Correspondence.

"Audi alteram partem."

THE PELLAGRA OUTBREAK IN EGYPT.

To the Editor of THE LANCET.

SIR,—In a paper appearing in THE LANCET of May 8th under this title Dr. J. I. Enright gives an account of pellagra amongst German prisoners of war in Egypt with observations upon the food factor in the disease. This contribution is of great interest with respect to several clinical points and very important in its bearing on the fundamental question of the ætiological nature of the disease. It is this latter phase alone that I desire briefly to consider. As the results of his study in relation to the causation of the disease he concludes (p. 1003) that a "food deficiency" can hardly be considered as of paramount importance. In his view "a critical analysis of the diets which these German pellagrins ate before capture and during their period of captivity prior to the onset of the eruption is sufficient to explode the food-deficiency theory as a predisposing factor. These diets were ample, both in quantity and quality, for any possible requirements, and were of such varied composition that it is perplexing to understand how they could have been improved." If, as Dr. Enright reports, these German pellagrins actually consumed before capture and during their period of captivity prior to the onset of the eruption diets that were ample both in quantity and quality for any possible requirements, then it must be granted that the "food-deficiency" theory is, indeed, exploded. Evidently the question upon which the validity of this conclusion depends is: "Did these prisoners actually eat the adequate diet that Dr. Enright believes they did?" Let us examine the facts presented by Dr. Enright and see.

First of all it may be well to note that the great majority of the 7000 to 8000 German prisoners, amongst whom a total of 65 cases are said by Dr. Enright to have occurred, were captured either in September or early November, 1918. The monthly incidence of the disease is stated to have been as follows: January, 13 cases; February, 11 cases; March, 12 cases; April, 14 cases; May, 4 cases; June, 2 cases; July, 7 cases. Certain details with respect to 32 of the 65 cases are presented in a table, from which it appears that in 2 of these 32 cases the period of captivity before onset of the rash was 2½ months; in 3 it was 3¼ months; in 13 it was 4 to 4½ months, and in 14 it was 5 to 6½ months. No information as to this interval is given with respect to the remaining 33 cases, with the exception of two German East African cases, who are stated to have been in Maadi Camp, Cairo, for 13 months prior to the onset of the rash. In the absence of any statement of any shorter period it would seem fair to assume that the shortest period of captivity before the onset of the rash was that given in his table—namely, 2½ months. This is important, and should be kept in mind in relation to the points next to be considered.

On the theory of "dietary deficiency" there must, of course, be some period of deprivation before the clinical onset of the disease. As to the duration of this period the available knowledge is unfortunately very meagre. In the experimental cases in Mississippi convicts' mild subjective symptoms were definitely recognisable before the end of the second month, but the rash did not begin to appear until the close of the fifth month. This experience, however, is probably not altogether typical, for certain more recent observations by the writer, made in association with W. F. Tanner (unpublished), make it highly probable, if not certain, that this period may be as short as three months. Under these circumstances it becomes evident that the diet to be studied in relation to the cases of pellagra amongst the German prisoners is that which they ate after, not that before capture. When we attempt to do this we find that Dr. Enright gives us no account of the diet actually

eaten by the pellagrins during the period subsequent to their capture and prior to the onset of the pellagrous rash. He does, indeed, give an account of the diet of the pellagrins before capture, but the "diet after capture" is simply that with which the prisoners in camp and hospital were presumably furnished, not what those who developed pellagra had consumed. Dr. Enright evidently assumes that the pellagrins ate the diet as furnished. In connexion with a point of such crucial importance, however, this assumption is not permissible, as general experience with the vagaries of racial and individual taste in relation to foods, methods of preparation, seasoning, &c., and the literature of scurvy and beri-beri impressively teach. Interestingly enough, however, Dr. Enright himself, quite unconsciously, presents evidence that strikingly illustrates the danger of fallacy involved in the assumption under consideration. He states (p. 999) that "two [of the patients] had had symptoms of scurvy before the rash [of pellagra] appeared." Now, if Dr. Enright's assumption is allowed in relation to pellagra, it must, of course, be allowed in relation to scurvy; it would thus follow that a "food deficiency" can no more explain the origin of scurvy than it can, according to Dr. Enright, explain the origin of pellagra in these men, and consequently the "food-deficiency" theory of scurvy must be considered as "exploded" equally with that of pellagra. Far from being "exploded," however, the food-deficiency theory of scurvy was never more solidly established than at present. It follows, therefore, that the diets allowed the German prisoners were (1) not "ample both in quantity and quality for any possible requirements"; or (2) not distributed as prescribed; or (3) not eaten as furnished them.

Since the number of cases of scurvy and of those of pellagra were relatively very few, it is fairly certain that the diet allowed was adequate so that such faults as developed were probably the result either of improper distribution or of individual eccentricities of taste or, perhaps, of both. In this connexion it may be recalled that prior to capture these German prisoners are said to have lived on "the fat of the land," so that it seems not at all improbable that at least some of them did not find the prison camp diet furnished them after capture physiologically adequate, though it may have been altogether to their liking, leading such either to reduce their normal total intake or to reject certain, to them, objectionable (because of character, flavour, seasoning, mode of preparation, &c.) articles. That a "food deficiency," leading to such condition or conditions as scurvy, war oedema, beri-beri, or pellagra, should thus arise is no more than might be expected, particularly in such as might at the same time be suffering from a depressing illness or mental aberration.

From the foregoing considerations it is evident that Dr. Enright's conclusion that the food-deficiency theory of pellagra is exploded is not well founded, and it follows, further, that the "toxin hypothesis" which Dr. A. D. Bigland² felt it necessary to advance in explanation, seemingly, of Dr. Enright's cases, would appear to be superfluous. In this connexion brief reference may perhaps be made to Dr. Viswalingam's paper³ mentioned in the editorial on "Pellagra and Diet" in THE LANCET of May 8th (p. 1019). It appears that from his observations Viswalingam was "led to conclude that faulty diet in itself cannot cause 'pellagra,' and that there is a superadded infection." For, he argues, "if diet should be the sole factor, then the affected individual should improve, and there should be no recurrence when he is removed to a hospital and placed on a liberal diet, &c." "But," he goes on, "this is not so, as shown by cases that were kept in hospital for considerable periods and in which the disease, far from showing any improvement, advanced and the cutaneous lesions and gastro-intestinal disturbances were seen to recur at each succeeding season." One might accept this reasoning as perhaps conclusive if (1) the "liberal diet" is a biologically complete and adequate one, and, more important still, if (2) being "placed on" such a diet and actually

¹ Goldberger and Wheeler: U.S. Public Health Reports, Nov. 12th, 1915, and U.S. Hygienic Laboratory Bull., No. 120, February, 1920.

² THE LANCET, May 1st, 1920, p. 953.

³ Journ. Trop. Med. and Hyg., Feb. 19th, 1920, p. 46.

consuming it could be assumed to be synonymous. That this assumption can be allowed only as a broad general proposition and that it may fail in individual instances has already been sufficiently indicated in connexion with the essentially identical assumption by Dr. Enright. In the light of the results of large-scale experiments designed to test the preventability of pellagra by means of an appropriate diet,⁴ I may, perhaps, be permitted the suggestion that Dr. Viswalingam's unsatisfactory results may be accounted for on the ground either that this "liberal" diet was not really an adequate one, or, more probably, that the patients did not eat it. They may not have consumed this diet either because it was not given or because the patient refused some parts (poor nursing), or, finally, because he was dealing with complicated cases (evidences of dysentery, ankylostomiasis, malaria, and scurvy were present in at least 60 per cent. of his cases), or cases so far advanced that recovery was impossible. In this connexion it may be pointed out that the value and the significance of the results of the therapeutic test of diet is much inferior or, more properly, must be differently gauged from that of the preventive test. In evaluating the significance of the results of the former allowance must be made (exclusive of complications) for the varying degrees of severity and extent of the existing damage from the disease, so that recovery in the majority of (severe) cases is all that can, perhaps, be reasonably expected; in prevention, however, assuming proper control of the diet and of the pellagrin, practically complete eradication may with confidence be expected.

It follows from the foregoing that Dr. Viswalingam's hypothesis of a "superadded infection," like Dr. Bigland's "toxin hypothesis," has been invoked without substantial grounds, and like the latter must be regarded as superfluous.

I am, Sir, yours faithfully,
JAS. GOLDBERGER,
Surgeon, U.S. Public Health Service.

Washington, June 5th, 1920.

CARRIAGE OF MALARIA BY HIBERNATING MOSQUITOES.

To the Editor of THE LANCET.

SIR,—I have been reading Colonel S. P. James's most interesting book on "Malaria at Home and Abroad," which has just been published, and I should like to correct an error which appears on p. 14. He says that in the Balkans I showed that 40 days were required for the malaria parasite to attain maturity in the mosquito at a temperature of 62°F. I conducted a number of experiments with mosquitoes and malaria, but in none did I find such a long period of development, except in cases where the latter was intentionally arrested by exposing the mosquitoes to hibernating conditions. Large numbers of mosquitoes were dissected from Lehanah village, where there was a high malaria rate amongst the children and where the prevalent mosquito was *Anopheles superpictus*. These dissections were carried on through the summer and winter of 1918-19, and an interesting result was that partially developed oöcysts were found in mosquitoes all through the winter. The oöcysts appeared to be normal, and the question arose as to whether it would be possible for such oöcysts to continue their arrested development when temperature conditions became more favourable.

In the Macedonian village the anopheles pass the winter most usually in the barns or cow-houses below the living-rooms of the natives. In these the temperature is considerably higher than in the empty barns, where mosquitoes are more rarely found. During the winter the mosquitoes are constantly feeding on the animals. Experiments were instituted, and it was found that in mosquitoes which had been fed on malaria cases and then incubated for a week the development of the oöcyst could be completely stopped by exposure to a temperature corresponding to the winter hibernation temperature of the mosquitoes. After three weeks'

artificial hibernation the oöcysts continued their development when the mosquitoes were again incubated. It is thus quite possible, if not probable, that the oöcysts which I saw in the hibernating mosquitoes collected in the village, and which resembled in every way those seen in the experiment, would be capable of completing their development in the early part of the year when the temperature became sufficiently raised.

The experiments I carried out seem to prove that the malarial parasite can be carried over the winter by the hibernating mosquitoes. Exposure of infected mosquitoes with partially developed oöcysts to a low temperature in the ice-chest for 12 hours did not produce degeneration of the oöcysts, whereas a longer exposure did do so. It would seem that in nature a temporary exposure to a very low temperature on an exceptionally cold night would not prevent the subsequent development. I think it is possible that Colonel James has obtained the figure of 40 days from some of these hibernation experiments. I call attention to the carriage of malaria by hibernating mosquitoes because of its importance in antimalaria work, for it emphasises again the advisability of destroying the hibernating females during the winter. My experiments will be fully described in a paper which will be published later.

I am, Sir, yours faithfully,

Wellcome Bureau of Scientific Research, C. M. WENYON.
Endsleigh-gardens, Gordon-square, N.W., June 23rd, 1920.

VARIOLA, VARICELLA, AND ALASTRIM.

To the Editor of THE LANCET.

SIR,—In the annotation in your issue of May 29th entitled Anomalous Cases of Small-pox, a list of characteristics is given which are said to differentiate the disease called "alastrim" from varicella on the one hand and from variola on the other. It is not stated where or when the observations which must underlie these formal distinctions were made, or on what authority the claim that they serve to differentiate alastrim from varicella and variola is based. To enter upon a detailed criticism of each feature in the list would take up too much space, but considering the supreme importance of the accurate differentiation of the small-pox rash from other rashes of similar type, it seems to me to be remarkable that the value in differentiation of observations of the distribution of the rash is not even suggested. Is it to be assumed from this omission that the distribution of the rash in alastrim cannot be distinguished from the distribution of the rash in varicella or in variola? Or is this factor in diagnosis considered to be of minor importance in comparison with those given in the list? In either case it is desirable that the omission should be rectified and that any observer who has had the advantage of opportunities of comparing alastrim with varicella on the one hand and with variola on the other should set down the results, whether positive or negative, of his observations. Dr. S. Monckton Copeman in the last sentence of his letter in your issue of June 5th says: "In both outbreaks the symptoms and general course of the disease more nearly resembled those of alastrim than of typical small-pox," but he makes no reference to the existence of a distribution sign as a factor in diagnosis, and although I had the advantage of hearing the interesting paper which he read before the Epidemiological Section of the Royal Society of Medicine, I failed to appreciate that he had observed any differences in the features of the distribution between the cases which he cited and the cases of true variola which were associated with his cases in the Norfolk and Suffolk epidemic.

If I seem to lay undue emphasis upon this factor in diagnosis my excuse is that in the London Receiving Station and in this hospital, following the teaching of my predecessor, the late Dr. T. F. Ricketts, we have always regarded the features of distribution as of paramount and really overwhelming importance in the examination of suspected rashes of focal type. It may therefore be understood that a list of tests in which this factor finds no place must fail to inspire confidence in anyone who has appreciated Ricketts's teaching or has personal know-

⁴ Goldberger, Waring, and Willett: U.S. Public Health Reports, 1915, vol. xxx., p. 3117, and unpublished data.