Correspondence.

FERMENT-VIRUSES AND PATHOLOGICAL HÆMETABOLY.

TO THE EDITOR OF "THE JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—May I briefly draw attention to two passages noted in papers I have recently seen, which bear, respectively, upon my general theory and upon the special case of the "Rickettsia"-bodies.

In an instructive, critical review of present knowledge relating to herpes febrilis and encephalitis lethargica, Doerr and Zdansky (Zs. Hyg., 102, April, 1924), write as follows in their concluding remarks (p. 47, et seq.):

"It appears not impossible that the ultimate true solution of the problem of encephalitis lethargica has been wanting hitherto because the microbiologists working thereon have had under consideration in a too one-sided manner a contagium animatum as the necessary and sufficient cause of this disease. The reason for this standpoint has lain in the assumption invariably made that an infective agent, transmissible from animal to animal, can only be a living, parasitic organism, a supposition which, however, as Doerr has several times pointed out, is not necessarily in all circumstances right." (The authors do not appear cognizant of my work, and the views I have first enunciated and consistently advocated in this Journal since 1921.)

Doerr and Zdansky refer to certain most interesting experimental work by Frey, Fuchs, and Jahnel and Illert, in various directions, to outline which here, however, would occupy too much space. But the observations cited are regarded by the authors, if they are subsequently confirmed, as pointing unmistakably in the direction of a non-living, transmissible virus or materies morbi, which would be of the nature of a cytotoxin and might, indeed, be regarded as a ferment, the resulting contagious diseases being an enzyme-infection. (This ferment I have preferred to regard, on my evidence of abnormal hæmetabol, as, primarily, a pathogenic hæmetabolic enzyme, which eventually induces cytolysis also.) The authors even recognize the possibility of a type of process such as I indicated in the case of influenza, where the primary disease may be due to a micro-organism, but its transmission can be subsequently effected by the corresponding cytotoxin (ferment) produced by the injured cells in the course of their dissolution, without the further intervention of the actual organism (cf. JOURNAL OF THE ROYAL ARMY MEDICAL CORPS, vol. xxxix, October, 1922, p. 258). This hypothesis the authors apply to the case of the experimental encephalitoid conditions, as to which I can as yet say nothing. I hope, all being well, to take up the study of the alleged Microsporidian parasite, "Encephalitozoon," myself.

Now, as regards the "Rickettsia"-bodies, I quote the following from
a paper by Wolbach and Schlesinger on the cultivation of the microorganisms of Rocky Mountain spotted fever and typhus fever in tissue-cultures (Journ. Med. Research, xliv, December, 1923, p. 254): "The number of infected [endothelial] cells in all cultures has been disappointingly small. It is evident that cells containing the micro-organisms [i.e., the "Rickettsias"] die, and yet we have been unable to obtain any evidence that the organisms multiplied or even persisted in the plasma. There seems to be no mechanism for the passage of the micro-organism from cell to cell, and we are forced to the conclusion that the cells containing the microorganisms in tissue-cultures are cells originally infected in the animal body, or descendants of such cells." Well, in the first place, since the cells containing the organisms die (as a result of their infection with the parasites), it seems extremely unlikely that they (the cells) thrive and multiply, and give rise to fresh descendants! Again, what a hopeless kind of organism this "Rickettsia" must be! Has anyone ever heard of such a microbe, unable to spread from one cell to another and maintain and propagate itself, given the presence of its susceptible host-cells? ! Really, I think further comment is unnecessary. Scarcely anything could more strikingly indicate that the alleged organisms are merely products of abnormal endothelial cell-metabolism! For some reason, under the conditions prevailing in the artificial culture, either the pathogenic enzyme is neutralized or destroyed extra-cellularly, or else the cells can obtain no blood-elements to metabolize in the particular abnormal manner, with further production of "Rickettsia"-bodies.

May 20, 1924.

I am, Sir, etc.,
H. M. Woodcock.

TREATMENT OF MALARIA.

TO THE EDITOR OF THE "JOURNAL OF THE ROYAL ARMY MEDICAL CORPS."

SIR,—A proprietary preparation of foreign origin has been vaunted of late as a specific in malaria infections. Medical officers recently returned from abroad have cited cases of malaria resistant to ordinary quinine-iron-arsenic treatment, which yielded in a most gratifying manner to a course of this secret remedy. The other day a lady patient told me of its miraculous effect in "curing" her own infection in India. Her persistent relapses of malaria since returning to England have left this touching faith unshaken.

A quantity of this preparation submitted to the Hygiene Department, Royal Army Medical College, for analysis, is reported on as under:—

"The pills were coated with a black layer and weighed on an average 0·293 grammes each.

1 The normal cells lived all right in the cultures.