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## The neuron doctrine - theory and facts

*Nobel Lecture December 11, 1906*

It may seem strange that, since I have always been opposed to the neuron theory - although acknowledging that its starting-point is to be found in my own work - I have chosen this question of the neuron as the subject of my lecture, and that it comes at a time when this doctrine is generally recognized to be going out of favour.

The subject, however, is still a very important one in spite of these signs of decline; but more than that, it is a very real one, for the majority of physiologists, anatomists and pathologists still support the neuron theory, and no clinician could think himself sufficiently up to date if he did not accept its ideas like articles of faith. It is a subject which deserves to be re-examined all the more because there is a growing tendency to attach to the word *neuron* a meaning different from the proper one. Many authors, in fact, play on words by substituting the word *neuron* for nerve cell, and this has now become legalized through common usage and tradition. Admitting that eventual substitution scarcely involves a question of principle and that after all it is nothing new, for continuity between the cell and nerve fibres was already well known, I ought to say that I am against giving a meaning to a word which differs from that given it by the person who introduced the word into science.

At a point of time that the results of black staining had hardly started to become generally known, while I had already for about ten years achieved results much better in terms of *clarity* than those which had attracted attention elsewhere, the idea that cells and nerve cells formed an anatomical unit became much more acceptable to the mind in a far more objective way than that made possible by previous studies. The concept then developed that the cell body and all its processes make up one independent elementary organism which is not joined to others but merely contiguous with them. Waldeyer perceived the existence of such a unit and called it the *neuron*. In an effort to use the word *neuron* with the meaning given it by its creator, I must quote in its entirety Waldeyer's own description in a lecture which he gave in 1891 on the latest research into the anatomy of the nervous system. On page 52

of this publication Waldeyer says: "The nervous system is made up of innumerable nerve units (neurons) which are anatomically and genetically independent of each other. Each nerve unit is composed of three parts: the body, the fibre, and the terminal branches.

Physiological conduction can take place just as well from the body to the terminal branches as in the other direction.

Motor impulses can only be transmitted from the body to the nerve terminals, while sensory impulses can travel in either direction."

In these words the unity, both anatomically and embryologically, of the neuron is stated; but the physiological delimitation, which seems not to have been comprised in this synopsis, is found better outlined by the author at the end of the same work, where he says that the chief result of the research which he has summarized is that it has made it possible to demarcate more precisely the anatomical and functional parts of the nervous system.

Still greater precision soon appeared, thanks to the work of several scholars, so that the concept of the neuron finally appeared with the triple attribute of being an independent anatomical, embryological and functional part.

To define all this in a still better way, here are the ideas on which the neuron theory is based:

(1) *The neuron is an embryological unit*, that is to say, it derives from a single embryonic cell.

(2) *The neuron, even in its adult form, is one cell*. The whole system - ganglion cell, protoplasmic and nerve processes - in the adult animal too, is only one cell.

(3) *The neuron is a physiological unit*. This fundamental idea which Waldeyer expressed with perfect precision has been enlarged upon both from anatomical and functional sides with additional propositions, for example : The communication between neurons is only established by casual contact. There is scarcely any nervous tissue apart from the neurons; the neurons are also trophic units.

From the physiological point of view, the neuron theory finds its most perfect expression in the so-called theory of dynamic polarization, which van Gehuchten had already outlined, and which my distinguished colleague Ramón y Cajal developed and supported in a most complete manner.

The fundamental points of the doctrine may be summed up like this: the transmission of nerve impulses is conducted from the protoplasmic extensions and the cell body towards the nerve extension; consequently, each nerve cell possesses a receiving apparatus constituted by the body and the proto-

plasmic processes, a conducting apparatus - the nerve process - and a transmitting or discharging organ. The protoplasmic processes should, therefore, act as conductors towards the cell body, and the nerve process should act as a conductor away from it. The author himself has successively, as one knows successively modified his theory, either for adapting it to some particular topographical arrangements of the point of origin of the nerve process, or to harmonize it with data resulting from more intensive studies of the structure of sensory organs and of the central nervous system of lower animals.

More particularly, he modified his original scheme to meet some opposition concerning the nerve pathways in spinal ganglia: he accepted that the pathway from the periphery goes directly into the central branch of the T-shaped bifurcation, avoiding the cell.

This theory, which I thought ought to be briefly summarized, should not be considered as an essential part of the neuron theory. In fact it only expresses one interpretation of nerve function and does not exclude the possibility of

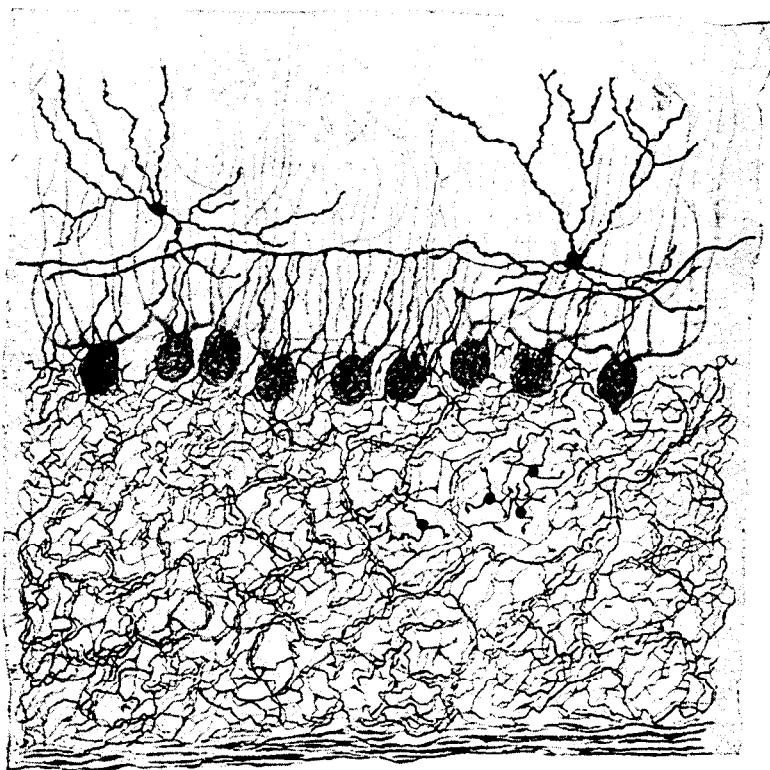


Fig.1

others. I do not think that I need prolong the discussion of the above any longer to achieve the purpose I intended. I shall therefore confine myself to saying that, while I admire the brilliancy of the doctrine which is a worthy product of the high intellect of my illustrious Spanish colleague, I cannot agree with him on some points of an anatomical nature which are, for the theory, of fundamental importance, for example, that the peripheral branch of spinal ganglion cells must be identified with a protoplasmic process, since one must consider the myelin sheath as an absolutely secondary event, for it is only necessitated by the length of the process. Similarly, I cannot accept as a good argument in support of the theory the statement which, however, is its starting-point, that says the processes of the cells of the molecular layer of the cerebellum terminate by forming endings on the bodies of the cells of Purkinje, for I have verified that the fibres coming from the nerve process of the cells of the molecular layer only pass near the cells of Purkinje to proceed into the rich and characteristic network existing in the granular layer.

Fig. 1 illustrates this arrangement as regards the formation of the nerve network; the bundles of fibrils coming from the nerve process of the small cells of the molecular layer, and which ought to form the alleged ending on the surface of the cells of Purkinje, can be seen to continue by an infinite number of subdivisions into the nerve network.

Fig. 2, which is an exact reproduction after life, shows in detail the way

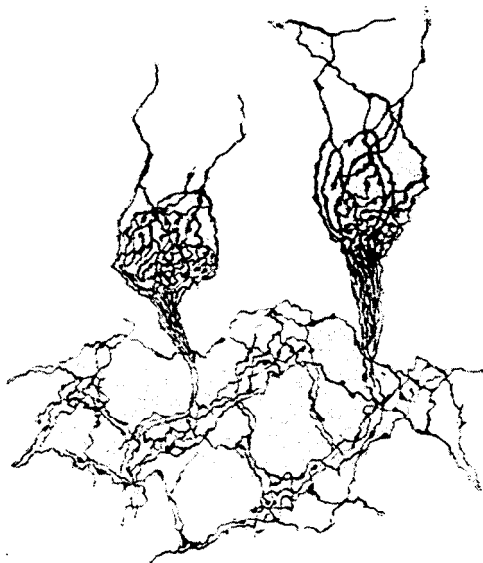


Fig. 2

this same nerve network is formed by the passage through it of fibres which come from the molecular layer running across the surface of the cells of Purkinje.

As for the new form of the theory of dynamic polarization, I have not been able to agree with my illustrious colleague about the direct pathway of the nerve current from the peripheral branch to the central branch of the T division in the spinal ganglia, for it is not difficult to show that, from here, the fibres of the peripheral and central axis cylinder change their course and sweep towards the cellular body, while no direct path from the peripheral branch to the central branch can be seen.

At this point, while I shall come back to this question later, I must declare that when the neuron theory made, by almost unanimous approval, its triumphant entrance on the scientific scene, I found myself unable to follow the current of opinion, because I was confronted by one concrete anatomical fact; this was the existence of the formation which I have called the diffuse nerve network. I attached much more importance to this network, which I did not hesitate to call a nerve organ, because the very manner in which it is composed clearly indicated its significance to me. In fact, although in various ways and to varying extent, every nerve element of the central nervous system contributes towards its formation.

At the same time I could not altogether lose interest in a statement dealing with the behaviour of protoplasmic processes, something I shall return to later. How could I consider these types of processes as being intended purely for the reception of nerve impulses if I saw the processes of some sorts of cell arborize at the surface of convolutions and penetrate beyond the molecular layer and sometimes even into the meninges?

But returning to a methodical account of this subject, I come to the first statement on which the neuron theory is based:

1. *The neuron is an embryological unit, i.e. it is derived from a single embryonic cell.*

The basis of this fundamental conception, as formulated by Waldeyer, is essentially depicted by the well-known and classic works of His on the development of nerve elements and, in particular, formation of neuroblast. As a result of these studies we must consider nerve fibres as the direct emanations of neuroblasts, which the author considers as independent units.

It is not superfluous to recall that these results and, more particularly, the essential fact of having proved the independence of neuroblasts are based

upon data established, mainly on the human embryo, with ordinary staining methods. It is now easy to show, from the fourth day of incubation, surprisingly detailed and complex structures in the chick embryo, using black staining, and one can follow the filament representing the nerve process from its origin in the cell body across a background of the central nervous system and, out of this, to the primitive muscle segment; if we take this into account, we must be led to believe that really we suppose they are independent only because we cannot prove that more intimate connections do exist.

However that may be, I do not know whether an absolute value can be attached to it or not. So far no one has been able to exclude the possibility of there being a connection between nerve elements already from the beginning of their development. Even if these elements were originally independent, one would not be able to show that this independence was going to be maintained.

Then the arguments surrounding other sorts of body tissue should be considered, for example, the epithelial cells of the Malpighian layer were thought, only a short time ago, to be independent, whereas now with modern methods, any one can show that these cells are intimately connected by bundles of fibrils passing from one to the other.

One may now ask if we are dealing with an original connection or with connections which are successively formed during development. The answer would not be easy. From the embryological point of view two sets of data are directly or indirectly of importance for the question of the cellular unity of the neuron. On one hand there are the studies on the multicellular origin of the nerve cell (Capobianco, Fragnitto) and, on the other, those studies on the origin of nerve fibres from chains of cells (Balfour, Beard, Dohrn, Raffaele, Bethe)... These studies would oppose the neuron theory; but the data put forward in support of the theory of the multicellular origin of ganglion cells are hardly conclusive. With regard to work done on the origin of nerve fibres from cell chains, I must take into account that done in my laboratory by Perroncito on nerve regeneration, although its argument is of only indirect value. These studies show that the fibres of the new structure always come from pre-existing nerve fibres connected with the cell of origin, and that they do not come from so-called peripheral cell chains.

Because I cannot consider results of a histogenetic nature, which I have just mentioned concerning the multicellular origin of nerve fibres and ganglionic structures as arguments against the neuron theory, I cannot consider the results of even the most recent work which showed the central origin

of regenerated nerve fibres, contrary to the theory of independent regeneration, as conclusive evidence in favour of the theory.

I do not think that the latest studies can do more than prove the central origin of nerve fibres; which is a long way from meaning that each fibre is derived from a corresponding cell, as one should have to accept it for these results to support any argument in favour of the neuron theory. It will be enough, on this point, to bear in mind that nerve fibres in general, motor fibres included, are connected in the central nervous organs by means of their collateral fibres in a very complicated way - just as my studies have shown and to which I will come back later.

Finally, still on the subject of criteria which may be gathered from histogenetic studies, I do not think that this is the place to dwell on statements like those of Joris, etc. who accept that fibres arise independently of the cell and are subsequently incorporated in it; neither can I consider the incomprehensible results of Besta which do not bear any relation to the fundamental knowledge we have of the detailed anatomy of the nervous system.

I think that what I have said would justify the statement that, in the present state of our knowledge of the histogenesis of the nervous system, it is not possible to state with certainty that what we know of the origin of nerve cells has a well-proved fundamental value, and can be used to support the alleged embryological independence of the nerve cell.

*2. The neuron is, in the adult state too, an independent cell unit.*

Referring to a time when the results of black staining started to become known, it is not difficult to imagine how they provided a basis for the idea that the whole elementary nerve structure ought to be considered as one independent anatomical unit.

One recalls how strikingly these silver nitrate preparations differentiate between the varieties of cell shapes and show how they are provided with many processes which one can follow, just as one may their innumerable subsequent branches, for a long way from their cell of origin without it being possible to detect anastomoses; if one takes this and the fact that each cell in these preparations shows the constant presence of a single functional process - the nerve process (or axon) - into account, then I say one can easily understand how the idea of cellular unity has imposed itself at that time.

However, even then, our knowledge was not all that elementary or limited as one might have thought. I must point out here that, as regards the behaviour of the nerve process, I had already distinguished two different types

of nerve cells which I simply called types one and two to reserve my opinion on their possible physiological significance.

I called a cell of the first type a cell whose nerve process, while maintaining all the time its individuality, and having given off a more or less large number of collateral fibres, seemed bound, at least in most cases, to change into the axis cylinder of a myelinated nerve fibre. A cell of the second type was one whose nerve process I had seen divide indefinitely to lose its individuality and spread over an indefinite area, without demonstrable limits.

The two types of cell which I have described are illustrated in Figs. 3 to 6. Fig. 3 shows a cell from the antero-lateral portion of the anterior horn of the spinal cord. Its nerve process enters an anterior root having given off a number of collateral branches which are finely subdivided. Fig. 4 shows a Purkinje cell from the cerebellum. Its nerve process crosses the granular layer giving off a number of fine branches, and then goes to communicate with the nerve fibres in the white matter of cerebral gyri. Fig. 5 is a reproduction of a nerve cell of the second type from the spinal cord (pathway between the anterior and posterior horns); its nerve process branches indefinitely. Finally, Fig. 6 shows a cell from the granular layer of the cerebellum. It is one of the most

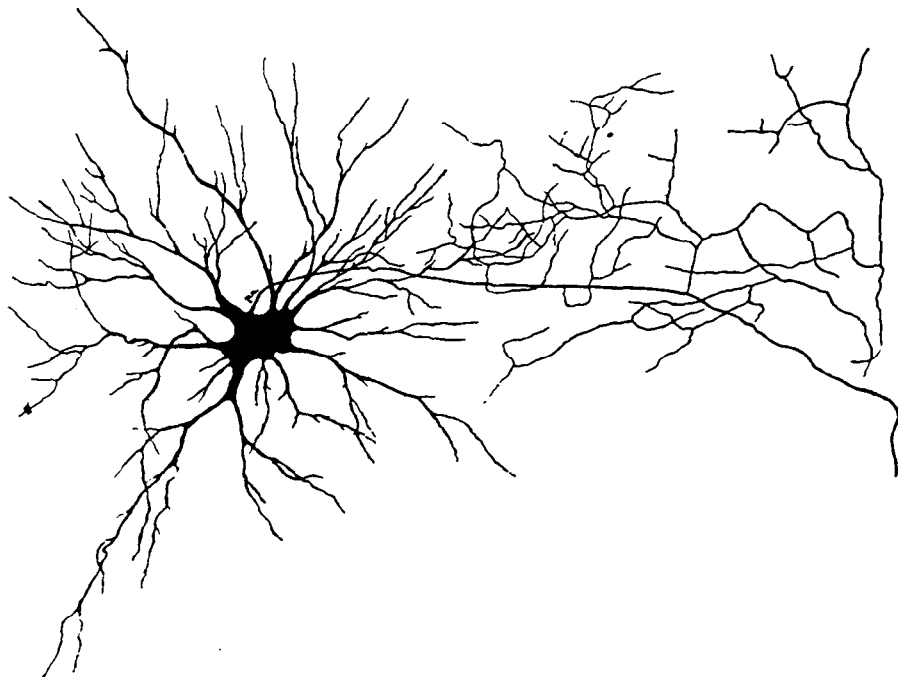


Fig.3



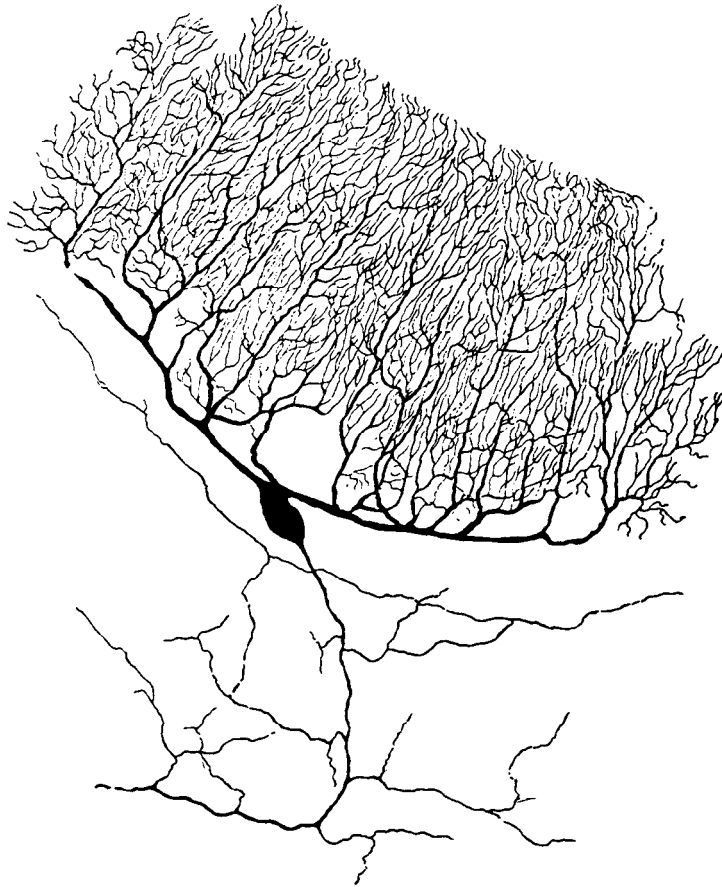


Fig. 4

characteristic examples of the way in which the nerve process of the second type of cells behave.

As regards the distinction which, from the beginning of my work, I have made between the two types of nerve cells, I have relied on a definite morphological fact - that is on the different behaviour patterns of the nerve processes. I must, therefore, stop for a moment to call attention to a remark which has been attributed to me. This was that I had given the two types of cell a motor and sensory function, respectively, without considering that these two types of cells are found mixed in the same regions of the nervous system.

This remark is not unimportant, even from the point of view of the neuron theory. For two reasons, therefore - for history's sake and for the subject I am considering - I feel I must recall the facts as I described them.

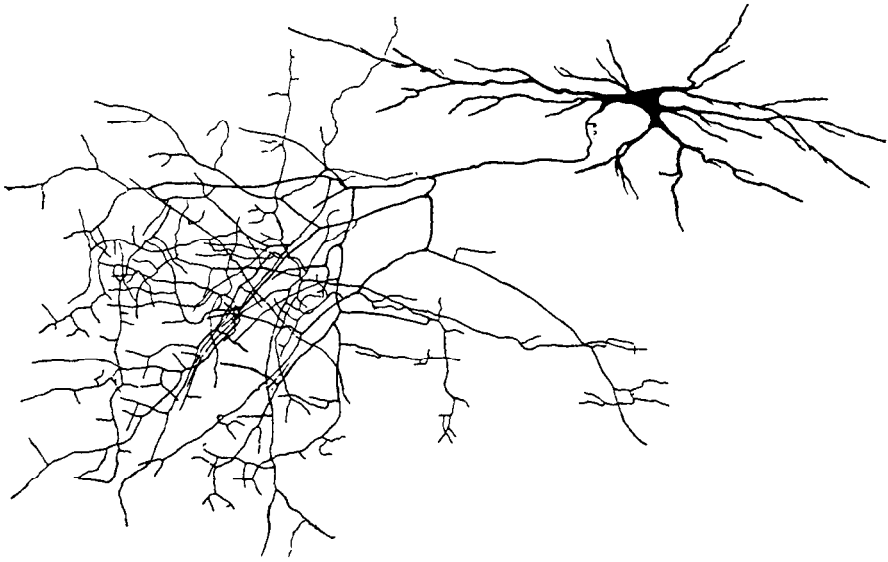


Fig. 5

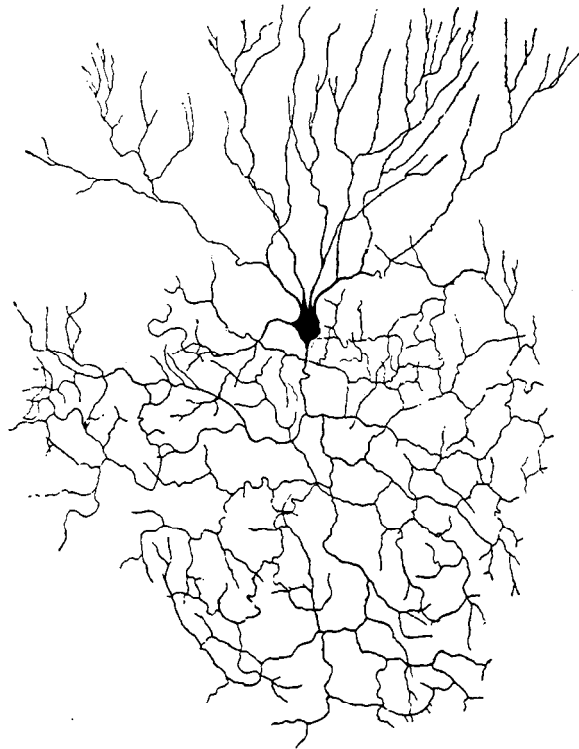


Fig. 6

I have called attention to the necessity for finding a *criterium* capable of giving a basis for such an opinion with an insistence which can be faulted for its excess rather than the opposite. Several times I have pointed out that it would be easy enough to say what functions both these types of cell had if there were some parts of the central nervous system where cells of one type were found exclusively and which were shown to be entirely sensory or motor in function. The two types of cell are, however, scattered and intermingled everywhere in the central nervous system, even in the motor area of the cerebral cortex, which is called the zone of Rolando, which is regarded as being essentially motor with some sensory function. In the occipital lobes, which have not been shown to be entirely sensory, the position is the same.

The spinal cord would lend itself better to the solution of the problem if, as we would like to have thought, one has been inclined to think the anterior horns were entirely motor and the posterior horns entirely sensory. But it is a distinction which no longer has any absolute value and will not stand up to criticism, either from the anatomical or the physiological point of view.

There are some fibres which go from the anterior roots to the posterior roots and others which go from the posterior roots to the anterior; so that, at the most, one can talk about predominance, not exclusiveness, of fraction.

But if we look at the question from the other way, as it were, and try to establish whether the fibres whose function is unquestionably proved, show differences in their connection with the centres, and if for that purpose we direct our studies towards the anterior and posterior root fibres, we notice the following characteristic and important points :

(1) that the axis cylinders from the anterior roots, after giving off a small number of collateral fibres, continue in the nerve process of a cell;

(2) that the axis cylinders from the posterior roots, obliquely entering the grey matter, subdivide after a short course and become immeasurably slender fibrils which by successive division become lost in the diffuse network of the grey matter.

Relying on these results it has been possible to prove that - in the spinal cord - sensory and motor fibres behave in an opposite way to each other. Without foregoing the strictest caution we may suppose that the sensory and motor fibres of spinal nerve roots behave in accordance with a general rule, common to all types of fibres which behave in the same way: direct and undetached contact of nerve fibres with ganglion cells would be particular to motor fibres, and sensory fibres would be characterized by their indirect contact through the intermediate diffuse network.

From these data, I have decided to consider cells in direct contact with anterior roots as motor, and cells from other parts of the nervous system which behave in an analogous way as probably motor or psychomotor.

As for cells of the second type, it is much less decisively and in the form of a simple hypothesis that I have been able to consider them as sensory, or psychosensory. I have willingly kept their name of type two which, far from suggesting their physiological role, restricts itself to expressing the idea of the direct anatomic contact which these structures have with the diffuse network in which their functional process passes, and loses its individuality.

I may, from the foregoing, state an anatomical fact which, in my opinion, is of major importance in judging the physiology of the nervous system: that is, the existence of a diffuse network which I have spoken of several times in my paper.

I recognized the existence of this network several years before the neuron theory made its triumphant appearance on the scientific scene. It is a true nerve organ which is found, with differences in detail, in all layers of the grey matter in the central nervous system.

What immediately seemed to me of capital importance with regard to the role of the network in the function of the central nervous system, was its very structure, as it appeared to me. I shall confine myself here to the description I gave then, repeating here that the following elements take part in its formation:

(1) collateral fibrils which radiate from the nerve process of cells of the first type;

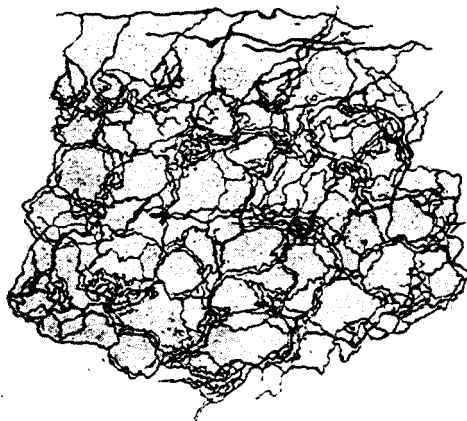


Fig.7

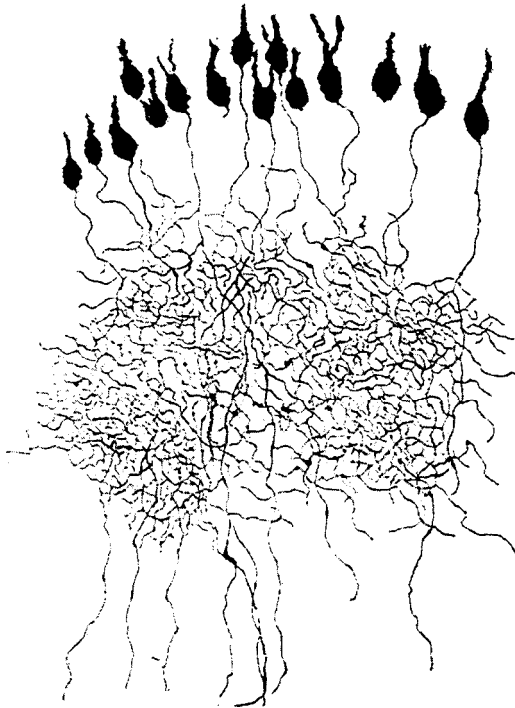


Fig.8

(2) the assemblage of nerve processes of the second type breaking up in a highly complicated way;

(3) collateral fibrils coming from nerve fibres of the first type to make direct contact with ganglion cells of the first type;

(4) a large number of other nerve fibres which, like the nerve process of cells of the second type, gradually lose their individuality while dividing to become extremely thin filaments.

As regards the importance I attached to this network, may I repeat again that I have always considered it as an organ playing a fundamental part in the specific function of the nervous system and that I have, many times, said that I considered it as an entirely distinct anatomical entity and not as a simple hypothesis.

I have demonstrated its existence in the chief areas of the nervous system, in the spinal cord, in the cerebellum, and in the cerebral cortex, realizing that it is a little modified from site to site.

Figs. 7 and 8 are examples of this. Fig. 7 shows the nerve network as it is

seen in the granular layer of the cerebellum. Fig. 8 gives an idea of the network existing in the *dentate gyrus* of the pes Hippocampus. I shall have to return to this subject later.

When I spoke of the diffuse nerve network of the spinal cord, remarking that, in fact, one can also find connections from fibre to fibre which make a real, closed lattice-work, I did not think it superfluous to point out that, in view of the extreme complexity and intimacy of the connection between the filaments of the network - such as could be seen in my preparations, it was no longer necessary to invoke a material connection, a fusion of one fibre to another, to be aware of the functional connections between the different groups of cells and between the different parts of the central nervous system.

In the presence of a reticular arrangement as fine as the one I have described, an arrangement in which the fibres, having no insulating myelin sheath, run side by side or very closely to each other and have frequent and extended communications, I have stated that there was no reason to think that direct contact between fibrils of different origin was an indispensable condition for the transmission of the impulse from one to the other. On the contrary, I thought that these contacts were more than sufficient for an impulse to be transmitted in any direction. From then I had even adopted the idea which Forel, after my work, put forward which stated to see less and less why a mutual connection with really continuous contact of fine branching nerve elements must always be considered as a postulate necessary to explain nerve transmission. What I have just said of the network, on its structure and, above all, on the fact that all the parts of the central nervous system make up a part of it, proves the anatomical and functional continuity between nerve cells. And that is the reason why I have not been able to accept the idea of this independence of each nerve cell which is the essential basis of the neuron theory.

While speaking of the connections between cells and nerve fibres, I should like to call attention to a particularity in the organization of the *dentate gyrus* which illustrates these connections well and also demonstrates the *group action* of nerve cells which I have defined as being opposite to their alleged *individual action*.

The small nerve cells which make up the elegant and characteristic layer of the *dentate gyrus*, send their protoplasmic processes towards the surface of this rudimentary gyrus while from the opposite pole they send a single and very thin process. This process, at least in the majority of cases, subdivides a

short way from the cellular body into extremely fine filaments which go to make up a *reticular zone*. On the other side a well-differentiated bundle of fibres from the *fimbria* and from the medullary layer which clothes the ventricular surface of the horn of Ammon turns towards the *dentate gyrus*.

The fibres of this bundle, on approaching the reticular layer, which has been mentioned above, subdivide in a most complicated way and interlace with branches of nerve processes of cells to form with them the reticular zone. (Incidentally I would point out that, with ordinary methods, this reticular layer looks finely dotted in just the same way as the "Punktsubstanz" of older authors.) One therefore gets the impression that the reticular layer is interposed as a common region between the nerve processes of one side and the nerve fibres of the other (Fig. 9).

All one can gather of these connections speaks in favour of the whole *dentate gyrus* acting as a group and against any individual action of these cells.

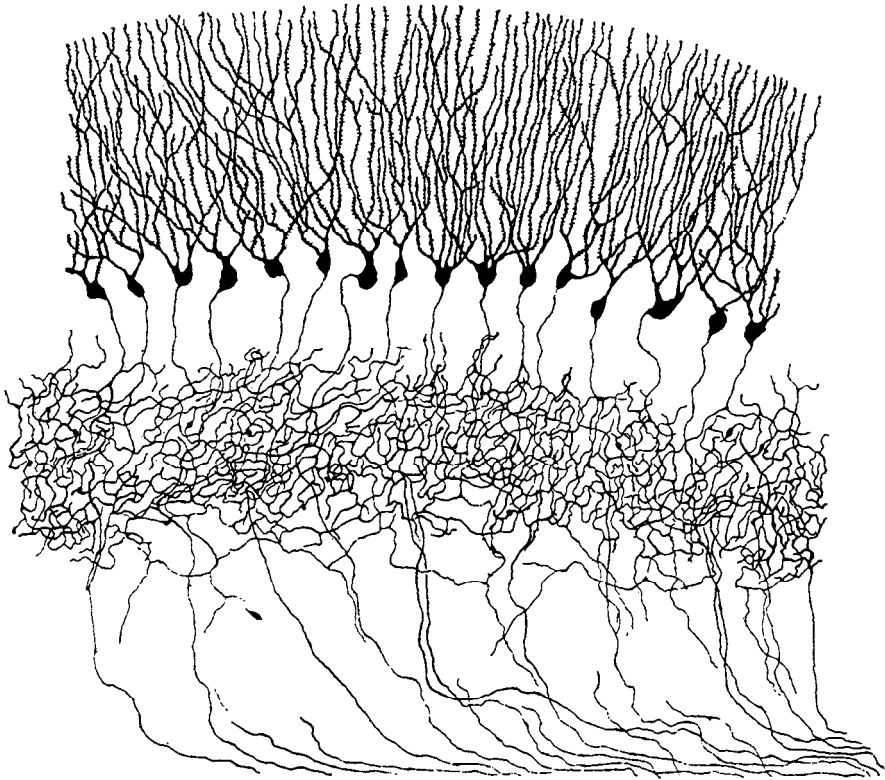


Fig.9

The diffuse network is exclusively formed by filaments which must be considered to be nervous because they are derived from nerve processes of the first and second type, and from fibres certainly recognizable as nerve fibres by their classical properties. To assert that this network is the organ by which both an anatomical and functional liaison is effected between the different parts of the nervous system, I had to consider the doctrines concerning the traditional idea that the nerve cells must be exclusively considered as the primæval centres where the physiological activity of the nervous system takes place.

In this connection I recall that Nansen, for example, referring to his work on lower animals, had already thought that the true organ of specific nervous activity was the fibrillar network rather than the ganglion cells. This idea, as one knows, has recently been confirmed by Bethe who had no hesitation in writing *"the doctrine attributing to cells the role of being the centre of specific nervous activity is only a morphological speculation which is supported by no convincing proof, while there are several facts which are decidedly against it"*.

However, I do not think I ought to linger on this argument, about which we possess only too little data.

The protoplasmic processes, or cellulipetal processes according to the theory of dynamic polarization, have played just as important a part in the discussions about the neuron.

After I had shown that we must abandon the old view of Gerlach, which said that these processes subdivide indefinitely, to make up the nerve network, and after I had denied the constant presence of direct anastomoses, I set about investigating, in a very determined way, how this sort of process behaves.

Without committing myself on this question for the moment, I should like to recall, in this connection, a fact which may easily be verified in several types of cell; that is, that the majority of these processes, having more or less subdivided, turn on the extreme border of the gyri where they terminate either in a fairly large pyriform or spherical swelling or by an expansion attached to a vessel wall.

It is not difficult to see some of these terminal boutons going even beyond the limit of the gyri to the meningeal vessels. This is a fact I have seen from the beginning of my work and one I have been able to reproduce each time I subsequently wished to see it. It is especially easy to verify this arrangement in the cerebellum of young birds (Fig. 10). With this fact in mind is it not



logical to think that these protoplasmic processes are also organs of nutrition supplying the cell body?

May I add here that the idea of the protoplasmic processes having a nutritive function is, in my opinion, corroborated by the well-known facts concerning the behaviour of blood vessels towards nerve cells deprived of protoplasmic processes. In these cases the blood vessels establish an exceptionally intimate contact.

Most of the well-known giant cells of *Lophius piscatorius* do not have any protoplasmic processes; surely enough we notice an actual invasion of blood vessels which penetrate the cell body and get close to the nucleus.

Deiters' cells in the base of the corpus quadrigeminum, in the adult state, do not have these processes either, and their cell body, too, is enclosed by an actual network of capillaries.

The function which I have suggested the protoplasmic processes carry out, is confirmed by some anatomico-pathological studies and in the results of experimental work which has been done on this point.

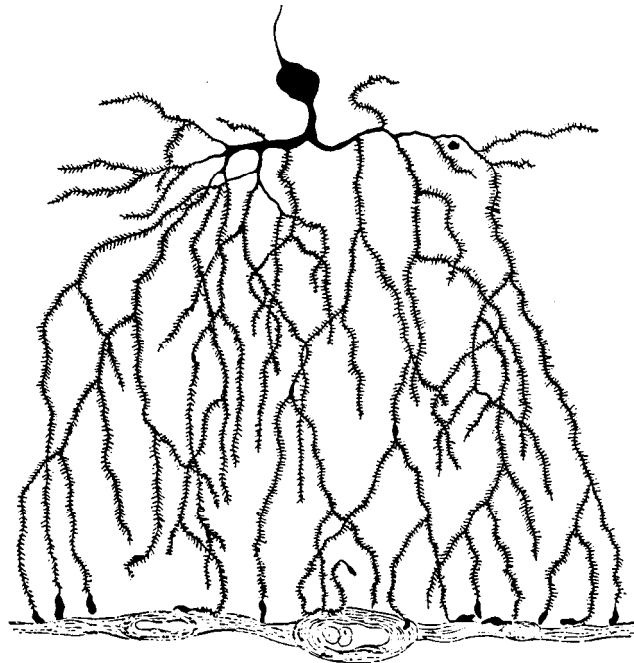


Fig. 10

In some work done on changes seen in the central nervous system of a case of chorea, I drew attention to the fact that the calcific degeneration (involving the cells of Purkinje in the cerebellum) was not evenly distributed throughout the part, but was found much more frequently in the small peripheral branches of the protoplasmic processes than in the larger branches, the cell body or the nerve process. Then I recorded that such a fact should bear a relation to the manner in which the degeneration progresses, that is to say, it could mean that the change begins at the periphery in the smallest branches of the protoplasmic processes and from there progresses centrally. We have the work of Monti on cerebral embolism in the experimental field. As a result of this work using black staining on the centres of softening produced by the embolic occlusion of small cerebral vessels one can see that the cell changes (varicose atrophy) begin at the extremities of the protoplasmic processes and only of those which are directed towards the occluded vessel. It is also seen that the degenerative process only seems to involve the protoplasmic processes along which it advances some time after it has affected the cell body.

That said, I feel in a position to recall that whenever speaking of the function of protoplasmic processes, I always said that they may share in the particular function which we attribute to the nerve cell. Since protoplasmic processes are a direct product of the body of the nerve cell whose structure they reproduce, I have always said that naturally, one must also accept that they may possibly share in the specific function of the cell substance.

I am aware that strong objections have been raised with regard to this part of my work. Above all there has been the question of the spines which the protoplasmic processes are provided with. A major functional role has been given to these spines and there are innumerable accounts in the literature which aim to show what modifications they undergo in varying states of activity, rest, sleep and of wakefulness. Now, if one considers that similar structures are found not only on protoplasmic processes but also on cells of the netiroglia and on the nerve process, it is not hard to imagine that, even disregarding any estimation of the worth of the experiments, I have never attached any final importance to this statement regarding protoplasmic processes as cellulipetal conductors.

In the discussion on the significance of protoplasmic processes I must mention what has been called the *pericellular and peridendritic network*. This has also been described as being a nerve organ and of great importance in the mechanism of nerve actions. Even my name (*Golgi-Netz*) has been given to

this network, thus confusing it with the real diffuse nerve network which I described.

While confirming the description, which I have already given, of a sheath surrounding the body of the cells as well as their protoplasmic processes, this sheath only sometimes appears to be reticular. *I should also like to reaffirm* the interpretation I gave to it, as a sheath whose nature is not well understood, probably a combination of nerve tissue and keratin and perhaps connected with the cells of the neuroglia.

As for the fibrillar structure which has been recorded in protoplasmic processes as well as in cell bodies and which provides a new argument in favour of the theory of cellulipetal conduction in these processes, I shall speak of it later in connection with my work on the structure of nerve cells. I must say, here, however, that the problem of the structure of nerve cells and more especially that of the significance of the different formations these cells may present, is still very far from its solution.

The modern trend of research into the nervous system has been characterized above all by a succession of investigations of cell structure and we know well how each new structural detail has served as an argument either for or against the neuron theory.

On this subject I must first of all declare that it is absolutely impossible for me to follow certain arguments which, in my opinion, leave the realms of anatomy and which relate to statements avoiding any possibility of verification.

The lucubrations of Nissl on the "Centralgrau" are such an example. When I see that he denies the existence of a nerve network without at first putting himself in a position to verify it, and that he can only define the "Centralgrau" by saying that it denotes everything which is found between the end of the myelinated fibres and the cells, when I hear him refuse to give any credit to evidence established by black staining on collaterals of nerve processes and of fibres, I must conclude from it that Nissl's ideas are more in the province of speculation than of anatomy.

Neither can I follow any more the long disputes which have followed each other with regard to what Bethe has called the "network of Golgi", referring to a structure I described as sometimes assuming the appearance of a network and which has nothing in common with the real diffuse nerve network.

Seeing this network associated with the endings of nerve fibrils of different origins and with nerve processes on the surface of cells, I am naturally obliged

to ask myself on what proven anatomical facts these conclusions have been based.

These so-called results aside, I must hasten to say that in modern times new technical methods have opened up new horizons in the study of detailed cell structure, and in first place are those methods of Ramón y Cajal.

Although these methods have produced some wonderfully good results there is, however, no agreement among them, so that we think they represent paths converging perhaps towards a common goal and that one day they will lead to an unfolding of the mystery around the nerve cell, but we must realize that at present these paths are still not united.

Following such a statement it will be easily understood that, in my opinion, we cannot draw any conclusion, one way or the other, from all that has been said on the importance which different structures identified in ganglion cells have, in being for or against the neuron theory.

Besides the work of Nissl on chromatic and achromatic substances which, in spite of their importance in the study of experimental or anatomico-pathological changes, have not shed any new light on cell structure, the work of Apáthy must be placed in the fore-front of the field. This work is so well known that I need not recall it in any detail. Apáthy's results, which demonstrate the passage of neurofibrils from one cell to another, as well as the existence of a simple diffuse network should, to tell the truth, imply the destruction of the neuron theory and be formal corroboration of the existence of a diffuse nerve network and of the theory which has been built on this fact.

I must, nevertheless, say that Apáthy's work, which is illustrated so brilliantly by his preparations, is on invertebrates and particularly the worm, and that we can find no support in them to allow us to apply these data to vertebrates.

This denial of any correspondence refers, too, to all the most recent studies in which the fibrillar structure has been shown in the nerve cells of vertebrates.

We thus arrive with regard to my studies to what I have called the *endocellular reticular apparatus*. This structure is illustrated by Figs. 11, 12 and 13, which show nerve cells from the intervertebral ganglia of the horse, and by Fig. 14 which shows a cell of the same type from the dog. In showing these pictures here, I must repeat what I have persistently said before, that is, that the significance of these structures still represents an unsolved problem.

I must also recall those fibrils with the typical appearance of nerve tissue which I best demonstrated on the surface of cells of the cerebral cortex and

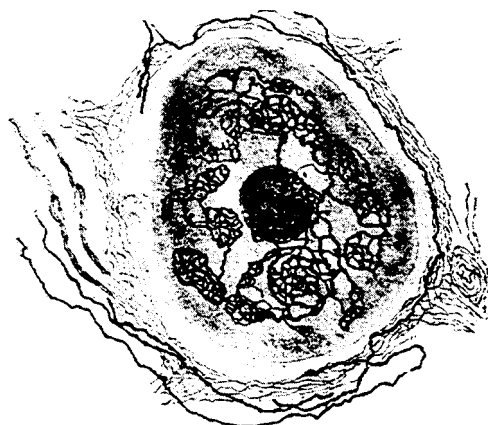


Fig. 11

say that I do not feel able yet to make any statement about the associations which they may have with the fibres which have been described in succession by other authors.

This system of superficial nerve is illustrated by Fig. 15, and in order to demonstrate the difference between them and the internal reticular apparatus, Fig. 16 shows a group of nerve cells from the cerebral cortex in which only the internal reticular apparatus has been differentiated.

As regards reticular apparati, my honourable colleague Holmgren has arrived at really remarkable results which he has illustrated under the title of *Trophosponges* or *Endocellular canaliculi*. Here again, however, we are faced with differing facts...; subsequent study will settle the question.

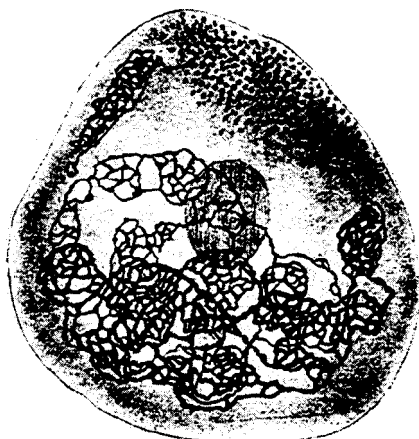


Fig.12

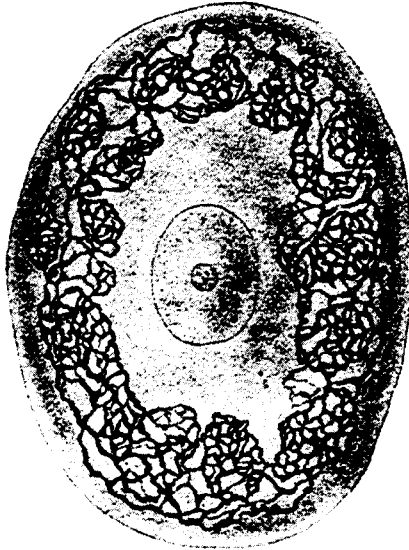


Fig.13

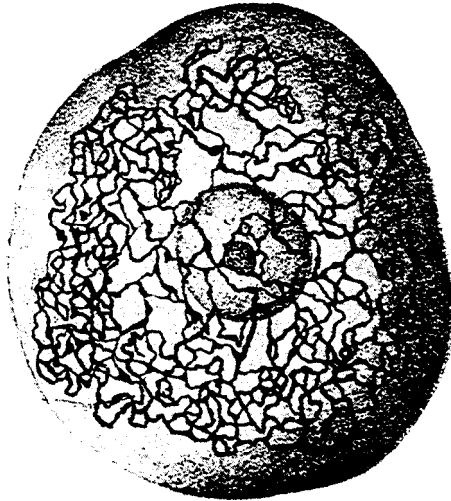


Fig.14

After the work which I have just briefly quoted comes that of Bethe and of Donaggio, who have demonstrated the exquisite fibrillar structure of nerve cells. Using similar methods, based on the use of a mordant before applying the stain, these two last authors have achieved results which are essentially in agreement with each other.

However, Bethe's studies aim at emphasizing the independence of fibrils,

i.e. of supposed paths of conduction, and state that the fibrils may pass directly from one protoplasmic process to another without forming any sort of connection with the interior of nerve cells, while Donaggio tries to prove that fibrils penetrate to the inside of the cells and that the cell body has an extremely complicated and fine reticular arrangement around the nucleus.

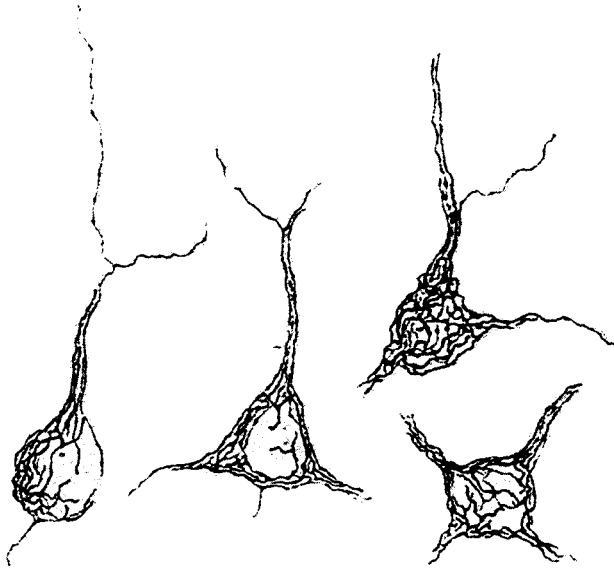


Fig.15

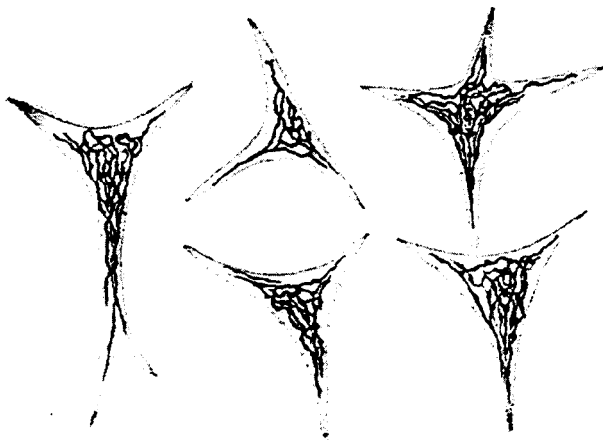


Fig.16

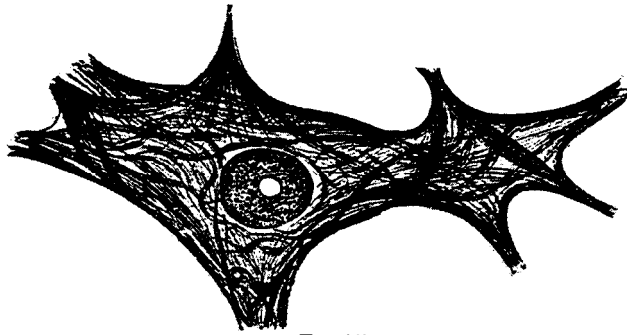


Fig.17

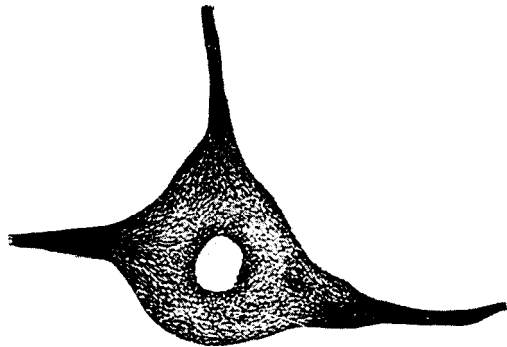


Fig.18

I do not think that the fibrillar or reticular structure which Bethe and Donaggio describe can be identified with the structure demonstrated by Apáthy in the nerve cells of worms; nor do I think that we can at once accept that all the fibrils and reticular structures, and, in particular, the internal structures described by these two authors, represent neurofibrils and nerve networks respectively.

Fig. 17, taken from Bethe's work, shows the structure of a nerve cell from the spinal cord as it appears using the special technique of this author. Fig. 18, taken from a paper by Donaggio, reproduces the fine network structure described by him.

On this subject, and particularly with respect to Donaggio's work, I must recall the words of Jäderholm who expressed his conclusions as follows:

"In my opinion, reticular formations in cells must be considered as artefacts produced by an agglutination phenomenon. These reticular structures can also be simulated by the plasma which, coagulated in the form of a network,



stains at the same time as the fibrils. This happens most often using Donaggio's method, much less frequently with Cajal's and very rarely with those of Bethe and Bielschowsky."

In this order of work done on the detailed structure of nerve cells, Cajal's classic results, obtained with his method of silver reduction, have an eminent place. These results are the finest and most important on this subject, and are to be noted, too, for the ease with which they may be demonstrated.

With Cajal's method one can demonstrate fibrillar structure with such precision of detail that one can see how the fibrils behave in the cell bodies as well as in the processes (Fig. 19). Among the advantages of this method

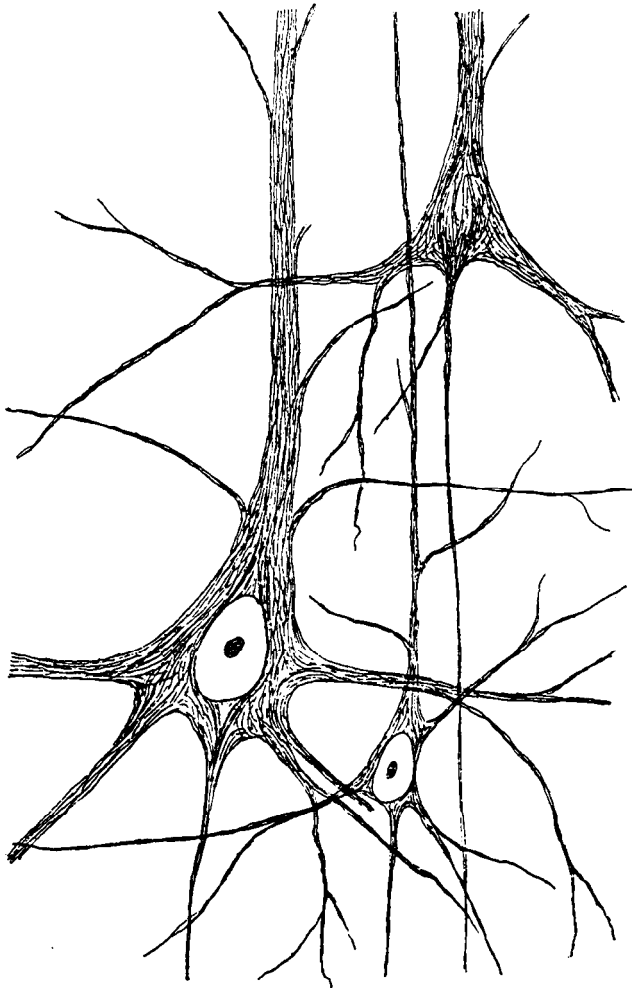


Fig.19

we may note that, unlike any previous method, it makes it possible to demonstrate the fibrillar structure of nerve elements even at the beginning of their development in the first stages of embryonic life.

It is true that one cannot state that the problem of nerve cell structure is solved, even with the help of this method which gives such exceptionally fine results.

I have said that these results do not agree with other results which represent, however, a great deal of certain and indisputable knowledge of the structure of the nerve cell. I would further add that the question of the fibrils, be they nervous or not, of the internal parts of the cells and of the protoplasmic processes is no more answered than the question of the relations between fibrils and the reticular apparatus which, as can be seen in my illustrations, penetrates the protoplasmic processes and extends a long way along them.

Finally, we are no more completely decided on the ultimate destination of the fibrils which travel in the protoplasmic processes.

Even with these results I must repeat that we are perhaps dealing with many roads converging towards the same end, but which, at the moment, have not yet met.

### *3. The neuron is a physiologically independent unit.*

The doctrine of the physiological independence of the neuron, which has been stated to have its origin in Waller's law as well as in the data on the systematic degenerations of nerve centres, has had its expression and its application in the appealing doctrine of dynamic polarization. The proof which people have wanted to find in Waller's law is, in my opinion, completely worthless. In fact, with what foundation could one assert that Waller's law is applicable to cellular unity?

Can one deny that secondary degenerations are connected, just as I have always upheld, with a general effect on more or less extensive territories, of the nervous system? Then again, I don't think one can take Waller's law into account as an argument of any worth against the idea of the existence, through the diffuse nerve network, of intimate connections between nerve cells.

The doctrine of dynamic polarization can be seen to escape any kind of direct physiological analysis; it is only one of the expressions of the physiological concept and, as I have already noted, does not exclude the possibility of there being others.

If the idea of cerebral localization had kept its original form, i.e. that well-defined and distinct sensory and motor functions can be ascribed to carefully demarcated areas of the brain, then the theory of the functionally independent neuron could have found indirect support from studies performed on cerebral localization.

But, today, our ideas on localization have undergone profound modifications.

Without considering experimental data which, by demonstrating the imprecision of the boundaries of central excitatory zones has shown the possibility of substitution and compensation, a prejudicial question arises, at the start, which, from the anatomical point of view, I have asked from the beginning of my work when I demonstrated the existence of the collateral branches of nerve fibres and of functional nerve process of the ganglion cells.

Here is what I wrote on the subject:

<"With regard to physiological deductions, particular consideration should be given to the nerve fibres and fibrils which come from the nerve processes, which always behave in such a manner as to form the most complicated and extensive connections between the nerve fibres, which enter or leave the centres, and the nerve cells."

If one halts, to consider these connections, one becomes convinced that one single nerve fibre may have connections with an infinite number of nerve cells, as well as with completely different parts of nerve centres which may be a long way from each other.

Referring more directly to the doctrine of cerebral localization, I shall not embark here on any analysis, which I have already made in my previous studies, and which is illustrated by a series of plates showing the lack of all the reputed anatomical conditions necessary for giving a basis to the physiological conception of localization, and I shall limit myself here to recalling how I summed up my views on the subject on the basis of those data: "While denying the existence of finely demarcated central zones depicting the exclusive seat of the central distribution of nerve fibres, we feel that we can accept, however, the existence of territories to which *most of the fibres are directly distributed.*"

The nerve fibres coming from, or going to, the periphery would have a more direct and intimate connection with these territories, rather than with those around or at some distance from them, with which they are, however, also associated.

It goes without saying that in speaking of territories with a preponderant

distribution it is understood that these territories slowly merge with other regions where other bundles of fibres prevail. These anatomical observations relating to localization may be expressed almost entirely in a physiological argument.

As for the specific function of the central nervous system, I have, on several occasions, contested that it was correlated with a specificity of organization of the nerve centres, and I have come round to the idea that specific function is not associated with the characteristics of the organization of centres, but rather with the specificity of peripheral organs destined to receive and transmit impulses, or again, with the particular organization of peripheral organs which must receive the central stimuli.

The conclusion of this account of the neuron question, which has had to be rather an assembly of facts, brings me back to my starting-point, namely that no arguments, on which Waldeyer supported the theory of the individuality and independence of the neuron, will stand examination.

We have seen, in fact, how we lack embryological data and how anatomical arguments, either individually or as a whole, do not offer any basis firm enough to uphold this doctrine. In fact, all the characteristics of nerve process, protoplasmic process and cell-body organization which we have examined seem to point in another direction.

We find the same situation regarding the so-called physiological independence of the neuron. Just as we have said regarding the functional mechanism, far from being able to accept the idea of the individuality and independence of each nerve element, I have never had reason, up to now, to give up the concept which I have always stressed, that nerve cells, instead of working individually, act together, so that we must think that several groups of elements exercise a cumulative effect on the peripheral organs through whole bundles of fibres. It is understood that this concept implies another regarding the opposite action of sensory functions. However opposed it may seem to the popular tendency to individualize the elements, I cannot abandon the idea of a unitary action of the nervous system, without bothering if, by that, I approach old conceptions.

The statement I made about the horn of Ammon corroborates, in a most objective way, I should almost say schematically, my interpretation of the action of cells from different regions of the central nervous system.

Thus, as briefly as I could, because of the time and because I do not wish to

submit the kind attention of the assembly to too severe a test, I have covered the field of work which is most closely connected with my subject.

Before I finish, there remains for me to present my most lively and sincere thanks to the illustrious Institute which has judged me worthy of this, the highest distinction the world. I do not dare to attribute this supreme distinction to my personal worth, nor directly to my work, however patient and constantly directed towards scientific research it has been, but I allow myself to believe that it has been ascribed in the not undeserved recognition of the work accomplished by all those who drew from my studies an impulse which was rich in good results. When I think how the modern period of scientific research, whose developments I have had the good fortune to witness, has really enlarged, in a surprising way, our knowledge of the organization of the nervous system, I feel irresistably bound to trust in the fine words of Alfred Nobel, whose mind was so widely open to the highest idealism, who said: "Each new discovery leaves in the brains of men seeds which make it possible for an ever-increasing number of minds of new generations to embrace even greater scientific concepts. My wish is that these new anatomical studies, on which this Institute, in such a high order of thought, has wished to draw the attention of the world, may represent a new element of progress for humanity.