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Research into the Trypanosomiasis Problem:

A Critical Consideration of Suggested Measures.

With Discussion and

Report of the Glossina Sub-Committee.

BY

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(Paper read at a Meeting of the Royal Society of Tropical Medicine and Hygiene, Friday, October 15th, 1920).

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RESEARCH INTO THE TRYPANOSOMIASIS PROBLEM: A CRITICAL CONSIDERATION OF SUGGESTED MEASURES.

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Six years have elapsed since the subject of trypanosomiasis was last discussed by this Society. During this period comparatively little has been done on the subject, but since the war ended several very important papers have appeared, and further investigations are now contemplated by both the French and ourselves. I feel, therefore, that it is now opportune to review the present position, and consider briefly the direction which further research should take.

If we refer for a moment to the position shortly before the war, we remember that as a result of a considerable polemic which arose out of certain observations made by Kinghorn and myself in Northern Rhodesia, and by the Royal Society Commission in Nyasaland, the Secretary of State for the Colonies appointed an Inter-Departmental Committee, the terms of reference of which were as follows:—

To report—

(1) Upon the present knowledge available on the question of the parts played by wild animals and tsetse-flies in Africa in the maintenance and spread of trypanosome infections of man and stock.

(2) Whether it is necessary and feasible to carry out an experiment of game destruction in a localised area in order to gain further knowledge on these questions, and, if so, to decide the locality, probable cost, and other details of such an experiment, and to provide a scheme for its conduct.

(3) Whether it is advisable to attempt the extermination of wild animals, either generally or locally, with a view of checking the trypanosome diseases of man and stock.

(4) Whether any other measures should be taken in order to obtain means of controlling these diseases.

This Committee examined numerous witnesses, and considered written statements from various foreign experts and others who were unable to attend in person. In May, 1914, it issued a report, the general conclusions of which are as follows:—

Knowledge of the disease, its cause, and its remedies, is still in the making, and hasty and imperfectly considered action of a drastic character, such as the attempt to effect a general destruction of wild animals, is not justified by the evidence before your Committee. On the other hand, your Committee recommend that until direct means of checking the fly have been discovered, the food supply of the fly and the chances of infection should be lessened in the vicinity of centres of population and trade routes by the removal of wild animals, and that for this purpose freedom be granted both to settlers and natives to hunt and destory the animals within prescribed areas and subject to prescribed conditions.

So far as regards the disease in Uganda, the measures already taken have effectually checked the epidemic, and removed the mass of the population from the danger of further infection. While, no doubt, it is desirable that the land lying near Victoria Nyanza should be rendered again available for the use of that population, this is not a question of immediate urgency, and may well await the acquisition of further knowledge.

With regard to the Nyasaland and Rhodesian form of the disease, its incidence on the population is slight, and it is not increasing. The evidence points to its being an old disease, endemic and not extensive, and though it is unsafe to prophesy, there is no apparent reason to anticipate its appearance in an epidemic form. Having regard to the importance of the question whether man forms a reservoir of the human trypanosome, your Committee would lay emphasis on the desirability of futher experiments, as suggested in paragraphs 41 and 42 above. (See also Appendix D.)

It must be recognised that the evidence all points to the conclusion that, if tsetse-fly could be eliminated or removed from contact with human settlement, sleeping sickness would practically disappear, infection conveyed by other biting flies being a negligible factor in the spread of the disease.

For this reason your Committee attach great importance to a proper and sufficient equipment of entomological research into the bionomics of the incriminated tsetse-flies. This form of research has, in their view, been insufficiently pursued up to the present time. The workers have been zealous, but few in numbers, and the work consequently limited to only a very small portion of the fly-belts and areas from which the danger arises.

Different views are taken as to the prospect of dealing with the fly, but it was, as your Committee think truly, said by more than one of the witnesses that in this form of research there is a large element of chance—that accident may at any time lay bare a secret which may lead to the solution of the problem—and that the multiplication of workers is the multiplication of those chances.

Your Committee think, therefore, that, within reason, there should be devoted to this form of inquiry a considerable portion of such funds as may be available in British Possessions, and that endeavours should be made to obtain the co-operation in this work of Foreign Powers in their African Possessions, the results of the work being from time to time tabulated and collected.

Research will, no doubt, be continued as to the nature of the different trypanosomes, and the part they play in the infection of man or of domestic stock.

The proposed experiment of removal of wild animals from a selected area may produce valuable results, both as regards knowledge of the habits of the fly, and as to the extent to which the infectivity of the fly, and subsequently the infection of man or stock, is derived from the wild animals.

As has been pointed out, the result of this experiment cannot be confidently anticipated. There are possible fallacies and uncertainties involved from the very nature of the problem, and in dealing with natural conditions there is always the possibility of unknown factors vitiating or defeating action based on the apparent results of any such experiment.

Nevertheless, your Committee think that there is sufficient to justify an expectation of useful results, and they recommend that if a suitable locality can be found, where an experiment can be carried out at a reasonable cost, it should be undertaken. They are, however, of the opinion that the carrying out of the other measures recommended should not be delayed pending the results of the experiment, which cannot be expected to emerge for two or three years.

It is perhaps needless for your Committee to say that they hope that medical research as to treatment of the disease and the production of immunity will be continued.

The above recommendations relate mainly to the acquisition of knowledge on which further action may be based. As regards immediate action, your Committee strongly recommend that measures of clearing should be undertaken where they are practicable and would tend to check the spread of the disease, and render life in settlements and travel by road safe for men and stock.

This question has been fully dealt with already, and it is only necessary to say here that your Committee attach much importance to this matter.

Your Committee would point out that the action in various directions which they have suggested is for the general benefit of every part of Africa in which tsetse-fly exists, and is or may be the cause of sleeping sickness, so that so far as the cost would fall on local funds they should not fall only on that Possession which may be selected for any particular experiment, as being that in which the knowledge sought is most likely to be attained. It is even possible that Foreign Governments might be willing to share in the cost, but this is hardly a matter on which it is fitting that your Committee should make any substantive recommendation. Nor is the question of a contribution from public funds one for them to deal with, though it is, they think, doubtful whether local funds would be sufficient to bear the costs of the various inquiries and experiments which they think desirable.

Action on this report was necessarily deferred owing to the outbreak of war; other work and other diseases monopolised attention, and research into trypanosomiasis practically ceased. It has now become apparent that the ravages of sleeping sickness have increased during the war in certain of the French equatorial colonies, and also in portions of the Belgian Congo. This knowledge caused the Société de Pathologie Exotique to appoint a committee, under the presidency of M. LAVERAN, to consider what means should be taken to deal with the situation. This Committee reported in July to the Société, and its report was accepted and forwarded to the Colonial Minister, and also to the Governors-General of the French Colonies in Africa. About the same time our Secretary of State for the Colonies took action on the report of the Earl of Desart's Inter-Departmental Committee, and asked the Imperial Bureau of Entomology for recommendations. The matter was referred by the Bureau to its Glossina Sub-Committee, which subsequently issued a report containing certain recommendations for future action.

Now, the different procedures adopted in France and in this country seem to me to be a matter not without interest to this Society. In France the Société de Pathologie Exotique took action and advised their Government; in this country the Government has to seek for advice, and does so, not from this Society but from the Imperial Bureau of Entomology. One cannot help feeling that herein lies a moral which this Society should take to heart. Surely on matters relating to tropical medicine, in all its various aspects, a Society like ours, which contains amongst its hundreds of members, medical men, veterinarians, entomologists and zoologists from all quarters of the earth, should be able to crystallise its ideas, and advise the Government accordingly. If this Royal Society is to be a living force, and not a mere agglomeration of academic pedants, it should be able to function in this manner on questions of great medical and economic importance.

In this paper I propose to refer briefly to the report of the Société de Pathologie Exotique, and then to discuss in more detail the report and recommendations of the Glossina Sub-Committee of the Imperial Bureau of Entomology. It is important to realise at once that the problems which confront us are, in many respects, different from those with which the French have to deal.

In the French equatorial colonies the most urgent need of the moment is the control of the epidemics of *gambiense* sleeping sickness, which are ravaging certain parts of them. The primary prophylactic measure recommended by the French is the atoxylisation of the sick, with the object of destroying the flagellates in the blood of all infected cases. Marvellous results are to be anticipated from this procedure, provided a sufficient personnel be available for some years in all infested districts, so that all the infected could be recognised and treated. With reference to measures directed against the tsetse, the report states that one can hardly dream of causing the complete disappearance of the Glossina, which swarm on the banks of most of the streams of equatorial Africa, but measures can be taken to remove the natives and to protect them against bites. Clearing has been declared by all observers one of the most useful measures which can be practised. There is room for research to ascertain whether we cannot destroy Glossina by opposing them with other insects or animals which are their natural enemies. It is recalled that pigs are particularly attractive to Glossina, and it is suggested that possibly herds of these animals, and of other resistant beasts, could be grouped at certain places in the vicinity of the villages in such a manner as to form a sort of protecting screen. As the spread of sleeping sickness is due to the incessant movement of porters and travellers, and as the disease is aggravated by want, excessive porterage, and fatigue, strict administrative control is necessary. The manner in which the above recommendations should be applied, and the organisation necessary, are discussed in detail at the end of the report.

Although in none of the British colonies is there at the present time any very serious epidemic of *gambiense* sleeping sickness, we have in Central and South-Eastern Africa to deal with a situation, which in its way is just as serious, and at least as difficult of solution, as that which confronts the French in their equatorial colonies. Fortunately, the amount of *rhodesiense* sleeping sickness occurring in Central and South-Eastern Africa is at the present time small, but the question of trypanosomiasis of domestic stock is one of the greatest economic importance. This fact is clearly indicated in the opening paragraphs of the report of the Glossina Sub-Committee, which reads as follows :—

> Some idea of the importance of the tsetse-flies of the *Glossina morsitans* group as an obstacle to the development of Tropical Africa may be gathered from a consideration of the wide areas over which they range. In Southern Rhodesia, in which the fly-belts are comparatively small and have been more carefully surveyed than elsewhere, their extent has recently been estimated at 9,000 square miles. With reference to North-Eastern Rhodesia, Messrs. KINGHORN and MONTGOMERY have expressed the opinion that it would be difficult to find a continuous area of fifty square miles free from *G. morsitans* anywhere except on the Serenje plateau, around Fort Jameson, and on the high plateau between Lakes Nyasa and Tanganyika. In Nyasaland probably more than a fifth of the area is infested by tsetse-flies ; while in Tanganyika Territory the fly-belts must cover nearly half the country.

The Report of the Glossina Sub-Committee consists of three portions : the first summarises very briefly the present position as regards the various tsetse-flies ; the second gives a list of suggestions for observations and experiments ; and the third makes detailed recommendations concerning the organisation necessary for the elucidation of the foregoing problems. It is to the second and third portions of this report that I desire to draw your attention.

It is impossible here to give a list of all the suggestions for observations and experiments, but they deal with the following points : (1) the distribution of the fly, (2) the effects of clearing, (3) breeding-grounds, (4) food of the fly, (5) destruction of adults, (6) parasites, (7) enemies of the fly, and (8) influence of odours. It will be observed that the subject which was the main one before the Earl of Desart's Inter-Departmental Committee in 1913-14, and which was really the cause of the appointment of that Committee, viz., the inter-relationship of game, tsetse, and the pathogenic trypanosomes of man and domestic stock is completely ignored. This is the more remarkable as the report of the Glossina Sub-Committee was called for directly as the result of the report of the 1913-14 Inter-Departmental Committee. However, I will not go further into this matter at the moment, but proceed to discuss the methods by which the Glossina Sub-Committee propose to investigate the problems which they themselves have decided it is desirable should be solved. The following are the recommendations :—

> For the elucidation of the foregoing problems it seems desirable that there should be at least six different experimental stations, widely distributed over Tropical Africa, and the countries that suggest themselves as being most suitable for the purpose are Zululand, Southern Rhodesia, Nyasaland, Tanganyika Territory, the Sudan and Northern Nigeria.
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> The selection of the precise areas in these countries that would afford the best facilities for these investigations would require a more intimate knowledge of local conditions than the Sub-Committee at present possesses. But it may be laid down generally that the various areas should be representative of different types of environment, and that they should, as far as possible, be situated in localities in which the presence of tsetse-flies is actually obstructing settlement, or is likely to do so in the near future.
>
> Each of these stations should be under the control of a competent entomologist, and he should have at least one assistant who would be able to carry on the work in the event of the senior man falling sick or going on leave. Unfortunately, at the present time there are very few men who combine the requisite qualifications and experience for this class of work. This difficulty could be met by starting, in the first instance, say, two stations only. In these the new men could be concentrated for training purposes. After a time, the most promising pupil could be left in charge, and the others would proceed to open up a new station elsewhere.
>
> While the observers in each station would be expected to keep an alert watch on all aspects of this complex problem, it would probably be advisable for some of the stations to give special attention to certain lines of work, such as control by clearing, artificial breeding-places, rearing of parasites, etc.
>
> Each station ought to send in a monthly progress report to the Imperial Bureau of Entomology, so that the Glossina Sub-Committee may be kept advised of all the latest developments. They

would thus be in a position to co-ordinate the work, to prevent overlapping, and to arrange promptly for the testing under divers conditions of any new hypothesis that may be put forward.

As the work progresses, it is probable that special lines of inquiry will become necessary, for which the aid of a protozoologist or veterinarian may be required, and provision should be made for such eventualities.

It will be observed that what, in brief, the report recommends is (1) that there shall be set up at least six different experimental stations in widely-separated places in Tropical Africa; (2) that each of these stations should be under the control of a competent entomologist, who should have at least one assistant who would be able to carry on the work in the event of the senior man falling sick or going on leave; and (3) that each station should send in a monthly report to the Imperial Bureau, which would then be in a position to co-ordinate the work

Now the sum of £50,000 (if such a sum can be raised) has been mentioned as the cost of this organisation over a period of five years. A brief calculation will, however, shew that such a sum, which allows for an annual expenditure by each experimental station of £1,700, is totally inadequate for the purpose. Such an amount is hopelessly insufficient to provide for salaries, transport, camps, labour, equipment and leave of the personnel of the Commission; there will be no balance for the purpose of experiment and investigation, to say nothing of the many other incidental expenses inseparable from such undertakings, and the possibility of any experimental work requiring the employment of labour is completely excluded.

The first criticism I have to make, therefore, is that if the above recommendations are adopted, a much greater sum than £50,000 must be provided.

Let us, however, assume for the moment that money is a matter of no consequence, and proceed to consider whether such an organisation as this is likely to succeed in obtaining the information which will enable the Imperial Bureau to answer the questions which it enumerates within a period of five years.

It is admitted that "unfortunately at the present time there are a very few men who combine the requisite qualifications and experience for this class of work." In order to get over this difficulty it is proposed to start with only two stations and concentrate the "new men" in these for training purposes. Now the task of training these "new men" would obviously be a lengthy one; ill trained men who were ignorant of the literature of the subject, which is already rather extensive, and who know neither Africa nor the native or his language, could hardly be expected to advance knowledge of this most complex subject very much. If four of the proposed stations, therefore, require to be staffed by "new men," nothing very valuable in the way of information is to be anticipated from them for a number of years.

Again, if we assume that this difficulty can be surmounted and sufficient well-trained entomologists can be found within, say, a year, to provide each of the six experimental stations with its full staff of one competent entomologist and at least one assistant, would such an organisation prove adequate to the task which is required of it? If allowances be made for such contingencies as sickness and furlough, it is clear that during a considerable proportion of the five years there would be in each station only one entomologist—a condition of affairs not far removed from that which has obtained during the past ten years.

Realising these facts, we must ask ourselves whether it is probable that the solution of the problems enumerated in the report would be obtained within any reasonable space of time by such an organisation. Let us for a moment consider several of the more important problems which it is proposed that the one or two men in each of these widely scattered stations are expected to investigate:—

1. What are the precise factors that determine the limits of a fly area?
2. In the open "orchard bush" so much frequented by *G. morsitans* is it sufficient to clear away shrubs and branches of trees to a height of twelve or fifteen feet, or must trees of twenty to thirty feet be felled?
3. What is the minimum width of cleared belt that would effectively check the spread of the fly?

4. Can the number of fly be materially reduced by judicious burning of grass at times when food is scarce and shelter limited?
5. Compare the results obtained by indiscriminate clearing and by the clearing of breeding places after these have been defined.
6. Test the possibility of introducing additional parasites of different kinds from remote parts of Africa.

These are doubtless important questions, and their solution and that of the others enumerated in the report might enable us to formulate some scheme for dealing with the problem, but what I wish us to consider is whether it is reasonable to expect that one or two entomologists, unable by lack of sufficient funds to dispose of more than a very limited amount of native labour, are likely to succeed in providing answers to them.

My general criticism of this portion of the report is, then, that the organisation which is recommended is entirely inadequate for the work which it is expected to perform. The solutions of the very questions which the report states require elucidation can be obtained only by intense and concentrated effort and not by the adoption of such a scheme as the one we have just considered, which involves dissipation of energy and precludes possibility of experimental investigation on a large scale. The adoption of the measures recommended in this report will result in failure not only to elucidate any practical solution of the problem, but to advance knowledge in any considerable degree; the report is, in fact, a proposal to perpetuate as regards research the state of affairs which existed before the war.

These remarks, however, do not complete my criticism of the report of the Glossina Sub-Committee; as I have already stated, the main question which was before the Earl of Desart's Committee, viz., the relation of game, fly, and trypanosomiasis of man and domestic stock is completely ignored. The work which immediately preceded, and which was the direct cause of, the appointment of Lord Desart's Committee demonstrated that the main, and for practical purposes the only, reservoir of the pathogenic trypanosomes of man and domestic stock is the big game. So far as I am aware, nobody has criticised the results obtained by KINGHORN and myself in the Luangwa Valley, and by the Royal Society Commission in Nyasaland, in so far as they relate to the pathogenic trypanosomes of domestic stock. Now it is important to recognise that, judged from the economic point of view, trypanosomiasis of domestic stock is at present in Central and South-Eastern Africa immeasurably more important than is trypanosomiasis of man. On this account alone it seems to me that the effect of game elimination in a definite zone should be decided once and for all by a carefully designed and controlled experiment.

The observations by KINGHORN and myself that 16 per cent. of the game in the Luangwa Valley were infected with *T. rhodesiense*, the trypanosome pathogenic to man, although quickly confirmed by the Royal Society Commission in Nyasaland, met with the most hostile criticism on all hands. While most of this criticism was based on hypotheses and speculation, and was therefore of no value, some of it was founded on scientific observations, and requires careful consideration; I shall refer to it later.

The question whether game really forms the main reservoir of the trypanosome pathogenic to man is not of mere academic importance, as a glance at the report of the Conference on Sleeping Sickness, held at Pretoria on 9th March, 1920, will shew. This Conference was called by the Minister of Public Health in connection with an application to the Union Government by the Natal Coast Labour Recruiting Corporation, Ltd., for the removal of the prohibition in respect of recruitment of native labourers for the sugar estates from the country between the Limpopo and Zambesi Rivers—also with the recently reported extension southward of tsetse-fly and the infection of sleeping sickness. Unfortunately, time does not allow one to record in detail the findings of this Conference. The following are, however, among the most important.

It was unanimously agreed that it would be dangerous to allow recruiting for the Natal Sugar Estates in the belt of country between the Limpopo and the Zambesi Rivers, or in any area where there is any risk of an infected native being recruited.

In regard to the question, "What are our present southern limits of sleeping sickness infection, and what are the risks of the introduction of the disease into the Union?" it was decided by the Conference that under the present conditions the risks of introduction of infection were undoubtedly very great, and that both as regards the Zululand-Mozambique border and infected areas in Rhodesia, the existing arrangements for safeguarding against introduction of sleeping sickness are totally inadequate.

After discussion, it was agreed :—

> That the Zululand-Mozambique border should be closed for all native movement inwards, except at two ports of entry, namely, Nduma and Maputa—this not to interfere with the existing system of visiting passes for local natives. Medical examination at the border would be very difficult to enforce, and would be unreliable even if supplemented by a microscopic examination of the blood. A prolonged period of medical observation, with blood examinations and animal inoculations, is necessary in order to exclude the disease.

This resolution is followed by others relating to the strengthening of police patrols on the border the provision of penalties for infringement of regulations, and the issuing of certain additional regulations.

I have ventured to refer to this report at such length because it bears most directly upon the subject under discussion. It will be seen that the resolutions are of a rather drastic character, and interfere in a greater or less degree with the economic life of the community.

Now the deliberations of this Conference and the resolutions it passed are based on the assumption that man constitutes the main reservoir of the trypanosome which affects man in these localities, viz., *T. rhodesiense.* If this be not so, and the game be the main reservoir of the virus pathogenic to man, then the above recommendations, with the resulting economic disturbance and the expenditure involved in putting them into force, are futile.

I will now review briefly the evidence on which those who deny the identity of the human parasite with that of the same appearance found in game base their contention.

(1) *Epidemiological.* It has been asserted that these two parasites are undoubtedly different; because in certain *morsitans* areas, the animal trypanosome (*T. brucei* vel *T. ugandae* vel *T. pecaudi*) is found in game and stock and in tsetse-fly without cases of human trypanosomiasis occurring.

(2) *Experimental inoculation of the human subject with the game trypanosome.* As it is upon these observations that most faith is placed, it is necessary to refer to them in detail. In 1913 TAUTE fed upon himself, with negative result, laboratory-bred *G. morsitans* which had been rendered infective with the *rhodesiense*-like game trypanosome; he also subsequently inoculated himself with 2 cc. of the blood of a dog naturally infected with the same trypanosome, and here again the result was negative; these experiments were carried out at Lubimbinu, in Portuguese East Africa. Much more impressive, however, is a later series of experiments performed in 1919 by TAUTE and HUBER. They inoculated themselves and 129 natives, many of whom were in poor condition from malaria and other causes, with the *rhodesiense*-like trypanosome from four naturally infected horses and two mules. In no instance was an infection obtained in man, although the animals (rats, dogs and a goat) used as controls all became infected and died. Naturally, the greatest importance has been attached to these experiments, which by many are regarded as conclusive in shewing the human and game parasites are not the same.

While I am quite prepared to admit that at first sight this conclusion may seem correct, I think, if we consider the facts more closely, it will become clear that KLEINE and TAUTE have not proved their point, and that their observations are quite in harmony with the view that the two trypansomes are identical. To my mind the various facts which have been elicited regarding human trypanosomiasis in South Central Africa can best be explained on the view that the game is the great reservoir from which man (as well as his domestic animals) becomes infected. To develop my argument, I find it necessary to commence with the postulate that man is extremely resistent to infection with any member of the genus Trypanosoma under natural conditions, or, in other words, that he possesses great natural immunity to infection. This postulate is, however, no mere baseless hypothesis, but is supported by considerable evidence. In view of the fact that man has never been found infected with *T. congolense*, and only on a single occasion with *T. vivax*—pathogenic trypanosomes which are exceedingly common and of universal distribution in Tropical Africa—it is evident that man must possess a considerable degree of natural immunity to them. Again, certain facts regarding *T. gambiense* are in harmony with this postulate. If man be readily susceptible to infection with *T. gambiense*, it is difficult to account for the absence of sleeping sickness in an epidemic form in the West Coast Colonies and in certain other parts of Tropical Africa. The cause of the epidemic in Uganda has recently been discussed by DUKE, in a most able paper, and explained by him on the ground of a mechanical transmission by *G. palpalis* from man to man of a strain of *T. gambiense* of increased virulence. It is a well recognised fact that passage of *T. gambiense*, or of any other African trypanosome, in the laboratory, from animal to animal greatly increases its virulence for that species of animal; for example, the strain of *T. gambiense* maintained by us in Runcorn was at the time of its isolation from the human being of very slight virulence for rats, these animals living, on an average, over a hundred days, but after the strain had been passed through these animals for a number of years its virulence for them had so increased that they died in about fourteen days. This increase of virulence is, of course, artificial and does not take place when the parasite is passed alternately through the vertebrate and invertebrate hosts; but, as DUKE has pointed out, an analogous increase in virulence probably also takes place in nature when conditions are favourable—that is, when there is a broad and intimate contact between man and fly. Such conditions were found in the densely-packed canoes on the fly-infested shores of Victoria Nyanza, the fly acting the part of the syringe in the laboratory and conveying infected blood mechanically from one native to another.

To return now to *rhodesiense* sleeping sickness, how can we explain its distribution otherwise than on the assumption that man possesses great natural immunity? The more its distribution is investigated, the more does it become apparent that the infection is distributed more or less uniformly over an enormous stretch of country in Central and South-Eastern Africa. Now, if infected man were the reservoir from which other men become infected, one would naturally expect to find a more or less considerable focus of the disease in every locality where an infected case is found. This, I think, is not so; the total number of cases of *rhodesiense* infection which have as yet been discovered is still rather small, but the figures, such as exist, suggest that the disease is widely and uniformly distributed, allowances, of course, being made for differences in the density of *G. morsitans*. I hold, therefore, in direct contradistinction to KLEINE, that what epidemiological facts we have at our disposal suggest that man becomes infected from the widely diffused game reservoir rather than from other infected men.

Turning now to TAUTE's remarkable experiments on the human subject, let us consider what exactly he has proved. In my judgment, all that TAUTE has proved is that man is very resistant to infection. I believe that if, in his first experiment, TAUTE had fed his laboratory-bred flies on an infected man, instead of on monkeys and antelopes, he would have obtained a similar result; whereas if, in the second experiment, his inoculations had been made from the blood of an infected man, instead of from infected horses and mules, the result might well have been different, because of the exaltation in virulence of the strain for the human being, which might manifest itself as the result of passage through two human hosts consecutively without an intermediate passage through the invertebrate host. TAUTE's results, far from militating against the hypothesis that the game is the reservoir from which man is infected, are only what would be expected if the hypothesis is correct. KINGHORN and I shewed that in the Luangwa Valley one in five hundred flies was infected (in an infective condition) with the *rhodesiense*-like trypanosome, and similar figures were afterwards obtained by the Royal Society Commission in Nyasaland. Under these circumstances large numbers of human beings must be bitten every day by an infective fly, and yet cases of human infection are very few. The only conclusion we can draw from this is that man is remarkably resistant to the trypanosome in question; consequently, on the assumption that the game trypanosome is the same as that found in man, many more than 130 persons would have to be used in experiments such as that conducted by TAUTE and HUBER before one could expect to obtain a positive result.

Before leaving the subject, I should like to draw attention to another aspect which has apparently been entirely overlooked by those who, like KLEINE and TAUTE, deny the identity of *T. rhodesiense* with the trypanosome of the same appearance found by KINGHORN and myself in game. It is clear that a large proportion of the game must be infected with *T. rhodesiense.* The Royal Society Commission in Uganda, in 1910, succeeded in infecting all of eleven antelopes by feeding upon them laboratory-bred *G. palpalis*, experimentally infected with *T. gambiense* from a case of sleeping sickness. FRASER and DUKE (1912) found that the antelopes so infected remained in perfect health for over a year, and DUKE later recorded that he was able to infect *G. palpalis* from an antelope infected experimentally with *T. gambiense* twenty-two months previously. WECK (1914) records that in German East Africa he inoculated successfully an antelope with blood from a human being infected with *T. rhodesiense*; the antelope shewed no signs of disease, but its blood was infective for monkeys and dogs five weeks later, and there appears no reason for doubting that the blood of this antelope would also have infected *G. morsitans;* nor is there any ground for doubting that if antelope can be infected directly from the human being, they could also be infected from one another. This being the case, it follows that antelope must become infected with *T. rhodesiense* one from the other by *G. morsitans* precisely in the same way as they are infected with the common pathogenic trypanosomes of domestic stock.

I maintain, therefore, that the game constitute the main reservoir from which *G. morsitans* draws the trypanosomes pathogenic to man and his domestic stock. For practical purposes the game constitute the only reservoir, because man, domestic stock, monkeys and the small vermin rapidly die of the infection, whereas the antelope are tolerant, and harbour the parasites in their blood presumably for long periods without exhibiting signs of disease.

There seems to me, therefore, to be a clear indication that any investigation which is contemplated with a view to enable us to deal with the problem of trypanosomiasis in South and Central East Africa should have as its primary objective the obtaining of precise information concerning the inter-relationship of game, fly, and the trypanosomes pathogenic to man and stock.

Information on two questions, both of great importance, might be expected to emerge from a carefully conducted and controlled experimental elimination of game from a prescribed district. These are :—

1. The effect on the tsetse-fly.
2. The effect on the trypanosomiasis of man and domestic stock.

May I at this point refer for a moment to the great game drive which took place in Zululand in August, 1920, wherein 500 hunters are said to have taken part, and to have been aided by an army of 5,000 natives acting as beaters. The area comprised in this drive is stated to be the size of Cornwall and Devon. Unfortunately, I have no information regarding the organisation of this great drive, beyond the brief reference which has appeared in the daily press, but I should like to point out that, unless it was prece ded by a thorough and scientific investigation of the conditions, both in respect of fly and of the trypanosomes of game, man and domestic stock which existed before the drive, and unless it is followed by an equally careful investigation extended over a sufficient length of time, we shall have no precise information regarding the results of what must have been an unparalleled slaughter of game. I sincerely trust, therefore, that those who organised this drive had this fact in mind, otherwise the failure to take advantage of an unrivalled opportunity would be deplorable.

To sum up, I am of opinion that the report of the Imperial Bureau of Entomology is open to serious criticism, and that the recommendations contained in it should not be put into operation. I hold this view, firstly, because the report evades what I regard to be one of the main problems requiring immediate investigation, namely, the dependence of fly and trypanosomiasis on the game; and, secondly, because its recommendations concerning the method by which the problems, which the report itself considers to be of primary importance, should be investigated are wholly inadequate for the purpose, are little if anything in advance on the methods adopted in the past, and are calculated to result in failure at the cost of much money and time; in short, these recommendations appear to me to indicate a lack of appreciation of the gravity and difficulty of the problem demanding investigation.

In contradistinction to the recommendation of the Imperial Bureau of Entomology, I would suggest (1) that in future investigation effort should be concentrated instead of dissipated; (2) that the work of entomological and medical and veterinary research into the trypanosomiasis problem be combined under one central organisation in Africa, and that such organisation be supported by the pooled contributions of all African States interested; (3) that the personnel of the investigating commission be large enough to ensure continuity of work in all directions, thus obviating interruptions due to such exigencies as illness or leave, and preventing the staleness and inertia which are so likely to result from isolation; (4) that sufficient funds be placed at the disposal of the investigating commission to allow of the employment of adequate native labour, so that experimental work can be undertaken on a sufficiently large scale, thus enabling the investigation of the dependence of fly and trypanosomiasis on game, and of the various problems enumerated in the report of the Imperial Bureau of Entomology to be carried out in a satisfactory manner, and with some reasonable prospect of success.

Discussion.

The PRESIDENT: Professor WARRINGTON YORKE has raised some very important issues in this valuable paper, and as there are a number of Fellows here who have had a great experience in regard to trypanosomes and sleeping sickness, I shall expect the contribution to give rise to considerable discussion.

Professor YORKE has shewn the serious situation of sleeping sickness from the epidemiological as well as from the economic standpoint, and he has strongly emphasised the important fact that, in any investigation which may be undertaken to discover the best methods of combating or preventing sleeping sickness, the inter-relationship of game, the tsetse-fly and pathogenic trypanosomes in man and in domestic stock must not be ignored. He complains—very rightly, I think—that this aspect of the question has not been considered sufficiently in the recommendations of the Bureau of Entomology. And he further points out that the organisation which is proposed is totally inadequate for the problems to be solved. I agree with Dr. YORKE's point of view; I do regard it as a most important thing that the inter-relationship I have mentioned should be thoroughly investigated; and I agree, too, that the organisation proposed could not possibly advance the question further than its present stage. I remember that when, in 1913, I went to Uganda, I was amazed to find that there were only two scientists working at the subject there; or perhaps I should say one scientist, for one was a lady, Miss ROBERTSON, who had been working on the subject for some considerable time, while the other was an American, Mr. FISKE, who had just arrived, and this in a region where 200,000 deaths had taken place within a few years, and when, even at the date of my visit, there was a certain amount of anxiety with regard to this disease. On my return home I availed myself of the opportunity of submitting, in my general report, an expression of my astonishment at the inadequate means being taken to deal with the subject. I do not blame the Colonial Office, because they do not hold the pursestrings; they are held by the Treasury, and the Treasury are not interested in tropical diseases or in the health of the inhabitants of the tropics. Colonies which are rich have to take take care of themselves—and rightly so, as they are quite able financially to look after their own health. Colonies that are poor have to do the same, and they suffer accordingly. It is in this uneconomical attitude that the difficulty lies in securing proper research and efficient health conditions.

I remember an instance which illustrates the attitude taken by the Treasury in regard to tropical diseases. A meeting was held in the Mansion House for the purpose of obtaining funds for the London School of Tropical Medicine, and Lord HARCOURT, who was then Secretary for the Colonies, announced that, after some difficulty, he had been able to secure £500 a year for the Tropical School, but he added that this was not under any circumstances to be regarded as constituting a precedent. Sir GEORGE REID happened to be present, and he was the next speaker, and, in a humorous and somewhat sarcastic speech, expressed his pleasure at hearing that the grant was not under any circumstances a precedent, for he hoped that next year the amount would be £5,000, That is how it strikes other people. They see clearly we do not receive sufficient money for this scientific work, and that we do not treat the matter in a really serious way.

Professor YORKE has flattered this Society in making his suggestion that we might have been consulted in this matter, and I hope the Secretary of State may be induced in future to adopt the principle that, on the few occasions when there is something of great importance on which he desires to seek outside advice on epidemic diseases of the tropics, it would not be a bad thing to consult this Royal Society, for, as Professor YORKE has stated, this Society consists of medical men, of veterinarians, of entomologists, of zoologists, most of whom are familiar with the tropics, and, therefore, likely to be well qualified to give valuable advice on a broad and practical basis. There has been in the past, and still is, a tendency to work in compartments, but however useful it might have been in the past this system is not adapted to solve the problems of to-day. Under the present conditions it is desirable that the advice should be much more general, and that, instead of isolation, we should have co-operation.

I hope Fellows will give us an interesting evening, and perhaps some will criticise Professor YORKE's criticisms.

Dr. GUY MARSHALL: I have listened with the greatest of interest to Dr. YORKE's very able and instructive paper. But, while naturally one must pay the greatest attention to the opinions of a man of his experience in these questions, it would be an act of insincerity on my part if I were to pretend that he has won from me either agreement with his views or approbation of his methods.

As a member of the Glossina Sub-Committee of the Imperial Bureau of Entomology, on which he has poured his scorn, I hope to be able to shew that the picture of its incompetence which he had prepared for your delectation is overdone. In the first place, he has drawn a contrast between the action of our own Government and that of the French Government in regard to the trypanosomiasis problem. Apparently the head and front of the offending of the Colonial Office is, that they consulted the Imperial Bureau of Entomology on a purely entomological question relating to tsetse-flies, although that Bureau was especially created by the Colonial Office to advise on entomological matters. Nor, I think, can that department be fairly charged with neglecting the medical and protozoal aspects of trypanosomiasis, for in the past they have freely sought the advice of the Royal Society, whose Tropical Diseases Committee comprises a number of the leading authorities on Tropical Medicine. I do not know whether the author really thinks that this Society ought to set up a rival organisation to that Royal Society Committee, but that is a matter upon which I do not care to express an opinion.

Professor YORKE has unwittingly given an entirely erroneous complexion to the whole aspect of the Glossina Sub-Committee's report. After setting out some of the proposals which were made by this body, he draws attention to the fact that we have entirely ignored the main subject which Lord Desart's Committee was appointed to consider, and he says, "This is the more remarkable as the report of the Glossina Sub-Committee was called for directly as the result of the report of the 1913-14 Inter-departmental Committee." I can assure him that that statement is absolutely incorrect; there is no shadow of truth in it. The matter that led to the appointment of the Glossina Sub-Committee was purely an entomological one, and it arose in this wise. The Principal Medical Officer of one of our African Colonies was asked by his Government to make arrangements for entomological investigations in connection with Glossina, and he was told that a certain sum would be available for them. He replied that he had no entomologists on his staff, that he saw no opportunity of securing the services of one, and that, moreover, the sum named was quite inadequate for accomplishing any really useful work. He, therefore, suggested that it would be better if amounts, voted by various colonies, could be pooled, and used for entomological work under a central control. This letter was sent to me from the Colonial Office for a report. I supported the proposals with some modifications, and my memorandum was referred to the Committee of the Imperial Bureau of Entomology, who appointed a Sub-Committee to deal with it. The matter had no connection whatever with Lord Desart's Committee. Nevertheless, it is hardly fair to say that we have ignored the recommendations of that body, for they specially emphasised the need for more work along entomological lines in the following words: "Your Committee attach great importance to a proper and sufficient equipment of entomological research into the bionomics of the incriminated tsetse-flies. This form of research has, in their view, been insufficiently pursued up to the present time." There is no doubt that it has been unduly neglected in the past, and the suggestions of our Glossina Sub-Committee were framed in the hopes of being able to remedy that defect. The criticisms levelled against their report are really quite premature, for their proposals are merely tentative and preliminary, and not a cut-and-dried scheme. We have to remember that the whole matter is dominated by the question of funds.

Professor YORKE has urged that the sum suggested as necessary for carrying out the work outlined by the Glossina Sub-Committee, namely, £50,000, is inadequate for the full accomplishment of the programme. That is, no doubt, true; but in these matters reasonable discretion is necessary, and it would be foolish to start by making demands that to the layman might appear excessive. It seemed, therefore, wiser to ask for a smaller sum with which to make a start, and then, if we can shew good results, we shall be justified in seeking further assistance.

The more extreme advocates of wholesale game destruction appear to think that those who disagree with that policy do so because they refuse to admit that there is any vital connection between tsetse-flies and game. But this is not so. If all the mammalia could be permanently eliminated from a given area, no one can doubt that the flies would disappear. But this would also be the case if all the shade upon which these flies are dependent were eliminated without killing the game. Now, these two lines of attack both suffer from the same drawback, namely, that they are only feasible in small areas, and cannot in practice be applied to wide stretches of country, such as N.E. Rhodesia, four-fifths of which is under fly. Dr. TAUTE, in his communication to Lord Desart's Committee, in 1913, pointed out the folly of trying to eradicate Glossina by such means, on a big scale, unless you can at once place settlers on the land, so as to maintain economically the conditions averse to the fly.

In regard to the use of shade destruction as a means of eliminating tsetse-flies, Mr. W. F. FISKE's excellent work on *G. palpalis* in Uganda has shewn that the maintenance of the fly population in a fly area is due to the existence of a number of foci presenting optimum conditions of food, shade, and breeding-places. It is not, therefore, necessary, as had been previously supposed, to destroy all the shade in a fly area in order to get rid of the flies (which is economically impracticable), but only the shade in the vicinity of these foci; and this discovery has rendered clearing operations much more feasible as an effective sanitary measure.

It seems not unreasonable to suppose that a more exhaustive knowledge of the life-history and essential requirements of *G. morsitans* may lead to a similar modification in current views as to game destruction as a means of controlling this insect. It may well be that we may be able to limit destruction to certain kinds of mammals only, and this only in certain limited areas or at certain definite seasons. For our present knowledge as to the essential factors that restrict the range of this species is so inadequate that most suggestions for its control are purely speculative. For a similar reason, it seems desirable that further entomological research should precede any argument in game destruction, otherwise the results of the experiment may be quite erroneously interpreted.

Professor YORKE has criticised the proposal of the Glossina Sub-Committee to establish experiment stations in a number of different parts of Africa; he desires that all efforts should be concentrated in one place. This arises from the fact that he is not attempting to consider the subject from an entomological standpoint, but has in view merely the inauguration of a game-destruction experiment in relation to *G. morsitans*. Our Sub-Committee, however, has taken a broader view of the tsetse problem. We have to consider that there are at least four distinct species of major importance, whose life-histories and idiosyncracies require much further elucidation, for we know that what is true of one species is not necessarily true of another. Moreover, in different parts of Africa the same species may occur under very different conditions, so that control measures that may be suitable in one place may be quite inapplicable in another. For example, the measures that were effective in absolutely eliminating *G. palpalis* from a small, entirely isolated and relatively populous area like the island of Principe, would be impossible to carry out on the shores of Victoria Nyanza, and still less so in Southern Nigeria.

From the entomological aspect, therefore, it is obvious that there is much research yet to be done in many different localities, and the more men that we can have at work simultaneously, the more quickly are we likely to secure the information that is necessary for the solution of this complex and important problem.

Major E. E. AUSTEN: There are one or two points to which I would like to direct the attention of Professor YORKE and the meeting, though, after Dr. MARSHALL's speech, there is little for me to say. As another member of the Glossina Sub-Committee I accept whatever share of responsibility falls to me on that account. But I would point out that, so far as I know, the Sub-Committee's report has never been published, in the sense that the Report of the Departmental Committee on Sleeping Sickness was published; unless I am mistaken, it is a private Memorandum, drawn up to formulate our own ideas, and to explain in general terms to those from whom we hope to be able to raise funds what we intend to do. Therefore, I support Dr. MARSHALL in deprecating criticism at this stage, since matters have not advanced far enough to warrant it.

As to the subject-matter of Professor YORKE's very able paper, it is quite true, as he has admitted, that in Central and South-Eastern Africa trypanosomiasis in domestic stock is an immeasurably more important problem than the disease in man, and that big game is the reservoir of the trypanosomes that are fatal to stock is no new discovery; we have known it ever since Sir DAVID BRUCE (then Surg.-Major BRUCE) carried out his epoch-making investigations in Zululand in 1895-6. The real point at issue, however, is involved in the fact that, while admitting that game is the reservoir of the trypanosomes which affect stock, and render many parts of Africa at the present time unsuitable for colonisation, it is yet denied by some people—who, I think, are competent to be regarded as authorities—that Professor YORKE is justified in declaring it to be established that game in those regions is the main reservoir not only of the trypanosome which is fatal to domestic animals, but of that which is so lethal to man, in other words, that these two trypanosomes are one and the same. There is—if I may say so without offence, and no offence is intended—a *trail of sleeping sickness* over this paper, and this feature has always been noticeable in the campaign against the game. Obviously, game as the reservoir of sleeping sickness is a proposition of vastly greater and more sinister import than game as the reservoir of trypanosomiasis of domestic stock, yet, before we sanction anything in the nature of wholesale game destruction, we need to be very sure of our ground. Professor YORKE tells us that the game is the main reservoir from which *G. morsitans* draws the trypanosomes of Rhodesian sleeping sickness. But Professor YORKE, while in the Luangwa Valley, Northern Rhodesia, acquired, at any rate, some evidence shewing that man does not always rapidly succumb to this parasite. In the "Final Report of the Luangwa Sleeping Sickness Commission of the British South Africa Company, 1911-12' (*Annals of Tropical Medicine and Parasitology*, Vol. VII., No. 2, June 10th. 1913), the authors (Professor YORKE, Dr. KINGHORN and Mr. LLEWELLYN LLOYD), dealing with *Trypanosoma rhodesiense* infection in man, write as follows (*loc. cit.*, pp. 186, 187):—"No exact data exist as to the duration of the disease in natives, but it would appear to be short. Many of the cases complained of no subjective symptoms of the disease when diagnosed, and presented very few objective signs, but in general they lived only a few months. Occasionally, however, a patient is more resistant, and one native definitely proved to be infected with *T. rhodesiense* is still alive and in a state of apparent good health a year later." Professor YORKE has told us to-night that, in his opinion, the result of the heroic experiment by TAUTE and HUBER "might well have been different" if the inoculations "had been made from the blood of an infected man, instead of from infected horses and mules," owing to the enhanced virulence of the parasite that might have been expected under such conditions. On the other hand, in the Luangwa Valley, as on the shores of Victoria Nyanza, infected blood may often be conveyed by tsetse-flies "mechanically from one native to another," a process which, as DUKE has pointed out, is not incompatible with increase of virulence on the part of the parasite. Professor YOKKE himself will admit that he cannot have it both ways. If the trypanosome taken from man is more dangerous to man than that from animals, I venture to maintain that this native, who had trypanosomes in his blood, but was still apparently well at the end of a year, and may be alive now for all we know, is, if he be not under lock and key, far more dangerous to the surrounding inhabitants than very many antelopes. Dr. AYLMER MAY (Principal Medical Officer of Northern Rhodesia), in the course of his examination before Lord Desart's Committee, stated, in reply to questions put to him by the Chairman, that 40,000 natives in Northern Rhodesia had been examined for sleeping sickness, that the disease was not increasing there, and that there were fewer cases than in the previous year (120 in all in four and a half years, since 1909). I asked Dr. MAY in how many instances out of the 40,000 natives examined a blood examination had been made, and he replied, "Roughly, in about 20 per cent." In other words, while but 40,000 out of 100,000 natives in the sleeping sickness area were examined for sleeping sickness at all, it was only in the case of 8,000 individuals that an actual blood examination was made. Does Dr. YORKE believe that this examination of the blood of 8,000 out of 100,000 people is sufficient to prove that not one of the remaining 92,000 had trypanosomes in the blood? And does he maintain that his native, who was alive and well a year after being proved infected, was the only exception to the rule in Tropical Africa, and that he was so unfortunate as to come upon that single exception?

Professor YORKE, when himself examined before the Departmental Committee, and questioned about the result of TAUTE's original experiment, to which reference has been made to-night, would not admit that it proved anything more than that TAUTE himself was immune to the particular trypanosome concerned. But Professor YORKE proceeded to add: "If he had inoculated one hundred human beings the result would have been very different." Here, in this paper, we are told that TAUTE and HUBER, in 1919, inoculated themselves and 129 natives, *i.e.*, they made 131 experiments. Results were uniformly negative, and Dr. YORKE now says that more than 130 experiments would have to be conducted before one could obtain positive results. I may be unduly obtuse, but I cannot see that, in this matter, Professor YORKE's attitude is very different from that popularly known as "Heads, I win; tails, you lose!"

Dr. B. BLACKLOCK: There are one or two points I would like to ask about. Professor YORKE mentions the recommendations of the French Commission, that pigs should be used, being resistant animals, as a screen to protect native villages from attacks of glossina and from being infected. The connection of the pig with the fly has been worked out by several observers. In the Island of Principe, the Portuguese Commission said that the absence of the tsetse fly from the Southern part of the Island was due to the absence of the pig in that part. Further observations were recorded by MACFIE from Nigeria, and he quotes Dr. FORAN on the relationship between *G. tachinoides* and the pig. Pigs are known to be naturally infected with trypanosomes, as has been proved on many occasions. In Principe and the Belgian Congo, pigs were heavily infected with trypanosomes, and in Principe a cattle trypanosome of a polymorphic kind was inoculated into a pig successfully. The trypanosome appeared to be like *T. gambiense*. Inoculations have been done into pigs with animal and human trypanosomes, and in BECK's gambiense case the pig remained infective six weeks. The most interesting experiment of this kind is that of MESNIL and BLANCHARD, who infected one pig with *T. gambiense* and one with *T. rhodesiense.* Neither of the pigs shewed symptoms for 48 days, then they became paralysed, and after over 100 days the rhodesiense pig died. At no time did direct examination reveal trypanosomes in the blood, but mice inoculated every tenth day from their blood were infected with the disease. In the gambiense pig sub-inoculations were successful to the 60th day, and in the rhodesiense pig to the 80th day. In view of this close connection between the pig and the tsetse fly, and in view of the obvious fact that the pig can harbour various forms of trypanosome, including two of the human forms, I ask what is the underlying idea in the recommendation that these animals should be used as a screen to protect human beings, or native villages? I ask whether it is proposed to atoxylise those pigs at the same time as the humans are treated; otherwise one cannot but see disaster approaching villages surrounded by pigs.

I would refer to some statements which have been made, I think in reference to an experiment on a limited scale which has been proposed for the removal of big game from an area. The criticisms which have been directed against this proposal are somewhat various. Some critics say you cannot possibly do it, that you cannot clear completely of game any limited area. Some say it is too expensive, that the money for it cannot be obtained. Others say you may be able to get the money, but even if you clear the game away, the fly will not go. A fourth set of people say you can do all the previous things and the fly will go, but when you have banished the fly you will not be able to apply the knowledge you have got. I cannot see the logic of such statements. Why should we not apply knowledge if we have obtained knowledge? We do not know whether we can obtain the knowledge until we try experiments.

There is also another kind of objection raised to this experiment, and I am sorry to say I heard Dr. MARSHALL use these words: that there are certain hot-heads who are for the wholesale destruction of game in Africa, and I think Major AUSTEN said he was against wholesale destruction of game in Africa. For men who have heard this subject discussed for years to propagate such impressions as to the intention of those who advocate an experiment is a pity, and what they say is certainly not due to lack of intelligence. No sponsor of this experiment has suggested the killing of game in a wholesale way in Africa: that would be contrary to the very nature of an "experiment." To make such statements is to misrepresent the facts entirely, for whose benefit I do not know; it cannot be for ours, but I do not understand the reason for such misrepresentation. If we take the area involved in the experiment and the numbers involved, I suppose it might be taken as comparing the area of this room, and the number of people in it, with the area and numbers in the whole of London. It would be very deplorable if, through some misfortune, we in this room were suddenly to be exterminated, and it would, we flatter ourselves, mean the loss of important scientific men, but I do not think anybody walking down the Strand or Regent Street to-morrow morning would be any the wiser. I think that is the sort of relation this proposed experiment in Africa would bear to the whole of that Continent.

The Glossina Committee appear to have diffidence in allocating areas to the members who will work at the subject in Africa, but you will notice that they exhibit no such diffidence in being prepared to direct from London the whole of the operations of those Commissions when they are at their work, and that is a very much more courageous proposition that the mere allocation.

A suggestion which seems to me to have some importance is, that we have not, so far, done badly in the way of finding out some connection between fly and the protozoon and the disease. The work of BRUCE, DUTTON, KLEINE, KINGHORN and YORKE has not occupied a very long period, and the work has been, I think, very quickly done in comparison with that on many problems of bacteriology carried out by earlier observers, and has taken into consideration all three factors: the fly, the protozoon, and the animal or man. When such successful results have been attained by certain methods of research, I maintain we have only to increase the application of those methods of research, employing more entomologists, more men who are studying protozoology, and more men to study and treat the disease; these men should work in a state of co-operation. As the research will involve a large amount of experimental work, which I think it impossible to expect from isolated units of men, no matter who they are, studying in the wilderness of Africa, it is necessary to centralise in a large Commission.

Dr. A. G. BAGSHAWE: As I was another member of the much-criticised Glossina Sub-Commitee, if I were not to say something it might give rise to misconception. In fact, I came with notes, prepared "to keep my end up," but Dr. GUY MARSHALL has done that so excellently for me that I shall refrain from saying anything on that head. He has shewn that this scheme which has been critcised by Dr. YORKE is not a cut-and-dried scheme, but one which can be modified. The method of carrying it out will depend very much on the funds which can be obtained.

Professor YORKE would have the work of entomological, medical and veterinary research into the problem of trypanosomiasis combined under one central organisation in Africa. I wonder if the time is suitable for a great investigation of that kind: it would cost a great deal of money, and, if the funds were not ample, the pinch would be felt either by the entomological or pathological side; hitherto it has been the entomological side which has suffered. I doubt if it is practical politics to explore all the links of the chain of the causation of trypanosomiasis on a great scale at once. It is the entomological link which has been least explored, and it is proposed that entomologists should now have their chance. The disease has never been systematically approached from the insect side. I incline to think that the restriction of fly is at least as likely to be realised as is the restriction of game, including under that head such animals as pig. At the moment both seem to be extraordinarily difficult problems. Professor YORKE still believes that the morphologically identical trypanosomes which infect domestic animals and man respectively in East Tropical Africa are identical biologically. But, apart from the results of TAUTE's and HUBER's gigantic experiment, there is the epidemiological objection to accepting his view. In the late campaign in East Africa there was no case of trypanosomiasis in man met with among British or German troops north of 9° S., though tsetse-fly abounded and draught animals succumbed. It was not until the Germans got to 9° S. that they found human trypanosomiasis. Between that line and the other side of the equator there is a large amount of morsitans, and this trypanosome of game which corresponds morphologically to the human one has been found many times, and it has been found on the West Coast of Africa too. But no human case of the disease has been found north of the 9° line near the East Coast.

Now, to pass to another matter, which has not been touched on to-night. In the report of the French Committee there is a point which Professor YORKE has not mentioned—of course he had not time to mention everything. The French Committee say that in French West Africa epidemics of human trypanosomiasis are due to mosquitoes, which, in their opinion, transmit infection from one member of a family to another, leading to the so-called house or family infection. The endemicity is due to the Glossina, but the epidemics in villages are due to transmission by domestic mosquitoes. The Governors of the French Colonies in West Africa, in accordance with this advice, have enacted that all Europeans leaving French Possessions in Africa should be examined for trypanosomiasis, and, in the event of their being infected, that they shall receive an injection of atoxyl before they embark. It seems very sound for their own sakes that such cases should be discovered early, but the reason is lest these persons should infect their cabin companions through the agency of mosquitoes on the voyage home.

Infection through the agency of mosquitoes is against British experience. Obviously it might occur, but I know of no record that it has, and this French measure seems to be an ultra-precaution. In the great epidemic in Uganda, though large numbers of natives visited the lake shore beyond the narrow fly-infested zone, and returned to their homes with sleeping sickness, no case of transmission of the disease to a person who had not visited the lake shore was reported or discovered, though in many of the villages concerned mosquitoes must have abounded.

This goes against the theory that the spread of sleeping sickness n the great epidemic on the Victoria Nyanza was due to mechanical transmission. If it was due to mechanical transmission by *G. palpalis*, one would think there must have been a few cases of mechanical transmission through the agency of other biting flies which occur far from the lake, but this never occurred.

A better instance is afforded by the conditions in Principe and S. Thomé, which have been already referred to. In Principe, some years ago, sleeping sickness was rife, whereas in S. Thomé, a few miles away, not a single indigenous case could be found by the Portuguese investigators, though many infected persons had been imported from Angola. Both islands had abundance of biting insects. The only difference was that S. Thomé had no tsetse-flies.

Dr. G. C. Low: We have had both sides of the question—the entomological and the medical—discussed to-night, and it is évident that there is still a great amount of doubt and uncertainty as to many of the points concerned. We have heard that the French authorities propose to treat with atoxyl all infected natives in their areas, so as to destroy the trypanosomes in their blood; but will this be successful if antelopes and other game are reservoirs of infection? Can entomologists alone settle this very important point?

Dr. Guy Marshall: No.

Dr. G. C. Low: Well, that is the whole thing; for settling this and other equally important questions a combination of medical specialists, protozoologists and entomologists is required, and not one of these branches alone.

As regards Rhodesian trypanosomiasis, white people infected die with great rapidity, and some natives do so also. It is, however, possible that the disease may last longer in the latter in certain instances, and only large numbers of blood examinations can settle this. To me it looks far more likely that man gets his infection from the game, because human cases are few and far between. Again we await a definite solution of this, and until we get it money will be thrown away.

The talk of destroying the whole of the game of Africa is generally indulged in by people who have never being in that country. I would suggest to such enthusiasts that they first try to destroy the rats in a small farm at home before airing their views as to destroying the fauna of a whole continent. The only way an experiment of destroying antelopes and the larger game can be carried out is to enclose a large area with fencing so as to prevent other animals from getting in, but, even then, how can one be certain that small rodents, pigs, and animals of that sort are not present? The fly might even take to feeding on birds in lieu of game.

All these matters require careful consideration before any large scheme and spending of public money takes place. I heartily agree with Professor Yorke's suggestion that this Society should have a say in such matters. We have in it men who know the subject from every standpoint, and a discussion such as we are having now shews how useful and valuable such knowledge is.

There are many other points I should like to raise but time presses. I shall limit myself to only one more question. If we destroy these special areas on the shores of the Victoria Nyanza, mentioned by Dr. Marshall, what proof have we that the *G. palpalis* will not adapt itself to other areas? If anopheline breeding-grounds are destroyed, we know the insects will lay their eggs on water in barrels and other sites never selected under natural conditions, and the same may hold good for tsetse-flies in depositing their eggs in special soils under special conditions. The habits of *G. palpalis*, as Dr. Marshall has already mentioned, are much different on the West Coast of Africa than in Uganda, approximating more to those of *G. morsitans*. Here, again, we have another important point requiring solution. I would suggest that for further work on the subject there should be a happy combination of all experts, and that the research should not be limited to any one individual branch.

[Adjourned to Friday, November 19th.]

transmission by *G. palpalis*, one would think there must have been a few cases of mechanical transmission through the agency of other biting flies which occur far from the lake, but this never occurred.

A better instance is afforded by the conditions in Principe and S. Thomé which have been already referred to. In Principe, some years ago, trypanosomiasis was rife, whereas in S. Thomé, a few miles away, not a single indigenous case could be found by the Portuguese investigators, though many infected persons had been imported from Angola. Both islands had an abundance of biting insects, and the only difference was that S. Thomé had no Tsetse-flies.

Dr. G. C. Low: We have had both sides of the question—the entomological and the medical—discussed to-night, and it is evident that there is still a great amount of doubt and uncertainty as to many of the points concerned. We have heard that the French authorities propose to treat with atoxyl all infected natives in their areas so as to destroy the trypanosomes in their blood; but will that be successful if antelopes and other game are reservoirs of infection? Can any one definitely settle this very important point?

Dr. Hale Manson: No.

Dr. G. C. Low: Well, that is the whole thing; for settling this and other equally important questions a combination of medical specialists, protozoologists and entomologists is required, and not one of these branches alone.

As regards Rhodesian trypanosomiasis, white people infected die with great rapidity, and some natives do so also. It is, however, possible that the disease may last longer in the latter in certain instances, and only large numbers of blood examinations can settle this. To me it looks far more likely that man gets his infection from the game, because human cases are few and far between. Again, we want a definite solution of this, and until we get it money will be thrown away.

The talk of destroying the whole of the game of Africa is generally indulged in by people who have never been in that country. I would suggest to such enthusiasts that they first try to destroy the rats in a small area at home before airing their views as to destroying the fauna of a whole continent. The only way an experiment of destroying antelopes and the larger game can be carried out is to enclose a large area with fencing so as to prevent other animals from getting in, but, even then, how can one be certain that small rodents, pigs, and animals of that sort are not present? The flies might even take to feeding on reptiles, &c., instead of game.

All these matters require careful consideration before any large scheme and spending of public money takes place. I heartily agree with Professor Yorke's suggestion that this Society should have a say in such matters. We have here men who know the subject from every standpoint, and a discussion, such as we are having now, shows how useful and valuable such knowledge is.

There are many other points I should like to raise but time presses. I shall limit myself to only one more question. If we destroy these special areas on the edges of the Victoria Nyanza, as mentioned by Dr. Macfie, what proof have we that the *G. palpalis* will not adapt itself to other areas? If anopheline breeding-grounds are destroyed, we know the insects will lay their eggs in water in barrels and other containers not selected under natural conditions, and the same may hold good for tsetse-flies in depositing their larvae in special soils under special conditions. The habits of *G. palpalis*, as Dr. Macfie has already pointed out, are much different on the West Coast of Africa than in Uganda, approximating more to those of *G. morsitans*. Here, again, we have another important point requiring solution. I would suggest that any further work on the subject should be a happy combination of all experts, and that the research should not be limited to any one individual branch.

Adjourned to Friday, November 10th.

RESEARCH INTO THE TRYPANOSOMIASIS PROBLEM: A CRITICAL CONSIDERATION OF SUGGESTED MEASURES.

By Professor WARRINGTON YORKE, M.D.

CONTINUED DISCUSSION

On the above Paper at a Meeting of the Royal Society of Tropical Medicine and Hygiene, Friday, November 19th, 1920.

Professor WARRINGTON YORKE: Whilst listening to the remarks of those who took part in the discussion which followed the reading of my paper, the first thing that struck me was that, curiously enough, Dr. MARSHALL and Major AUSTEN both took exception to the fact that I had ventured to criticise the report at all. Dr. MARSHALL says I did not win from him approbation for my methods, and Major AUSTEN stated that the Report of the Glossina Sub-Committee was never published; that it was a private document, drawn up to formulate their own ideas, in order—and this is significant—that they might be able to raise funds for what they intend to do. What happened was that, in June last, I was invited to attend a meeting of entomologists from all over the world, and there I heard discussed this very report which I criticised at the last meeting. The principal speaker at that meeting took this report as his text. I took the opportunity of criticising it on that occasion, but only briefly, because the report was not before me—I simply criticised the digest which I heard at the meeting. It was public property from that moment. The second point is that it is printed, and at Government expense. Finally, it is proposed to raise money on it, and to spend public funds. Now, if one cannot criticise a document which is being used to raise funds from the public, what are we coming to? Suppose the report is of the tentative nature which these gentlemen suggest, surely, now is the time for criticism, before it gets crystallised in such a way as to waste money and waste time. Or are we to consider that the remarks which emanate from this Committee are beyond criticism and above reproach? I hope not. Therefore, I think, I was justified in criticising this report.

I was consulted about this report by the principal medical officer of one of our colonies. The report had been sent to him for comment. I think, as Dr. MAY's name has been mentioned, there is no harm in saying he consulted me, and that his views on the subject are similar to mine.

I am glad, however, that the fact that these gentlemen hold these views did not prevent them attending the meeting last month, and endeavouring to restate their position by a somewhat violent counterattack.

Let us take some of the points raised by Dr. GUY MARSHALL. He said that I objected to the Colonial Office consulting the Imperial Bureau of Entomology, and it alone, on what is a purely entomological question relating to the tsetse-fly. But it is not a purely entomological question, and never has been. Flies *per se* matter nothing, but the diseases they convey do. The point is that these flies produce disease in man and stock, so that the matter does concern us as medical men as well as the veterinarians and the entomologists. Dr. MARSHALL took exception to my remark that the report of this Glossina Sub-Committee was called for directly as the result of the report of the Earl of Desart's Committee. Well, all I can say is that I was given to understand that that was the case; and I hope it is the case, because it is lamentable if, after spending so much money and time on a gigantic Inter-departmental Committee, the Government did not take action on the report of that Committee. I do not think the Colonial Office or the Secretary of State could have done otherwise than take action as a result of it. Dr, MARSHALL says—and this is a volte-face on his part—if all the mammalia could be exterminated from a given area no one would doubt that the flies would disappear. I am glad to have had that admission from him. But that is not the only point which interests us. A great point is this: Suppose you eliminate these carriers of trypanosomes, what would be the effect on the infectivity of the flies? That is a matter which requires investigation.

The next point is hardly relevant—but many things in the criticisms were not relevant. Dr. MARSHALL quotes the work done by Mr. FISKE, who has shewn that the maintenance of the fly population in a fly area is due to the existence of a number of foci presenting optimum conditions of food, shade, and breeding-places. He says it is not therefore necessary, as has been previously supposed, to destroy all the shade in the fly area in order to get rid of the flies, which is economically impracticable, but only the shade in the vicinity of these foci; and this discovery has rendered clearing operations much more feasible as an effective sanitary measure. If that is not an example of a drowning man clinging to a straw, I do not know what is. Dr. Low did well to point out the fallacy of such a generalisation on hypotheses which are far from proved.

Dr. MARSHALL says, in general criticism, that my suggestion that effort should be concentrated in one place instead of being dissipated is a result of not attempting to consider the subject from an entomological standpoint, but that I have in view merely the inauguration of a game-destruction experiment in relation to morsitans. That is not correct.

Turning now to Major AUSTEN's remarks, he complains that there is a trail of sleeping sickness over my paper. There ought to be a trail of trypanosomiasis over it. I rather admire Major AUSTEN when he goes into figures. He plunges courageously not only into figures, but into the epidemiology, and even into the pathology of the disease. You will remember he came to the last meeting armed with an enormous Blue Book, the Report of the Inter-departmental Committee on Sleeping Sickness, and read copious extracts from it. He told us how he asked Dr. MAY, in the course of his examination before that Committee, in how many instances out of the 40,000 natives in Northern Rhodesia examined for sleeping sickness, the blood was examined, and that Dr. MAY had answered about 20 per cent. Major AUSTEN went on: "In other words, while but 40,000 out of 100,000 natives in sleeping-sickness area were examined for sleeping sickness at all, it was only in the case of 8,000 individuals that an actual blood examination was made." He then asks, "Does Dr. YORKE believe that this examination of the blood of 8,000 out of 100,000 people is sufficient to prove that not one of the remaining 90,000 had trypanosomes in the blood?" It is difficult to follow Major AUSTEN's logic, but to comfort him, I can assure him that Dr. YORKE believes it no more than he believes that if Major AUSTEN went with a boy to Central Africa, he could solve the tsetse-fly question. How could anyone believe anything so absurd? Again, Major AUSTEN asks: "Does he (Dr. YORKE) maintain that his native, who was alive and well a year after being proved infective, was the only exception to the rule in Tropical Africa, and that he was so unfortunate as to come upon the single exception?" I never suggested anything of the sort; it is another example of Major AUSTEN's argument. What I did say was that we found one case, the case which has been quoted so often; one case who harboured trypanosomes in his blood for a long time without shewing symptoms of the disease, one case among the many found infected. Similarly, Dr. MAY stated of the 7,000 to 8,000 persons examined out of a population of 100,000 a certain proportion was found to be infected, but neither he nor any other person of ordinary intelligence believed that they were the only cases which were infected among the 100,000; he said that they were the only cases which were infected so far as we could ascertain in the 8,000 individuals, not in the whole 100,000 people. Is it likely that these odd cases of human trypanosomiasis constitute a reservoir of the human virus, comparable with the millions of antelope which are always exposed to the fly?—that is, assuming my contention is correct that the antelope harbours the human parasite. That is the question. Heaven knows what Major AUSTEN's point is.

In dealing with TAUTE's remarkable experiment, Major AUSTEN quotes a remark I made when giving evidence before the Inter-departmental Committee in, I think, 1913, that if TAUTE had inoculated a hundred human beings, the result would have been very different. Well, that computation was perhaps not justified; I had been subjected to some hours' pretty close examination by, I think, some rather hostile people, in that they did not hold my views,. And I think a hundred was not a bad estimate considering that the statement was made seven years ago, and that one had not any time to consider the matter thoroughly. If BRUCE's work, and KINGHORN's and mine is correct, then in order to determine the chances of infection one must know the number of tsetse-fly in Rhodesia, the number of times they bite the individual, and also the number of infected cases there are! Therefore I withdraw that hundred, and apologise for having, seven years ago, been induced to state it.

I was disappointed with Dr. BAGSHAWE, because he said he came with notes prepared, to support his contention, and I thought we should have something worth hearing; but he only said that Dr. GUY MARSHALL has done this so excellently that he would refrain from saying anything under that head. That was a bitter disappointment for me, because I had not considered Dr. MARSHALL's remarks in that light. But Dr. BAGSHAWE does make one, and as far as I can make out, the only real, relevant point in the discussion, and that is the epidemiological point, where he points out that the Germans did not encounter sleeping sickness north of the line 9° S. I cannot answer that point at present, but I am sure Dr. BAGSHAWE will be the first to acknowledge that a great deal more knowledge is required on the epidemiology of Rhodesian sleeping sickness before we can make a deduction from that.

Dr. BAGSHAWE also referred to Dr. DUKE's remarkable work, which resulted in his striking theory of mechanical transmission, but that is outside the scope of this paper, and so I do not propose to discuss it.

I submit that the members of this Glossina Sub-Committee have not answered my criticisms of their report. My criticism is two-fold—and one has to repeat it time and time again—that the problem is not a purely entomological one; it is the disease in man and in stock which matters, not that there are so many millions of flies in Africa. They are annoying, but what we bear them a grudge for is the disease which they carry. I hold that previous work—not only that of KINGHORN and myself, but that of the Royal Society's Commission, work which called forth that Committee—indicates that the further line of research should be in the direction of ascertaining the relationship which exists between the game and fly and the trypanosomes of man and of domestic stock. That has not been touched upon.

My second criticism is that the organisation, which this Glossina Sub-Committee proposes to set up to investigate the problems which it itself states should be solved, is not competent to do anything. It will fai lamentably. It is no advance on what has been existing in Africa for the last ten years. They propose to send to Africa twelve entomologists—if they can get them, and it is admitted that they are not available at the present time—to send them to six different places, thousands of miles apart, and to leave them there, with the sole stipulation they should report to London, to the Glossina Sub-Committee. These twelve isolated entomologists will be expected to answer questions which can only be solved by a gigantic organisation, involving a great outlay, and a concentration of energies.

Those, in brief, are my criticisms of the report of this Glossina Sub-Committee, and I think those who read the replies to those criticisms will realise, as I realised, that the replies evade the point, and put into my mouth, as was done seven years ago, statements to which I never gave utterance, and against which, of course, I protest.

Sir JOHN ROSE BRADFORD, F.R.S.: As a member of the Departmental Committee which has been alluded to, I would like to make a few remarks.

I think the Society is to be congratulated on this paper by Professor YORKE. Unfortunately, I was not here last time, but, though I had not the opportunity of hearing it, I have read it, and there is no doubt it is an interesting paper. It is also interesting to the Society, because, whatever its other merits may be, it certainly, as I gather from what has passed to-night and from what I have read, led to a very lively discussion.

I do not propose to deal with any of the special technical or scientific questions, because my first-hand knowledge of trypanosomiasis is limited, as it is some years since I worked at this subject and I am not thoroughly familiar with the subject now. So in the few remarks I make I would like to deal with the matter from a general point of view.

One of the things which struck me in Professor YORKE's paper was his criticism that the Government had not approached this Society in the matter, and he made somewhat unfavourable comparisons between the methods of the British Government and the methods of the French Government. I think that that criticism, in this particular instance, was a little beside the mark, if I might say so, because although, doubtless, the Government might derive great advantage from consulting this or other societies in matters of scientific moment, the comparison with the method of the French Government was not quite apt, because the Government did not consult, so far as I understand, the Glossina Sub-Committee on this proposal. It is not a question of the Glossina Sub-Committee *versus* this Society, but the real comparison with the French is the comparison between consulting a Society, and appointing a Departmental Committee. The Departmental Committee appointed some years ago was the step taken by the British Government, which is comparable to the action of the French Government in consulting their Tropical Society. That Departmental Committee, whatever its faults may have been, was a very remarkable Committee, because it was a Committee with such wide interests. I have served on many committees in my time, but I doubt whether I have served on one of such striking character as that one, owing to the very varied interests which were represented on it. I do not think any society could have included amongst its members representatives of such varied interests as that Committee contained: sportsmen, naturalists, entomologists, pathologists, physicians, officers with administrative experience, Government officials; it was a very large and representative Committee. It sat for a very long time, and took a great deal of evidence on a very difficult problem. So far as my memory serves me, that Departmental Committee was originally constituted owing to the great conflict in opinion amongst scientific men and amongst naturalists and sportsmen, with reference to the question at issue, *i.e.*, the relation of game to sleeping sickness, and allied diseases, and especially with reference to the destruction of game. Anybody who served on that Committee, or anybody who read the evidence which was brought before it, must realise the extreme difficulty and complexity of the subject. That is seen in the report issued. I think if any comparison between the action of the British Government and the action of the French Government is made, the real question is whether it is better to appoint a Departmental Committee or to consult a Society. That comparison in the present instance, however, does not arise, since the real question is this: Since that Committee met has there been any advance in scientific knowledge which completely alters the point of view from what it was at that time? I confess I am not thoroughly familiar with the matter now, but I am not aware that in the last few years there has been any revolutionary advance which puts the subject in a totally different position from that which it assumed at that time. Therefore I do not think there is very much in Professor YORKE's criticism as regards the action of the Government.

To go back to the Departmental Committee. That Committee recognised, as Professor YORKE points out, and I think everybody in this room will agree, that the problem is a mixed one, partly entomological, partly pathological, and so forth. The Committee recognised that, and it was brought forcibly before that Committee that, although much scientific work had been done in the last few years on the purely pathological side, no corresponding amount of work has been done on the entomological side. The members of your Society and the like are those best qualified amongst pathologists, perhaps, to realise the great services which entomology has rendered to pathology and to medicine. It does not seem to me a very strange act on the part of that Departmental Committee to advocate in their report further entomological enquiry. Furthermore, I think I am right in saying there is more obscurity about some of the entomological problems connected with Glossina than there is in regard to some of the more pressing pathological problems. So that there was much to be said for an extensive entomological enquiry. The summary of the report—the practical result of that Departmental Committee—was, to my mind, two-fold; firstly, they recommended a game-destruction experiment, with certain limitations. They were bound to point out the extreme difficulties of carrying out such an experiment; everybody must admit that. It is a very desirable thing to do, but it is open to many fallacies, and it is one which would be extremely expensive. Secondly, they reported that it was very desirable to prosecute entomological enquiry. Looking at it now from the point of view of an outsider, I say that, although, no doubt, it is a counsel of perfection to have an enquiry and an organisation which will deal with the whole problem—pathological, clinical, entomological and so forth—yet that is no reason why impediments should be raised or difficulties placed in the way of carrying out a portion of the enquiry, namely, the entomological portion, which must, humanly speaking, bring forth results of some value, and in all probability results of very great value. You all know, in medicine, instances where the spread of certain diseases has been due to insects, and where the prevention of such disease has been carried out by methods derived from entomological enquiry, and where knowledge concerning the disease, and of the virus causing the disease, is in a far more chaotic condition than it is in the case of trypanosomiasis. So it seems to me it would be highly desirable for an entomological enquiry, presumably on the lines recommended by the experts of the Glossina Sub-Committee, to take place; and it would be a pity for impediments to be put in the way of that, simply because it does not deal with the whole problem of trypanosomiasis.

Dr. A. S. NEAVE: I also would like to thank Professor YORKE for providing us with a paper which has given rise to an extremely interesting discussion, even though I confess I shall differ from him fundamentally.

Before I state my own views on the subject, I would like to refer to the question which he himself has just raised as to publication. Nothing that Professor YORKE has said this evening seems to alter the fact that the report he has been criticising has never been published. It was discussed, it is true, in an indirect manner, at the Imperial Conference, because the principal speaker was a representative of one of the Governments to which the report had been sent with the intention of getting their views, and, on his own authority, he used some of the data in that report for the paper he read. Nevertheless the fact remains that the report never has been published. I think Professor YORKE is wrong in supposing that the speakers who oppose him object to his criticism of the report as such. The point is—I say it with diffidence as a visitor—ought the criticisms of the report in question to be made more public that the report itself? Professor YORKE's criticisms are most valuable, but it is one thing to criticise such a report in this room, and another thing to publish such comments on an unpublished document.

Now with regard to the report itself, Dr. MARSHALL is said to have pointed out—and I think Professor YORKE has treated Dr. MARSHALL'S speech rather unfairly, by extracting one or two isolated sentences and divorcing them from their context—that the *question* was a purely entomological one. This is not the case, and no one, I imagine, thinks it is. The *report* of the Glossina Sub-Committee is a purely entomological one, and as Dr. MARSHALL has said, the remarks Professor YORKE made on the relation of the Glossina Sub-Committee to Lord Desart's enquiry were based on a misconception. But let us suppose he was right; surely the result of Professor YORKE'S conclusions would become all the more surprising. As has been pointed out by others, and is also quoted by Professor YORKE himself, the Desart Committee attached great importance in its report to a proper scientific equipment of entomological research. If so, was it surprising that a purely entomological Sub-Committee, appointed by an entomological body such as the Imperial Bureau, dealt solely with the entomological side of the question? It seems to be precisely what we should have expected.

We have here a problem in which we desire to exterminate from the world a blood parasite with a vertebrate host and an insect vector. There are consequently two main lines of investigation. That is to say, we must endeavour to discover methods of exterminating either the vertebrate host or the insects concerned. Of the two, it seems to me that attempts to destroy the insects are more likely to meet with some measure of success. In the first place, they are less likely to interfere with the balance of nature in Africa, and that is a factor of vital importance. Half mankind's difficulties in dealing with insects, whether disease-carriers or plant pests, and with practically all the higher organisms, have been due to the upsetting of the balance of nature. And that is a point we should bear in mind. Secondly, attempts to destroy the fly are less likely to interfere with the convenience of the human inhabitants of Africa than is the destruction of vertebrates. I think our chances in succeeding in dealing with the fly are better than those in trying to exterminate such vertebrates as may be proved to be its hosts.

The holding of that opinion does not denote any objection to carrying out experiments on protozoological lines, such as Professor YORKE wishes to see. Professor YORKE, on the other hand, wishes to confine investigations to experiments on the destruction of the vertebrate hosts, while at the same time putting difficulties in the way of entomological research. A small point in regard to this was raised by Dr. BLACKLOCK at the last meeting, and is worth examination. He took to task several speakers because they had talked about total extermination of game, and said that such a thing was not suggested, nothing more than an experiment being contemplated. The logic of that position is difficult to comprehend. Is Dr. BLACKLOCK only prepared to advocate experiments on game destruction without carrying them to a logical conclusion? It seems to me that the opponents of game-destruction experiments base their objections not on the fear that all game may have to be destroyed, but on the fact that we do not know enough about the subject at the present time to devise a means of carrying out such experiments on lines the results of which will have general acceptance. The fear is that there will only be an expenditure of money without getting a result that we can accept. I think a game-destruction experiment may be justified in years to come, but at present I do not think we can devise conditions under which we can secure a satisfactory result, one which will appeal to the scientific mind. There is the great difficulty of having a control under the conditions of the experiment, How can you arrive at a result in any game-destruction experiment over a given area unless you study carefully and over long periods the conditions in the area before and after the experiment is made? Professor YORKE himself sees this difficulty, and in his paper called attention to the risk of that point of view being overlooked in the experiments being conducted in Zululand. The fact of the matter is, that in the present state of our information a game-destruction experiment would be of doubtful value, compared to a deliberate attack on the fly based upon a study of its habits. We do not know enough about the species of Glossina such as *G. morsitans;* if we knew as much about them as we know about *G. palpalis*, the situation would be a good deal clearer, and that information might lead us to hold opinions very different to what we hold at present as to the nature of the fly's dependence on game. It is clear, from FISKE'S work, that to exterminate the main food supplies of *G. palpalis* on the Victoria Nyanza, you would have have to kill off, not the antelopes, but the lizards and crocodiles. I ask you, How would you propose to set about this on the Victoria Nyanza? When we come to *G. morsitans* and *G. pallidipes*, we will find that the favourite host is some such animal as the bush pig, an animal that would be excessively difficult to exterminate in any part of Africa.

It seems to me that there is only one way, so far as we know at present, of exterminating Glossina, in the sense of rendering an area permanently free from it. That method is the occupation of the area and the country generally by man on a large scale; under such circumstances the destruction of game would take place side by side with that of Glossina. I do not think that at the present moment any form of game destruction, nor even any form of fly destruction, would have more than a temporary effect in driving the fly back. But I think we have some hope of a permanent result if entomological investigations are carried out on the lines recommended by the Sub-Committee, and if our workers would supply evidence and knowledge as to *G. morsitans*, such as FISKE has produced in regard to *G. palpalis* in Uganda. By a combination of various methods—driving back the vertebrate hosts, partial clearing, introducing parasites and traps in the breeding places, and so on—we might render an area habitable to man and his domestic animals at the moment we render it free from fly, always provided that man was prepared to occupy the area immediately. I think the extermination of the fly can be rendered largely automatic, provided man is living sufficiently densely in or near the area it is required to clear.

Professor YORKE objects to the scope of the scheme drawn up by the Glossina Sub-Committee, and thinks it represents no advance on the bad old days of the last ten years or so. Surely, he is somewhat mistaken. The recommendations of the Sub-Committee go a long way beyond the days when it was thought sufficient to attach to a Sleeping Sickness Commission one medical officer, who was supposed to have some knowledge of entomology. The Sub-Committee recommend that there shall be a *minimum* (not a *maximum*, as Professor YORKE suggests) of two entomologists working together at one time, whereas the few entomologists engaged up till now on this subject have had to work singly. I admit it is difficult to get the trained men we want, but is that our fault? We can put our hands on five good men, who know the African conditions. It must take time to produce more.

I find myself bound to disagree profoundly with Professor YORKE in regard to his general conclusions. And I cannot quite understand his basic objection to carrying out entomological research, nor why he criticises such work. It is true that it is not the line of work he is particularly interested in, but it has the same object, and hence should have credit therefor. Surely, he might leave it to the entomologists to work out their line, as he works out his.

Professor R. T. LEIPER: One point which has struck me very forcibly, in listening to this discussion, is the very large sum of money which it is proposed to ask the Colonial Office to devote to this particular line of enquiry. It has been suggested that jealousy on the part of Professor YORKE has given rise to his protest, but is it not his longsightedness? I am sure he sees that if the Colonies are to be asked to spend £50,000 *at least* on the purely entomological side of this investigation, there will be very little money available from these Colonies during the next five or ten years for cognate researches in trypanosomiasis or any other subject of importance in Tropical Medicine.

It should be recalled that the Colonial Office receives in small grants of £100 or £500 from each Colony a total sum of perhaps not more than £10,000 a year for the service of its Scientific Committees and their publications, and that the cost of the enormous research organisation built up during the war in this country by the Medical Research Committee came to less than £40,000 per annum.

The time has come when some one should make the suggestion (and I venture myself to commit the indiscretion) that the Colonial Office would be well advised to co-ordinate the work of its various bodies and committees like the Tropical Diseases Research Fund, Tropical Diseases Bureau, and the Imperial Bureau of Entomology by appointing a Tropical Research Council on lines similar to the admirable Medical Research Council which has recently been formed for the co-ordination of research work under Government auspices in this country. Increased support from the Treasury might be forthcoming if a close link were established with this Council.

It would be the duty of the Tropical Research Council to enquire into the needs and to delimit the activities of the various bodies receiving Government aid.

Colonel D. HARVEY: I would like to make a few remarks on the discussion, but I have no intention of entering into the controversy.

Professor YORKE, in his paper, referred to the fact that he had found 16 per cent. of the game in the district where they were working, the Luangwa Valley, were infected with trypanosomiasis, with the organism pathogenic to man, and he said that Commission, of which I was a member, confirmed this fact. That is hardly how we should have expressed it. I am giving my own ideas, as Sir DAVID BRUCE is, unfortunately, away from England. If Professor YORKE had said we found that the trypanosome that was present in cases of human trypanosomiasis in our district of Nyasaland was identical with the trypanosome which we found in game, I should have been at one with him. But when he says this trypanosome which they and we found was pathogenic to man, that was just the bit of evidence that was wanting. We spent three years trying very hard to identify as one or separate these two trypanosomes, but at the end of that time we had to confess we had not done so. The trypanosomes of game and those in man were the same morphologically in their action on experimental animals and their development in the fly, yet we could not say that the game trypanosome was pathogenic to man. TAUTE came to our camp, and stayed a fortnight, and we said to him, "All that remains now is to find out whether this game trypanosome is pathogenic to man." He said, "I am convinced it is not," and he added, "I am willing to demonstrate it on myself." We said, "Very well, we have a nice strain of game trypanosome here." He replied, "I will not do it here; you have the human trypanosome in your district; I will go to the other side of the lake, where there is no human sickness, and I will carry out the experiment there." He went off, and shortly afterwards we received a letter from him, giving full details of the experiment on himself, shewing that he, at least, had not been infected either by the fly or the inoculation of the blood of infected animals. We said that this shewed only that TAUTE himself was immune to this particular trypanosome. Therefore, that point had not been settled. The only other thing we could see that remained to be done was to work out carefully the epidemiology, that is, to find out whether this disease was widespread wherever the game was to be found. We had to leave the question like that because we could not say more. Sir DAVID BRUCE stated in the report, "It will be found difficult, or even impossible, to arrive at a certain decision with regard to identifying these two strains."

The only fresh evidence which Professor YORKE tells us about are these further experiments of TAUTE'S, and Professor YORKE says he does not think this carries us much further. But I think it carries us to this point, that the strain of trypanosome which TAUTE was working on was not pathogenic to man.

The only other piece of evidence which has come along since is the epidemiological evidence which was afforded during the war, and which Dr. BAGSHAWE referred to. We have had interesting reports about that. Not only German but our own troops—South African and British—went through Gérman East Africa, and it was only in certain parts—although morsitans is widely spread all over the country—that a few men were affected with the Rhodesian sickness. These regions were known before as the parts where this sickness was to be found, and it was when the troops got down near the Rovuma River, and one main road in Portuguese East Africa, that one or two men were infected. When the men went to a certain part of this road, half a dozen of them contracted rhodesiense sickness. This evidence, therefore, favours the view that the human Rhodesian trypanosome is a different strain, though a similar trypanosome.

Professor YORKE said the only explanation he could give of the distribution of the disease was that man was relatively immune to this trypanosome. In the district where we were one would think that, if man was immune to the trypanosome, we should have found various types of severity in cases of the disease. But it was not so. When a man became infected he went down in three or four months ; there were no gradings of severity in the disease.

With regard to the question of entomologists being employed for the investigation, I am at one with Dr. YORKE in saying that the way to attack the disease is to do so from all sides, and I am sure one of the advantages which Professor YORKE's Commission had in Rhodesia was that they had as entomologist with them LL. LLOYD, who did such excellent work.

Professor YORKE : May I ask Colonel HARVEY one question ? He made a statement which is new to me, and which is of the utmost importance. Do I understand him to say that TAUTE, when he called at the Nyasaland laboratory and was asked whether he would be willing to inoculate himself with the strain of game trypanosome with which they were concerned, replied that he would not do this because he felt sure the human trypanosome was in that district and must, therefore, be in the game ?

Colonel HARVEY : Yes, and that was also our opinion, that if you got human trypanosomiasis, the fly is infected with the parasite, and, therefore, the game of the district was also bound to be.

Dr. A. BALFOUR : As a member of Lord Desart's Committee I may perhaps be permitted to contribute to the discussion. As regards the main point under consideration I have nothing to say, but I think it is useful to speculate a little upon those problems of immunity which Professor YORKE has raised.

In the case of *Trypanosoma gambiense*, is there really a *natural* resistance of man to the infection ? May it not rather be that man is the original mammalian host of this trypanosome ? I confess that, along with others, I had always thought the comparative immunity of the West Coast native to be due to the fact that sleeping sickness was an old disease on the West Coast, and the native population had, as a result, acquired immunity, presumably in the same way as is seen in the case of other communicable diseases.

Again, what really occurred in Uganda ? The ordinary view is that Stanley opened up Central Africa, that the West Coast sleeping sickness was introduced into Uganda, and that *T. gambiense* attacking an immune population played havoc with it, just as measles did in the case of the Fijians.

On the other hand, as Dr. DUKE has shewn, there is evidence that sleeping sickness was known in Uganda long before the outbreak of 1901. The natives had a name for it, *mongota*, signifying "to nod the head."

Was this original Uganda sickness *T. gambiense* infection ?

Is it the case that merely a new strain of *T. gambiense* was introduced into Uganda ? Could a new strain, apart from a new trypanosome, produce the terrible effects which occurred, and which used to be attributed to the new disease ?

Is Dr. DUKE's interesting and ingenious hypothesis, cited by Professor YORKE, correct ? There are, as Dr. BAGSHAWE has stated, points against it. For example, if the transmission was mechanical, why was the disease completely checked when the natives were removed from a palpalis area ? Though no longer in danger from tsetse-flies, they were still liable to attack by mosquitoes and other biting insects, and the work of HECKENROTH and BLANCHARD has shewn that experimentally, at least, mechanical transmission by mosquitoes can occur.

The immunity problem in Rhodesia and Nyasaland is specially interesting. There is little evidence to shew that the disease is an old one in these regions. I remember Sir JOHN KIRK—an historic figure—who gave evidence before the Desart Committee, saying that when he was in those parts of Africa with Livingstone there was no word of any form of sleeping sickness. Are we then witnessing, as has been suggested, the gradual adaptation of an animal trypanosome to life in man, the latter having hitherto been naturally resistant to the infection, as he is in the case of *T. congolense, vivax, evansi, pecorum*, etc.?

If so, and if man becomes fully adapted to *T. rhodesiense*, may we one day expect an outburst of Rhodesian sleeping sickness in epidemic form? In any case it is curious that, at the present moment, *T. rhodesiense* is known to be more virulent to man than *T. gambiense*. Why then is the infection not more extensively spread? There are two possible explanations:—

1. It kills off its human host more rapidly, and hence the period of infectivity of the latter is shorter. At the same time it must be remembered that those infected live for a considerable period.

2. It may be necessary to distinguish between what we may call a pathogenic virulence and a virulence of propagation. Something of this kind is seen in certain outbreaks of small-pox, where you may have a severe but limited outbreak, and, on the other hand, a mild epidemic extensively widespread. In the case of sleeping sickness, however, the question of the tsetse-fly, of course, complicates the problem; hence the necessity for entomological research.

All these questions are at present of a hypothetical nature, but it would be interesting to hear Professor YORKE's views regarding them, although, I fear, it would take more time than we can spare to discuss them adequately.

Dr. C. M. WENYON: Dr. BALFOUR has told us that Sir JOHN KIRK had informed the Committee that sleeping sickness had not been recognised in Rhodesia, and on that account it is possible the Rhodesian infection was a new disease. Is it not a misnomer to talk about the Rhodesian disease as sleeping sickness? It does not produce sleeping sickness. A person who gets the Rhodesian sickness dies quickly of infection which does not take the sleeping sickness form. Therefore, the disease was probably overlooked.

Dr. H. S. STANNUS: I have seen trypanosomiasis due to rhodesiense infection with symptoms which could not be distinguished from those of the Uganda illness due to gambiense.

Professor W. YORKE (in further reply): Sir JOHN BRADFORD misunderstood me, for he was under the impression I was blaming the Government for not consulting this Society in this matter. On the contrary, I was rather suggesting that I thought this Society ought to be in a position—and ought to so organise itself that it should be able—to give advice to the Government on matters of the sort; that I still think is an important function of such a body as this. If we are not ready to offer advice, we are never likely to be asked for it.

Sir JOHN BRADFORD referred, at considerable length, to the Departmental Committee. The report of the Committee is published, and anyone can see what was said about the proposed game-destruction experiment. I do not wish to go into that, but I do wish to emphasise this fact most definitely: I am not placing difficulties in the way of entomological research; I want as much of it to be done as possible; I wish to encourage it. What I object to, in the first place, is the method adopted. There is an atmosphere of secrecy about it. It has even been contended again to-night that the report of the Glossina Sub-Committee is a secret document. It has been discussed publicly; certainly the principal speaker at the meeting I have alluded to discussed it. It is being used to obtain money, and not a small sum—£50,000 to begin with. This Society must feel that those responsible for that report ought to be in a position to justify that expenditure of money. I say their organisation is defective. You cannot, by planting two men—even though those two be Major AUSTEN and Dr. MARSHALL—in the middle of Africa, with insufficient funds at their disposal, without other European help, solve those problems which it is stated in the report have to be solved. That is my main indictment, and no one has attempted to reply to it.

I was very much interested in what Colonel HARVEY said; I think the question