

What we wish more especially to point out is the great need for more thorough work and closer investigation of mental disease associated with organic lesions of the cord, and to point out that the octopus of mental disease, general paralysis, must not spread out one of its arms to include cases of primary organic lesion of the cord with symptoms of progressive mental deterioration such as the above.

*Discussion.*

The PRESIDENT said that he had seen a large number of cases of tabes without any symptoms of general paralysis; but with very distinct forms of mental disease. A most frequent form among the cases that were not general paralysis was the delusion of persecution. He had once seen melancholia with tabes; and also patients with that form of mental disease which went more properly under the heading of primary deterioration. Some cases had not the typical characteristics of tabes, and these were extremely difficult to decide on.

Dr. BAISCOE said he had seen at Guy's Hospital a man who was admitted with unmistakable symptoms of locomotor ataxy, who suddenly became unconscious and afterwards developed the delusion that he was going to be smothered. That patient was removed to Bethlem, where the mental trouble disappeared, although the ataxy remained.

Dr. MACDONALD said that his main object was to stimulate careful and thorough investigation in every case of general nerve break-down. It was possible that they had been induced to look upon the facts recorded in this communication as exceptional, because of the scanty and imperfect references to such cases in the majority of text-books. They did not, however, wish it to go forth that they held that locomotor ataxy and simple mental symptoms such as met with in the case under consideration were the general rule, but they did maintain that cases are recorded under the heading of general paralysis, following or preceding tabes, which are cases of tabetic insanity. In a recent number of the *Journal of Mental Science* such a case was reported, from an Irish asylum, minus pathological or microscopical facts. It would have been easy and convenient to have labelled our case one of general paralysis; but this we did not do. The subsequent history and pathological data clearly support our diagnosis. There was complete absence of fibrillar tremors, which he (the speaker) considered one of the most reliable symptoms of general paralysis.

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*Note on Weigert's Theory regarding the Structure of the Neuroglia.\** By W. FORD ROBERTSON, M.D., Pathologist to the Royal Edinburgh Asylum.

For some years past the leading authorities have been generally agreed that the neuroglia is a tissue composed exclusively of special cells and their processes. Ramon y Cajal, for example, describes it as consisting of small cells provided with very fine, wavy, and only slightly ramified processes, which, after a variable course, terminate freely or attach themselves to the surface of the capillaries. From observa-

\* Abstract of Paper read at the meeting of the Scottish Division of the Medico-Psychological Association held in Edinburgh on the 12th November 1896, and illustrated by a microscopic demonstration.

tions made with the aid of a new method, Professor Weigert, of Frankfurt, has arrived at a theory regarding the constitution of the neuroglia which is diametrically opposed to this now generally accepted view. He published a preliminary note on the subject in 1890, but it was only a year ago that he gave a full account of his researches, together with a description of an important improvement in his method.\* This improved method, in which methyl violet is the staining agent employed, is one that is exceedingly complicated and troublesome to carry out. It gives preparations in which the neuroglia-fibres and the nuclei of the neuroglia-cells are stained violet, while the protoplasm of the cells remains unstained and invisible. The reaction succeeds only in the human subject, and, as a rule, only with tissues that are moderately fresh. The results that he has obtained with this method have led Weigert to advocate the view that the neuroglia-fibres, which have hitherto been regarded as processes of the Deiters' cells, are chemically distinct, and morphologically separate, from the cell-protoplasm; in other words that the fibres which are stained violet in his preparations are not processes of the cells, but are completely differentiated from them. It is clear that we cannot rightly understand the nature of the pathological changes that occur in the neuroglia until this question of the structure of the normal tissue is settled in our minds. Weigert's theory is certain to give rise to a large amount of discussion in the near future. I wish now to offer merely a small contribution to that discussion. I have for some weeks been working with Weigert's method in the course of a study of the normal histology and pathological anatomy of the neuroglia. Already I feel convinced in my own mind that Weigert has been led into an error by certain fallacious appearances produced by his method, and that the older view of the structure of the neuroglia is the correct one.

Before proceeding to state what appears to me to be the case against Weigert's theory I wish to refer for a moment to Ranvier's theory of the structure of the neuroglia. This theory has generally, I think, been understood to be merely that the neuroglia-fibres pass uninterruptedly through the protoplasm of the cells. Weigert, however, interprets it in such a way as to make it identical with his own, that is to say as implying that the fibres do not even pass into the

\* *Beiträge zur Kenntnis der normalen menschlichen Neuroglia.* Frankfurt, 1895.

protoplasm, but are lying outside it, and are altogether anatomically distinct from it. He has attached great importance to the fact that the appearances obtained by his own method thus coincide with those described by Ranvier. Other authorities, however, including Golgi, who have carefully studied preparations made by Ranvier's method, have stated that they are convinced that the appearance of fibrils passing through the protoplasm is due merely to lines of shrinkage produced by the osmic acid employed.

The following are some of the more important considerations that seem to me to disprove Weigert's theory. In the first place the appearances presented by the neuroglia in sections prepared by the fresh method of Bevan Lewis are entirely inconsistent with it, seeming to show with perfect clearness that the fibres are anatomically continuous with the protoplasm of the cells. It is true that the fibres cannot generally be distinguished in the normal human brain by this method, but they can be clearly seen in some of the lower animals, especially the sheep, and they can be readily observed in the hypertrophied state in the human brain. Such preparations show that the neuroglia-cells have an irregular spinous form, and that the fibres are given off only at the tips of the spines. If these fibres had no anatomical continuity with the protoplasm they would surely not have this special relationship to it. In his historical review Weigert makes no reference to Bevan Lewis's work, and appears never to have seen neuroglia-cells in fresh sections.

Again, the appearances presented by the neuroglia-cells in preparations by Golgi's method are equally inconsistent with Weigert's theory. Golgi's method generally picks out only an occasional neuroglia-cell along with the fibres which radiate from it, while scores of adjoining cells and fibres are left unstained. If, as Weigert would have us believe, these cells have no anatomical continuity with the fibres, but form merely centres of radiation for them, it seems impossible to understand why a cell and the fibres radiating from it should display this peculiar chemical identity which results in their being darkened by the silver or mercuric salt to the exclusion of neighbouring cells and fibres. Weigert, as far as I can ascertain, makes no attempt to meet this difficulty in the way of accepting his theory. His argument in opposition to the apparent demonstration of the continuity of neuroglia-cells and fibres furnished by Golgi's method amounts in

effect simply to this, that in preparations by his own method such continuity cannot be seen, and that therefore the pictures given by Golgi's process must be fallacious. It appears to me that on this method of reasoning the positive evidence of continuity furnished by Golgi's method must far outweigh the negative evidence of Weigert's method.

It is true, however, that Weigert does adduce a certain amount of positive evidence in support of his theory. One of the points he most strongly insists upon is that the fibres may often be seen in the form of an uninterrupted thread, which either passes straight across the cell or curves back in the vicinity of the nucleus. I think it may be shown that there are two fallacies here which destroy the otherwise important value which these appearances would have in support of the new theory. Firstly, the fibres which pass straight across the cells can be shown in many instances to be the processes of other cells, and therefore it is possible that they are in every instance to be explained in this way. Secondly, I think it can be shown that the recurving fibres are not continuous fibres at all. In successful preparations by Weigert's method the protoplasm of the neuroglia-cells is invisible. But in preparations in which the reaction is incomplete—as in tissues from the human subject which have not been obtained sufficiently fresh or in which the neuroglia-cells are hypertrophied, or in tissues from the lower animals—the protoplasm is often visible, being faintly stained of a violet or yellow colour, while the fibres at the same time may be quite distinct. The protoplasm can also be demonstrated, even in perfectly successful Weigert preparations, by means of counter-stains, which, however, are somewhat difficult to apply. Now such preparations serve, I think, to demonstrate that the appearance of a recurving fibre results simply from the circumstance that the margin of the cell-protoplasm retains the violet stain, and thus produces a seeming continuity of adjacent fibres. Neuroglia-cells appear to have an envelope which, like their nuclear envelope, is stained violet by Weigert's process. The nucleus is a regular rounded body, and therefore in optical section its membrane appears as a ring. But the cell-body, which usually extends a considerable distance from the nucleus, has an extremely irregular and spinous form, so that its whole outline is practically never seen in one focus. If the cell had been flat then in Weigert's preparations the protoplasm would have appeared limited by a continuous thin violet line running

into the fibres (which would thus appear to fork as they approached the cell) at the tips of the large processes. Now in such preparations, by careful focussing, one can indeed frequently trace this deeply stained membrane almost completely round the cell, and forking of the fibres can be recognised in the vicinity of almost every nucleus. Further, it is probable that the contraction of the cell by the hardening mixture employed may produce in the cell-membrane lines of shrinkage which appear as specially dark bands in the stained preparations. It is exactly along the sickle-shaped edges of the protoplasm uniting adjacent processes that such lines of shrinkage would appear. In these ways I think one can fully explain the appearance of recurving fibres to which Weigert attaches so much importance. It is produced simply by two separate fibres being joined by the stained—perhaps sometimes shrunken—cellular membrane.

Since making the observations upon which the foregoing criticism of Weigert's theory is chiefly based, I have read a paper recently published by Pellizzi\* on the structure of granulations of the ependyma, in which he argues at considerable length against Weigert's views. As the article is one of the first contributions to the discussion that has appeared, I shall briefly state its chief points.

Pellizzi finds that in preparations of granulations of the ependyma stained by Weigert's method, the protoplasm of the hypertrophied neuroglia-cells is stained of a yellow colour, and can thus be easily distinguished. The fibres are much thickened, especially at one extremity, which either bifurcates or forms a membranous expansion, which is coloured somewhat less intensely than the fibre. These specially thickened extremities of the fibres appear often to be placed against a neuroglia-cell. He contends that the circumstance that with this method one cannot recognise direct connections between the fibres and the cell, does not suffice to disprove the existence of such connections. It would be as rational, he says, to deny the existence of the protoplasm of the neuroglia-cell, adducing as a reason that it is not seen. He objects to such conclusions being based upon the appearances obtained by a single method, and says that by using a variety of methods it becomes perfectly evident that the true structure of the neuroglia is such as has been described by Golgi.

\* *Rivista Sperimentale di Freniatria*, 1896, p. 466.

*Discussion.*

Dr. CLOUSTON thought that every attempt, such as Dr. Robertson had made, to find out normal structure would enable them better to understand the meaning of the pathological conditions of the neuroglia. It was generally understood that Dr. Bevan Lewis had made a mistake in looking upon the enlarged neuroglia cells as scavenger cells, and if their normal structure had been better understood he would probably not have fallen into that error. It seemed possible that these cells were for more than merely binding, like guy-ropes, the whole of the other tissues, though such binding cells must be necessary. They now saw how hypertrophy of the neuroglia cells seemed to cause a disturbance of the other structures of the nervous system. He could only say with regard to Weigert's theory that, so far as his examination of the specimens went, he was satisfied that Dr. Ford Robertson's view was correct.

*The Significance of Palatal Deformities in Idiots.* By  
WALTER CHANNING, M.D.,\* Brookline, Mass., U.S.A.

The present is the day when we are searching for any deviation from the normal type, that we may put it down as a sign of degeneracy, a word that is having a more and more extended meaning, and is already serving an evil purpose as signifying more than is actually warranted. Formerly a degenerate was an individual so different physically and mentally from the average, that he could be set off into a class by himself. We knew him when we saw him, because he *was* distinct from the average. Now it needs only some slight imperfection of development in an organ, or tissue, or some slight irregularity of action in the brain as shown in speech, or action, to brand an individual as presenting indications of degeneration.

Science, usually slow to reach conclusions, has been too quick, it seems to me, in accepting fragmentary bits of evidence as proving the whole case. Because departures from the normal are found physically, mentally, and morally in the defective classes, therefore scientific investigators have assumed that any one of these departures, occurring in the average individual, gives rise to the suspicion that the process of deterioration is already under way in him.

While I believe in most carefully noting and investigating anything of a pathological nature, I think we should resist the tendency to attribute so many things to degeneration, until the case is proven. Where is our standard of the absolutely normal? Are we as yet justified in saying anything more than that man is leading an ever changing life

\* Portion of a paper read July 24, 1896, at the Annual Meeting of the Medico-Psychological Association of Great Britain.