Editorial

THE PATHOGENESIS AND TREATMENT OF ACUTE ANTERIOR POLIOMYELITIS

GREAT BRITAIN has been fortunate, as compared with not a few other countries, in not having suffered from any extensive outbreaks of anterior poliomyelitis. The disease is endemic here, and cases occur every month of the twelve, rising in their numbers during the late summer and early autumn; every now and again, too, as the pages of current medical journals show, there are small and limited epidemics clustering in and round some village or town. But so far we have escaped the ravages of the disease such as they have been in Scandinavia, America, or Australia. For that very reason, perhaps, it is well to take stock of the situation, and, indeed, for the sake too of restricted attacks, which, however small they are, can be just as calamitous in their residua.

Of recent years knowledge has much increased in regard to the action of the still unrecognizable virus on the central nervous system. As long ago as 1913 Römer showed that intracerebral injection of the virus was succeeded not by cerebral but by spinal symptoms, and this line of study has been prosecuted with ampler material by Hurst and his co-workers. It may now be regarded as fairly definite that the noxa is a neurotrope in the sense that it turns to the central nervous system, although, like that of rabies and one or two others, it at the same time causes reactions in the mesoderm. Experiment has proved that infection can take place in the monkey by one or other form of inoculation—intracerebral, intranasal, intrasciatic, intrathecal, and intravenous. Whichever route be chosen, the result is the same, though easier to secure by some modes than others. Curiously enough, intranasal inoculation is not always so successful as other forms, yet this is believed to be the method selected by Nature. The course pursued by the virus from nose, theca, sciatic, or cerebrum has been followed with some minuteness, and the general conclusion is that the poison travels along
the lines of cell-fibre systems, i.e. adopts a periaxonal means of spread, en route from periphery to neuraxis or from brain to cord. But this is not the whole story; when, for instance, inoculation is by lumbar puncture, the virus is transmitted by the cerebrospinal fluid system to various places, and filters through ependyma or meninges to gain the nervous system proper. When injection is made directly inside the brain, meningeal reactions do not appear in any major degree—not until, as is claimed, cells begin to overflow from engorged vessels within the cord; but it is self-evident that this particular mode of experimentation does not reproduce the conditions as they occur in the natural disease. When Hurst injected his animals intranasally, in one at least there was severe and widespread infiltration of the meningeal system; and since infection by the intravenous route has proved successful as well, it is clear that the means whereby the virus may reach the nervous system are multiple. On its way from cerebrum to cord it turns aside to enter various ganglion-cell collections at the base, in the brainstem, and elsewhere; it does the same, more or less, when implanted on the nasal mucosa. These considerations should make us hesitate ere assuming that what the experimentalist can do with the virus is the same as what Nature does; she certainly does not choose to start the process within some region of the cerebral cortex or subcortex, inside the sciatic sheath or amid its fibres; the virus has first to overcome the outer barrier of unbroken surface, wherever that be; and if the evidence points to the olfactory mucosa there is nothing in the mass of experimental material to disprove that the fluid system may be implicated as a consequence.

These notions bear on the question of treatment. There has been a tendency of late to assert that treatment by serum of one or other sort is scientifically invalid, as the procedure leaves out of account the ‘fact’ that the virus is already tucked away in neural tissues before treatment can begin, and therefore that the serum cannot reach the spot where the poison is and neutralize it. The logical inference would be that serum treatment is nothing short of bogus. But we believe that the claim is based on mistaken conceptions. When serum is administered intravenously, it is given by the same route as that by which infection can occur, and has occurred, experimentally; when it is inserted into the theca, again it follows a route whereby experimental success has been attained, and no good reason has been offered for contending that the
virus can travel by these channels and its antidote cannot. Enough has already been said to deprecate the habit of applying experimental conclusions to natural processes of disease without due reflection.

The truth is, that the serum method has laboured under difficulties from the very outset, and that these have not as yet been overcome. Individual epidemics have contrasting features; the viricidal content of different sera cannot be standardized; strains of virus and potency of derivative sera must alter from time to time and place to place. Again, in seeking to evaluate its worth we must bear in mind the peculiarities of the disease—of which the most significant in this connexion is the fact that a large number of cases are by nature abortive, partial, or nonparalytic; and there is also the question whether every case treated has belonged to the poliomyelitic class. Hence adjudication on the merits of serum treatment has, so far, ended in little else than conflict of opinion. A good deal has been heard of 'control methods'; in the New York epidemic of 1931, according to figures supplied by Park, 519 cases were treated with serum, and 408 without; and the result would appear to be that no merit could be found on the part of serum technique in either reducing case fatality or preventing later palsies. Park's figures have been quoted far and wide as though they closed further discussion, but they have been submitted to stringent criticism by Dame Jean Macnamara in a recent issue of the Medical Journal of Australia. She says in so many words: 'Dr. Park's workers freely admitted that no one bothered to make certain that the donors [of convalescent or immune serum] had had poliomyelitis, and the patients labelled preparalytic were not examined to exclude paralysis'; moreover, she states that Dr. Park 'naively admits that the two groups [serum-treated and non-serum-treated] were not comparable. "There was accidental inclusion of a somewhat larger number of graver infections in the treated cases".' Macnamara's results have been better than those secured by any other worker; among 133 cases submitted to serum treatment there have been only seven failures. At the very least, then, empiricism (if it be that) has made out a prima facie case for the use of convalescent or immune serum in natural poliomyelitis. No doubt there are other objections; the method is sometimes looked at askance because whatever the therapeutic channel the results seem to be much the same. But at all events less should be said of absence of statistical proof. Dame Jean Macnamara raises a protest against this frequently
repeated 'argument.' She has not been able to find that any therapy used in medicine or surgery has been so tested. The use of diphtheria antitoxin does not rest on statistical proof, and it is recognized that some diphtheritic patients recover without specific treatment. The surgeon knows that some cases of acute appendicitis are cured without the knife, but he does not ask for a series of patients treated with and without operation to guide him. We echo Dame Jean Macnamara's words with approval: 'The physician's place in clinical medicine is to afford the patient the best protection against potential danger that he knows. In the case of poliomyelitis diagnosed before paralysis has developed, it is an adequate injection of immune human serum.'
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