While a final opinion as to the significance of the changes cannot be given at this time, the staining reactions in wet-fixed films indicate that they are all degeneration forms of *E. tetragena*. A fuller report, with drawings, will appear later.

It is believed that these findings in the cat establish a correlation between the findings of SCHAUDINN and CRAIG on the one hand, and those of VIERECK, WERNER, HARTMANN, and others on the other, and it emerges that *E. tetragena* is the common pathogenic entamoeba of man, as HARTMANN has already stated, and that "*E. histolytica*" is, in all likelihood, a spurious species, having been described from degeneration forms in senile races of *E. tetragena*.

THE NATURE OF ZAMBESI FEVER.

BY

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I should like to submit to the Fellows the following notes in extension of the paper on the above subject, which was accepted by the Society and read on the 19th May, 1911, in which I pointed out that I thought a mistake had been made in assuming that Zambesi fever was malaria.

When I took over medical charge of a sugar estate on the lower river, employing from 500 to 2,000 recruited Kaffir labourers, in addition to a staff of a score of Europeans, and about a dozen Eurasians and negroes, I found that there were always a number of Kaffirs in hospital with fever, bowel complaints, and debilitating conditions of obscure nature, and when I had obtained a stock of thermometers and temperature charts, and trained the attendants to take temperatures, found that the two latter complaints were also associated with fever, so that it was no exaggeration to say that all our medical cases in hospital were fever cases.

The Kaffirs being in proportion to staff as fifty to one, at first one saw nearly all the cases among them, but I gradually came to the

conclusion that the staff suffered from the same forms of sickness, that it was all one disease against which the West Indian negroes were not protected as compared with the Europeans.

If this proves to be the case we may reasonably hope, in course of time, to devise prophylactic measures which will render the Zambesi valley a healthy district, but if the fever of that country is calmly assumed without proof to be malaria, there is a danger that not only will there be no improvement, but that anti-malarial measures may be discredited among laymen.

At the time of writing the paper, I had examined the blood of something over 150 fever patients, without finding parasites (I expected to find spirochaetes), and I had not seen a decidedly malarial type of temperature curve, so I rashly suggested that the district was not really what is usually called malarial; but I admit now that I was misled by the protection given to Europeans by various forms of prophylaxis, and to Kaffirs by immunisation in childhood. The death-rate among children was very high, and when the Kaffir mothers knew me well enough to trust the infants to me I found that those with fever usually shewed the *plasmodium malariae*.

This disposed of my doubts as to my own ability to detect the parasite. I have looked over some of these positive smears and again searched some of the negative smears since arrival in England, and feel confident that I did not pass as negative more than a very small percentage, if any, that contained the parasite. After sending off the original paper I examined 148 smears from Kaffirs, all taken before any quinine had been administered, and all admitted into hospital for fever, and not including casual examinations of out-patients. In two I found ring form parasites, but no crescents. Of 109 in which I made a leucocyte count, 50 shewed a large mononuclear proportion of over 12 per cent., 11 quite 20 per cent., and obviously an increase without counting. The two patients whose blood shewed the parasites were well in a day or two, and most of the others were much longer, and shewed no evidence of being affected by quinine. The quinine was given in a solution of the bisulphate ten grains to the ounce, which I made myself and frequently saw administered and swallowed. The shrinkage of the bottle was compared with the amounts ordered on the bed-cards, and I do not think my hospital attendant shirked that duty.

Another reason why I missed the malaria at first was that there were very few newcomers among the Europeans; but in 1911 a number arrived and lived in the place at the same time as about thirty coloured and three white Mauritians, who were new arrivals, and then it was interesting to note that among the Europeans there were several attacks of fever which shewed the malaria type of chart and quickly recovered on the exhibition of quinine, whereas among the Mauritians who usually treat fever at home with a dose of castor oil and a day in bed there were none, but that quite two-thirds of them went down one by one with the long fever that I call Zambesi fever.

One of them, who was ill a long time, was an old man, who told me that he had had no fever since he was a youth, and that what they had was quite a different sickness.

Dr. C. F. HARFORD's remarks on the occasion of the reading of Sir W. LEISHMAN's paper in October, reminded me how these people had said: "We have no fever like this in our country, Doctor!" and how indignant they seemed that they had been brought there under the impression that it was their old enemy from which they thought they were pretty thoroughly immune.

For the purpose of serum test, 24 specimens of blood were submitted to the bacteriologist at the Government laboratories, Johannesburg, of which four gave a definite and two a feeble reaction with *mic. melitensis*. Two of these samples were taken at an interval of about ten days from a European case I had diagnosed as not malaria, and probably undulant fever. Both were positive. The others were Kaffir cases. This patient went home to Switzerland when convalescent, and informs me he has never had any illness since then. He had suffered in a vague way for some month or two before he had to lie up.

All the Europeans who arrived in my time, to the best of my belief, took five grains of quinine bisulphate daily, or most days. Some were never ill, but many one day developed an attack of fever. They had usually taken an extra ten or fifteen grains before I saw them. One of two things happened; either the temperature was of the malaria type, or normal too soon to shew any type and the patient back to work in one to five days, or the fever lasted a long time and shewed no signs of being affected by quinine, and no mid-day rise nor two-day periodicity, but a regular evening rise as in phthisis.

This long fever often cleared up for a day or two and relapsed again, and with the onset of the relapse there was often diarrhoea and vomiting, but a noticeable point was that the vomiting occurred with a clean tongue and without nausea, which is quite unlike the condition in malaria. Some would eat with good appetite and vomit the whole contents of the stomach soon after. In malaria when there is vomiting the tongue is always coated and appetite lost.

That I failed to find malaria parasites in the blood of the European cases that were clinically malaria is not surprising, because they had all been taking quinine, and does not affect as evidence the fact that I failed to find them in the case of hundreds of Kaffirs where I obtained the smear before any quinine had been taken, at the same time that I was recognising them in the blood of Kaffir children, especially as the charts of those Kaffirs are not at all like malaria charts.

I do not know whether there is more than one non-malarial fever classed popularly under the name Zambesi fever, but I feel it my duty to impress on practitioners all over Africa, that in the lower Zambesi valley at the present day the important disease is not malaria, because we can so easily prevent and control that disease but this other fever, because we do not at present know its etiology nor how to treat it, and it is of such long duration and leads to a great deal of invaliding and a certain number of deaths.

I should like to add that in the early part of 1911, I had hardly any staff, white or native, to attend for about four months, so I carried out an experiment. I stopped my prophylactic quinine and in two months had an attack of fever, which shewed a temperature curve in which alternate days corresponded, the second, fourth and sixth being higher. I also noted that the fever increased gradually from early in the day and not abruptly in the evening.

On the fifth day I took ten grains of quinine bihydrochloride and observed no effect. On the sixth, twenty-five, and the fever ended abruptly, like that of lobar pneumonia. I continued a daily five grains (bisulphate) and had no recurrence.

I believe this attack was malaria, which reacted promptly to quinine, because I have a good resistance, the result of severe and repeated attacks years ago, before we understood much about the disease. But the Kaffirs would have a better resistance, and as a matter of fact the two in whose

blood I found parasites were cured in 48 hours, whereas the other cases mostly appeared to be unaffected.

Now I wanted to demonstrate a contrast. Here was a whiteman who tried to get malaria, satisfied himself that he had got it, and by the exhibition of ten and twenty-five grains of quinine brought it to an end with dramatic suddenness; and there were some hundreds of Kaffirs, dosed with 20 grains a day, going through a long and exhausting fever associated with lymphatic inflammation and nerve pains (probably the "malarial neuritis" that one hears of).

Can anyone assert that their fever was malaria in the face of negative blood examination?

Personally, I do not believe that those Europeans who die on the voyage home to England, after treatment by experienced practitioners in West Africa for prolonged fever, ever die of malaria, and feel convinced that a little careful observation of such cases would satisfy medical men in that part that they have to deal with the same disease which we call Zambesi fever on the east side.

THE ETIOLOGY OF BLACKWATER FEVER.

BY A. G. NEWELL, M.D., D.P.H., LAHORE.

I have read with much interest the paper (and the notes of the discussion thereon) on "The Etiology of Blackwater Fever," by Sir WILLIAM LEISHMAN, F.R.S. and in view of the fact that he has called for "a free interchange of opinion on the debatable points," and this subject being one of the greatest importance to medical men practising in highly malarious districts, I should like to make the following remarks:—

On page 3 of the November, 1912, issue of the TRANSACTIONS Sir WILLIAM groups the various hypotheses as to the etiology into four groups. In my opinion, the first is the only one which represents the nearest truth, and the only one so far which has not been contra-verted by unquestionable proof. To say that "undoubted cases have