VOL. XXI. August, 1896. No. 8.

THE

Journal

OF

Nervous and Mental Disease

AMERICAN NEUROLOGICAL ASSOCIATION.

Twenty-second Annual Meeting, held in the hall of the College of Physicians of Philadelphia, on June 3, 4, 5, 1896.

The President, Dr. F. X. DERCUM, in the chair.
June 3, 1896, 10 o'clock A.M.

President's Address,

THE FUNCTIONS OF THE NEURON.

BY F. X DERCUM, M.D.,

President of the American Neurological Association.

GENTLEMEN:—My first duty this morning is a very pleasant one. It is to extend to you on behalf of my Philadelphia colleagues a cordial welcome, and in doing so I earnestly hope that this meeting, like those which have preceded it, will not only yield its quota of scientific work, but that it will draw still closer the bonds of old friendships as well as create and cement new ones. A spirit of enthusiasm for science, pure and simple, has always distinguished our Association and, at the same time, an honest friendship, a friendship not afraid of scientific criticism, has sprung up amongst us and thrives to-day as it does in few learned bodies.

Our lives are so filled with the details, the incidents and the excitements of our profession that years succeed each other with great rapidity. It seems as though the memorable meeting in Boston had been held but a few months ago, and yet an entire year has passed away. It is fitting, perhaps, that on an occasion like the present I should asked you to pause with me on the highway along which we are so hurriedly marching—should ask you to reflect and consider whether the ideas entertained by us regarding the nervous system need no modification, whether the state of our knowledge does not justify us, nay force us, to radically change some of our most fundamental conceptions of nervous action.

There has been noticeable of late years a tendency to strangely misconstrue the structure of the nervous system and to misinterpret the truths which that structure teaches. It is known, for instance, that the afferent fibres, those which convey impressions from without up to the cortex [themselves processes (axons) of peripheral neurons], terminate not in nerve cells, but in the uppermost layer, the so-called molecular layer or molecular plexus of the cortex. Here in some way the minute divisions of the afferent fibres affect the fibres of this molecular plexus. The latter are in turn in direct communication with the nerve cells of the cortex, are, in fact, themselves cell processes. These facts suggested the idea that afferent impulses are diffused through the molecular plexus without involving or affecting the nerve cells at all, and according to this interpretation the nerve cells are left out of consideration entirely in studying nervous phenomena. This view has been especially advanced by Nansen, who maintains that the old manner of view relative to the composition of the reflex arc and the physiological importance of the nerve cells can no longer be sustained, inasmuch as the cells are not in direct communication with each other, and because direct communication between the central nerve cells and the sensory or centripetal nerve fibres is equally lacking. The reflex arc is constituted, according to Nansen[1], first, by the centripetal nerve and its fibrillary ramifications passing directly into the nervous meshwork of the cortex, that is, into the molecular layer; secondly, by the propagation of the excitation through this molecular plexus; thirdly, by the transmission of stimuli to the minute lateral branches of the centrifugal or motor nerve fibres. It follows that impulses are transmitted to the superior centres without passing directly through the nerve cells. "We can admit in the same manner,"

[1] Soury, Les Fonctions du Cerveau, p. 316.

continues Nansen, "that the voluntary impulses emanating from the nerve fibres which emerge from the superior centres, transmit themselves directly to the centrifugal fibres of the inferior centres without passing through the nerve cells of these centres." He says, further, that it is impossible to admit that the nerve cells of the inferior centres possess a direct importance either in relation to reflex movements or to voluntary movements, and this seems to apply equally well to the nerve cells of the superior centres. This view forces us to the conclusion that the activity of the nervous system, intelligence, consciousness, etc., is seated really in a fibrillary meshwork of the cortex, the molecular layer, and has nothing to do with the nerve cells of the cortex. The latter, indeed, deprived of their psychic functions become simply "trophic" centres. They serve only for the maintaining of the nutrition of the nerve fibres and their innumerable aborescent ramifications.

This view of Nansen's has been adopted by Mills, who, in a discussion on cerebral localization at the last meeting of the Association, and also in the *Textbook on Nervous Diseases by American Authors*, has maintained this position; he holds that "impulses are conveyed from processes to processes through the entire reflex arc, through the entire length of a cortico-efferent, or a cortico-afferent, projection system," without passing through nerve cells, and that "the function of the nerve-cell body is trophic;" that its "nuclei and nucleoli preside over the nutrition of the long or short fibres which pass out of or grow into them; and, further, that cells are of enormous bulk in order that they may be able to sustain these processes. In his words, "the aggregation of gray matter at various levels of the nervous system are watering and feeding places, not places for renewing nerve activity."

Certainly, this view does violence to the fundamental principle that the properties of a given tissue depend upon its cells, and that the cells are the integral parts of the tissues. Nowhere in the whole range of biology do we find a similar anomaly obtain as is implied by this view. It asserts that merely incidental structural attributes are of greater value than the individual cells, whose building up constitutes the tissue. Surely, there is here an intrinsic contradiction.

Not only upon general principles is this view untenable, subversive of all that we have hitherto learned, but if carefully analyzed it is found to present insuperable difficulties. If it be true that a nerve fibre diffuses the energy which it conveys, in a general way, scatters it through all the fibres or nerve cell processes near which it happens to lie, it becomes impossible to explain the definite and precise actions of the nervous system, properties which are so characteristic of it. Nothing but hopeless confusion of function could result if such a thing were possible. It would mean that nerve currents course indiscriminately without relation to each other, through this network of fibres. It would mean that everything that had been done by nature to conserve and isolate nervous impulses by enclosing the nerve fibres in special sheaths of insulating material previous to their entrance into the cortex had after all no purpose, because in the end the currents are turned wildly loose into a common receptacle.

The conservation of nerve currents along cell processes, no matter how long these may be, or whether they be in the cortex or below it, is an absolute requisite. Were it not so, the various commissural and association tracts, whose existence we know of upon anatomical as well as upon physiological grounds, would assuredly be unnecessary, would have no meaning if the loose and unrestrained diffusion of nervous energy in the cortex, such as is implied by Nansen's view, were possible.

Further, such facts as we possess are directly opposed to a diffusion of nervous energy. According to Berkley[1] by far the larger number of the finer fibres of the cortex are medullated almost to the extremities of the end terminations. It is highly probable, Berkley maintains, "that in no instance except at the free termination is there actually such a thing as a naked axis-cylinder," and that "the conduction of the nerve-force from cell to termination and from termination to cellular protoplasm again, is only through the medium of the ending of the nerve-fibre, and that there is no diffusion of the dynamic forces through the cortex, but that the action is a direct one." In this connection he further says: "To suppose for an instant that naked axis-cylinders are present in considerable numbers in the cortex is to me an impossibility, for we must necessarily suppose in such an event that in the closely packed arrangement of axons and dendrons presented by the outer portion of the pyramidal

[1] Medical News, Nov. 9, 1895, p. 506.

and in the molecular layer, where fibres and dendrites touch each other in all directions, the stimuli would quite as often be aberrant as direct and as frequently reach the wrong as the right destination. Naked axis-cylinders are in all likelihood a physiological impossibility in the cerebrum, for were they numerous we can suppose nothing but a constant overflow of stimuli from one cell to another, and subsequent inco-ordination, not only of thought, but also of action."

Far from lending support to the view of Nansen that the cell is to be left out of account in the consideration of nervous action, the discoveries of Golgi, Ramóny Cajal, Van Gehuchten, and others have shown exactly the reverse. They have demonstrated beyond all question that as in all other tissues the cell is the actual integral structure. The nerve cell is a cell entirely by itself. It is a cell as distinct and as self limited as any cell of any tissue with which we are acquainted. Far from being continuous through its processes with other cells, we learn that its processes nowhere fuse with other structures. Its processes are well limited, sharply defined, and bear no relation to those of other cells save that of propinquity or perhaps contact. The individuality of the nerve-cell as a morphological integer is wholly preserved.

If we grasp this idea in its full meaning, our conception of the nervous system changes profoundly. It is no longer a stringing together of telegraph wires and way-stations, but it consists of an aggregation of cell integers, each one of which does its share in the production and in the transmission of nervous energy. For instance, the impulse proceeding from a motor neuron in the cortex is transmitted by the neuron through its own protoplasmic extension (the efferent nerve fibre) to a definite aggregation of cells in the spinal cord. It communicates its energy to these cells in the spinal cord without in any way fusing with their protoplasm or their processes. In the same way the impressions that come by the various sensory paths come from peripheral neurons, those situated in the skin, in the retina, in the ear, in the taste-buds, or in the Schneiderian membrane, and are conveyed by fibres which are merely protoplasmic extensions of these peripheral neurons up to the cortex. Here in turn these fibres transmit the energy they convey to the cortical neurons without fusing with the latter or with their processes. Everywhere, and no matter in what light we view the nervous system, the signal importance of the nerve-cell as an individual entity is strikingly apparent.

A consideration of the above facts have suggested to me the following thought: Can it be that the neuron is not an absolutely fixed morphological element? Can it be that it possesses a certain, though perhaps limited, power of movement? Realizing the practical value and the wide application of this idea, I have examined the literature to see whether a similar interpretation of nervous phenomena has occurred to others, and to gather such facts, if any, as could be brought forward in its support. I found that this thought had occurred independently to three observers, one in Germany and two in France. I found that in 1890 Rabl-Rückhard, in a short paper published in the *Neurologische Centralblatt*, had suggested the view that nerve-cells have an amœboid movement, and he also hinted briefly at the possible significance of such a fact, if true, upon our interpretation of the phenomena of hysteria. Rabl-Rückhard's ideas attracted little or no attention, and in August, 1894, without any knowledge of Rabl-Rückhard's theory, in a paper on "A Case of Hysteria of Peculiar Form," published in the *Revue de Médecine*, Lepine advanced the same view. In endeavoring to interpret the various shifting phenomena observed in his patient he advanced the idea that the neurons were capable of movement to such an extent as to enable them to vary the degree of their relation to each other. About half a year later Mathias Duval, without any knowledge of either of the views of Rabl-Rückhard or of Lepine, in a communication made to the Société de Biologie, advanced the same theory. A week later Lepine, before the same body, repeated his former arguments in its support. Curiously enough this view, so suggestive, so pregnant with possibilities, did not meet with the endorsement either of that veteran histologist Kölliker, or that other high authority, Ramóny Cajal. In a paper entitled, "Some Conjectures on the Anatomical Mechanism of Ideation, Association, and Attention," published in the *Revista de Medicina y Cirurgia Practicas*, May 9, 1895, Ramóny Cajal contended that the nerve cells do not move, because (1) the terminal branches of the nerve cells of the cerebellum, of the olfactory bulb, of the acoustic ganglia, optic lobes, etc., always present the same shape and the same degree of approximation to the cell bodies irrespective of the mode of death of the animal (chloroform, hemorrhage, curare, strychnine, etc.); (2) because the terminal nerve-branches of the retina and of the optic lobes in reptiles and bactrachians presented always the same appearance, no matter whether the organs mentioned had remained in a condition of rest (the animals having been killed after remaining in darkness for a long time) or whether they had been functionally active (the animals being killed after prolonged exposure to sunlight).

While Ramóny Cajal thus opposes the theory of mobility of the neuron, he maintains, on the other hand, that the neuroglia cells possess a great degree of mobility. He points out, for instance, that the neuroglia cells of the cortex are at times stellate and at others much elongated. Their processes have numerous short, arborescent, and plumed collaterals. Two phases can be observed in them: first, a stage of contraction, in which the cell body becomes augmented while the processes become shortened and secondary branches disappear; secondly, a stage of relaxation, during which the processes of the neuroglia cells are again elongated. Ramóny Cajal maintains that the processes of the neuroglia cells in reality represent an insulating or non-conducting material, and that during the period of relaxation they penetrate between the arborizations of the nerve cells and their protoplasmic processes, and render difficult or impossible the passage of nerve currents. On the other hand, when the processes of the neuroglia cells are retracted, the various nerve cell processes which they formerly separated from each other are now permitted to come into contact. To me it seems as though Ramóny Cajal admits the very thing against which he contends. Evidently if the nerve-cell processes are not at one time in contact, and at another are in contact, they must certainly move, and the question at issue is self-admitted. It certainly does not matter whether the nerve cell processes move little or move much, but that they move at all is the question at issue; and this, it seems to me, Ramóny Cajal admits, although he makes that movement a purely passive one and dependent upon the interposition of the processes of the neuroglia corpuscles. It is certainly a minor point whether the movement of the nerve cell processes is active or passive, though it is far from evident, from the histological facts at our disposal, that the neuroglia corpuscles play the role of an insulating material. To me it further seems that a single positive observation outweighs all negative observations, no matter how great the authority behind them, and this positive observation has actually been made. Wiedersheim[3] actually saw in the living animal *leptodora hyalina*, an entomostracan, the nerve cells in the œsophageal ganglion move. The œsophageal ganglion may in one sense be regarded as the brain of the animal, inasmuch as it receives the fibres of the optic nerve, and Wiedersheim saw its cells move and change their shape. He describes the movement as slow and flowing. Certainly, this observation possesses a profound significance. Even if the animal in which the phenomena were observed is far removed from the vertebrates, it must be remembered that it is just in the lower forms that general biological truths must be sought for, and it is just in the lower forms that they have been found. I do not for a moment contend that the nerve cells of vertebrates possess a gross amœboid movement as in the œsophageal ganglion of the entomostraca, but I do contend that it is in the highest degree probable that such facts as we have, scanty though they be, are in favor of the view that a certain amount of movement does take place in the terminal portions of their processes, their dendrites and their neuraxons, although this movement is probably small in extent.

Let us turn our attention for a moment to the subject of hysteria, and let us see what a flood of light is cast upon this subject heretofore so obscure and mysterious. Let us take the simple example of an hysterical paralysis, and see how easily it is explained. The neurons of a certain area of the cortex, for instance, retract the terminal branches of the neuraxons to such an extent that the latter are no longer in contact, or sufficiently near to the neurons in the spinal cord which supply the muscles of the paralyzed part. It explains also the marvelous fact that a hysterical paralysis may at one time be so real, so genuine, as to be indistinguishable from a grossly organic paralysis, and yet the next moment upon a suggestion may absolutely disappear. This shifting of symptoms in hysteria, this sudden disappearance of paralysis or anæsthesia, can be explained by the view here advanced as it can be by no other. When power is suddenly re-established in a hysterically palsied limb, it simply means that the terminal branches of the cortical neuraxon, previously retracted, are again extended so as

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[3] Anatomischer Anzeiger, 1890, p. 673.

to re-establish the proper relations with the spinal neurons. Take again the example of a hysterical anæsthesia. How often do we see a segmental anæsthesia or a hemi-anæsthesia coming and going under the influence of no other stimulus than that which applies to the psychic make up of the individual, namely, a treatment which we call mental or moral treatment, or that more powerful treatment, suggestion under hypnotism. It would be interesting, indeed, to follow out the ideas here brought forward in their application to the various phenomena presented by hysteria, its sensory, motor and visceral stigmata. Even the hysterical convulsion, I contend, can be explained by the view here advanced. Time will not, however, permit more than to indicate the line of thought.

When we turn to hypnotism we can see what a ready explanation it affords for the phenomena presented. Under the fixed stare necessitated by the ordinary method of bringing about hypnosis, and under the suggestion of sleep, the neurons are thrown into certain fixed relations with each other, corresponding solely to the ocular strain and singleness of thought induced. At the same time such relations of the neurons as ordinarily bring them into true contact with the outer world are suspended, probably by retraction of cell processes. We can easily understand, in the light of the theory here advanced, how under hypnotic suggestion a hysterical paralysis disappears, or how under hypnotic suggestion anæsthesia is produced in this or that part of the body. Further, the various stages of hypnotism itself—lethargy, catalepsy, somnambulism—are all of them capable of a scientific explanation upon this theory. Hypnotic lethargy, for instance, a stage so easily produced in the majority of patients, merely signifies that so general has been the retraction of the cortical neurons from each other that not only is sleep produced, but also a stage of general motor relaxation, due to the retraction of the terminal branches of the neuraxons in the spinal cord. In hypnotic catalepsy, on the other hand, the reverse obtains. Here the relations of the cortical neurons to the spinal neurons (contact or increased proximity, whatever it may be) are established to a degree beyond that which is normal, and the consequence is an enormous general increase of muscle tonus. In somnambulism, again, certain of the neurons, especially those which stand in direct relation with the various sensory organs, form partial combinations with a limited number of other cortical neurons, so as to produce the various limited psychic phenomena characteristic of somnambulism, whilst the great bulk of the neurons of the cortex, the summation of whose action constitutes the ego and brings it into close relation with the outer world, have their processes retracted in sleep.

Leaving this interesting field, let us see for a moment of what enormous value this interpretation of cortical action is for normal mental phenomena. Let us take the familiar instance of normal sleep. Sleep, instead of resulting from brain anæmia, or some other apochryphal condition of the circulation, merely means that when the substance of the cortical cells has been diminished by functional activity, which diminution we have reason to infer, from the researches of Hodge[4] (on the change in nerve cells in fatigue) there comes a time when the cell processes are retracted, so that the neurons no longer stand in active relation to each other. Interchange of action cannot then take place, unconsciousness follows; sleep is established. Spontaneous wakening merely means that after nutrition has reached a certain point, a point where the wasted cell has been replenished, extension of the cell processes again takes place, and interchange of active functional relations is re-established.

Numerous other ideas also suggest themselves in relation with the view here advanced. Take, for instance, a train of thought. This appears to follow purely *mechanical lines*. Thus a sequence of sound vibrations impinging upon the peripheral auditory neurons, the auditory cells, produces in them a change, which in turn affects the relations which their neuraxons bear to the auditory nuclei, and secondarily to the auditory cortical neurons. Not only are the latter affected by the impressions received from the afferent neuraxons, but they in turn react in such a way as to change their relations to each other, and the new positions assumed by them will depend largely upon the fact as to whether a similar sequence of impressions has passed through them before. If so, the old combinations will be reformed, and as a corollary, the recognition by the ego of the sounds as something heard before. From the cortical auditory centre there now pass through the general cortex a series of combinations among the neurons, also along

[4] Journ. of Morphology. 1892, Vol. VII., p. 95.

the oldest and best-travelled lines, so that a given sequence of musical sounds may suggest at first a familiar air, a moment later a vivid recollection of an opera once heard and seen. In this simple illustration is embraced the physiology of perception, of conception, of memory and the explanation of the very sequence of thought itself. Surely, we have here a basis upon which a rational and biological psychology can be based. If we but permit the nerve cell to retain its normal attributes, its individuality both morphological and functional, much of that mediæval darkness in which the mystery of mind is enshrouded breaks away.