

## Correspondence.

"Audi alteram partem."

### DENDRITES AND DISEASE.

To the Editors of THE LANCET.

SIRS,—Sir William Gowers's lecture on Dendrites and Disease<sup>1</sup>—surely one of the most luminous contributions to modern neurology—raises many questions of absorbing interest. To two of these I propose with your permission to make a brief reference.

1. Sir William Gowers assumes that the dendrites of adjacent neurons are separated by, and influence one another through, an intervening "matrix"—i.e., that there is no direct conduction of nerve impulses from one neuron to another—but that a dendrite of one neuron so influences the matrix in which it terminates as to generate a nerve impulse in the adjacent dendrite of another neuron. He, moreover, regards the dendrites as being contractile, and he suggests that the influence which the dendrites of one neuron exert on those of adjacent neurons can be regulated by means of this contractility; when, e.g., they contract firmly all communication between adjacent neurons is cut off, as may happen during sleep and functional anaesthesia; when, on the other hand, they relax neighbouring neurons are brought into functional relation.

I venture to raise the question whether during this dendritic relaxation adjacent dendrites of separate neurons may not merely come into close proximity, but be brought into actual contact, so as to admit of the direct conduction of nerve impulses from one neuron to another, which would be a far more simple process than the induction *de novo* of a nerve impulse in the second cell; and not only so, but by means of this direct conduction it would be easier, one would think, for a particular dendrite of one neuron to affect a particular dendrite of another neuron, *and no other*, than by the less direct method. The great difficulty in the way of accepting this view is the fact that, so far as our actual knowledge goes, nervous impulses are always started in neurons by a *de novo* generation through a non-nervous medium, as in the case of the sensory end-organs; it must, moreover, have been solely through a non-nervous medium that nerve impulses were generated when neurons first evolved in the animal kingdom.

2. The view that the influences passing between adjacent neurons can be regulated by means of the contractility of their dendrites is one which throws an altogether new light on many difficult problems in neurology. The ultimate dendrites of a neuron are probably to be counted by the thousand, and one can conceive how, by a greater or less degree of contraction of this or that dendrite belonging to adjacent neurons, one neuron may be capable of influencing another in a multitude of different ways. Modern histology seems to point to the conclusion that every neuron contains a definite number of fibrils along which nerve impulses are conducted. These impulses are thus compelled to travel along definite, pre-determined routes. The fibrils do not, as Sir William Gowers points out, end in the body of the neuron cell but pass right through it, so that all nerve impulses must start at their extremities—i.e., at the termination of the dendrites or axites. We do not know whether by education the fibrils can be increased in number; quite possibly they cannot be. Now, seeing that the fibrils pass right through the body of the neurons and that nerve impulses must travel along the fibrils, it is obvious that if there were actual permanent union between the adjacent dendrites of neighbouring neurons, all nervous action would tend to be stereotyped, and it would be impossible for the nervous system to acquire the power of executing new combinations except by the development of new fibrils, and, seeing how endless are the new combinations which the nervous system can be taught to execute, this would imply an increase in the number of the fibrils so great as to be physically impossible. These difficulties are removed if we assume that all nervous coördination is effected by an adaptive contraction and relaxation of neighbouring dendrites, for in this way it would be possible to link up in a multitude of different ways particular fibrils of one neuron with particular fibrils of

another neuron, and thus compel nerve impulses to pass along particular routes. On this view the education of the nervous system would essentially consist in the acquisition by the dendrites of special powers of adaptive contractility.

From analogy with telephonic methods, we may speak of this as the principle of "dendritic exchange," and though it does not explain how the dendrites come by their power of adaptive contractility, and of forming new exchanges, yet its formulation seems to be a step towards an ultimate explanation of the well-nigh limitless capacity of the nervous system to make new acquisitions, many of them of a bewildering complex nature.

I am, Sirs, yours faithfully,

Wimpole-street, July 16th, 1906.

HARRY CAMPBELL.

### LEPROSY AND FISH-EATING.

To the Editors of THE LANCET.

SIRS,—Referring to your review of Mr. Jonathan Hutchinson's book<sup>1</sup> and to his recent letter in your columns<sup>2</sup> I shall be obliged if you will permit me to point out a fallacy that underlies at least one of his arguments and invalidates some of his conclusions.

The contention to which I refer is that Christianity—especially the Catholic type, but, in lesser degree, Protestantism also—fosters leprosy by its encouragement of fish-eating as contrasted with other creeds which either forbid, or at least discourage, it. Mr. Hutchinson supports this view by quoting what he considers the excessive proportion of Christians among the inmates of some of the Indian leper asylums visited by him. Had he carried his inquiries a little further he would have found in almost all cases that the Christian lepers, from whose numbers he deduces the prevalence of the disease among Indian Christians, *had become converts after they were lepers*, and that whatever weight they possess as evidence should consequently go into the opposite scale. Let us take a specific instance in which the facts are beyond question. Referring to his visit to the leper asylum at Tarn Taran, Punjab, Mr. Hutchinson writes (p. 106): "On the calculation that in the community there were two lepers per 10,000, the Mussulman population (14,000,000) might have supplied 2800 and the Hindoo population of 13,000,000 2600, whilst the little Christian population of only 72,000 ought to have sent not more than 14; it did, however, to this one asylum alone, which is only one out of five under missionary care in the Punjab, send no fewer than 34. Under the term 'Christian' are here included Protestants as well as Catholics and English as well as natives. If we restrict the calculation to the native Christians, which is only fair, seeing that the Europeans supply no lepers, we should then find that its calculated proportion would be only eight. Very similar statements apply to the Calcutta Asylum, to that of Madras, and to that of Bombay."

Mr. Hutchinson takes for granted that these 34 Christian lepers are contributed by the native Christian community of the district and argues that they are four times as numerous as they should be. The fallacy of this assumption and of any argument based on it is clear from a single fact—viz., that in every one of these 34 cases *the leprosy preceded the Christianity*. Let me support this assertion by the testimony of a witness possessed of actual knowledge. The Tarn Taran Asylum is one of 45 such institutions supported or supervised by the Mission to Lepers in India. For 21 years religious teaching has been provided for its inmates under the care of the Rev. E. Guilford, who is now the superintendent of the asylum, the management of which has been transferred to the mission by the Punjab Government. In a letter just received and referring to Mr. Hutchinson's statement Mr. Guilford writes: "Of those 34, every one had become a leper before he or she was admitted into the Church. I have worked in the Punjab now for nearly a quarter of a century and during that time I have never seen one single case of leprosy amongst our native Christians." This emphatic testimony also rebuts another of Mr. Hutchinson's suppositions—viz., that "the number in any given asylum does not represent the total in the Christian population of the district. Many of the Christian lepers are, no doubt, still at their own homes" (p. 106). Whatever may be the case in those parts of India in which the Christian population is largely Romanist, this is certainly not correct as

<sup>1</sup> THE LANCET, June 16th, 1906, p. 1695.

<sup>2</sup> THE LANCET, July 7th, 1906, p. 45.