

NOTE ON THE FOLLOWING ARTICLE

Although the following article is written from a medical standpoint, it deals with a matter of practical importance to economic entomologists, and presents the first experimental data bearing on the hypothesis¹ that poliomyelitis (infantile paralysis) may be carried by *Stomoxys calcitrans*.

Since it appeared, Anderson and Frost² of the Public Health and Marine-Hospital Service have reported similarly successful results in transmitting poliomyelitis among monkeys, by repeating these same experiments with *Stomoxys*. They have further been able to induce the disease in a third monkey by inoculation with an emulsion of the spinal cord taken from a monkey that developed the disease after being bitten by infected flies, thus proving the diagnosis in the case of the fly-bitten monkeys. (Ed.)

SOME EXPERIMENTAL OBSERVATIONS UPON MONKEYS CONCERNING THE TRANSMISSION OF POLIOMYELITIS THROUGH THE AGENCY OF *STOMOXYS CALCITRANS*, A PRELIMINARY NOTE.³

By M. J. ROSENAU,

Professor of Preventive Medicine and Hygiene, Harvard Medical School,

and

CHARLES T. BRUES,

Bussey Institution, Harvard University.

The work we are about to report was done for, and under, the auspices of the State Board of Health of Massachusetts.

We should like to have it distinctly understood, and therefore emphasize the fact right in the beginning, that this announcement is to be considered as a preliminary report, for the work is still

¹ Brues, C. T. & P. A. E. Sheppard, The Possible Etiological Relation of Certain Biting Insects to the Spread of Infantile Paralysis. Month. Bull. State Board of Health of Massachusetts, Dec. 1911, pp. 338-340 and Journ. Econ. Entom., Vol 5, pp. 306-324 (Aug. 1912).

² Anderson, J. H. and Frost.

³ Reprinted from the Monthly Bulletin of the State Board of Health of Massachusetts, Vol. 7, n. s., pp. 314-317. (September, 1912).

in progress. Certain results have been obtained which it seems advisable to announce at this juncture. In taking this action in announcing work before it is completed we have not assumed the sole responsibility, but have taken counsel with older and wiser heads, friends for whose judgment we have the highest regard.

When we first took up the study of this disease—infantile paralysis—with the State Board of Health of Massachusetts, we considered all possible modes of transference of the virus from the sick to the well, but gradually focussed our attention upon the fact that the disease seemed to be spread rather directly from person to person. In other words, the disease appeared to us at first blush to be a “contagious” disease, but one in which mild or abortive cases, missed cases, and third persons probably played an important rôle in the transfer of the infection. We were probably prejudiced in favor of this viewpoint on account of the splendid work of Wickman, whose publications we studied with care. We were further influenced to regard poliomyelitis as a “contagious” disease owing to the views of Flexner, who compared it to epidemic cerebro-spinal meningitis, and who regarded that it spread in the light of a contact infection through the secretions from the mouth and nose. The analogy to meningitis was a very close one, and the experimental fact that the virus could be demonstrated in the nasal mucosa of monkeys (Osgood Lucas and others) seems to corroborate the suspicion that we are in fact dealing with an infection spread very much as cerebro-spinal meningitis is spread.

If these assumptions were correct, then the virus should be demonstrable in the secretions from the nose and throat. Rosenau Sheppard and Amoss therefore injected 18 monkeys with the nasal and buccal secretions obtained from 18 persons who were suffering with the disease at the time, or in the stage of convalescence, or from persons suspected of acting as carriers. These results were negative. At the same time Straus of New York had a series of negative results, and other American workers were also unable to find the virus where we assumed it should be. These negative results seemed to us to have positive significance, and was the first definite indication that we were upon the wrong trail.

That poliomyelitis is not a “contagious” disease was clearly brought out by Dr. Richardson and other observers who have

spoken this morning,¹ all of whom have emphasized the point that the disease shows little or no tendency to spread in crowded districts, in schools, in institutions, in asylums, in camps and in other places where one would expect a disease spread by contact through secretions of the mouth and nose to spread most readily. We have in mind the fact that many cases of the disease have been brought into asylums and hospitals throughout the State of Massachusetts, in all stages of the infection; yet secondary cases have not occurred under such circumstances. On the contrary the disease prevailed in Massachusetts more particularly in rural and country districts sparsely settled.

Another reason that led us away from the theory of contacts, and made us believe that we were not dealing with a contagious disease in the ordinary sense of that term, was the close analogy between rabies and poliomyelitis. All investigators in laboratories who have worked with these two viruses have been struck with the similarity between rabies and poliomyelitis. Both viruses are diffused widely throughout the body, both exist in special concentration in the central nervous system, both are filterable, etc. Rabies being a wound infection made us conjecture that poliomyelitis may also be similarly transmitted.

Our experience with yellow fever, perhaps more than anything else, influenced us concerning the probable mode of transmission of poliomyelitis. It had been the privilege of one of us to work with yellow fever before and after the mosquito days, and many analogies came to mind which made us believe that poliomyelitis also was not a contagious disease.

All the various reasons that influenced us in turning from contagion to some other mode of transference need not engage our attention now, for the history of this part of the work has been ably and accurately given by Dr. Richardson in the paper which he has just read. In justice to Dr. Richardson we desire to state that all the essential conclusions of his paper were arrived at before he knew of the results in the laboratory with the monkeys.

The work which we now briefly desire to report consists in exposing monkeys during all stages of the disease to the bites of *Stomoxys calcitrans*. The monkeys were infected in the usual way

¹ Before the International Congress of Hygiene and Demography, at Washington, D. C. September 27, 1912.*

by bringing an emulsion of a known virus obtained from human sources in direct association with the central nervous system. After the flies had had abundant opportunity to bite these infected monkeys during the various stages of the disease, including the period of incubation, healthy monkeys were then exposed to the bites of these same flies. Of 12 healthy monkeys indications of the disease have been obtained in 6, 3 of them in a virulent form, resulting in death, the other 3 with transient tremblings, partial paralysis, diarrhoea and recovery. It is interesting to note that several of the monkeys had diarrhoea, therein the disease resembles the human disease more closely than when monkeys are simply inoculated with the virus into the brain, for gastro-intestinal upsets in children are frequently associated with infantile paralysis.

In these experiments it is important, we think, to use the proper technic in order to obtain successful results. The flies should be handled as little as possible, It is much better to handle the monkeys and leave the flies alone. In our experiment the flies were caught in nature, some of them were bred, placed in a large cage about 6 feet long by 5 or 6 feet wide, and some 3 or 4 feet high. The monkeys are stretched out at full length and wrapped in chicken wire. In this way they can be placed in the cage and the flies have full opportunity to bite. The flies appear to need a feed of blood about every day or two. They sometimes visit water which is kept in the cage, but apparently cannot be induced to eat any other food than the blood. At least, in our experiments, bananas, fruits and other substances exposed apparently were little visited by the flies. Furthermore, in our experiments a very large number of flies were used.

In conclusion we desire simply to summarize the fact that we have apparently transferred the virus of poliomyelitis from monkey to monkey through the bite of the stable fly, *Stomoxys calcitrans*. We would like to emphasize the fact that this does not appear to be simply a mechanical transference, but rather a biological one, requiring a period of extrinsic incubation in the intermediate host.

What conclusions can we draw from these facts? At present it seems to us we would not be justified in drawing any conclusion—the significance of the facts if confirmed¹ is self-evident.

¹As previously noted (p. 191), these experiments have been confirmed by Anderson and Frost.



Hindawi

Submit your manuscripts at
<http://www.hindawi.com>

