principle in question to the action of bloodvessels. I strip up my sleeve, and apply a small sinapism on the forepart of the wrist—irritation and consequently innervation is caused. The bloodvessels are not *contracted*, but, on the contrary, are *enlarged*, and the part appears red from the enlargement of those vessels. They evidently contain more blood than before the experiment. After what has been written, is it not more satisfactory to attribute this increased quantity of blood to the dilatation of those vessels caused by innervation, than to attribute this dilatation to the increased flow of blood to the part? The importance of the subject we are treating is so great that I shall be excused for introducing the following instance to show the fallacy of the principle I have been combating, by showing the extremly ridiculous conclusion to which it must inevitably lead. I refer to the condition of the organs of reproduction

* See letters from some of these gentlemen at the end of my little pamphlet entitled "Outlines of a New System of Physiology," published in 1848.

in the act of copulation. The male organ, the penis, is clearly muscular; the female organ, the vagina, is surrounded by muscular fibres. In the venereal orgasm these organs are highly innervated. If it is true that innervation causes muscular contraction, and that the tendency of the action of the muscular fibres about tubes is to an obliteration of their cavity, then, in this condition of things, * * * * * *
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LOUIS MACKALL, M. D.

Georgetown Heights,
February 24, 1843.

AN ESSAY ON MUSCULAR ACTION.

It is assumed by physiologists that the transmission of the vital fluid to the muscular fibre, causes the globules of which that is composed to approach each other—causes the muscular fibre to be shortened or contracted.

The very basis of physiological science, as it now exists, is founded on the assumption that innervation causes contraction of the muscles; and, in all our systems of physiology, muscular action and muscular contraction are considered synonymous and convertible terms.

The view I take of muscular action is, that the transmission or accession of the vital fluid separates the globules of which the single muscular fibre is composed, causing the extension or elongation of the fibre.* The general law of the animal economy to which I wish to call the attention of the reader, is that innervation causes extension of the muscles.

I will not now take the reader through the circuitous and devious route by which I arrived at this conclusion, but will apply the general law which I have proposed, in explaining a few phenomena mentioned in natural history; and thus the best proof that could be given of the truth of my position will be found in the facility and readiness with which it explains those phenomena. We will take, as the first instance, a phenomenon observed in the snail, (*Helix Nemoralis.*) The eyes of this singular animal, we are told by naturalists, is situated at the extremity of two muscular tubes projecting from the head, to the length of nearly an inch.† The muscular fibres surrounding these tubes are placed circularly. There are also distinct muscles arising from the lower part of its abdomen, traversing the body of the animal and these muscular tubes, and are inserted around the eyes at the extremity of the tubes.‡ The large optic nerves, enclosed in their own sheaths, accompany these muscles through the tubes. In a state of repose,

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* As the subtle fluid, heat, separates the particles of bodies, and causes expansion, so the vital fluid separates the globules composing the fibres, and causes extension of the muscles; and as the abstraction of heat causes the contraction of bodies, so the abstraction of the vital fluid causes the contraction of muscles. The means by which that abstraction is effected I propose to show hereafter.

† Jones' Animal Kingdom, fig. 195. ‡ Fig. 184, § 430.

the tubes are inverted, and the eyes are drawn within the body; but when the animal is roused by the want of food or other cause, it protrudes these tubes so as to have the benefit of its eyes. If the reader understands the anatomy of the eye of the snail, which I have attempted to describe, he will understand at once, how this animal could, by an act of volition, determine the vital fluid to the longitudinal muscle I have mentioned—cause its extension, and thus cause the protrusion of the eye. A like determination of the vital fluid to the circular muscle constituting the tube, would enable it to act in concert with the former muscle to moderately enlarge the tube, and thus assist in protruding the eye. Thus, in the instance before us, the application of the general law I propose, enables us clearly to understand the appearance presented. But let us advert to the explanation of this phenomenon given by physiologists—and it is the only explanation that can be given if we assume that innervation causes muscular contraction. They tell us that portion of the circular muscle or tube which happens to be immediately behind the position of the eye, begins to contract, *and by continuing this action alternately to the extremity of the tube, overcomes the contraction of the longitudinal muscle, and protrudes the eye!* Does not the reader perceive that if the eye of this animal were drawn within its body, and beyond the range of the tube, as it undoubtedly is very frequently, that the contraction of the tube at any point would only tend to prevent the protrusion of the eye. Again, if the eye could be protruded by the means referred to, would not a contraction of the circular muscle, carried to such an extent as to overcome the contraction of the longitudinal muscle, be sufficient to interrupt seriously, if not altogether, the function of sight, by compressing the optic nerve which lies by the side of this muscle?

But let us take another instance from natural history, somewhat similar to the above, in which the explanation given by physiologists is still more unsatisfactory, and in which the assumption that innervation causes muscular contraction will appear still more absurd. Let us direct our attention to the tongue of the ant-eater, (*Mynmeco-phaga.*)

Naturalists inform us that the tongue of this animal is formed of two muscles: one longitudinal, arising from the sternum or breast bone, and passing among the muscles of the neck, is inserted in the end of the tongue; the other arises from the os hyoides, and is placed spirally around the former.* The length of the longitudinal muscle, in an animal of middle size, would be about seven inches, in a state of repose; the length of this mus-

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*Op. Cit., §751.

cle, in a similar state, from the os hyoides to the extremity, would be between four and five inches. It is stated in natural history that this animal can protrude its tongue to the extent of *seventeen or eighteen inches;** and we are gravely told by physiologists that the protrusion of the tongue of the ant-eater is effected *by the contraction of its spiral muscle!* The absurdity of such a supposition is monstrous. As there is no possible way of accounting for the protrusion of the tongue in this animal by the *contraction* of any muscles attached to it, we are forced to admit the truth of my position, that innervation causes *extension* and not contraction of the muscles.

I will refer to another instance out of many in which the same protrusion or elongation of the muscle is seen, but in which there is no spiral muscle by means of which that elongation could be effected—I mean the tongue of the frog. The tongue in this animal is principally composed of longitudinal muscular fibres which arise from the anterior portion of the lower maœilla.† The frog can protrude its tongue to the extent of several inches, seize a fly or other insect, and draw it into its mouth. This act it performs with wonderful celerity—with the speed of lightning. There is no way of explaining this phenomenon but by means of the general law that innervation causes extension of muscles.

But in order to illustrate this law, I need not confine myself to instances taken fron natural history. I will take them nearer home. If the reader will protrude his own tongue to some extent, and continue it in that position for a moment, he may observe that the protrusion is mainly effected by extending or elongating the longitudinal fibres of his tongue; and that he is actually determining to that organ a flow of vital fluid. Again, let the reader take a small mirror in his hand, and, standing in a part of the room where the light is partially excluded, let him observe the muscular fibres of his own iris placed around the pupil of his eye, then let him approach the light, and he will perceive that the muscular fibres of the iris are extended or elongated in proportion as the stimulus or cause of motion in the vital fluid is increased—light being the appropriate stimulus of this organ.

I request the reader's attention to another familiar phenomenon. I wish to show that the law I have been endeavoring to establish is not the only important object that has escaped the observation of physiologists. Let the reader place his arm on the table before him; let him strip up his sleeve, so that he may observe the action of the muscles of the forearm. In a state of

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*Cuvier's Animal Kingdom, edited by Griffith, vol. 3, p. 300.
†Op. Cit., Jones' Animal Kingdom, §611 and 612.

repose the fingers are slightly flexed; but let the reader shut his hand tightly—the muscles on the front of the forearm and in the palm of the hand are contracted; the fleshy fibres have their extremities drawn nearer together; the upper portion or belly, as it is called, of those muscles is swollen or thickened. Physiologists have inferred—from the appearance of the muscles of the forearm and the palm of the hand, in the action we are now contemplating, and especially from their appearing enlarged or swollen—that those muscles were in a state of *action;* and being entirely convinced of the correctness of this conclusion, they thought it useless to look any further. I now assure the reader that that conclusion was altogether erroneous—that it was a gross fallacy. Let us look a little further. What is the condition of those muscles called the *extensors*, during this act of shutting the hand? These muscles are elongated—their fleshy fibres are extended; and but a little attention will convince the reader that in the act before us the muscles, now regarded as the *extensors*, are in a state of action; while those muscles, now regarded as the *flexor* muscles, are in a contrary state, or in a state of inaction—the shortening and consequent thickening or swelling of their fleshy portions being the effect of an abstraction of the vital fluid. Again, in the act of opening the hand, the muscles now termed flexors are in a state of action, and those termed extensors are in a state of inaction; so that physiologists have made a grand mistake in their nomenclature. The view I have advanced of muscular action is confirmed by the fact that, when an individual has the tendon of the gastrocnemius muscle ruptured, he is unable to raise his toes.*

In connection with this subject, it may be observed that the tendons should not be regarded as mere ropes or pullies, by means of which the muscles are inserted in and move the bones. But, as recent anatomists have considered a muscle and its tendon as constituting one organ, so they should be regarded as being influenced by the same cause of action. Thus, the tendinous portion of a muscle is elongated equally with the fleshy portion, when that organ is in a state of action; and shortened, when in a contrary state. Ligaments, too, may be elongated or shortened by the same cause.

I need not remark that the view I have given of the action of the muscles employed in opening and shutting the hand will apply equally well to the action of all the muscles in the animal system. Thus, in locomotion—walking, running, leaping—the *extensor* muscles are in a state of *action*, &c.

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* This fact is stated by Mr. JOHN HUNTER, who suffered from a fracture of the tendo Achillis.

But the most important application, in a physiological point of view, is to the muscular fibres placed around the tubes or hollow organs. Some of the fibres placed around those organs are semi-circular, some spiral, and others longitudinal. If it be assumed that innervation contracts those fibres, it must be admitted that innervation causes the diminution of the calibre of those organs—causes their systole; and this conclusion is actually adopted by physiologists, and acted on by physicians. I assert here that this conclusion is utterly erroneous, and contradicted by all the phenomena of animal life in which such law is involved. The truth is, as I shall proceed to show, that innervation causes dilatation of the tubes or hollow organs. It was in observing the uniformity of this fact in nature that my attention was first drawn to our present subject. I observed many instances of the fact in question, and to convince myself of its reality, made many experiments. (My first step was the discovery that this dilatation was effected by means of the muscular fibres. This fact was committed to writing by me, and attested by a number of the most respectable physicians of my acquaintance in the year eighteen hundred and thirty-four, 1834.)

But to proceed. I will here state only one of the experiments, out of many, as this one is perfectly conclusive. A barn-yard fowl was secured on his back to a board by ligatures passed through holes bored in the board; his neck was extended and secured in the same way, by a ligature passed through the crest or comb. An incision was made through the integuments on the side of the neck, from the pharynx to the crop; the œsophagus was thus fairly brought into view. A feather was introduced into the mouth of the animal, and the fauces irritated, so as to cause an act of deglutition; the pharynx was instantly dilated, and the dilatation passed deliberately and regularly through the whole course of the œsophagus; the portion of the œsophagus dilated at any time did not exceed probably the length of three lines, or about a quarter of an inch, but this dilatation proceeded alternately from one portion of the tube to another, throughout its whole course, beginning at the pharynx and terminating at the crop; the dilatation was very palpable, being of twice the diameter of that part of the tube which was in a state of repose. To convince myself that the dilatation was not caused by any substance within the œsophagus, I passed two ligatures under this tube, about the middle, at such a distance from each other as would include the dilated portion of the tube; the ligatures were tied by a single turn, but left perfectly loose. I held the ends of the ligatures, and made an assistant cause an act of deglutition in the animal; when the dilated portion reached the space included between the two ligatures, I suddenly drew them tight—the dilatation between the ligatures ceased, and that part of the tube returned to its normal state or state of repose, showing that there was no substance contained within it that could cause its dilatation. I then dipped the feather in a strong solution of tartarized antimony, and applied it to the fauces—the dilatations were greater than before. After continuing this irritant for some little time, the whole tube was somewhat enlarged, and then the action of the tube was reversed; the dilatation commenced at the extremity of the tube next the crop, and proceeded to the pharynx. Finally, some of the contents of the crop were ejected through the mouth.

I observed in this experiment that when the dilatation passed away from a portion of the tube, that that portion of the tube was immediately contracted to a smaller diameter than before its dilatation, and then instantly returned to its normal size. This appearance seems to have monopolized the attention of experimentalists; but I conceive this contraction is owing to the circumstance that the vital fluid had been expended in this portion of the tube by the previous act of dilatation. The above experiment has been repeated often by me, and with the same results.

If the reader will examine carefully the above experiment, or will make others on this subject, he will be convinced with me that innervation causes *dilatation* of the tubes; and consequently that the general law is true which I have been endeavoring to establish, namely, *that innervation causes the extension of the muscles.*

This truth is still more apparent when the law is applied to the explanation of the functions of animal organs.

1st. Of the nervous function. We will assume that the brain and nerves are composed of tubes through which the vital fluid circulates. This circulation, I believe, is very similar to the circulation of the blood. The nerves have been divided into those of motion and those of sensation. The nerves of motion correspond with the arteries; the nerves of sensation with the veins. The brain and spinal cord, or the cerebro-spinal axis, correspond with the heart. This heart of the nervous system, in a state of action, dilates; in its opposite condition or state, it is contracted. When it dilates, it draws the vital fluid from the nerves of sensation and of motion; when it is contracted, that fluid is imparted to the nerves of motion. Thus the circle or circulation of the vital fluid is made.

Before I proceed with this point, I will endeavor to give some general notion of what I mean by the vital fluid. I do not know that I can do this better than by comparing it to the fluid called heat. I propose to call this fluid Life. It would serve to give a definite meaning to a word which is in everybody's mouth—whose meaning now is extremely vague and indefinite. This fluid, Life, then, is the most important substance in the universe. It is possessed of ubiquity; it enters as the principal ingredient into every object in creation. Take away this fluid, Life, from any object, and it immediately ceases to exist. Like heat, too, it may be considered as existing in bodies in different states. Bodies may be possessed of fixed life, or life of composition, and free life, or that which they are continually receiving from and imparting to the bodies around them.

Every object in creation, animate and inanimate, is continually receiving or parting with a subtle fluid, Life, and—to proceed with the nervous function, this fluid is absorbed by the nerves of sensation of animals.* Animals receive this fluid, Life, through the medium of the nerves of the senses—those of respiration, digestion, &c.

As there appears a design in nature that there should be a relation between the reception and expenditure of this vital fluid, animals expend it in muscular action, in extension of the long muscles, those of locomotion, &c., in dilating the tubes, but principally in the secretions. This last mentioned mode in which the vital fluid of animals is expended, is very apparent and very important, in a practical point of view, though physicians seem not to have regarded it. The secretions of animals contain an excess of this vital fluid; and the rapidity with which the vital fluid, or what is called the strength of an animal, is reduced by increasing the secretions, is very striking. The sounds caused by animals, the human voice, the singing of birds, the cries of other animals, the humming of insects, &c., seems to me the means provided by nature for the expenditure of the vital fluid.

Having thus given a very rough outline of my views of the cause of muscular action, and of muscular action itself, we will proceed to apply the general law I have laid down to some particular cases. Not to dwell on the instances in the lower classes of the animal kingdom, wherein elongation of the muscles is the result of acts of volition, we will only enumerate a few of them, and pass on to the higher classes of animals, in which the muscular and nervous systems are more fully developed.

In Actiniæ† the tentacula having longitudinal muscular fibres can be elongated or contracted according to the will of the animal. In the act of feeding, the tentacula are protruded or elongated; in a state of repose, those organs are retracted.

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* I meant the afferent nerves. † Jones' Animal Kingdom, fig. 11, § 54.

The Physalus* is said to possess the power of elongating its tentacula to the extent of a fathom or more, when it wishes to attach itself to some object at the bottom of the water on which it is floating.

The Beroe† extends its tentacula in a remarkable manner in the act of feeding, and retracts them within its body in a state of repose.‡

Leaving the first division of the animal kingdom, (the acrita,) and entering upon the second division, (the nematoneura,) we will take the Stephanoceros Eichornii.§ This animal is enclosed in a horny case or tube attached at its base to some submarine object, and having its upper extremity open, through which the animal can, at will, protrude not only its tentacula, but its body to a certain extent. A muscle, composed of longitudinal fibres, arises from the bottom of its horny case, and is inserted into the membrane surrounding the body of the animal. The protrusion of the body is effected by the *elongation* of this muscle.

I refer the reader, for other instances in this division of the animal kingdom, to a paper on some species of bryozoa, published by Dr. Arthur Farre in the Philosophical Transactions for the year 1837. In this paper we have a clear and beautiful description of the phenomena presented by those animals, and also of their muscles. Application of the general law which I have proposed would furnish an easy and satisfactory explanation of those phenomena.

In Echini and Asteriæ|| the *extension* of the longitudinal muscular fibres attached to their pedicles or suckers would explain the protrusion of those organs more readily than the mode now resorted to for that purpose.

In the third division of the animal kingdom we will take as an instance the Leech. This animal** has a strong muscle composed of longitudinal fibres extending through the whole length of its body. It fixes itself by means of a sucking disc placed at the posterior extremity, and elongates its body to a considerable extent; that this elongation is not caused by the contraction of any circular fibres surrounding its body will be very manifest to any one who will closely observe the phenomenon in question. The elongation of the body of the leech must be attributed to the extension of its longitudinal muscle.

The Serpula Contuplicata†† has a disc attached to longitudinal muscular fibres, with which it closes its external covering; when

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* Jones' Animal Kingdom, Fig. 24, § 96.
† Fig. 22, § 95.
‡ Fig. 48, § 154.
§ Sec. 105.
|| Figs. 64 and 65, § 190.
** § 234.
†† Op. Cit., fig. 78, § 233.

it wishes to protrude its tentacula it pushes up this disc by means of these muscular fibres.

The tracheæ of insects* are lined with a delicate muscle placed spirally within the cavities of those tracheæ throughout their whole extent. It is very evident that the elongation of the muscle thus situated would tend to dilate the tracheæ, and thus cause an inspiration of air; and, per contra, the contraction of this spiral muscle would tend to lessen the calibre of the tracheæ, and cause an expulsion of the air.

The Onychoteuthis, in order to seize its prey, suddenly extends its long brachial tentacula to a considerable distance.†

The Tortoise has muscles arising from the dorsal vertebræ, by means of which it protrudes its head from beneath its shell or covering.‡

We have given some of the more striking out of innumerable instances of the extension or elongation of muscles when in a state of action, or under the influence of the will; but a still more important application of the general law I have proposed remains to be made; I mean its application to that portion of the muscular system placed about the tubes or hollow organs.

We will take instances again from natural history.

The Physalus, which we have already mentioned, has a bag or hollow organ on its upper surface which it can dilate at will.

The Cuvieria Carisochroma, another species of physograda, has a number of similar organs arranged about the margin of its upper surface which it can dilate at pleasure.

M. Lister published in the Philosophical Transactions for the year 1834 an account of the Tubularia indivisa, in which animal the act of dilating the upper portion of its body by a voluntary effort was very apparent.

Certain classes of insects are possessed of a membranous bag called the sucking stomach, which Treviranus has seen dilated at the will of those insects, and thus by rarefying the air in their proboscides cause the fluids on which they feed to be drawn into their stomach.

Do not the cœcal appendages of the stomach of the leech perform the same office in the economy of this animal?

The swimming bladder of fishes is said to be dilated at will.

We have seen the œsophagus dilated under circumstances that could leave no doubt that the dilatation was owing to the elongation of its muscular fibres; this dilatation of the œsophagus is very apparent in the deglutition of many animals, as in horses, cows, &c. We are sensible of a distension of the stomach on

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* Op. Cit., Fig. 119, § 308. † Fig. 203, § 476. ‡ Fig. 265.

taking exciting articles of food; the same condition is observed in the intestines when the seat of irritation; also in the urinary bladder.

That the uterus in a state of pregnancy is not dilated by means of its contents is very evident to any one who has observed this viscus in this condition in the living animal.

Dr. Marshall Hall, in a paper recently published, states that he has seen the heart elongated and actively dilated in its diastole, which condition he attributes to the injection of the arteries of this viscus.

Any one whose mind is unprejudiced may, by placing his finger on the artery at his wrist, be convinced that the arteries are dilated independently of the blood they contain.

In the act of inspiration the chest is enlarged or dilated by the elongation of all the muscles of respiration, and has its calibre diminished by the contraction of those muscles.

Having considered the above instances of the dilatation of tubes or hollow organs which could only be effected by the elongation of the muscular fibres placed about them, the reader will be prepared to understand the function of the nervous system to which I formerly adverted. Supposing the nerves and cerebro-spinal axis to be composed of tubes through which the vital fluid circulates, and those tubes to be surrounded by very delicate muscular fibres, we can readily conceive how a dilatation of those tubes would abstract the vital fluid from the muscles of the general system.

Some confirmation of this view of the nervous function may be found in the fact that the brains of all animals possessed of this viscus are enclosed in a bony case, so that the dilatation of the brain could not be prevented by accidental external pressure.

I have thus attempted in a very imperfect manner to generalize some of the facts of natural history, and to trace them up to a general law of the animal economy. I have carefully observed and considered the phenomena of muscular action not only in the human subject, but in other animals, and I can confidently assert that I have met with no fact at variance with the general law which I have proposed, namely, *that innervation or the accession of the vital fluid causes elongation of the muscular fibre, and the contraction of the muscular fibre is caused by the abstraction of that fluid.* I can further assert that in the above examination I have met with innumerable phenomena which are altogether inexplicable without the use of this general law.

LOUIS MACKALL, M. D.

GEORGETOWN HEIGHTS,

————, 1844.