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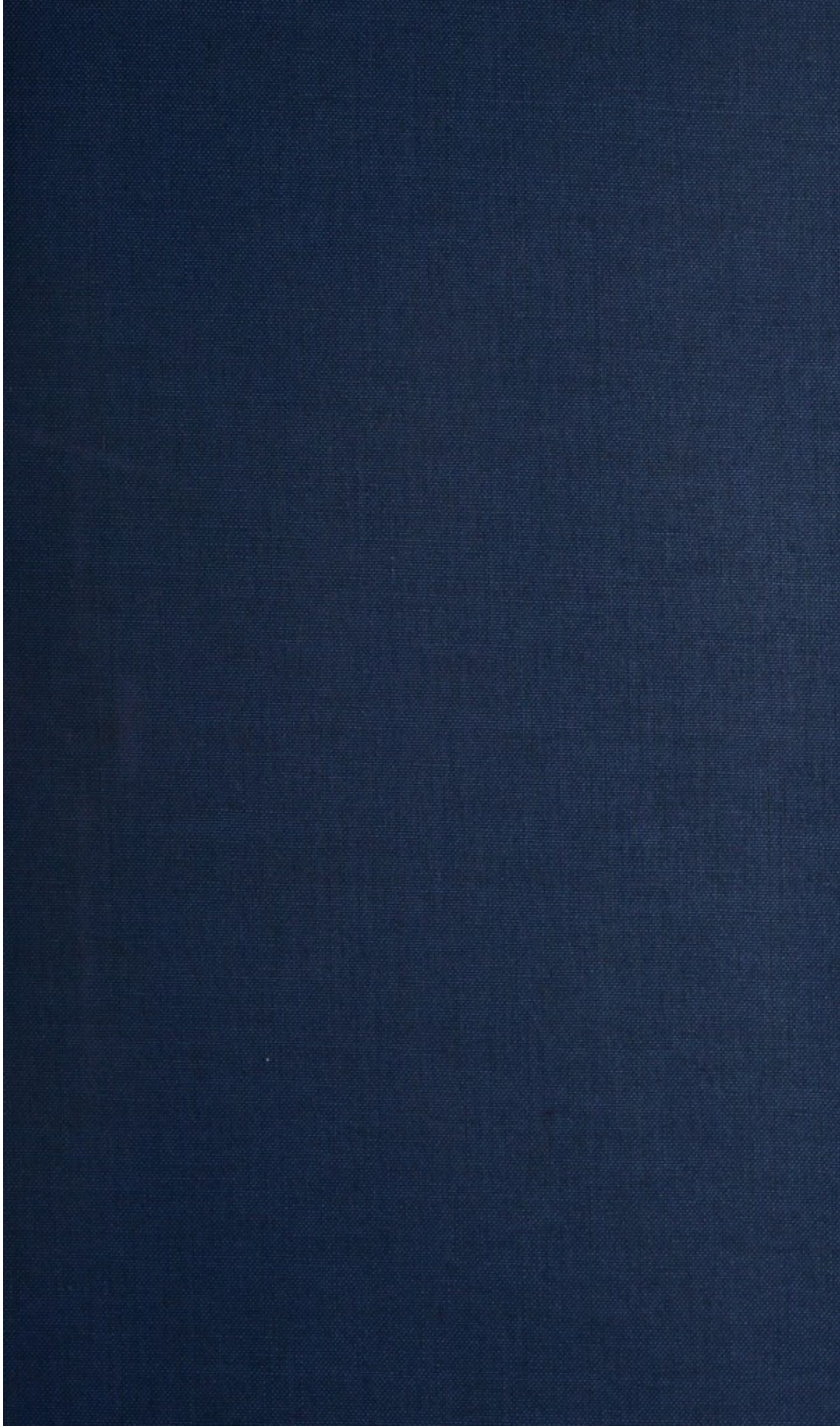
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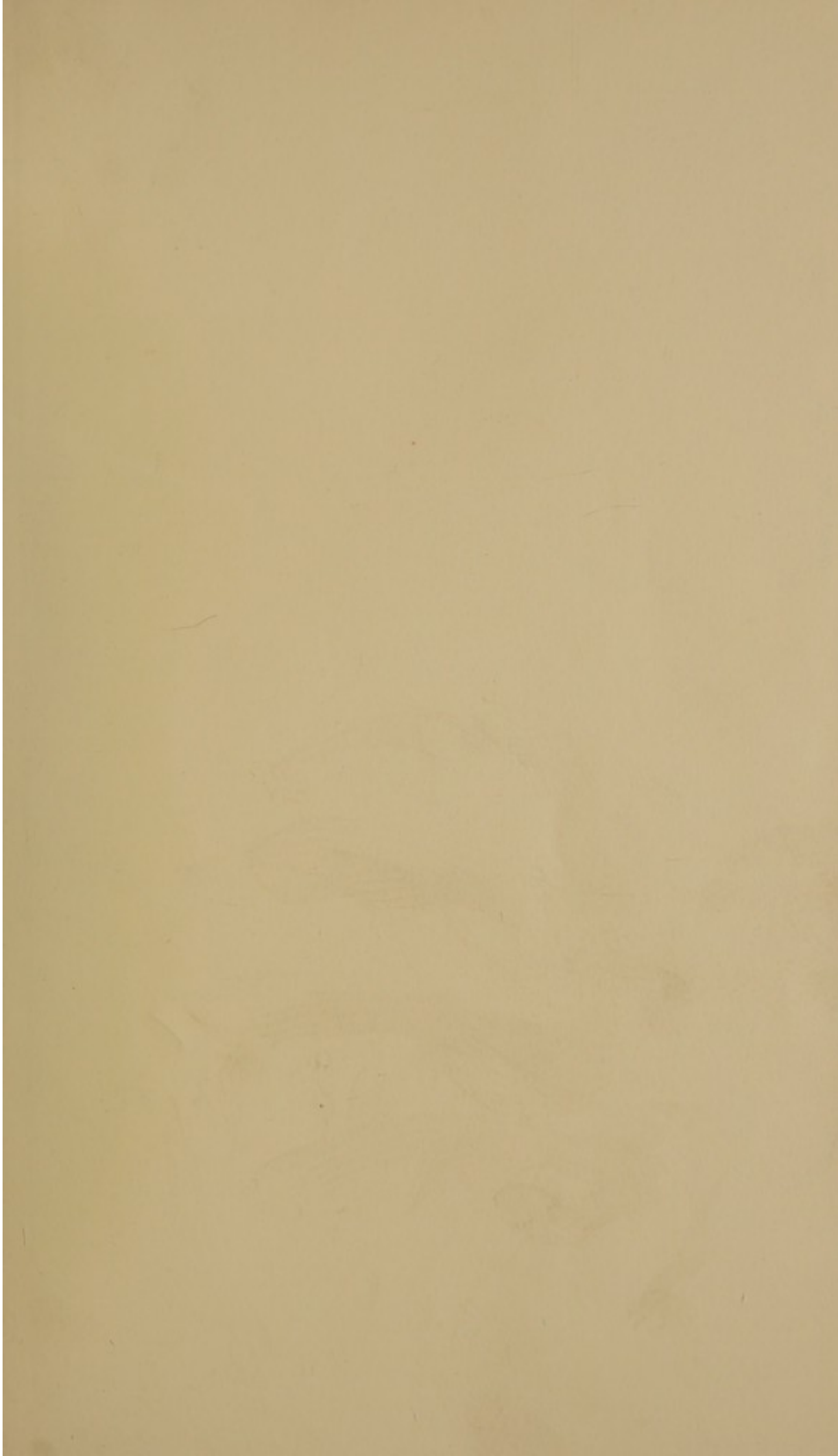
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UNITED STATES DEPARTMENT OF AGRICULTURE

WASHINGTON, D. C.

MINUTES OF THE

MEETING

DEPARTMENTAL COMMITTEE

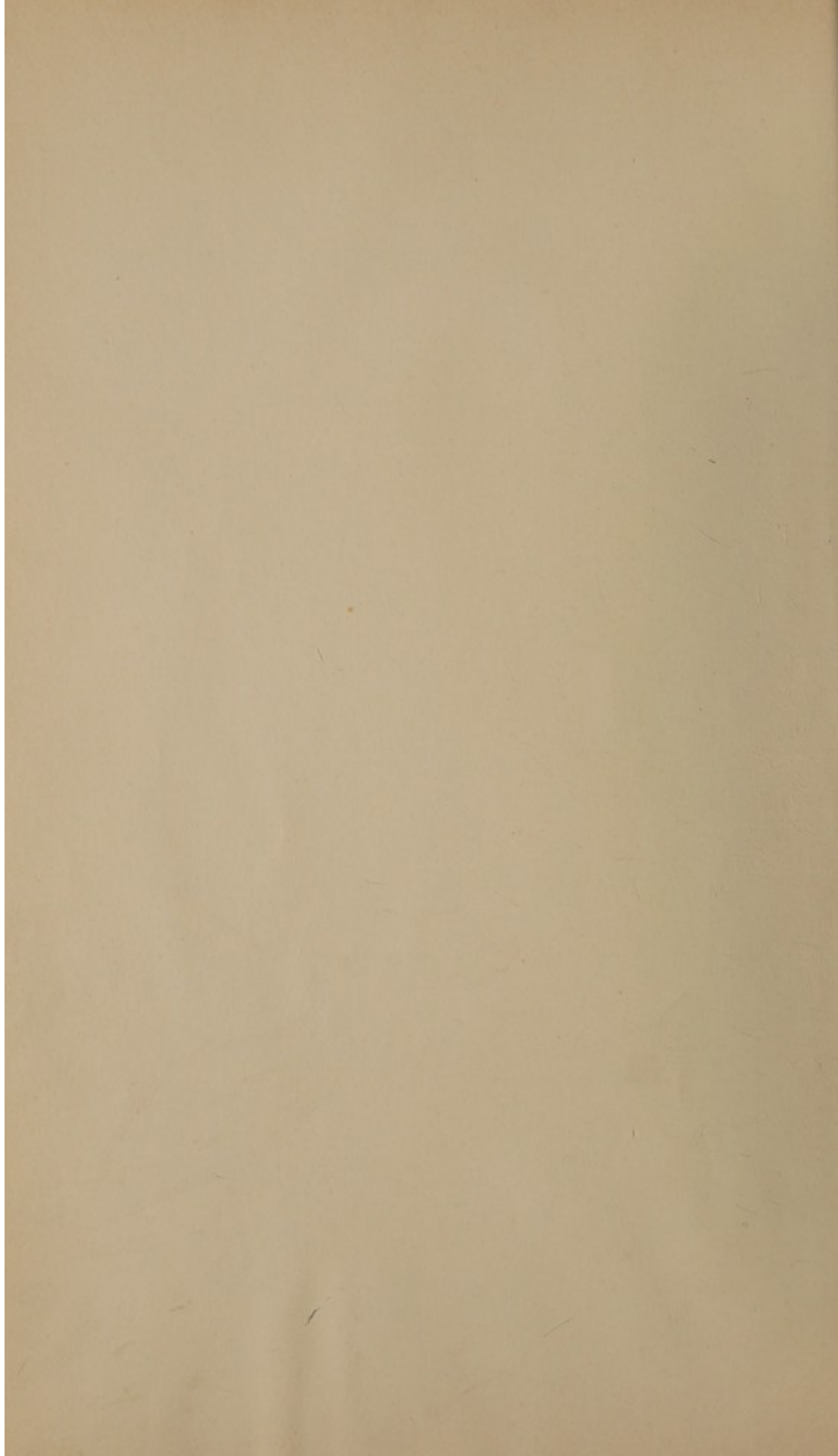
SLEEPING SICKNESS

THE BOARD OF THE DEPARTMENT OF AGRICULTURE

1904

REPORT OF THE DEPARTMENTAL COMMITTEE

1904



SLEEPING SICKNESS COMMITTEE.

MINUTES OF EVIDENCE

TAKEN BY THE

DEPARTMENTAL COMMITTEE

ON

SLEEPING SICKNESS.

(The Report of the Committee is printed separately as
[Cd. 7349].)

Presented to both Houses of Parliament by Command of His Majesty.

June 1914.



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DEPARTMENTAL COMMITTEE OF THE COLONIAL OFFICE ON
SLEEPING SICKNESS.

APPENDIX A.

Questions furnished to Witnesses.

QUESTIONS FOR WITNESSES.

Note.—These questions are sent to indicate generally the matters on which the Committee will seek assistance from the witnesses, and no written replies are required.

Witnesses are expected to deal only with the subjects which come within the range of experiments which they have made or inquiries on which they have been engaged, but any information as to the results of their observations on all matters connected with sleeping sickness will be valued by the Committee.

HUMAN TRYPANOSOMIASIS.

1. In what countries have you observed sleeping sickness and for what time?
2. What was the nature of your observations (administrative, clinical, research, &c.)?
3. When was the first case observed in the districts with which you are familiar?
4. Whence did it come? Do the natives consider it a new disease?
5. What is the incidence and case-mortality of the disease?
6. What is the average duration of the disease? Does it appear to be getting milder in character or more severe?
7. Is it spreading or likely to spread? Is it epidemic or likely to become so?
8. Has a systematic search for cases been made among the inhabitants?
9. How long were trypanosomes searched for in the blood or glands of suspected cases? Was the thick film method or centrifuging employed, or was blood inoculated into susceptible animals?
10. Is it difficult to persuade the natives to submit to examination? Have you any reason to suspect that cases are concealed by them?
11. Were trypanosomes numerous or scanty in the blood, glands, or cerebro-spinal fluid?
12. Have you noted any periodicity in the number of trypanosomes in the blood?
13. Does infection of man appear to occur with greater frequency at any particular season?
14. Was opacity of the cornea a frequent complication?
15. Do you consider that only one species of human trypanosome is in nature transmitted by one species of tsetse, or that any tsetse may in nature transmit any human trypanosome?
16. How far are trypanosomes of the "*brucei* group" (*T. brucei* vel *rhodesiense*, *T. gambiense*, *T. evansi*, and *T. equiperdum*) distinct and distinguishable from other trypanosomes?
17. With regard to the *brucei* group, how far can *T. gambiense*, *T. rhodesiense*, and any others be distinguished morphologically—
 - (1) in man?
 - (2) in mammals, birds, and reptiles?
 - (3) in the fly?
18. How far can the varieties of trypanosomiasis produced by the trypanosomes of the *brucei* group be distinguished—
 - (1) in man?
 - (2) in different mammals, e.g., monkeys, antelopes, domestic stock, and experimental animals?
 - (3) in birds?
 - (4) in reptiles?
19. Are pathogenic trypanosomes, whether actually producing disease or not, found in any area where—
 - (1) no large wild mammals exist?
 - (2) no tsetse have been discovered?

20. Do you consider that the bite of an infective *Glossina* is the only way in which infection takes place?

21. Do you consider it possible that Tabanidae or other blood-sucking arthropods may play a part?

22. Do you regard man as very susceptible, susceptible, or relatively immune to the disease of either type?

23. May man become tolerant of the trypanosome, as is stated to be the case with antelope?

24. Do you consider it probable or improbable that there may exist stages of the trypanosome other than those known to occur in man and the fly?

25. Is there anything in the nature of the disease as met with in man and animals which would support the hypothesis of an unknown stage in the life cycle of the trypanosome?

26. What part do human cases of trypanosomiasis play in the spread of the disease and as reservoirs of the virus?

27. Can infected flies transmit the trypanosome to their offspring?

28. Do you think that the different types of trypanosome are distinct and invariable species, or are some or all of them types varying and differing according to environment?

TRYPANOSOMIASIS IN DOMESTIC ANIMALS.

29. Is trypanosomiasis common in domestic animals in the districts with which you are familiar? When was it first observed and whence was it introduced?

30. What is the estimated amount of stock in this district? What mortality is caused by trypanosomiasis?

31. Is it spreading or likely to spread, or become epizootic?

32. Has a systematic search for cases been made among the stock? What methods of search were employed?

33. What animals are infected, and with which species of trypanosome? Is the disease fatal to these animals, or do they exhibit tolerance to the presence of the trypanosome?

TRYPANOSOMIASIS IN WILD MAMMALS.

34. Are wild mammals common in the district, and what species are found?

35. To what extent are they infected with trypanosomiasis and with what species of trypanosomes?

36. Is the disease fatal to these animals, or do they exhibit tolerance to the presence of the trypanosome?

37. Is trypanosomiasis encountered in parts of the district which are free from tsetse?

38. Are trypanosomes rare or abundant in the blood of infected animals?

39. Did you study the disease experimentally, and if so, are you aware whether your results were in accord with those of other observers?

40. What is your personal view as to the suggested danger of infected wild animals acting as reservoirs of the trypanosomes causing—

- (1) Human trypanosomiasis?
- (2) Trypanosomiasis of domestic animals?

MEASURES FOR CHECKING TRYPANOSOMIASIS.

41. What steps would you recommend for checking trypanosomiasis—

- (1) in man?
- (2) in stock?

42. What measures have been tried in the districts with which you are familiar, and with what results?

43. With regard to the following suggested lines of action, state whether you consider any or all of them as—

- (1) desirable, and if so, on what grounds?
- (2) feasible, and if so, how action could be carried out and what expenditure would be incurred?
 - (1) General extermination of game.
 - (2) Local extermination of game.
 - (3) Clearing operations.
 - (4) Removal of healthy natives from fly areas.
 - (5) Segregation of the sick.
 - (6) Destruction of infected stock.
 - (7) Destruction of tsetse.

EXPERIMENT IN GAME DESTRUCTION.

44. Would you recommend that an experiment in game destruction should be carried out? If so, state exactly what you would hope to prove by such an experiment. Supposing that it were established in a given area by the experiment that a certain result followed from the extermination of the game, do you consider that it would be justifiable to argue that the same result would follow from the same measures if adopted in any other area? Do you think an experiment in game destruction is feasible? If so, say—

- (1) Exactly where it can be carried out.
- (2) The size of the area for the experiment.
- (3) The means by which the game would be removed and other game kept out of the area.
- (4) Whether fencing would be necessary. If so, what kind of fence. (Cf. No. (10) *infra*.)
- (5) What staff would be required for the conduct of the experiment.
- (6) The cost.
- (7) How the experiment would be carried out in detail.
- (8) How long would elapse before you could expect any definite conclusions from the experiment.
- (9) The possible fallacies, and whether they might vitiate the experiment.
- (10) Whether similar results could be obtained by the selection of an isolated fly area for the experiment, without fencing.

TREATMENT OF TRYPANOSOMIASIS.

45. Do you know of any successful prophylactic or therapeutic remedies which have been adopted in the case of—

- (1) *Gambianse* disease?
- (2) *Rhodensiense* disease?
- (3) Trypanosome diseases of stock?

ENTOMOLOGICAL INVESTIGATION.

46. Do you think that more information is needed as to the bionomics of tsetse, in particular *Glossina palpalis*, *Glossina morsitans*, and *Glossina pallidipes*?

Do you think that it is desirable to carry out extended entomological investigation with a view to obtaining fuller knowledge of the "carrier"?

47. What special points in the bionomics of tsetse would you wish to have investigated?

48. Do you regard it as possible to devise measures for the destruction, or at any rate the limitation of the numbers, of the fly?

SPECIAL QUESTIONS FOR ENTOMOLOGISTS ONLY.

49. In the fly districts with which you are personally acquainted, do you know where *Glossina palpalis*, *Glossina morsitans*, or *Glossina pallidipes* breed?

50. Is there any peculiar physical character (e.g., soil, vegetation, moisture) in these breeding places as compared with the surrounding portions of the fly area?

51. Do the mother flies select particular spots in the breeding grounds in which to deposit their larvae, or drop the latter about promiscuously?

52. If particular spots are selected, are these associated with vegetable humus or decaying wood?

53. Have you anywhere noticed a marked preponderance of one sex over the other?

54. In Principe Island a great reduction in the numbers of *Glossina palpalis* has been effected by trapping the flies by the aid of men dressed in black clothes smeared with birdlime. In the districts with which you are acquainted, do you think it would be possible to reduce the numbers of or abolish *Glossina palpalis*, *Glossina morsitans*, or *Glossina pallidipes* by similar means? Do you know of any other system for trapping the fly?

55. In a district in which *Glossina morsitans* and big game were both present, have you ever known the fly permanently to disappear while the game remained, or to remain when the game disappeared?

56. As regards the possibility of eradicating *Glossina palpalis*, *Glossina morsitans*, or *Glossina pallidipes*, can you suggest any lines upon which experiments might usefully be made?

57. Are you acquainted with any area in which the physical characters are uniform, and throughout which game occurs, while *Glossina palpalis*, *Glossina morsitans*, or *Glossina pallidipes* is found only in certain spots? If so, can you suggest any reason for this limitation?

58. Is there any difference in the extent of the areas occupied by *Glossina palpalis*, *Glossina morsitans*, or *Glossina pallidipes* in the wet and dry seasons respectively? If so, can you account for this?

APPENDIX B.

Oral Evidence.

MINUTES OF EVIDENCE TAKEN BEFORE THE DEPARTMENTAL COMMITTEE OF THE COLONIAL OFFICE ON SLEEPING SICKNESS.

FIRST DAY.

Friday, 10th October 1913.

At Winchester House, 21, St. James's Square, S.W.

MEMBERS PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Dr. ANDREW BALFOUR, C.M.G.
Sir JOHN ROSE BRADFORD, K.C.M.G.
Mr. E. NORTH BUXTON.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Dr. W. A. CHAPPLE, M.P.
Colonel Sir W. B. LEISHMAN, F.R.S.

Dr. C. J. MARTIN, F.R.S.
Mr. J. DUNCAN MILLAR, M.P.
Dr. P. CHALMERS MITCHELL, F.R.S.
Professor R. NEWSTEAD, F.R.S.
Mr. H. J. READ, C.M.G.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
Mr. A. C. C. PARKINSON (*Secretary*).

Surgeon-General Sir DAVID BRUCE, C.B., F.R.S., A.M.S., called and examined.

1. (*Chairman*.) We are very much obliged to you, Sir David, for supplying us with a very interesting statement. Unless you have any objections, as we have all read these papers of yours, we should propose to-day to treat them as read (they will be printed as an appendix to our report), and that the members of the committee should ask you such questions as they desire, which will save both your and our time, and I think would be a convenient way of conducting the inquiry. It is the case, is it not, that there are two theories developed about sleeping sickness; one school has supported the idea that it was brought into Rhodesia and Nyasaland from Lake Tanganyika, and is in fact the same disease as broke out in Uganda, conveyed by persons who have come from that country and communicated by the agency of *Glossina morsitans* to cattle and people in Rhodesia and Nyasaland; the other that the disease in Rhodesia is caused by a different parasite, and produces a different disease, which, although of the same family, is quite different in its character. That is so is it not?—That is my opinion. I hold the second theory.

2. I understand that the view which you have taken as the result of your experiments, without much hesitation or doubt, is that the disease in Nyasaland is, in fact, separately acquired and is a separate disease, and is caused by *Trypanosoma brucei* or *rhodesiense*, if they are the same, conveyed by *morsitans*, and developed in a different way from the disease as it existed in Uganda. In the first instance, in the questions I have to ask you I should rather proceed on the basis of your own theory, that the *brucei* or *rhodesiense* disease is conveyed by *morsitans*, and I should like to ask one thing, which I do not think is referred to: is it possible, or is it known whether it is possible, that the trypanosome may be hereditary? Supposing you had a bull or a cow of any species that had trypanosomes in its blood, might the offspring, quite apart from any other agency,

be born and develop trypanosomes in its blood?—No, that is quite impossible. From everything we know there is nothing hereditary in this disease; the trypanosome never passes from an infected animal to its offspring except through the agency of a fly.

3. So that however the trypanosome gets into any animal or man, it must be put there by a fly?—Yes, in nature it is put there by the fly; in the laboratory it is put there by a hypodermic needle.

4. Then the fly must either acquire it by heredity or by some trypanosome; it gets to the fly somehow and the fly communicates it to the animal?—There is no difficulty in the fly getting it, because the wild animals round about are full of these parasites.

5. The animal cannot get it without the fly, and the fly cannot get it without the animal. I know I may be asking questions which cannot be answered, but it is interesting. Is your experience enough to say whether in countries where there is no fly the game is free from the trypanosome?—According to our experience that is so; in fly-free areas the antelope are free from those pathogenic trypanosomes. Of course, naturally, if it was within a few miles of a fly area one would expect to find some of the game infected. I mean 50 or more miles away.

6. I know it was suggested in one paper I read that that was the case, but I did not know quite what the material was on which it was founded.

7. (*Dr. Chapple*.) Do I understand you to say that there are no pathogenic trypanosomes where there are no flies?—That is according to our experience. We have examined some hundreds of animals, but we could give a more trustworthy answer, of course, if we had examined several thousands.

8. (*Mr. Rothschild*.) May I ask in connection with that: it is I believe a known fact that the infection is transmitted from the tick to its offspring, but

10 October 1913.]

Surgeon-General Sir DAVID BRUCE, C.B., F.R.S., A.M.S.

[Continued.]

according to the various publications on the subject I was unable to find out, whether, when laboratory-bred tsetse-flies had been examined, as it is stated that the infected flies only become infective after 17 to 32 days, the fact had ever been ascertained as to the transmission of the trypanosome by heredity in the fly, that is to say, whether these laboratory-bred flies had been examined after a period of from 17 to 32 days to see if there might not be a development in them of trypanosomes?—There is no transmission of the parasite from the mother fly to the offspring.

9. How has that been ascertained? Has a laboratory-bred fly been examined?—Yes, thousands of them have been examined.

10. (Chairman.) I was going to ask as an interesting speculation whether, when you said, as is probably the case, that in a fly-free area cattle which have not been with flies have not been infected with trypanosomes, the contrary would be the case, namely, that if there were no wild animals in the area the fly would be free from trypanosomes?—I have never been in a fly area in Africa where there were no wild animals; I have heard of such places.

11. Now you say that without a reservoir the fly cannot acquire the disease, and therefore it becomes of the utmost importance to know where or how the fly acquires the trypanosome, whether by heredity, from a wild animal or from some other source unknown. Would it not be the case that if the fly had other means of acquiring the trypanosome, however important cattle may be as a reservoir they would not be the only cause, or even necessarily the chief cause of the infection?—In my opinion there is no way of the fly obtaining this infection except through the wild game, the wild mammals. They could get it from a native, of course, he is a wild mammal, and they could get it from cattle and goats if these animals existed in the fly country, but they do not get it from moss or from trees, or from vegetables, or birds, or lizards, or crocodiles. They get it from the wild mammals and nothing else.

12. (Dr. Chapple.) Nor from each other?—Nor from each other; they do not get it from each other. We have tried the experiment again and again of putting clean flies among infected flies and allowing them to soil themselves with the dejecta of these infected flies, but we have never got any successful result.

13. (Chairman.) I would like to follow this up: do you accept the statement by some observers that flies may exist in large numbers in the absence of the larger game mammals?—I think the flies, if they do not get the particular kind of food they like best, will take what they can get, like the rest of us, so that if there were very few mammals along the shore of a lake, say the Victoria Nyanza, and a great many crocodiles, the particular fly bred there would get into the habit of living a great deal more on the crocodile than the ordinary fly would, which could choose its food. The fly, given its choice will drink the blood of a warm-blooded animal rather than a cold-blooded animal. It is very difficult to get them to feed on a crocodile, or a lizard, or a frog, but the warmer the blood is, I think, the more they like it; that is to say, the *Glossina palpalis* will feed much more readily on a chicken than on a monkey, but they will feed quite readily on a monkey, while you can hardly get them to feed on the lower animals, such as the frog and lizard.

14. Is it the case with regard to *Glossina morsitans* that there is no very great tendency for them to feed on reptile blood?—It may depend on the place, of course, but where I have been in Zululand or in Nyasaland there is no necessity for them to search for these reptiles; the place is teeming with great herds of big animals.

15. That is not quite the question I began with; what I really wanted to know from you is whether you do accept the view which has been put forward, that there may be flies in very large quantities in countries where, at the time, at any rate, there are no large wild mammals; do you controvert or accept that view?—I cannot say anything more than this, that the fly, both male and female, must have blood, and it must

have the blood every four or five days. If they cannot get it from a large mammal they may be able to get it from the smaller mammals. I have no experience of flies feeding much on the rats or mice of the country.

16. Birds?—In Uganda they do; *Glossina palpalis* lives amongst birds, but in the *morsitans* area, on the whole, there are very few birds and they are the kind of birds which would not lend themselves to being fed upon. You see, the birds on the Victoria Nyanza lend themselves to being fed on by the fly because they are huge cormorants and divers, and they sit on low trees along the lake shore with their wings out, drying themselves, so that there is very good opportunity for the fly to creep under the wings, and bite them on the featherless portions there; but the ordinary birds you find in the *morsitans* area, guinea fowl, vultures, &c. do not sit in that kind of way, and they do not give the same opportunity for the fly to feed on them. As a matter of fact, we have not studied the food supply of *Glossina morsitans* in the *morsitans* area in Nyasaland. We did it to some extent with regard to *Glossina palpalis* in Uganda, but we have not sat down and dissected, say, a couple of thousand flies caught and dissected straight away, so that I cannot answer the question as to whether *G. morsitans* feed much on reptilian blood or on avian blood in the *morsitans* area. I should say not. The contents of the stomachs of thousands of *morsitans* brought up to the laboratory were noted, but as far as I remember reptilian or avian blood was never found.

17. Your view is that whether they did or not, blood of some kind is a necessity?—Absolutely. If the men who went through fly-country and found no big mammals had sat down and dissected a thousand flies and examined the contents of their stomachs under the microscope, and given us a table describing what they found, it would have been much more satisfactory than simply walking through the country and saying, "The place is swarming with fly, and there are no big mammals." It would have shown what they were feeding on, because they must feed; just as water is necessary for a fish so blood is necessary for the tsetse-fly.

18. To what extent could man be a reservoir?—If there are many men about, a man is as good a food supply as anything else, and I should say a good deal better, because he has no hair. I have seen a couple of hundred tsetse-flies on one man's back at the same time.

19. The domestic cattle or domestic animals also form reservoirs?—If the cattle lived in fly-country they would form a reservoir, but they do not; there are no cattle in those places.

20. Is it not conceivable that in a herd which apparently is immune from fly there might be individuals that are immune and would live to form a reservoir?—No.

21. How can that be conclusively proved? All the herd of cattle do not die?—There are no cattle in fly country; there are no cattle in the Zululand fly country or in the Nyasaland fly country. As a matter of fact, you do not find cattle in the *morsitans* country. You may find an odd one here and there.

22. This difficulty does remain, that although it is conceded that wild mammals form extensive reservoirs for trypanosomes they do not form actually the only reservoirs and their absence would not necessarily deprive the fly of food from other animals which might also contain trypanosomes?—I should say that practically if there were no big wild animals the fly would not get infection. The infection the fly would get from men with this disease, I think, would be very small. The cases are very few and far between and men only live a short time. Most of the time they are so ill they are unable to travel about. It is quite possible a few flies might become infected, but that is very different from the condition of things at present, where the flies are living among thousands of wild animals every one of which practically is infected, and these flies live on them and move with them and are constantly sucking their blood. They are constantly having the opportunity of being infected from the wild game. I think you could put the men and the cattle, the monkeys and

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the rats, the goats and the mice out of practical consideration.

23. With an infected mammal would the trypanosomes be present in large numbers in the blood?—Of an infected antelope?

24. Yes.—In very small numbers.

25. The reason I asked that question is, that a fly biting a mammal would not necessarily acquire a trypanosome from that mammal?—Certainly not. We have done a good number of experiments and are able to give a percentage of the number infected; that is to say, we had antelope in Uganda and kept them for some years and fed fly on them and found that the fly were very readily infected from the antelope. The experiments we made seemed to show that the fly were more readily infected from antelopes than from any other animal. There is also a certain amount of evidence—it has not been proved at present—that there may be bodies other than the ordinary forms of trypanosomes which may be present in the blood in large numbers, which the fly take in with the blood and which may give rise to infection. Personally I do not think that it is at all proved, but there has been a good deal of writing lately on infective granules.

26. Would this be right—whatever the actual numbers may be, the proportion of the flies becoming infected from biting mammals would not be a very large percentage?—No; if a hundred flies bit an infected animal you might get (it depends on the species of trypanosome and a number of other things) five or six or seven per cent. infected.

27. Of course, of those infective flies only a small proportion would come into contact with man and only a small proportion of men would contract the sickness. I am trying to see how far the presence of mammals is a formidable danger to man. At present, as I understand the disease, it has not assumed any formidable proportions in Nyasaland and Rhodesia?—Not among the men. Of course it is very deadly to domestic animals, that is to say you cannot take a dog through 10 or 15 miles of the country once without losing it, or if you took a couple of oxen along the road 20 miles, ten to one you would lose both of them. The other day, there were three dogs brought to Domira Bay and instead of being sent through the fly country in a fly-proof box or boxes they were sent along the road and we lost two of them. That is the general experience. It is a very dangerous country for the domestic animals. I look on man as being not very susceptible to this disease, but that is a thing you cannot experiment upon and you cannot give a numerical answer with regard to it.

28. To sum it up. I do not want to go into this at great length—your conclusion is that wild game is the chief source?—I think that they are the chief source, and I think the other sources may be left out of account. I think in regard to man, perhaps an infected man is rather important because, in the case of a trypanosome not accustomed to live in the blood of man, if the fly took that trypanosome from the blood of man it would be more likely to become infected with a strain of trypanosome pathogenic or dangerous to man than if it had taken it from a wild animal.

29. You know that an experiment has been suggested with regard to fencing in an area and what that experiment is to prove will become a very important question. I suppose you say that it might show that the absence of the game meant the absence of the fly; that is the one thing?—Yes.

30. The other thing is that even if the fly did not disappear, it might after the game was driven out be free from trypanosomes and harmless?—Yes.

31. Those are the two propositions which might be established?—Yes.

32. As regards the first, of course that might be established fairly soon, but as regards the second, whether the trypanosome ceases to exist in the fly, it would be a very considerable time before you could speak with any certainty upon that?—Yes, it would take two or three years.

33. And as regards the disease in man, having regard to the fact that it is not a very formidable

disease at the present time, it might take a very long time indeed before you were assured that the disease in man had ceased, partly because there are many cases of which you would not hear at all, but, in any case, it would take a long time before you could be assured that the disease in man had ceased to exist?—I do not suppose the experiment would assist in that very much, because in the fenced-in area there would be no men.

34. Assuming that the game is all destroyed or driven out, there might be game immediately outside the cleared area, and would it be possible, with any confidence, to prophesy the result with regard to the fly without further knowledge than we possess as to the distance which the fly travels? Is not that a factor of importance which we cannot gauge at all at present?—I suppose the fencing-in experiment would be as good an opportunity of discovering these habits of the fly as any other.

35. It is important as a factor in considering the probabilities of the experiment giving some evidence of value in checking the disease; the distance the fly travels must be a matter of importance in connection with that?—We know the fly travels, because we see him circling round us when we are travelling at 30 miles an hour on a motor bicycle, so that, no doubt, he can travel for a short distance at an enormous rate, there can be no doubt about that. The fly also sits on animals and men, and may be carried 50 miles in a day. They were often carried up into our camp at Kasu, but as to whether this is a very important thing from the practical point of view I do not know.

36. You would agree with this, would you not, that if you could eliminate the fly you eliminate the disease, as far as present knowledge goes?—Certainly.

37. A complete knowledge of the habits of the fly, if obtainable, is of the first importance?—Yes, I should say of great importance.

38. The expulsion of game of itself would merely be a shifting of the fly, not in any way an elimination of it?—I imagine the fly would simply go outside and live among the wild animals outside as they have been accustomed to do all their lives. It would be the most natural thing for them to do.

39. Therefore, if the experiment showed that with the mammals the fly left the area, the argument would be that, wherever possible, in any country that was settled or about to be settled, all the mammals should be destroyed. I am trying to think where the experiment would lead us?—You may take it as a certainty that when a country becomes settled the wild animals go. There is no use in considering what would happen, because it always happens that whenever a country becomes settled the wild animals go. The farmer is not going to have his fences knocked down by herds of stampeding zebras, and wildebeestes, and that kind of thing, or his telegraph wire knocked down by giraffes. The moment men settle a country the wild game go. Of course, in the north of British East Africa at present, where the conditions are a little artificial, there is a certain amount of settling and still a great deal of game, but I imagine a great number of those settlers keep down the wild animals as much as they can.

40. Does the fly disappear?—There are no fly there.

41. If the mammals disappeared in that way, would not the fly disappear?—You must take from me mere hearsay evidence. It is only from reading that I can answer. My own idea is that where the wild animals have been driven away the fly has gone too, but we have never made the experiment. This is one of the objects of this fencing experiment. I have read many accounts. Any practical person will tell you, for example, "Here is a great big piece of jungle or fly country, suppose they find a gold mine here. When they come there at first the place is swarming with flies, but when they bring a few thousands of natives and a few hundreds of white men to work the mine, in a short time you will not find a big mammal within 30 miles, as they are all shot, and you will not find a fly either." That is what the practical man says.

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42. I have only two more questions to ask. Are you satisfied that *Glossina* of some kind is the only carrier of the trypanosome?—Practically, yes.

43. Are you also satisfied as to the question of mechanical transmission, whether there can or cannot be mechanical transmission either from man to beast or from beast to man?—I think on very rare occasions there might be a mechanical transmission, but I think it might be left out of account. If you go up the country where there is no fly you hear nothing of tsetse-fly disease; it is only when you are living on the edge of the fly country that you begin to hear about it. Fifty or sixty miles away from the fly country the cattle live in thousands, and goats and dogs, and you never hear a word about tsetse-fly disease, and yet there may be fly-infected animals passing through now and again.

44. As regards the disease in Rhodesia, in your judgment has it not been endemic? If you are right in saying that it is a separate disease, the conditions for it have always existed?—Yes.

45. And the presumption would be that from that point of view it is a disease which has always existed there?—I think it is an endemic disease, and that it has always existed there.

46. (Mr. Read.) I think there is an impression in some quarters that there is a very heavy mortality at the present time from sleeping sickness among natives, and I think one ought to correct that impression as far as one can. You say here in your printed evidence that the mortality among the natives in Nyasaland from sleeping sickness is insignificant, and may be ignored; that is to say, as far as I understand, there have been about 120 to 130 cases in five or six years out of a total population of a million?—Yes.

47. Then with regard to Rhodesia you go on to say that the cases in the Luangwa Valley are not increasing from year to year, so that I presume you consider it is not a serious question there?—Yes.

48. In Uganda, may we say that the position has immensely improved since the natives were moved out of the fly area and that the disease has been checked at any rate for the present?—I think so; I think the disease has been stopped altogether in Uganda proper. I have not seen the statistics of late, but I have had letters from the Katakira from time to time, and he seems to think it is stopped in Uganda proper, but away up in the north-west of the country on the Nile, where nothing has been done, the thing continues as it was before. In Uganda proper I should say the sleeping sickness has been stopped altogether; there are no fresh cases.

49. Even in the north-west there is nothing approaching the dangerous position which existed in the territory round Victoria Nyanza?—No, it is a very fertile and populous part round Victoria Nyanza; up in the north-west it is a much wilder and desert country.

50. With regard to your fencing experiment, what time would that take to carry out? You do not state that.—It all depends on what kind of fencing experiment is carried out. What we thought of doing a couple of years ago was simply to employ the unemployed natives of the sleeping sickness area (there are about 20,000 there), say a couple of thousand or so, to build a wooden fence and to clear round it to the extent of, say, 20 yards on each side. I do not know anything about these things, but the magistrate of this district thought it was a perfectly feasible plan, and that the men could carry it out in about three months.

51. (Mr. Buxton.) What district is that?—The sleeping sickness area in Nyasaland. It is on the map there.

52. That is a very wide area; you said "In this district," and I presume you have in your mind a particular place, which it would be convenient to know?—The fencing experiment would be carried out, I presume, in some part of this sleeping sickness area or proclaimed area; it is some 70 or 80 miles long and some 20 or 30 miles broad. This is a thickly populated area, and you see the large number of villages. This is Lake Nyasa; this is the river which forms the northern boundary, and you see there are very numerous

villages dotted along the rivers, these red marks mean the cases of Nyasaland sleeping sickness which have occurred in this area. You see large portions which are desert, where there is no water and where there are no villages, and this is, of course, the place where most of the game is found (pointing on the map). This is all very full of game and very full of fly. That is the district in which we propose to carry out the fencing experiment.

53. (Mr. Read.) I see you propose the fence should be made "of logs from that kind of tree which, when placed in the ground sprouts, so that in a short time the area would be surrounded by a living hedge. This would protect the standards from white ants." How long would it take this tree to sprout and form this hedge? I suppose it would take a year or two, would it not?—I do not think so; I think at the end of the first rainy season most of them would be sprouting and living. It is the question of being alive. If you take a log of wood in these countries from many of the native trees and stick it in the ground it grows into a tree, so that you have within a year or so a living fence; they are living trees, and they grow bigger and bigger year by year.

54. You think it would take a year or so to form the fence from the time it was started?—Yes.

55. You give no estimate of the time required to carry out the experiment after the fence had been formed?—Say that the fence was built in the dry season and that it was finished by the 1st November, when the rains begin. In November and December when the grass is still short you might drive the game out of it. Then the following year, there having been no game in the area for six months, you might find out if there were fly left in it. If there were fly left in it you could find out the ratio of infected flies to non-infected flies, and then you would require to go on on the same lines the next year. I should say that in three years after the fence was built you might be able to get some information.

56. That is to say it would take four years, at any rate, from now. What staff would be required for all this? I suppose it is no small operation driving out the game from an area of 100 square miles, quite apart from the scientific investigations to be undertaken subsequently?—The Director of Public Works has given an estimate of the cost of the staff required. I am not a practical man in these matters.

57. That is only the cost of the fence?—No, he gives you the cost of the staff and what staff is required for five years.

58. That is for the maintenance of the fence?—For the making and the maintenance of the fence and the carrying out of the scheme.

59. I do not think the Director of Public Works would be competent to express an opinion as to what scientific staff was required; you would want a certain staff of entomologists to go round and make an entomological survey, and then I suppose you would want a certain number of doctors to go round and examine the game after they had been killed off?—I think that is a matter for the administration.

60. Still, it is a thing to be considered in estimating the expense?—To the administration, certainly very important.

61. In Part VII. of your evidence you rather fall foul of the entomologists, I see. You say, "Carpenter has been working in Uganda for several years and his results in this direction are practically nil"; as a matter of fact Carpenter has only been working for three years.—That word "several" is very difficult to define.

62. "Several" is rather overstating it, I think?—Has he only been three years?

63. (Sir John Bradford.) I should have thought two to three years, speaking from memory.—He was there a long time before I left two years ago.

64. Not more than three, I think.—I am sure it is more than two and a half, because he sent in reports before I left for Nyasaland; we considered his reports in Committee before I left England, and that is two full years ago.

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65. (Mr. Read.) Do you think in Uganda it would be possible to kill out the game; for instance, I believe there is an antelope there called the sitatunga, which is one of the animals implicated; it lives in the swamps, is very much sought after by sportsmen, but is scarcely ever seen or killed. That is one animal it would be almost impossible to clear out?—If you gave one of these islands to a syndicate who wanted to plant rubber upon it, and they had sufficient capital, there would be very few sitatunga in that island in a very short time.

66. I am talking more of the mainland.—Give them a strip of mainland.

67. (Sir John Bradford.) I have not much to ask you. With reference to the question asked by the Chairman as regards the reservoir and the part played by the game *versus* other mammals, cattle, and so forth, and man, is it not a fact of importance that the trypanosome is not very pathogenic to the game? Is not that one of the reasons why the game is such a very important factor as a reservoir?—Yes.

68. I think that had not been brought out, perhaps, although it is a matter of very great importance?—As far as one can see, the trypanosome has no effect on wild animals; they appear perfectly sleek and fat, and they do not appear to be affected in any way by the trypanosomes.

69. It would be correct to say that is one reason why the wild game is such a very important reservoir?—Yes.

70. Could you tell the Committee shortly the evidence that the game is immune? You look upon it as a fact that the game is immune, do you not?—I suppose it depends to a certain extent on the meaning of the word "immune"; if a trypanosome can live in the blood of the animal, that animal is not quite immune from the disease.

71. We might take it in the loose sense as to whether it kills the animal; what is the scientific evidence for or against these trypanosomes killing wild animals?—We have inoculated a good number of antelope with different trypanosomes, but none of these antelope have died of the disease. Some of the antelope in Uganda remained infective for two years or more. There is no evidence from the wild game in a wild country as to whether they are killed off by trypanosomes or not, because in a country of that sort if an animal dies it is eaten up within a few hours by the wild animals round about. The best evidence is, of course, to inoculate the ordinary antelope as a laboratory experiment, and we have done that quite 12 times. It is difficult to keep the antelope alive when you have got them, so that we do not have many opportunities of making these experiments, but the 12 or so we have made indicated that the animals have the trypanosome in their blood for a certain time, that they do not show any signs of disease, and after a certain time the trypanosomes become very scanty in their blood, but are still there and can still infect the flies if the flies are fed upon them.

72. Is it not the fact that there are antelope at the present time in Uganda which have been inoculated and are still living in health, and that their blood, at any rate, comparatively recently, was still virulent although they themselves were healthy?—That is quite true.

73. I am right in suggesting that that is one of the main reasons why the question of the reservoir in wild game is of such great practical importance?—Certainly.

74. If domestic cattle and men and so forth become infected with this trypanosome they die, and therefore the danger of spreading infection is shorter?—Yes.

75. There is another question I wanted to ask: Is there any evidence of pathogenic trypanosomes in wild game in fly-free areas?—All our experience shows not. We have examined a large number of antelope from fly-free areas, and we have never found pathogenic trypanosomes in them yet.

76. Does that apply to your work in Nyasaland or to your other work?—I am thinking more of the work in Nyasaland. We have a large number of specimens of blood sent in from fly-free areas round about.

When I say a large number I mean 200 or 300 perhaps. It would be better if it was 2,000 or 3,000.

77. At the present time you have not found any pathogenic trypanosomes in the blood of wild game in fly-free areas?—We have not, but I would not despair of finding them if I looked long enough.

78. As a matter of fact, it has not been found?—Not up to the present, and in the case of antelope in a fly-free area, that may be due to the fact that after a few months the trypanosomes may disappear so much out of their blood as to render it impossible to find them by ordinary blood examination.

79. They can be found in the antelope in the Uganda laboratory?—Not by the microscope but by inoculations, yes. What I meant was that the antelope in a fly country are probably always being re-infected.

80. As regards the fencing experiment, would it be correct to say that the main object of the fencing experiment from a scientific point of view is to determine and ascertain whether the percentage of infected flies would be diminished by the destruction of game?—If the fly remained in the fenced-in area then that would be one method of discovering the result of driving off the wild game; if the fly disappeared from the area that would be another.

81. That would be the main object of the experiment from the scientific point of view?—Yes. We have dissected some thousand flies last year and this year to form a sort of standard, and the ratio when I left was almost the same for this year as for the year before; that is to say, the same number of infected flies per thousand. It seemed to be very much the same this year as it was last year, so that it would not be at all difficult to find out whether that percentage had fallen or not.

82. You look upon that as a very important thing to ascertain?—Yes, I think so.

83. Further, you believe, having carefully thought out this question and knowing the ground, that such an experiment is possible?—Yes.

84. And you think the possible fallacies would not vitiate it?—It is very difficult to tell what will be the result.

85. At any rate you think from the scientific point of view, speaking as a scientific man, it is an experiment worth doing?—I think so, especially as I wanted to do it in an economical way and, as it were, just using the material on the spot. I spoke to all the different people round about there; the magistrates are very practical men, and the magistrate in charge of the district said he would undertake to do it, and that there would be no difficulty in carrying it out. The Director of Public Works was also approached on the subject and said that so far as he could see there would be no difficulty in carrying it out.

86. And lastly, the object of this experiment is not simply to determine the incidence of the disease in man, but also to determine the incidence of the disease in cattle, and to render stock raising more possible; is not that so?—You allow that this trypanosome infection in Nyasaland is more important from the cattle point of view than from the human point of view?—Yes.

87. So that this experiment, if successful, would afford information of some value with reference to problems of that kind, as well as with regard to the human problem?—Certainly.

88. (Dr. Bagshawe.) There is only one thing I should like to ascertain. You were telling us just now about the antelope experiments in Uganda; you said that various antelopes which were infected with trypanosomes survived and some are surviving now. I should like to ask about the sheep, goats and cattle in Uganda that have been infected in a similar way, whether they have invariably died as they did in Nyasaland, or whether they have survived?—The sheep and goats infected with *Trypanosoma gambiense*?

89. Yes.—I do not think many sheep or goats die of *T. gambiense*. It is very difficult to get the percentage of mortality among those animals but I do not think *gambiense* kills goats or sheep, whereas I should say *brucei* or *rhodesiense* does kill them fairly often. It is a slow chronic disease; I have not got my papers

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here, but I think you may take it that *gambiense* is a very mild disease in goats or sheep, and that few or none of them will die of it. You will find the trypanosome in their blood for a good long time if you look hard enough for it, but *rhodesiense* or *brucei* is a fairly fatal disease to goats and sheep.

90. Would your reply apply to the cattle as well?—Yes, I should say *gambiense* has a very slight effect upon cattle. Of course *brucei* or *rhodesiense* is not a very fatal disease to cattle either, a large number of them recover but it is a much more dangerous disease for cattle than *gambiense*. It is so difficult, cattle being expensive things, to experiment upon a sufficient number of the animals to get a good idea, but there is no doubt that a good big percentage of cattle will recover from *brucei* under laboratory conditions, whereas *gambiense* would have little or no effect upon them. If you get *gambiense* straight away from a man it is very difficult to infect the ordinary animals like dogs or rats; you require to inoculate them again and again before they take at all.

91. (Mr. Buxton.) I only want to ask you two questions. Assuming this experiment to be tried, I understand that one of the most important and instructive things that might be evolved from it would be to ascertain the ratio of infected and non-infected flies after a certain period; but would not the activity of these flies (and they seem to make light of travelling 30 miles an hour from what you say) destroy the value of that, because they would invade your 10-mile area, make nothing of it, and be continually invading it?—It is very difficult to know what these little creatures will do unless you try an experiment; it is a difficult thing for a man to know himself, but when man tries to get behind the brain or the mind of a fly it is still more difficult. I could not say what the flies will do at all.

92. The *morsitans* is a very active fly?—Very active; it can fly with great rapidity, but whether he would go five miles, or three miles, or four miles to get a feed and then come back again to his ordinary haunts is a matter for experiment. I expect if you fenced in this area and drove the big game out, there would be no fly left except round the edges.

93. There would be nothing to tempt them in, you think?—That is so; where the carcass is there will the flies be gathered together.

94. It struck me, as a non-informed layman, as a most curious anomaly that you have not a repetition of the epidemic in Nyasaland that you have had in Uganda. You have all the conditions, it would seem to me from what you have told us this morning, very interesting facts—a large native population, a vast number of wild animals infected with the trypanosome and the fly continually moving about. You have told us you have seen as many as 200 flies feeding on the back of a native. What strikes me as very difficult to account for is that you have not had already a tremendous epidemic of sleeping sickness. Can you differentiate between the two countries in your mind? Can you surmise why it is that there should be comparatively very few cases in Nyasaland and a tremendous loss of life in Uganda?—You are dealing with two different diseases altogether. In the one case you are killing off the natives in Uganda with a machine gun, and in the other case you are knocking them down one by one with a big elephant gun perhaps. They are two different diseases; man is very susceptible to the one and not very susceptible to the other. He is susceptible to the Congo sleeping sickness; he is not at all susceptible to the Nyasaland sleeping sickness. He has had the Nyasaland sleeping sickness for thousands of years; he has been born and bred among it. The blood of a man has no deleterious effect on *Trypanosoma gambiense*; the blood or serum of a man has a very deleterious effect on *Trypanosoma brucei* vel *rhodesiense*. If you put *Trypanosoma gambiense* under the skin of a man that man will certainly get the disease, but if you put *Trypanosoma brucei* vel *rhodesiense* under the skin the blood of the man will probably kill off the trypanosome before it is able to multiply. That is the reason why cases of the disease in the one country are few and far between,

and in the other country why it has knocked the men down by hundreds of thousands.

95. I follow that, but may we take it that to a large extent, or in the case of a great many individuals, they are practically immune from the disease in Nyasaland? If we have 200 flies, a proportion of which are infected, biting a man simultaneously, what is the reason he does not take the disease for a certainty?—I have been trying to explain that the blood of man has a very deleterious or killing off effect on the *Trypanosoma brucei* or *rhodesiense*. If you take a rat full of *Trypanosoma rhodesiense* or *brucei* and put a small quantity of human serum under the skin of that rat you will kill off all the *Trypanosoma brucei* in that rat or a great number in a very short time, whereas if you take another rat of the same description and with the same number of trypanosomes, but the trypanosomes being *Trypanosoma gambiense*, the injection of the human serum will have no effect upon them.

96. Is not that another way of saying that the human animal, or the native human animal, has become for one reason or another more or less immune in the course of centuries?—We people who work at these things by experiment seldom or never try to answer these difficult questions. You are talking about a person having become immune. I would say that man had never been anything else. I should say that very probably at first (and it is a question of evolution) *Trypanosoma gambiense* and *brucei* were the same, but at a particular time and place there were a large number of natives about and the flies fed a great deal on the natives and *brucei* got into a man and lived there; on account of some peculiarity about this particular *brucei* he was able to live in this man and multiply. Then the flies took it from that man and it went on for many generations and after several thousands of years *gambiense* changed from *brucei* so much that it could live with ease and comfort in the blood of man without any danger of being killed off, whereas the original stock from which it came, *brucei*, still retained the original habit of being killed off by means of the blood serum of man; but these things are so much in the clouds and in the region of speculation that we very seldom spend any time over them.

97. (Sir William Leishman.) I will not trouble you very much, but I would like to ask one or two points about your work with flies in captivity. How long will a fly go without blood?—Four or five days. I think if you kept some flies they might live 12 or 14 days if you absolutely starved them to death after having given them a big feed the first day, but I do not think there would be many living on the fifteenth day. It would depend a great deal on the temperature and the humidity of the air how long they lived.

98. You have bred a lot of flies from pupae; have you ever succeeded in breeding the second generation in captivity?—Yes, but not to any great extent.

99. Have any of the second generation been the descendants of infected flies which you have proved to be infected yourself?—Yes.

100. Have you had any results from that—I am talking of *morsitans*?—I was thinking you were talking about *palpalis*. We have had thousands and thousands of *palpalis* pupae. In fact the natives brought up 7,000 pupae of *Glossina palpalis* in one day but you could offer a sovereign for every wild pupa of *morsitans* without getting one, so that we had far more laboratory-bred flies to work with in *palpalis* than in *morsitans*. It is a difficult matter to get the *morsitans* pupae and I was talking about *palpalis* when I said we had them.

101. With *palpalis* have you any observations on the second generation of flies coming from a fly you have known to be infected?—The only observation is that we never found these laboratory-bred flies infected; I think that has been proved as well as we can prove a thing, that the fly does not transmit the parasite to its offspring like a tick.

102. Have you confirmed the work on the influence of temperature on the infectivity of the fly? In certain seasons do you get their infectivity more readily than in others?—Yes, it came out in the ordinary course of work; I do not think we studied it for itself but it came out in the ordinary course of

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work. We keep the temperature and the humidity of the air all the year round and the number of days within which the flies become infective, the percentage which becomes infective, and so on.

103. About what difference in temperature do you get at the different seasons?—In Nyasaland?

104. Where you have been working in Nyasaland?—I suppose an average mean temperature of perhaps 15 degrees difference.

105. What situations have you found pupæ in?—As to *morsitans* we never find the pupæ at all; one or two have been found but practically they are never found.

106. In the case of *palpalis*?—They are constantly at the edge of a lake or river and they have a predilection for sandy shores, so that very often they are found in large numbers on some suitable beach of sand.

107. I remember that in your Uganda work; you found the same thing in Nyasaland?—We never found the pupæ of *morsitans* at all in Nyasaland; there are no *palpalis* in Nyasaland.

108. (Dr. Balfour.) I only wish to ask you two questions and the first is an entomological one. Have you ever seen or heard of a tendency on the part of tsetse-flies, either *morsitans* or *palpalis*, to congregate on any special form of vegetable growth, such as tree-fungi, euphorbia, or plants of that nature?—No, I never saw anything of the sort.

109. We know that certain insects, Hemiptera or plant bugs, are known at the present time to acquire from plants, especially euphorbia, protozoal parasites which are allied morphologically in some respects to the trypanosomes, and one of those has been recently shown by a French observer to be pathogenic for small animals. Some such fact may possibly answer the question which the chairman put as regards the original origin of blood trypanosomes, and I wish to ask if you thought—I do not say for a moment that at the present date any such thing may play a part in the infection; but is it possible that these leptomonas or herpetomonas may, owing to changes in environment, become pathogenic blood trypanosomes?—Yes, I think that is a thing which may have happened in the past.

110. It would take a very long time for such a change to occur?—Yes.

111. You do not think at the present time any pathogenic form of trypanosome would be likely to be found in such vegetable growths?—I do not think so.

112. The only other question is with regard to Part VII. as regards other methods of dealing with the flies and destroying them: do you think it would be possible to devise any form of effective trap for trapping tsetse-flies?—I do not think so.

113. If such a trap could be devised do you think it could be used with advantage to protect zones round villages and in *palpalis* country, at watering places and so on, as a subsidiary measure to what you have already proposed?—I have never been able to think of anything which would be effective.

114. I take it any such trap would be better if it had some form of movement about it?—Yes, and life; we tried blood in a gourd but it was a very great failure.

115. (Professor Newstead.) I think I will not venture to ask Sir David Bruce any question with regard to *Glossina morsitans* in Nyasaland, seeing that he and I have traversed the same country, but I should like to ask him a question with regard to the animal and fly prophylaxis. Do I gather you advocate three methods or three systems, first the clearing of big game from a given area; secondly, to clear that particular area of all shade-giving vegetation; and thirdly, to fence that area with a fencing of some kind. Do I understand all these three experiments are to be conducted in one and the same area?—The first thing is to put up the fence, and the next to clear out the game from the area and see what the effect of that is. If the fly still remain in numbers and infective, then try the third thing, that is, clear the scrub.

116. That is to say, it is just possible that after having driven out the game from that particular area tsetse-flies might still remain, and that then it might

be found necessary to clear away the shade-giving vegetation?—Yes.

117. (Sir Stewart Stockman.) I think you probably gave the answer to a question I wanted to ask you, namely, to account for the absence of sleeping sickness in Zululand. May I take it the answer you gave just now was the explanation or a possible explanation?—There is one answer to that, and that is that in the fly area in Zululand, there are no natives, and there being no natives there can be no case of sleeping sickness among them. The reason why there are no natives is that the Zulu lives on his cattle; his cattle are his money and his life, he buys his wives with cattle, and everything is done with cattle; he will not live in a country where he cannot keep cattle, and therefore in the fly country there are no natives. The natives in this particular region here in Nyasaland are quite of a different class; they do not deal in cattle in that way so that this little area here has a population of some 20,000 inside it, which gives a better opportunity for the *Trypanosoma brucei* to get a chance, as it were at some particular man.

118. (Chairman.) When you say *brucei* you mean *brucei vel rhodesiense*—you do not distinguish between them?—No.

119. (Sir Stewart Stockman.) Is it not rather curious there should be no case? I understand there has been no case, but I am not quite certain?—I think it is very curious but I think it is quite possible that there may have been cases in these areas of this disease in man without its being noticed. I think it is only within a very few years that blood examinations have been made in these cases. I wrote in my summary of evidence, which Sir William Leishman will bear me out in, that one of the most common diseases in the world was not recognised for hundreds of years; it was only recognised 13 years ago. Before that all these cases were returned as malaria. It has nothing to do with malaria. If the medical men had made proper blood examinations they could not have said it was malaria. So in the same way I think cases of *T. brucei* disease may have occurred, a few cases here and there, and that they may have been called malaria and the man died and there was an end of it.

120. (Chairman.) Malta fever is the same thing?—Yes; when I went to Malta in 1884 there was no such thing as Malta fever. It was all returned as remittent fever, or if it was a very serious case it was returned as enteric. Such is the habit in man of deceiving himself that I have seen a medical officer who had five years' medical education looking at a piece of intestine, and saying, "There is the enteric ulcer," and I would look at it very closely and say, "I can see no ulcer there"; he says, "Do you not see that ulcer there?" and I say, "No, I cannot see it," and he goes and writes in his report that the intestine is markedly ulcerated. How these things happen I do not know, but that is the fact.

121. (Sir Stewart Stockman.) With regard to this suggested experiment it is the fact, is it not, that in a partial way we have already had something, not exactly an experiment, but something done following on the rinderpest in Africa? Is it within your knowledge that after the rinderpest had swept away the game, certain parts of South Africa where stock farming was impossible before became possible for stock farming?—Yes, I think so; again, that is one of those things you do not do by experiment, you have often to trust to hearsay evidence. When I was in Zululand in 1894, 1895, and 1896 (I left in 1897) the fly country was full of fly, and the rinderpest came in the next year and killed off most of the big game. When I came back from the war in 1902 there was some question (Mr. Austen here remembers it) as to the particular species of tsetse-fly which inhabited this particular part of Zululand, and I was anxious to get some specimens so that Mr. Austen might see what they were. I wrote almost every year to the magistrate who lived on the top of the Ubombo hill overlooking the fly country asking him to send me specimens of the fly and he replied each year, "There have been no flies here since the rinderpest passed through the country." About 1908 I wrote again and got a box of flies which I gave to Mr. Austen. Then the magistrate said,

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"The fly has come back; the big game are getting more numerous." But that to my mind is not very good evidence, as it depends on the magistrate, he might be a man who told fairy tales, or he might be a man too idle to send a native down to the fly country to see if there were flies there in the particular year, but I give it for what it is worth. With regard to Nyasaland in the same way, in this country here there was no fly up to a few years ago. When the rinderpest passed through in or about the year 1895 it killed off most of the big game, and at that time (this is the report of the white people, missionaries, and others who knew the country) the fly disappeared out of the whole of this fly country, but remained at a spot near the south of the lake, and it was known they were there. I have a map of Nyasaland here which may be useful; this is only a map of a small part. Here is Domira Bay; it was down south of the lake that the fly remained. There were thousands of cattle all over this country a few years ago, that is to say, eight or ten years ago. There was a man here at Lingadzi who had large numbers of cattle, and he had them round Domira Bay and all over the country. There were no deaths among them at all until suddenly they began to die, and he was forced to take them out of the country altogether in order to save further mortality. This is said by certain people to have been due to the fact of rinderpest passing through this country about 1895, it being 10 years before the fly again spread over this area. There can be no doubt that this is typical fly country, and has been from time immemorial.

122. I think in Africa in some of those areas here it is almost the unanimous belief of farmers that you cannot stock-farm in a place where big game is kept; do you agree with that?—In a country where there is no fly the wild animals would naturally damage the stock farmer by eating up the food his own stock ought to get, or by destroying his fences.

123. I refer to the fly country particularly?—In the fly country the farmer could not keep stock at all.

124. (Mr. Austen.) At the risk of increasing Sir David Bruce's disdain for entomologists—I am very fond of them. The entomologists say, and go on saying, "We want to find out more about the bionomics of the tsetse-fly"; you can say that for 20 years and in the meantime nothing is being done, and I have only a few more years to live myself.

125. I can assure Sir David that the entomologists are as anxious as he is to find out the real remedy. I would like to ask two entomological questions, and also two others. When this new trypanosome was discovered in North-Eastern Rhodesia and in Nyasaland, certain structural peculiarities were noticed in it which led people in the first place to think they were dealing with a new parasite. Subsequently, those peculiarities were re-considered; they were found elsewhere, and Sir David Bruce came to the conclusion that this trypanosome was nothing but *Trypanosoma brucei* which he himself had discovered in Zululand in 1894-7, and that it was disseminated by the same fly. I see he says here on page 2 in the first section of his evidence that, "It was proved at that time" (that is in the nineties) "and for the first time that these parasites were carried from sick to healthy animals by the local tsetse-fly *Glossina morsitans*." I should like to ask whether the figures you yourself published at the time did not show, and whether the specimens you sent from Zululand also did not show that the fly you had to deal with then was not *Glossina morsitans* at all but a totally different species known as *Glossina pallidipes*?—There is no doubt that the evidence is that the tsetse fly we were dealing with in Zululand was *Glossina pallidipes*. I look on *Glossina pallidipes* and *Glossina morsitans* as being very closely related. They have the same habits, they inhabit the same country and live among the wild game in the same way, so that I look on the two as being very closely related, and you can call them one for practical purposes.

126. But the structural differences between them are at least as great and, I think, I might call them considerably greater than the structural differences between two so-called species of trypanosomes. I think you admit that?—That is your trade, it is not mine. I

know they are very nearly the same size and live in the same kind of country and have the same habits, and I am perfectly certain the trypanosomes develop in the interior of *pallidipes* in the same way as in the interior of *morsitans* and that is sufficient for me.

127. That is not doubted. I think I may take it that this is a misprint here on the second page of your statement?—Yes, that word "*morsitans*" is meant to include *pallidipes*. We find that even in *brevipalpis*, which is a very different kind of fly from *morsitans* or *pallidipes*, the same development goes on of the *Trypanosoma brucei vel rhodesiense* as in *morsitans*.

128. Recent results have shown that whereas we used to think that all tsetse-flies had the same habits we know now, in certain cases at any rate, that the habits of different species of tsetse-flies are very markedly different, and therefore I think when we mean one particular species we must now be careful to call it by its right name; it may have a practical bearing?—Yes, but in this particular case I think these two species—*morsitans* and *pallidipes*—are very much the same in habits and habitat. I was talking about *palpalis*, which have very different habits, and I would differentiate strictly between *palpalis* and *morsitans*.

129. They belong to totally different groups, of course. My second question has reference to the statement in Part V. of your evidence as to the great powers of flight of the fly. Sir David Bruce says that *morsitans* can possibly fly a mile a minute, but I humbly beg to doubt that; certainly no one will deny that, if we have a deadly fly such as this tearing about Africa at the speed of an express train, the question is far more serious even than we now think it is, and when this particular fly is said to overtake a motor-cycle going at 30 miles an hour, I should like to ask how much start the motor-cycle had?—It is quite possible that the fly can fly at the rate of a mile a minute, but it does not do it, because it does not want to do it; it does not come into its particular habit of life. I can run at five miles an hour, but I never do it if I can possibly help it.

130. Erroneous impressions get abroad on very slight foundations, and I think we ought to clear up the point as regards how much start the motor-cyclist had. I doubt whether there is a fly in existence which can fly at 60 miles an hour for any distance at all. It is admitted that the faster an ordinary cyclist goes the more flies will follow him, not only *Glossina morsitans* but *palpalis* too?—Yes.

131. How did you time them?—We had not a starter.

132. Do you mean to say that a cyclist going at 30 miles an hour actually passed a point and got a good way beyond it, and that the fly started from that point and caught him up?—What I mean is that when you are going along at a great rate the flies are passing over your head and seem to be in front of you at one minute and behind at another, and they seem to alight on you as they like, you knock them off, and they come buzzing on to you again. They seem to me to have command of the situation, you can call it 60 miles an hour or 100 miles an hour, but all I say is that they are very rapid on the wing.

133. I might suggest that a cyclist going at a rapid speed through the air forms a partial vacuum in the air, and diminishes the air resistance for any insects which may be about him or following him, and therefore in your view he renders it much easier for the fly to travel over the ground at a rapid pace than if there were no cyclist at all?—We are pacemakers, in fact.

134. Yes, if you like, and you facilitate the passage of the fly, you would admit that?—I admit that.

135. I notice in Part II. of your evidence, in referring to Dr. Taute's experiments upon himself, which, of course, all the Committee are familiar with, you say "It is a matter for keen regret that these experiments were not successful." I am not sure that the Committee will agree with that remark, because if they had been successful we should have been mourning the loss of a very able investigator. You say that a negative experiment proves nothing. With regard to that you will admit that Dr. Taute is at issue with you. You have referred

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to the question of infective granules. Have not Dr. Taute's results possibly some bearing on that point? Dr. Taute claims that his results show that we are not entitled to assume that a trypanosome in an animal which looks like the Nyasaland trypanosome of man is in fact that trypanosome unless it does actually produce the Nyasaland or North-East Rhodesian sleeping sickness in man. May not the real cause of this disease in man be not *Trypanosoma brucei* or *rhodesiense*, or whatever you like to call it, but the infective granules of a hitherto unrecognised trypanosome, possibly indistinguishable in appearance from the *Trypanosoma brucei* although of infinitely less common occurrence?—I do not know exactly how to answer this, but I imagine there might be two species of trypanosomes exactly like each other, and that the one was pathogenic to man and the other non-pathogenic to man, but for practical purposes we cannot distinguish between them. They are exactly the same under the microscope and have the same action on different animals. The only animal we cannot experiment on is man; if we could, in a couple of months we could answer this question. These trypanosomes vary extraordinarily. If you take another protozoal disease called horse-sickness in South Africa, it is said that you could make a horse immune for Valley A; you take this horse and put it into Valley B and he takes horse-sickness and very probably dies. You can immunise a horse against A and B and C Valleys in the Transvaal, and when you put him in D Valley he again takes horse-sickness and probably dies. That means there are a great number of different strains, and one strain does not protect from another; so, in the same way there may be strains of *brucei* which are more virulent towards man than the ordinary strains. The reason of that may be that it has gone through a man once or twice, and the fly has got it from the man. Those questions are all very difficult. The strains of these protozoal diseases are very different. Professor Laveran, in Paris, tries to separate one species of trypanosome from another by giving an animal, say, trypanosome A. He has another B which looks very like trypanosome A which comes from another part of Africa, and when the animal has recovered from A he gives it B; he takes the temperature very carefully, and if he finds there is a little change in temperature he says there is a reaction, and therefore A trypanosome is different from B. I think you will give a new name to the same trypanosome if by chance it should come from a different part of the country, if you go in for this way of diagnosing them. I do not know whether I have answered your question; if not, I will try again.

136. I am afraid we are rather substituting the word "strain" for "species"—?—I do not think you can call them species; they are surely too close to one another to be called species, I think they are the same thing, but one may have passed through one particular animal for some generations, and so become changed to some slight extent, and I think the word "strain" is better than "species." We would be simply buried under species if you allowed the word "species" to be read instead of "strain."

137. If there are two strains, one of them the common cause of disease among domestic animals, while another is uncommon but causes sleeping sickness in man, that may account for the very slight incidence of human trypanosomiasis in Nyasaland and North-Eastern Rhodesia?—That is so. If you can show me how to elucidate this and prove it, I will be very much obliged because we have been out for two years trying to solve it and have not done it yet.

138. Only one other question; it is claimed that, game infects man with the trypanosomes of sleeping sickness. Why should it not be the other way about, and that man infects the game? Have not recent results in Uganda tended to show that infection of the game with human trypanosomes tends to die out entirely, even in so short a time as four or five years?—How many questions are there there?

139. Perhaps two, but it is not necessary to take them singly. Why should not the fact be that man infects the game instead of game infecting man? Have

not recent results in Uganda served to show that the infection of game by *Trypanosoma gambiense* tends to die out in time, even in so short a time as four or five years?—I suppose there is a little evidence that the fly on the shores of Victoria Nyanza are becoming less infective than they were three or four years ago, but that is a kind of evidence that is very difficult to get at, and one would require to wait some years before one could answer the question.

140. Whereas four years ago you could get an infected tsetse-fly if you fed, say, 500 flies on an infected antelope, now it would take 1,200 or 1,500 tsetse-flies?—But still it is a very delicate kind of experiment that, with a big possible error.

141. It points to something if you get that result surely; does it not tend to show that the infectivity of the game is decreasing or remaining stationary in Uganda?—I would leave it for you gentlemen to judge as to whether the few experiments made on this point are sufficient to allow us to come to a decision. If a few years hence the tsetse-fly on Lake Victoria are absolutely non-infective, and this is proved for some years, and men can go back to the shores of the lake and live there with impunity, I should say that what Mr. Austen is saying is true. For my own part I think the wild game on the shores of Victoria Nyanza will keep the *Trypanosoma gambiense* going for a very long time.

142. But if the game did become absolutely non-infective after the lapse of a certain time after the removal of the human population, surely it would show that the game had become infected from men and not men from the game?—No one will go against you for a moment or deny that it was man that brought the *Trypanosoma gambiense* into Uganda. Before 1905 or 1906 or 1907 or 1908 there was no *Trypanosoma gambiense* in Uganda as far as I know; it was brought in by man from the Congo, and from the natives it went to the wild animals, but what does it matter?

143. It matters in this way that if you let the wild animals alone, and remove the source of infection, which in this case is man, the wild animals themselves are harmless as regards sleeping sickness in Uganda. I think it shows that, does it not? The game does not infect man, but man infects the game; that is a rather important difference?—I think it would be far better if you stuck to *morsitans* in Nyasaland and did not go up to Uganda, because you are dealing with two different species of disease, two different species of tsetse-fly, and two different species of trypanosome. I think we had better stick to the *morsitans*. I think myself the *gambiense* has probably become acclimatised to man and is a human trypanosome.

144-5. (Dr. Martin.) Supposing you admit that the human beings infect the game, does it in any way influence your opinion that the game may be an important reservoir of the disease in the absence of man?—No. I think the wild game will prove to be a very important factor in regard to sleeping sickness in Uganda. It is a question of speculation, but I imagine 10 or 20 years hence you will find wild animals infected with *T. gambiense* on the shores of Lake Victoria. A few years ago I prophesied that within 48 hours of the men being removed from the shores of Victoria Nyanza the whole disease would disappear. But I reckoned without my host, I reckoned without the wild game, and I also reckoned without the development of the trypanosome in the fly, but I really think in Uganda the presence of infected wild animals will be found to be a very practical and very important factor in the question.

146. Are there considerable areas which are inhabited by *Glossina palpalis* in Nyasaland?—There are no *palpalis* in Nyasaland at all.

147. Is that the answer to the question asked by Mr. Buxton: "Why there had been no epidemics of sleeping sickness there"?—Of "Congo sleeping sickness"—certainly.

148. I do not think you have quite satisfied Mr. Buxton?—The absence of *palpalis* I look on as the reason why there has been no extension of "Congo sleeping sickness" beyond the habitat or the boundaries

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of *Glossina palpalis*. We have maps giving these distributions, if any person would like to see them.

149. (Mr. Rothschild.) What I was trying to ask you before was this—seeing that the fly, after feeding on the infected animal, only becomes infective after 17 to 32 days, was the examination of laboratory-bred flies done immediately they were hatched or soon after they were hatched, or had a similar space of time, 17 to 32 days, been allowed to elapse before they were examined in order to see whether they might not inherit the sleeping sickness and yet only become infective after the same time as they do when biting an infected animal. I wanted to know whether that experiment had been tried?—I do not think it has ever been tried as an experiment for that particular purpose. I think laboratory-bred flies have probably been fed on animals for three or four weeks, because you have a great number of laboratory-bred flies; you have a stock of them, and they have to be fed every day or two to keep them alive, and I have no doubt in these cases they were often fed for more than three or four weeks, because in Uganda we had any number of these laboratory-bred flies. As I have said, the boys brought up 7,000 in one day of the pupæ, so that we could have a thousand laboratory-bred flies a day if we liked, and we had large numbers, and all the laboratory-bred flies kept in stock were fed every two days to keep them alive. I am sure if there had been any hereditary transmission at the end of 32 days that would have come out, but we never made an experiment for the particular purpose of proving that question which you have asked or disproving it.

150. The only other question I should like to ask you is this: Dr. Yorke has stated in one of his articles that he found one of his laboratory boys showing trypanosomes of "Nyasaland sleeping sickness" in his blood, and he kept him under observation for a year, and he did not exhibit any signs of illness. As it had been ascertained that most, if not all, the cases of human trypanosomiasis of the Nyasaland type died within six months, has any regular examination of a large number of apparently healthy natives been made in order to see if that was a single case of immunity or not?—I am afraid we have not made any experiments on that line; we have not taken a thousand men in the sleeping sickness area and examined their blood for trypanosomes. We have examined a fairly large number of natives, but we have not done it on the large scale which would be required. Kleine, who lives over 600 miles north of us on the east shore of Lake Tanganyika, took this point up, and he thought it might be that the natives living in the *morsitans* area became to a certain extent immune to the disease by taking it when they were very young, just as young calves have red-water and other diseases and recover from the disease very rapidly and remain immune all their lives. Kleine examined 500 native children in one of these areas and found nothing. With regard to Dr. Yorke's statement, I do not remember the exact circumstances of this statement; did he only find the trypanosomes once or more than once?

151. He found them more than once throughout the whole year of observation; they were living in the blood. He merely mentioned that he kept the case under observation for a year, and that no trypanosomiasis appeared.—Did he see the trypanosomes once?

152. He does not state that exactly.—I think he ought to say exactly, because that native may have been desirous of obtaining the sympathy of his master, Dr. Yorke, by giving him a specimen of blood which he said was taken from his own arm, whereas it may have been taken from a monkey.

153. I do not think that could be so, because he distinctly states that he examined the blood of this native, and he would not make a statement like that?—This is a very important point, and he does not seem to have treated it with the seriousness which it deserves. Does he give dates, photographs, and charts? This is a thing which deserves any amount of labour. Has he given it? Is it not rather a casual sort of statement that trypanosomes had been in the blood of the boy

once, and he examined him to see afterwards and never found them again?

154. No; he simply states he had found this one only. He was discussing the point whether the man was a reservoir, and he had only come across that one case, and I wanted to know if other people had made similar experiments?—No, we have never found the *Trypanosoma brucei vel rhodesiense* in the blood of any native who was not seriously ill of the disease and who shortly afterwards died; but I certainly, if I found a case of that sort, would describe it at great length.

155. (Dr. Chalmers Mitchell.) A certain heaviness of bombardment is useful in giving infection, is it not? Theoretically, one or two trypanosomes would give the disease, but, in practice, the more the human being has been attacked the more certain the result?—As a matter of fact, papers have been written saying that if you put in too much blood you will not get the disease.

156. You do not believe it matters whether you are bitten by a number of flies or not?—No, I think one fly is quite enough.

157. You think the trypanosomes multiply so rapidly that one is just as good as a whole lot, roughly?—Roughly speaking, yes.

158. They are not like bacteria in that way?—No. If you see the salivary duct of the fly, you see the enormous number of trypanosomes in the fluid. As soon as the fly bites its prey it injects a drop of that fluid, so that every time it bites it must inject probably several thousands of those trypanosomes.

159. So that, really if you are bitten once by an infective fly it is probably good enough?—That is what we find; it takes every time.

160. You take the view, I think, do you not, that it is pretty certain you cannot distinguish the trypanosomes of the *brucei* group very well morphologically from one another; it is very difficult to distinguish *gambiense* and *brucei* morphologically?—Yes, especially if you are taking the blood straight away from man. If you made some inoculations into the smaller animals, I think in most cases you would be able if you examined a large number of the trypanosomes to make a very shrewd guess as to this being *rhodesiense* or *brucei* and that *gambiense*; but if you took specimens from the blood of a man and only had these trypanosomes to examine, you could not tell the difference between *gambiense* and *brucei*, and you never could tell the difference between *brucei* and *rhodesiense*.

161. That I understand, but the fact that you cannot distinguish very certainly *gambiense* from *brucei* would also apply. Supposing *rhodesiense* and *brucei* are distinct, you would not expect to find that morphologically, would you?—You would not be able to distinguish between them morphologically.

162. So that the fact that they are practically identical in appearance does not carry you very far in distinguishing them?—I think the morphology of trypanosomes is one of the best characters we have for distinguishing between the different species. If you look at the pictures there you can tell the species of trypanosome by morphology alone, with the exception of the *brucei* group.

163. Supposing these three or these two are all very close morphologically you have to rely on some of the other tests?—Yes, the human serum itself is a very good test between *gambiense* and *brucei*.

164. So that, if you give *gambiense* to an animal—a monkey, for instance—and get it to take the disease which is produced, that is very closely parallel to the human *gambiense*—sleeping sickness?—Yes.

165. You get the same fall in the temperature before death and all that sort of thing?—Yes, and the same change in the structure of the brain in the monkey as in man.

166. It is quite clear that the clinical picture of *gambiense* disease in man and in animals is similar; you do not expect it to be identical, because no two animals are alike; but it is very close, is it not?—Yes, especially in the monkey.

167. Then when you come to nagana disease, what you showed to be tsetse-fly disease, is that quite like

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Nyasa sleeping sickness—is that as close to it?—Among animals it is absolutely the same.

168. The parallel between men and animals in the case of *gambianse* disease is very close indeed?—Yes.

169. Is the parallel between men and animals in the case of the *brucei* or *rhodesianse* disease just as close?—Yes, it is an acute disease; it gives rise to oedemas just as you have in horses with nagana. It is a rapidly fatal disease just as it is among animals, that is to say, *brucei* will kill a monkey in a fortnight or three weeks, or a month, whereas *gambianse* will kill a monkey in a year. *Gambianse* will kill a rat in 70 or 80 days; *brucei* will kill a rat when he has got accustomed to it in two days. It is a very much more acute disease.

170. The one is always fatal and the other not necessarily fatal?—Yes, but *gambianse* is also a pretty fatal disease.

171. The parallel is pretty well the same in both cases?—Yes.

172. You say you cannot at present make the crucial experiment on human beings?—No.

173. Has it been attempted to test in the other way? Have you infected animals with the Nyasa human sleeping sickness disease and produced exactly the old tsetse-fly disease?—Yes; the moment I became suspicious that this was really nagana I got strains from Zululand from the very spot where we studied nagana in the old days and worked with them.

174. Strains from what?—Strains from Zululand nagana.

175. From animals?—Yes, living animals were sent up from Pretoria which had been inoculated. They sent a couple of mules into Zululand and let them be fed on by the flies in that particular area, and then those mules were brought back to Pretoria. Rabbits were then inoculated from the mules and sent to me, so that I had the opportunity of studying this Zululand form of nagana alongside the local Nyasaland form, *rhodesianse*; we studied them from every point of view we could, and came to the conclusion that they were exactly the same.

176. Have you ever made the experiment of taking a man with Nyasaland sleeping sickness and infecting a test animal from him and at the same time infecting another test animal from this strain you brought from Zululand and comparing the result?—Yes, if you look at the papers you will find we describe fully different strains of human trypanosome and also what we consider to be the same trypanosome taken from the wild game and the wild tsetse-fly. The paper on the Zululand strain has not been written yet.

177. And they produced exactly the same result?—Exactly.

178. That is really what I wanted to find out.—Absolutely parallel in their action on animals. The only thing wanting is the experiment on men.

After a short adjournment.

190. (Sir Mackenzie Chalmers.) There are a few questions that I want to ask you Sir David, and there is a question that Mr. Buxton suggested that I should ask you. You told us that the *palpalis* fed very freely on the chicken or bird?—Yes.

191. If the *palpalis* is infected, what is the result on a bird?—We were never able to infect a bird with *gambianse*, and we have never found any of the numerous birds that we examined, shot on the lake shore, living as they did all their lives among innumerable *palpalis* infected with *gambianse* or with any mammalian trypanosome.

192. None of the pathogenic trypanosomes?—No.

193. None of the disease-bearing trypanosomes either of men or animals?—No.

194. You said, I think, that the same applied to the small animals?—We examined a large number in Nyasaland, and we have never, up to the present, found any of the pathogenic trypanosomes among them. They have their own trypanosomes, but we have never found *brucei* vel *rhodesianse* or *simiae*, or any of the others that I show plates of, in the smaller animals,

179. But you have done it the other way and it has worked all right?—Yes.

180. There is only one other question about infecting the animals with those trypanosomes. You get flies and you examine their blood—at least that has been done in a good many cases, and you can tell if they have been feeding on a mammal or a reptile or a bird or what. You cannot effectually, distinguish, can you, between the different mammals?—No.

181. You have simply to put it down mammal, big or small, or reptile or bird?—Yes, that is so, because the blood changes rapidly in the stomach of the fly.

182. Then you say that they have certainly a preference for warm blooded animals and especially mammals: what is that opinion based on?—If you have a large cage of flies, say a couple of thousand in a big cage the size of this table, and you have a warm blooded animal like a chicken and lift up its wing and put it against the wire of that cage, in a moment or a few moments, the whole of the exposed skin of that chicken will be covered with a dense black mass of flies; you would not be able to put a pin's point between the flies. If you take away the chicken and put a monkey on with the bare skin against the cage, the flies will go to it readily enough but not in such an extraordinary crowd and they will not crowd each other so much.

183. They prefer the bird to the mammal?—Yes, I think so. That is *palpalis*. We have never tried it with *morsitans*. Then if you take a lizard and tie the mouth of the lizard so that he cannot get at the fly first, and put in flies and allow them to be starved for some days, you will find one now and then sitting on the head of the lizard, digging his proboscis into the skin and, no doubt, feeding on it. And the same with crocodiles.

184. But they are not so keen on them?—Not at all so keen; they will not feed on cold blooded animals if they can possibly help it.

185. Do you think it is a question of temperature? Here you have your bird which is of a higher temperature than a mammal and probably they are both higher than a reptile?—Much, I should say.

186. Is the laboratory test conclusive? What is the temperature in the laboratory?—At the top of the hill about 65° to 75°.

187. So that you would have the preference going parallel with the temperatures, choosing first the hottest?—Yes.

188. Then out in the open where the reptiles are lying sunning themselves, you would eliminate that temperature difference, would you not?—Yes.

189. So that the laboratory experiment in that case you would not say was complete, and it would not be complete?—No.

such as cats, rats, mice, and such like, which are common there.

195. But the same animals are susceptible if you inoculate them in the laboratory?—Yes.

196. Have you any explanation of the fact that they can be inoculated in the laboratory but are safe in nature?—Their habits do not lead them to form any food supply for the fly. Most of them are doubtless nocturnal, while the fly is diurnal.

197. Does that apply to the *palpalis* and *morsitans*?—Yes. The flies are not out in the morning at day-break. When the sun is up and it is warm, the flies come out and feed.

198. Your experience up to the present gives you no reason to suppose that birds or the small mammalia are potential reservoirs?—With regard to the birds, I am certain, but with regard to the smaller animals, if you examined a very large number you might find an infected one some day, but I have never found one up to the present.

199. If the supply of large animals went off the *morsitans* might be driven to the smaller mammals might they not, or are they mainly nocturnal?—I do

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not think they would. The habits of the small mammals would not lend themselves, I think, to forming a reasonable food supply to the fly. For example, we have examined thousands of monkeys and have never found a monkey infected. He lives during the day in thick foliage, and is a quick animal, and if the fly comes near him he would probably get it before the fly got him. In Nyasaland we have examined thousands of monkeys and have never found one infected, although they live in the fly country, which would show that the habits of the animals of which we are speaking are against the fly feeding on them.

200. Do your answers apply to *morsitans* as well as *palpalis*?—We have never tried *morsitans* on chickens. We have never put a large number of *morsitans* into a cage and fed them on different animals.

201. Is it worth trying, having regard to the different character of the two diseases?—I do not think so. The birds in the *palpalis* area are different in numbers and in habits. You never find *morsitans* alongside lakes or rivers; they are all in the wild desert country, where there are comparatively few birds. The birds have too many enemies there to allow them to increase in numbers to any great extent.

202. Are there places where the *palpalis* area touches the *morsitans* area?—Not in Nyasaland.

203. Is there any possibility of this being the case—that the trypanosome develops in the fly and that the different types of diseases produced may depend on the difference between the flies in which it is developed?—I do not think that the species of trypanosome would change by being put through another species of tsetse-fly.

204. Suppose that you infected the *morsitans* with *gambienne* for instance?—We can.

205. If you pass it on to a susceptible animal it is still *gambienne*?—Yes.

206. And it is not in any way modified by the intermediary host?—No.

207. If I may go back to your suggested experiment, you have suggested a site on the Domira road?—Yes.

208. Dr. Yorke suggested another site. Have you considered at all the relative merits of the two sites? He suggested a site in the Sebungwe district of Southern Rhodesia, and he said: "A place which conforms to the above requirements" (he mentions the requirements of the experiment) "is the Sebungwe fly area of Southern Rhodesia, where the cases of sleeping sickness (including one European) were discovered lately," in a population of about 3,000. Have you any opinion on the relative merits of the two sites? Would you like to see what he says?—I think that I know. I suppose that what he means is that you have there, or he fancies you have there, an isolated patch of fly, and that in the surrounding country there is no fly and that he would deal with the whole of the isolated patch of fly, whereas in our experiment we are only dealing with a small area fenced off from the surrounding fly country. There is a good deal to be said in favour of dealing with an isolated patch of fly.

209. His would be a larger and more expensive experiment, I suppose?—You could not very well fence in the Sebungwe district. It is a very large place.

210. It being an isolated area, you would have a natural fence. You would clear the game, and trust to that?—Yes, you would try to clear the game. If you could clear the whole of the patch of the bigger animals, then it would be a very good place in which to try the experiment.

211. That would get rid of the difficulty of the fence being broken?—Yes, of the fly going outside to feed, and so on.

212. In a 10-mile square area you have a zone of infection all round, and you have a fly which can fly for a considerable distance?—Yes; it can fly quickly.

213. Would a 10-mile square area be a fair test, having regard to what one knows?—One would like to see the whole of the sleeping sickness area taken as an experimental area. It is a question of expense.

214. Could you draw satisfactory conclusions from an area so small as 10 miles square—100 square miles?

Do I understand that you think that you could?—I think that possibly something might be learned.

215. A more satisfactory experiment would be an isolated area, such as that suggested by Dr. Yorke?—Yes. If you could get an isolated patch of fly country surrounded by flyless country, that would be better than an area in the middle of fly country.

216. Do you know the area which Dr. Yorke mentions?—No.

217. You do not know how far it is possible to clear the game there in a short time and eradicate the *morsitans*?—I do not know the conditions of that country at all. It is evidently a wild country, sparsely inhabited by natives. We do not at present know the numbers of game there or the numbers of fly there. All that has been done is that two medical men have gone there and written a report with regard to the number of cases of Nyasaland sleeping sickness that they found there.

218. So that we have no materials for judging the relative expense of the two experiments. Now, as to the expense of your experiment done in the way that you suggest, the initial cost would be somewhat under 1,500*l.*, I understand?—Yes, I think so. An estimate was given by Mr. Binnie.

219. £1,400 odd pounds?—Yes.

220. He did not give an estimate of the upkeep expense, I think. Have you considered at all, having once started the fence, what you must pay to keep it up? Do you know whether that would be expensive or not?—I do not think it would be expensive if the natives were allowed to pay their hut tax by giving work instead of paying the hut tax in money.

221. You mean paying the hut tax in kind?—Yes. On paper it would probably come to the same thing. I suppose that the Treasury of Nyasaland would wish to return as many hut taxes as possible, and they would return the hut taxes as having been paid in, and paid out again for work.

222. That is a matter of paper account?—Yes.

223. Practically, if people cannot pay their hut tax it is profitable to get their work?—Yes. They are put on to road making in order to do work in lieu of the hut tax. The hut tax is credited to the Government, and then the Government is debited with the cost of making the road. There is no exchange of money.

224. You suggest that the population which has to be supported in some way, or which has to be helped in some way, should be used for the experiment?—Yes.

225. Are you satisfied about the efficiency of the hedge? Would it require to be supplemented by barbed wire or anything of the kind?—It is one of the things which you require to try before you can say much about it. It is an experiment.

226. You would not yourself, would you, advise the trying of the 40,000*l.* experiment until you had tried the other?—I think it better to try a cheap experiment first to see what you can find out from it. The local people thought that the experiment was a feasible one, and that it would cost very little under the circumstances, except on paper, that something would be learned from it, and that it would give employment to the people. If the road from Domira Bay was enclosed and made free of fly, it would be a very useful road.

227. At present it is closed to cattle, and it is dangerous to man?—You may say that it is practically closed altogether. It was built at first with a great expectation of its being used, but on account of some change Broken Hill came in, and they get things round by Broken Hill now rather than by the Domira Bay Road. When the road was made some years ago at a great deal of expense, it was intended that the road should bear the whole or the greater part of the commerce from the coast into North-Eastern and North-Western Rhodesia, but now they find that Broken Hill is easier and cheaper, especially as *this* (pointing to the map) is covered with fly. In a few years the road will again assume its old importance when the railway comes to the south end of the lake.

228. As a carrier?—As a carrier. It is easy to carry things to Domira Bay by dhows and steamers

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from the south of the lake, and wagons would then take the merchandise into Fort Jameson and Rhodesia along the Domira Bay Road.

229. (Mr. Read.) There is an idea of opening the Domira Road at once, but I imagine that the transport will be by motor vans?—It would be useful, if the road could be made free from fly.

230. (Sir Mackenzie Chalmers.) It would be a great extension of the original experiment if you freed the whole road. You propose to start with 100 square miles, 10 each way?—Our idea was to fence five miles on each side of the road, right through the fly country.

231. I had not appreciated that.—That is in the summary of evidence. The 100 square miles was the official Nyasaland Government idea. The idea was to have a regular deer fence with steel standards and barbed wire. My first proposal was to put down a wooden fence along each side of the Domira Road and see what happened after driving off the game, and if the fly remained in numbers and remained infective the natives could very quickly clear the whole of that area of scrub. There is a large population there and the trees are small and easily dealt with. The natives usually carry an axe with them and they are accustomed to the kind of work.

232. If you clear the scrub do you think that the fly will disappear?—I am sure that it will.

233. It depends on the scrub for its dwelling?—It depends on the shade of the scrub; you never find them in open spaces (*the witness went to the map*). This is the end of the lower part. The yellow line to the left is the beginning of the higher plateau. If the road was cleared the wagons could go with cattle up and down quite readily.

234. Does the fly stop at about 3,000 feet? How high up does it go?—It depends on the nature of the country. On the plateau in North-Eastern Rhodesia the fly goes up to 4,500 feet. Take Kasu, where we are about 3,500 feet up; the natives say that a generation or two ago the place was covered with forest with any number of buffalo there and any number of fly. That was in the old days when the slave raiding was going on and the people were few and far between, but when they became more numerous they cut down all the thorn for firewood and so on, and now it is clear and we never see a single fly anywhere about there except one that might be brought up on the back of a motor-bicyclist from the low country.

235. If your suggested experiment is successful we are in for a very big business, because it will have to be extended throughout the whole of the fly country?—After having got your information, you can let things remain as they are until it is necessary to go further. Before 50 years this country will be covered with plantations of rubber, sugar, and other crops, and then there will be no fly and no big game.

236. There is nothing like an epidemic in Nyasaland, is there, but there are only sporadic cases?—The red spots which you see on the map show all the cases in the last five years, and they are very few.

237. Having regard to the small number of cases, is it possible that they have all been cases of man-to-man infection, and not of infection brought from animals?—Well, I do not know; I am not sufficiently in the confidence of the fly.

238. It is possible, but you cannot tell whether it is likely?—It is possible, but I think not very likely. Mr. Roux, the pastor of the Dutch Reformed Church, went into an absolutely uninhabited country down in the south-east corner of the area for a few days, and no doubt he got it there, and it was the same with Captain Hardy, one of our officers. He was in the country trying to find *palpalis* and he got the disease there. I think they got it in the wild country, rather than in the populous parts.

239. Looking at the dots there seems to be a focus of infection. If you got an infected spot it would be very easy to carry infection from one man to another. It looks as if in the corner there might be man-to-man infection?—With the intermediary of the fly.

240. Of course with the fly to carry it from man to man?—The fly is not very fond of the villages.

People coming out of the jungle carry a fly or two in but the flies are not common in villages. The flies are wild animals and they live in the wilds among the wild things. They are not house-flies.

241. The further you get from the villages the more you find?—Yes; our boys never go to the villages to find them; they always go to places like that (*pointing to the map*). Here is the road and the boys go into this wild part here. The boys go three or four miles and catch them here. Naturally there are no wild animals round the villages; they are all driven away and naturally the wild fly is not to be found there except by accident.

242. His food is further off?—His food is further off.

243. Does he prefer animals to men? You have not tested that perhaps. I understand he prefers a chicken to an animal?—That is *palpalis*; stick to *morsitans*.

244. You have not tested with regard to that?—No, I do not think *morsitans* is very fond of man, because you can walk with natives through the jungle for hours and you will see the tsetse-fly sitting on their shoulders but not feeding. Now and then a native will snap round at a fly that has bitten him; the sharp prick makes him turn round, but that does not occur very often. At the same time, when riding, for example, on a motor bicycle you might get 200 bites in the day or more, but then they come round you in such numbers.

245. The native has much more exposed surface than we have. Do the flies bite through clothes?—I suppose not?—Yes, we wear clothes that they cannot bite through. We take precautions.

246. Have none of the fly boys ever been infected?—Not in Uganda or here (*pointing to the map*), and we examined them every week. The boy is rather like the monkey. He is as quick as the monkey, and gets the fly first.

247. He does not give the fly a chance?—No; he is careful because he is frightened of the fly.

248. The boys know the danger?—They know the danger.

249. You were asked some questions about the refractoriness of man to the *rhodesiense* poison. Do you think that that refractoriness is acquired because it is a very old disease in the country, or would it apply to Europeans?—I think that it applies to Europeans as much as to the natives.

250. It is a natural refractoriness in man, not acquired?—I think it is natural.

251. You would require a large number of persons to be exposed to infection in order to form any trustworthy test, would you not?—Yes. If you could inoculate a hundred men with *gambiense* and a hundred men with *rhodesiense*, you would learn something, but it would require a good big experiment, and you could not do it.

252. It cannot be done, of course?—It cannot be done.

253. Now what is the effect of the trypanosome when it is inoculated into a susceptible animal? Is the effect mechanical or toxic or what?—When you inoculate a trypanosome, say, into a rat, in a few hours you find these things in the blood multiplied to a large extent. In, perhaps, 12 hours or 15 hours you would find them in some numbers in the blood. If you took nagana and put it into a rat, at the end of 24 hours the blood would be very full, and at the end of 48 hours there would probably be more trypanosomes than there would be blood corpuscles, and with a very virulent strain of nagana which had been going on for some years in the rat, the rat would be killed on the second or third day. I imagine, though it has never been proved, because it is in too small quantities to analyse chemically, that the trypanosomes give rise to a slight amount of toxin which gives rise to fever, and in many cases causes death. Then, in other cases of more chronic trypanosome infection, the trypanosomes are found in large numbers blocking up the capillaries of the brain. If you block up certain parts of the brain, you run a risk of killing the person by that mechanical means, but it has not yet been shown with regard to the mechanism how death comes about.

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254. You cannot show, say, from a post-mortem examination what pathological changes have caused death?—No, I think that you could tell from the pathological changes in the brain whether a man had died from *gambiense* or not.

255. Whether it was a case of *gambiense* or *rhodesiense*?—*Rhodesiense* being a much more acute disease, you would find fewer changes than you would in the case of *gambiense*, which is more chronic.

256. (Dr. Balfour.) Whether it is mechanical or not is quite uncertain?—I think that it is probably both. I think that the trypanosomes give rise to a certain amount of toxic poison, just as bacteria do.

257. (Sir Mackenzie Chalmers.) Have you found them in such quantities after death as to suggest a mechanical cause by the blocking up of the capillaries?—Yes, you get enormous numbers.

258. What do you say with regard to the peripheral blood?—I do not think that if you found them scarce in the peripheral blood you would find them in any other part in great numbers. Some species of trypanosomes multiply to an enormous extent in a short time and others do not. The less rapidly they multiply the more chronic the disease will be, as there will be fewer to act on the body. In a *gambiense* case, if you examine 50 blood preparations, you would not find a single trypanosome, but every time you examined a *rhodesiense* case you would find several in each specimen and half-a-dozen in every field you looked at if the man had been ill for, say, six weeks.

259. With the *gambiense* there is slow poisoning going on or something slow going on?—Yes; it is a slow toxic poisoning.

260. The question of treatment is outside your work?—Yes.

261. You said that treatment is useless in the case of *rhodesiense*?—You would probably prolong life by careful treatment and by careful nursing, but up to the present no case has been cured.

262. Has treatment been tried experimentally on animals as well as on men?—Yes, I think so.

263. There have been certain suggested remedies?—Nagana has been worked on for the last 18 years more than anything else. It has been the trypanosome that has been used for most of the experiments in regard to treatment by drugs for the last 18 years. Thousands of papers have been written on the subject, and if nagana is the same as *rhodesiense* then there have been plenty of experiments made in regard to treatment.

264. As regard Dr. Taute's experiment on himself, that is a single experiment, and it is very desirable that it should be repeated if possible. You cannot draw any inference from a single experiment?—No.

265. He may be resistant or the inoculation may have been badly performed?—I think that it was performed properly because he is a very good worker.

266. Then he is resistant?—Yes. Our theory is that man is more or less resistant to this particular trypanosome judging from the effect on it of man's blood serum. We think that man is generally resistant, more resistant than he is to *gambiense*.

267. When the resistance is once overcome then it is quicker?—Much quicker than *gambiense*.

268. If Dr. Taute's experiments should be confirmed, what will be the effect on all the theories?—I cannot say.

269. Would it prove that he had inoculated himself with one strain but not with the strain that is specially fatal to man?—It might be so.

270. In the case of *gambiense*, if a man recovers is he protected against subsequent attacks?—To a great extent he is, I should say. In the case of animals which have recovered from a particular strain of trypanosome, if you give them a second inoculation they take a second attack very often, but a milder attack, and on the third inoculation they take a third attack still milder. After that you may put in a large quantity of infective blood without making any difference at all. I should guess that a man would be to some extent resistant after having gone through an attack of *gambiense*.

271. If he does not die in the process?—Yes. It depends on the time. If he was treated by a rapid method of cure a few days after he got the *gambiense* and they were all killed out of him, then he would be as susceptible as ever, because the trypanosomes would not have had time to bring about the changes in his body which would render him immune. For instance, if you cure a man of syphilis in the first week he can take it again, but if you let it run its course in a man he is immune for the rest of his life.

272. It requires a certain time to bring about the necessary changes in the organism?—Yes.

273. Can one and the same fly inoculate a man with two or three different kinds of trypanosome, or does each fly carry its own trypanosome?—I think that a fly could carry two or three. There is no reason why these little trypanosomes should not live in amity.

274. They get on very well amongst themselves and with the fly?—Yes.

275. The fly does not suffer as far as you know?—No, he does not seem to suffer. We can keep them alive in cages for months after they have been infected.

276. The trypanosome goes on flourishing in the fly's body whether the feeding is on infected blood or not?—Yes.

277. (Mr. Millar.) Has there been any invasion of new areas by the fly within recent years? Has that fallen within your experience?—A few years ago this (pointing to the map) was all flyless, and it has been invaded within the last few years—the whole of this country that we have been talking about. I imagine that at one time before it became flyless it was full of fly because it is a typical fly country.

278. Is there anything to indicate to your mind what accounts for the invasion of a new area where there has been no fly immediately preceding?—In this particular case I would say that it is the multiplication of the game, as in Zululand.

279. Is there a direct connection in your mind between the invasion of an area by the fly through the multiplication of the game within that area?—Yes. I think that *morsitans* bears a very distinct relation to the game.

280. With regard to the gravity of the problem, do you regard sleeping sickness as becoming more serious at the present moment, or has it been showing signs of becoming more limited within recent years?—I think that the Congo sleeping sickness is becoming more serious every day, but that the Nyasaland sleeping sickness has never been serious and that it will not become serious, but it is a dangerous thing to prophesy.

281. Do you mean serious to man or serious to animals?—The Congo sleeping sickness is becoming more serious to man every day, but I do not think that the Nyasaland sleeping sickness has ever been very serious to man, and I do not think it will become so.

282. With regard to Nyasaland, is it mainly a question of the protection of domestic animals and the opening up of trade routes by enabling beasts of burden to pass through?—Yes.

283. But it is not so with regard to West Africa?—No. The two problems are quite different.

284. Have you had any occasion to consider the working of the game laws in Nyasaland and the surrounding districts where you have made investigations in order to find out whether game is multiplying unduly at the present moment under the present restrictions?—I think that game is multiplying unduly in this district, and in my summary I say that there is a large number of wild animals in this district, and that the senior magistrate says that there must be 30,000 eland in this one district. As these are large animals and they have a particular liking for young maize, I think 30,000 too many.

285. Apart altogether from the question of sleeping sickness?—Yes, apart from the question of sleeping sickness.

286. But our inquiry is directed to that entirely. Taking sleeping sickness itself, do you consider that there is a direct connection between the presence of the fly and the increase of big game in the district?—Yes. I think that the more game you have the more fly you have.

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287. Two methods have been suggested for dealing with the danger—one dealing with the host and the other with the carrier. Now with regard to the carrier, have you formed any conclusions of your own as to whether seeking to deal with the fly direct would be the most useful and most conclusive of the two?—I have never been able to think of or to imagine any method of fighting the fly direct. I do not know how you would go about it.

288. So far as the *palpalis* is concerned, I understand that the pupae can be found in considerable numbers?—Yes.

289. You said that the boys once brought 7,000 in in one afternoon. Does not that suggest that there are means of attacking or dealing with the pupae in that particular case?—I do not think so, because the flies are too numerous and are too much scattered.

290. Are they not limited more than in the case of the *morsitans*?—They are limited in distribution, but they still have plenty of room to breed in. If you look at the Victoria Nyanza and the islands you will see that the place is full of bays and indentations of the coast.

291. They are found generally in the vicinity of water?—Yes, on the edge of lakes and rivers. You will not find them 10 yards from the edge unless they have followed you. There is a very large area of absolute virgin forest. You cannot walk along the edge of the lake and so you cannot get at the flies. They show a preference for a sandy beach, but even a few feet of sand, and there are thousands of such places, would be quite sufficient for their wants. I think that to attack the *palpalis* through its pupae is an impossibility.

292. Is the life history sufficiently well understood and are habitat and the surroundings understood sufficiently to enable one to come to any conclusion?—I think so.

293. Do you think that there are sufficient data?—I think so.

294. It has been suggested that certain enemies might be brought into districts to deal with the fly direct. Have you formed any conclusion on that point?—The only conclusion I have formed is that I consider it very impracticable.

295. It has been suggested that wild birds of a species not indigenous might be introduced?—It would be a good thing to remove all the cats and snakes beforehand, because the introduced birds would not stand much chance with these animals there.

296. What about the native birds that are the natural foes of the fly. Are they not there in certain numbers?—I think that they do not come into practical consideration. You mean fly catchers.

297. Yes, and wild guinea fowl, for instance?—The tsetse-fly does not lend himself to being caught by fly catchers. He lives quietly under leaves near the surface of the ground and he does not fly round very much; he does not leave his hiding place until he sees some animal he wants to feed on.

298. You think it more practicable to deal with the host than with the carrier?—Yes, certainly.

299. After considerable investigation you have come to the conclusion that the wild game furnish the main reservoir of the sleeping sickness?—Yes, Nyasaland sleeping sickness.

300. So far as the small animals are concerned and domestic animals, they are practically negligible?—I think that they may be ignored.

301. You personally, I think, do not share the view that if the wild game were exterminated in certain districts, the fly would be driven to domestic animals?—*Morsitans*?

302. Yes?—There would be no domestic animals in the *morsitans* area.

303. I am assuming that in certain cleared areas where the natives reside there would be some. It is suggested that if the wild game were exterminated in certain areas you might have the fly attacking domestic animals and a reservoir being created that way?—If you took a hundred buffaloes out of a place and put in a hundred cattle, I do not think the flies would know the difference. I think that they would feed on

the cattle as readily as on the buffalo because there is very little difference.

304. Would not that point to the continuation of the reservoir in the district even although the wild game were destroyed? The point is a very important one, I think, because it really goes to the root of the whole question that we have before us. We want to know whether there is another possible reservoir which would take the place of the wild game under certain conditions. I gather from what you told us before that in some cases the wild game is immune, whereas the domestic animal succumbs?—Yes. They do not remain a reservoir as long as the wild game do. A man only lives for three or four months; he is only a reservoir for a week or two.

305. You said that some of the cattle might recover from the *Trypanosoma brucei*?—Yes, but the fly does not depend on *brucei* alone for his murderous acts; he has half-a-dozen weapons in his belt. When he does not kill an ox with *brucei* he does it with something else—*pecorum*, for instance.

306. Have you considered whether domestic animals would become to a certain degree immune if they were continually exposed in certain districts to the fly? Do your experiments not suggest a degree of immunity in certain cases?—That question is rather too theoretical, I am afraid. It opens up speculation.

307. If it is a purely speculative question I do not ask you to answer. I was dealing with experiments which I understand you have made in certain cases where there has been recovery.—The Zulus are very fond of oxen, and if the Zulus could have found a single ox during 200 years that would withstand the fly and that would give a progeny that would withstand it, you may take it that they would have found it, and there would have been an immune race, but there is no such immune race in Zululand, in spite of the great love that the Zulus have for oxen. It must be very difficult to develop any such immunity.

308. Is the tendency in Nyasaland for there to be a clearing of the bush corresponding with the increase of population in those areas where the natives reside?—I think that the population is increasing because there is no war. The whole of the country was a waste desert a few years ago on account of slave raiders. These Arabs lived along the shores of the lake and their dhows took the slaves across to the other side. They raided the whole of the country right back to the hills, so that there was not a single native living in the district. The natives lived among the hills, where there were caves, in which they could hide from the slave raiders. When the raiding was stopped, the population crept down from the hills, and it has been multiplying ever since. If you saw the number of little children about the height of this table in a native village you would understand how they increase.

309. Do you get a clearing of the bush corresponding with the increase in the population?—Yes. For every head of population they must have a certain area of maize field.

310. As the population increases, then, the tendency is to drive back the fly?—The tendency is for the population to drive away the wild game to a great extent from the neighbourhood of villages.

311. And with the wild game the fly?—That is my opinion.

312. Short of fencing in a certain area, do you regard the clearing of areas round about villages as having the effect of driving away the fly from the immediate district?—Yes, from the immediate vicinity of the village. At the same time, a native coming in from the forest would always be liable to carry one or two in on his back.

313. As an immediate remedy, would the clearing of a certain district within a certain radius of human life not have a considerable effect in reducing the risk?—The natives do not spend all their time in the villages. They are always in the fields, and the fields are not like those of Midlothian; there is very often more scrub and hedge than field.

314. With regard to the actual fencing in of an area, do I take it that although it might be impossible to exterminate the last animal in an area (it certainly

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would be I think), and even although a certain percentage was left, there would be very valuable data afforded by the experiment?—Do you mean with regard to the infectivity of the fly?

315. Yes.—Every infected animal is a centre of poison to the flies round about and every animal killed means one centre less.

316. Although it might not be practicable absolutely to exterminate all the wild animals within an area, you might succeed in getting a reasonable degree of elimination of the host which would enable you to arrive at some definite and valuable decision; that is my point. Is that your view?—Yes, I think so.

317. You have consulted those on the spot who have studied the question, and do you understand that in their view such an experiment is practicable?—Yes, referring especially to the magistrate of the district. There are not many white men in the whole country.

318. Four years have been suggested. May I put it to you that it might be possible to arrive at results within a shorter period than four years?—Yes. I think that the first year after the animals were driven out of the fenced-in area, you might find a difference in regard to the infectivity of the fly.

319. What is the age of the fly? For how long does it exist?—You can only make a guess, but we keep them alive in captivity and infected for three or four months. In the wilds they might live for a year or so.

320. If you added a year on would that make a fair test?—Yes, say a year. You may remember that Lord Avebury said that he had known an ant intimately for 15 years, and perhaps the tsetse-fly might live as long as the ant.

321. (Dr. Chapple.) You have referred to rinderpest. Have you any evidence of the alleged fact that where rinderpest decimated the game the fly disappeared and with them the disease?—The only place where I came into contact with that was in Zululand, and I only came into contact with it as a matter of hearsay afterwards. I had written every year for specimens of the fly from the fly area and the magistrate always replied, "There are no flies here now." After eight years, about 1908, he said that there were flies and he sent a boxful, which I gave to Mr. Austen, and since then there have been any number of flies in the fly area. That is fairly good evidence, but it is a very difficult question, for although an enormous number of wild animals are killed, when a rinderpest epidemic goes through the country, still there are a good number left. It will kill the koodoo, impala, inyala, and buffalo, but it will leave a good number of wildebeeste. It may be said that the wildebeeste live on the plains to a great extent, and therefore may not be as good food-stuff for the fly as the bush-buck and the koodoo which live in the thickets, but you would think that there would be a sufficient amount of food left to keep a certain number of fly going, at any rate until the game increased, and that the fly would increase *pari passu* with the game, so one cannot understand how the fly altogether disappeared. It is an opinion expressed by a great number of people that when rinderpest goes through a country it clears the fly out as well as the game. I wrote to Mr. Montgomery in British East Africa with regard to trying the effect of feeding flies on rinderpest animals. I thought that there might be poison in the blood or it might be that the parasite of rinderpest had a deleterious effect on the fly and that the flies fed on rinderpest animals would die. A lot of them were fed on rinderpest animals and it did them no harm.

322. Is it true to say that when game disappears, either from epidemic disease or from the progress of settlement, the fly disappears too?—That is my opinion.

323. It is a generally established fact that game is the reservoir and *morsitans* the carrier of trypanosome disease?—Yes.

324. Is it an established fact that the game and the fly are found together, and that when there are trypanosomes in the blood of the game the flies are infected?—Yes.

325. If what I have put to you is true have you not sufficient data on which to give advice to the

Colonial Office as to what steps should be taken to cope with the disease?—My advice is to clear out the game. I say that in my summary.

326. Have you given that advice to the Colonial Office?—I write papers for the Royal Society, and in one of them I discuss the infectivity of wild game. The paper is open to the Colonial Office; it is an ordinary publication of the proceedings of the Royal Society. In the conclusion of that paper I state that it would be quite as reasonable to allow mad dogs to run about English villages and towns under the protection of the law as to allow this poisonous big game to run about in the fly country in Nyasaland. I think that that puts it as plainly as I can put it.

327. (Sir John Bradford.) The Colonial Office asked the Royal Society to investigate a particular problem, and the Royal Society asked Sir David Bruce to report. Sir David Bruce does not report to the Colonial Office but to the Royal Society. He has not given any advice as far as I know to anybody.

(Witness.) Not the slightest. I only wrote down what I considered to be ascertained facts, and I left it there for the present.

328. (Dr. Chapple.) I want to know whether you think that sufficient data are established upon which to take immediate action with regard to the spread of this disease in Nyasaland?—I think that there are sufficient data gathered to enable us to say that the wild game there act as the reservoir of certain trypanosome diseases of man and the domestic animals. It is a matter of opinion, and many people have a different opinion from mine, but in my opinion if you cleared the wild animals out of the fly country you would stop the diseases in man and domestic animals caused by the trypanosome parasites. Wild animals, even though they are infected, in a non-fly country are harmless.

329. Do you draw a distinction between the extermination of the game and the driving of the game from inhabited areas—the villages?—I do not think it is a practicable thing to drive the wild animals out of a wild jungle country. You may drive them off to-day, but they will be back to-morrow.

330. If you had a village area with a certain number of inhabitants would it be possible by relaxing the game laws, for instance, and giving certain facilities to the villagers to attack the game, to make the animals shy and drive them from the inhabited areas without actually attempting their extermination?—You would only drive them away from the vicinity of the village.

331. Would that be sufficient in your opinion?—They would live in the great stretches of country where there are no villages, the wild country. Before the white man came to Africa the native tried to cope with the wild animal as well as he could by means of pits and traps and so forth, but the wild game existed in countless numbers when Gordon Cumming and others went into the country. Even without game laws the native could not cope with the wild animals.

332. Where animals are attacked by natives is it not common experience that they get shy and remain at a greater distance from the inhabited centres?—Naturally they would be shy, but all these animals are of nocturnal habits. Man in that country goes to bed at night, but the wild animal comes out of his lair at night and does his work during the night. Wild animals, even though they were shy, would approach fairly near the villages in search of food when men were sleeping. They would know that they were safe at night in the dark.

333. I want to suggest to your mind that there is a possibility of attempting something less ambitious than the actual extermination of the game. If it were possible to permit the villagers to attack the game within certain radii from the inhabited centres, would the game become sufficiently shy to remain at such a distance from the village as to keep the flies away, thereby protecting the villagers from infection. Is that possible in your opinion?—I do not think so. As I say in my summary, I think that in the fly country the game laws should be in abeyance and the native ought to be allowed to kill and trap the wild animals as much as he likes. It is part of his food. The wild

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animals are a legacy to him from the gods as it were, and they are of no use to white men except to one or two sportsmen who shoot them. If you could arm the native with good modern guns there would be very few wild animals left, but then policy comes in. You cannot arm large numbers of natives with modern guns. With bows and arrows and traps and pits they will not do much against the wild animals. They do what they can now in spite of all the game laws. They go about with bows and arrows and kill as many wild animals as they can, but the wild animals know them and keep out of their way. They get a certain number of course. If you gave the natives modern rifles and plenty of ammunition there would not be a single wild animal left in the fly country in a year.

334. Would you suggest that as a remedy?—I said just now that it would not be practical politics because you cannot arm natives in places where there are only a few white men amongst them.

335. How would you exterminate the animals then?—I would exterminate the animals in a small fenced-in area by certain means. I might get a couple of companies of the King's African Rifles armed with the best weapons and the best ammunition.

336. Would you do that for experimental purposes?—Yes, to clear a certain area.

337. But why do you want another experiment if you have established all the data that I have put to you? What more do you want to prove?—A thing may be proved to me, but is it proved to everybody?

338. You establish the facts that game are reservoirs, that the *morsitans* is the carrier, and that wherever the game are absent the fly is absent and the danger to the villagers is practically negligible. Now you suggest to us that we should exterminate the game in an area, and I want to know what more you want to prove?—I want an experiment in order to bring forward one more argument on my side. There are some things which if an angel from Heaven came and tried to make people believe they would not believe, and it is the same with me.

339. I think you said that the facts that I have put to you are established?—I think that they are established.

340. If they are established then, why make the costly experiment of railing off?—Mr. Austen, for instance, does not believe that they are established.

341. Are these data which I have enumerated not established by common consent, and if they are established by common consent what further costly experiment do you need? What do you hope to prove by fencing off a limited area that is not now proved?—It is for the Committee to decide whether an experiment should be carried out or not. For my part, I think that our work as a Commission is pretty well finished.

342. Would you suggest that no active steps should be taken by the Colonial Office pending the result of your experiments?—I think that the time has come when the game laws might be relaxed without waiting for any more experiments.

343. (Chairman.) In fly areas?—Yes, and in non-fly areas where game is increasing to too great an extent. I think that there is a tendency for game preservation to be overdone just as there is a tendency for the extermination of game to be overdone, perhaps.

344. (Dr. Chapple.) That is another point?—I think that in fly areas all restrictions should be taken away and that white men should be allowed to shoot game in the country without a licence or anything, and that the natives who live in the area should be allowed to hunt the game as they like. I think that they might be allowed a certain number of guns to assist them in it—whatever the Government thought reasonable—one to every 100 or one to every 50.

345. If that course were followed, might not the natives select those animals which it was profitable to kill and not necessarily those that create the greatest danger?—No. The native is a very catholic sort of creature, and anything that runs on four legs and is eatable he will shoot at.

346. Have you formed any conclusion with regard to the breeding places of *morsitans*. You said just now,

"I understood that they inhabited the vicinity of water and streams"?—That is *palpalis*. You see (pointing to the map) green bushes all over this place, an uninhabited country without any water; that is *morsitans* country. You see streams here and there with marshes round them; you do not find *morsitans* there at all. Only where the scrub is do you find the fly. There is no fly within five or six miles from the edge of the lake. That is palm country. The *morsitans* are inside that dotted line. That fairly represents the distribution of the fly. They are inside that line among the dry thorny scrub. When a female fly grows a big pupa in its inside, when it gets to a certain size, the fly wants to get rid of it, and it goes to the nearest tree and drops its pupa. The pupa can creep into a hole or corner, and within a couple of hours it becomes a hard black puparium. It does not look for food; it cannot feed. When it has been extruded it at once creeps into a corner, into a crack in a tree or among dead leaves. Any quiet sequestered corner is good enough for the pupa.

347. You know of no natural enemies to the fly that could be encouraged?—No. I think not. It is quite outside practical politics. It was once said that chickens might be introduced into the areas so that they might scratch among the dead leaves and find the pupae, but I think that the chickens would succumb first because the country is full of wild animals, snakes, cats, and such like.

348. Is any inquiry now going on along those lines?—Yes. I think that the inquiries in regard to the fly have always gone along these lines. Dr. Carpenter, who has been working on *palpalis*, has been working on those lines all the time for the last 3 years, and on the West Coast the French Government have had Commissions going out for the last 10 years sending zoologists like Roubaud, who have studied the fly to find out some kind of enemy to the fly, but I do not know how it is going to be found. It is not like a peach tree that you can put a large sheet over and then generate hydrocyanic gas underneath.

349. Did I understand you to say that you had seen 200 flies on the back of one native?—I did not say a native.

350. On the back of whom?—On a man driving a motor bicycle. I was sitting in the side car and I had nothing to do but amuse myself by counting the tsetse flies on his back.

351. Not on his bare back?—No; he had a coat on.

352. Somebody asked whether 200 fed on one native?—The native would be too quick for that. In the case of the motor bicycle the man had his hands full with the handles, which he could not let go. I have never seen more than two or three flies on the bare back of a native at any time. Flies are attracted by motion, I think; the quicker the motion the more they are attracted. They come in thousands to a quickly moving object, but you very seldom see more than two or three on a man's back at the same time in the case of people walking quietly through the scrub.

353. Do they alight more readily on the black skin than on the white skin? Do you know of the experience of the Frenchman who found on examining the French flag that the flies were deposited on the dark parts and not on the white?—There is a certain amount of truth in that with regard to colour. They will not alight on white, for instance, as they will on a dark colour, but they bite white people with great virulence. I have been bitten in hundreds of places. It is only when you are sitting on a motor bicycle and cannot move your hands that you get bitten in that way, because you cannot move your hands from the handles. The paths in parts are like a frozen ploughed field, and you bump about like anything, and unless you hold on tight with both hands you are into the jungle. The flies take advantage of the fact that you cannot let go, and sit in rows on your face or neck or hands and bite away as hard as they can.

354. Do you think that you have been bitten by an infected fly?—I should say that I have run a very fair risk of being bitten by an infected fly. We consider about two per thousand of the flies to be infected with *brucei vel rhodesiense*.

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355. Two in a thousand in an infected area are infected with this particular trypanosome?—If you take all the trypanosomes, there are more flies infected.

356. Can you tell me what percentage of those bitten by infected flies get the disease?—I wish I could tell you. That is exactly what I have been trying to get at for years, but I cannot try experiments on man.

357. What percentage of the antelopes bitten by infective flies would show trypanosomes in the blood?—Antelopes are few and far between and difficult to handle, but we can deal with monkeys in hundreds. If 100 monkeys are bitten by an infective fly, I expect 100 per cent. of infection. The monkey is very susceptible to *nagana* or *rhodesiense*. He is killed in a fortnight or three weeks by this particular trypanosome.

358. You get immunity to the organism in some animals and immunity to the disease in others?—You get certain blood that resists the organism. Human blood has an antagonistic effect on the trypanosome *brucei* vel *rhodesiense*.

359. Do you think that an anti-toxin is developed in the blood of any animal that has trypanosomes in its blood?—Yes.

360. Can you make use of that for practical purposes so as to give an artificial immunity to animals that are susceptible?—No, not for practical purposes.

361. Have you tried it?—I do not think we have tried it from that point of view. I told you a little while ago, you will remember, that an animal, say a goat, might be given *brucei*, and recover from that; that if you give it a second dose it has a milder attack, and if you give it a third dose it has a still milder disease, and that after that it will take any amount of that strain of that particular trypanosome. These strains are so different in virulence that you can get another strain of the same species of trypanosome and inject it into the goat and give it the disease again.

362. Have you taken serum from the blood of a goat rendered artificially immune and injected it into another animal and produced immunity in that way?—The results up to the present are rather meagre, but every man in Europe who has been experimenting with trypanosomes has been making experiments of the sort for years. For five, six, or seven years they have been made on these lines, but the results have been very meagre. Only now and then you hit upon an anti-diphtheritic serum.

363. Do you know any case in which salvarsan has been used?—It has been tried very often in Congo sleeping sickness. I do not know whether it has been tried in Nyasaland sleeping sickness.

364. Your work has not so far covered experiments on those lines?—No; I try to stop a man getting a disease, but when he has got it I have done with him.

365. (Chairman.) You have told us very clearly your opinion in the course of your evidence, that if the game is removed or removes itself from the fly area, the fly goes at the same time, but I think you will admit that there may be unknown factors, and that in some cases there are other things to which the fly might turn its attention which deserve inquiry. I do not say that it is so, but it may be so. As regards any extensive attack on game throughout Africa, may I take it that your view is expressed in Part VI. of the Report? I will read one sentence: "This inevitable disappearance of wild animals before the advance of civilisation is very different from the instant carrying into effect of an international measure for the wholesale destruction of big game all over Africa. Such a measure in the present state of our knowledge would be quite unjustifiable, and would probably fail to a great extent in its effect." Is that the view that you hold on that matter?—Yes, it is.

366. Are you acquainted with the circumstances under which leave was given in 1912 to natives to destroy game? Am I right in saying that the natives did not show much enthusiasm, and that a very small proportion of the game was killed or affected by that leave? Is that within your knowledge?—I have not been on the spot, and I do not know anything about

the native population, and I do not know much about the white population. There are a few missionaries in the neighbourhood. I think that, generally speaking, the effect of the opening up the district to sportsmen was nil.

367. With regard to experiments, although you yourself clearly hold the opinion which you have expressed, there are others who would possibly controvert it, and think that other factors might come in which should modify your opinion?—Every person has his own idiosyncrasies.

368. In order to obtain further knowledge upon the subject experiment is suggested, in order to get better and stronger data?—That is my opinion.

369. I only wanted to get on record your attitude in the matter. Now, there are two methods by which to approach the question. One is the elimination of the fly by the extermination locally of game; and another is the destruction of the fly by some method, if it is possible to do it. I understand that in your opinion the entomological side of the inquiry has not gone very far at present, and you think that to wait for the entomological side of the inquiry might mean an increase of disease and the postponement of measures for dealing with it. Have you seen the suggestion that there are certain probable breeding areas where, if the theory of Mr. Fiske is correct, very extensive destruction of the fly might be effected?—Yes.

370. While other matters are being considered, is it not very important that every effort should be made to promote and carry on entomological inquiry in regard to *palpalis* and *morsitans*?—I think that all knowledge is good. With regard to Mr. Fiske's theory, that leaves me quite cold. Who knows what may happen in a few years? I have made big blunders in matters of opinion in the past and I may be making another now.

371. In regard to laboratory experiments, you said that the smaller animals would acquire the trypanosomes?—Yes.

372. The fly, you said, is not a nocturnal feeding animal; but is that generally accepted? I have read papers in which it is suggested that there are only two hours of the night in which the fly does not bite—between 12 and 2 or 1 and 3 o'clock, and that there are cases in which people have been bitten during the night by the fly?—But those are all exceptions. It is a day feeding insect. It is not even a question of daylight, because flies do not trouble you at daylight, say at 5 o'clock, in the tropics. It is not until 8 or 9 or 10 o'clock that the fly becomes troublesome; when the sun is well up and the place is warmed.

373. (Sir Mackenzie Chalmers.) When you go back, what line of research are you going on? You have not quite completed your researches, have you?—Well, we have not fully worked out this Nyasaland strain in relation to the Zululand strain yet. That is on the stocks. I am going out there to finish up, and to write the remaining reports that require to be written.

374. To correct proofs and papers?—Our Commission was supposed to be for three years, and on the 31st March 1914 our third year will have come to a conclusion.

375. Quite apart from the term of the Commission, do you suggest that any further work should be done on any particular line?—That is a question that I am asking myself every hour of the day.

376. Further work on any specific line?—I know that there are hundreds of things that ought to be done, but after two or three years in the tropics you get stale and you cannot think of them. You know that they ought to be done, but you cannot think of them. Perhaps this Committee would send out a list of such things and that would assist. After a certain time you get stale and you cannot think of what things ought to be done until after you go away.

377. You have no new line of research that you are going out to take up now?—We are going out to round up, to complete the outstanding work of the Commission.

The witness withdrew.

SECOND DAY.

Tuesday, 14th October 1913.

At 3, Queen Anne's Gate, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
 Dr. A. G. BAGSHAW.
 Dr. ANDREW BALFOUR, C.M.G.
 Mr. E. NORTH BUXTON.
 Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
 Dr. W. A. CHAPPLE, M.P.
 Colonel Sir W. B. LEISHMAN, F.R.S.

Dr. C. J. MARTIN, F.R.S.
 Dr. P. CHALMERS MITCHELL, F.R.S.
 Professor R. NEWSTEAD, F.R.S.
 Mr. H. J. READ, C.M.G.
 The Hon. L. WALTER ROTHSCHILD, F.R.S.
 Sir STEWART STOCKMAN.
 Mr. A. C. C. PARKINSON (*Secretary*).

Mr. WARRINGTON YORKE, M.D., called in and examined.

378. (*Chairman*.) I do not propose to put many questions to you, because there are a great many gentlemen here more skilled and more acquainted with the matters under consideration than I am, but I would like to ask you two or three questions on the proof you have been good enough to furnish. I may take it broadly, I suppose, that whatever the exact details of the experiment, you think some experiment which would have the effect of driving, at any rate, the larger mammals out of a given area to be selected, would be likely to lead to results of some value?—I do.

379. You think that probably they form the principal reservoir, but certainly a reservoir of the trypanosome, and that by the removal of the mammals one of two results might follow, either the fly might follow the mammals and the area might be cleared of the fly, or the mammals having gone the fly would gradually cease to be infective?—Yes.

380. Those, I infer, are the results which you would hope for so as to throw some light on the problem submitted to the Committee?—Yes.

381. The thing I should like to draw attention to is, how far such results would be reasonably probable: firstly, is it the case, in your judgment (we have evidence on that point already), that *Glossina morsitans* is dependent on blood and only on blood for its sustenance?—So far as I know, yes, and I think so far as anybody knows, it is.

382. And without blood it would not survive in any numbers?—I think not, but that point would be brought out by the experiment.

383. You have examined, I imagine, a good many flies?—I have.

384. Is it your experience that all the flies you have examined had traces in them of blood?—Oh no, it might be completely digested, but I think in those flies in which we found blood about 90 per cent. had mammalian blood, and the other 10 or 12 per cent. had avian blood.

385. But they all had blood?—No, very many of them had no blood at all, but that might be due to the fact that the blood was digested, because the flies do not feed every day.

386. There is a good deal of information, you are probably aware, from observers to the effect that you will occasionally, at any rate in a good many cases, find the fly in a country where there is practically no large wild game now?—Yes, I have no experience on that point at all myself.

387. You are aware that others have so stated?—Yes.

388. And the converse is also the case, that there may be quantities of game and no flies?—That is so.

389. Take the case, if you would not mind assuming for the present purpose that that observation is correct,

that there may be fly and no wild mammals, on what blood would that fly subsist?—I think it is rather an assumption to admit the statement; I do not know that I am prepared to admit it.

390. I do not ask you to admit it; I only ask you to assume it?—I should imagine in that case they would feed on birds.

391. There would be smaller animals which could not be limited very well?—Yes, there are rats and mice and smaller vermin, but, of course, rats, mice, and monkeys are very easily killed by the trypanosomes, so that I think if they were fed on by the tsetse-fly to any considerable extent they would be exterminated. I examined a very large number of monkeys, rats, and mice, I think over 400, without finding a single instance of infection with trypanosome.

392. What about man?—As a reservoir, do you mean?

393. Yes.—Of course, there are a number of cases of sleeping sickness, and those will be reservoirs; but in my experience the human beings who are infected with this trypanosome quickly become ill and do not travel about; they remain in their villages, and, in comparison with the game, they form, to my mind, a quite insignificant fraction of the total reservoir.

394. I do not quite follow it, and I dare say it is my ignorance; but I am rather supposing the case of men who are immune from long habit; you would not have any actual knowledge of the possible reservoir they might form?—People who are immune? If they are immune they cannot become infective, and therefore they cannot be reservoirs.

395. I thought the basis of the animal case was that they were immune, and therefore they were permanent reservoirs?—It is a question of terms, perhaps—"tolerant," I will say; that is, they harbour the parasite without showing symptoms of disease. All I can say is that there is no evidence that man is tolerant.

396. You admit that man is a possible reservoir?—No, I am not prepared to do so except to the extent mentioned before.

397. As regards domestic animals (I would like to exhaust this), might they to some extent form a reservoir in some circumstances, either (I use the word "immune," but you know what I mean) immune individuals or goats and sheep, which, I understand, are considered to be possible reservoirs?—In the Luangwa Valley, in 1905, there were large herds of cattle and goats and considerable numbers of dogs; in 1911, when Dr. Kinghorn and I went to the valley, there were very few animals left, and in the middle of last year I crossed right across the valley from Fort Jameson to the mountains on the other side, and there was not a solitary domestic animal alive. The explanation of that is that the fly has gradually spread in

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[Continued.]

the Luangwa Valley since 1904 up till now, when it has killed off every domestic animal; so that I cannot think they can be reservoirs of the disease to anything like an extent at all comparable with the big game, the wild fauna.

398. Is it conceivable that fly could affect one another?—I say, no.

399. Assuming the wild mammals to be the principal reservoir, to be the principal cause of the spread of the trypanosome, do you consider it is a very serious factor as regards the disease being conveyed to human beings?—Yes. I consider that the trypanosome is conveyed from the game to man by the tsetse-fly, *Glossina morsitans*, in the great percentage of cases in Rhodesia and Nyasaland.

400. That, surely, hardly produces anything very serious if you put together the proportion of the flies you find to be infective in nature and the opportunities which that proportion of infective flies would have of biting man and man taking the sleeping sickness. I am trying rather to gauge how serious it is as regards the human disease?—We found that 1 in 500 wild tsetse flies were infected with the human trypanosome. I should say that, unless man was to a considerable degree resistant to infection, that would be sufficient probably to clear off everybody in the place in a very short time.

401. But it does not?—No, and the explanation of that is that man is probably difficult to infect, but when once he becomes infected he goes down hill rapidly. That is the only explanation I can offer, because I am convinced that 1 in 500 is sufficient to wipe out the population if each time an infective fly bit a man that man becomes infected.

402. Do you admit or reject any theory of mechanical transmission?—We never found mechanical transmission after 24 hours.

403. I do not quite follow that; there can be mechanical transmission within 24 hours?—The flies do not transmit mechanically after an interval of 24 hours; if you feed them on an infected animal and keep them 24 hours they will not infect a clean animal.

404. (Dr. Chapple.) Until a later period?—Precisely—until a later period. Sir David Bruce showed that after an interval of two or three hours they might in a proportion of cases, but not after 24 hours.

405. (Chairman.) I leave that point. As regards the proposed experiment which has been suggested, to fence in an area and to kill or expel the game and watch the result, do you think that is practicable?—I hardly think it is practicable to fence in an area sufficiently large; it could be done, I suppose, if sufficient money were spent in putting up an enormous fence, but it would have to be exceedingly strong to keep out animals like an elephant or buffalo or a rhinoceros.

406. If you could not fence it in successfully or keep the fence successfully, in what way would you be able to carry out the experiment?—I suggest myself that the experiment should be undertaken in an isolated fly-belt, such as the Sebungwe district which you see on the map. That is a fly area that is isolated by between 70 or 80 miles from the nearest other fly area, and the plan, I think, would be to shoot down everything in that area. There is very little water left, I understand, in the dry season, and one could poison what there was, and in that way I think one could clear out the mammals by constant shooting. Many of them would leave the area, but that would not matter because they would go into a non-fly area.

407. Whether you fence, or whether you do not fence, do you know sufficient of the habits of the *Glossina* to determine what would be a satisfactory result? If after an interval of years you found there was still fly there and that some of the fly were infective, would it not be possible for that to be misleading, as fly might travel into that area from another place where they had become infected?—That is one reason why I should indicate this particular area; it is 70 miles away from the nearest fly area, that is to

say, they would have to cross over a lot of clean country, which is very unlikely, I should imagine.

408. Supposing such an experiment were undertaken and was apparently successful in its working, how long a period do you think it would be before any judgment could be formed as to the effect?—I should say probably three years; of course that is a very difficult question to answer.

409. If the fly had gone, then it might be conclusive, but if the fly remained infective, which is what I was wanting to know, how long would it be before you think the fly, on account of the absence of wild mammals, would have lost its infectivity?—We do not know how long the fly live. About 272 days is the longest record of a fly in captivity, but quite probably in nature they may live for a considerably longer period, and one must wait until those flies are dead, until the next generation of flies comes, before one can judge as to the effects.

410. Is it not one of the greatest difficulties, almost the central difficulty, that we do not know much of the habits or general movements of the fly?—I cannot see that at all; if this experiment were performed we should know a good deal more at the end of it than we do now.

411. It might be a step, but one of our difficulties at the present moment is that we know so little about the movements of the fly?—I cannot see that that is any objection to the experiment.

412. (Dr. Chapple.) Would you tell us what facts you hope to ascertain by further experimentation which are of practical importance, and which facts you think are of simply academic importance?—I think the most important point is whether or not, after you have eradicated the wild animal, the tsetse-fly will become non-infective. That is the first point. In other words, whether they will cease to be capable of infecting men and domestic stock with trypanosome diseases. That is the most important point. Secondly, whether the fly is dependent on the big game, the wild fauna; that is, if you remove the blood supply, the food supply of the fly, whether or not the fly will disappear. I think those are the two essential points; there are many others which you might call of academic interest.

413. Are those points not yet proven?—I say, no.

414. Are you not satisfied in your own mind?—I am satisfied in my own mind as regards the first of those two, that is, if you destroy the game you get rid of the trypanosomes, but, of course, many people say that is not proven, and that we must do this experiment, and that they will not be satisfied until we do it. I should suggest that at the present time all game restrictions should be removed in fly areas, and that natives and Europeans should be allowed to destroy game in any way they like in fly areas, and especially in inhabited portions of fly areas.

415. Do you know any area where there are both game and flies but no disease?—I do not.

416. You do not know of your own knowledge?—No. I think the more one examines the more you find the cases in quite unexpected places. I myself remember finding a case quite close to, in fact actually in, Fort Jameson. A native had come into Fort Jameson from a short distance. Fort Jameson has been held to be absolutely free, but I was examining a gang of natives going to work in Southern Rhodesia, and there was a very early case who had been infected comparatively close to Fort Jameson. There was another case which turned up, in a white man, in North-Western Rhodesia, which was considered to be free, and again, there is a white man in Southern Rhodesia. When you get white men infected, you examine the natives, and we then found out in Sebungwe a number of natives infected. Therefore, I am of opinion that whenever you get *morsitans* and game you get sporadic cases of sleeping sickness.

417. It is conceivable, however, that you might have game and fly without the trypanosome?—No, I should say not.

418. It is not conceivable, you think?—One might conceive anything.

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419. You do not suggest that the trypanosome starts *de novo*?—No, I do not.

420. It must extend from some infected area in some other part?—I should say it has been in those areas for some considerable time—anyway, for hundreds of years.

421. Is there any evidence at all that the trypanosome dies out after having had a period of existence and infectivity for any length of time?—Out of the fly?

422. No, out of any area?—No, I have not heard of any.

423. Could you not find, by laboratory experience, whether these flies could live without blood?—I think experiments have been conducted on that line, but without result.

424. But they die?—Yes; I know of no evidence to support that view at all.

425. Have you tried that experiment?—Yes, I tried feeding them on vegetable juices, but it did not work; they died.

426. Have you made extensive experiments to find out whether there is any other diet which these flies will live on?—No.

427. In answer to the Chairman you said man was not a possible reservoir. What did you quite mean by that?—He is a reservoir, of course; every sick person is a reservoir, but my statement was that, in comparison with the big game, human beings form an extremely small fraction of the total reservoir. It is like a drop in the ocean.

428. That is so now?—Yes.

429. But in any populous part, if they spread they might become a very formidable reservoir?—Certainly.

430. Would it not be possible always to isolate them?—Yes, if you have sufficient medical officers you can isolate them, certainly.

431. If domestic animals contracted the disease at what stage in the progress of the disease would you be able to ascertain its existence in such a way as to enable you to say that that animal would certainly die, and therefore that it would be best to kill it?—You could recognise the disease as soon as one sees the trypanosomes in the blood. You can say the animal is going to die then, and that might be within six or seven days of the infection.

432. What would be the first clinical indication that the owner of the stock would recognise?—Probably emaciation.

433. How soon would that come on after the infection?—It varies with the different trypanosomes. You are referring to the human trypanosomes?

434. Yes.—It all depends upon whether the stock happened to be European or native or whether they happened to be half bred. European cattle would die very quickly.

435. How soon would they manifest unmistakable signs of the disease so that they could be killed and thereby cease to be a reservoir?—We infected three native cattle, and they were emaciated after a month or so. I have inoculated a number of horses and donkeys at Runcorn, and the course of the disease is exceedingly rapid, and they have been dead within a couple of months. I have not inoculated any European cattle.

436. You are not able to say then what is the earliest period at which clinical manifestations would justify anyone in killing a European domestic animal?—Clinical manifestations are no use. You cannot diagnose the disease by such manifestations. These diseases can only be diagnosed by the microscope. Clinically, one cannot diagnose these diseases.

437. If an ox is inoculated with the trypanosome it must surely manifest some clinical sign of disease at some period in the history of the disease?—Precisely.

438. What time elapses between the time of infection and the first clinical manifestations observed?—You mean manifestations that may be recognised by the owner?

439. Yes?—I should say a month, perhaps.

440. Do you not get swelling and oedema round the seat of inoculation?—No, certainly not early.

441. And a whole month might elapse until the trypanosomes are swarming in the blood?—They do not swarm in the blood of cattle.

442. Perhaps that is not exactly the word—but the blood remain infective?—Yes. I say a month might easily elapse. If you will allow me to refer to this paper for a moment, we inoculated three cattle at Mpika, and trypanosomes were found in the blood seven days after inoculation; one died in 209 days, the second in 210 days, and the third in 186 days. These were native cattle, and there were signs of emaciation as far as I recollect within two months, that is to say, in about 50 days.

443. That would be the first indication the owner would have that his animal was diseased?—Yes, I should say it would be.

444. If that is so, from the moment of inoculation by an infected fly up to the moment at which the owner first recognised that his animal was diseased, it would be a reservoir of the disease?—Yes, it would.

445. And therefore a source of danger to other cattle and to man?—Yes, undoubtedly.

446. That is the point I wanted to get at; I am afraid I did not make my questions particularly clear.—Of course, it is necessary to point out that there are, in addition to the human trypanosomes, other trypanosomes in Africa which kill domestic stock; so that I should imagine that in a fly area an ox would have a very poor chance of living 210 days; it would be killed long before then by one of the domestic stock trypanosomes.

447. Is it not obvious that if a domestic ox contracted this disease and it was invariably fatal, the owner would immediately be recognised that his animal was affected with a fatal disease, kill it?—Yes, he certainly ought to.

448. But up to the point at which he would recognise that you would not expect him to kill it, nor would you expect him to have a veterinary surgeon to examine its blood for trypanosomes?—No.

449. To that extent then, there is a danger of domestic stock becoming a source of infection, even although the game were eliminated?—No, they might for a time, but you cannot have fly and domestic stock in my experience. There is the Luangwa Valley in which the Government official at Nawalia, where we were stationed, had in 1905 a large herd of cattle. The fly was only beginning to come back again in 1905, after the rinderpest, and he told me that it was with extreme difficulty that they could get a specimen of the fly; a boy would only bring home one or two specimens in a day in 1905. In 1911, when Dr. Kinghorn and I went to the valley, the fly was exceedingly thick; one had three or four on one's back all the time; there were no cattle at all at Nawalia, but there were a few left scattered up and down the valley. At the end of that year or the beginning of 1912, I think in April 1912, I crossed, as I told the Chairman, from Fort Jameson to the Muchinga Mountains and there was not a solitary domestic animal alive except three cattle, all of which were infected and dying from trypanosomiasis, so that if you have much fly the cattle die and they cannot form a reservoir of the disease at all comparable with the game which are tolerant to the infection.

450. You told the Chairman that for the first 24 hours after the fly had bitten an infected animal, it was capable of transmitting the disease by mechanical means, that is by direct infection?—I was quoting experiments of Sir David Bruce; that is what Sir David Bruce stated originally a number of years ago in 1908. Dr. Kinghorn and I did not find that. I will not say that; that is incorrect; we did not examine for that point but we found that after 24 hours they could not infect.

451. Up till what period of time?—It varied according to the temperature; I think the first was 11 days afterwards, after the infected feed. It varied from 11 days up to as much as 60 or 70 days on the plateau, varying inversely with the temperature.

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[Continued.]

452. Do you assume after that that some metamorphosis took place in the body of the fly?—Yes, there is no doubt on that point at all.

453. And then it would remain infective for life?—Yes, for the rest of its life.

454. Is there any change in the degree of the infectivity?—Not that I know of.

455. Can you explain why there are so many infected flies which bite natives and whites and yet produce the disease in one person and not in another?—I think it depends on individual susceptibility, perhaps on the condition of health. That is a pure hypothesis, a conjecture; it is obvious that one cannot determine that point.

456. You have not been able to detect in any individuals who have fallen a prey to the disease any peculiar diathesis that makes them susceptible?—No.

457. Do you take any personal precautions against contracting the disease, and if so, what?—Yes, one avoids being bitten as much as possible.

458. How do you do that?—I will give you an illustration. I remember going into a fly belt in 1907 before it was known that *Glossina morsitans* transmitted the disease, and there I met a gentleman in a pair of dancing pumps and a shirt, with nothing else; he walked round the country like that, and he was being bitten all the time. We were completely clothed; we wore puttees, and one always turned up the collar of one's coat, so that as the result one only exposed the face and hands. It is a question of clothing, I think; one could wear veils, but in the Luangwa Valley it was too hot.

459. Do you know any specific cases who have been bitten by infected flies and yet not contracted the disease?—Yes, one. Dr. Taute of the German Sleeping Sickness Commission.

460. Are you satisfied then that the data you have now, or the data that are established facts with regard to game, tsetse-fly and sleeping sickness justify you in giving any specific advice with regard to the prevention of the disease?—Yes. I consider that the game ought to be destroyed in the neighbourhood of the human habitations in fly areas.

461. Do you think the relaxation of the game laws would be sufficient to drive the game away from inhabited areas, or would you carry out a systematic extermination of the game?—I should carry out a systematic extermination in those particular fly areas in the neighbourhood of human habitations. I should encourage the natives to shoot.

462. Wherever you had a population at all, would you exterminate the game?—Yes, wherever there was population in fly areas.

463. Irrespective of its value or of the nature of the game?—Yes.

464. Do you think the problem is an urgent one?—Yes, indeed.

465. (Sir Mackenzie Chalmers.) Would you recommend those drastic measures before your suggested experiment had been tried, or not?—I say, if the experiment is to be done, we must wait and see the result.

466. Of course, if your experiment is successful, you mean it would involve exterminating game throughout fly areas?—Yes, doing as much as one could, of course.

467. I wanted to ask you a little more about this isolated fly area: what is the size of the fly area?—In the Sebungwe district?

468. Yes.—It is given as approximately 2,500 square miles, say 50 by 50.

469. Then round that there is 70 miles of fly free area?—Yes, that is in one direction; in other directions it is indefinite, practically hundreds of miles. The nearest Mafungabusi, which is about 70 miles away, I understand.

470. So that the nearest infective flies would have to fly 70 miles?—Yes. We do not know whether those are infective or not; no sleeping sickness has been found in this other area, and that may be due to the fact that it is at a greater altitude.

471. You are not aware of any smaller fly island which would be an easier experiment to carry out?—No, I do not know of any. There are a number of

areas in Southern Rhodesia, and if one examined them one might find sleeping sickness in other areas, but one is only likely to find sleeping sickness in those areas which are above a certain level, say 3,000 feet. That is quite arbitrary.

472. You have not discussed with any of the district officers what would be the expense of this operation?—No; that I have not gone into at all.

473. Is it at all known the amount of game which is in this 50-miles island?—No; personally I am not acquainted with this district in Southern Rhodesia.

474. Do you think it might be done by hunters?—European hunters?

475. What practical steps would you propose to take supposing this experiment were decided on?—I think one ought to have three or four medical men to make the necessary examinations; first of all to determine the infectivity of the fly, what percentage of fly was infective, and to decide the number of cases of sleeping sickness and the number of cases of infection in domestic stock, in goats, and whatever happened to be in the district, and get all these data first of all; the main thing being the infectivity of the fly, the percentage of the fly infective with each of the trypanosomes; then I should encourage the natives to shoot. I think there are 3,000 natives in this area; I do not know how many adult males there are, but I would hand out rifles to the lot of them, and they would make a considerable impression on the game in a short time I am convinced.

476. You must make an absolute clearance of the game, must you not, because a small amount of game would infect a great many flies?—That is true; I think one could, for practical purposes, exterminate everything.

477. You think it could be done?—Yes, you might have an odd duiker left.

478. You cannot give any estimate of the expense or the time that it would take; that has not been worked out at all?—The difficulty would be for me to determine the expense of the administrative part of the work, that is to say, the expense of the arming of the natives and the cost of the ammunition.

479. Is there no danger, if you exterminate these animals, of animals coming from other regions and occupying the area?—No, I do not think there is any danger of animals coming, because you would keep on shooting. That must be done, unless you put up a fence.

480. One could not fence an area of 2,500 miles; that is impossible?—I do not think you could put up a fence for 50 miles. I have seen these large antelopes, eland, going through the enormous fences which the natives put round their gardens.

481. I suppose a rhinoceros will go through most things if he is in a hurry?—Yes, and I have seen an elephant twist up an iron telegraph pole like a hair-pin.

482. In order to try the experiment you must have a fly island with a free area all round; that is the only practical experiment?—Yes; and the main advantage of that is that you do away with the problem of infective fly coming in from outside, which would vitiate the results of the experiment.

483. Have you any tornadoes in this region? Are flies blown for great distances in tornadoes?—I do not think one does get very much in the way of wind there.

484. It is not like the West Coast?—No.

485. Your theory is, that if you exterminated the game, the fly would probably follow it, and disappear?—No. I express no opinion on that point at all.

486. You think it would become non-infective?—Yes; and I think it is very important to determine whether or not the fly would disappear.

487. That is one of the objects of the experiment?—Yes, that is the second main object.

488. Do you agree with Sir David Bruce in this, that the *Trypanosoma rhodesiense* and the trypanosome which produced ordinary nagana are one and the same?—I must be quite definite on this point.

489. He came to the conclusion—you saw his proof?—Yes, I am perfectly familiar with the

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[Continued.]

literature. Sir David Bruce was sent out to Zululand first of all about 1896, and he described the trypanosome disease of domestic stock, which he called nagana. In those days we thought there was only one trypanosome in Africa, and he sent home a dog infected with the trypanosome, and this was described by Mr. Plimmer and Sir John Rose Bradford as *Trypanosoma brucei*. Unfortunately for Sir David Bruce the dog he sent home was infected with a different trypanosome to the one with which he was working, and therefore this *rhodesiense* is not nagana, but I think for practical purposes it is the trypanosome which affects domestic stock. If Sir David Bruce says it is the *Trypanosoma brucei*, I think he is wrong.

490. There is no morphological difference between the two?—Marked between the *Trypanosoma brucei* and the *Trypanosoma rhodesiense*; I can recognise them at a glance with the microscope. The difference is that in *rhodesiense* there are trypanosomes of two sizes; it is what we call a polymorphic thing. In *brucei* there are only monomorphic trypanosomes, and one can recognise them at a glance, and there can be no question about it.

491. You think *rhodesiense* is the only one which produces the human trypanosomiasis?—Yes. I do not say for a moment that *rhodesiense* is not scattered through the domestic stock; it is. It is probably the thing that Sir David Bruce was working with in Zululand—certainly in Uganda. It is the cattle disease of Uganda and Zululand really.

492. There have been great outbreaks of nagana without any sleeping sickness, have there not, or should you doubt that, and think that the cases have not been discovered?—I think if one looks in Zululand, one might find them. I should not be at all surprised if this trypanosome has occurred in Zululand, if Sir David Bruce is right in saying that the trypanosomes he found in cattle in Zululand are the same.

493. It is very difficult to get at the diseases in the native population?—Yes, it is.

494. Does this trypanosome merely spread by fission, or is there a spore, or what?—In the mammalian blood it divides by fission.

495. Is there no other way of multiplying it?—No.

496. In the fly, for instance?—In the fly it is quite different; there, I think, they undergo a sexual multiplication, comparable to the malarial parasite in the mosquito.

497. In the mammalian blood it is pure fission, is it?—Yes.

498. Is there any experimental work of this sort? Are the symptoms produced in man or mammals merely the mechanical multiplication of the trypanosome, or is there a toxin?—That is a very difficult question to answer; I do not think it can be for a moment due to the mechanical presence of the trypanosomes, because in experimental animals one often finds them with as many trypanosomes as red blood cells in the body, and they live for months, or for weeks at any rate, like that without exhibiting any signs of disease at all. In man with a comparatively insignificant number of trypanosomes you find marked symptoms of disease. It must be a toxin, really.

499. Do you find the trypanosomes in the muscular tissue or only in the blood?—You find them all through the body, in the nervous tissue as well. I have seen them in the cornea of the eye in large numbers.

500. Have you made post-mortems on human beings or animals when they died from trypanosomes?—Hundreds.

501. What are the symptoms which account for death at the last? What are the appearances?—In men meningitis—nervous symptoms.

502. That is probably from a poison, you think?—Yes.

503. Or is there mechanical blocking of the veins?—No, I should think not; it is poison.

504. Will the fly feed on dead animals?—Yes, freshly killed animals they feed on, not on animals that have been killed for some time.

505. I was thinking whether a dead animal would be a source of infection?—I should think not; the

trypanosomes disappear from the blood very quickly, and they cannot get blood out of dead animals.

506. I suppose there is not much danger in making a post-mortem on an animal or man?—For a certain number of hours; we found after as much as 48 hours the human trypanosome in the cadaver, but the fly could not get blood from an animal which has been dead for a few hours.

507. So far as you know, the blood is necessary for the fly's life?—So far as I know it is necessary.

508. With any of those animals did you try any remedial treatment?—No.

509. With human beings it has been absolutely a failure?—Yes, I tried with negative results.

510. Is life prolonged by arsenical treatment?—It may be a little.

511. It is with the *gambiense* considerably?—Yes, *gambiense* is apparently much easier to cure, but I think there have been no well-authenticated cases of *rhodesiense* recovering.

512. Do you think the resistance to *rhodesiense* is a matter of a population which has got used to the disease, or are white and black alike resistant to the disease?—I do not think people become readily infected; Taute himself did not become infected although he fed infected flies upon himself.

513. You cannot judge from one experiment?—No.

514. (Mr. Rothchild.) Did he not also inject infected blood into himself as well?—Yes, he did.

515. (Sir Mackenzie Chalmers.) He might be specially poisonous to the poor trypanosome?—He may have been, but from one experiment one can judge nothing. On the other hand, one can state Lanfranchi's case, where he became infected with what is known as *brucei* in the laboratory.

516. Was that from a cut?—Yes, but it is the same thing; Taute injected it with a syringe.

517. So far as you know, in the case of human beings the only source of the infection is an actual puncture?—Yes.

518. Eating infected game?—No, that would not have the slightest effect. People have done experiments in which they proved that trypanosomes could go through the unbroken skin; they painted the infected blood of a rat on an animal's skin.

519. Which had been shaved?—If it had been shaved they would take great precautions that there were no lesions or abrasions.

520. Was that experiment successful or unsuccessful?—It was successful in rats.

521. But in nature, for all practical purposes, if you eliminate the fly bite you eliminate the danger of disease?—Yes.

522. (Chairman.) Was that *brucei* or *rhodesiense*?—I think it was one of the animal trypanosomes; I do not recollect.

523. (Mr. Buxton.) But by mere contact the infection was established?—It was in this case.

524. (Dr. Chalmers Mitchell.) You have worked a good deal at the morphology of these things, have you not?—Yes, I have.

525. You agree that there is a kind of group called the *brucei* group of trypanosomes?—I think the *brucei* group is a very unfortunate name.

526. You agree that there is a group—never mind what it is called?—Yes, there is a group, what I should call the *gambiense* group.

527. You certainly have *gambiense*?—Yes.

528. And you certainly have something you call *rhodesiense*?—Yes.

529. And you may, or may not, have something else, a purely animal one?—Yes, that is possible; that is the one Sir David Bruce calls *brucei*; and which other people call *ugandæ*.

530. You are fairly satisfied you can tell those morphologically?—No, I can undertake to distinguish *gambiense* from the other two, but I could not distinguish between the thing that is called *brucei* by Sir David Bruce and *rhodesiense*. It is a rather complicated thing. What is commonly known as *Trypanosoma brucei* is described by Plimmer and Bradford as a monomorphic trypanosome, and that can be

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distinguished by everybody, but Sir David Bruce unfortunately calls something else *Trypanosoma brucei*.

531. (Sir Mackenzie Chalmers.) You and Sir David Bruce are really not at variance?—No.

532. (Dr. Chalmers Mitchell.) You agree definitely that there are two trypanosomes, and you distinguish *gambiense* and *rhodesiense* morphologically quite apart from the clinical symptoms?—Yes, you cannot distinguish them clinically; morphologically you can.

533. Do you think that is generally admitted—the morphological distinction?—Yes, I do.

534. When you tell me that something like one in five hundred of flies is affected with the human trypanosome, that is of course a morphological distinction you are making?—The way we ascertained that was by feeding the flies on monkeys, and then examining the monkey's blood. We fed the flies in groups of a hundred on monkeys, and if a monkey became infected we isolated the fly, and we found that in one hundred there might be two infected flies, and in another hundred none. It worked out at one in five hundred.

535. When you say that one in five hundred flies are infected, you do not mean that you catch the flies in the open, and make experiments?—No, we say they are infective, not infected; that is to say, they are capable of transmitting the disease.

536. And it is the morphological evidence you depend upon?—Yes, and any other evidence.

537. If you had only the morphological evidence to depend upon you could rely upon that? I am trying to make my mind clear about that, and I might explain what I am trying to get at. Apparently we had the idea when this disease called sleeping sickness appeared that it had been endemic for a long time in West Africa. Then it marched across the continent, going eastwards, and causing all sorts of outbursts and explosions, and possibly increasing in virulence. Then it turned down southwards, and all that time that was not the *morsitans*, but the other species?—In the *palpalis* area.

538. Then suddenly it began to appear further south in Rhodesia, Nyasaland, and places of that kind, and it was thought for a time to be the same disease: am I right?—Yes.

539. It was at that time that you took up the work?—Yes.

540. One of the early discoveries you made was that it was not the same disease but a different disease?—That is so.

541. That would depend upon morphological evidence partly?—Yes, the distinction depends mainly upon morphological evidence.

542. A little more than that, because you have a different fly?—I think any tsetse-fly can transmit any trypanosome.

543. But in practice it does not?—I would not like to say that.

544. I thought a good many experiments had been made and that it was shown that in a laboratory you could get *palpalis* to work where it ought to be *morsitans*, and *vice versa*, but in practice it did not actually do so?—You mean that in practice *palpalis* does not transmit *rhodesiense*; that is your point.

545. Yes.—I would not like to be too definite about that.

546. (Mr. Rothschild.) There are some areas where the two flies occur together?—I say that I should like to examine these trypanosomes.

547. (Dr. Chalmers Mitchell.) You do not know that is so?—No.

548. There was a break; instead of dealing with one disease which we could call sleeping sickness you certainly came upon another disease which is also called sleeping sickness?—Yes.

549. Due to a different trypanosome?—Yes.

550. And generally at least conveyed by a different fly?—Yes.

551. I think Sir David Bruce was rather clear in insisting on the distinctness of the two diseases?—Yes.

552. Now you tell me in your evidence that there is no essential difference between the clinical mani-

festations of the disease in man caused by the two different things—that is to say between the Congo disease, the *rhodesiense* and the *gambiense*?—I think there is no essential difference.

553. So that although you have quite a different trypanosome it produces practically what appears to be the same disease?—Yes.

554. Is that generally admitted?—I think so.

555. Although they are due to different trypanosomes and transmitted by different flies?—Yes. Of course there are, as you know, a very large number of animal trypanosomes. The disease produced in any animal, take a rat, by any one of these trypanosomes or by the human trypanosomes is identical.

556. I am coming to that. So far I have got clear that in your opinion the two diseases are practically alike, clinically I mean?—Yes.

557. With a very important difference—that one is very slowly fatal generally, and the other very rapidly fatal?—Yes; the one, one cannot cure, and the other one can cure to a certain extent.

558. When you come to take *gambiense* and introduce that into animals—monkeys, for instance—there is a very close parallel between the clinical picture in the monkey and the clinical picture in man, is there not?—No, I think there is not at all a close parallel.

559. Will you tell me some of the differences? We are talking about *gambiense*. Perhaps I might remind you that people like Laveran and Mesnil give practically the same temperature curves, and so forth, but not in all animals, because the disease shows different symptoms in different animals; but on the whole am I right in saying that the *gambiense* disease, say, in man and the monkey, is very closely alike indeed in its clinical picture?—No, I do not think so at all, because the clinical picture of sleeping sickness in monkey is not characteristic at all, nor is it until the last stages in man when the man is dying. You cannot diagnose the disease until you see the trypanosomes, unless one has a patient practically *in articulo mortis*, in the last stages of coma.

560. You have seen the temperature curves which I have referred to?—Yes, but I do not believe in them at all.

561. You know, of course, that you get the mental symptoms to which you have referred in both?—Yes, in a certain number of monkeys. I have inoculated a large number of monkeys with *gambiense*, and they died long before they got to that stage. I have only once seen an authentic meningitis in a monkey, and that was after sub-dural inoculation.

562. You do not think there is anything like a close parallel?—Not the slightest; you may get a monkey which lives a very long time and develops this meningitis, but as a rule it does not.

563. Is this the case, that the *gambiense* disease is generally rather more prolonged in animals than the *rhodesiense* disease?—Yes.

564. There is that difference?—That is so, I think.

565. So that if the central nervous system symptoms came on late in the course of the disease you would be more likely to get them in the *gambiense* infections?—Yes.

566. Now you do not quite accept what certainly some people state, that there is a close parallel in the case of *gambiense* between monkeys and human beings?—No, my experience is limited to ordinary monkeys, *Cercopithecus callithrix* and *Macacus rhesus*.

567. When you come to *rhodesiense*, I have got from you that that disease in man is almost exactly identical clinically with the other disease except for its swifter course?—Yes.

568. When you come to that in animals, is it not the case that you always have much more fever?—In man do you mean?

569. No, in animals; that is not your experience?—No, I do not think one does; one has the same irregular fever dependent partly on the animal and partly on the particular strain.

570. Some people say that the fever is much more marked in animals with *rhodesiense*?—I say those people have not worked a sufficient number of animals.

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571. There is very often oedema of the head?—I reckon nothing of that; I have seen oedema of the head with all sorts of trypanosomes, and it is not characteristic.

572. Then there is keratitis?—I have seen that in other trypanosome diseases, and I do not think it is characteristic.

573. Then you do not accept the picture at all which I have been laying before you?—No, one cannot diagnose one disease from the other clinically.

574. But you can morphologically?—That is the only way in which it can be done.

575. If some people state that you could not diagnose the thing morphologically, but that you could very well clinically, you would say they were all wrong?—I would say they were wrong.

576. Sir David Bruce gave us an account of the kind of order of preference in which flies attack animals. This is the laboratory I am now upon. He said they preferred fowls, I think, birds anyhow, and that if they did not get birds, next they put up with mammals, and only in the absence of either of these would they take reptiles?—He was referring to *morsitans*.

577. He was referring to the *palpalis* of *gambiense*. Could you give any parallel factor for *morsitans* and *rhodesiense* to that—laboratory experiments only?—No, they feed readily on fowls and readily on mammals.

578. They feed on almost anything you give them?—My experience is limited to birds, hens, and small mammals.

579. They feed either on birds or hens and things of that kind and small mammals practically indifferently?—Yes. I think the blood of the hen does not appear to be so completely digested by the fly as the mammalian blood; one finds it in the intestines of the fly a very long time afterwards in the form of a more or less solid mass practically undigested. The entomologist attached to our Commission had the idea that this prevented the development of the pupae inside the fly, and I am more than inclined to agree with him.

580. You think that feeding them on hens was rather bad for them?—Yes, they did not do so well as when fed on goats or monkeys.

581. Only one last question: it is big game you are going for. What exactly do you mean by big game—elephants, of course?—Any antelope at all.

582. Rhinoceros, zebra, and that sort of animal, elands, and all the big antelopes?—Yes, all the antelopes.

583. Does that include the small duikers—the dik-diks?—Every antelope.

584. The tiny little dik-diks?—Yes.

585. Does that include things like the coneys?—I know nothing about those, whether they are capable of being infected or not.

586. Does that include the small carnivora?—No, because they are easily killed by trypanosomes; they die quickly from infection; if they were fed on to any considerable extent they would be killed naturally.

587. You do not eliminate animals like the elephant and rhinoceros?—No.

588. Are you sure that the elephant and rhinoceros harbour these things?—No.

589. Why do you include them?—Because they may.

590. Just to make a good job of it?—Yes.

591. But there is no evidence that the tsetse-fly could get into the rhinoceros or the elephant?—Trypanosomes have been found in elephants; Sir David Bruce says so.

592. In one case an elephant was examined?—Yes, we did not find those trypanosomes in elephants and rhinoceroses.

593. You looked for them?—Yes. I think that the tsetse-fly could bite an elephant very easily.

594. You spoke of 3,000 natives with their guns?—There are 3,000 natives in this particular area.

595. And the English sportsmen, and so forth, would shoot and drive away all the big game?—Yes.

596. Do you think they could also kill off all the tiny little dik-diks and all that sort of things?—I think they would in time; if you had 200 or 300 guns going off it would clear them away.

597. But with regard to the small things you would have to poison the water?—Yes.

598. You would have to burn the grass?—Yes.

599. And burn down the area?—No, if you begin to clear the bush you vitiate the experiment at once.

600. You do not think you could clear out the small game without poisoning the water and burning the grass?—The grass is burned in the ordinary course every year.

601. That would not vitiate the experiment?—No, that is done every year, but the fly still remains.

602. Why does it remain?—I do not know why it happens; it might go off to one side.

603. Would not the game do that, too—go off to one side?—Yes.

604. You would not get rid of the game by burning?—No, I would not burn at all, except in the ordinary way.

605. You have told me that you could not get rid of the small things by shooting?—I think you could.

606. (Mr. Rothschild.) Do you not think that poisoning the water would also vitiate your experiment, because we know that all flies drink water to a certain extent, and do you not think that a very large number of flies would be poisoned by drinking the poisoned water?—I was unaware that *morsitans* ever did drink water.

607. I think, more or less, all flies absorb water irregularly if not regularly?—I have no evidence on that point.

608. I have three other questions only to ask you. In one of your articles you say that you found one native who was under observation during a year in a tolerably good state of health, although he showed trypanosomes in his blood?—Yes.

609. Did you ever, on finding that out, make larger examinations of blood of apparently healthy individuals in order to see if that was an isolated case, or if there were others like it?—We examined very large numbers of natives in order to find these 20, or whatever it may be that we did find; very many hundreds of natives were examined, but we only found these 20 cases. Of the 20 cases, 19 died within the six months.

610. (Mr. Buxton.) You mean 20 cases infected?—Yes, I think it was about 20 cases; they all but one died within six months, this one lived on.

611. (Mr. Rothschild.) I will tell you why I ask this question. In the case of that one, when you examined him from time to time and he lived on, was the blood taken by you or your laboratory assistants?—Either Dr. Kinghorn or I made the diagnosis and we always did the examinations ourselves.

612. The reason I asked was that it has been suggested that if the blood was drawn by the man himself and handed to you, there might have been some transference from an animal?—No, we always took the blood ourselves.

613. You have told the Committee in answer to a question that after feeding on an infected animal the flies became infective in from 11 to 60 days afterwards?—Yes.

614. So that you practically have an average of 30 days?—No, it depends entirely on the temperature; in the Luangwa Valley the flies became infective in from 11 to 20 days, and the average for the Luangwa Valley is 13 or 14 days. Taking the Congo-Zambesi watershed over 4,500 feet high the average incubation period is very much longer; I should say in the winter months they do not become infective at all; but in the summer, in the hotter months, it is just warm enough for them to become infective, and very probably the incubation period is very much longer.

615. That being the case, we know that somewhat similar parasites, although not, of course, trypanosomes, are transmitted from parent to offspring among the ticks?—Yes.

616. I know that laboratory-bred flies have been examined and found immune from disease, but have you known or do you know of anyone who has examined these flies from 11 days onwards after hatching?—Yes, certainly; all our experiments were done with those flies bred in this way, and some of them were kept under observation for as much as three months.

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617. And they never became infected?—No.

618. You have stated that as far as you were aware there was no case of Nyasaland sleeping sickness, *rhodesiense* sleeping sickness, in human beings having been cured?—No.

619. Is there not the standard case of Sir Edward Grey's brother?—Yes, I should think probably Sir Edward Grey's brother was infected with *gambiense*.

620. But he was in Rhodesia at the time?—Yes, and he was also in the Congo.

621. (Dr. Martin.) Am I right in understanding that, in your opinion, Sir David Bruce, when he was studying nagana in 1898, was really dealing with the group of diseases termed nagana?—Undoubtedly I should say so.

622. And that the sample which he sent home in the dog which was examined by Sir John Bradford and Mr. Plimmer and which has henceforth been called *Trypanosoma brucei* was not the same organism as you have been studying?—No.

623. Which do you think is the one most responsible for what is called tsetse-fly disease in South Africa?—That is the one I have been studying—that is *rhodesiense*.

624. Do you happen to know what brand of trypanosome Taute inoculated himself with?—*Rhodesiense*; it is a trypanosome morphologically identical with *rhodesiense*.

625. It was one he isolated himself from stock, was it?—Yes, he got it from a dog naturally infected with it, and then he fed flies on the monkeys which were inoculated from game and then on himself, and he inoculated himself with the dog's strain.

626. You are satisfied that was *rhodesiense*, anyhow?—Yes.

627. You were out in Northern Rhodesia for three years, were you not?—No, 18 months.

628. Is it your opinion that this disease which you have been studying is seriously progressing there amongst men?—Yes, I say undoubtedly it is progressing.

629. Taking into account the fact that you were on the look-out, of course, it being your business there, you think that you would discover large numbers of cases as time went on which would never otherwise come to light? I am referring purely to man.—You mean in the Luangwa Valley, where we were?

630. Or any other part of Northern Rhodesia?—In the Luangwa Valley undoubtedly, because, in the first place, in 1897 there were no flies there at all shortly after the rinderpest swept through the country.

631. Simply taking it as a human epidemic, do you consider it is a serious menace there to the population?—I would not express any opinion on what is likely to happen at all.

632. (Mr. Austen.) I think you told the Committee you considered that the *Trypanosoma rhodesiense* is the trypanosome of domestic stock?—One of them, yes.

633. The one with which Bruce worked in Zululand?—It is the one he worked with in Uganda; I do not know what he worked with in Zululand. In those days the descriptions of trypanosomes were very vague. I have at Runcorn the trypanosome from the dog which Sir David Bruce sent home, and that is quite different and distinct from *rhodesiense*, but Sir David Bruce says that when he looks at the slides made out there the trypanosomes in Uganda and that are identical. The only assumption one can make is that there was more than one trypanosome.

634. Anyhow, the trypanosome with which you worked in the Luangwa Valley, and found in infected men, you considered to have been the same trypanosome as the trypanosome you found in domestic stock?—Yes.

635. I hope my next question is not an unfair one: Do you consider Dr. Taute a reliable witness?—I do.

636. His reputation stands high?—Yes, I say it does, undoubtedly.

637. Dr. Taute states that in the district in which he is now or has recently been working in Portuguese South-East Africa, although game is abundant and there are plenty of human beings and plenty of *Glossina*

morsitans, there are no cases of sleeping sickness; can you explain that?—No, he makes that statement, I know, but I think one would have to look a little more at it before one talked on those lines.

638. Although you consider he is reliable you think he may be mistaken about that?—Yes.

639. You would rather expect to find he was mistaken?—Undoubtedly; we have heard that before, that there was no sleeping sickness in Southern Rhodesia and North-Western Rhodesia until a European got it, and then they began to look and they found it.

640. I rather think Dr. Taute is speaking of a somewhat circumscribed area in his statement?—I suppose he is.

641. The sort of area in which he could make extensive examinations himself?—Yes.

642. I think you told the Committee just now, did you not, that you could diagnose the trypanosome capable of causing the *rhodesiense* disease from the morphological characteristics of that trypanosome?—I could distinguish it from certain of the other trypanosomes—yes.

643. As you are aware, Dr. Taute claims that, as the result of his recent experiments on himself, in which he got infected tsetse-flies to bite him and also injected infected blood, seeing that he did not get the disease, no one is entitled merely on morphological characteristics, on finding a trypanosome in the blood of an animal, to say, "That is the trypanosome which causes the *Trypanosoma rhodesiense* disease in man"; do you agree with that?—No, I do not; all Taute proved was that a single healthy human being is able to withstand infection from this particular trypanosome.

644. But he proved that in his particular case, and you do not admit that he proved any more?—Certainly not; if he had inoculated 100 human beings the result would have been very different.

645. That is only your opinion?—Yes, but I quote Lanfranchi's case.

646. In Italy?—Yes.

647. The trypanosome with which you have been working in the Luangwa Valley and the trypanosome with which Sir David Bruce has been working in Nyasaland are, I think, admitted to be the same?—Yes.

648. Sir David Bruce told the Committee the other day that he regarded the danger of human sleeping sickness spreading in Nyasaland as negligible; would you say the same with regard to the danger of human sleeping sickness spreading in the Luangwa Valley?—No, I should not certainly; nobody could possibly be certain; it is merely conjecture. I do not know and I do not think anybody else knows.

649. If it is negligible in Nyasaland but not negligible in the Luangwa Valley there is a serious discrepancy?—There is, certainly.

650. Anyway, that is your opinion?—I have no opinion on the subject; I do not know.

651. You would not say that the danger was negligible?—No.

652. Sir David Bruce said that it was as regards Nyasaland. I think you said it was increasing or had increased, did you not?—It has increased undoubtedly in the Luangwa Valley since we have records.

653. Are you aware that in saying that you are at issue with Dr. May, the Principal Medical Officer of Northern Rhodesia, who says that it is not increasing?—One must be guided by the facts; I am not responsible for Dr. May's opinion or Sir David Bruce's.

654. You know that Dr. May says it is not increasing?—The main road from Fort Jameson to North-Eastern Rhodesia used to run across the Luangwa Valley through Nawalia. Every week European officials and traders travelled from Fort Jameson, the main place of entry, into Northern Rhodesia across the valley and on to the plateau, and yet in spite of that there is not a single case of sleeping sickness on record until 1909, in spite of the fact that all these people went through.

655. How many years are you speaking of until 1909?—Up till 1909. Dr. Prentice says that between 1894 and 1897 he knew both the Domira Bay district

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and the Nawalia district, and there was no tsetse-fly there at all. That statement is supported by the statement of many other Government officials with whom I have spoken, District Residents stationed at Nawalia, and there is no doubt in my mind that there was very little fly in the Luangwa Valley until, say, 1906 or 1907. Therefore I am convinced that there is more trypanosome disease in that part of Africa now than there has ever been before since 1894.

656. I do not think that is quite what I wanted to elicit or quite the point Dr. May has alluded to; I think what Dr. May meant is, that since attention has been directed to human sleeping sickness within the last three or four years the disease has not increased but rather decreased. Do you agree to that?—I should like to have Dr. May's figures.

657. But you are not prepared to agree to that off hand?—No, I am not.

658. I take it you think in Northern Rhodesia human sleeping sickness is a new disease, in the sense that it did not occur where it now occurs 19 or 20 years ago?—It did not occur in the neighbourhood of Nawalia 19 or 20 years ago because there was no fly there. If you mean a new disease in that sense of the word—yes.

659. Do you think it is a new disease in every other sense? Do you think that the disease was never in Northern Rhodesia before 1894?—I do not know; we have no record.

660. Do you think it is a new disease in Southern Rhodesia?—I do not know; I cannot answer that question.

661. You have no evidence?—No; it was discovered for the first time last year. In other words, it is discovered when a European becomes infected. That has happened time and again. A European becomes infected, and then people look and find it.

662. You would admit that that is no proof that the disease did not exist there before?—Not the slightest; probably it did exist before.

663. (Professor Newstead.) I have a question I should like to ask Dr. Yorke, and that is, if he can tell me what is the nature of the vegetation which forms the borderland, so to speak, between the fly-infested area and the fly-free area in the region or district which he has referred to on a previous occasion?—The Sebungwe district?

664. Yes?—This is Dr. Stohr's report; he says, "You cannot differentiate; we have found it impossible to correlate the boundary of the fly area with any physical feature."

665. That is to say, there is no open country immediately outside the fly area?—That is so. "The characteristic feature of the country is that the bush is small or medium size without undergrowth; trees generally are thinly scattered."

666. Were those particular localities or pupal habitats which were found by you and your entomologist widely separated or did they occur within a short range of each other over a small area?—We found several within a few square yards; we found them in an old stump, and going another six yards we found them in another stump. There were several close together.

667. In such places about what would be the average number of flies present on you or your assistants?—They varied at different places.

668. To take it from another aspect in the particular district where the pupae were, did you find the flies swarming about as bees would swarm round a hive or had you just two or three on your backs?—Two or three; I think the most numerous were at the place where Dr. Livingstone died, and there I counted as many as 12 or 14 or 20 on the backs of the natives.

669. Were you stationary or moving?—We were trekking.

670. (Dr. Balfour.) Do you think the methods of blood examination you and Dr. Kinghorn carried out with blood smears and so on, amply sufficient to give you all the information required to enable you to form the opinion you have formed?—I do not quite grasp that question.

671. I might explain it in this way. You are aware of what is known as cryptic trypanosomiasis in animals?—Yes.

672. In European and American cattle trypanosomes have been found, but only after culture and not by the ordinary methods?—Yes.

673. Do you think it would be any advantage to carry out cultural methods, provided they could be carried out in places like Nyasaland and Rhodesia?—With regard to the human blood?

674. Not only that, but with regard to the blood of animals?—We need not so test in the case of human blood and animals infected with pathogenic trypanosomes; the best way of culture you have is to inoculate a susceptible animal, and that is what we did.

675. You think that was sufficient in examining the blood of small animals?—Undoubtedly the most satisfactory method of finding out was to inoculate a monkey or some other susceptible animal.

676. You do not think you would find further evidence from cultural work? You might get a monkey immune from inoculation by your trypanosome as Taute was immune; you might get evidence from cultivating the blood that trypanosomes were present in the circulation not appearing by ordinary methods but discoverable by culture?—I cannot think that is likely; all those cryptic trypanosomes are non-pathogenic as far as we know.

677. I gather you do not think there is any evidence of hereditary infection in the fly whether *palpalis* or *morsitans*?—No, I have seen no evidence.

678. Do you think it has been sufficiently looked for?—Yes, probably it has.

679. I suppose there is no evidence of sexual transmission with *Trypanosoma rhodesiense*? There may possibly have been cases, as Koch suggested, where *Trypanosoma gambiense* was transmitted by sexual intercourse; have you any evidence that such a mode of transmission occurs in the case of *Trypanosoma rhodesiense*?—No evidence at all.

680. (Sir William Leishman.) With regard to the Sebungwe fly-area, you have told us that you had no personal knowledge of its limits?—No.

681. From whom has that knowledge come?—From reports.

682. How long is it since it has been recognised as an isolated fly belt—how far back?—I do not know, really; I think probably a considerable number of years as an isolated fly belt.

683. Are those fly belts from your general knowledge of them fairly stable? Does the fly remain there pretty permanently within somewhat circumscribed limits, or do they tend to expand and migrate?—That is a question on which I have no personal experience, but one judges from what one hears, and they tend to expand. For instance, in North-Eastern Rhodesia there is not the slightest doubt that the amount of the fly has increased and spread enormously out of all proportion in the last 15 years.

684. So that it is conceivable that this isolated fly belt might in the middle of our experiment expand widely and cease to be an isolated belt?—I think that is hardly likely to happen in the course of four years; I hope not.

685. With regard to the very interesting experiments about the effect of temperature on the infectivity of your flies, did you at any time find a series of flies non-infective, say, on the Plateau?—Yes.

686. But subsequently you found that infectivity appeared in the same flies?—We did, practically. We used about 700 flies on the Plateau, and none of them became infective; but, of course, a number of them became infected, that is to say, trypanosomes got a hold in the intestine but they did not become infective until one put them into an incubator at about 85° Fahrenheit, and then within about 60 or 70 days, and over 80 days in one case, the flies became infective.

687. You think that is the limit you have established?—No.

688. You think it is possible they might remain with the possibility of further infectivity, if the temperature suited them, longer than 80 days?—As long as they lived, I think.

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[Continued.]

689. You are familiar with Captain Fry's and some others' work on "granule shedding"?—Yes.

690. Have you any personal opinions on that matter?—No, I have no opinion at all.

691. You said you thought in the fly there was a sexual method of multiplication. What makes you think that? Did you distinguish the male and female form?—No; what I meant really was that the trypanosomes undergo a very distinct metamorphosis. They become non-infective; they alter in shape; they undergo a metamorphosis which is not seen in mammalian blood. They become unrecognisable; you cannot distinguish one species from another by examining the morphology in the fly. They become non-infective for various periods. I do not say anything further. I do not say they split up into male and female forms.

692. I misunderstood you, then; I understood you to say there was a sexual process?—I was referring more to what some people call it.

693. That is not your personal view?—I do not know at all; they undergo an extraordinary metamorphosis, and that is as much as I would say.

694. (Mr. Buxton.) Might I ask you first a question or two about this proposed experiment in this area? It is a very large area. Do you really think you could, within a reasonable time, kill out all these species?—It is a question of money, of course, really.

695. Is it only a question of money? Is it not the case that when once wild animals have been shot at and feel they are in constant danger, they become exceedingly cunning, as, for instance, they only get their food in the night, and so on? Would not that be a very great difficulty after the first few weeks?—I think they would probably go.

696. Assuming they are driven out of this very large area, is not the homing instinct very strong in wild animals; will not they return?—They will not return if there is shooting going on. If shooting drives them out it will keep them away.

697. In an area of that size you would want a very large number of skilled riflemen to keep them clear, would you not?—I do not think it makes much difference whether they happen to be particularly good shots; the animals would be frightened and I think that would be the main thing. One would shoot a considerable number.

698. Assuming that this is the case, I only want to point out what struck me as practical difficulties. If you did for a time so frighten animals that they left, would they not tend to return as their fear subsided?—Then they must be met by fresh shooting.

699. I should like to ask you this: Do you hold all the big game (you use the expressions "big game" or "wild animals," which are rather wide expressions) equally guilty?—No, we find some more heavily infected than others—some antelope are more heavily infected than other antelope.

700. But you did not exclude any, in point of fact, if there was contamination?—No. Take elephant, it would be impossible to say.

701. Can you say from your own experience and knowledge that every species you might include in the title of big game is liable to have the trypanosome in its blood?—I cannot say; I do not know. We know a good many species can harbour it.

702. You say that if man is immune he would not be a reservoir?—No, he would not.

703. I am very anxious not to misrepresent you; do you wish to qualify that in any way?—I do not qualify that statement; if man is immune he cannot become infected with the trypanosomes. You mean can he be tolerant of the disease?

704. In the same sense as wild animals?—No, I say he is not likely to be a reservoir in the same sense as wild animals.

705. Assuming he may be infected with the trypanosome and yet not get the disease?—One has no ground for assuming any such thing.

706. How do you account for the fact that there are circumstances present which would seem to point to the probability of a tremendous outbreak of disease? You told us you have seen 14 or 15 of the fly feeding

on the back of a native, and I suppose you might have a fresh set of flies every five minutes or every two minutes. That points to a very large number of flies biting the natives?—Yes.

707. A proportion of those were assumed to be infected flies?—Yes.

708. Does not this widespread number of flies and the fairly numerous population point to the probability of something like the catastrophe in Uganda?—No, I do not think so; it may; I do not know at all. You mean whether or not in the future we will have an epidemic comparable to that in Uganda?

709. Is it not surprising that we have not already had such an epidemic? On your premises are not all these conditions for a great epidemic present?—So far as I know, yes; but what we do not know is, how resistant man is to the infection—that is, how difficult it is to infect him.

710. It does not surprise you that there has not been a great epidemic so far?—We do not know how long this condition of affairs has existed.

711. You say, in your opinion, the disease is a new one?—No; it is new in the sense that the fly has only comparatively recently become so numerous in these localities. As I tried to point out before, at Nawalia, in 1905, there was practically no fly; there was so little fly that the Government official was able to keep a large herd of cattle. At the present time the fly is so thick that there is not a single domestic animal alive in the valley between Fort Jameson and the mountains on the other side. All the domestic stock is dead. What will happen to man I do not know, and I do not think anybody else knows. That, I think, places the matter in a nutshell.

712. You do not think that a human being can be immune from the disease, or, if he is immune, that he can be a reservoir? I do not quite follow that.—I do not think that a human being can harbour the trypanosome for long periods of time without exhibiting signs of the disease.

713. In the same way that an antelope does?—That is so.

714. (Dr. Bagshawe.) I have only one point to put. You referred just now to Lanfranchi having infected himself in Italy, and you gave that as an instance of the infection of a man with nagana; are you sure it was nagana?—I do not know; I was quoting Castellani.

715. The reason I asked was that, as far as my recollection goes, Professor Mesnil, in commenting on the case, said he was not sure what it was; that it might have been *gambiense*, but he proposed to make experiments to find out what it was?—I simply quoted Castellani; I know nothing about it.

716. (Mr. Read.) I think that there is one thing which this Committee ought to have before it and that is the position with regard to sleeping sickness in each of these colonial possessions of ours. We have heard from Sir David Bruce that in five or six years 130 cases have been discovered in Nyasaland out of a total population of a million, and that only gives an insignificant mortality per annum of one per 50,000, or something of that kind?—Yes.

717. In North-Eastern Rhodesia, I believe that the danger spot is the Luangwa Valley?—Yes, that is one of them.

718. That is where the question is most serious?—Yes.

719. The population there is about 120,000, and making a liberal allowance for cases which have been overlooked and trebling those that have already been discovered, it appears that the mortality works out at 54 per 1,000?—Yes.

720. You would not call that a serious state of things?—At present, no.

721. I imagine that death from malaria is probably a hundred or a thousand times as great as that?—Yes.

722. Here is one point which is probably within your knowledge. You were working with Dr. Kinghorn?—Yes.

723. There is a statement made here in a letter from Dr. May. "In the same area" (the Luangwa proclaimed area) "a recent examination of 91 per cent.

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[Continued.]

" of the censused population by Dr. Kinghorn resulted in the discovery of two cases of the disease " ?—Yes.

724. " This identical district examined at periods varying from 18 to 24 months had previously resulted in the discovery of eight cases." Do you bear that out?—Yes; it is a trifling thing at present as far as man is concerned, but as far as domestic stock is concerned, it is very serious indeed. As far as man is concerned it is a more or less trifling thing, but I would not take the responsibility of saying that it is going to remain in the present state.

725. At any rate Dr. May, who was there a considerable time, evidently does not think the disease as a matter of fact has spread; in fact he states that it is diminishing?—Yes. Dr. May obtained his information from Kinghorn and myself.

726. You rule out the human being and domestic animals as possible reservoirs?—Yes, for the reasons I have given; of course they are possible reservoirs, but they are like a drop in the ocean.

727. I have here a published paper from which I would like to read an extract—the medical report of Nyasaland. " I think it will be found that natives travelling spread fly, and I also believe that fly has adapted itself to new conditions and learned to make use of man as its ordinary food where game used to supply its needs"; if you got rid of the game is there a possibility of the fly turning its attention to man? The writer of this report evidently thinks so?—I do not think it will turn its attention to man more than it does at the present time; if a man goes into the bush he is bitten as often as the fly can do so.

728. With regard to domestic animals, here is a paper from one of the West African medical doctors: " The tsetse-fly naturally follows its means of subsistence, and though with the game the fly may be driven away, it is possible that it may in numbers be forced into closer intimacy with domestic animals, which might thus in greater proportion become the natural reservoir at present afforded chiefly by wild animals. It is already known that cattle can, and do, serve as a reservoir of pathogenic trypanosomes in the same way as antelopes. Sheep and goats are also susceptible to most known trypanosomes, suffering but mildly and betraying but few symptoms"; that is evidently a man who does not think the same as you do, but the conditions may be different?—They are quite different.

729. There is just another point. I assume for the moment that you are able to kill off on a very big scale these larger game, but is there not some risk that in doing that you may upset in some way the balance of nature? We know that strange things have happened in the West Indies and Australia and other places. I am not a naturalist, and I daresay some other people will be able to suggest more ingenious questions, but I take it for granted that any given area in Africa can only maintain a certain number of game, and that if you kill out certain species the species left would tend to multiply; is that a fair assumption to make? Assuming for the moment that it is possible to kill off these antelope and the elephant and the rhinos, and so on, you are left then simply with the ground game and these animals intermediate between the ground and the big game like the duiker. I should think it was almost impossible from the little I have seen of Africa to kill off the duiker over any extended area. Is it not possible, having killed off the big game, that the duiker might increase tremendously—we know it is a most destructive animal—and might not this great increase of duiker seriously affect the native plantations, so that their last state might be worse than their first. I speak with some knowledge of the country round Nairobi, which is the capital of East Africa, and quite close to the official quarters there is grass 7 or

8 inches high, and the duiker abound there; they come into the gardens although you never see them in the ordinary way, and if they can flourish so near to human habitation, what must be the result in the vast tracts of country outside the inhabited places?—I think it would take a great many duiker to do as much damage as a very few elephant. I have seen two or three elephants destroy a whole range of native gardens in a night. What is to happen as a result of the experiment, I do not know.

730-1. Is an experiment carried out over a limited area any guide to what might happen in the case of game destruction over a whole protectorate? I gather from your evidence that you want to exterminate the game right and left?—I want them to shoot game in the neighbourhood of human habitations in fly areas to clear them away from the villages in fly areas.

732. Will not they come back again?—No, not if the natives are constantly shooting them.

733. You propose to arm the natives?—Yes, certainly.

734. That opens up a very serious question of policy. You come up against the General Act of the Brussels Conference there. In Nyasaland we have some 200 to 300 armed men to keep the peace over an area of about 40,000 square miles, and if you arm your natives you do not know what you are letting yourself in for. I assume that you intend to arm them with arms of precision?—Yes, of old patterns, certainly.

735. That is forbidden under the General Act of the Brussels Conference. All the signatories to the Act definitely undertook to keep arms of precision out of the hands of the natives, and very rightly too. You would have very much worse things than this sleeping sickness; you would have inter-tribal fighting and the whole administration put in danger.—Not only does the administration prevent the natives killing game by the use of firearms, but they refuse to let the natives harass the larger game in any way. They are not allowed to dig pits or to kill a large antelope at all. They are only allowed to kill a duiker.

736. Are you in favour of pushing the entomological side of the investigation?—Certainly.

737. Have you had any experience of the effect of clearing in the neighbourhood of villages in the way of reducing the fly?—We cleared out round our camps and we lived right in the centre of the fly area, and in that way we found comparatively few fly came in. When the natives came into the camp they brought a few with them.

738. That might be a practical suggestion?—Undoubtedly.

739. I read in a recent medical report from Nyasaland by the Principal Medical Officer that in the endemic area of the Dowa sub-district: " The clearings which have recently been made around villages in the area have been attended with the best results. Dr. Conran in his recent report stating that the cutting-down of bush and scrub, and the lopping of branches of overhanging trees, had produced a very appreciable diminution in the number of flies from the immediate neighbourhood of these villages " ?—Yes.

740. (Chairman.) One question only. We can gather from your evidence that you recommend, under some circumstances, a very large destruction of game. Is your recommendation grounded on the necessity of preventing the spread of disease in man or the spread of disease in cattle?—Both.

741. As regards man, looking to the probably endemic character of the disease in Nyasaland and Rhodesia, considering that it is of a practically stationary character, and comparing it with other diseases in that country, in your opinion is the measure which you advocate urgent?—Yes, I say it is.

The witness withdrew.

THIRD DAY.

Friday, 24th October 1913.

At 3, Queen Anne's Gate, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Dr. ANDREW BALFOUR, C.M.G.
Sir JOHN ROSE BRADFORD, K.C.M.G.
Mr. E. NORTH BUXTON.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Colonel Sir W. B. LEISHMAN, F.R.S.
Sir EDMUND G. LODER, Bart.

Dr. C. J. MARTIN, F.R.S.
Dr. P. CHALMERS MITCHELL, F.R.S.
Professor R. NEWSTEAD, F.R.S.
Mr. H. J. READ, C.M.G.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
The Most Hon. The MARQUESS OF WINCHESTER.
Mr. A. C. C. PARKINSON (*Secretary*).

Mr. LL. E. W. BEVAN, M.B.C.V.S., called in and examined.

742. (*Chairman*.) You are a bacteriologist with considerable African experience, I understand?—Yes.

743. You were good enough to write a letter upon the subject before us some time ago and it has been furnished to us for our assistance. You tell us a good deal there about which other people better qualified than I am will ask you. I have not many questions, but there are two or three things that I would like to have your opinion about. Do you know Nyasaland as well as Rhodesia?—No, only Southern Rhodesia.

744. From inquiries with regard to the condition of things there, and deductions made, do you think that the disease has probably been endemic there for some time?—I cannot say.

745. How does it compare in the case of man with regard to mortality with other tropical diseases having regard to present conditions?—From the reports of Dr. May and Dr. Fleming, I think the mortality is very low, as compared with that from sleeping sickness in other parts of the Continent.

746. In your experience, has it ever had anything of an epidemic character in that country?—No.

747. Have you ever considered what, if any, conditions might arise, or be likely to arise, which could produce an epidemic of the disease?—According to the findings of Kinghorn and Yorke, I think that all the conditions are there which are favourable to an epidemic, if their deductions are correct.

748. But there is no epidemic?—No, there is no epidemic.

749. And there has not been for, at any rate, a very long time?—Not as far as we know.

750. Would it be possible, or probable, that the majority of natives who have always lived in that country have become immune (that is one view) or tolerant of the trypanosomes. You know what I mean by that?—Yes.

751. Is it possible?—It is possible in the same way that native cattle have distinct resistance to local trypanosomes, but I do not think it is probable in this case.

752. Do I understand from that answer that there is more than one breed of cattle or that there are many breeds of cattle in which a large proportion are resistant to the trypanosome?—It is not quite that, my lord, but we find that cattle brought down from Central Africa, and to some extent our Mashona cattle, show far greater resistance to infection with local trypanosomes than cattle which have been "bred up" in the country.

753. Can you say from experiment on those cattle whether the trypanosome dies in the blood or whether it remains in the blood without causing a deleterious effect?—I do not think it dies. I think it is still there in a latent condition, just in the same way that piroplasms are present and can always be obtained.

754. Following up what I asked you before, would there be a special danger of epidemic conditions if the district were at any time either largely settled by Europeans, or if a large number of natives from elsewhere, who had not hitherto been exposed to trypanosomes, were brought into the country? Would that new population be likely to produce an epidemic while the existing population were practically safe? You see what I mean?—Yes, I quite understand the question, but it is so problematical that I would not like to express an opinion.

755. The reason why I asked the question was that I was thinking rather of Uganda at the moment, where, as I understand, there was an importation of disease into a population that had never suffered before?—I think that I can say that in all probability should the trypanosome be passed from one susceptible animal or person to another more susceptible than the first host, it would probably become exalted in virulence.

756. You are not acquainted, I understand, with the conditions in Uganda?—No; I have never been north of the Zambesi.

757. I will leave the point to people who are more competent to deal with it. Now with regard to Rhodesia and the proposal to fence, as I understand from your letter you are of opinion that fencing would never be reliable?—I do not think it would be.

758. In your judgment anything like a stampede of game, particularly big game, would break down almost any conceivable fence?—Yes, that is my experience.

759. If the game having broken in by a stampede from outside, were then hunted from inside they would stampede out and so make another hole in the fence?—Yes, it is possible.

760. Therefore the fence would be liable to constant breaking from animals coming in and from animals breaking out?—Yes.

(*Sir Edmund Loder*.) I do not believe in a fence at all from what I have seen. I have seen zebras go through.

761. (*Chairman*.) (*To the witness*.) Whether with or without a fence, if in a given area all game should be driven out, if you could have a sufficient number of hunters do you think that it would be sufficiently lasting and not merely effective for the moment, to justify the experiment?—No. I think that any organised shoot would succeed in driving the game out of a district for the time being, but that as soon as hostilities ceased they would return.

762. Would it be difficult to ensure that for any long period of time an area of any real size was altogether free from game?—Yes, I think that it would.

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[Continued.]

763. In any case, would it be difficult to drive out all the small antelopes and other small animals?—Yes, it would be very difficult.

764. Have you considered the question of clearing the bush, or anything of that kind, on a large-scale as affecting the fly?—We have had to consider it on several occasions in Southern Rhodesia.

765. With success?—It has been somewhat successful, but that is dealing with small areas.

766. Have you any personal knowledge as to whether the fly is found in large quantity where there are practically no large wild animals?—I cannot quite follow you, my lord.

767. Have you any practical knowledge from your own observation of the presence of the fly (*morsitans*) in large quantities where there are practically no wild mammals?—No, I have not.

768. (Mr. Read.) In the experiment to which you refer in your paper what kind of fence was employed?—A barbed wire fence with iron standards.

769. Not a fence with two supports placed at an angle like that (describing); simply a plain upright fence?—Yes, with standards.

770. Was it placed on the top of a bank?—It had to follow the country.

771. Was there a ditch as well?—Yes. There were streams and rivers and ant heaps.

772. (Sir John Bradford.) You said just now in answer to the chairman that certain cattle were more resistant to trypanosome infection than other cattle?—Yes.

773. What trypanosome were you working with?—The trypanosome which appears to belong to the *pecorum* type.

774. So that those remarks apply to that variety?—They were in reply to the observation about the resistance of animals.

775. Have you worked at all with the trypanosome of nagana, i.e., *T. brucei*?—Yes.

776. Have you any experience of cattle or of any particular breeds of cattle being more resistant to that?—No; I cannot express an opinion on that.

777. So that the remarks with reference to certain cattle being resistant apply only to the one species?—Yes, the common species of our country.

778. In the part of Rhodesia (I am ignorant about it myself, and I want to know) where you were working was there any nagana?—Nagana is a Zulu word which means "wasting," and it is applied haphazard to any form of trypanosomiasis.

779. Was there any nagana that was dependent on *T. brucei*?—I have never come across the monomorphic variety of *brucei* in Southern Rhodesia.

780. Do you take the view that *T. brucei* is a monomorphic trypanosome?—I cannot say that I do.

781. I do not want to go into hair-splitting differences as regards the different trypanosomes: I want to bring out clearly before the Committee, because it is a very important matter, whether this question of particular breeds of cattle being resistant to trypanosome infection applies to the particular trypanosome which we are dealing with in this Committee, whether we call it *T. brucei* or *T. rhodesiense*?—No, it does not.

782. As far as I understand your answer this question of resistance applies simply to *T. pecorum*, which is another species altogether?—Yes, I was answering the question put to me.

783. Quite so. I only wanted to make the point quite clear. You have not worked with *T. brucei* or *T. rhodesiense*?—Yes I have, but we do not find cattle infected in any large number with that species.

784. In Rhodesia, where you were working, the common infection and the most important infection of cattle was that produced by *T. pecorum*?—Yes.

785. Now putting that aside, in reference to wild game, have you made observations on the trypanosomes present in wild game?—Yes.

786. What trypanosome have you found in wild game?—I have found *pecorum*, and recently in the Sebungwe district, that is to say, the district where the most recent cases of human trypanosomiasis have been met with south of the Zambesi, I came across a

trypanosome which I cannot distinguish from the so-called *Trypanosoma rhodesiense*, or it may be *brucei*, I do not know which it is.

787. Now that being the case, do you look upon the trypanosomiasis in wild game as a serious danger to man and to cattle?—I am inclined to do so.

788. Do you think that the danger is more important as regards man or as regards cattle?—Well, in view of the fact that I met with a trypanosome in waterbuck, which I could not distinguish from that met with in man, I am inclined to look upon waterbuck and animals of that sort with grave suspicion.

789. From a practical point of view do you think that the danger of the game is a serious menace to man?—Yes, I do.

790. As regards diseases of cattle, is it a serious danger to cattle?—Yes.

791. What do you look upon as the most important reservoir of this pathogenic trypanosome?—Those species enumerated by Yorke and by Bruce—waterbuck, reedbuck, wart-hog, and bushbuck—I think they were.

792. Do you think it is at all feasible to eliminate wart-hogs?—Yes, I think that could be done.

793. But as regards the other wild animals which you mention, what do you think about them?—Waterbuck could be easily got rid of, I think. There would be more difficulty with some of the other species.

794. (Mr. Buxton.) Could you name any other?—Yes, bushbuck.

795. (Sir John Bradford.) What measures would you suggest should be taken in order to diminish the incidence of the disease in man and in cattle?—As we know no way of dealing with *G. morsitans*, I think we must direct our attention to doing away with potential reservoirs and, as far as possible, getting rid of the species at any rate that have been found infective.

796. Should I be right in assuming that what you suggest is that there should be a certain discriminating slaughter of game?—Yes.

797. You would schedule certain game?—I would bring those animals into the open ordinance, I think it has already been done, and allow them to be destroyed. In fact I think I would recommend that a reward be paid for sets of horns.

798. You look upon a certain kind of game destruction, then, as quite a suitable and proper measure to take?—Yes, I think so.

799. Is there any other measure that you can suggest should be taken?—I think that expert officers should be appointed to investigate continually the matter on the spot, to watch the suspected areas and to go into the whole question.

800. Yes, but I want to ascertain whether you have any suggestion that you can make, other than the destruction, to a greater or lesser degree, of the game in order to check the spread of these human and cattle diseases?—There are certain restrictions I should place on the movement of cattle from the north to the south of the Zambesi, but that is merely a local affair. Apart from that I do not think that there is any other.

801. Then, lastly, do you think that the scientific evidence is complete that the danger arises from the reservoir in wild game and the transmission of the virus by means of fly?—No, it is not complete.

802. In what way is it incomplete?—The critical experiment has never been performed.

803. You mean as regards man?—Yes, except by Tante.

804. I said human and cattle diseases?—I think that we are pretty safe in assuming that the trypanosome carried by buck is transmissible to cattle by means of *morsitans*.

805. Do you think that the scientific evidence is complete as regards cattle?—The experiment has been performed by means of the syringe and by artificial feeding. I do not think that we shall ever get closer than that.

806. (Dr. Bagshawe.) I should like to get your opinion on this map. This is a map of the Sebungwe fly area which we have on the authority of the Medical Director of Southern Rhodesia. We were told the

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[Continued.]

other day that the fly area was an isolated fly area separated from the nearest fly area by a distance of 70 miles, and that there would be no chance of an interchange of tsetse-flies between the two areas. This map seems to show that the Sebungwe fly area is separated from the Mafungabusi area by a distance of about 23 miles. Would you like to see it (*handing a map to the witness*)? Is that map probably correct as far as your local knowledge goes?—I think it is probable that this map was drawn up by Mr. Jack, and he is a very careful draughtsman and I should say that it is correct.

807. If it was said that there is a distance of 70 miles between the Sebungwe fly area and the other fly area would it be wrong?—Probably. I cannot say definitely, the areas are so shifting that I do not think you can say that there is that constant distance between them.

808. Judging from your knowledge of *morsitans*, is there likely to be an interchange of flies between the two areas on that map?—I think it is highly probable.

809. Here is a map which shows a much wider distance. Possibly it is an older map (*handing a map to the witness*)?—This area here (*pointing*) is not a very well known area. Very few people have been there. It is quite possible that this map was drawn up from the reports of policemen or native commissioners, and I should say that it is quite likely not to be correct.

810. Is there any date on it?—It is November 1912. It was early this year probably that the divisional map of Southern Rhodesia was compiled.

811. You think that the smaller map then is probably the more reliable?—I should not like to say because I have not been in the Mafungabusi district, but I should think that Dr. Fleming's information is the more recent of the two.

812. (*Mr. Buxton.*) With regard to your remark about water-buck, wart-hog, and bush-buck, I assume that you think that they are more important reservoirs than other species?—I think that more observers have found trypanosomes in those species than in others.

813. Is it the case that you would discriminate between the species?—Yes, I think I would.

814. And if there was no evidence, or no strong evidence with regard to any particular species, you would not advocate the extermination of them?—I should prefer to tackle first those particular species mentioned.

815. We have had before us a very general condemnation of all wild game. You would not agree with that?—I think that probably too drastic means would defeat the end in view.

816. You have not made investigations yourself which would enable you personally to classify the various species of game that are dangerous?—No, I have not.

817. You say that the conditions are favourable to an epidemic in those parts of Rhodesia which you know, but that nothing that we can call a bad epidemic has occurred. Can you think of any hypothesis which would account in your mind for that?—I think it is possible that some of the deductions that have been drawn are incorrect.

818. Would it commend itself to your mind that the native tribes who have been as long resident there as the wild animals might in the same way have become to a great extent immune?—I do not think so, for this reason: after rinderpest, the tsetse-fly disappeared or tended to disappear—it was not a total disappearance—but they were very much fewer in number, and during that time there would be less infection, and, therefore, in all probability, any degree of resistance which the natives had would tend to decline, so that at the present time they would have less resistance than they had formerly.

819. It is the case, is it not, that the wild animals harbour the trypanosome and yet show no signs of being any the worse for it?—Yes, that is so.

820. Is it not a conceivable hypothesis that the native races who have been indigenous there for a very long period have something in the same way acquired immunity?—Yes, it is quite conceivable.

821. (*Sir Stewart Stockman.*) You said that, in your opinion, the gravity of the situation was exaggerated. Did you refer to the disease in man or in animals, or in both?—Both, but more particularly in man.

822. With regard to organised shoots, is it your experience that where settlers come along and settle on farms and shoot, the game goes away?—Yes, I think I can say they do. There are some species of buck which seek protection—some of the smaller species. The large buck disappear.

823. (*Dr. Balfour.*) Is there any evidence to show that stomoxys or other biting fly apart from the tsetse can transmit animal trypanosomiasis in Southern Rhodesia?—No; I have abundance of evidence to the contrary.

824. Have you ever found cryptic trypanosomiasis in cattle in Southern Rhodesia?—Yes.

825. Have you looked for it by culture?—I have made cultures, but have never come across it at all.

826. Do you think that you might get true cryptic trypanosomiasis caused by a trypanosome usually regarded as pathogenic? In other words do you think that you might get infection with a pathogenic trypanosome which showed no sign of being present in the blood? Do you see what I mean?—Yes. It is rather difficult to answer. I have no experience of that.

827. You think it might possibly occur?—It is a possibility which should be considered.

828. Have you any idea as to the form in which trypanosomes exist in the blood in cryptic trypanosomiasis?—No. The life cycle of the trypanosome has not been worked out.

829. (*Professor Newstead.*) Have you examined the blood of wild animals shot outside fly areas?—Yes.

830. Have you found pathogenic trypanosomes in them?—No.

831. Or any trypanosomes?—No. I have found a large trypanosome resembling *T. theileri* in cattle but not in buck.

832. About what number of animals have you examined which had been shot outside the fly area?—I have had several hundreds sent to me at one time or another.

833. And not found trypanosomes in their blood?—No.

834. You have suggested, I think, that it would be very good to eliminate the smaller ruminants, the small buck from any given area. You have said that it would be quite a relatively easy task to drive out or to destroy the larger antelopes and other big game, but you have admitted that it would not be an easy matter or possible to drive out the smaller game. What method therefore would you adopt for the purpose of clearing such an area of the smaller animals—duiker and so on?—By constant shooting you might do it, and the natives might be encouraged to trap them and also to take part in the operations against them.

835. Would you recommend the poisoning of the forest pools in order to destroy game?—It all depends on whether there are domestic animals about or not.

836. Assuming that there were no domestic stock there, say, in a virgin forest away from all domestic animals, would you adopt such a drastic method as that?—Hardly, I think.

837. Assuming that you did poison the pools, could you say what effect such treatment would have on the birds, the avi-fauna of such a district?—No. That is the reason why I hesitate.

838. You think that possibly such a method might affect agricultural interests?—Yes, it might have all sorts of untoward effects.

839. I think you said that there was not a complete disappearance of the *Glossina morsitans* from areas which were cleared of all game during rinderpest. That is to say, that although the game was killed off by rinderpest, *Glossina morsitans* still survived in small numbers?—It did in some areas.

840. That was your experience?—I was not in the country in those days, but I have obtained a lot of information from white men who were, and from native chiefs.

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[Continued.]

841. Some men, I take it, said that the flies entirely disappeared?—Some people say that they entirely disappeared, and others that they only became fewer in numbers.

842. In your opinion then *morsitans* did not entirely disappear?—I can judge by one district, the Eiffel flats in the Hartley area. I think we can say that there the fly entirely disappeared.

843. (Mr. Austen.) You said in the letter addressed by you to Dr. Bagshawe that you consider that panic measures or anything of that nature, any extremely drastic measures, would have an unfavourable effect on the colony?—Yes.

844. May I take it that you mean by that, that it would cause people to believe that the danger of human trypanosomiasis spreading in Rhodesia was greater than it actually is?—That is one of the things I meant.

845. So that the wholesale slaughter of game you consider would have a bad effect on the development of the colony in Rhodesia?—Yes, I think it would.

846. Can you account for the disease in man not spreading in Rhodesia?—No, I cannot.

847. Do you admit that we have all the factors present, assuming that game is the reservoir? We have the reservoir, the disseminating agent and the population?—Yes, we have everything there, according to Kinghorn and Yorke, which should be conducive to an epidemic, but we do not get the epidemic.

848. You cannot account for that?—I cannot account for it.

849. Do you consider that the parasite which causes human trypanosomiasis in Rhodesia is the same as that which we have hitherto called *Trypanosoma brucei*, or is it distinct from it?—The question is a difficult one as to what is *brucei*. Recently, I had a strain that was called *brucei* sent up to me from Dr. Theiler's laboratory, and in it I found posterior nucleated trypanosomes, which were regarded as peculiar to *rhodesiense*, and if that is *brucei*, then I think we have in human beings the same trypanosome as that which has hitherto been regarded as *brucei*.

850. Assuming it to be the same as the trypanosome which has hitherto been regarded as *brucei*, can you account for the fact that in the old days when perhaps white men went more into fly belts than they do now, in the days of the pioneer and the elephant hunter, none of the well-known men of that time ever died of trypanosomiasis?—No, I cannot account for it. It would be a very curious thing.

851. You admit that it would be a curious and striking thing?—Yes. Hunters are very careless.

852. In answer to a previous member of the Committee who suggested that human beings in Rhodesia might possibly be reservoirs, you said that you did not think that could be the case; or rather he asked whether they had acquired immunity, and whether they were tolerant of the trypanosome. You said you did not think that could be the case, because the fly had died out to a large extent as the result of rinderpest?—Yes.

853. If tolerance in human beings existed, would it not be the result of evolution through ages of time or a very very long time, in comparison with which the temporary disappearance of *Glossina morsitans* was merely momentary, so to speak?—There is something in that, but I think that resistance takes longer to build up than it does to break down.

854. You think that resistance once acquired can very soon be broken down if it is not maintained by struggle with the parasite?—Yes.

855. Do you know the Sebungwe district personally?—No, I have not been there.

856. Have you ever found the breeding places of *Glossina morsitans*?—No, only a year or two ago Jack found them. He found them in the Lomagundi district.

857. You have not found them?—No.

858. You stated in answer to a question by a member of the Committee, that you consider certain species of game more dangerous than others from the point of

view of human trypanosomiasis?—I am accepting the statements of Bruce, Kinghorn, Yorke, and Duke.

859. You found your conclusions on those?—Yes, I have had to.

860. As regards Dr. Duke, you would accept his conclusions that the trypanosome that he found in the sitatunga was in fact the *Trypanosoma gambiense* in that case?—I cannot say that I have thought that point over.

861. It is a question of reservoir. He said that sitatunga was a reservoir, but of what?—I cannot deal with that because I have not thought it over.

862. You base your conclusion on the written statements of other investigators?—Yes, and my own finding of the trypanosome in the water-buck. I think I found it in five water-buck out of seven.

863. You would, therefore, I take it, disagree with Dr. Yorke when he proposes to destroy all the game?—I think that is theoretically sound, but I do not think it is practicable.

864. Is it merely from the point of view of practicability that you disagree with him, or as a matter of fact?—Well, if it is a matter of fact, I have to accept his statement; but I do not think that you could go into a district and kill off all these things. I do not think that you could possibly do it.

865. I want to know whether you consider that the game is equally culpable—not whether it can all be destroyed?—I am inclined to think that it is those species which are chiefly found about water that are the most dangerous or possibly the most dangerous.

866. Therefore you do not consider that they are all equally dangerous?—We have not sufficient evidence.

867. You think that we have not sufficient evidence?—Yes.

868. I think you pointed out once in a paper of yours that the majority of cases of human trypanosomiasis in Rhodesia have been in men rather than in women, although the women, since they work in the fields on plantations, are at any rate as much exposed to the bites of *Glossina morsitans* as men?—Yes, I did draw attention to that.

869. Can you account for that differentiation?—Yes, it has been explained to me since. The explanation was that men travel more than women.

870. So that really they are more exposed?—Yes. I think that that was the argument used against me.

871. I think you also mentioned that the majority of cases have been found along main roads?—Yes. Dr. May drew attention to that.

872. Is not that rather curious if game is the reservoir?—Yes, that is a point that I have always thought of.

873. It is not what you would expect to find?—No. The *morsitans* and buck are not confined to those routes.

874. Am I right in thinking that the existence of a main road would tend to scare away the game from an area?—I do not think so, because the main roads are not continually travelled and buck get very used to people.

875. You find *Glossina morsitans* and game in other places, but the majority of cases have been along the roads?—That is pointed out by Dr. May.

876. (Mr. Rothschild.) You said, in answer to the Chairman, that no fence that you knew of would keep out large herds of stampeding game; would not a large ditch, some 12 or 14 feet, or even 15 feet across, and of the same depth, keep them out?—It would cost a tremendous lot to prepare such a ditch.

877. To go to another question, have you ever in your own experiments tried to find out if the fly transmitted the infectivity to its offspring or not, or have not you dealt with laboratory experiments with the fly?—Yes, I have, and I have hatched out pupae; but such fly were not infective under the conditions of my laboratory.

878. But the point that I wanted to get at was this: In no account that I have read has it been stated that a distinct experiment has been tried for finding that out, because only flies directly they were hatched have been examined, and not those which had been

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kept from infection for a considerable time and then examined. In the experiments in transmission of the disease it has been found that the fly after infection only becomes infective itself after a certain number of days, so it might possibly be that the freshly-hatched fly in the laboratory required a certain number of days before it developed the infection transmitted from the mother fly?—I see your point. As a rule, in order to carry out experiments with clean flies, as they are called, the flies are bred out in the laboratory, and as far as I know there has never been positive infection which could be attributed to them.

879. You, at all events, have not examined the laboratory-bred flies after keeping them for a certain time; you have only examined them when they were hatched?—They have to be fed very soon or they will die; but none of the animals that have been fed upon in those early stages have ever become infected.

880. No; but have you examined flies, having fed your laboratory-fed flies on clean animals not already infected, having kept the laboratory-bred flies alive for 30 or 35 days? You have not examined the flies then or tried to infect an animal after that time?—I do not quite follow you.

881. Say that a fly is hatched to-day and you examine it microscopically, and you find that it is not infected, and you hatch some more flies and feed them on a non-infected animal, and they do not infect the animal, have you continued to keep these flies in your laboratory for 30 days and then tried to infect an animal with them?—I see what you mean now—no, I have not.

882. (Dr. Chalmers Mitchell.) You have worked a good deal on *Trypanosoma pecorum*, have you not?—Yes, or a trypanosome which comes under that group.

883. Taking it directly from wild animals?—Yes.

884. And identifying it morphologically by its shape under the microscope?—Yes, and by passage.

885. But can you be sure of it morphologically?—Yes, I think so. I can recognise the common trypanosome that we meet with.

886. As belonging to one of the trypanosomes of the *pecorum* group?—Yes.

887. Simply by making a preparation direct from the wild animal?—It is very rare that you find it in the wild animal, but if it is there, I think it is fairly characteristic.

888. Have you recognised it yourself in a wild animal?—Yes, I have seen it in a wart-hog, and I have seen it in a dog, but you cannot call that a wild animal. I have seen it in the waterbuck.

889. Have you seen it in domestic stock?—Yes, I have seen the same trypanosome, or apparently the same.

890. What do you mean by apparently the same?—Well, I am not too sure about these morphological distinctions.

891. You are not?—No, I do not believe in drawing the line too fine.

892. Have you found them in the fly itself?—No, I cannot say that I have.

893. Now take the *rhodesiense* or *brucei*; you do not quite distinguish between those, but you have worked that one of them?—Yes. Might I show you a picture which I happen to have here which has a bearing on the point you are raising?

894. Yes, I should like to see that. (The witness produced a picture to the Committee).—This is a smear from a dog.

895. Is this your own work?—Yes. Here you see all forms. This one is more or less typical according to the descriptions of *rhodesiense*. Here are two which might be regarded as typical of *pecorum*, but I cannot say where one type begins and another ends.

896. Then there is no such thing as a pure culture of these organisms?—I would not like to say that, but I must say that I am rather sceptical about it.

897. In your experience there is not?—I am inclined to think that it has been overdone.

898. Then how far would you extend that? You say that if you take the *pecorum* group you are quite sure that, so far, you cannot definitely distinguish between them purely morphologically?—I am rather

inclined to this opinion—it is not a final one; it is merely hypothesis—that in those animals which show a considerable degree of resistance, it is the *pecorum* or the short type of trypanosome that we generally meet with; but where animals are suffering from an acute form of the disease it is the long free flagellated type of the organism that is generally present. I have quite a number of experiments and cases now which have rather a bearing on that.

899. Do you mean that you think that the evidence is pointing in the direction of these alleged different groups being culture-phases, so to speak, of the same organism?—Yes, I think it is pointing in that direction.

900. When you get them in the fly they are practically indistinguishable at present, are they not?—The distinctive features are more masked.

901. Even although they are difficult to distinguish in other circumstances, they are very much more difficult in the fly?—Yes, I think so. I cannot say that I have studied them very widely, and I certainly have not studied them from my own preparations.

902. (Sir John Bradford.) Have they not a different distribution in the fly?—Yes, some people claim that they have.

903. (Dr. Chalmers Mitchell.) But so far as your own experience goes, you are inclined to think that the trypanosomes themselves, even taking two more or less separate groups like *brucei* and *pecorum*, are in effect the same organism under different conditions?—Well, you have drawn it out of me, sir. I should rather have not said that.

904. But that is what you think?—That has been at the back of my mind.

905. So I gathered; and in the fly they are even a little more difficult to distinguish?—I would rather say nothing about the fly stages, because I have not enough personal experience.

906. (Sir Mackenzie Chalmers.) I have only a few questions. How long have you been working in Rhodesia?—Nearly 10 years.

907. What has been the nature of your work?—I am a Government veterinary bacteriologist.

908. Your whole 10 years have been occupied in bacteriological work?—No. During the first three years I had field work to do, but I was also doing bacteriological work in my spare time.

909. What has been the general line of the experiments you have been making or the work you have been doing? I do not want to go into details. What problems did you set yourself?—The first problem that I had to solve officially was to determine whether Montgomery and Kinghorn were correct in their supposition that biting flies other than the tsetse-fly could transmit cattle disease in Southern Rhodesia. That led to my work in connection with trypanosomes.

910. You tried the other biting flies and acquitted them?—Yes, and not only that, but we had to base our opinion chiefly on the past experience of people all through the country. We came to the conclusion that no tsetse-fly, no trypanosomiasis.

911. You think that the other biting flies have been properly tried and properly acquitted?—Yes, I think so with regard to Southern Rhodesia.

912. Then we may disregard them?—Yes. I am only talking about Southern Rhodesia.

913. What was your next problem?—The next thing that happened was that Walter Armstrong, the first white man from Northern Rhodesia to be infected with trypanosomiasis, arrived in Hartley, and Dr. Mackenzie brought up the smear for me to examine, and I found a trypanosome in the blood.

914. *Rhodesiense* or what?—I am not going to give it that name.

915. But a trypanosome?—Yes, a trypanosome. I immediately performed experiments with it. I inoculated all the small laboratory animals that I had on hand, and sheep and mules, and was very much struck by the virulence of the organism and certain features in connection with it, which made me doubtful as to whether I was dealing with *gambiense*. I published those results in 1910.

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916. What is your opinion now?—It has worried me considerably ever since, and it has opened up a tremendous field of discussion as to what the trypanosome is.

917. Is there also a doubt in your mind as to what the resulting disease is? Would you agree that the Nyasaland type is quite distinct from the Uganda type?—No, I cannot express an opinion about that.

918. What further experimental work are you doing now, or what line of research are you on now?—Just before I came away on leave, I was involved in the Sebungwe outbreak, which was the first appearance of the disease south of the Zambesi.

919. In cattle or human beings?—It happened that a civil servant submitted himself for treatment at Bulawayo, and it was found that he was carrying trypanosomes.

920. Is he well or ill?—He is dead. They all die very quickly. The medical department organised an inquiry and went down into the Sebungwe district, and from there I received various strains of trypanosomes to work with in the laboratory.

921. With what object and what result?—The object was to determine what trypanosome it was that we had to deal with, and the result was that I found the same type of trypanosome in every species—in buck, in goat, in a native, and in water-buck.

922. You mean morphologically?—I mean morphologically, and the animal reactions were almost identical. The human strain seemed to be a little more virulent than the others, but I could not distinguish between them.

923. Having regard to the small number of cases in Rhodesia, do you think it possible that the source of infection is infection from man to man, of course *via* the fly?—One case points to it very closely, and that is that in one kraal the only two cases met with were those of a woman and her child. The child was carried on the woman's back.

924. Do you think that the child infected the mother or that the mother infected the child?—I think that it was one way or the other. Probably the mother infected the child. I think that I am right in saying that those were the only two cases in the kraal.

925. Therefore you think that it is possible that while you have not an epidemic in Rhodesia, the source of infection is from man to man, and not from the antelope or other animal to man?—This is merely theory, but I think that before man becomes infected the organism undergoes some passage which exalts it and renders it infective to man. While according to Kinghorn and Yorke everything is favourable to transmission to man, yet we only find a few cases. I think that it is more or less an accident that man becomes infected, and not the rule. What the conditions are which enable man to become infected I do not know.

926. I do not know whether you have yourself experimented in this respect, but a witness whose name I forget told us that as a laboratory experiment human blood, or human serum, is very destructive to the trypanosome. I am told that it was Sir David Bruce who said that. Have you experimented in regard to that at all?—Yes, I have found myself that it agglutinates or agglomerates very rapidly, and I have compared it with the blood of cattle. I think that the human serum is more active.

927. Therefore you would expect to find more resistance in human beings, speaking generally?—Yes, you would.

928. Do you think that there would be any difference between those who had for long inhabited the country and, say, a European? Do you think that one would have developed more resistance than the other?—I should say that, if anything, probably the native would have more resistance.

929. But no actual experiments have been made to determine that point?—No, I do not think so; I have certainly made none.

930. Now, with regard to the Sebungwe district, it was suggested that the fly area is an area of many square miles, and that around that there is, so to

say, a belt of non-fly area. Do you think that it would be in any way practicable to clear out the more dangerous big game from an area as big as that?—No, I do not.

931. Not the bigger game?—No, not in any rapid way; eventually you could, perhaps.

932. You could wear them down, could you not?—Yes, but it is a difficult district to deal with.

933. A big hunting party, according to that, would have no effect?—No.

934. But if once you wore them down, I suppose that a hunting party could keep them down?—I doubt it.

935. A hundred miles each way?—Yes, it is a tremendous area, it is not populated and it is not well known.

936. There are about 3,000 people there. You do not think it a practicable experiment?—No, I do not.

937. Is it possible to find any smaller fly area, an area isolated from other areas, where such an experiment could be tried, or is it not?—I do not think there is such a place in Southern Rhodesia. Perhaps the authorities in Northern Rhodesia might know of one.

938. At all events, you cannot suggest an area which is suitable for such an experiment?—No, I cannot.

939. When you talk about giving the natives leave to keep down, and encouraging them to keep down, the big game, that only applies, I take it, to fly areas?—Yes, that only applies to fly areas and to incriminated fly areas.

940. It only applies to areas where there is actual sleeping sickness among human beings?—Yes.

941. Does that cover a large part of Rhodesia or not?—No.

942. (Sir Edmund Loder.) When you were looking at the map you said that the fly area had shifted. Do you know of your own experience any fly area that has altered, and if you do will you tell us how much it has altered? What is the movement and how many miles do they alter?—I can take the instance that I gave before, the Eiffel Flats.

943. Is the movement considerable or inconsiderable?—Several miles.

944. So that if we cleared a place we might presently find that the fly moved in or moved out?—Yes, the fly is liable to travel.

945. And we should have all our trouble for nothing?—Quite so.

(Sir Edmund Loder.) That is rather serious.

946. (Chairman.) Where do they move to?—They shift about from belt to belt.

(Chairman.) Inside the area.

(Sir Edmund Loder.) Therefore a place which is said to be now 25 miles off from another fly area, next year may be only 3 miles off.

(Chairman.) I have not the map before me, but my impression is that the outside edge of the fly area is 25 miles from the edge of any other fly area.

(Sir Edmund Loder.) My point is that the fly area is not a definite thing.

(Chairman.) It is definite in the sense that the outside edge is a certain distance from any other fly area.

947. (Sir Edmund Loder.) There is another fly area, is there not?—The whole of that area is a potential fly area (pointing to the map).

948. (Chairman.) You expressed the opinion to Mr. Buxton that natives were not immune or tolerant to trypanosomes?—Yes.

949. Is it not the case that in the neighbourhood of the fly area the natives are constantly and perpetually bitten?—I cannot be definite on the point.

950. Would you not expect that unless they were immune or tolerant, there would be very much more sleeping sickness among men than there is at the present moment?—The same argument might have been applied to the natives in Uganda in connection with *gambianse*.

951. Do you mean before the disease was brought there or after?—They do not become tolerant unless

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the disease is there. It is by reason of the constant infection that they derive their resisting elements.

952. You mean that the same conditions exist in Uganda, but that notwithstanding that, at some particular period an epidemic did break out, and that the same conditions might arise in this district?—Yes. If you will allow me, my lord, I will draw a comparison with another disease. In the case of piroplasmiasis in cattle, the native cattle of Rhodesia are infected from the bite of the tick. A tick is a parasite which transmits the blood parasite. The cattle are bitten almost as soon as they are born, but very little harm is done to them. Now if after that they are bitten by ticks and re-infected eventually they become immune, that is to say, no harm is done to them; but if those cattle are put into a tick-free area and no ticks bite them, after some time their immunity breaks down, so that they are susceptible to disease.

(Chairman.) I see what you mean. Is it not a remarkable fact that there is no more sleeping sickness if natives are not as a rule resistant, or tolerant, or immune? However, I will not press the question.

953. (Mr. Buxton.) I want to ask you a practical question about enclosures, the efficacy of which you

seem to doubt. Is your experience with barbed wire as we know it here?—Yes.

954. Was it barbed wire which was broken through?—Yes.

955. Have you any experience of twisted wire rope, say seven or nine strands twisted?—No, but the difficulties in connection with a fence are the gullies and the sluices.

956. It was alleged that the fence was broken through by game?—It is not only a question of the strength of the strands but of the uprights and the standards.

957. That is a question of making them strong enough. You mentioned barbed wire?—The fences that I have had experience of have been barbed wire fences. I have had no experience of others.

958. You have had no experience of twisted wire fences?—No.

959. That is, of course, very much stronger?—Yes.

(Chairman.) Thank you very much.

The witness withdrew.

FOURTH DAY.

Tuesday, 28th October 1913.

At the Privy Council Office, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (Chairman).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Dr. ANDREW BALFOUR, C.M.G.
Sir JOHN ROSE BRADFORD, K.C.M.G.
Mr. E. NORTH BUXTON.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Colonel Sir W. B. LEISHMAN, F.R.S.
Sir EDMUND G. LODGE, Bart.
Dr. P. CHALMERS MITCHELL, F.R.S.
Mr. H. J. READ, C.M.G.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
Mr. A. C. C. PARKINSON (Secretary).

Lady BRUCE called and examined.

960. (Chairman.) You have been good enough to write us a letter as to the matters about which you can give us some assistance. Might I just quote from it as the Committee may not have it before them: "I think the only evidence I could give before the Committee would be concerning the identity of *Trypanosoma brucei* and *Trypanosoma rhodesiense* from the morphological point of view, and also the identity of the trypanosome as it occurs in the 'wild game and the wild fly.' I have very few questions to ask you on the subject, because it is rather technical. No doubt others can examine you better than I can. You have come to the conclusion, as I understand, that *T. brucei* and *rhodesiense* are in fact identical?—Certainly.

961-2. Am I right in supposing that you base that opinion on the morphology of the trypanosomes?—Yes, I do.

963. I will put this to you; it is a matter of speculation to some extent. Are you of opinion that with regard to distinguishing between one trypanosome and another morphology alone is a safe and reliable guide? It is an assistance, but can you say that you think it conclusive on the point?—No.

964. I gathered not. I am only going to ask you this question; are there within your knowledge any other trypanosomes that are indistinguishable morphologically from these two?—I do not know any.

965. Or this one, I will call it?—I do not know any except in the case of blood from a man. Take the two sleeping sicknesses; you cannot tell the difference in the man, but you can if it is put into animals. You understand?

966. I think I follow that. I do not know whether you have ever seen a paper by Dr. Blacklock and Dr. Warrington Yorke, published by the Royal Society?—Yes.

967. I will read the passage that led to my asking the question. They are dealing with dourine: "It is important to record that we are unable to distinguish morphologically the parasite of Hagenbeck's dourine horse from *T. rhodesiense*, *T. pecaudi*, or *T. uganda* (*T. brucei* of Uganda). Nevertheless, we hesitate to suggest that it is identical with any, or all of these, in view of the fact that it produced in a horse symptoms clinically known as dourine." The suggestion is that there are at least two other trypanosomes which are morphologically identical with *brucei* or *rhodesiense*?—They are ones that I have not come across. *Pecaudi* we look upon as the same as *brucei*.

968. I only wanted to get it clear. From a morphological point of view you draw no distinction between *rhodesiense* and *brucei* if I follow you rightly?—None at all.

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[Continued.]

969. You would not say that morphology is conclusive as to their being really identical?—No; not until you have tried them in animals. You must have more than one way of proving a thing.

970. (Sir Mackenzie Chalmers.) For how long have you been working in Africa at these trypanosomes, Lady Bruce?—Since 1894.

971. How many drawings have you done?—I do not know how many altogether, but during the last 18 months in Nyasaland about 30,000.

972. When you take trypanosomes of the same type, do you find much variation morphologically?—In the trypanosomes itself?

973. Yes.—Yes, enormous variation.

974. Approximating sometimes to other types?—Yes.

975. I did not quite understand an answer which you gave to Lord Desart. Taking the trypanosome from a man suffering from the Nyasaland form and the trypanosome from a man suffering from *gambiense*, can you distinguish between the trypanosomes or not?—I do not think so.

976. The trypanosomes of *rhodesiense* and *gambiense* in man are very much alike?—They are very much alike.

977. But the symptoms of the disease are very different?—Quite different.

978. Morphologically?—Morphologically from man they are very difficult to distinguish; it is almost impossible. There are very few in the blood in *gambiense*. You hardly ever find them. You only find one or two perhaps; and it is difficult to judge. You do not get sufficient numbers.

979. But in *rhodesiense* you get more?—Most certainly, but not very many, even then.

980. (Dr. Chalmers Mitchell.) Do you think that *gambiense* and *rhodesiense* are very difficult or impossible to distinguish in man?—Yes.

981. Then you said that in animals it is different. Do you mean by that morphologically different?—Yes.

982. If you put *gambiense* into an antelope and *rhodesiense* into an antelope will they be morphologically different?—I could not say. It is very difficult to work with antelope. You get very few. Take white rats for instance—that is the simplest.

983. When you put the things into a different culture medium, that is to say the rat, they come out different?—Yes; there are differences.

984. Morphologically, not physiologically?—Morphologically.

985. (Mr. Rothschild.) One witness said that he was under the impression that the true *Trypanosoma brucei*, as originally discovered in Zululand by

Sir David Bruce, was not the same as what had been identified as *brucei* in Nyasaland. Is that so or not?—I should say certainly not, but then I do not know what strain he has been working with. He may have been working with a strain that has been in England for 20 years. It may have got mixed up with something else, or anything may have happened. The trypanosome we worked with in Zululand is exactly the same as we work with in Nyasaland.

(Mr. Rothschild.) My question was merely founded on the statement that he made.

986-7. (Mr. Austen.) Have you studied the bionomics of *Glossina morsitans* at all in Nyasaland?—No. My work is only the morphology of the trypanosome.

988. (Sir William Leishman.) What method did you employ in the measurement of trypanosomes of which you have made such an enormous number of sketches? Did you draw them and measure the picture?—I drew them with the camera lucida and they were measured with very fine callipers.

989. Do you put great faith in the curves of average measurement as a means of distinguishing them?—Do you mean in being able to distinguish one from the other by the curves?

990. Yes.—No. They are all too different.

991. Do you remember an illustration in one of the earlier publications of the Sleeping Sickness Commission of the Royal Society of the chromatine granules found in the salivary glands? There were coloured plates?—I do not remember that.

992. You have done a good deal of work with the fly itself in Nyasaland?—Yes, examining the trypanosome in the fly.

993. Have you ever come across anything to suggest a further stage of development, different from ordinary fission?—No.

994. Another witness said, I think on Sir David's authority, that trypanosomes had been found in elephants?—Yes.

995. Where?—From some slides taken from an elephant when we were in Uganda and sent to us from Ankole, I think, a part of Uganda.

996. Was he able at that time to identify the trypanosome?—No.

997. Is that the only case that you know of of infection of an elephant with trypanosome?—Yes. We have had so few elephants; we are not allowed to shoot them.

998. I know. Have you heard of others?—They will not send the slides to us. The elephant hunters will not send slides and we cannot get them.

(Chairman.) Thank you very much, Lady Bruce.

The witness withdrew.

Sir DAVID BRUCE, C.B., F.R.S., A.M.S., recalled and further examined

999. (Chairman.) I do not know whether you have with you the summary of evidence which you were good enough to send to us before you came?—I have not.

1000. You will remember, no doubt, that on page 15 you go very fully into the question as to whether *brucei* and *rhodesiense* are identical, and put the pros and cons at considerable length. I should be right in saying, should I not, that the identity is probable, but it cannot be said to be conclusively proved by mere morphological examination. It is still in the region of hypothesis as to whether they are identical, if you only look at the question morphologically, is it not? They may be apparently similar and yet really different trypanosomes? I gather from this paper that that is your conclusion. I do not want to go into it at any length.—I do not think it is fully proved that the two trypanosomes are identical. I do not think it is absolutely proved; it is so difficult to prove a thing of that sort absolutely. It is quite possible we may be dealing with two different species. It is possible we have two species intermingled, and we are treating them as if they were one.

1001. We quite appreciate that. It stands that there is very considerable probability, or more than probability, that they are the same, but it cannot be said to be established in fact. That is about how it stands in regard to existing knowledge, is it not?—We have tried the two on various animals and find that they act in exactly the same way. We have examined them morphologically and found them exactly identical. We have tried their reaction with human serum and found they agree. The only link in the chain that is wanting is whether one will act upon man, and the other not, and we cannot try that experiment.

1002. I referred just now to a paper, which probably you know, by Dr. Blacklock and Dr. Warrington Yorke, in which they say incidentally that they are "unable to distinguish morphologically the parasite of Hagenbeck's dourine horse from *T. rhodesiense*, *T. pecaudi*, or *T. ugande* (*T. brucei* of Uganda)." From that I infer that not only may these two trypanosomes (or this trypanosome) be identical and yet different, but there are other trypanosomes that are clearly different which are still morphologically indistinguishable?—It mentions *pecaudi*.

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[Continued.]

1003. *T. rhodesiense*, *T. pecaudi*, and *T. brucei*. It is dealing with the parasite of Hagenbeck's dourine *T. equinum*?—I wrote a paper some years ago showing that *pecaudi* and *brucei* were identical. I think *pecaudi* and *brucei* are the same. In regard to dourine, that occurs in the north of Africa and it is out of my country. I have always had a strong feeling that *equinum*, *brucei* vel *rhodesiense*, *gambiense*, and *evansi* are all very closely related, and as I said before, I consider that *gambiense* is a descendant of *brucei*. These questions are very difficult.

1004. Yes, they are very difficult.—You find people fighting all their lives in regard to whether two particular birds are two species or only varieties of the same species. They often go on fighting all their lives, as it were, and never come to any conclusion. Now it is very much the same with trypanosomes. It is very very difficult to distinguish between some of them.

1005. Although in fact they are different?—I do not think that *ugandae*, *pecaudi*, *brucei* and *rhodesiense* are different. I think that *gambiense* is a different species from *brucei*.

1006. I only want to understand what the position is. There is no controversy between us. It is a question of how the thing stands. Since we were here last I have had an opportunity of reading, which I had not done before, about Dr. Taute's experiment on himself and the details of it. You are, no doubt, quite familiar with it. It is in the Tropical Diseases Bulletin. Probably you remember his conclusions, which are substantially that *brucei* and *rhodesiense* are different, and that *brucei* produces the cattle disease and *rhodesiense* produces the disease in man. If his theory is correct—assuming for a moment that it is—will not that square exactly with the conditions we find, that where there is the fly and there are wild animals cattle cannot live (I am talking of Nyasaland sleeping sickness), but man suffers hardly at all. Whether it is right or wrong it would fit that theory quite well and tally exactly with the conditions we find, would it not? I do not know if you follow me?—I think that you might repeat your question, my Lord.

1007. Assuming that the theory of Dr. Taute is the correct theory with regard to *morsitans* and the wild game country, it would fit in with the existing facts, namely, that in such a country cattle cannot live and that man suffers but slightly?—Yes, I think that is a statement of fact.

1008. That would be one theory, namely, that the *brucei* infected the cattle and came from the wild mammal, but the *rhodesiense* which infected man did not come from the wild mammal. The other theory is, that assuming that they are the same it might be the case that man can only acquire the disease by a strain that has passed through man, and not by a strain that has passed through cattle. That is another possible hypothesis, is it not?—Yes.

1009. Now in regard to your suggested experiment, I want to ask you one or two questions. Have you formed an opinion yourself as to a suitable place for it?—We proposed to enclose the Domira Bay road, as that will probably be a very important road in a few years, and if we render that road fly-free and safe for cattle transport, it will be useful.

1010. Would that exclude game from a sufficient area to afford a guide as regards the desirability of the destruction of wild game on a larger scale?—Well, we thought so—or I thought so.

1011. But I rather understood that your actual proposal in the first instance was not to fence at all, but to have a much cheaper experiment by driving cattle out by hunters, or otherwise getting them out in some way?—No. My experiment was always bound up with fencing, but the fencing was to be done with the ordinary material found on the spot.

1012. Now I want to ask you what may be quite an unnecessary question, and it is this: whether you have to drive and redrive game out of an unenclosed area, or whether you have to maintain the fence of an enclosed area, does not that involve the presence of a considerable number of men to watch the fence and see that it is

not broken down, and so on?—I think that in my summary of evidence I put a couple of men to every quarter of a mile of fence, do I not?

1013. Yes. Do you think it would be a sufficient number of men to vitiate or neutralise the experiment in any way?—I do not think it would be.

1014. There is one other thing that occurred to me as to that on thinking it over. Supposing that you do carry out this experiment, what are the possibilities of success? We know very little about the migration of *morsitans*. If you get out all the game, and assuming that the fly has gone with it, as far as your observation is concerned is there a danger of infected fly from elsewhere coming in seeking for food or staying there, having come from another area, and might it not vitiate your experiments if that happened on anything like a large scale?—I suppose that all these things are possible.

1015. I do not ask you to give an answer if you feel that you cannot. Can you form an idea of the effect of the fly constantly shifting from place to place? You might get rid of one lot of flies and get another, and so get infection from another source. Is not that a possible danger that might affect the experiment?—I can make a guess at it, but it is the kind of thing that I cannot answer.

1016. (Dr. Balfour.) Have you seen a recent paper by Dr. Darling, of Ancón, in the Canal Zone, Panama, on a method of immunisation for *murrina* the local trypanosome disease of mules and horses—the strain being weakened by passage through guinea-pigs? He carried out a very interesting experiment. Have you any evidence of any similar benefit that might be got from a similar therapeutic measure in the case of the animal trypanosomiasis of Africa? Have you any experience with regard to this?—No practical experience.

1017. I do not say with the guinea-pig, but some other animal possibly?—It is not in my line. I have always been trying to get rid of things at the bed-rock and not taking them halfway up the line, as it were. In regard to Malta fever, we took it at the bed-rock and stopped it, and you may say that we got Uganda sleeping sickness at the bed-rock and stopped it. I think the bed-rock with regard to a *morsitans* country is the wild game. I would not spend time on therapeutic measures.

1018. You would not waste time upon them?—It is not waste of time to a man whose business it is. Treatment is not only one man's work but that of many men. They spend the whole of their lives at it. If I went in for treatment I would go at it like others do, for example, Ehrlich.

1019. But have you any experience at all upon the point?—No, I have no experience.

1020. (Sir Mackenzie Chalmers.) What is the length of the Domira Bay road?—Do you mean across the fly country?

1021. Yes.—About 20 miles.

1022. Beyond the 20 miles the fly country ceases?—Yes, the fly country ceases. The road ascends to the plateau or begins to ascend.

1023. What causes the cessation of the fly; is it a question of height?—No, I do not think it is a question of height, because in North-East Rhodesia the fly goes up to a height of 4,500 feet on the higher plateaux. I think it is a question of the presence of particular vegetation and perhaps the presence of game. The game are more numerous down in the low country than up in the uplands.

1024. The fly follow the game, you think, and it is not a question so much of physical characteristics or vegetation?—I think that it has a great deal to do with the physical characteristics. I think it depends on the vegetation of the place. That is one of the first things. With regard to Kasu, where we are about 3,500 feet above the level of the sea, it is said that two generations ago the place was all covered with forest or scrub or jungle, and there were many buffalo and wild animals there, and it is said that at that time the fly was found at Kasu, but now that a large native population has come there and has cut down all the trees for firewood, &c., the game and the fly have

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both disappeared. We never see any game at Kasu because there are too many natives.

1025. The probability is that the particular vegetation attracts the game and the game attracts the fly. Is that the probable sequence, do you think?—I think that the fly, like any other animal, has its own particular habitat. It likes a particular kind of country. *G. palpalis* likes to be alongside clear open water. *G. morsitans* likes to be in a scrubby country away from water. There must be a sufficient amount of shady trees (thorn trees as a rule) to enable the fly to keep out of the direct rays of the sun. If you have a large clearing or plain in the middle of a fly country you do not find fly in the middle of the plain. Of course one of them may follow you into the middle of the plain or sit on your back, but you do not, as a rule, find fly in the middle of the plain when you go there. All kinds of animals and insects have their own particular habitat, but exactly why it is, is very difficult for man to find out. In regard to *morsitans* I would say that it depends upon a certain scrubby country usually formed of mimosa thorn trees and thickets. For example, you do not find *morsitans* along the shore of Lake Nyasa. I showed you that on the map. Along the lake shore there is what is called a lala palm country and there is any amount of grass there. You do not find flies there. It is not their habitat.

1026. As regards the Domira Bay road you suggest clearing the game from five miles on each side?—Yes, that was the first proposition.

1027. Having once cleared it of game you would patrol those five miles, I suppose. You would patrol along the 20 miles continually to keep the area clear, I suppose?—Yes. I think if you made a big effort at clearing the game out of the area at first, and then saw that your fences were kept intact, you would not require to patrol the inside of it very much.

1028. Would a small body of men be sufficient for the patrol?—Native boys would go into it every day to collect flies, in order to find out, in the first place, if they are there, and in the second place, if they are there to find out if they are infective.

1029. That would be sufficient, you think, to keep the area clear when once you had got it clear?—I suppose there would be several people employed to go through the area and see that there were no game in it, but I have never laid down any rules with regard to this. I have only made the suggestion. I look upon the carrying of it out as a matter for the Administration and not for me.

1030. As a matter of curiosity, how do your boys catch the flies, with nets?—Yes; with little nets about eight inches in diameter.

1031. A sort of butterfly net?—Yes, with a short handle.

1032. And they put them in collecting boxes?—Yes.

1033. (Dr. Chalmers Mitchell.) In the historical part of your summary of evidence you say, referring to *Glossina morsitans*, that there were no domestic animals in the Zululand fly country, but the trypanosome was found in the wild animals, and your inference is that wild animals act as a reservoir?—Yes.

1034. You say, "Thus was established the fact that the wild game act as a reservoir of the virus of the trypanosome diseases of man and the domestic animals." The inclusion of man in this statement here depends on the identity of *rhodesiense* and *brucei*, does it not?—That must be a slip of the pen. I am afraid, because there was no knowledge of man having a trypanosome disease at that time.

1035. That is what I wanted to get at. You would cut out "man." The word has got in by accident?—Yes.

1036. You cut out "man" on page 3 of this summary?—Yes. We did not know anything about a trypanosome disease in man at that time.

1037. (Sir Stewart Stockman.) I believe you have followed some of the various operations which have been undertaken to eradicate diseases of animals, and I would like to ask you whether you know if it is a fact that some of the most redoubtable diseases of animals have been eradicated by methods based upon epizootiological

studies alone, although the causal organisms of the disease were not even known. Take pleuro-pneumonia, for instance?—Yes, and horse-sickness would come in.

1038. Horse sickness is not eradicated yet? I ask you, do you know of diseases which have been eradicated on epizootiological principles alone?—It is more a question for a veterinary officer to answer.

1039. Quite so. I only want to know if you know whether what I say is a fact?—No, I do not think that I have any personal knowledge on these points. I might, if I thought for some time, think of something, but I have never come in contact, practically, with pleuro-pneumonia. I only know what I have seen in books, and that will not enable me to answer.

1040. (Mr. Austen.) Have you personally studied the breeding of *Glossina morsitans* in Nyasaland under natural conditions?—No.

1041. I am thinking of what you mentioned to, I think, Dr. Chapple on the last occasion when you described the breeding of *Glossina morsitans*, and said that a fly found itself incommoded by an inconveniently large pupa and repaired to the nearest tree and dropped it. Was that not actual observation on your part but assumption?—Yes. I have put flies in captivity under different conditions. I have put them in boxes with nothing but dry dust. I have put them in boxes with buffalo dung. I have put them in boxes with dry leaves. I have put them in boxes with half a dozen other different things, and I have found that the larva has been laid, turned into a puparium and hatched out, and in the same time, however I varied the surroundings; and, therefore, I thought I might indulge in a little imaginative writing or speaking, as it were, in regard to the habit of the wild fly, but that is all.

1042. I am speaking of what happens in a state of nature. I may take it as regards that that your reply was imaginative?—Yes. I have never seen a fly lay its larva in nature and I have never found a puparium in nature.

1043. I want to get the exact state of our knowledge of the breeding of *Glossina morsitans* in nature. I may take it that we do not know anything about it, or very little?—It all depends on how much you can take your knowledge of what happens under artificial conditions and say that these things will most probably happen in nature.

1044. That is not quite the same thing?—If we had never had a captive *morsitans* and had never seen a captive *morsitans* lay its larva and never seen the larva turn into a puparium, and never seen the puparium turn into the fly, then we should not know anything about what happens in nature as you say. In that case you would be quite right, but when we have seen these things thousands and thousands of times I think we are quite justified in saying that we are very strongly of opinion that such and such a thing occurs in nature. You want me to say that we do not know anything at all about it, but I do think that we know something.

1045. No, by no means do I want you to say that we do not know anything at all.—For instance, if I had you in a cage and studied you for six months and then somebody asked me to describe the habits of a savage in the middle of the Sahara desert, I could tell him a good deal about what the savage in the middle of the Sahara desert does.

1046. I grant that, but that does not help us very much, does it? I know, and you will agree, that *Glossina morsitans* will deposit its larva in a glass-topped pill box?—Certainly.

1047. But that does not instruct us particularly with regard to the deposition of larvae in a state of nature, does it. Do you admit that?—I think that it throws a good deal of light on the pupation of the tsetse-fly. If it will lay the larva in a glass-topped pill box and if the larva will turn into the puparium in two hours, and if the puparium will turn into a fly in five or six weeks, it teaches us a good deal about the pupation of the tsetse-fly.

1048. Am I right in saying that the majority of the larvae deposited under such conditions are immature—that they are smaller than normal if they are deposited under abnormal conditions?—No, I do not

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think so. Some of them are, but as a rule they are not. A large percentage of them are quite healthy looking and bring forth healthy flies. There certainly are cases of abortion among flies just as you find it among other animals.

1049. If I remember rightly, a number of the pupae sent home when Dr. Nabarro was with you in Uganda were certainly undersized judging from what could be seen in spirit, and that I think shows that there was some abnormal condition acting on the fly. We have no evidence that flies abort in a state of nature?—I suppose that just in the same way that you judge in other cases. From what happens under artificial conditions you would be able to assert with a fair amount of probability that wild flies do abort. If they did not get sufficient food for a little while they might abort. If, for instance, a bird made a dash at a fly and tore half a wing off or something of the sort and gave it a great fright, I do not know that it would not abort. Certainly, under artificial circumstances they do abort sometimes.

1050. Apart from trypanosomiasis, can you give me any instance of a serious protozoal disease of lower animals which is communicable to man?—You want an instance of a protozoal disease of animals and man other than trypanosome disease.

1051. Yes, a serious disease which is communicable to man?—Malaria is not an animal disease. It is a human disease. Piroplasma is not communicated from animals to man.

1052. (Sir William Leishman.) Kala-azar is probably communicable.—Sir William Leishman's statement that kala-azar is communicable from lower animals to man is probably true, but there is not absolute proof of this yet.

1053. (Mr. Austen.) I want to know whether there is any other instance. You cannot think of one?—I can only think of kala-azar.

1054. That is only a possibility; that is not proved?—That is not absolutely proved.

1055. (Sir Edmund Loder.) A witness said he thought that it would be very difficult to keep up a fence. He thought that the game rushing in and out would make it very difficult. Do you think that clearing the ground on each side of the road for a distance would be any use? It would keep animals back, but would it keep flies back?—It would not keep animals back.

1056. There would not be many animals on a clear flat plain, would there?—Why not?

1057. If they were wood animals they would not like living in the open. They would surely go into the wood?—I do not think so. The hartebeeste, the wildebeeste, the zebra, the reed-buck and a lot of these animals live out a good deal in the open plain.

1058. Do you get the fly in the open plain?—No, you do not get the fly in the open plain, but the hartebeeste, for example, come out of the wood in the evening into the big open plains to feed and drink, and in the morning at sunrise they go back into the woods, where the fly is waiting for them.

1059. In the daytime there would not be any fly in the cleared parts?—No. I have said again and again that you do not find fly in the open plain.

1060. It might make the road safe for cattle if sufficient ground were cleared on each side of it?—I think that is quite possible. It has been tried and proved again and again.

1061. (Sir William Leishman.) Your colleague and co-worker, Major Harvey, is just back from Africa, and you have seen him I understand. It would interest us if you could tell us whether he has brought you any recent reports in continuation of your work, and in confirmation or otherwise of what you told us before.—I do not think there is anything very new or very striking. They managed to transmit, as I understood him to say, a trypanosome disease, *rhodesiense*, or *brucei* as I call it, by *Glossina brevipalpis*. That is probably a new fact. I sent Major Hamerton, who had got rather seedy, away for a change of air 100 miles down on the Upper Shire, and he found the fly there very highly infected with the same trypanosome as we

found in the low country near us. I heard that Dr. Morgan, who has been in the district north of the fly district, has found several cases of sleeping sickness in his district, and Dr. Sanderson in the Upper Shire district, where Major Hamerton was, has also found several cases among natives.

1062. (Chairman.) Is this *palpalis* country or *morsitans* country?—*Morsitans* country. This to my mind is a proof that if you go into a game country and a *morsitans* country where there are numerous natives, not an uninhabited country, and examine the natives sufficiently thoroughly, you will find here and there cases of Nyasaland sleeping sickness among men. It was thought that as the Domira Bay road, running to the lake, had been used for a very long time as a sort of high road through Nyasaland into North-Eastern Rhodesia, and on to Lake Tanganyika, cases of sleeping sickness might have been carried along that road, and that therefore the cases occurring along the Domira Bay road in the sleeping sickness area of Nyasaland might have been imported. I said at the beginning of last year and at the beginning of this year to the Nyasaland Government, that I would like them to make a very thorough examination of the country north and south of the proclaimed area, as I was sure that the conditions there were the same as the conditions in the proclaimed area, and that in all probability we would find cases of sleeping sickness there. It is very difficult to find cases of disease among natives who are all fighting against you, and it takes time for medical men put on to the work to gain the confidence of the natives and get the organisation into proper working order. Last year no cases were found, but this year several cases were found in the northern district and several were found in the southern district. This is an argument in favour of my theory that wherever you find wild game with *T. brucei* in their blood and *Glossina morsitans*, you will find some cases of Nyasaland sleeping sickness among men if you only look hard enough.

1063. Did Major Harvey give you any information about the occurrence of human cases in the region where Dr. Taute had been working? Dr. Taute stated that he found no cases, I understand?—No; Major Harvey did not make any statement in regard to Dr. Taute's district; but that Dr. Taute could not find cases there is quite natural. It takes some time to know the district and to get the confidence of the natives, as I said before. You require to have trained natives in every village constantly on the look-out, and taking specimens of the blood of every person who appears ill. Such a man gets to know the villagers. If, for instance, Dr. Taute went into a native village his coming would be known for some hours before, and all the sick would be taken out and hidden in the bush.

1064. (Mr. Buxton.) One question about the Domira Bay road. That, I understand, is where you have suggested that the experiment of an enclosure should be made?—Yes; that was my suggestion to the Government.

1065. It was suggested just now that the position would be met by a clearance on either side of the road. You suggest enclosure and the exclusion of the animals?—Yes, without any clearing at first. The experiment is to find out the effect on the fly of the exclusion of the wild animals. If the first experiment failed we could then get the natives to clear the whole of the enclosed space, and in that way we could make the road clear of fly, I think.

1066. You would have a distinct belt enclosed on either side of the road?—Yes.

1067. (Dr. Bagshawe.) I want to ask you about one point. I do not know that it is of very great importance, and it leads to something rather speculative. You found in Nyasaland two trypanosomes, *simia* and *pecorum*, which were very fatal to monkeys under ordinary conditions. Is that not so?—Yes, especially *simia*.

1068. But if either of these trypanosomes were put through a goat it would be found very difficult to infect monkeys?—Yes; the passage through the goat destroys

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[Continued.]

the virulence of that particular species of trypanosome for the monkey.

1069. I want to know whether it is possible that some such hypothesis might account for the rarity of human infection with *rhodesiense* in Nyasaland, whether with regard to human infection it must go through some particular species of animal or perhaps through man himself?—Well, you know the facts as well as we do, so that you must frame your own hypothesis according to your own heart's desire, because I cannot help you.

1070. It is very hypothetical, of course, but a great deal of what we have to consider is hypothetical?—Yes. I have no facts before me. It must remain pure theory. You see you cannot experiment on man. If we could put *rhodesiense* through different animals and then put it into man and find out whether its going through certain animals made it more virulent to man, that would be one thing, but when you are debarred from experimenting upon man you come up against a brick wall. That is all.

1071. (Sir John Bradford.) I have two or three questions, Sir David, in reference to *Trypanosoma rhodesiense* and *Trypanosoma brucei*. Would you mind if I tell the Committee the history of the trypanosome that was sent home and was called *Trypanosoma brucei*? Just tell us shortly.—After the trypanosome was discovered in Zululand in the blood of cattle and the blood of wild game it was thought that it would be useful if this new kind of parasite was sent home to the laboratories in England in order that it might be worked at under more favourable laboratory conditions than we had in Zululand. I wrote to the Governor, Sir Walter Hely Hutchinson, asking if animals could be forwarded to England for this purpose, and he wrote back saying, certainly. I then sent by a native policeman two dogs inoculated with nagana down the country and they reached Government House in Pietermaritzburg, and I think their blood was there reinoculated into other dogs which were sent on board one of the home-going steamers. Dr. Wagstaffe took charge of them. He took several healthy dogs and the two inoculated ones, and during the voyage he had to inoculate the healthy dogs from infected ones in order to allow infected dogs to reach England alive. They did reach the Royal Society and they were handed over to Messrs. Kanthack, Blandford, and Durham. They worked with them at Cambridge, I think, and they wrote papers on the morphology of the trypanosome and on the action of the trypanosome on experimental animals.

1072. They reached the Brown Institution, or one dog did?—I believe so.

1073. Was any other material sent by you?—No other animal was sent to England in that way. When I was in Zululand, Danyz and Bordet, from the Pasteur Institute, were working at rinderpest and horse-sickness near Pretoria, and they sent a man on a horse to ride over country to find me. He went through the country very much as the Saracen maid did when she came to England looking for her runaway knight, just saying, "Bruce, Bruce," and at last he arrived at me. He had passed through a certain amount of fly country. His horse was a good one, and I had a horse which was not so valuable but a good healthy horse, and as he had run out of money I said, "If you give me your horse I will give you mine and 5*l*." That was in order to do a charitable action, as it were, in a graceful way. We inoculated two dogs and he went off leading them, but had not got very far when I saw a suspicious swelling on the belly of the horse he had left with me, and we examined its blood and found that it had nagana. I sent my interpreter after the man as hard as he could go. He found him at the end of the first day 30 or 40 miles away and they exchanged horses again. When he got to the borders of Natal and the Transvaal the Transvaal people would not allow the two dogs to cross the border and they were left behind, but they did not recognise that the horse he was riding on was a nagana horse. He was allowed to ride on, the two dogs being destroyed, so that he got through to Danyz and Bordet with a horse infected with nagana. I do not know, as a fact, but I believe Danyz and

Bordet or one or other of them took animals infected from that horse to the Pasteur Institute.

1074. Those are the only two infected animals as far as you know that have been sent?—Yes, I think so.

1075. I wanted to know how they reached Europe; that is all. In your early description of the trypanosome of nagana did you describe it as monomorphic or a dimorphic form or did you recognise that there were more than one form present in the blood?—At that time, as it was quite a new subject and a vocabulary as it were had not been invented for it, we did not go as fully and thoroughly into these points as we ought to have done, but in my report on nagana I think you will find that we say that there are various shapes and forms of the trypanosomes—that some are long and slender and others are shorter and stumpy.

1076. So that it would not be quite correct to rest a distinction between *T. rhodesiense* and the trypanosome that you sent home on the question of one being dimorphic and the other being monomorphic?—No. I think that the trypanosome of nagana that I worked with in Zululand was dimorphic. You can see that in the photographs which were made at the time. Then in regard to the trypanosome in the dog brought home by Dr. Wagstaffe and studied by Kanthack and his two co-workers: they distinctly state that the trypanosome they were dealing with was dimorphic. It was recognised that there were long and slender forms and stumpy forms with practically no free flagellum. To my mind it is exactly the description that we give now-a-days to these same trypanosomes.

1077. You think that the *Trypanosoma rhodesiense* is identical with *Trypanosoma brucei*?—That is my opinion.

1078. Do you base that opinion entirely on morphological grounds or have you any experimental grounds to support it?—I formed my opinion on the following grounds: The fly country of Zululand is not very far south of the fly country of Nyasaland, and there is to a great extent a continuous belt of fly country lying between these two fly belts. The wild game of Zululand is very much the same as the wild game of Nyasaland, the same reedbuck and steinbuck, duiker, and so on, and it is the same fly or a very closely related fly. When we got to Nyasaland, and I could not find nagana, which I thought must be in the wild animals there, as it was in the wild animals in the closely related fly belt in Zululand, I thought that as there was no nagana in Nyasaland, *rhodesiense* must in reality be nagana. That was the first argument. We then examined *T. brucei* and *T. rhodesiense* morphologically very closely, and found that they were absolutely the same morphologically. That was the second argument. The third argument was that we put the Zululand form, that is *T. brucei*, which we got straight from Zululand, through all the different animals we could, and compared them with animals infected by the local Nyasaland trypanosome, and we found that the symptoms in the animals and the length of the disease were identical. For instance, if a rabbit has nagana, it has very peculiar symptoms.

1079. And that applies to *rhodesiense* also?—Yes.

1080. A rabbit inoculated with *rhodesiense* has the same peculiarities?—Yes, the same symptoms.

1081. So that your opinion of the identity of the two was based on something more than morphology?—Yes—on the action on animals and the continuity of the fly belt.

1082. One witness here made the suggestion that the different varieties or the different asserted varieties of trypanosomes present in the blood of wild animals were really stages or different forms of one and the same trypanosome. Does that view appeal to you?—I do not think it is at all possible.

1083. Have you, in the course of your work in Nyasaland, come across instances of one and the same animal being infected by two or more distinct trypanosomes at the same time?—I think we found an animal infected with four trypanosomes in one case.

1084. (Dr. Chalmers Mitchell.) In the same fly, do you mean?

1085. (Sir John Bradford.) Have you had instances of a fly being infected with more than one variety?—

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[Continued.]

We did not work with one fly as a rule. We have a cage with 100 or 200 flies in it, and from the cageful we would get several trypanosome diseases, but whether one fly gave two at the same time I do not know—I do not see why it should not.

1086. I have been thinking of some results in Uganda. Perhaps you would rather not answer, but were there not instances in Uganda of certain flies containing more than one variety of trypanosome?—I am speaking from memory. I cannot remember, but I am quite certain that it often occurs.

1087. (Chairman.) If one fly or any number of flies can have different trypanosomes, supposing a man is bitten by a fly that has the trypanosome of *simia*, *pecorum*, *rhodesiense*, or whatever you like (and we know that *pecorum* and *simia* do not affect man at all), are they immediately destroyed in the blood or do they remain in the blood and not affect it? Assuming that the *rhodesiense* remains and the other two disappear are they killed at once? Do they live at all or are they killed by the blood the moment they are injected into it by the fly? I want to know whether a trypanosome that does not affect man has any existence at all in the blood or is it killed at once?—I should think it is killed within a few hours. If one *simia* was put into a man it would not divide, it would not multiply, it would not find the conditions for multiplication and therefore it would be lost, and I think that after some hours it would probably degenerate and die. But when *gambiense* is put in it at once begins to divide into 2, 4, 8, 16, 32, and so on, so that in 3, 4, 5, or 6 days you find a certain number in the blood; but the one *simia* would not reproduce its kind and it would be lost, and in my opinion it would die within a few hours, dissolve, and disappear.

1088. (Sir John Bradford.) There is a question which I forgot to ask you. In Uganda you saw a great number of cases of sleeping sickness, did you not?—Yes, a great number.

1089. Can you tell us whether any of them followed an acute course at all similar to what has been described in the case of Nyasaland?—No, I never saw one that followed an acute course.

1090. I mean an acute course as regards duration lasting only a few months. Was there not a case of an officer in the Royal Army Medical Corps who was attacked?—Yes.

1091. Was not that case acute?—He came home and he died after three or four months, but it looked as if his death was rather an accident. He had epileptic symptoms.

1092. I am familiar with that case. I want to know whether there were any number of cases of that kind as far as you knew in Uganda at the time you were there?—No.

1093. You heard of no others?—No.

1094. (Chairman.) How long do they generally last?—It is very difficult to tell because it is so difficult to tell how long the natives have been ill, but I put it down as an average of about two years. A Frenchman who examined a number of cases of Congo sleeping sickness, and re-examined them at the end of six years says that 17 per cent. of the natives were still alive.

1095. How long did the epidemic last in Uganda?—It began about 1898, and it was there in 1909. It lasted about 11 years.

1096. (Sir John Bradford.) A European who was sent home from Uganda lived between three and four years?—Yes, between three and four years. I gave some instances in my summary of evidence with regard to this.

1097. (Dr. Balfour.) With regard to the various species of trypanosome taken up by the tsetse fly, I suppose it would only be the human species that would pass through the cycle to fit it for reintroduction into the host again. The others would not pass through a similar cycle, would they? They would die out in the fly, would they not; or if they passed through a cycle they would not pass into man again, would they?—It is only the *T. rhodesiense* as it has been called. I call it the trypanosome causing disease in man in Nyasaland. It is rather a long term, but you do not fasten yourself down to any particular specific name if you say that. Then there are *simia*, *pecorum*, and *caprae*. Take these four. They all pass through a cycle in the *Glossina morsitans*, and they are all capable at the end of so many days of re-infecting animals which are susceptible to the disease.

1098. Might all the forms be injected into man?—The whole four must be injected into natives living in the fly country because the flies are heavily infected with all these four.

1099. That is what I want to know.—One point that comes up is this: that if a fly was injecting two kinds of trypanosomes at the same time the presence of one of the trypanosomes might have an effect in making the other trypanosomes, which usually did not affect the animal, infect it to a certain extent. I think that has happened; I cannot remember exactly where, but it is laid down in the papers. We thought we had a little evidence that if you infected an animal with two trypanosomes, to one of which the animal was susceptible, and to the other of which the animal was non-susceptible, the second trypanosome would, under the circumstances, take a certain hold and you would be able to find it in the blood afterwards—that is to say, find it in the blood of an insusceptible animal, the animal having been made susceptible to the trypanosome by the presence and the action of another, the blood having changed to a certain extent.

(Chairman.) We are very much obliged to you, Sir David.

The witness withdrew.

FIFTH DAY.

Friday, 31st October 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
 Dr. A. G. BAGSHAW.
 Dr. ANDREW BALFOUR, C.M.G.
 Sir JOHN ROSE BRADFORD, K.C.M.G.
 Mr. E. NORTH BUXTON.
 Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
 Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODER, Bart.
 Dr. C. J. MARTIN, F.R.S.
 Dr. P. CHALMERS MITCHELL, F.R.S.
 Professor R. NEWSTEAD, F.R.S.
 Mr. H. J. READ, C.M.G.
 The Hon. L. WALTER ROTHSCHILD, F.R.S.
 Sir STEWART STOCKMAN.
 Mr. A. C. C. PARKINSON (*Secretary*).

Mr. H. L. DUKE, M.B., B.C., D.T.M. & H. (Camb.), called and examined.

1100. (*Chairman*.) You have been acting as medical officer in Uganda, I understand?—Yes.

1101. Have you any experience outside Uganda? Have you experience of *morsitans* as well as *palpalis*?—No.

1102. Then your evidence would be limited to Uganda and *palpalis*?—And other species of tsetse not *morsitans*.

1103. With regard to *palpalis* do you consider that *palpalis* is dependent for its food upon warm blooded mammals, or that it can find food and does feed as readily on reptiles, birds, and other things?—It certainly feeds readily on reptiles, but I have no experience of birds.

1104. To put it quite roughly should you say it was as much or nearly as much dependent upon and addicted to reptile blood as the blood of mammals?—I would say it depended on the food supply of the district which any particular tsetse-fly was taken from. If there was a large excess of reptiles on certain islands it would probably feed exclusively on reptiles, but if it got the opportunity it would probably take mammalian blood in preference to reptilian.

1105. Is it right to say that the presence of *palpalis* would not depend on the presence of wild game and that it would equally be present if it had reptile or avian blood?—Yes; I should say so certainly.

1106. We have been told that the wild mammal is a reservoir of trypanosomes which could be conveyed by the fly to man or domestic cattle. Could you from your own observation say whether reptiles would provide a similar reservoir?—I have no experience to justify my saying they would.

1107. Do you know one way or the other?—No, nothing I have ever done would lead me to suggest that reptiles could act as a reservoir in nature.

1108. Have you as regards mammals?—Certainly, yes.

1109. I put practically the same question about man as a reservoir?—I say there is no question about his acting as a reservoir, but not such an efficient one as certain types of mammals.

1110. I wanted to ask you just one or two questions about the epidemic in Uganda. Were you there at that time?—No.

1111. But you have heard a great deal about it, I have no doubt?—Yes.

1112. How did that epidemic, in your judgment, arise? I understand the population prior to that epidemic had not suffered from sleeping sickness?—As far as I know that is so.

1113. What is your theory as to the cause of that epidemic?—I think probably *ex* infected porters. I have always understood that; that is to say, porters coming in from a district where sleeping sickness was endemic.

1114. If I follow you aright that means that the source of the epidemic was man?—The conveyor of the epidemic was man, not necessarily the source.

1115. The original source—whence he got it—I was not going back to; but in Uganda, where the population had not got it before, the source was man—the immediate source I will call it if you like?—I think so.

1116. That, if I understand aright, was met, and the epidemic was stayed by the removal of the population to a certain distance from water where *palpalis* would not go?—Yes.

1117. Within what period was there a diminution or cessation of epidemic conditions after removal of the population?—Do you mean as regards new cases?

1118. Yes.—It so much depends on the skill of diagnosing the new cases. I should say probably directly the natives were removed there was no case of re-infection. Among those natives no longer exposed to *palpalis* there is no possibility of any case of re-infection.

1119. Is there still some sleeping sickness among that population?—There is no population on the lake shore now.

1120. But in the population that has been removed from the lake?—There are, certainly, people with sleeping sickness.

1121. But they may have been infected some time before?—Yes, there are cases of new infection which one hears of now and again, but I think it is very often possible to prove that those people have been in contact with *palpalis* somewhere or other.

1122. The reason of that would be, I suppose, that although there may be men who still, in the presence of fly, would be capable of being a source of infection, there is no fly where they live to convey it?—No; I think the new cases have been near fly.

1123. There must be a certain amount of traffic which brings, at any rate, individuals of the population into proximity with fly which are near the water?—That would account for the new cases, to my mind.

1124. Have you yourself considered the suggested experiment that wild mammals should be fenced off or rather driven out of a particular area with a view to testing whether this would get rid of the fly or make the fly non-infective? Have you considered the possibility of an experiment of that character?—Not in Uganda.

1125. Have you considered it anywhere else?—Yes, I have read about it.

1126. You have considerable knowledge of the habits of wild game?—Yes, to a certain extent.

1127. Do you think it is a practicable thing?—I think it would depend enormously on the district chosen, the natural limits to migration, and that sort of thing, and also on whether or not you can guarantee to exterminate every living animal in that area.

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[Continued.]

1128. You say you have no experience of the habits of *morsitans*?—That is so.

1129. But as regards *palpalis*, if I understood the answers you gave me to my first questions, the absence of wild game would not at all involve the deprivation of *palpalis* of food on which they could live and thrive, or necessarily be followed by the absence of *palpalis*; the *palpalis* could subsist and remain without the wild mammals?—I think certainly on the lake shore, and I think elsewhere too.

1130. The *palpalis* is always in the neighbourhood of water, is it not?—Yes, but not necessarily with a big supply of reptiles such as it has on a big lake.

1131. (Mr. Read.) *Morsitans* does occur in the northern part of the Protectorate?—I believe I am right in saying they do in the Western Province.

1132. I seem to have seen a recent dispatch from the Governor in which he refers to a great number of *morsitans* on the road from Masindi to Butiaba?—Unfortunately I have never been there, but I have been in a place where *morsitans* is supposed to occur. I gather from Dr. Hodges that the evidence on which *morsitans* was diagnosed was quite inadequate and I never saw it. I covered the same ground. I saw *pallidipes* in plenty but never *morsitans*.

1133. Do you consider that any scheme of game extermination is feasible in Uganda where so much elephant grass exists?—No.

1134. (Sir John Bradford.) You worked for two years as a member of the Royal Society's Commission on Sleeping Sickness in Uganda?—Yes.

1135. What part of the inquiry were you specially concerned with?—Chiefly the role played by the antelope in the spread of trypanosomiasis both human and of domestic animals.

1136. You made a considerable number of observations on the presence of trypanosomes in wild game, did you not?—Yes.

1137. Have you formed any conclusions as to what trypanosomes are commonly present in wild game?—Most commonly?

1138. Commonly?—Yes, I have.

1139. Would you tell the Committee?—I think the commonest trypanosome in wild game in Uganda is *Trypanosoma uniforme*, and then possibly *viææ*. *Pecorum* is also common in certain parts, and also on the lake shore a trypanosome which, as far as one can diagnose by laboratory methods, seems to be the human *Trypanosoma gambiense*.

1140. In what wild game did you specially find *gambiense*?—Only in situtunga antelope.

1141. Was that strain obtained from the situtunga inoculated into antelopes at the laboratory?—No, there did not seem to be any occasion to do so.

1142. What strain was it that was in the antelopes?—Sir David Bruce called it a human strain.

1143. Do you know the history of that strain?—No, I do not; I think it came from sleeping sickness cases at a particular camp.

1144. Do you know whether the strain was injected directly into antelope from a human being or whether it had passed through other laboratory animals first?—I believe it was direct as far as I know, but the only information I have got is that it was an undoubted human strain of trypanosome put into the antelope in the first case. I believe it came straight from the blood of the patient.

1145. Those antelopes were in the laboratory when you first arrived, were they not?—Yes.

1146. Are they still alive?—They were alive when I left, and as far as I know they will be when I get back, most of them.

1147. What period has elapsed now?—It is, I think three years, if not more, since they were first infected.

1148. Is the trypanosome still recoverable from blood of these animals (I am talking of when you were last in Uganda)?—I last recovered it 22 months after the first infection, and since/then no exhaustive experiments have been done on them, but I am to do some when I get back again.

1149. And the antelope, I understand, remain in good health?—Yes, they were breeding quite frequently in captivity and in excellent health.

1150. Have you yourself found the *Trypanosoma gambiense* in the blood of wild animals?—Yes.

1151. May I ask how you formed the opinion that you were dealing with *Trypanosoma gambiense*?—For several reasons, one of which was that when this antelope trypanosome was exposed to laboratory-bred *palpalis*, in other words, when a monkey infected with this trypanosome was fed upon by clean laboratory-bred flies, the flies became infected, and after dissection they showed the trypanosomes in the salivary glands and the gut exactly in the same way as we know *Trypanosoma gambiense* behaves in the fly. That I consider quite an important test. Then the sub-inoculations into rats and other small laboratory animals corresponded roughly with the presence of *gambiense*; and thirdly, the trypanosome was resistant to the action of human serum.

1152. With the present methods available, do you think it possible to form a definite opinion as to whether the blood of a given wild animal contains *Trypanosoma gambiense*?—I think matters have come to such a pitch that the only final test of *gambiense* is definite knowledge as to whether or not a human being is susceptible to that particular trypanosome.

1153. Have you any doubt in your own mind as regards your own work, for example, that the blood of wild animals contains *Trypanosoma gambiense*?—Not the slightest.

1154. So that you do believe that you can identify a certain trypanosome to which the name *gambiense* has been given?—I believe that this trypanosome I obtained from these antelopes on the islands of the lake is the same trypanosome that came from the natives and that caused the epidemic, but there is not the slightest doubt it has altered enormously during its sojourn in wild animals and its absence from natives.

1155. That is not quite what I want to bring out. We have had some evidence that it is difficult, if not impossible, to distinguish the trypanosomes present in the blood of animals, and what I want to know is whether you think in the present state of knowledge of the subject it is possible to form an opinion as to whether the blood of an animal does or does not contain *Trypanosoma gambiense*; or, in other words, can you be certain that the given trypanosome you find in the blood of man is *gambiense*, and not some other trypanosome?—Unless I am allowed to do my own test, I do not think I can, because the *gambiense* was primarily obtained from the human blood, and therefore, to my mind, you cannot definitely state that any trypanosome is *gambiense* unless you see it in a human being.

1156. Would you go so far as to say that the trypanosome you found in the blood of an animal was a trypanosome pathogenic to man?—As far as one can be certain, I should say it would be pathogenic.

1157. For the practical purpose of controlling the disease, would you think that our knowledge at the present time is sufficient to justify measures being taken on that hypothesis?—Certainly.

1158. Leaving *gambiense* for a moment, have you any experience as regards your own work of the trypanosome which is called *Trypanosoma rhodesiense*?—I have found trypanosomes in game which, again on laboratory methods of diagnosis, seemed to me to correspond with *Trypanosoma rhodesiense* to such an extent that Dr. Kinghorn, who saw my experiments before they were published, said that had he met that trypanosome he would have called it *Trypanosoma rhodesiense*.

1159. So that in the course of your work in Uganda you have found in game a trypanosome which corresponds with the description of *Trypanosoma rhodesiense*?—The particular trypanosome was sent to me by Mr. Montgomery from East Africa.

1160. You have not found it in Uganda?—I have found some trypanosomes which, as far as I can see, are extremely closely allied with *rhodesiense*, if not identical with it, from game in Uganda.

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[Continued.]

1161. Do you draw a sharp line of differentiation between *rhodesiense* and *gambiense*?—I think one can distinguish between types, but I think it is quite possible that certain trypanosomes would be just about midway between the two. The two extremes are quite definite, but I do not think it at all necessary to exclude the possibility of intermediate types being found.

1162. You mean on morphological grounds?—I mean the great distinction is the morphological one.

1163. Leaving that part of the subject, you have had some experience of *pecorum*, have you not?—Yes.

1164. Do you look upon that as a perfectly separate form of trypanosome?—From *gambiense*, yes, certainly.

1165. You look on it as a perfectly distinct species?—Yes, from either *rhodesiense* or *gambiense*.

1166. Would you base that simply on differences in morphology or other differences?—Chiefly differences in morphology, but they are very constant. They behave differently in the fly.

1167. Then have you found in the course of your inquiry that game may be infected with more than one trypanosome?—Yes.

1168. You are quite satisfied that the different appearances are not different stages in the life history of one and the same?—Quite certain on that point.

1169. You are perfectly satisfied they are different trypanosomes?—Yes.

1170. Could you give us an instance?—Of finding two trypanosomes?

1171. I only want you to mention the trypanosomes?—You can find an animal infected, say, with *Trypanosoma vivax* and *Trypanosoma pecorum*, and you can separate them by sub-inoculations and exclude one perfectly easily by choosing the proper animal to sub-inoculate the blood into.

1172. So you are satisfied that there is a considerable number of trypanosomes?—Not a considerable number, but a few species fast.

1173. Could you give them to us?—I think *vivax* is a fairly fast species, and I think *uniforme* is, and I think *pecorum* is. *Pecorum* and *nannum* are allied closely. I think the so-called *brucei* group are at present very unstable.

1174. What would they include?—They would include *gambiense*, *rhodesiense*, *brucei*, *equipardum*, *equinum*, and *pecaudi*.

1175. Have you had any experience of sleeping sickness in man?—No.

1176. Were you not in medical attendance at any of the sleeping sickness camps?—No.

1177. (Dr. Bagshawe.) You were saying just now that *Trypanosoma gambiense*, or, a trypanosome you could not distinguish from it, was found in the situtunga. Was that on the mainland or on an island?—On an island.

1178. Have you examined those antelope caught on the mainland as well?—Yes, I have examined them. On that particular island I shot four, and injected the blood of two into one monkey, and the blood of the other two into another monkey, and both monkeys became infected, and certainly they were all four infected with the trypanosome. On the mainland, I think, I examined about nine or ten of that particular species, and I found *uniforme* and *vivax*, but never any *Trypanosoma gambiense* in the mainland buck.

1179. Supposing it was considered advisable to exterminate this particular species of antelope, do you think it would be possible?—It would be extraordinarily difficult. You mean situtunga alone?

1180. Yes.—On the mainland or the islands?

1181. We had better say first on the mainland?—I think it would be extremely difficult to exterminate them on the mainland.

1182. Do you think it would be possible on the islands?—No; I do not think it would. It would mean the introduction of an enormous number of natives into an infected area, anyhow, and even then you would not get them all.

1183. With regard to the infectivity of *Glossina palpalis* caught on the lake shore you have made a good many observations, and previous observations,

I think, were made by other Commissioners of the Royal Society?—Yes.

1184. From your observations and those of the Sleeping Sickness Commission, do you think the infectivity of the lake shore flies with *Trypanosoma gambiense* is keeping about the same, or is it diminishing?—I think it is just about the same; it is very little less, and certainly not markedly less, not more than would be accounted for by the removal of the enormous number of infected natives.

1185. How long have observations been carried out on that point—for how many years?—I think it is between four and five years since the natives were finally removed from the island and coastline to the mainland.

1186. You do not think it is more than that, that observations have been made on the infectivity?—Not since the natives were finally removed. Observations were certainly made some years before the removal of the natives.

1187. But it is only about four years since they have been removed?—That is all.

1188. During that period the infectivity has kept about the same?—I do not think it is materially altered; it is a little less but certainly it has not disappeared. The figures I obtained were obtained also by Sir David Bruce. I have never reached his maximum figures, but on the other hand we exceeded his minimum, I believe.

1189. To go to another point, you made some experiments on the prophylactic properties of a certain drug arseno-phenyl-glycin in the blood of a monkey against the bite of infected *palpalis*, did you not?—Yes.

1190. You found that if infected *palpalis* were fed on monkeys which had been previously treated with this drug, there was some disinfection in the flies?—Yes, but they had to feed on the monkey a very short time after the monkey had received its dose of arsenic, otherwise there was no effect at all, and the dose of arsenic was relatively a very large one from the human point of view.

1191. Did you find that when flies known to be infective were fed on monkeys which had been previously treated with this drug, the monkeys were protected from infection?—Yes, to a certain extent dependent on the time which elapsed between feeding and inoculation with the drug.

1192. Do you think anything of the kind could be applied to man? Do you think man could be protected in some such way?—Certainly only for a very short time, and the question is whether he would stand a proportional dose.

1193. For how many days did protection last?—I think about 10 to 14 days, if I remember rightly, not longer.

1194. Do you think it would be possible to obtain a similar result with a less dangerous drug?—I want to try some other drugs with that end in view; I think it might very conceivably be done because the effect with fly infection is very much more marked than it is if you infect by means of a syringe; the immunity is much longer in the case of fly infection than it is in the case of infection by a syringe.

1195. So that you think there is some prospect of successful work in that direction?—Yes, given a less violent sort of drug, I think it is quite possible you may, anyhow for a period of say two weeks, discover some means by which you could confer immunity on a person passing through an area where trypanosomiasis exists.

1196. (Mr. Buxton.) I understand that in the blood of the situtunga, which you shot yourself on an island, you found *gambiense*?—Yes.

1197. So at least you thought you proved that they were a reservoir of *gambiense* because you used their blood to infect antelopes?—To inject the small laboratory animals to be exact.

1198. As to those you shot on the mainland the effect was not the same?—To my mind that was only due to the fact that the antelope on the mainland are far less frequently bitten by the fly than the antelope on the islands are.

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1199. That is what I wanted to arrive at; that is the reason for differentiation you think?—I think that is the only conceivable reason; it is a fact one observes that the places where the situtunga occur on the mainland are not necessarily places where there are a lot of tsetse.

1200. Is the situtunga the only antelope on the islands?—Yes.

1201. Supposing there were a practical experiment, I want to ask you this practical question: Do you think you could exterminate the situtunga on an island?—I think you could on one island certainly.

1202. It is a very small animal, is it not?—No. It is very easily killed by the natives; it is much more easily hunted than most antelope from the native point of view.

1203. Are they rather a secretive animal?—Yes.

1204. And live in rather dense growth?—On the particular island I am speaking of there is very little cover for them; the trouble would be in the big swamps. I think they could be exterminated from one particular island but you could not guarantee that they would not swim over from a neighbouring island to it after you had finished exterminating.

1205. Dealing with the epidemic in Uganda you said it was introduced by porters, that the conveyor was man: Did you mean to say by that that man was the reservoir?—I think an infected native started the epidemic.

1206. Infected in his blood?—Yes, he walked over to the lake shore with trypanosomes in his blood presumably.

1207. And infected the flies there?—Yes, the flies bit him.

1208. That is the origin, in your opinion?—Yes.

1209. Then he must have been a reservoir?—Yes.

1210. (Dr. Balfour.) Have you ever done cultivation experiments *in vitro*, that is to say, in the test tube with animal blood in Uganda?—No.

1211. Do you think any useful information could be obtained by such experiments?—Do you mean as regards diagnosis?

1212. Yes, finding out reservoirs which might not otherwise be detected?—No; I think if you made cultivations from the blood of an animal and found organisms in your culture you would be in a difficult position to say what the organism was.

1213. You mean trypanosomes?—Yes, because there are trypanosomes like *T. ingens* in buck, which presumably will grow very readily on culture media, and may give forms quite like the pathogenic trypanosomes.

1214. That is in cultural forms?—Yes, and they are of absolutely no significance; in other words, the less pathogenic trypanosomes will grow more readily than the pathogenic. To a certain extent I have no doubt you could distinguish between them, but I do not think you would be able to distinguish between the pathogenic trypanosomes in culture if you cannot distinguish them in the blood itself.

1215. And you do not think it would give you any other useful information?—It would only give you useful information as regards the number of animals infected with some sort of trypanosome; I do not think it would help you very much towards the diagnosis of the species of trypanosome they were infected with.

1216. Then you do not think this method should form an essential part of the investigation?—I think wherever possible it should be done.

1217. In some places it is not possible?—In some places it is very difficult; in fact, we could not do it with the number of people we had and the staff at our disposal. We tried on one occasion and failed.

1218. Have you ever found or heard of any fungoid disease in tsetse-flies like that which affects the domestic house fly?—No; in my small experience from that point of view, the only time I have ever seen the tsetse-fly worried by anything is when it sometimes has a little arthropod creature growing on its thorax; a little red thing which does not worry it very much.

1219. But nothing in the nature of a fungus?—No, they very often contain yeasts in their gut, but as far as one can see the fly is perfectly healthy.

1220. (Professor Newstead.) I should like to ask whether you think that *Glossina palpalis* would reproduce its species at the same rate on the blood of lizards as on the warm-blooded animals?—I am afraid I have no experience on that point.

1221. You have not made any laboratory experiments?—No; but other people have.

1222. (Mr. Austen.) Have you studied the breeding of *Glossina palpalis* on Lake Victoria in a state of nature?—I have frequently been on the breeding-grounds, but I cannot say I have particularly studied the conditions; I know where they breed, and I have seen the places where they breed in that particular district.

1223. Would you kindly tell the Committee where they breed in that district?—Far and away the finest breeding-ground I know of is on Damba Island, one particular piece of shore; the second one is on Sesse, and the third one is the place where Sir David Bruce got most of the flies for the laboratory, but nowadays you can find very few pupae there.

1224. Have these localities any special characteristics?—Sand is present on two of them, and there are certain characteristics of undergrowth that are common to all of them; but the main thing which strikes one is that it is undergrowth—bush—which terminates very abruptly on shingle of fairly fine quality which runs about 20 yards down to the lake shore. You see a line of forest fronted by a strip of more or less sandy, shingly beach.

1225. Are the pupae, then, in the shingly beach?—Just inside the undergrowth.

1226. Not out in the open?—As far as I know, no.

1227. Do you ever find them associated with dead logs or anything of that sort?—Frequently; the place where the natives go for them and where you find most pupae on these particular beaches of Sesse is an old, fallen-down tree.

1228. So that one would be likely to find more pupae in a place where there was such an old tree or a piece of drift-wood than in a place where there was no cover of the kind?—It certainly seems so.

1229. Can you suggest anything which possibly attracts the parent flies to a particular spot in order to deposit their larvæ there?—Presumably, they know the conditions they like. There must be something about these places which attracts them, because they are not found, as far as I know, in many places which, from my point of view, look just as good. I do not know what it is.

1230. You would say there is something?—I think there must be, because there are places where you think you ought to find pupae, and you take quite a time to look for them and do not find anything—sand and various things which look as if everything was all right, and you find nothing.

1231. So that if we could discover what this factor is in the breeding of *Glossina palpalis* it would be a useful piece of knowledge or might be an important thing?—It might be.

1232. Have you come across any evidence of migration of the fly from the islands to the mainland?—That is not my department.

1233. I know that, but have you, as a matter of fact, observed anything of the kind?—I have, by taking them over myself in a canoe; that is all.

1234. You have not observed them passing naturally from the islands?—No.

1235. You are aware, I suppose, of the method which has been tried on Principe Island for the trapping of *Glossina palpalis*?—Yes.

1236. The black cloth and birdlime method?—Yes.

1237. Do you think you could utilise that on the shore of Lake Victoria?—No, because we have had anything from 2,000 to 5,000 pupae a month for four years from this particular strip of shore which is about a quarter of a mile long; and the flies have not altered in their numbers in the slightest degree as far as one can see. In addition to that, the natives have been catching the

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flies themselves and destroying the pupæ. My own supply runs up to 5,000 a month apart from Dr. Carpenter's experiments on the same piece of beach. It has made not the slightest difference to the number of the flies.

1238. Has an experiment with the black cloth method actually been tried there?—I do not think it has ever been tried on Damba. I do not think it has ever been tried on a serious scale in Uganda. The fly on Damba is in appalling numbers, and we have had this information about the pupæ to act upon. As to the first fly beach from which the pupæ used to come, something has happened there which has determined an enormous diminution in the number of the fly. The only thing I notice is that the lake has sunk and that the stuff where they used to deposit their pupæ is now a little further off the lake than before. Whether that has done it or not I do not know.

1239. It is something you think other than the collecting of the pupæ?—The collection has gone on and the boys themselves say they have caught all the fly, but they cannot catch all the fly on Damba. It is possible that the numbers were never anything like what they were on Damba. They were never so numerous on this other beach as on the Damba beach. Nevertheless, the boys say they caught all the fly on the first beach and the fact is that very few are left now, and it is hard to find pupæ.

1240. What is the exact position of the area where the fly has diminished?—It is on Buku Bay on the eastern side on the mainland. I know exactly where it is.

1241. You could point to the particular place on a map?—Yes.

1242. In reply to another member of the Committee you stated that one means on which you would rely in your diagnosis of what you consider to be *Trypanosoma gambiense* in the game would be its behaviour in the fly after feeding on an infected animal?—I think that is far and away the most important means of diagnosing that we have as regards trypanosome species nowadays; I think it is the only natural test because in nature if the trypanosome could not survive in the fly it would never get any further—it would die with its host.

1243. Am I not right in thinking that *Trypanosoma brucei* if ingested by *Glossina palpalis* behaves much in the same way as *Trypanosoma gambiense* does?—I believe it does exactly.

1244. How then would you distinguish by that method between them?—I do not think you could, but you can distinguish between the trypanosome called *pecaudi* which settles in the proboscis of the fly and does not settle in the salivary glands and *gambiense*; in other words I think the trypanosomes which behave in the same way in the fly are probably suspiciously nearly allied and one would hesitate to call them different species on other grounds. I should apply that fly test as the final test between two species, which have been pronounced to be identical on other tests.

1245. I thought you told me just now that both *Trypanosoma brucei* and *Trypanosoma gambiense* in *Glossina palpalis* behaved in the same way?—Yes.

1246. You could not apply it as a final test in that case?—I think you could; it would make you very suspicious about the significance of their differences; it would make me very suspicious about introducing a trypanosome, which behaved like *gambiense* in the fly, into a human body because other people said it was not pathogenic to man.

1247. Would it enable you to say definitely that that trypanosome was *Trypanosoma gambiense* and not *Trypanosoma brucei*?—No, not that test.

1248. Therefore so far as that test goes a trypanosome in a wild animal which appeared to be *Trypanosoma gambiense* might, in fact, be *Trypanosoma brucei*?—Yes.

1249. You are familiar of course with Dr. Taute's well-known experiment recently on himself?—Yes.

1250. I think you said in reply to a previous member of the Committee that you regard the *argumentum ad hominem* as the final argument?—Yes.

1251. May I take it that to that extent you agree with Dr. Taute's conclusion when he says (I am translating from his statement): "The trypanosomes in 'naturally infected game and in domestic animals' can be regarded with certainty as the causative 'agents of sleeping sickness in man only when they are proved to be pathogenic to man.' Would you accept that?—No, I have got to stipulate what sort of man he is using.

1252. How do you mean?—If he had used a native from an area from which he derived the trypanosomes, the experiment would have had very little scientific value, and even as the experiment stands no reliable deductions can be drawn from a single inoculation.

1253. Reading "white men" for "man" you would accept that statement?—I think I should have to.

1254. With regard to what you said in reply to Dr. Bagshawe as to the infectivity of *Glossina palpalis* not having markedly diminished on the lake shore since the removal of the population, can you tell the Committee anything about the infectivity of your captive antelopes? Has that remained the same? Have you any information on the point?—I think the frequency with which one gets evidence of their being infective is decreasing; it is more difficult to prove it from the laboratory point of view, but the fact remains that most of the evidence I get now as regards their infectivity is obtained by feeding laboratory-bred flies on those antelope; in other words the flies seem able to pick up the infection from the antelope as quickly as anything else; that is to say, the trypanosome in those antelopes is particularly favourably situated for perpetuation by the fly.

1255. Is it more difficult now to infect fly from those captive antelopes than it was?—Yes, it is.

1256. Does that point to anything do you think?—I think for that fact alone to be of any value one would have to do very many more experiments than have been done and that the flies would have to be fed for a certain definite period—a daily batch of flies being fed on this antelope for a definite period—and if you found that all those flies were more difficult to infect than they were in the old days you could say that something has happened in that antelope to make it less infective. You might if you fed them at long intervals possibly strike a more or less negative period in the antelope and get a false impression; in other words the positive evidence is what I am after at present to see whether they are infective still.

1257. May I take it that you consider at present there is not sufficient evidence on which to found a conclusion that infectivity in your antelopes is diminishing?—No, I do not think there is; I think it is diminishing, but I see no prospect of being able to say it has died out.

1258. But you think it is diminishing?—I think it is. In my own mind I do not see why it should die out, that is a biological point of view.

1259. (Sir Stewart Stockman.) There are several considerable herds of native cattle, are there not, in Uganda?—Yes.

1260. Have you made any examination of their blood?—No.

1261. I would just like to ask one other question on this diminution of virulence in the antelope. You say you think it has diminished to a certain extent?—Not the virulence but the infectivity.

1262. I beg your pardon; that observation is made on the antelope in captivity?—Yes, on various antelopes in captivity.

1263. Do you think it would be the same if they were running in nature?—I think so.

1264. (Dr. Martin.) You acceded to Dr. Taute's conclusion that the only way of deciding that a trypanosome found in a wild animal was pathogenic to man was to try?—Yes.

1265. But would you accept the converse, that if in one instance a trypanosome found in a wild animal did not infect a man therefore that trypanosome could not infect men?—Certainly not, because the contrary has been proved; Dr. Kleine and I myself have on occasion injected virulent blood into an animal which is suscep-

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tible to a trypanosome which I knew to be in the syringe, and the animal has not become infected.

1266. In distinguishing between *brucei* and *gambiense* you replied to Mr. Austen that you considered the final test was the behaviour of the trypanosome in the fly: you do not mean to say that you would have any difficulty in distinguishing between *brucei* and *gambiense*, given all the methods which are available, do you?—The only method I know of distinguishing between *brucei* and *gambiense* is the behaviour against human serum, otherwise I think it extremely difficult to distinguish between them.

1267. You would not place much reliance on the varying pathogenicity for different animals and on the type of disease which followed infection of the one or the other?—Not if one was dealing with strains of the trypanosomes which had been kept up in laboratories for a long time.

1268. (Mr. Rothchild.) You said in answer to the Chairman that you thought the fly might possibly survive the destruction or driving off of the big game by feeding on reptiles, but do you not think it is highly probable that if the big game is destroyed or driven away, not only will the fly feed on the smaller mammals but that it will also infect those?—I think the small mammals have to be reckoned with just as much as the larger mammals and that is the difficulty of the experiment which has been suggested.

1269. That is what I thought and what I wanted to bring out. I have been told that in experiments elsewhere baboons are specially sensitive to various human diseases; would it not be extremely probable that the fly would feed on the baboons and might even cause a serious epidemic to be carried by them?—You have got to exterminate your baboons with the rest of your animals.

1270. And mice, rats, foxes, and all small mammals?—Yes, I think the fly would probably feed, and do probably feed on anything with blood in its veins, but the frequency with which it would get the opportunity to feed on the small mammals would probably be very small.

1271. Do you mean by that, that the small mammals are more or less nocturnal?—Partly.

1272. But has it not been proved by the expedition of Mr. Willoughby Lowndes that they were equally harassed at night by the tsetse-fly as in the daytime?—It depends on the species of tsetse.

1273. These tsetse-flies were submitted to Mr. Austen, and he knew what species they were.—I do not think *palpalis* will ever worry you at night.

1274. Although at the present moment the chance of the flies getting at smaller animals, such as mice and rats, is rather problematical, might it not be that owing to the difficulty of rapidly driving off the big game their habits would alter somewhat, and they might hunt out these smaller animals?—If they have sufficient intelligence, yes; I think they would have to take every opportunity of feeding what they could, but I do not know what effect it would have on the flies.

1275. I merely want to get at the fact that the smaller mammals having warm blood as opposed to the reptiles, they would more willingly search out the small mammal than go straight from the bigger mammals to the reptiles?—That I do not know.

1276. (Sir Mackenzie Chalmers.) How long have you been working at sleeping sickness in Africa?—About three years.

1277. How long have you held your present appointment as medical officer?—The same time.

1278. I thought you went out there originally for the Royal Society?—I went straight to the Royal Society's laboratory, and have been there ever since.

1279. Is your work purely research work or is it administrative work?—Purely research work.

1280. When you go out again your work will be purely research?—Yes, for this next tour, anyhow.

1281. Do you know what administrative measures have been taken recently? Does that come within your knowledge?—I happen to know in a certain part of the Western Province of Uganda what has been done.

1282. What is that?—Simply moving the natives from the fly districts.

1283. That has proved successful?—Yes, in this particular part of the world there are very few natives, and the fly country is a very unprofitable sort of country, and it is singularly easy to move the natives from the flies.

1284. When you move the natives do you segregate the sick in any way?—They are segregated temporarily, to a certain extent.

1285. You do not think there is any danger of infection as long as you keep flies away from the sick?—I think not.

1286. I do not quite know what your opinion is about this suggested experiment of clearing an area: Do you think it is a possible one?—I do not think it possible in Uganda, but I do not see why it should not be possible, provided you can do what you say you can do about clearing the area of everything. You have to move leopards, elephants, hyenas, and every conceivable form of animal in order to make the experiment valuable.

1287. Is the trypanosome ever found in carnivora?—I have found *pecorum* in the hyena.

1288. But not *gambiense*?—I do not see why it should not be found; it is only a matter of luck.

1289. Do you regard the *sitotunga* as the main reservoir in Uganda?—No, only the islands, because it happens to be the only antelope; as far as I can see there is no reason to suspect one particular species over another.

1290. But the antelope generally you regard as the main reservoir at present?—And buffalo, as far as one knows. I have not had the opportunity of shooting enough elephants, but I do not see why they could not do it too; in fact, any of the big game.

1291. You do not regard any class of big game as more guilty than the rest?—No, I do not think so—possibly the antelope.

1292. You think it must be a sweeping experiment, as far as you can, getting out all the mammals?—Yes.

1293. You told us there were a good many other tsetse-flies in Uganda?—There are a lot of *pallidipes* and *fusca*.

1294. Are any of these disease-bearing?—I found trypanosomes in several *pallidipes* and in several *fusca*, but I was not able to decide what they were.

1295. Whether they were pathogenic or not?—No, but one is perfectly certain that the *pallidipes* or the *fusca*, one or the other, or both, must be carrying trypanosomes, because they infected the game which I found infected. There were very few *palpalis* about.

1296. So that a crusade against *palpalis* itself would not be of much use?—No.

1297. And there must be a crusade against all *Glossina*, if there is such a word?—Yes.

1298. Do you know whether any of the other biting flies are possible carriers?—I think it possible in connection with a large herd of game.

1299. You told us about the breeding places of *palpalis*; is it known at all how far off their breeding places you find the *palpalis*—how far they migrate?—It is known, but I do not know it; that is not my province. I do not know about it myself. Dr. Carpenter knows—I do not.

1300. In reptiles, I think I understood you to say, you never found trypanosomes of a pathogenic character?—No.

1301. As regards antelopes which are inoculated, I suppose before inoculation you tested them to find whether there were not already trypanosomes in their blood?—That was Sir David Bruce's work; he inoculated them.

1302. I imagine as an ordinary precaution he would discover that?—Presumably—yes, I know he did.

1303. As regards the resistance which you told us about in laboratory experiments, the trypanosome suffers a good deal from human blood or serum, does it not?—It depends on the species; the *gambiense* does not suffer at all, so far as I know, but the *rhodesiense* does *in vitro*.

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1304. Do you draw any distinction between the resistance in a man, who is an inhabitant of the district, and one who is a stranger to the district?—I think I do. I do not know that I have any reason for doing so.

1305. You have not tried laboratory experiments to see whether the blood of an inhabitant is more fatal to the trypanosome than the blood of a stranger?—No, I have not done that at all.

1306. But from your general observations, do you think there is any difference between people who have been born and bred in a district, and strangers coming into it?—I only think there is; I have no reason for saying so.

1307. It seems probable, you think?—Yes, that is with regard to the trypanosomes which belong to the country in which the natives live, not trypanosomes introduced from another country to which they have never been.

1308. And, of course, resistance varies with different individuals and different states of health of that particular individual?—I should say so, certainly.

1309. That would be, at any rate, in accord with general experience in connection with other infectious diseases?—Yes. On the other hand I think certain trypanosomes would get into anybody you put them into whether they were healthy or not.

1310. You think Dr. Taute was rather lucky in the particular trypanosome he injected into himself?—Particularly lucky.

1311. You would not care to repeat the experiment?—Certainly not.

1312. (Sir William Leishman.) Have you observed the effects of feeding flies on reptiles in the laboratory?—On the longevity of the fly, do you mean?

1313. Do they feed readily on reptiles in captivity?—Very readily indeed. On one occasion I put a toad into a cage with six flies and he was annoyed by their buzzing round him. He exuded a lot of stuff on his back and the six flies got stuck on this stuff; then the toad went to sleep. The stuff dried and the flies began to clean their legs and then proceeded to feed on the toad, and they fed very readily.

1314. Any other besides?—Lizards and water lizards. I know from observation that in nature they feed very readily on lizards, but they would not do so in captivity.

1315. Did you observe the effect on these flies of feeding on lizard blood, first of all with regard to the digestion of the blood in the flies you tested as compared with mammalian blood?—No, I cannot say I did. I did an experiment dealing with newly hatched flies in the laboratory to see whether the blood which the fly imbibed, either reptile, mammalian, or human, might not add to the longevity of the fly and I did not find any difference at all. Provided the fly fed, apparently the blood was enough no matter what it was.

1316. Did the reptilian blood seem to have any influence on gestation?—That I did not try.

1317. Do you think it is the case that the temperature of reptiles increases when they are exposed to the sun?—One knows that externally it does.

1318. Have you any other evidence as to the temperature of the blood?—No, I have not.

1319. But you think it is known?—I have never heard any opinion on that point.

1320. You mentioned that you had come across *pallidipes* and *fusca*, as well as *palpalis*; did you get any information as to their breeding places?—No. I did not find any pupae and I did not look for them, but Dr. van Someren found the breeding places of both of them in the district.

1321. Do you happen to know what class of breeding place it was? Did it resemble that of *palpalis*?—I really do not know.

1322. Are *pallidipes* and *fusca* common in the areas where you find them?—*Pallidipes* are very common and *fusca* are common in the forests of their districts, but nothing like so common as *pallidipes*.

1323. As regards their feeding habits, are these two flies nocturnal feeders?—*Fusca* is to a certain extent, I do not know anything about *pallidipes* feeding at night.

1324. I suppose you have had the same experience as most people in finding a disparity in the sexes, that is to say, you find more males?—I am afraid I did not notice that.

1325. You know that is the case?—Yes.

1326. Have you any views on the causation of that—any explanation?—I have never thought about it.

1327. Do *fusca* and *pallidipes*, as far as you know, cause cattle disease—trypanosomiasis of stock?—I think they must in this particular district, the Western Province, for instance, I think it is extremely probable that *pallidipes* will carry the cattle trypanosome. I found relatively a large number of wild *pallidipes* infected with trypanosomes, and it was very much the predominant tsetse in that district.

1328. I would like to know about the number of trypanosomes you found in the blood of antelope, not your experimental ones, but antelope naturally infected. Are they numerous?—*Uniforme* is usually relatively numerous, but *vicax* not very often.

1329. In a thin blood film?—I usually examined fresh blood films.

1330. How long do you usually take to find them?—Very often the first found was *uniforme*, but of the others I sometimes found one between two slides; I looked over two slides and found two trypanosomes, or one per slide, when I had missed them in the fresh preparation.

1331. If, say half a dozen tsetses bit an infected antelope, would you say that every one of them was likely to become infected?—I think it is very probable, because one never examines anything like the quantity of blood a fly would take up.

1332. Have you ever observed the nature of the species of bacteria in the intestinal contents of the tsetses?—Only just from continually seeing them; one sees the long bacteria and the coccus which appear to be the same organism, not necessarily so; they are present in characteristically different parts of the gut, the fore and the hind gut respectively, as a rule.

1333. Have you ever thought of where these came from?—Yes. I once thought they might have something to do with the trypanosomes for one thing. We have had various talks about them but we have never found out anything about them.

1334. You have not investigated it?—No.

1335. Have you any evidence of the flies feeding on vegetable juices?—I have not.

1336. Or of drinking water in captivity?—No, I have never seen them do that; I have tried them, but never got them to do it.

1337. (Sir Mackenzie Chalmers.) In your laboratory are you working away also at remedies in any way?—Only the prophylactic question. I have only tried one drug so far.

1338. That is part of your work?—Yes.

1339. That is going on in the laboratory?—Yes, I have done one set of experiments, and I am going to try others.

1340. How many people have you in your laboratory?—Miss Robertson was the only one; she was doing her own work.

1341. Is your laboratory sufficiently supplied?—The only thing is that the incubator will not work; otherwise it is wonderful considering where it is.

1342. (Chairman.) Just one question following up the questions of Mr. Rothschild. Have you experimented at all on small mammals in the laboratory?—Edible rats are the only wild ones.

1343. Do they survive? Are they apparently either immune or tolerant, or do they die as a rule?—The ones I have used die very quickly.

The witness withdrew.

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Mr. G. D. H. CARPENTER, M.D., B.CH. (OXON), M.R.C.S., L.R.C.P., called and examined.

1344. (Chairman.) You are a medical officer in Uganda?—Yes.

1345. How long have you been working there?—I went out in June 1910.

1346. On what particular work have you been principally engaged?—Entirely entomological work.

1347. And entirely in Uganda?—Yes.

1348. Your work, if I understand aright, is not related to *morsitans* at all?—No, I have never seen *morsitans*.

1349. As regards *palpalis* I gather you have not considered the question of how far game does or may form a reservoir of the trypanosome of sleeping sickness?—I have had no opportunity of considering it; I have not worked at that question.

1350. Any information you have is simply derived from others?—Just as you might get it yourself from reading papers.

1351. Have you formed an opinion at all as to the movements of *Glossina palpalis*, whether they are seasonal, or what they are dependent upon, and how far their movements extend?—I think, round the shores of the lake where I have been working, where you do not get any great seasonal changes, there is not likely to be any great migration; but in any place, such as West Africa, where you get seasonal changes it is extremely likely the fly would migrate from place to place where certain places dry up and others remain damp; that is to say, a little stream which dries up in the dry season would be inhabited by the fly in the wet season and probably not in the dry season at all. I think that is a question of great importance.

1352. In the course of such migration would it as far as possible stick to the line of water or migrate over open ground?—I think it would stick to water, or at any rate to damp shaded forest.

1353. You think under most circumstances *palpalis*, whether migrating or living in one place, would not leave water?—I think not.

1354. It might be impossible for it to migrate without doing so, might it not, in some cases?—In certain parts the upper part of a river might dry up, but the lower reaches remain moist, in which case it would migrate down the river.

1355. As regards the normal food of *Glossina palpalis* are you of opinion that it has a preference for warm-blooded mammals over reptile blood?—Yes.

1356. Beyond that have you any opinion as to whether it will prosper and thrive on reptile blood as well as on warm mammal blood or other food?—I do not think enough work has been done on that to enable us to say much yet.

1357. You would not like to commit yourself to an opinion on that?—No, because it is difficult to get flies to feed on cold-blooded animals unless they have been in the sun and got warmed up. I tried to feed some on a big lizard which had been kept in a dark box and was cold, and the flies would not bite it. If the lizard had been in the sun the flies would soon have bitten it.

1358. Where it has not sufficient food from other sources *palpalis* will feed fairly freely on reptiles?—Most certainly.

1359. I notice you also state in your summary: "I have obtained evidence that *Glossina palpalis* does 'imbibe water'?"—Yes, microscopical evidence; that is to say, I have seen in a microscopical examination of the contents of the gut of a fly a minute Ostracod crustacean which must have come from water. It was small enough to have been sucked up by the fly.

1360. We have been told it is most improbable, but would it be conceivable, that the trypanosome might be acquired from any plant substance?—I only know that animals closely allied to trypanosomes are found in plant tissues, but I could not say more than that. I have not studied that side of the question.

1361. Are you of opinion that, apart from wild mammals and reptiles, there are other possible reservoirs of infection, if I may use the expression, such as birds and bats?—I think that bats certainly ought to be

investigated. I do not know of any work being done on them at all, but I have seen them hanging up among the herbage where *Glossina* was abundant, where they could certainly have been bitten during the daytime, and I think work ought to be done to ascertain whether they may carry trypanosomes.

1362. I gather what you think is that the evidence as to the possible reservoirs, apart from wild mammals, is not yet sufficient to enable us to form a final judgment: is that right?—That is what it comes to.

1363. It seems to be common ground that in the absence of the fly both cattle and people would be safe. Have you formed any opinion as to a practical method of any large destruction of the fly?—I think theoretically there is no difficulty; it is a matter of clearing, the practical difficulty of doing it is very great.

1364. Is your remedy clearing?—That is the only one I can think of at present.

1365. You do not think the introduction of insect enemies to places which are known to be infested with the fly would be practically effective?—Do you mean to say enemies which are present in one particular locality, but not widespread, or one from some foreign country altogether?

1366. I rather left that to you.—It is entirely a question of the balance of nature, I think. I think you could never get an animal completely to eradicate the fly unless it was something which preferred the tsetse to everything else, and in default of tsetse fed on other things so as to keep itself going, and I do not see how one is to get that. Such animals as there are at present have struck a balance long ago. There are enemies of the fly now; at present there is a certain kind of wasp preying on the tsetse fly, but they have been doing that for a considerable time, and the fly is still there; they have struck a balance, and I do not see how one can in any way interfere with that balance.

1367. You would not anticipate that a systematic search for the breeding places would be effective?—I think it would help, but I do not think a localised breeding place is absolutely necessary. I have been round two of the islands in a canoe searching for localised breeding places, and found none, but I found there were fly on the islands. Every here and there one finds a single pupa in a log at the root of a tree, and I think the flies are kept going by that means.

1368. (Sir William Leishman.) The Chairman has asked you about the evidence in your summary that *Glossina palpalis* will imbibe water, and you mentioned the finding of this little crustacean. How often did you find that?—Only once. I examined several thousand flies altogether, and only once found that. Of course the chances of a fly sucking up anything which you could recognise are very small; although they may suck up water it is very seldom they will suck up anything you could recognise as coming from water.

1369. As to the other part of the same paragraph you add, "and from plant tissues." What is your evidence with regard to that?—I found banana-starch grains in the preparation from the gut of a fly I was examining. One had to consider any possibility of error in that. I had a little monkey running about, and he had been eating banana, and at first I thought possibly he had dropped a little bit, and that I had soiled my preparation. The first time I found it I disregarded it. The second and third times that possibility was excluded, as the monkey had not been running about where I was, and I did not handle banana, and there was no possibility of a fouling of the preparation accidentally in that case.

1370. You found it three times?—Yes.

1371. Out of the same number of thousands?—It would be somewhere about 2½ thousand—a very large number of flies.

1372. You have often observed the fly on roots and plants?—I have seen them resting, but never seen them sucking anything.

1373. Have you made any observations as to the kind of plant you have seen them on? Do they show

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greater affection for one plant than another?—Not as long as there is thick shade.

1374. Not for juicy leaves in preference to dry ones?—I could not see any preference.

1375. Have you any personal observations of the nocturnal feeding of those tsetse?—I have been down sometimes at the edge of the forest where the fly was after dark, and I have never seen one; and the boys know quite well they do not get bitten if they go into the fly area in the dark.

1376. That is *palpalis*?—Yes, only *palpalis*; but *palpalis* is the only one I am acquainted with.

1377. You have not come across *pallidipes*?—No, not round the lake.

1378. (Sir Mackenzie Chalmers.) What are you working at now? When you go back what particular line of research are you taking up?—With Mr. Fiske, who is out there, I hope to try the migration of the fly, the distance it will fly, which I think extremely important.

1379. Nothing is known about that as yet?—I think not; it has been shown that they will fly about two miles to get food, but it is quite possible they go much farther than that.

1380. Whether the horde migrates?—Yes, and, if I may say so, I think the whole question of the game experiment depends very largely upon that. Supposing you are to enclose an area (if it is practicable), and you destroy all the game, and you find out how many flies are infected, and so on; then you leave things, but at the end of two years how do you know where the flies have come from that are in that area? They may have come from several miles away, where the game is still infective. You have to make your enclosure fly-proof, and that seems to me a very practical difficulty. I cannot make out that anybody has thought about it, but it seems extremely important.

1381. (Chairman.) We have thought about it!—I mean one does not see it mentioned.

1382. (Sir Mackenzie Chalmers.) One theory is that if the game was taken away the fly would leave the area and go after the game?—But I understand you are to leave men and cattle in the area; at any rate, men.

1383. Cattle, I think Sir David Bruce suggested, should not be in the area until the experiment had gone on a certain time?—Anyway, you have birds, and they will feed on birds and they will feed on reptiles.

1384. There is no evidence at present that either birds or reptiles can be infected with the trypanosome?—No; but infected flies from outside would still have something to feed upon; they might come in from outside and remain in that enclosed area.

1385. You think the results of the experiment would be purely negative?—They would be open to suspicion unless you had an area fly-proof.

1386. Have you any other suggestion in place of this clearing experiment?—I do not think we have enough knowledge at present.

1387. On what lines do you suggest that work should go?—One should certainly try clearing the breeding grounds we know, and then see whether I am right or wrong as to the scattered pupae being sufficient to keep the fly going. I know myself that where I have been working there were only three regular breeding grounds anywhere near me, and yet the fly was extremely abundant.

1388. You think the most of them were bred on those particular nurseries?—No; because of the few pupae one could find there. On Damba the boys found several thousand per month; on Sesse the fly was more numerous than on Damba, and yet one could obtain only 200 or 300 pupae a month.

1389. So that you rather infer that the fly migrated from somewhere else, or that the breeding was in a scattered fashion?—If there is a breeding ground they make the most of it; if there is not they have to breed where they can.

1390. You are not very hopeful of any experiment?—Not at present.

1391. You look rather to prophylactic treatment?—I say the clearing will drive them away, but there is the practical difficulty of doing it.

1392. (Mr. Rothschild.) Have you tried any experiment in connection with the infectivity of the fly being hereditary?—No.

1393. In all the statements which are made that it is not hereditary it has never been proved that the pupae from which laboratory-fed flies have arisen have come from infected flies?—I believe that is so; it is quite certain, I think, that bacteria are passed on from a fly to the larvae.

1394. Do you think it feasible to try a definite experiment of taking flies which you know are infected and obtaining pupae from them, and breeding those pupae, and then examining them after keeping them alive for different periods; for the infected fly only becomes infective after a given time?—Yes.

1395. I am not sure that any record has been kept of laboratory-bred flies as regards the space of time between the examinations?—I think that is so. I think those experiments were done before the date of infectivity was known so certainly as it is now. I think all the experiments upon the hereditary transmission are rather old ones, and perhaps they might be done again.

1396. (Dr. Martin.) Supposing what is called the experiment were tried, and at the end of two years the number of fly had been reduced, we will say, to one per cent., could you not draw some useful conclusions from that?—One would want to do experiments in knowing the number of flies outside the area at the same time; the dry weather might have killed them off, as it certainly does, or they might have simply flown away from the area. One would want to know exactly what was going on outside the area at the same time.

1397. That could be ascertained?—Yes.

1398. Supposing that there were equal chances that the results of the experiment might be one way or the other, you might still get some use out of it?—Yes, as regards the mere number of the fly one might, provided that the experimental area was fly-proof.

1399. One of the objects of the experiment, as I understand, was to ascertain whether the departure of the large beasts was followed by the departure of the fly?—Yes.

1400. And that might be ascertained by the experiment?—Yes, I think it might.

1401. Notwithstanding the criticism you have made of it?—The criticism was, of course, as regards the infectivity.

1402. If you take an area of such proportions that it would be possible to clear 100 yards round it and keep it clear during the progress of the experiment, do you think the number of flies (you can only hazard a guess) that would migrate into that area would seriously militate against the conclusions to be drawn from the experiment?—I do not think 100 yards of a clearing is enough to keep the flies off.

1403. From your knowledge of the habits of the tsetse fly you do not know yet, I suppose, whether, if you took a certain area which was favourable to the fly, the population was largely stationary or made up by migration? There is no reason why they should migrate, as far as one can see?—Not round the lake; I think not.

1404. This would not be round the lake?—You are referring to Nyasaland?

1405. Yes.—I think probably there they would migrate very far.

1406. Have you no knowledge whatever of the habits of *morsitans*?—No, except what one has read.

1407. (Sir Stewart Stockman.) I think you suggested that the *palpalis* probably migrates in the direction of water?—Yes. I think a humid atmosphere is very necessary for it.

1408. Is that independent of the movement of game in the same direction?—I have no personal knowledge of its migrations, because round the lake shore there is no reason why it should migrate; it has all it wants all the year round.

1409. But they do follow the game, do they not?—I do not know.

1410. (Mr. Austen.) Do you consider that *Glossina palpalis* is dependent upon big game for its continued existence?—That is really a question one can only

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hazard a guess at. One knows, judging by microscopical examination of the gut of the wild flies, that mammalian blood is the most important source of food on the island; but at Jinja on the mainland I have found the reverse. There is about 68 per cent. of mammalian blood and 32 per cent. of non-mammalian blood on the island.

1411. What part of that mammalian blood is contributed by man?—None where I was; there was no one there except myself and my boys. They had all been removed.

1412. Was not that rather a special case?—I am not thinking of Damba Island in particular, but generally speaking. The reports from different areas have varied very much; sometimes on large rivers the proportion of mammalian is in excess of the proportion of non-mammalian blood and other observers have found on smaller rivers that the proportions were reversed.

1413. I would like to put this to you. We will assume two localities in both of which *Glossina palpalis* is present, but in one of which, though there is big game, there are no reptiles. In the other there are reptiles but no big game; would you not consider that under conditions such as those the two groups of *Glossina palpalis* would flourish equally well, one feeding of necessity on reptilian, and the other on mammalian blood?—Yes, I think so.

1414. Could you support that assumption from your own experience?—Only from noticing the extreme avidity with which they would feed on reptiles. I was sitting on the fly beach one day observing things, and a big *Varanus* lizard came and crawled out on the beach close by me, and the flies buzzing round me went and fed on him. I, being a mammal, would presumably have been more attractive, but that was most certainly not the case. Several times I have seen the top of the head of one of these lizards black with flies on the beach. It was in the sun and resting in the water, the top of its head being out of the water, and its head was black with tsetse-flies feeding on it.

1415. I take it that owing to circumstances the tsetse-flies of that locality had been obliged to depend on reptilian rather than on mammalian blood for their food?—I think anything that comes along is fed upon. For instance, I used to examine a certain number of flies every day in the afternoons after being down on the beach in the morning, and I found if there were tracks of a big lizard on the shore in the morning, the flies caught and examined microscopically in the afternoon would have a bigger percentage of non-mammalian blood. If there were tracks of hippos and situtunga, there would be more mammalian blood. It depends on what animal happens to come along the shore.

1416. Surely there is no ground for saying that if you abolished wild mammals you would abolish *Glossina palpalis*, because you would still have reptiles and birds?—There is not, in my opinion.

1417. And you would not expect the fly to disappear?—No. On the lake shore birds are not very important. Something like 2 per cent. of the non-mammalian blood one finds can be put down to birds and the rest is reptilian. I do not think birds are very important.

1418. As regards breeding places, you spoke I think of localised breeding places?—Yes.

1419. Would you tell the Committee what you mean by the expression: what are the characteristics of a localised breeding place?—I think the ideal place is a tree trunk which has fallen down horizontally over sand at the edge of the forest and about 20 to 30 yards away from the water's edge. Under the tree trunk you must have air space in order that the soil should not be damp and yet the air must be humid. I think damp soil is harmful to the pupae, but the air must be humid, and therefore they are near the water. If one finds dead leaves, dead sticks, and debris on the top of the soil, one does not expect to find many pupae there, but the ideal thing is a bare surface of loose soil sheltered by an overhanging tree trunk or the arching roots of the tree trunk. Sometimes one gets the same conditions under a thick tangle of bushes where they run over the ground;

they prevent any roots growing near the surface of the sand. There are no roots to make the sand hard on top, and they allow a free circulation of air so that the soil is kept dry and the pupae are sheltered from the rain and sun. The pupae are always in the shade.

1420. Would it be possible to reproduce artificially the conditions which you have just described?—Yes, quite well.

1421. Therefore in a locality where there are *Glossina palpalis*, but which is not a breeding place, it might be possible to construct an artificial breeding place and so attract the fly and trap the pupae?—Yes.

1422. Has that ever been tried?—Not that I know of.

1423. Do you consider that it would be worth while trying, and that it ought to be tried?—Yes, but it never occurred to me; I think it might be tried.

1424. Have you any evidence that *Glossina palpalis* is attracted by odours, apart from the odours of mammals, I mean?—No; but I tried to get evidence that it was repelled by odours, and I was unsuccessful; that is to say, I tried several very strong-smelling things which would keep mosquitoes off, but nothing would keep the tsetse-fly off. On the other hand, they will come and perch on a canoe pulled up on the beach—a large black mass—I do not know whether they think it is a hippo. It is also an old observation that a moving thing will attract them, whereas a stationary thing will not; so that I think sight has a good deal to do with it.

1425. I was rather thinking with reference to these breeding places, whether there is any particular odour there which attracts the pregnant flies?—No, I should say not.

1426. Simply the conditions?—Are you thinking of some particular plant?

1427. Or some odour given out by the dead tree or whatever it is?—No, I think not. The ideal place at Damba was not under a dead tree but under bushes. The ideal place at Sesse, where I was, was under a dead tree. In both places you had the shade, you had the loose sand, and so on, the conditions being similar, but provided by different agencies.

1428. There was no such thing as a distinctive odour given out by the vegetation?—I did not notice any.

1429. There might have been, I take it, perceptible to the fly, although not to the human nostril?—Certainly.

1430. I think you said you had no evidence of migration from Damba, or any other island, to the mainland?—It has not been tried, but I think it unlikely, because this *palpalis* is a shade-loving thing; it does not seem likely to fly over 10 or 15 miles of open water in the daytime, and there is no evidence at all that it moves at night. That is what I hope to be able to work upon when I go out again.

1431. You are acquainted, I suppose, with the method of trapping *Glossina palpalis* by means of black cloth smeared with bird lime. Have you tried that?—Not personally, but I know of it. It seems to me it would be useful in a localised area, but not any use when flies come in from surrounding areas. In a small island cut off from others I think one might hope to do something in that way in addition to catching by hand.

1432. (Professor Newstead.) Do you know if the tsetse-fly, *Glossina palpalis*, could reproduce its species on a purely vegetarian diet?—I think probably not, as observers have never been able to get it to feed on fruit and vegetable tissues in captivity. I took a piece of fruit down to the fly ground one day and sat for two hours watching it. The flies settled on it but would not feed on it, although mosquitoes did. I think it is probably only a sort of last resource on the part of the fly.

1433. I think you said that the *Glossina palpalis* feeds very largely on certain lizards—*Varanus*, I think, is the genus?—Yes.

1434. Does that particular genus of lizard occur at any great distance from the river?—I cannot say, because all my work has been on the shore of the lake

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and it is essentially a water-frequenting species. When alarmed it takes refuge in the water.

1435. Then you think it is a species which is more or less confined to the river banks?—Yes, it is a modified form of aquatic life.

1436. Have you found *Glossina palpalis* feeding upon batrachians of any kind?—No, I was never able to trace batrachian blood in *Glossina*. One could do it by measurement of the corpuscles which are of large size, and unusually broad in proportion to their length.

1437. With regard to the birds, you say you found avian blood to the extent of, I think, 2 per cent.?—Yes, a very small percentage. The 2 per cent. is the proportion in all the non-mammalian blood only.

1438. What kind of birds would you consider most vulnerable to the attacks of *Glossina*?—The cormorants undoubtedly; they sit by the lake shore on trees and rocks with their wings open, and they undoubtedly offer great facilities for the fly to bite them.

1439. What about birds of prey?—I do not think so; they sit too high up, right on the tops of the high trees, and do not come down into the undergrowth.

1440. What about the smaller owls?—That is possible; I have seen them during the day in thick undergrowth.

1441. You think they might possibly attack the owls?—I think it is possible.

1442. Are those birds at all plentiful in the particular districts with which you are familiar?—Yes, one sees them fairly often.

1443. Do you find them sitting on the branches in the day time at all?—Yes, I have both seen and heard them.

1444. You have referred to the introduction of certain insect enemies of the tsetse-fly: I suppose you would take into consideration the advisability of introducing a bacterial disease, would you not?—Yes, only the difficulty is this; in the case of a thing like the bee disease it is transmitted from bee to bee by fouling of the food; one bee infects a flower and the next bee which comes to the flower picks up the infection. *Glossina* is very different; they could not transmit it from one to another through the agency of food, and the only way they could transmit it would be from sex to sex through copulation. Assuming it to be a pathogenic bacterium, if it is to attack the male, the male presumably will be rendered diseased by it, and very shortly he will not be strong enough to catch the female and copulate, because in the case of the fly it is a very violent process, the male fly storms on to the female, and they both go off so quickly that you cannot see them, and I think if the male fly was infected, and therefore sick, he would not be able to copulate, and therefore not able to infect the female, and it is the female one wants to infect.

1445. You think there would be great difficulty if you found a particular bacterium which would be pathogenic, in disseminating it sufficiently?—I do not see how one could.

1446. What about the fungoid diseases? Another member of the Committee has already put questions to you about that matter?—I have never found one on *Glossina*, having examined very many thousand wild specimens, and I have never seen a dead fly diseased by fungoid disease lying about.

1447. Do you think it would be possible to introduce a fungoid disease?—How are you to infect them? It is not even known how the ordinary household fly gets infected.

1448. With regard to the predaceous insects you have made some reference to certain fossorial Hymenoptera attacking the *Glossina palpalis*; do you think it would be within practical politics to introduce a species of Fossorial Hymenoptera from some other tropical region which might possibly attack them?—They have already got one which does attack them, but they have struck a balance.

1449. You might introduce another one perhaps: do you think it would be advisable to try this?—I did say so in the summary I sent to you, but I came to the conclusion afterwards that it was perhaps not justifiable to say it. As I say, the thing will strike a

balance; supposing at first it attacked the tsetse-fly very readily, in a short time it would reduce its numbers and then perforce it would have to attack something else to keep itself going. During that time the tsetse-fly would again increase; you would never get an enemy which would destroy the tsetse-fly. In so doing it would destroy itself.

1450. What about the internal parasites?—I have never found them and I have had hundreds of thousands of pupae through my hands.

1451. Do you think it is within practical politics to introduce a species of some kind which would attack tsetse-flies through their pupae?—I once reared a species from some other species of fly pupa which I found in the *Glossina* breeding ground, but the difficulty is to induce them to attack the *Glossina* if they do not do so in nature.

1452. On the whole you think it is not possible to introduce anything of the sort?—Not in big nature; one might do it in small cultivated plots.

1453. Some reference has already been made to clearing away vegetation, I think, outside the localised areas. It seems to me you would suggest not exactly putting the area inside a net, but that one ought, in addition to enclosing the area and driving out the game, to clear a wide area surrounding the pale of the enclosure?—If you want to make certain the flies are not coming in from outside.

1454. What width would you consider it necessary to clear outside the pale?—I think Dr. Bagshawe has said that the fly would go two miles to get food.

1455. (Chairman.) Is that *palpalis*?—Yes.

1456. (Dr. Balfour.) Reverting to this question of fungus disease, I have a suggestion to make to you on which I would like your opinion. You are acquainted, no doubt, with Rodhain's methods of feeding flies artificially by means of citrated blood in a tube with a rat's skin fastened across the mouth of it. Do you think it would be possible in that way to transmit a fungus disease artificially to tsetse-flies, i.e., by seeding the blood tubes with the spores of the fungi?—Yes, but the difficulty is how are the flies to transmit it to the other flies.

1457. You do not think you would have a sufficient reduction of flies simply by communicating the disease by these feeding tubes, so to speak?—Not in nature, I think.

1458. And you do not think you would get any transmission from fly to fly?—Supposing the female fly got it and transmitted it to her larvæ, those particular larvæ would die perhaps, but you have to infect each particular fly to kill it. You cannot get it transmitted from one to another.

1459. Do you think it would be worth while trying it as an experiment?—You could do it in the laboratory, certainly.

1460. There might be some form of the fungus which might be transmitted by copulation?—Yes, if it was transmitted by copulation. It seems to me that is the only way it could be transmitted, and even then you would get beyond—

1461. You do not think the outlook is very hopeful in this direction?—No.

1462. The only other question I wanted to ask you was this: have you ever tried to differentiate the mammalian blood cells found in *Glossina*, as regards their sources of origin?—No, they are too much alike in size.

1463. You might do it by the precipitin test?—Yes.

1464. You have not done that?—It has not been done.

1465. Would the quantity of blood be sufficient to enable you to carry out the test?—Yes.

1466. It might be worth doing in certain cases, might it not, to find out the main sources of food of the fly?—You would require a large laboratory and installation and a large number of experimental animals, so that it would be a very expensive matter, and you would require a very skilled bacteriologist. I should not be competent to do it.

1467. (Dr. Bagshawe.) In the last sentence of your summary you say: "It would be an experiment

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"worth trying, to introduce some enemy into an island infested with *Glossina*"; what had you in your mind?—It was perhaps the enemy of the pupæ, but I think it would be only an enemy on a small scale on a small island.

1468. Is it an insect?—It would have to be a hymenopterous insect, but the difficulty is to introduce it. I have never come across one. If one could introduce one into an island it would reduce the fly to small numbers, but it could not wipe out the fly altogether, one would have to supplement it with other methods.

1469. It has been suggested that one might introduce jungle fowl into an island?—There are

already fowl around the shore of the lake now, but I never saw any grounds for supposing that anything had scratched up or attacked the soil of the breeding grounds with a view to getting to the pupæ. I used to go to the fly ground every day and I always had a look at the breeding grounds and, if I made a little hole there one day it was there the next day. There was no sign that anything had attacked the breeding ground, either bird or mammal or insect for that matter.

1470. What scratching birds are there on Damba for instance—Guinea fowl?—I did not see Guinea fowl on Damba. I saw them on the mainland, but there were plenty of quails and partridges and that kind of birds.

The witness withdrew.

SIXTH DAY.

Tuesday, 4th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Mr. E. NORTH BUXTON.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Dr. W. A. CHAPPEL, M.P.
Sir EDMUND G. LODER, Bart.

Dr. C. J. MARTIN, F.R.S.
Dr. P. CHALMERS MITCHELL, F.R.S.
Professor R. NEWSTEAD, F.R.S.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
The MARQUESS OF WINCHESTER.
Mr. A. C. C. PARKINSON (*Secretary*).

Mr. A. D. P. HODGES, C.M.G., M.D., M.R.C.S., L.R.C.P., called and examined.

1471. (*Chairman*.) You are the Principal Medical Officer in Uganda?—Yes.

1472. How long have you been engaged with your work there?—Fifteen and a half years.

1473. You have been good enough to send us a proof of your evidence, and I should like to ask you one or two questions on it. We are dealing, you know, with the question of an experiment of removing from or destroying animals in a particular area, so as to see what the effect of that will be?—Yes.

1474. I notice that where you make some remarks as regards the destruction or removal of the game you define that as "all susceptible vertebrates"; do you think it would be possible to remove all susceptible vertebrates from any area in Africa which you can suggest, because "susceptible vertebrates" would include a number of small animals it would be difficult to touch, would it not?—There are very few other animals besides the game that are at present under suspicion.

1475. That is not quite the point. In a sense a rat is a susceptible vertebrate, from this point of view, that, although he has not hitherto been found infected in nature, he is capable of being infected in the laboratory?—Yes. What I meant by "all susceptible vertebrates" was the vertebrates known to carry infection in nature.

1476. Then you refer to areas becoming non-infective or clean. Assuming now that you were able to evict the principal reservoirs, the large game, how long do you think it would be before, from the disinfection of the flies, you could say that area was clean?—It is impossible to give a definite time, but I should say that the period which would actually have to pass would be the lifetime of the race of flies then existing, which might be a few months or might be a year.

1477. Have you considered, in connection with that, the possibility of an invasion of flies from some other place where there was also wild game which might infect them with trypanosomes?—I have said that it was necessary for the experiment that the migrating flies should be kept off the clearing, or that migration should be impossible to the place chosen for the experiment.

1478. Do you think that would be possible?—I think so; it would be possible over a certain area, not over a very wide area.

1479. Does not that depend on a thing, which you may have formed an opinion about, but which I feel I do not know at all, that is, the distance flies will travel?—Yes, it must depend upon that; unless I am right in supposing that they migrate only or chiefly along water-sides, I do not think it would be possible.

1480. I suppose, in saying that, you are thinking mainly of *palpalis*?—I am thinking only of *palpalis*.

1481. Because there would be no guide of that sort in connection with *morsitans*?—No.

1482. You have not had much experience of *morsitans*?—I have had plenty of experience of it, but I know no more than anybody else does, which is not very much, about the migrating of *morsitans*.

1483. Could you form any opinion, from any knowledge you have, as to the distance which *morsitans* might migrate? At present, I think the general opinion has been that we do not know?—My opinion is that there is no limit, in favourable country, to the distance. I think, if you gave them months and they followed game, they would be found to wander about from place to place.

1484. If *morsitans* (for the moment I want to stick to *morsitans*, because we have not the same knowledge about it as about the other) did in fact

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migrate quickly over a considerable area, might not that stultify the result of any experiment as regards the non-infectiveness of the fly in the cleared area?—Yes, it might.

1485. Then I want just to go one step further. You say in paragraph 6, I think, "trypanosome infection" may be carried by the human being for 10 years "or more; so that a population which has been removed from contact with fly cannot safely be restored to a 'clean' fly area for at least this 'period': do you mean that before you could really know the results of that experiment on man you must wait another 10 years?—No. I mean that if you have moved the infected population from a fly area, and then you clean the fly area, it is not safe to reinstate such a population for at least 10 years.

1486. Then your suggestion, if I understand it aright, is to clear the area of game, and remove the whole of the population: would that be a conceivable thing?—Do you mean if you "cleaned" the area or "cleared" it?

1487. If you cleared the area of wild game for the purposes of the experiment, and you removed at the same time the whole of the population, I will not say absolutely, from that area, but from the immediate neighbourhood of that area?—You could not remove them altogether; you could not keep out the game unless people went in. You could prevent people living there, but you could not clear the place of population if you were to keep the game out of it; that is certain.

1488. If the fly did not altogether disappear, must not the human being remain a possible source of infection?—That is true.

1489. In any case, whether you fenced this area, or whether you drove out game which came in, so as to keep them out, you must have some men to carry out the experiment?—Yes.

1490. Have you had any experience of both the Uganda sleeping sickness and the Nyasaland sleeping sickness?—No, I have never seen the Nyasaland sleeping sickness.

1491. Have you yourself undertaken any experiments in connection with the examination of various trypanosomes?—I have examined trypanosomes, but I have done nothing in that way for a good many years now.

1492. (Dr. Chapple.) Can you say whether you have done, so far, any effective work in stopping the spread of sleeping sickness?—Oh yes, I think there is no doubt the work in Uganda has been very effective indeed, and that the spread of sleeping sickness may be said to have practically stopped altogether for some years.

1493. To what chiefly do you attribute that?—Entirely to the removal of the population from the source of infection, from the fly, and to regulating the traffic.

1494. You have done it chiefly by removing the population from the source of infection?—Yes.

1495. Do you think that is the most practicable and most effective way?—At present it is the most practicable and most effective way, but it is only holding the disease at bay, it is not stamping it out; it is good enough till we find something better.

1496. Why would you take the population from the source of infection, instead of taking the source of infection from the population?—We could not do that, you see, so that it was our only alternative.

1497. Do you think that there is the ultimate solution of the problem?—As far as I see, the ultimate solution of stamping it out is to remove the source of infection in some way.

1498. So that the removal of the population then, is only a temporary expedient due to the necessities of the case?—Quite so.

1499. Are you looking forward to the removal of the game and the fly?—I think certainly something should be done to show whether it ought to be done on a big scale. It seems to me that sufficient evidence has been collected to show that the game is a reservoir, and that the infection does not die out when the

people are removed from the places where there is game.

1500. Is it not obvious that if taking the population from the game protects the population, the taking of the game from the population would also protect the population? What you want to do is to separate the population from the game, which is the reservoir?—I want to separate the game from the fly.

1501. Do you not associate the game and the fly together? Are they not always associated?—Not in the case of *palpalis*.

1502. If you could separate the fly and the game, you would not require to shift the population; is that what you suggest?—It remains to be seen, but it is quite possible that, after a certain number of years, the population might go back.

1503. Go back in what sense?—Go back to where there is fly. It seems to me there are only two alternatives; either we must destroy the fly, or we must take away from the fly every possibility of keeping up its infectivity.

1504. That is to say, either destroy the fly or destroy the game?—I would not go quite so far as that, but, so far as we can see at present, it is the game principally which keeps up the infection in the fly when the population is removed. We have removed the population and the fly is not yet disinfected, and I think I go so far as to say this, that we ought to experiment to show whether the fly can be entirely disinfected by removing the game also.

1505. What other removable source of infection has the fly besides the game?—I do not know of any.

1506. So that, so far as our present knowledge goes, the game is necessary to the infectivity of the fly?—Yes.

1507. So that if you either removed the game, or removed the fly, you would remove the disease?—I think so—yes.

1508. Is not that sufficient data from which to proceed; admitting these things, have we not sufficient data upon which to proceed?—You mean towards the destruction of game?

1509. Yes.—I think we have sufficient data to justify an experiment to show that we are right in thinking that the removal of the game would entirely clean the fly.

1510. Is not the removal of the game a process which in itself is in the nature of an experiment which would demonstrate, as you proceeded, the success of the destruction of the game?—Quite so, but the difference in my mind is one between destroying the game in a certain limited area and destroying the game wholesale as a matter of routine on account of fly.

1511. If you started destroying the game in the immediate vicinity of populous areas, would you not see, as you proceeded, whether that experiment was successful or not?—I think that you would get data in that way, but I do not think I feel inclined to offer a very decided opinion on the matter. I think it quite possible that you might get data much more quickly by carrying out a definite experiment.

1512. Would you suggest the carrying out of one isolated experiment, notwithstanding your belief in the data you have just announced, and allowing other centres of population to submit to the infection?—I am afraid there is a misunderstanding somewhere. I am speaking of sleeping sickness in Uganda, where the people are not subjected to infection now; they are away from it. The question is whether, by removing the game, having removed the population already, we shall then remove the last source of infection of the fly.

1513. Do you suggest that an experiment, the extermination of the game in an isolated area, should be conducted in Rhodesia, or are you suggesting that it should be conducted in Nyasaland?—I am not saying where it should be conducted, but I think it should be done.

1514. What would you hope to prove? I confess I am a little mystified about this. What would you hope to prove by an isolated experiment which you have not yet proved which would be of practical

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import, because that is the ultimate end of this Committee?—I think we should prove, without searching through all the small animals and birds which exist in the forest, whether the removal of the game removed the infection. It is very difficult to prove that by going through all the fauna of the forest.

1515. If you have proven or demonstrated that certain game are the reservoir of the trypanosome, could you not proceed at once to exterminate that game in the immediate vicinity of populous areas?—Yes; I think that should be done now.

1516. Do you think the matter is urgent?—Yes; I think it should be done everywhere round populous centres and traffic routes.

1517. (Mr. Rothschild.) I would like to put one question which, I think, will provide the answer which Dr. Chapple is trying to get at. I think what you intended to say to Dr. Chapple was this, that in spite of your knowing that the big game was a reservoir it had not been proved that it was the only reservoir, and therefore you thought an experiment would demonstrate whether it was the only reservoir or not, and if it was the only reservoir it would justify the destruction of the big game?—Yes, that is so.

1518. (Dr. Chapple.) Is it impossible, then, for you to continue experimentation if, at the same time, a war of extermination has been made upon the big game? Would it in any way vitiate your experiment if, simultaneously with that experiment, this Committee were to advise, and the Colonial Office were to carry out, a war of extermination?—I think if that were decided on the experiment would scarcely be necessary.

1519. Do you think that the conditions are such that we would be justified in delaying in order to make the further investigation which Mr. Rothschild suggests, if you want to discriminate between those game which carry the trypanosome and the game which does not?—No; I must answer from the point of view of Uganda, and, so far as we are concerned, there would be no risk, I believe, at all in delaying for experiment, but, at the same time, I think that round the populous centres, the stations, and along the traffic routes, all restrictions against game destruction should cease as soon as possible. I think the game should be driven away from the traffic routes and the population centres.

1520. Do you draw a distinction, then, between systematic extermination and a simple driving back of the game which would come from the relaxation of the game laws?—I think it all helps; I do not draw any great distinction, it is merely a question of degree; if you shoot along the roads the game goes away a certain distance, some miles.

1521. That is to say, the game gets shy?—Yes.

1522. Is it your experience that the fly follows the game?—The *morsitans* type does. The *palpalis* chiefly resides along the water-sides, wherever the game is, and it attacks the game as it comes to the water-side, and follows it a short distance.

1523. Do you find *palpalis* remaining, although the game disappears?—Yes.

1524. And remaining as numerous, although the game disappears?—So far as we have been able to tell at present, yes.

1525. What, then, is its source of food?—I think it is omnivorous; anything cold or hot blooded it will feed on.

1526. Have you been able to keep *palpalis* alive on any vegetarian diet?—No.

1527. Have you tried?—Yes.

1528. And failed?—Yes.

1529. Did you arrive at the conclusion, then, that it would be impossible for *palpalis* to live unless it had some animal diet?—I have not done enough work myself to say, but I believe that is the conclusion.

1530. Have you formed any conclusion as to *morsitans* in that direction?—I have not worked with *morsitans*.

1531. You have formed no opinion?—I believe the same thing holds, that you could not keep them alive with vegetable juices.

1532. Have you formed an opinion with regard to the disappearance of fly when the game disappears, in the case of *morsitans*? Does *morsitans* disappear when the game disappears?—That I only know from hearsay. I have never seen anything of the kind happen because I do not know of any tract in my country where the game has actually utterly disappeared.

1533. With regard to the country of many of these South African places, is it not a matter of general knowledge that when the rinderpest exterminated the game the fly disappeared from these areas?—Yes, I believe it is.

1534. Have you formed any opinion—could you say from the habits of these flies that they were likely to disappear if the game disappeared?—*Morsitans*, yes; I should think they would.

1535. (Sir Mackenzie Chalmers.) Your work has been administrative, not research work?—For the last seven years it has been administrative.

1536. And you have been concerned with the removal of the population from the *palpalis* area?—Yes.

1537. What is the mortality at present from sleeping sickness in Uganda?—It is very small; I cannot give you the percentage, but I think there were only 200 deaths last year or perhaps not quite 200. This figure is for Buganda Kingdom. For the whole Protectorate it would be nearer 2,000.

1538. In what population?—Two millions, perhaps.

1539. How are those deaths accounted for if you have removed the population from the *palpalis* area? Are they old cases, or what?—I think, as far as we can tell, they are mostly old cases, and some of them come from areas where the population has not been removed for so very long.

1540. Have you got *morsitans* in Uganda to any great extent?—You mean in the Protectorate?

1541. Yes, under your jurisdiction, whatever it is?—There are three parts of the Protectorate where it is quite common.

1542. Do you have sleeping sickness there?—No, only where *palpalis* exists also.

1543. The *morsitans* there is not infective?—Not so far as I know.

1544. Did you examine the antelopes there? Do they bear pathogenic trypanosomes?—Pathogenic, yes, to domestic animals.

1545. But not to man?—Not as far as we know.

1546. Morphologically, are they distinguishable from the pathogenic?—I am afraid we have not quite done enough upon that to say, but there is a report being submitted which tends to show that there is something very near the human parasite which has been found in the game.

1547. But you do not have sleeping sickness?—That covers rather different ground. We do not have sleeping sickness unless there is *palpalis* as well. That is very definitely shown in Uganda.

1548. You attribute the Uganda disease really to *palpalis*?—Yes.

1549. And you acquit *morsitans* in Uganda?—All the evidence hitherto goes to show that *morsitans* does not carry human infection.

1550. Can you say in any way why *morsitans* should carry it in Rhodesia and Nyasaland and not in Uganda?—There is no reason I know, and it is quite possible that isolated cases may have occurred and have not been seen, but I think if there were many they would have been found by now.

1551. Do you suggest that a game-clearing experiment should be carried out in Uganda, and if so, is that to be in the *palpalis* area or in the *morsitans* area?—I think it would have to be a separate experiment for the two; the conditions are different.

1552. Could you fix on any likely sites where the experiment could be carried out effectively?—I could find sites or describe them in Uganda, but it would be difficult to put them into terms which could be followed.

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[Continued.]

1553. What kind of site would you recommend, and how would you proceed? What is your notion?—I am afraid I have not thought the whole thing out. The experiment with regard to *morsitans* would be a very difficult one to carry out.

1554. You mean it spreads over such a wide area of country; it is not confined to a special kind of country like the *palpalis*?—No, it is a wandering fly.

1555. Is it known how far it wanders?—No.

1556. But in your opinion it wanders widely?—I think so.

1557. What is the experiment you would suggest in Uganda with regard to *palpalis*? Could you give us any details?—I had better give you the conditions which would have to be present in the area.

1558. Yes.—First, you would have to have a coast line or shore where there is plenty of fly and also plenty of game, and the fly is known to be infected at the time the experiment commences.

1559. And also sleeping sickness?—Not necessarily, because you see we have already moved the sleeping sickness and the fly continues infected with the same infection, as we believe, through the game. It would not be necessary to send any people back there except for the purpose of carrying out the experiment.

1560. What sized area would you take, and how would you proceed? Would you fence it? I want to get at what your idea of the experiment would be?—You would mark it off by fencing for the *palpalis* experiment. You would clear also wide belts on each side to prevent migration of the fly; that is one thing.

1561. How wide must they be?—From 300 to 500 yards, I think, would be quite wide enough; that is about a quarter of a mile, say.

1562. You think *palpalis* would not cross that?—It does not as a rule.

1563. Then it would not be a very expensive experiment there?—Not so expensive as the *morsitans* experiment.

1564. You have got the lake on the one side; you would have a clearing on the right and a clearing on the left, and behind nothing?—Behind I do not think it would be necessary to clear at all, unless you wanted a large area.

1565. The fact is that *palpalis* does not go many hundred yards away from its own water?—That is so, as a rule.

1566. What length of strip would you desire to make a clearing for an experiment you could in any way rely on?—If one had a favourable place, say, containing a drinking place for game, you would not want a large area. I think a mile would probably be quite sufficient.

1567. Would you clear out the population?—I should presume they had already been cleared.

1568. There would be no source of infection from human beings?—No. The people sent in for the purposes of the experiment would have to be medically examined.

1569. How would you test the experiment afterwards? You would clear away the game; but how would you test the infectivity of the flies afterwards?—That would have to be done in the same way as it is tested now; it would have to be done by animal inoculations.

1570. The ultimate test cannot be applied, of course, that is man?—No.

1571. That is quite impossible?—Yes, that is impossible.

1572. So that there would still be a lingering doubt at the end of the experiment whether you had cleared away the trypanosome which was infective to man?—I think for all practical purposes it would be conclusive.

1573. Have you thought at all what would be necessary in the *morsitans* area, or is that outside your province?—I do not myself know enough about the habits or the bionomics of *morsitans* to lay down exact conditions, but I am quite sure you would have to have certain conditions, which would be very difficult to carry out completely.

1574. An experiment has been suggested to us, and from your long experience in Africa, perhaps you could give us some opinion about it: there is a road

called the Domira road in Nyasaland, which passes through 20 miles of fly country; there is a population of about 3,000 people on the sides of the road, I understand, and it is suggested as an experiment that the game should if possible be driven out from five miles on each side of that road and a fence erected to prevent their coming back?—I think that would be quite practicable and not present much difficulty and would be of value, but of course there are many things connected with it. If you were to have a perfect experiment to test infectivity before you began and afterwards at intervals, and all that sort of thing, it would be very difficult and would take a long time; if you just drove the game back, killing some and driving the rest away and fencing it out, that is perfectly easy and ought not to cost a great deal of money.

1575. Do you think it is easy to put up an effective fence for 20 miles on each side of a road?—Yes, such fences are put up round farms in East Africa—a barbed wire fence. Of course, you cannot fence elephants out.

1576. If elephants broke through the fence, would you not get all the other animals back as well?—Yes, I am afraid you would.

1577. Is it easy to drive them away some miles permanently?—They are very difficult animals to deal with.

1578. Is it a practicable experiment?—I think it is quite practicable. You would have to look out for the elephant breaks and mend them up as they do with the telegraph wires. We keep up our telegraph wires in spite of the elephants.

1579. You would have continually to be patrolling the fence?—Yes.

1580. How would you get rid of the animals which went in after the elephants? If an elephant makes a hole in the fence a lot of other animals will crowd through, will they not?—I think not; I think once they got into the habit of staying out they would not come back unless there was some temptation in the way of better food or something of that sort.

1581. Do you think five miles is a sufficient belt on each side of the road to make the experiment a successful one?—It depends on what the experiment is to show. If it is to show the effect on the users of the road and the cattle driven along it, and so on, I think it is quite sufficient.

1582. Have you formed any opinion as to the cost of such an experiment?—I am afraid I could not do that.

1583. Who could?—The local authorities in Nyasaland would be able to give an idea of what it would cost, if they were told what work was to be done.

1584. The object of the experiment would be to show either that the fly remained behind and ceased to be infective, or that the fly followed the game?—Quite so.

1585. And ultimately one could only judge as regards man by men using the road again?—Yes. There is exactly the counterpart of this road in Uganda, a 20-mile belt of *morsitans*, which I had in mind; this perhaps is a suitable place for experiment with regard to *morsitans* but not as a complete experiment with examination before and after. There is, however, no known human infection and little, if any, population.

1586. Why not have an examination before and after?—Because the whole thing would take years to get data that would really be conclusive bacteriologically, but to protect the road, drive back the game, and in all probability turn away the fly as well, would take a very little while and could be carried out at once.

1587. (Dr. Chalmers Mitchell.) How long is it since the population was cleared away from the infective lake shore in Uganda?—The removals began in 1907, I think, and lasted until the end of 1908, and I think the islanders were removed in 1909; there have been smaller removals going on since.

1588. So that for four or five years there has been a pretty effective clearance?—Yes.

1589. But the fly still remains equally abundant?—Yes.

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1590. Do they still remain equally infective?—Unfortunately we do not know the percentage of infectivity at the time the population was removed because the work of testing could not then be done. The percentage of infectivity has, however, decreased.

1591. But they still remain infective?—Yes, they still remain infective.

1592. How do you prove their infectivity?—It is proved by the inoculation of susceptible animals.

1593. Such as?—Such as monkeys and rats; they have been the chief animals used.

1594. You have seen a great many cases of the Uganda sleeping sickness, of course, in human beings?—Yes.

1595. It is a disease which is quite definite, is it not?—Yes.

1596. Not difficult of diagnosis?—I am afraid it is difficult in certain stages, because at certain stages the person may appear to be well.

1597. I did not quite mean that you can tell the moment they are ill.—If you see a case through for any length of time it is quite a definite type of disease.

1598. Do you happen to know the Nyasaland disease?—I have not seen it.

1599. So you cannot tell me if the type is closely similar or suggests a difference?—From what I have read about it it seems to be a more acute disease—I think that is all I can say without having seen it—a more rapid disease.

1600. You have seen this Uganda sleeping sickness in animals, of course—in monkeys?—Yes.

1601. Is it very much the same picture in monkeys as in human beings?—There are very great differences. In some monkeys you get a picture almost like the human sleeping sickness, but in other experimental animals there is very little resemblance clinically.

1602. Then in rats?—In rats it is different again.

1603. What I am trying to get at is really this. When your flies are proved to be infective by giving a certain disease to certain laboratory animals, are you quite sure that you are giving them the same disease as human sleeping sickness?—I think we feel quite sure, but I am not a bacteriologist and I take the bacteriologist's word for it.

1604. I do not want the bacteriologist's word.—I am quite satisfied that the bacteriologist is right.

1605. Excuse me, but I do not want that from you; I ask you as an expert on the disease, on clinical symptoms of the disease. You are watching the character and the history and so forth of the disease, and you are quite satisfied, if you see a case sufficiently long, that you cannot mistake the Uganda sleeping sickness for anything else in a human being?—Yes.

1606. Are you quite sure that you cannot mistake it for anything else in a monkey?—I am afraid I do not quite understand what you want.

1607. I am sorry, but I will put exactly what is in my mind; I want to be satisfied, apart from morphological evidence, that when you say the fly remains infective it remains infective with the Uganda type of human sleeping sickness?—I am satisfied in this way: if you take a fly infected from a human being with sleeping sickness and another fly infected from the forest and inoculate them both into susceptible animals, say the monkey, if you get the same disease from both flies in the monkey, then the infection is the same infection and it is the human infection. That has been done.

1608. Then, when you say the fly remains infective, you mean that it produces in the laboratory animals a disease which you recognise to be as nearly as possible identical with the disease which the infecting animal would have received from a known case of sleeping sickness?—Yes.

1609. You are satisfied of that?—Yes.

1610. You know, of course, that a good many people think that the disease which is called nagana is simply a general native name given to various kinds of wasting diseases?—Yes.

1611. But notwithstanding that you are quite clear there is only one kind of disease you can see in

various but identifiable phases in man and animals?—Yes, I am convinced there is one disease produced by the trypanosome which is called *gambienne* in man and animals.

1612. That is different, because you admit that to identify the trypanosome called *gambienne* is a very difficult morphological feat, is it not?—No, I do not think it is so very difficult; it requires expert knowledge, but I should not call it very difficult.

1613. You are quite certain that an expert, which you at present do not profess to be, from the merely morphological point of view, would have no difficulty or no reasonable difficulty in identifying the trypanosome known as *gambienne* wherever he found it?—I have no doubt he could identify it under reasonable conditions. I mean that he could recognise the type, not that he could necessarily diagnose individual trypanosomes in a given specimen.

1614. Morphologically?—Yes.

1615. And this supports you in the view that it is the same disease in man and animals?—Yes.

1616. (Mr. Rothschild.) We have had evidence before us that *palpalis* will feed ravenously on other creatures than the so-called large game animals. Do you not think that, supposing the extermination or driving away of the big game out of the fly areas must necessarily take several years before it is complete, the fly in the meanwhile will be able to transmit the infection to other reservoirs not at present existing, such as baboons, foxes, and animals of that sort?—I do not think that a new reservoir is likely to be evolved in the course of a few years. Do I understand that I am asked whether new reservoirs are likely to arise if the game is driven out?

1617. What I meant to say was that at present, with the exception of the large lizards and crocodiles and certain birds, the tsetse-fly, *palpalis*, at any rate, and *morsitans*, prefers the large animals and human beings to the smaller animals; but do you not think it possible that during the destruction of the game animals the biting of other animals by the tsetse flies would infect them and they would naturally become reservoirs, because as you reduce the favourite food of the fly it must search for something else. I understand that if you carry out an experiment on a small scale and drive out or destroy the game very rapidly, which you say could be done, this would not be so likely to arise, but if you start exterminating or driving out the game over a very large area it would take a considerable time, and during that time the fly would find means of inoculating other creatures. As far as I can gather from the various witnesses who have been examined before, the bulk of the experiments on monkeys have been carried out from other monkeys than baboons, and the fact that the other monkeys died extremely rapidly does not at all mean that the baboons would do so, because they are much more powerful animals?—As far as my knowledge of the experiments go, I believe I can say that no baboon has been infected yet with *gambienne*.

1618. But they have never tried?—Yes, they have.

1619. They infected the baboon with scarlet fever from a human being, so why not with the other thing?—Sir David Bruce brought home a baboon with him from Uganda which had been experimented upon certainly for a year and was never infected.

1620. It had the trypanosome in its blood?—No.

1621. (Sir Stewart Stockman.) In saying that a trypanosome infection might be carried by a human being for 10 years, do you mean that the human being becomes salted, as it were?—No, I mean that the infection has been known to last for that time.

1622. The real question is, do you consider man a reservoir of any importance?—No, not from the epidemiological point of view. I look upon man as a temporary and game as a permanent reservoir.

1623. (Mr. Austen.) You refer in paragraph No. 1 of your evidence to game and other wild animals which form reservoirs for trypanosomiasis or trypanosome infections: were you referring to any particular animals when you said "other wild animals"?—I was thinking only of the wild pig, but there are possibly

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others which may be found by this Committee to be reservoirs.

1624. You do not know of any other wild animals which have been proved to harbour the trypanosome?—No.

1625. When you say the trypanosome infection may be carried by a human being for 10 years or more, are you acquainted with many instances in which that has been proved to be the case?—No, I know it is quite the exception.

1626. Can trypanosomiasis (I do not say sleeping sickness) be transmitted by a human mother to her offspring?—I can only say that all the experiments I have ever read about go to show the contrary. I believe that all experiments tend to show it cannot be transmitted.

1627. As regards those experiments do you mean that new-born children of mothers infected with trypanosomiasis were examined and found not to contain the trypanosome in their blood?—I have no data. It is very unusual, in my experience, for an infected woman to bear a living child.

1628. You were not referring to experiments of that sort?—No, I am referring to animal experiments.

1629. Would systematic extermination of big game be practicable in Uganda?—Total extermination, I should say, would be quite impossible, but extermination of game in the parts of the country that chiefly matter from the point of view of traffic and residence, I think would be quite possible.

1630. (Chairman.) Do you mean large game or all game?—I mean antelope generally.

1631. (Mr. Austen.) It would be quite impossible to kill off all the animals?—Quite impossible, or all the antelope either.

1632. Are you aware that something of that kind has been suggested with regard to Rhodesia?—Yes.

1633. But whatever may be the case in Rhodesia it would not be possible to carry it out in Uganda?—It would be possible. I am not quite sure what you want me to say.

1634. You have practically answered my questions before, I know, but would it be possible to kill off every individual of every kind of antelope and everything else that is commonly known as big game in Uganda?—It would not be possible, I think, and it would not be necessary.

1635. I did not ask about the necessity, but whether it would be possible?—I do not think it would be possible. In the course of a century or so you might do it, but the only way you could kill off game entirely is by replacing it with either human population or domestic animal population, something to drive it out.

1636. You said something about the disappearance of *Glossina morsitans* as the result of rinderpest in game. Are you aware that a case has been reported on competent authority in the north-eastern Transvaal in which at the time of the rinderpest epidemic of 1896 and 1897, although all the game was not exterminated, although a very considerable remnant of game remained, *Glossina morsitans* itself disappeared? Are you aware of that case?—No.

1637. Would you admit that if that were the case that would tend to show that it would not necessarily follow that if the game were exterminated *Glossina morsitans* would go too?—Yes, I admit that; I admit it apart from that. I do not think it follows that if you exterminate game you exterminate *morsitans*. You might in some places; it all depends on what other food supplies they have, and so on.

1638. Have you any evidence, of your own knowledge, which throws any light on that question either way?—The question of the connection between the *morsitans* and game?

1639. Yes, the probable disappearance or non-disappearance of *morsitans* subsequent to the disappearance of game. Have you any personal knowledge of that from your own experience?—No, I have no experience of the destruction of game.

1640. Or the driving of it away?—No, only the natural movements of game, and then the fly appear to me to follow the herds to a great extent.

1641. (Sir Edmund Loder.) Do you think there is any island in the big lake that would make a good place for an experiment of clearing the animals, for that would be the easiest method?—Yes, but I do not think we could get the conditions very well, and it is very difficult to use one island or to admit any access to it or allow communication with it without opening a whole group.

1642. There is no one island isolated by itself?—No, they are all very much together in groups.

1643. Do you know whether the crocodile, or any of the big birds on the lake, the cormorants, for instance, have ever been found infected with trypanosomes?—I do not think they have; so far as I know, they have not.

1644. They have been examined?—Over and over again.

1645. *Palpalis* feeds on them?—Yes.

1646. But does not infect them, as far as you know?—That is so.

1647. (Mr. Buxton.) Did I understand you to say that you know the areas in Uganda where the *morsitans* exists?—Yes.

1648. And the game exists?—Yes.

1649. And that game is infected?—It is infected, but with what particular trypanosome infection I have no precise data as yet.

1650. It has not been examined for that purpose?—It has been examined in one district, but the report has not yet come in.

1651. Supposing the *gambiense* is present in the game it would be rather a curious thing if the natives were not infected by the disease?—Yes.

1652. It is therefore rather important to ascertain that?—As a matter of fact, in the area I am speaking of there are either few or no human inhabitants.

1653. Does that, to your mind, account for the fact that the disease has not been found?—No, I think there is some other reason than that.

1654. However, it has not been ascertained?—I am afraid I do not quite follow the question.

1655. I mean that the disease conceivably might exist there without its being observed?—You mean the Rhodesian disease?

1656. Yes.—Yes, it is quite thinkable it might be there, but I think it is quite impossible that it should be there to any great extent, in epidemic form. There may have been in the course of time sporadic cases.

1657. I understand in exceptional cases you have known the infection to last for ten years?—Personally I know of one case in which the infection was almost certainly present for nine years, but the other cases I am thinking of I have not seen myself, I have read about them.

1658. Would that be ten years from the time the natives were removed from their original habitat, or 10 years that that case was suffering from the disease?—Ten years that the case was known to be infected, known to carry trypanosomes.

1659. For those ten years I suppose that case was a reservoir?—Yes.

1660. In those areas where you know *morsitans* exist in Uganda (my colleague asks me to ask this question) does cattle disease occur?—Yes.

1661. (Dr. Bagshawe.) I only have one question I should like to ask and that is, what percentage of *palpalis* is now infective on the Victoria Nyanza, from the more recent experiments?—I am afraid I could not say offhand; it is something like 1 per mille.

1662. Is it as much as that? I thought it was very much less?—I may be wrong; I have forgotten for the moment what the percentage is, but I know it is a very small one.

1663. (Chairman.) I just want to ask one or two questions. You told Dr. Chapple that the removal of the population in Uganda was only a temporary expedient. I did not quite follow the meaning of that. —What I meant to say was in relation to stamping out the disease it is a temporary measure. Removing people from the source of infection may stamp out the disease as long as we keep them away, but in the meantime we

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are letting a lot of the best land in the country lie idle.

1664. What you contemplate is, that if you found the fly free from infection, or if the fly disappeared, this would be really a temporary measure, and the population could go back to its old quarters?—Yes, we hope some day that something will be discovered which will enable the population to go back.

1665. I want to ask you about something I did not quite follow in your answers to Sir Mackenzie Chalmers. You said that in Uganda, as far as you are aware, *morsitans* does not carry infection?—Yes.

1666. Has the *morsitans* fly in that area been examined?—Yes.

1667. Do you know what, if any, trypanosomes it has which carry infection?—It carries trypanosomes pathogenic to cattle.

1668. Can you say what trypanosome it is?—It carries *vinax*, *pecorum*, possibly *nanum*, and I think another, *ingens*.

1669. Does it carry *gambiense*?—It has not been found, so far as I know, to carry *gambiense* in its natural state, but the examinations have not been very many so far.

1670. Does it carry *brucei* or *rhodesiense*?—I should think, no doubt, it carries that type, but it has not been reported so far as I know yet to carry *rhodesiense*. There is a report, I believe, at the Colonial Office now which shows the latest work upon that, but I have not seen it myself.

1671. In that area there is wild game?—Yes.

1672. In that area there is *morsitans*?—Yes.

1673. Can you account, apart from the sparsity of the population (there is population, I understand), for the absence of human sleeping sickness?—No, I cannot; there is no reason so far as we know.

1674. Can cattle live in that area?—No; there are no cattle actually in that area (i.e., the area referred to in answer 1585).

1675. You have the conditions in that area that cattle cannot survive, but man is apparently unaffected? I say "apparently" advisedly. — Yes, I think we may say that, practically speaking. There are no people living in the area, but there are plenty passing through.

1676. We are told, you know, that in Nyasaland, where the native villages are, there is no *morsitans*, so that where the natives would meet the *morsitans* would be while working in the bush and in the fields outside?—Yes, or passing along the road.

1677. That would be the same whether it was in this region of Uganda or in Nyasaland?—Yes.

1678. The same conditions really would exist, subject to the population being less?—Yes.

1679. You were in Uganda, I understand, before the epidemic?—I am not quite sure. We do not know when the epidemic began. I was there before the epidemic was discovered.

1680. That is quite sufficient for my purpose, but up to the time of the epidemic, whenever it began, the *palpalis* was there?—Yes.

1681. And the wild animals were there?—Yes.

1682. What, in your judgment, either from what you know yourself or from what you have learned, was the apparent cause of that outbreak?—My opinion is that it was due to the opening up of the porter traffic from the Nile.

The witness withdrew.

Professor E. A. MINCHIN, F.R.S., called and examined.

1693. (Chairman.) You have been good enough to furnish us with a proof of your evidence, and also we have before us the address you gave to the British Association on the 15th September of this year?—Yes.

1694. They are both of very great interest, if I may say so. I would like to take your proof and ask you this: in the first paragraph of your proof you raise the point as to whether, assuming the game to be a reservoir of the sleeping sickness trypanosome both

1683. By what agency was it carried?—By human agency.

1684. And as far as you know, from inquiries prior to that occurrence, was there sleeping sickness in Uganda?—So far as I have been able to get from natives by inquiry there is nothing to show it was there before. They have no definite history of anything of the kind, but that is not conclusive when you know the native. His memory goes back a very little way.

1685. (Mr. Buxton.) From what source do you think this epidemic came?—From the west, that is to say, from the Congo. I think the trypanosome was a new factor introduced.

1686. (Sir Mackenzie Chalmers.) And it was introduced from a human being, so to speak, into the antelope?—Yes, that is a possibility. It is impossible to say.

1687. Do you think probably the antelopes were free from *gambiense* before it was introduced by human carriers?—Of course it is a very difficult question. From one point of view it would be more likely that the antelope would have it first, because they are not harmed by it, but on the other hand, as far as we have been able to account for it yet, the only method of infection we can think of is human agency, because there has been no movement of game from infected areas.

1688. Do you think the main spread of the disease in Uganda was from man to man *via* the fly, or from game to man *via* the fly?—Undoubtedly from man to man during the epidemic.

1689. (Dr. Chapple.) Is it not your opinion that the immunity enjoyed by wild ruminants compared with the susceptibility of domestic ruminants suggests that that immunity over ages and ages has been acquired by the native game?—I do not know quite how to express myself, but I admit that is a very reasonable view to take of the infection of game and the condition under which they are infected. At the same time I can think of no other way of the disease being introduced into Uganda. I do not attempt to reconcile the two.

1690. Do you think it possible for a whole race of animals to acquire immunity in so short a period although in contact with the infection for only a few years?—The game might have acquired immunity elsewhere; the game might have acquired immunity to this trypanosome before it ever got to Uganda at all.

1691. You mean immunity in relation to a similar organism, not necessarily this one?—Perhaps this one; the game which is now in Uganda may have originally come from West Africa for aught I know. The animals may have acquired immunity before they came to Uganda by the ordinary migration of animals, and the disease may have come to man later—that is a possibility.

1692. (Chairman.) If I follow your answers to Sir Mackenzie Chalmers aright, you suggest (I do not ask for more than an opinion, of course) that the animals which have always been there and the *palpalis* which have always been there were both free from infection until it was brought by man from the Congo, and that then the animals which acquired the trypanosome from man became the principal reservoirs and infected men or cattle. That is the chain if I follow it?—I think that is quite right.

in man and cattle, it may not be the only reservoir, and you refer to domestic stock, I think?—Yes.

1695. With regard to domestic stock, is it not a possible hypothesis that while the enormous majority of a herd of cattle might be susceptible to infection, there might be individuals in that herd which would be tolerant or immune to the trypanosome and still maintain themselves as a reservoir?—Certainly, I should think it was quite possible that there would be every degree of variation in the extent to which cattle

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would be affected by the trypanosome; and that some might live much longer than others. Whether there would be any which would outlive it altogether one can hardly say, although I should think it extremely probable with native cattle and less likely with imported cattle. I think with native cattle it would be probable that some might maintain their existence in spite of the infection.

1696. I only want your opinion about it, as we know in some diseases in men or in animals some individuals apparently existing under the same conditions are resistant and some less resistant?—Yes, certainly.

1697. With individuals living under the same conditions there might be individual variations, as well as variations in a breed of cattle?—I should certainly think so.

1698. Have you considered the question we have to deal with, both as regards Uganda and Nyasaland, or only as regards Uganda?—I have personal experience only of Uganda. I have been in Uganda eight months and I have not been in Nyasaland; in fact, I have never been to stay for any time in a country where *morsitans* occurs. I merely passed through in going to Uganda, so that I cannot speak from experience with regard to the *morsitans* country; I can only speak from personal experience with regard to *palpalis* country.

1699. You would agree that the question as regards dealing with fly, at any rate, in Uganda and Nyasaland is very different?—Yes.

1700. *Palpalis* has only a limited area?—Yes.

1701. For *morsitans* the area is large and unknown; is that right?—Yes. So far as I can gather from reading about Nyasaland, and so forth, it is very different. As I have said in this evidence of mine, *morsitans* has a much more unrestricted range; it is not limited to such special and narrow conditions as *palpalis* is, which, of course, does make a great difference to the problem.

1702. It has been suggested in various quarters that, acting on the basis that the wild game is a reservoir of the sleeping sickness carried by *palpalis* in Uganda and *morsitans* in Nyasaland, experiments should be made in a limited area of a considerable size either to exterminate or to expel the wild game, with a view to seeing what effect it would have either on the fly itself or on its infectivity. I will ask you first whether in Uganda you think that would be feasible and sufficient to enable you to expect results which would be at all conclusive?—Yes, I think in Uganda it would be very feasible, because it would be possible to take an island, a place which was limited naturally, and to keep it under observation perhaps more easily than could be done in Nyasaland. It is not a question in Nyasaland of dealing with a fly which lives close to the water, and the fly there has a very much wider range, whereas in Uganda it is a fly which lives near the water, and therefore it would be easy to make an experiment with regard to it on an island.

1703. The objects of the experiment would be two-fold; one to see whether when the wild game disappeared the fly disappeared, the second being, if that were not the case, whether the fly which remained continued to be infective. Those are the two main objects of the experiment?—Yes.

1704. Supposing you cleared the game off, could you find any island so far from the mainland that newly infected fly might not come to the island and make your experiment in that respect of no effect?—I think it would be quite easy on the Victoria Nyanza to find an island which was at least three miles or a mile or two from another one, and much further than the fly would migrate across.

1705. Are you quite sure that is safe from the point of view of the distance the fly will migrate?—I know that when you approach an island in a canoe the fly comes out a hundred yards or so to meet the canoe, but I do not think unless some flies settled in the canoe and were carried across they would in any other way go more than a short distance across the water from the island.

1706. Do you not think there are periodic migrations much wider than the flight occasioned by their disturbances?—I have no evidence of that.

1707. I do not know if you have seen the paper written by Mr. Fiske on that subject?—I did just see it; those were rather theoretical ideas or suggestions thrown out before he saw the fly at all, were they not?

1708. If he is right, it would leave it perfectly open to the fly to go to any island?—It is quite possible that in a continental area there may be migrations of the fly across the country, but I doubt if there would be extensive migrations of the fly across water in the case of an island, because it is very different in the two cases.

1709. Take your island; supposing all the game was killed there—and when I say “all the game,” I ask this preliminary question, what do you include in game?—In an island in the Victoria Nyanza, practically, so far as I know, the only wild animal implicated is the situtunga, and it would be merely a question of destroying the situtunga. I do not think, so far as I know, on the islands of the Victoria Nyanza there is any other what one would call large wild ruminant at all implicated in the matter.

1710. That could be done, you think?—I should think quite easily in any island.

1711. Do you suggest that the smaller mammals, rats and those sort of things, could be destroyed?—I do not know; it would be rather a matter for investigation. It is very difficult to say what is on these islands. There are a great many monkeys, but, so far as I know, there is only one instance, or perhaps two, of a monkey having been found in nature infected with the trypanosome of sleeping sickness.

1712. Take it that you have destroyed substantially the warm-blooded mammals, or most of them, do you consider that that would be likely to produce the disappearance of the fly?—I do not think it would, because I am very well acquainted with an island on the Victoria Nyanza called Kimmi, where the fly swarms more thickly than I ever saw them swarm at any other place at all, and where there is no game except lake birds, which rest along the shore, and crocodiles, and things of that sort. So far as I was able to see, on that island there was no game of any kind, no monkeys, and practically no large mammals.

1713. What do they feed on?—So far as I know the flies feed on the wild birds, the divers, cormorants, &c., which rest in enormous numbers on the trees; on crocodiles, and possibly also hippos.

1714. Reptiles?—There are reptiles; the large lizards of the genus *Varanus*, snakes, &c., but in the case of the island of Kimmi, so far as I know, the water birds, the crocodile, and the hippopotami, are the chief food of the tsetse-fly, which must have a great deal of food there, because I never saw it in any other place so abundant.

1715. Have you examined those flies at all to see what their food has been?—Not personally, but I think Captain Gray, who was with me there, examined some of them, I think, without much result.

1716. Did you generally or uniformly find traces of blood of some kind, whether of bird or reptile?—No, I have not enough evidence. Personally I only paid day visits to the island, as it were, coming in the morning and going in the evening. I did not carry on extended observations there, but my colleagues did on Kimmi and on the neighbouring island Nsadi, which is two or three miles off. They conducted investigations for some time, but I do not know that they paid attention to the special point as to what blood the flies contained.

1717. I want to ask a question, not about Uganda, but about Nyasaland, for this reason, because in Uganda you have removed the population from the fly. Take Nyasaland; supposing the experiment was carried out there and the fly was deprived of the food it derives from the wild game, do you think there would be a substantial risk of that driving them into closer connection with domestic animals, and so bring this reservoir really nearer man than the present

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reservoir?—Yes, I think there is very considerable danger of that, and I have always thought so.

1718. Being deprived of one sort of food it would seek food as similar as it could to which it could get access?—I think that is extremely probable, especially in Nyasaland, where the fly would inhabit continuous country, not like flies on islands, so that it would be much more easily able to migrate in any way. I have always thought of that as a great risk. Whether it is a real risk or not one can only determine by experiment.

1719. That danger would be modified, would it not, by the fact that domestic cattle are more or less kept in cleared areas which are not convenient to *morsitans*?—Of course, that is a question of a knowledge of the country itself, which I have not got so far as Nyasaland is concerned.

1720. Assuming an experiment of this kind was undertaken, whether in Uganda or in Nyasaland, how long do you think it would be before any opinion could be formed as to its having become what one witness called a clean area, that is to say, where the fly had ceased to be infective?—That is a very difficult question to answer, because we have no accurate data at present as to how long the fly lives in nature. Of course, the length of time a fly lives in captivity is not necessarily a sure indication of how long it would live in nature. It is perfectly certain that when once a fly is infective and has the trypanosomes established in its salivary glands it remains infective as long as it lives, so that the answer to that question would be, that it would depend on the length of the fly's life. Supposing, for instance, the fly lives a year, I should think you would require quite a year to know whether the fly had ceased to be infective or not. As the fly is a very slow breeder and only produces one larva, probably at intervals of about six weeks, it means it must live a fairly long life in order to maintain its existence.

1721. That answer is subject to being clearly satisfied that the flies cannot infect each other and that a fly cannot inherit infectivity. If either of those things were possible that would upset the assumption altogether, would it not?—It would certainly; of course it is very difficult to establish negative points, but everyone who has tried to obtain positive evidence for hereditary transmission in the fly from parent to offspring has failed to obtain any. That is all one can say; there is no evidence that the fly transmits the trypanosome to its offspring.

1722. How about the fly infecting each other?—I think that is even less likely because, taking flagellates of this kind, parasitic in insects, when there is an arrangement, so to speak, for the flagellate to pass from one insect to another insect, it nearly always takes the form of cysts—encysted stages of the parasite which pass out of the insect with its faeces and contaminate the food of other insects and infect them in that way. There are a great many flagellates of this kind which are parasites of insects, and parasites of insects only, and which pass from one insect to another by what one might call the contaminative method. All that is known of the development of these pathogenic trypanosomes shows that the normal method of propagation of the parasite is for it to pass forward through the proboscis, and there is no evidence whatever of any of the stages such as one would expect if one insect was going to infect another insect. I mention that because we have in Brazil another species of trypanosome which is transmitted by a large bug, and in that case there is some evidence (and it is believed by some of the writers and authorities) that the bug can infect another bug directly without the parasite having gone through the body of the vertebrate host. In the case of the trypanosome of sleeping sickness and the tsetse-fly there is no evidence whatever of any such stages in its life, such as one would expect to exist if that mode of infection took place; and I think the presumptive case, so to speak, against one fly infecting another directly is even stronger than the case against its transmitting its infection hereditarily, because in the second case, that of hereditary infection, the evidence against it is

purely negative, that is to say, one has never found any evidence for it, while in the other case one can advance positive reasons, I think, against the infection from fly to fly taking place.

1723. Are you satisfied that the tsetse-fly is the only carrier?—I think it is the only what I might call normal carrier. I mean the parasite certainly goes through its normal development in the tsetse-fly. It is quite possible you may sometimes have the parasite persisting perhaps long enough to produce an infection in some other fly such as the *Stomoxys*; you may get a mechanical infection by the proboscis of the *Stomoxys* or some other insect. Just as in the case of the common trypanosome of rats, which is so common in London and everywhere else, it is quite certain that the normal transmitter of the parasite is the rat-flea, but if you experiment industriously for about 18 months or so with the rat-louse you may sometimes get an infection with it. One man experimented for 18 months with rat-lice and got three infections. So in the same way there may be an occasional slip from the normal in nature, but I should think it would be such a small chance that it is practically negligible. I do not think it is altogether negligible.

1724. You think that *Glossina* may be taken to be the carrier?—Yes, it is the normal carrier, so to speak.

1725. Have you seen cases of both the Uganda disease and the Nyasaland disease?—I am not a medical man and I do not come in contact with the patients. I have, of course, seen many cases of the Uganda disease, but I have never seen any of the Nyasaland disease. Not many have come to this country and I have not seen any myself.

1726. Have you yourself examined the various trypanosomes concerned in these matters, *gambiense*, *rhodesiense*, and *brucei*?—I am very familiar with *gambiense* and *brucei*. I am not so familiar with *rhodesiense*, although I have seen preparations of it. I have not myself made preparations of *rhodesiense* or studied it in great detail.

1727. Have you studied it sufficiently to enable you to form an opinion as to whether *rhodesiense* and *brucei* are distinct trypanosomes, or whether they are identical?—I have formed a strong opinion on that, if I may state my opinion in detail. When *rhodesiense* was first discovered I felt convinced that it was not merely a newly discovered species, but that it was a newly arisen species, and that it was quite possible it had one of two possible origins; either it was a strain of *Trypanosoma gambiense* which had become adapted to being carried by *Glossina morsitans*, or it was a strain of *Trypanosoma brucei* of which the normal strain dies out in man and will not live in human blood, but which by process of mutation had acquired the power of living in human blood. At that time the evidence was not very clear, and I inclined rather to the view that it was a modification of *gambiense*, but now, from recent evidence brought forward, especially by Sir David Bruce and others, I feel quite convinced that it is a special strain of *Trypanosoma brucei* which has arisen recently and which has acquired this power of living in human beings which the typical *brucei* has not got, and it is in this way an entirely new parasite of man, a fact which I think explains its extreme virulence at present. I gather from what I have heard from Dr. Low, for instance, who was in Uganda when sleeping sickness first came there, that the sleeping sickness (that is to say, *T. gambiense*) had at that time an extraordinary virulence equal to that which *rhodesiense* has now, and that in the course of time *gambiense* has greatly lost, not entirely, but partly lost its virulence. Its virulence has changed, that is to say, it does not kill people off so quickly as when it first appeared in Uganda, whereas *rhodesiense* has still this extraordinary virulence which, in my opinion, is correlated with its being a new parasite. On the analogy of what has happened with *gambiense* in Uganda I should expect that in the course of time the virulence of *rhodesiense* in human beings will be a great deal less than it is now.

1728. So that to whatever extent the sleeping sickness may exist in Uganda at present, it is likely to

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diminish rather than to increase?—I think the virulence is likely to diminish; people attacked with it will not be killed so rapidly by it as they are at present. That is from the analogy of what happened in Uganda.

1729. A question I must ask you is this: Are you proceeding on the hypothesis that the wild game in Nyasaland is a reservoir both for sleeping sickness in man and sleeping sickness in animals? You have told us that in your judgment *rhodesiense* is a different strain of *brucei*.—That is what I think.

1730. My question is this: Does the wild game harbour both those strains, and do the fly carry them, and only the original *brucei* strain produces *nagana* in cattle and only the *rhodesiense* strain produces sleeping sickness in man? I do not know if that question can be answered?—I think first of all you had *brucei* transmitted by *morsitans*, as a more or less natural parasite of ungulates and quite harmless to them, like the trypanosome of rats in London is quite harmless to rats, and that it was transmitted by *morsitans* amongst the game; and then this strain has arisen which has been carried by *morsitans* from the game to man, and which is extremely virulent to man, and which can only be distinguished by this fact—it is morphologically indistinguishable, but it is physiologically distinguishable by the fact that the new strain *rhodesiense* will live in men as well as in ungulates, and the old strain *brucei* would live only in ungulates and not in men. A given antelope in Nyasaland might contain both together, and a fly might take its fill of trypanosomes from that antelope and inoculate them into a human being. Then, specimens of the original unmodified strain, the true *brucei*, would not live in the human blood, while the specimens of the new strain inoculated would do so. It is like the way in which flowers are pollinated by an insect, and the pollen which is the right pollen grows and the other does not. I think it extremely probable that a given antelope in Nyasaland may have both species together, and they would be distinguishable by nothing but that one test, whether they would live in a human being or not, which is not a test that one can practically apply.

1731. If I follow, it is only the *rhodesiense* strain which would affect man; you also say it is only the *brucei* strain which would affect animals?—No, both together; the fly might take up both and put them into a man, and what would happen would be that the unmodified strain would die off in the human blood and the newly arisen modified strain would live in the human blood; but if the fly put them into an antelope, both the strains would live in it equally well.

1732. I want to put the converse: supposing you took the blood of a man infected with sleeping sickness and put that into an animal, that being clearly the *rhodesiense* strain by which the man would be affected, would that affect a susceptible animal?—I do not think it would affect an antelope or wild game—animals which are both susceptible and tolerant; it would live in them in the normal way but it would not upset them any more than the other.

1733. Are both strains pathogenic to animals and only one pathogenic to man?—Both strains can live in the wild game without being strictly pathogenic to them. They live with them as parasites which do not cause any upset in their health, but only one strain is pathogenic to man. The word "pathogenic" introduces a consideration which means it produces disease. These things do not produce disease in the animals of which they are natural parasites, only in those to which they are new.

1734. (Mr. Buxton.) I understand from your evidence—you will correct me if I am wrong—that you think, in the case of cattle which are more or less immune to certain trypanosomes, that immunity, where it exists, is gradually acquired?—Yes, I think so.

1735. Where you find wild game harbouring these trypanosomes with no apparent evil consequences to themselves, is that probably a very long process?—Yes, I think that is the final process in the mutual adaptation between parasite and host. The final stage

is that the parasite can live in the host and the host tolerates it—does not suffer from it.

1736. The time occupied being ages and ages, possibly?—Possibly.

1737. If you find that human beings do not seem to take the disease in great numbers, that there is no very bad epidemic, although they are exposed to being bitten by flies, a proportion of which it is presumed are infected, what inference would you draw from that? Putting it generally, what inference do you draw from the fact that we had a very fatal epidemic in Uganda, and one which is much less fatal in Rhodesia and Nyasaland?—I think it is very difficult to say. I think, for one thing, it may largely depend on the difference between the two flies, if I may say so, in their tastes. I think it possible that *palpalis* is very fond of human blood, and more fond of human blood than of ruminant blood, and that it goes after human beings more than *morsitans* does. I do not know if that is the true explanation, but I think it is, and possibly when this trypanosome was introduced into Uganda it spread with tremendous rapidity because you had a fly which was particularly keen on human blood. *Morsitans*, on the other hand, I think (I say this without having studied it living, but I think it possible), is more fond, so to speak, of ruminants' blood and would keep more amongst ruminants, whether domesticated or wild, and they therefore would probably attack man less, whereas in the case of *palpalis* it is the other way round. I think that may possibly account for it.

1738. Do you think it is possible that tribes which have existed there perhaps as long as the wild game might, to some extent, become immune as the wild game have?—That would be difficult to say. Of course, it is quite evident that this *Trypanosoma brucei* must have been put into human beings by the fly many millions of times as far back as it ever existed, and apparently, in the great majority of cases, it has never succeeded in establishing itself in human beings. Of course, it is possible with the natives of the country that it may have perhaps, from time to time in the past, established itself and died out again so that immunity was set up, but it is really very difficult to answer that.

1739. We have had evidence before us that in Nyasaland and Rhodesia, as a matter of fact, the natives are bitten by large quantities of flies, the *morsitans*; one witness, I think, said he counted 200 on the back of a native, and if a proportion of those flies is infective it is very difficult to understand how that native escaped?—It is very difficult to understand.

1740. You have no hypothesis to advance upon that?—Not on the spur of the moment. I do not know if one could think out something about it, but offhand it is certainly a very difficult question to answer.

1741. (Professor Newstead.) I am more particularly interested in the subject of the introduction of the Guinea fowl into the *palpalis* areas. I gather from your statement to this Committee that you have an idea that such an introduction might be successful?—I think it might be extremely successful in *palpalis* areas. I am not speaking of *morsitans* areas.

1742. Is it not rather remarkable that we should have existing side by side *Glossina morsitans* and the common Guinea fowl, *Numida coronata*, in such abundance?—I understand from the papers I have read by Jack that the fly carefully protects its pupae against these particular birds by depositing them in sheltered places. According to Jack in his paper, which I dare say you are acquainted with, he says, that all the ground under the bushes is scratched up by guinea fowl, and you never find a pupa there. It would not have a chance. The tsetse-fly, being a very slow breeding creature, can only maintain itself, according to Jack, by putting its pupae in sheltered places where the Guinea fowl cannot get at them.

1743. Is there not some evidence in Mr. Jack's paper, at any rate his illustrations, or some of them, seem to me to show that the pupae or larvae are deposited in places which are accessible to Guinea fowl.

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For instance, if you take figure 1, plate 18, of Mr. Jack's paper on page 361, you see the lower arrow there points to a spot which is certainly accessible to Guinea fowl?—It looks as if it was.

1744. As a matter of fact, although the records have not yet been published, the pupæ of *Glossina morsitans* have been found in similar circumstances to those you have indicated in figure 1 of Mr. Jack's paper.—Yes.

1745. Without any sign at all of gallinaceous birds of any kind searching for them, you would, without some further evidence, seriously advocate the introduction of Guinea fowl or any other gallinaceous bird into a *palpalis* area?—I certainly think I would in a *palpalis* area, because I feel sure that Guinea fowl or some other gallinaceous birds of that nature would destroy an enormous number of pupæ if they were introduced. In such an area as that, for instance, in which Sir David Bruce told me that his boys collected the pupæ for him, and brought him 7,000 in a few days, if the native boys could find him 7,000 I am sure that gallinaceous birds could find a great many more, and would eat them. I think such an area as that where the pupæ swarm is the one which should be made the basis of the experiment to see whether a diminution resulted.

1746. Would you advocate that without having some more evidence before you? It seems to me that we really need some additional evidence.—All I would advocate would be the making of experiments which would obtain that evidence.

1747. You would prove that by dissection of the birds, to begin with?—That would be one way in which it might be proved, but I think it would be better possibly by observations extending over some months to see if the fly diminished.

1748. You would rather do that than examine the crops of the birds themselves?—You would have to kill the birds to do that.

1749. I dare say, if the truth were known, both Mr. Jack and Mr. Lloyd who made these observations saw a great many Guinea fowl and never troubled themselves one moment to examine the contents of the crops. I should point out that Guinea fowl do scratch a great deal, but in the part of Nyasaland with which I am most familiar they do not scratch round the bases of trees, whereas, as you know, the pupæ of many insects occur in larger numbers under trees than in the open?—May I say one thing with reference to that? Recently there has been a good deal of examination of the crops of birds from quite a different point of view altogether in connection with mimicry in fowls, and I heard Professor Poulton say not long ago that numbers of people had just examined the crops in the ordinary way with the eyes and said there was nothing there, and that the flies had not eaten butterflies or whatever it was, but really to determine whether they do or not you must examine them microscopically.

1750. (Mr. Austen.) I gather from what you told the Chairman with reference to *Trypanosoma brucei* and *Trypanosoma rhodesiense*, that you regard *rhodesiense* as being a strain of *brucei*, which is indistinguishable from the true *brucei* morphologically, but is capable of living and causing disease in man, while the other is not?—Yes; I think that is the only test.

1751. Supposing you find a trypanosome in the blood of an antelope which looks like and presents the morphological characteristics of *Trypanosoma brucei*, is there any means of ascertaining whether, in fact, you are dealing with the ordinary *Trypanosoma brucei* or whether you are dealing with *Trypanosoma brucei*, variety *rhodesiense*?—I do not think there is, beyond the experimental one of injecting it into a human being; if he dies it is *rhodesiense*, and if he does not it is *brucei*. That is what Taute did, as I gather from his paper, on himself, and I am not sure that he did not do it with other people too. His conclusion is that there is no other test for distinguishing *rhodesiense* and *brucei* except that *rhodesiense* is the strain which will live in human beings and *brucei* will not. I would like to add that this matter, so far as nomenclature is concerned, would be greatly

simplified and made intelligible if one adopted for those two trypanosomes the trinomial nomenclature, such as is used with regard to many birds and mammals; that is to say, to call the unmodified strain of *brucei* the typical strain, *Trypanosoma brucei* or *Trypanosoma brucei typicum*, the other one *Trypanosoma brucei rhodesiense*.

1752. (Chairman.) Is not that exactly the same as Dr. Taute? Dr. Taute says *brucei* and *rhodesiense*, and that the one produces *nagana* and the other sleeping sickness; is not that which you are suggesting exactly the same thing?—I only meant to suggest that the trinomial nomenclature would make it understandable.

1753. Is it not, in other words, Dr. Taute's theory?—Yes, but it is a way of making the facts clearer by means of a nomenclature.

1754. (Mr. Austen.) I know that on this subject you speak with the very greatest authority; you are recognised as being perhaps the foremost authority we have in this country on trypanosomes and other protozoa. If you are correct, am I right in thinking that the whole case of Drs. Kinghorn and Yorke as regards the game falls to the ground, to the extent that their case is based on an assumption which cannot be maintained; that is to say, on the assumption that they have found a certain percentage of the game infected with *Trypanosoma brucei* or *brucei rhodesiense*, when they have no means, in fact, of coming to that conclusion?—Yes, I think, as Taute has already said, and Kleine also, the figure of 16 per cent. of the game infected with *Trypanosoma rhodesiense*, which is the figure given by Kinghorn and Yorke, is unreliable, because that 16 per cent. includes without distinction both what I should call *Trypanosoma brucei brucei* and *Trypanosoma brucei rhodesiense*, and for aught we know there may be 15 per cent. of *Trypanosoma brucei brucei* and only 1 per cent. of *Trypanosoma brucei rhodesiense*. It is not a reliable figure for *rhodesiense* alone because that 16 per cent. includes both, and the actual percentage of *rhodesiense* must be much lower probably.

1755. So that assuming, to avoid argument, that it has been proved that big game is a reservoir of the human trypanosome in Nyasaland and Rhodesia, we have nevertheless no means of discovering what percentage of the game is so infected?—No means whatever, unless we could use slaves for experiments.

1756. (Sir Stewart Stockman.) I think you suggested that the disturbance of the natural conditions of the tsetse-fly would be dangerous to human beings; do you also suggest it would drive them on to domestic cattle if there were any?—I think it might make them come closer round the human settlements and habitations, wherever there was cover they could take shelter in. In my experience, derived from Uganda, the human habitations are often surrounded by very dense cover. I do not wish to state positively that it would be dangerous, but I think it is a matter for investigation.

1757. What was in my mind was this: you referred to all the tsetse-flies. It is on record that settlers have driven the game out, in fact it has happened over and over again, and only after that their cattle were able to live.—Is there definite evidence that driving the game away has driven the fly away too?

1758. I think there is in relation to *morsitans*, but I want your opinion really.—I could not give an opinion; it is simply a question of fact whether it is so or not.

1759. If it is a fact, then disturbing the balance has not done the cattle any harm in those cases to which I refer?—I am not personally acquainted with all the data relating to the case, and without that I should not like to give an opinion.

1760. I merely state the fact that some of these areas have been settled by settlers and the game driven out, and they could then keep cattle, which they could not do before.—Is there any evidence as to whether at the same time clearances were made or any other steps taken?

1761. In the case to which I refer there was no clearance; it was before the days of clearance.—Such

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a thing as that does not necessarily introduce only one condition; it may introduce other conditions of which one is not aware.

1762. With regard to the introduction of domestic fowls, I was rather interested in that; do you know if the domestic fowl lives well in those places? They die off so often in some places; is it within your knowledge that they live well there?—So far as my experience in Uganda goes they live very well.

1763. The domestic fowl flourishes around these places?—Yes, I think so. One of my suggestions was that if possible one should introduce the Indian jungle fowl as well.

1764. On the ground that it might live better?—Possibly it might maintain itself better in the wilder parts. What I had in mind was, that immediately around the houses and villages the domestic fowl, which of course would not be driven away by the presence of man, would be useful in keeping down the fly, while further out, beyond the part close to human beings, the jungle fowl might be a valuable addition to the native fauna, which would also keep down the fly.

1765. (Mr. Rothschild.) One of the objects of this Committee is to inform the Colonial Office whether the evidence shows that we are justified in recommending that an experiment should be made. From your experience of the fly, and what you saw in Uganda, do you think it is a possible or probable hypothesis that if you drove out or shot out the game in an experimental area, and the conditions necessary for that experiment were fulfilled, the experiment might be successful, while, on the other hand, if the logical consequences of the experiment proving successful should be followed up, and the game be destroyed, or sought to be destroyed, on a large scale throughout the fly areas, owing to the necessarily much slower proceeding of destruction, the fly might gradually take to feeding on other creatures and introduce a new reservoir and thus defeat the object to be gained by the extermination of the present existing reservoir?—Yes, I think it is extremely possible. Of course, it is always difficult to argue from a small set of conditions to a large one. Whether or not a new reservoir could be established does not depend quite so much on the fly as on the trypanosome. I mean to say, that supposing you had an area inhabited by antelopes which could harbour the trypanosome, and by birds; so far as we know at present, these particular human trypanosomes do not live in birds; but whether, after the antelope were destroyed, the birds could form a reservoir for the trypanosome or not would depend on the trypanosome. The flies could still go on feeding on the birds, but whether they could infect them with the trypanosome and make them into a reservoir would depend on whether the trypanosome could establish itself there. The question was rather what other animals besides wild ruminants the trypanosome could establish itself in.

1766. I was not referring so much to such different things as birds, only in the course of the evidence it has been said that the great objection to the idea that there could be another reservoir in warm-blooded mammals outside the game animals was, first of all, that most of the small mammals were nocturnal; and, secondly, in laboratory experiments on jerboas, mice, and rats, the death of the creature had been produced so rapidly by the trypanosome that it formed a negligible quantity, and it struck me that owing to the necessarily much slower course of the game destruction over a large area than there would be in a small area the fly might after all have sufficient time to render some of these creatures tolerant?—Yes, I think certainly that might happen, and, of course, it is a question for local investigation to show whether there were any diurnal animals other than game which could act as a reservoir; I have no doubt monkeys, which are diurnal animals, could. On the other hand, I think monkeys are very seldom bitten by tsetse-flies, for the reason that they are much too quick; they catch and eat them, as I have seen them do.

1767. Baboons have so far proved absolutely immune?—I do not wish to take up the time of the Committee by merely irrelevant matter, but I would

like to mention what happened once in Uganda. It happened on the day that the Governor, Sir Hayes Sadler, paid a visit to our laboratory. Two tsetse-flies escaped, and one of them attached itself most persistently to Sir Hayes Sadler, greatly to his annoyance. I was walking behind him flicking it off every minute. The other one went straight for a baboon, and the baboon immediately caught it and ate it. That was the end of the other one. I think monkeys are very quick in catching these things with their hands and eating them, and I think even in areas where there has been an enormous mortality amongst human beings and an enormous quantity of sleeping sickness you never find a monkey in nature infected with it. I believe Dr. Bagshawe will probably know better than I do, but I think there are only one or two instances known of a monkey caught in nature with *gambiense* in its blood, although they live in areas swarming with infected flies.

1768. (Dr. Bagshawe.) I believe there are two cases.—I do not think there are more than two, but that is merely because monkeys are so clever in catching these things.

1769. (Dr. Chapple.) Are they equally susceptible in the laboratory?—Very susceptible. With regard to malaria, on the other hand, nearly all the wild monkeys are full of monkey malaria, but that is because the mosquitoes which carry the malaria bite at night when the monkeys are asleep. It is a matter for investigation by a naturalist, as to whether it is true that there are no small diurnal animals other than antelopes which could be a reservoir. These are simply questions of fact which want deciding.

1770. (Dr. Chalmers Mitchell.) The distinction you make between *rhodesiense* and *brucei* is, that one is pathogenic to man and the other is not?—Yes, I think that is so.

1771. And the wild antelope are tolerant to both?—Yes.

1772. How about domestic stock? Have you any idea?—I think in domestic stock you get a series. The native cattle which have been for many generations in the country, so far as one knows, are much more tolerant to the trypanosome than those which are freshly imported.

1773. Would there be any distinction in your opinion between the tolerance of *brucei* and *rhodesiense* in the case of domestic stock?—That I do not know, but I do not think so, because the domestic stock has always been killed off by nagana, by *brucei* to a greater or less extent. I think it has been found that the native cattle may live a long time with it, and have it in a chronic condition, whereas imported cattle are killed off at once, like imported horses—they die off almost at once with it.

1774. Your suggestion is that *rhodesiense* has been a definite mutation of *brucei*?—Yes, I think *brucei* has been inoculated into human beings millions of times for many thousands of years past; we know from reading Livingstone's Journals, how he and his followers were bitten by the fly and did not get the disease, while all their animals died from it.

1775. If *brucei* were stamped out once, would you expect it, on that theory, to keep cropping up again?—I do not see why an equally dangerous strain should not keep coming out there or anywhere else.

1776. You have been working at trypanosomes, or protozoa rather, ever since we were at Oxford together?—Yes.

1777. I do not remember that we knew much about trypanosomes then?—They were only zoological curiosities then, found in frogs.

1778. And the growth of knowledge about them was very slow for a time?—Very slow until they were found to affect human beings and stock, and then it was very rapid.

1779. Now there is a certain proliferation of knowledge?—Yes.

1780. At first it was supposed, roughly, that there was only one trypanosome; there was very little distinction drawn between different kinds at first?—There was the one in the frog, and various things were observed in fishes and birds, and, of course, the

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first pathogenic trypanosome to be discovered was that of surra, which was discovered about the same time as the one in the rat, *lewisii*, and they were then not distinguished clearly.

1781. When *gambiense* was discovered, was that thought to be the same?—No, it was discovered at first in Gambia and given the name *gambiense*.

1782. At that time they did begin to distinguish between them?—Yes, because that followed after all Bruce's work on nagana. That at once gave rise to a great many investigations of the fly and the trypanosome discovered by Bruce, which was named *brucei* in his honour. Then the human one was discovered. I remember myself being at the Royal Society Conversation when the first slide of *Trypanosoma gambiense* was exhibited, the first trypanosome known from human blood.

1783. It was then recognised as distinct?—Yes, it was given a distinct name.

1784. Because it had been found in man, or because it was recognised as distinct?—Chiefly because it was found in man; there were not enough morphological characters to separate them.

1785. It has been suggested to us here in various ways that all these distinctions between the different trypanosomes are a kind of hair-splitting work that may be interesting to the professor in the laboratory, but really has very little practical significance?—I think they have a great deal of practical significance. The way I have always myself regarded the matter is, that there are certain natural groups of trypanosomes each of which is probably a descendant of a common ancestor. There is the *lewisii* group that is parasitic in rodents like the rat and the rabbit, and there is the *brucei* group, which is probably much more recently descended from the common ancestor than the *lewisii* group, with a splitting up into a number of species which have not advanced so far in their differentiation as the other group.

1786. I agree with you; as morphologists we are agreed that these things are interesting and valuable, but, on the other hand, here is a great practical problem, the supposed relation between game and sleeping sickness, and it has been suggested to us that while morphologists are unable to distinguish exactly between these different trypanosomes, in the meantime man is dying, and all that sort of thing, and that the scientific case for killing off the game is practically complete. I would be much obliged if you would tell me whether you think a sufficient scientific case has been established, or if, before we make any recommendation as to destroying the game experimentally or otherwise, we should be more satisfied than we are now as to whether we are dealing with different diseases and different trypanosomes or not?—I think, so far as the distinction of the trypanosomes is concerned, we have quite enough evidence from the physiological tests as to the distinctness of the different strains.

1787. You mean by that, that probably different methods would have to be applied in the different cases?—For destroying different strains of trypanosomes?

1788. Yes.—Yes; in so far as they inhabit different hosts.

1789. If you make out the case against one trypanosome it does not establish the case against another?—No; I think each would have to be considered separately on its merits.

1790. Do you really think those distinctions are of practical importance to us in the advice we are being asked to give?—It is just from the practical point of view I should have said they were most important. I might give an example; the distinction between *rhodesiense* and *brucei* is that one will live in man and the other not.

1791. (Sir Mackenzie Chalmers.) Might I ask you on your last answer, having regard to the small number of cases of sleeping sickness in Nyasaland, is it possible they have all been man-to-man infection, and not from antelope to man, in each case *via* the fly, of course?—It is very difficult to answer that. I should think there was no doubt whatever that the

very first one of all, whenever it took place, was certainly an infection from a ruminant of some kind *via* the fly, but whether after that they have been all from ruminant to man and not from man to man I do not know. I think it is very possible, in view of the extraordinary virulence that the disease has at present, that many of them at the start have been from antelope to man. If we assume that the strain arose first of all by the trypanosome taken from an antelope and established in a man, that was, so to speak, the birth of *rhodesiense*, but it would be very difficult to say whether from that man it went back to other antelopes and then to man again, or whether it went from that man to other men. It may be more likely that it went from man to man.

1792. Is there any evidence, do you think, from what has taken place in Uganda that *palpalis* is the normal carrier of *gambiense* while *morsitans* is probably the normal carrier of *rhodesiense*?—Yes, I think so. *Morsitans* does not occur in Uganda at all. Where I was there was only *palpalis*.

1793. Dr. Hodges told us there was a good deal of *morsitans* in one part of Uganda, but there there was no disease.—I could not say; I have not been in that part. In the part I was in, round Entebbe, there was only *palpalis*.

1794. What was your special research?—I went to investigate the development of the trypanosome in the tsetse-fly as far as possible. At that time the tsetse-fly had never been dissected. I began by dissecting the tsetse-fly.

1795. I forget when the disease became epidemic in Uganda.—I went out in 1905 and it was well established then.

1796. It was supposed to have been brought by human carriers from West Africa?—Yes, I think so; that was the theory. It began about 1900 or a little earlier.

1797. The disease was introduced by human carriers. Do you think previous to the introduction by human carriers the big game were free from pathogenic trypanosomes?—Yes, at least from pathogenic human trypanosomes. They may not have been free from pathogenic cattle trypanosomes.

1798. How is it that, being a newly introduced disease into Uganda, man and the domestic animal seem to suffer while the antelope do not? Is there any explanation of that?—I think it is merely because in the antelope the trypanosomes have established themselves during so many countless ages that they have reached a condition of tolerance and immunity.

1799. But with regard to this particular *Trypanosoma gambiense*, *gambiense* apparently was introduced into Uganda at the beginning of the epidemic, and apparently you think it spread from man to the animal; but apparently, although it is a new disease to both of them, man suffers and the animal, at any rate the wild animal, does not?—Personally, my own belief is that if one goes still further back, the whole of this group of pathogenic trypanosomes came from a common ancestor, and that was a trypanosome of ruminants from the beginning, because *gambiense* and *brucei* are very difficult to distinguish morphologically, and they are much easier to distinguish by physiological tests. I think the common ancestor of the *brucei* group was originally a trypanosome of ruminants transmitted by a tsetse-fly, and it has evinced a disposition to break up into sub-species and initiate the building up of new species in this way. *Gambiense* and *brucei* are very closely allied morphologically, and they are probably sister species derived originally probably from a parasite of ruminants.

1800. But the domestic cattle, even the African cattle, suffer from it?—I think in that there is a great deal of variation; some of the native cattle are so tolerant to it as to be almost immune or practically immune, but of course it is very fatal to imported cattle, and it is simply a question of those races which have been most infected with it.

1801. Do you think there is a tolerance to it in the human race where the disease has been known long?—I think there is no doubt whatever that on the Congo, where the disease has been in existence very much

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longer, from all the data one can get from various reports and so forth, the natives are much less injured by it than they are in Uganda, where it is new. One reads of natives on the Congo having trypanosomes in their blood for years and going on working like ordinary beings, like the antelope almost.

1802. Those people being reservoirs who could infect other people?—Yes, I think there is not much doubt that in the Congo region the human species is chiefly the reservoir, especially, as I said before, as there is a good deal of evidence for stating that *palpatis* is a fly which rather likes human blood.

1803. (Chairman.) Was there much cattle disease in Uganda prior to that epidemic?—I could not say prior to the epidemic; it was so long before I was there.

1804. (Dr. Chapple.) I see in the summary of your evidence you have drawn an analogy for our guidance, and have supposed that if the Highlands of Scotland were covered with deer you would not exterminate those deer, notwithstanding the conditions that prevailed in the sleeping sickness areas?—I thought that as Scotland is now, if there were pathogenic trypanosomes both in deer and in the cattle, you would not get much further by killing the deer if you left the cattle, because the cattle are nearer to the human beings and do not avoid them, while the deer do.

1805. In this analogy have you not left out certain important and essential factors? To make your analogy scientifically correct, would you not require to also state that the deer were tolerant to the disease but immune, and would not you require to state that cattle and man were susceptible to the disease but the deer not? Would you not also require to state that there were thousands of deer to every one of the domestic animals?—Those would be all points. I did not wish to take up a lot of space by arguing the analogy at great length and contented myself with merely indicating it. There would be further facts to add which would balance what you have put forward and go on the other side, namely, that the cattle, of course, are close to the human habitations, while the deer as much as possible avoid human beings and keep away from them. If you had tame antelope around the houses they would be equally dangerous as reservoirs, and much more so than the cattle, because they would be much more efficient. Other things being equal, the deer would be more dangerous than the cattle, but other things are not equal. Whereas you have pointed out certain factors which would, so to speak, depress the scale in which you placed the deer, there are other factors, on the other hand, which would depress the scale in which you put the cattle, namely, that the cattle are all round the houses and are tame, and the deer are not.

1806. Is it not the experience in Nyasaland at least that the game surround the villages and actually invade and destroy the crops, and are much more difficult to drive off and much less timid than the deer of the Highlands of Scotland?—I could not say that without having personal experience of them.

1807. Do you not know as matter of your study of the conditions in South Africa that these facts I have stated are really true?—I was only in Uganda.

1808. Nyasaland?—I have not been in Nyasaland.

1809. Do you mean to say from your study of the subject you are not aware of the fact that the game are so tame that they invade the villages and destroy the crops of the villagers?—I have heard settlers complain of their fences being destroyed and so forth.

1810. Supposing I stated this case to you; that there are in Scotland a thousand times as many deer as there are domestic cattle, that the deer are tolerant to this disease and immune to the disease, that the deer are not of equal value to the cattle, that the disease was invariably fatal to cattle and man, and that the cattle are necessary to the existence of man, and the deer are not; would you persist in saying that those deer should not be destroyed? Do you mean to tell me that you would refuse to have the deer destroyed in a case like that?—If I thought it would ameliorate the conditions certainly I would destroy them, but if I thought it would not I would not do so. I would like to

add, however, that in my opinion, taking that case, if you do destroy the deer, leaving the cattle and the fly, and the fly, I suppose, requiring food, it would have to get nearer the habitations than before, so that I very much doubt whether any amelioration would be effected by destroying the deer.

1811. You would sooner have two reservoirs acting concurrently; you would destroy one reservoir and leave the other?—You intensify the one by destroying the other.

1812. Would you say that if the deer of Scotland harboured the trypanosome and were tolerant, but the domestic animals died off immediately they got the disease, you would not destroy those deer?—In the first place, you say that the domestic animals die off immediately.

1813. I meant that they die off invariably; we have had it in evidence that there has been no case of recovery?—How long did they live?

1814. Answer my question first, and I will answer you afterwards.—The answer to your question depends upon what I want to know.

1815. I tried to get that point out of a witness, I think Sir David Bruce. Perhaps you can tell me if it is essential to your answer what time elapses between the inoculation of an ox by a tsetse-fly and the first clinical symptom which would enable the owner to diagnose the existence of the disease?—I am afraid I do not know enough of the details, but if I may say so, what is more important to the inquiry is not the question of the clinical symptoms, but how long a time would elapse between the bite of the fly and the appearance of the trypanosomes in the cattle's blood. The appearance of the trypanosomes in the cattle's blood might considerably precede the appearance of clinical symptoms, and, so far as the problem of spreading the disease is concerned, as soon as the trypanosomes have appeared in the blood of the ox, or whatever it is, then flies by taking up that blood become infective and spread the disease. That is the crucial point, not necessarily the clinical symptoms. The whole question is how long, on the average, an ox or some other domestic animal will live with trypanosomes in its blood and therefore infective to flies.

1816. Is not the essential point the diagnosis of the disease in order that, knowing that the animal has got a fatal disease, you may kill it at once and cease its existence as a reservoir?—Certainly, from the practical point of view, that animal is a reservoir, but if it is the fact, as I think it is, that the trypanosomes would appear in the blood and would be infective to flies before you could diagnose the clinical symptoms, then, of course, there might be a period of some length during which your cow might be infecting flies without your knowing it. Most probably a European owner of cattle would make periodical examinations of the blood, and the moment he found trypanosomes he would destroy the animal without waiting for the clinical symptoms.

1817. Does not that draw a very important practical distinction between the deer running wild on the hills which lives throughout a long time after being infected, during the whole of its lifetime, and the cows which you could kill whenever you first diagnosed the disease?—Other things being equal, it would make an enormous difference; but supposing you found you had to wipe out the entire cattle of the country, would the natives stand that?

1818. In Scotland, I mean. Take the analogy. I suggest to your mind this analogy is an unscientific one, because it leaves out essential factors, and I ask you whether, if you did include all essential factors, you would still persist in the advice you give the Committee?—The only advice I give is that an experiment should be made to obtain knowledge. I do not say that they should not kill the cattle.

1819. You say in your summary that there should be no destruction of game, and that, rather than destroy the game, you should experiment with gallinaceous fowls first?—I do not think I said that; my conclusion is: "If any destruction of the wild game is to be carried out, it should be done at first

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" experimentally and tentatively in a limited area, in order to obtain accurate knowledge of the effects " produced by such measures." I think destroying the game is a way of getting at the matter which is only temporising with it and is not likely to be effective in the long run. I think if one could get at the fly, a creature no one would miss, the disease would be eradicated root and branch.

1820. Can you suggest any way, and have you had any experience of any way, by which the fly can be destroyed?—Yes, I have suggested a way here, and I believe other people who will be giving evidence, like Mr. Guy Marshall, will suggest other ways. I would not for a moment predict its success until it is tried.

1821. You suggest an experiment?—Yes, for the obtaining of knowledge.

1822. It has been brought to your mind that the game are the reservoir and the tsetse-fly the carrier, and that if you drive back the game you drive back with the game the tsetse-fly; do you believe those facts?—I very much doubt them. I very much doubt that you would drive back the tsetse-fly. I would say try it and see, but without trying it I very much doubt that driving back the game will drive back the tsetse-fly. That is a doubt I feel personally, but whether it is the truth or not I could not say.

1823. Do you doubt the alleged fact with regard to the rinderpest, that when the rinderpest destroyed the game the fly disappeared?—I have no experience. Rinderpest is believed to be due to an organism which is entirely outside my special branch, and I would not like to offer any opinion about it, but assuming it to be true, there may be more than one explanation of the results alleged. One explanation, which I think is the one you would wish to urge, is that when the game was destroyed the fly went because they had not their food. There is also another possible explanation, which is that the fly disappeared because it also was affected by the rinderpest. These things have so many possible explanations. Let us take the fact as proved for the moment; assuming that the rinderpest destroyed the game and that the fly did go, there is more than one way in which that may have been brought about.

1824. Is there not evidence that flies were fed on the excreta of those animals which suffered from the rinderpest, and that in no single case was a fly infected by such experiments?—I have not read of any such experiments, and I do not think from such knowledge as I have of the habits of the fly that you could get it to feed on excreta.

1825. We had evidence to that effect. There is no evidence that tsetse-flies are injured in any way by feeding upon the excreta of such animals. Do you know whether that is the case or not?—I have no personal knowledge of it. I have never experimented with regard to rinderpest. I know in all these things it is remarkable how little the invertebrate hosts are affected, and it would be of some interest and even some surprise to me to learn that the fly was injured by the rinderpest. I do not say that it is possible or impossible, but, speaking generally, when you transmit these parasites by invertebrate hosts it is remarkable how little they suffer as compared with the vertebrate hosts.

1826. Could you give any specific cases in which game and fly have existed for a time together where the game has been driven off and the fly have remained?—No, I have not any specific cases to give.

1827. You say that you would not mind experimenting tentatively with the destruction of game. Would you consider it an effective experiment to relax the game laws in villages where the disease exists, in order that the game might be driven off from the immediate vicinity of those villages?—I venture to think that if that were done it would be better done in an inclosed area of some kind for this reason: I think you will incur considerable risk of another kind, which so far as I am aware has not yet been mentioned. Tante found in his experiments, that in the area in which he worked there was game containing the ordinary *Trypanosoma brucei*, what I call the *Trypano-*

soma brucei brucei, but no *Trypanosoma rhodesiense*, and there was no sleeping sickness among the people there, and it was quite obvious that the *rhodesiense* parasite was absent from that region. Supposing you had a certain area in which there was *rhodesiense*, the human parasite, both in human beings and in game, and you saw fit to drive back the game from that area, it is quite possible, I think, that some antelopes containing *rhodesiense* might escape being shot, and might be driven out and migrate into a district where there is at present antelope and fly but no *rhodesiense*. Consequently, if you were not very careful you might, by driving the antelope containing *rhodesiense* out of one district, spread *rhodesiense* artificially into another district where at present it does not exist. I think that is a great danger which ought to be guarded against, and that is why I have advised that if this experiment is done it ought to be done in an area so inclosed or limited naturally by rivers or something of that sort, that there would be no possibility of antelope infected with *rhodesiense* being driven into districts where at present it does not exist. If you take Scotland again as an analogy, if you managed to drive some infected deer across the border into England, you might introduce the disease from Scotland to England in that way.

1828. It would be essential for any experiment of that kind to be universal?—Either universal or narrowly limited.

1829. So that all the villages would have the same facilities for driving the game back into the forest?—If they only drove them into the forest there might be no danger, but there are often villages and habitations all through the forest. In that experiment the great danger to be guarded against is that of driving game with *rhodesiense* into regions where at present there is not any *rhodesiense*.

1830. If you relaxed the game laws in all the inhabited spots would not the driving out be simply the driving away from habitations?—Would you only relax them where *rhodesiense* occurs at present?

1831. I would relax them where any danger whatever existed. If you alleged to me that there was a certain village which was free, then there the game laws need not be relaxed, but if there was a danger without relaxation of the game laws in a village somewhere else I would certainly relax them there, and would not that effectively meet the danger you mentioned of driving infected animals into a non-infected area?—Yes, I suppose it would if you relaxed the game laws everywhere. It would be an enormous business that would be practically unsuccessful.

1832. What harm would result? Supposing there was no necessity for the driving off of the game, and these tentative experiments which you suggest here were universal instead of in a small isolated area, what harm would occur—what danger or damage to the community would occur if you discovered by your tentative experimentation that there had been no necessity for the relaxation of the game laws?—You say if you do it on a universal scale? Of course one is assuming that it would be practicable to do it on a universal scale to begin with, which I very much doubt. Africa is so large that I do not think it could be done on a universal scale.

1833. You mean that extermination could not be done, but would the relaxation of the game laws around these inhabited areas not give such facilities to the natives and whites too to trap and shoot and otherwise kill these game in the vicinity as would drive them out, and I suggest to your mind that if that proved ultimately to have been an unnecessary procedure, what harm would have been done?—No harm, so far as human beings are concerned.

1834. What harm with regard to animals?—They would have been killed off.

1835. What harm would arise to animals? If you stop it it is a tentative experiment; you prove in two, or five, or ten years that it is not necessary, and then you go back to your game laws. Would not animals spread over these places again as they spread after the rinderpest?—If there are no animals to spread, they will not.

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1836. I draw a distinction between driving off the game and actually carrying out a war of extermination, which I did not suggest to your mind.—I certainly think if the game is becoming tame it should be stopped; I do not think game ought to be allowed to be tame and to destroy the crops. It is better to keep them wild.

1837. You would relax the game laws so far as necessary to drive off the game from inhabited areas?—Yes, I think I would if I was an administrator there. It is difficult to say, without being on the spot, but if I was an administrator, and was quite convinced that the game was a danger to human life, I would drive it back in those regions where it existed, provided I was quite sure I would not drive it into regions where the parasite did not exist, and thereby spread the disease over a wider area. Always keeping that danger in mind, if it was possible to avoid that danger, and if it was possible to do nothing more than drive the game back from villages where game was undoubtedly a danger, if I was an administrator in power, I certainly think I would carry that out.

1838. With regard to this island, did you prove, to your own satisfaction, there were no game on that island?—The island was only about two miles long. I forget its exact dimensions. It is partly covered with very thick bush, and we saw no monkeys on it, or anything of that sort, and no game, unless you count the hippopotamus as such. There were plenty of hippos everywhere. The hippopotamus is a very favourite source of food for *Glossina palpalis*.

1839. I am interested in that if it is the case that there are no game there and yet there are fly?—It is *palpalis*, not *morsitans*. They feed on the crocodiles and hippos and the wild birds as far as one can see. On several visits we paid to the island there was nothing else for them to feed on at all, and they swarmed in the most extraordinary manner.

1840. Did I understand you to say that the *Trypanosoma gambiense* could be conveyed by *morsitans*?—It has been conveyed as a laboratory experiment; whether it is in nature or not is quite another question.

1841. You talked about the virulence of the trypanosome of human beings as having diminished in Uganda?—I think there is strong evidence it has very considerably diminished.

1842. Has it diminished after passing through the human blood, or is it due to a heightened resistance in the human blood?—In my opinion this spread of tolerant relationship between parasite and host is an adaptation on both sides. I think there is considerable evidence to show that when the trypanosome has passed through the same vertebrate host a number of times the trypanosome itself becomes adapted to that particular kind of host and less virulent to it. That is one side of the relation. Of course the other side of the relation, I think, is that by a process of natural selection, the killing off of the most susceptible and the survival of the more tolerant, there is also an adaptation on the part of the host gradually brought about. I am certainly of opinion that the adaptation in question is more one of the parasite adapting itself to the host than the other way round. I may state that is also the opinion of Sir Ronald Ross with regard to malaria.

1843. You think that might take place in as short a period as 10 years?—I think it has come about, and the Uganda epidemic began about 1900, I think. As far as one can see there has been a considerable diminution of the virulence in 14 years if one reads the accounts of the earlier epidemic. If you care to see them I might refer you to the "Transactions of the Society of Tropical Medicine and Hygiene," July 1912. It was there that the matter was particularly talked about in a discussion on a paper by Mr. Meredith Sanderson, and there was a discussion in which Dr. Low, Dr. Bagshawe, and several others took part. I was present at that discussion and took part in it, and certainly the impression we all got was that what we might call the initial virulence of sleeping sickness in Uganda was very much greater than it is now.

1844. Do you think immunity in Nyasaland might come about as a result of that increased virulence or a decreased susceptibility?—I think it is more than one

could hope for; immunity would be a question of perhaps thousands of years.

1845. Is it not the case that the resistance of the human subject to a pathogenic organism varies directly with its contact with that organism during the phylogenetic history?—Yes, the resistance increases, but, I think, very slowly.

1846. You do not suggest there is any hope for us by either the decreased virulence of the organism or the heightened resistance of the individual?—No, I do not think that would develop so far as to be of any practical importance to human beings. I might mention why I bring up that point in my evidence; it was simply this: it was stated by Dr. Yorke and others that the course of the disease in human beings was so short, and human beings died so rapidly, that man would not be very efficient as a reservoir. I only introduced that to make this point, that in the course of time one might expect the disease to run a longer course in man, and therefore in course of time we might expect man to become a more efficient reservoir than he is at present. This was the only point of view from which I introduced that particular point. I do not think we can hope that man would acquire such immunity in such time as would be of any practical importance from the point of view of resisting disease.

1847. Is it possible that there may be another explanation arising from the fact that individuals have undergone a modified infection early in life, and have developed an antitoxin which makes them ontogenetically resistant?—That is possible; that is the condition apparently you have in Brazil with the human trypanosome there. In the infected districts every human being gets it, generally within the first two years after birth. But I think this condition belongs to a very late stage of the evolution of the disease, and one which it would be quite Utopian to consider as of any practical importance at present in the case of sleeping sickness. It is the kind of condition which might be brought about in 100 or 200 years.

1848. You do not think that is a possible explanation of the diminished virulence in Uganda; you do not think it is possible that a mild infection may have taken place earlier in life, and thereafter a resistance to that disease due to an antitoxin result?—I do not think there is any evidence of that at present. I think if small children get it they die as quickly as the adults. The disease has not reached that period of evolution in which the young people can get it and get over it; they get it and die; but I am not a medical man.

1849. I was thinking of the case of typhoid, the ontogenetical immunity which one gets from having a mild attack in infancy or early life. Would it be possible that there is a modified infection, not so virulent as to produce the disease, but an infection sufficiently strong to give subsequent immunity?—The thing is perfectly possible, but whether it takes place at present is another question. I do not think it is very probable that the disease has yet got to that point in the evolution of immunity in which that could take place.

1850. (Chairman.) Are you acquainted with the incidence of the disease in the population of Rhodesia and of Nyasaland?—Statistically?

1851. Yes, what proportion of the population is attacked?—I am afraid I do not know.

1852. We have been told it is very small indeed, almost negligible?—I am not aware.

1853-4. You agree, I suppose, that a very small number of game infected with trypanosomes could infect a very large number of flies, and if that is so, must not one of two things be the case—either the trypanosome from the game is not largely a trypanosome pathogenic to man, or man has developed such a power of resistance that the disease is not likely to become serious? I do not know if you have followed me? We have a large number of trypanosomes in game and a small number of cases in man; there is a doubt about the trypanosome, and one of two things would explain it: either the trypanosome, which mainly comes from the game, is not pathogenic to man

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or, if pathogenic to man, the resistance of man is so great that it does not spread?—I think the first of these explanations is most likely because, as I said, that figure of 16 per cent. given by Dr. Yorke is quite fallacious, and the actual percentage of the human trypanosome is very much smaller; perhaps 15 of that 16 per cent. is really *brucei brucei*, which would not live in man. Seeing the extreme virulence of the trypanosome to man at present we have no evidence that any human beings have any resistance to it at all. I do not think there is evidence at present that any human beings are resistant to *rhodesiense* and therefore I think the second explanation is not likely. I think it much more likely that the small spread of the disease, in spite of the large percentage of infected antelope, is simply due to the fact that in the percentage of infected antelope there are two things mixed up together.

1855. There is very little of the trypanosome pathogenic to man, you think?—I think that is more likely.

The witness withdrew.

SEVENTH DAY.

Tuesday, 11th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (Chairman).

Mr. E. E. AUSTEN.

Dr. A. G. BAGSHAWE.

Dr. ANDREW BALFOUR, C.M.G.

Sir JOHN ROSE BRADFORD, K.C.M.G.

Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Dr. W. A. CHAPPEL, M.P.

Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODGE, Bart.

Dr. C. J. MARTIN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.

Professor R. NEWSTEAD, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Sir STEWART STOCKMAN.

Mr. A. C. C. PARKINSON (Secretary).

Mr. G. A. K. MARSHALL called and examined.

1859. (Chairman.) You are the Director of the Imperial Bureau of Entomology?—Yes.

1860. You have been good enough to favour us with a very interesting memorandum on which I propose to ask you one or two questions. In the first paragraph of your memorandum you refer to the amount of sleeping sickness among men in Nyasaland?—Yes.

1861. Does that extend to Rhodesia?—No; that was purely with regard to the proclaimed area, not to Nyasaland as a whole.

1862. Have you any statistics as to Rhodesia, if not I will not trouble you on that point?—No, I am afraid not.

1863. We have been told that by examination of wild game they are found to be full of trypanosomes, among others *brucei* and *rhodesiense* (or *brucei vel rhodesiense* if they are the same) which are pathogenic to cattle and some of them possibly pathogenic to man. That being so, taking areas where the fly is in large numbers and is undoubtedly the carrier of the trypanosome from the wild game, have you any theory to account for the fact that in such districts it is practically impossible to keep cattle alive while man is relatively very slightly affected?—It seems to me that a theory that will account for that is that you are dealing in this case with two trypanosomes, that you have a purely human trypanosome and a cattle trypanosome, or rather an antelope trypanosome, which are very similar in their morphological appearances; that

1856. (Dr. Bagshawe.) You have told us in your evidence that the *Trypanosoma rhodesiense* disease in Nyasaland is a new trypanosomiasis?—That is my opinion certainly, that it is not merely a newly discovered disease, but a newly arisen disease in nature as a natural phenomenon.

1857. If that is the case is it not rather remarkable that the disease has been discovered almost simultaneously in many districts over a very large area; for instance, in the course of a few years it has been discovered not only in Nyasaland but in North-Eastern Rhodesia and North-Western Rhodesia, and also in Southern Rhodesia, in Portuguese West Africa and in German East Africa?—Is that all *rhodesiense* disease?

1858. I think there is not much doubt about it, and many of these areas are not connected by trade routes. That seems rather remarkable, does it not?—That would certainly go against my view; if it is spread over a wide area now it would certainly look as if it had been in existence for some time.

so far as the human trypanosome is concerned that has been present there for a considerable time, and the majority of the natives at all events are now immune to its effects, and you will only get a certain percentage of susceptibles occurring in every generation. That hypothesis would meet the facts as we find them.

1864. By man being immune, do you mean that the trypanosome perishes, or that he is tolerant so that it remains in the blood and does not produce the actual disease?—That point appears to have been entirely neglected up to now. There is no evidence to show which it is. It may be both. There may be some tolerant individuals and there may be some who are actively immune.

1865. I do not know whether you have studied the trypanosome itself very much?—My knowledge of it is entirely confined to the study of the literature.

1866. Do you assume that the wild game carries the trypanosome of cattle but not the trypanosome of man?—Yes.

1867. That is what you assume from what you have read?—Yes.

1868. But on your theory could you say (I must not press you too much on this point, because, perhaps, it is not your special point) whether man might form an important reservoir as well as game?—I should consider that man would form the only reservoir if the hypothesis I have put forward is true.

1869. I do not think I quite follow you at the moment. Do you mean that man can only be infected

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from man, or do you mean that if the fly from game bites man, the strain is innocuous unless it passes through man and then to another man?—I mean that there would be a strain (I do not know whether these things are specifically distinct) which is pathogenic to man, but which is not harboured by game, and a strain which occurs in game, but which is not pathogenic to man, and that they have superficially a close similarity.

1870. Passing to flies, we have been told that for the prosperity of the fly warm mammal blood is necessary. Do you consider that the fly can prosper and breed in the absence of warm mammal blood?—There is evidence that it feeds on either reptiles or birds; it is not certain which, but nucleated blood cells have been found in specimens of *morsitans* showing that it is capable of deriving nutriment from animals other than mammals, but whether it could exist permanently on such a diet nobody knows.

1871. Or whether it could breed?—Or whether it could breed.

1872. An experiment has been suggested of the clearing of the wild game from certain areas, larger or smaller, with a view to ascertaining in the first place whether, if the wild game (that means the larger game) were removed, the flies would disappear; and, in the second place, if they did not disappear, whether they would or would not still remain infected. Have you considered that at all?—Well, the question whether that experiment would be of any value would depend very largely on the details of the way in which it was carried out. If experiment is to be made to see whether the fly can exist in the absence of the larger game, I would like to know whether it is proposed to retain human beings in the experimental area.

1873. Assume for the moment that it is not.—I think that it should be done in both ways. If you make an experiment and do not retain human beings, then that experiment is of no value whatever as applied to the natural conditions which you propose to produce on a large scale. If you destroy the game in a large area that area is going to contain human beings. You have to consider the possibility whether the fly, having the more usual source of food supply, or a valuable source of food supply, removed from it, will not attack man more assiduously than it has done before.

1874. Do you say you do not think it is the more usual food?—I do not think there is evidence of that. Wherever man occurs, I think the fly attacks him quite freely. I think it is quite impartial. I do not think there is evidence of special preference for game as opposed to man, and I submit that the very existence of sleeping sickness itself indicates that the flies must bite human beings to a very considerable extent. If only two flies in a thousand are infected, the number of bites inflicted, even to produce the small amount of sleeping sickness that exists, must be considerable; if a high percentage of the indigenous population is immune, the number of flies that must bite to produce sickness is even greater still.

1875. You have given us the usual sources of food—the larger wild mammals and man. Is there any other usual source of food? I am talking now of *morsitans*.—It seems to me that that point has not been sufficiently investigated yet.

1876. Have you an opinion upon the point?—Well, there is a case that I refer to in my statement here. I forget the exact figures, but I think Mr. Lloyd, of the Rhodesia Sleeping Sickness Commission, found that 15 per cent. of the flies in which they could identify the blood contained nucleated blood cells, meaning that they were derived from birds or reptiles. There you have positive evidence that in a country where game was abundant the flies naturally fed on animals other than mammals, which would suggest that where you artificially removed the mammalian supply they would still have another supply on which they would naturally feed.

1877. Correct me if I do not follow you rightly, I understand that your conclusion is that the absence of the larger mammals does not make it probable that it is accompanied by the disappearance of the fly?—Not

necessarily at all, I think there is no valid evidence to justify any positive assertion to that effect.

1878. With regard to the proposed experiment itself, assuming that you could drive out the larger mammals or the greater part of the wild mammals, and could, with very few exceptions, exclude man from the same area for the purposes of arriving at any result such as we desire to obtain, can you suggest any fallacies that might be likely to neutralise the effect of the experiment?—I think that the absence of man in itself might do so to start with. That would be one thing. If the fly died out within an area it is conceivable that it might be attributable to the absence of man, and that had man been there, as he will be if you apply this principle all over the country in general, it would not have died out, so that your experiment does not reproduce the natural results which will follow the extermination of the game on a large scale, and your experiment does not give you any real light upon what is going to happen. But, apart from that, supposing that you cleared an area, a great deal would depend upon the locality of the area as to the validity of the results which you would obtain. Supposing that it was within or adjoining another fly area and you adopted a system of clearing a 10-mile belt all round it, and supposing that you removed the large mammals and man from that experimental area, and after a certain time the fly disappeared, I do not know that anybody could say what had happened—whether you had killed the flies or whether the flies had migrated, and until we know more about the habits of the fly, I do not think you will ever be able to come to a definite conclusion from an experiment of that kind.

1879. There is one question I would like to ask on what you said just now; take the case that you postulated—that the larger mammals had gone and man was there. Could the *morsitans* convey sleeping sickness from man to man and from man to cattle?—In the light of Sir David Bruce's recent paper there would be very little danger to cattle, because he finds that they are highly resistant to the human strain of *rhodesiense*, but it is possible.

1880. The result which you would produce possibly would be that you would save the cattle and leave man where he was?—Precisely.

1881. The question has been discussed a good deal as to whether *morsitans* is often found where game is absent. Have you any knowledge of your own about it?—No, not of my own. I have notes as to the observations of others.

1882. I understand you have some reports of Major Stevenson Hamilton, which may be of assistance to us.—Yes, I have several; I have collected here notes of observations from as many different localities as I could. Many of them are very brief. There is a note from Portuguese East Africa, from Nyasaland, from German East Africa, Congo Free State, North Eastern Rhodesia and Southern Rhodesia.

1883. Do those deal with the question entomologically?—No. Those are cases where observers have stated that the fly occurs in places where there appears to be no game.

1884. Do they all agree?—They all agree.

1885. You can produce the notes for the assistance of the Committee?—Yes, I can produce them for the assistance of the Committee. (*The witness handed in the following paper.*)

J. Stevenson Hamilton.—The Relation between Game and Tsetse-Flies.

"I have also had a personal experience of the existence of *G. morsitans* in large numbers, where there was little or no sign of the larger mammals.

"In 1908 I travelled through the northern part of Portuguese East Africa from Ibo on the coast to Lake Nyasa, roughly along the 13th parallel of latitude. Tsetse-fly was met with in two places; first, in a small belt near the Mwangidi river, not far from the coast, and secondly, from the right bank of the Msalu to the right bank of the Lujenda river, a distance of about 80 miles as the crow flies. No signs of any game were

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found until we had passed through the first fly area; then in one place we came on a very few kudu, and in another saw a single waterbuck. In the fly areas themselves there was no indication of the presence of big game, nor indeed of that of any of the lesser species of buck. Hares were pretty numerous. Monkeys not uncommon. The forest was full of small birds, and judging by their runs smaller mammals probably abounded. Throughout the larger area, fly was practically continuous, and though it was then the coolest season of the year, was extremely troublesome, often biting ourselves and our natives after sundown. I collected several of the flies from each belt, and handed them over to the entomological department at Lourenço Marquez, where Mr. C. W. Howard ultimately pronounced them to be *Glossina morsitans*. Now as regards the almost complete absence of game. Since it was the driest time of the year, and the Msalu and Lujenda are both of them perennial streams, one would have expected to find indications of game in their vicinity if anywhere. But though my companion, Mr. R. C. F. Maugham, and myself, having a large number of carriers to feed, hunted assiduously in the most likely spots on every possible occasion, we were entirely unsuccessful in finding even old tracks dating from the last rains. Although game often temporarily migrate from a country, I scarcely think it could ever have been very numerous along the route we took. The character of the vegetation was not such as finds most favour with antelopes generally, and during our constant examinations of the sand beds of the dry watercourses, I do not think it would have been possible to help noticing indications of the recent presence of wild animals in any numbers. Nor, in view of the fact that nearly every adult native we met was in possession of fire-arms of some description, did there appear any particular reason for doubting their statement that they had shot out most of such game as had existed in the country long before. Certainly the swarms of *G. morsitans* which we encountered must have been hard pressed by hunger if forced to depend for their existence upon the blood of a few stray herds of the larger species which may have existed. It was noticeable that the fly was most in evidence near the camping and halting places. The route which we followed is the usual one from the interior. It is, in fact, the old slave caravan road, and at the present time, or at least three years ago, natives were accustomed to make use of it only in very large parties, owing to their fear of the predatory bands of the independent Yao chiefs, which were always on the lookout to snap up solitary travellers and small detachments. The fly belts themselves are thinly populated, and the natives kept no stock except a few rather anæmic-looking goats. The fly was not much in evidence in the clearings round the villages."

1886. There are in your possession certain entomological reports which have been collected mentioned by Mr. Austen the other day?—The information contained in those, I am afraid, is not of much value. In issuing the list of questions my idea was that it would perhaps stimulate local residents to make observations along the lines of the questions suggested. What actually happened was that they simply sat down and wrote their recollections or what they thought they recollected, and the result has been, for the most part, merely general statements of an extremely contradictory character. With regard to any area where there is more than one tsetse-fly, the whole of the answers are of no value practically because they generally refer to "the tsetse-fly." Several of the species have very different habits, so that the answers are of no practical use. With regard to Nyasaland and North-Eastern Rhodesia, Mr. Neave, our travelling entomologist, has briefly summarised the results in a paper he has already published and that I have here for the information of the Committee. (The witness handed in the following paper.)

Mr. S. A. Neave's Summary of the Reports on Tsetse-Flies from Eastern Tropical Africa.

"There would appear to be a fairly general consensus of opinion that, though it is difficult to define

with certainty the limits of character of a fly-area, it is usually a region covered with fairly thin scrub or bush and at a moderately low level. One observer, however, Dr. Prentice, says: 'I have found *morsitans* in the open country, in sparse bush, in dense jungle and in deep dark shade (such as *palpalis* likes) by the water's edge.' Others, such as Mr. Barnshaw, state that it never occurs in 'msitu,' by which he means the patches of dense forest which occur particularly over the Nyasa-Tanganyika plateau and the Awemba country. Most observers appear to be doubtful whether there is any marked seasonal fluctuation of numbers of the fly. A large majority agree that the presence of water is not essential to the well-being of this insect, whilst three gentlemen, Doctors Pask and Murray and Mr. G. F. Manning, refer to the absence of this species from a belt about half-a-mile wide along the south-west shore of Lake Nyasa, a fact which had already been noticed by several other observers, including the writer. On the vexed question of the relations of *G. morsitans* with game, there is, as might be expected, much conflict of evidence, more particularly as on this subject opinions are apt to be somewhat biased. Six gentlemen are of the opinion that this species is entirely dependent upon the presence of big game. The view of eight others, however, is precisely the opposite, while three are of opinion that the fly is only partially dependent upon the larger mammals. It must be admitted that the majority of the more experienced observers group themselves amongst those who believe that *G. morsitans* can, and indeed does, exist in the absence of the larger mammals. The importance of this point is increased by the fact that it agrees with the recently published views of men of such great practical experience as Major J. Stevenson Hamilton and Mr. R. B. Woosnam. There is, however, a remarkable unanimity of opinion in the answers as to the spread of *G. morsitans* in recent years in Northern Rhodesia and Nyasaland, practically all observers, with the important exception of Sir Alfred Sharpe, agreeing that this has been the case. Some gentlemen associate this spread with the increase and scattering of big game; others, Messrs. Fraser, Manning, and Van Schalkwijk, with the movements of natives. Mr. Thornycroft notes a special increase along main roads. Amongst the few people who were in the country when the rinderpest swept through it, there is an agreement that the fly disappeared to a large extent about this time. This is an important point in respect of the above-mentioned spread of the insect, since only one observer, Dr. Prentice, even suggests that the fly has now spread to an area where it did not occur previous to the rinderpest. Five gentlemen, Messrs. Brown, Edwards, Barrington, Manning and Young, refer to the disappearance or reduction in numbers of the insect with the advent of natives and the consequent clearing in the neighbourhood of villages."

1887. In a certain part of your report I notice you make a strong recommendation that further inquiries should be made in the direction of entomological research. If any results can be obtained in that direction, do you or do you not think it would occupy a very considerable time before they were obtained?—That entirely depends on the nature of the organisation to carry the researches into effect. If you send a single man to sit down in the wilderness to solve an extremely complex problem of that kind—

1888. Do not put that on me, I do not mean that. If your recommendations were adopted and strenuous efforts were made to encourage research, would it from the nature of the problem, occupy a long time?—I think it is largely a question of the men. If you sent out a dozen competent men they would arrive at results far sooner than two men would, and it is really a question of money and men whether you get the results quickly or not.

1889. I will put a quite general question. Do you think that valuable results would be likely to be obtained within a reasonable time?—Yes.

1890. (Dr. Chapple.) You suggest that before experimenting in game destruction there should be an entomological investigation held?—Yes.

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1891. Are we not holding that now by the investigation that is going on under Sir David Bruce?—There is no entomologist under Sir David Bruce and there has not been for a long time.

1892. Is the entomologist in Uganda?—The only entomological investigations were carried out during the few weeks that Professor Newstead was in the country.

1893. What do you hope to discover of practical importance?—The obvious thing in dealing with an injurious insect is to attack it in its breeding grounds, and that is the very point on which we are singularly deficient in information with regard to *morsitans* in Nyasaland. Up to the present date the number of pupae found in Nyasaland of this extremely injurious insect can be counted on the fingers of one hand. That shows how little we know about it and explains our ignorance.

1894. But is the most essential thing to attack the breeding ground?—The thing is the destruction of the fly, I submit, and that you will not do until you have located the breeding grounds.

1895. Do you proceed on the assumption that the fly is the carrier of this disease?—Yes.

1896. You are satisfied that that is the case?—Yes.

1897. Are you satisfied also that game is the reservoir?—Of what?

1898. Of the trypanosome?—Which trypanosome?

1899. The trypanosome which produces trypanosomiasis in man and animals in Nyasaland.—I do not agree that it has been proved to be the carrier of the trypanosome in man. That is mere hypothesis at present.

1900. Do you agree that it is the carrier of trypanosomes or trypanosomiasis in animals?—That has been proved, I consider.

1901. You wish to prove that the fly is the carrier of the trypanosome that produces trypanosomiasis in man?—I think that is the first step before you undertake any destruction of game from the point of view of the human trypanosome.

1902. You are satisfied that domestic animals in Nyasaland die from trypanosomiasis?—Yes.

1903. You are satisfied that the *Glossina morsitans* is the carrier of the disease to the animals?—Yes.

1904. And that game is the reservoir of that trypanosome?—Yes.

1905. And yet you say that you would not take any step now towards the destruction of the game?—No.

1906. Why?—Because I do not know that the introduction of cattle into Nyasaland is essential to the welfare of the country. I daresay the Committee has had brought before its notice a statement on the part of one of the local officials that in his district there are estimated to be 600 head of elephant and 30,000 head of eland, without mentioning other animals. The eland is one of the most tractable and docile of wild animals. I think it a preposterous suggestion that in order to develop that country you should slaughter all those animals simply to replace them with a domestic breed which is not well suited to the conditions. From a purely financial point of view I think that the domestication of such animals as the elephant, buffalo, and eland would be the sounder way of developing the country.

1907. You will admit that dogs and horses and goats and sheep and cattle are all liable to this disease and extermination by it?—Not necessarily extermination. Cattle are adapting themselves to it in many places. We are getting resistant strains.

1908. Supposing that it was proved that the *morsitans* was not the carrier of trypanosomiasis to man, would you allow things to go on as they are now, having regard to domestic animals?—You misunderstand my position altogether. I quite agree that the fly should be tackled, but the system that has been advocated is to diminish only the food supply of the fly, and I say that the way to deal with the problem is to destroy the fly whatever else happens. Even if we adopted the system of the domestication of game I should still attempt to destroy the fly.

1909. You admit that the problem is urgent and something must be done?—I quite agree.

1910. And it must be done for the protection of domestic animals, not necessarily merely for the protection of man?—In certain places, yes. I do not think it is very pressing.

1911. Do you not think that settlement in South Africa is absolutely impossible unless you can protect all the domestic animals and man?—In certain limited areas, yes, but there is plenty of other ground to develop before these need be occupied.

1912. Are not the cattle which have been mentioned necessary to the progress of civilisation and settlement in South Africa?—I mean in Nyasaland; I did not say South Africa.

1913. In the region we are considering are not the cattle which have been mentioned necessary to the progress of settlement?—No.

1914. Are not the dog and the horse?—I think we could get on without them, but I do not see why we should not be able to introduce them later, if the course which I suggest be adopted, namely, that we should try to eradicate the fly; at the same time we can develop agriculture by other means than by cattle.

1915. If you do not see any necessity for these domestic animals in the progress of civilisation in these regions, then the position that you take up is quite understandable to me. You are not prepared to destroy game, although it is incriminated, because you do not think that domestic animals are necessary to the progress of settlement in these regions?—I think it is not necessary to introduce domestic animals which are subject to this disease into areas where you can make use of other animals for traction, meat, and milk.

1916. That is the advice you give us—to abstain from destroying game?—Yes; I regard game as of financial value to the country in which it occurs, as an asset.

1917. It is more necessary than domestic animals then in regard to the progress of settlement—is that your opinion?—Only in these areas.

1918. Am I to understand that if it was a question of choice between the destruction of the game and the destruction of the domestic animals you would destroy the domestic animals?—No, that is entirely wrong. I would not introduce them into the area in question. They would not be destroyed.

1919. You say that you would not introduce them?—Yes.

1920. That is the same thing, is it not?—It is not destroying them.

1921. Instead of destroying the game, you would allow conditions to exist which would prevent the introduction of domestic animals at all?—No, not necessarily, if you destroy the fly.

1922. I am coming to the destruction of the fly. At present if you had to choose between the destruction of game and the impossibility of introducing domestic animals, would you submit to the impossibility of introducing domestic animals?—I do not quite follow you, I am afraid. I say that I think that it is impossible to introduce them into the areas now and it is foolish to attempt it when you might use other animals.

1923. You do not mind that condition remaining, if to remove it means the destruction of the game?—What I mean is that I do not see the necessity for the destruction.

1924. We want to understand your motives so that we can understand what actuates you. Now with regard to flies you say that something ought to be done to prevent the existence of disease and that that something ought to be the destruction of the flies?—Yes.

1925. How would you proceed to destroy the fly?—I cannot give any definite information on that because we have not the data required for the purpose, as I have explained. I think the first step to be taken is to eradicate the breeding places of the fly, which we do not yet know, and that is where the urgent need of an entomological enquiry comes in. What we do know is that in all other cases of dealing with injurious insects on a large scale, that method is invariably adopted.

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1926. Apart from the question of the value of the game, do you think its destruction would annihilate the disease?—Which disease?

1927. Trypanosomiasis in man and animals.—I do not think it would necessarily eradicate the disease in man.

1928. In man or animals I will say then?—I discriminate between them. You mean the destruction of game only?

1929. Yes.—What do you include in that; because the hyena has been found to carry it, and possibly various other animals, including the smaller carnivora which have not been investigated, may likewise do so.

1930. Do you think that the destruction of incriminated animals would annihilate the disease?—I could not say. I do not think that the destruction of only large game would do so.

1931. Do you think that the destruction of incriminated animals would annihilate the disease?—Yes, if every animal was destroyed, certainly.

1932. As I gather from your evidence, you are not prepared to agree to that, although you know that that would annihilate the disease?—No, I am not prepared to, because I do not think you could do it.

1933. You would prefer to have the disease and the game rather than be without both of these things?—I do not think that your alternative is possible; that is the point.

1934. You said just now that you think the destruction of all incriminated animals would annihilate the disease?—I also said, I think, that you could not destroy all the animals.

1935. I am putting the case hypothetically. If you could destroy the disease by annihilating incriminated animals, you would agree then?—If you could, yes, but it is not possible.

1936. Then you would prefer to have the game and the disease rather than be without these things?—No, I have not said so at all.

(Chairman) (to Dr. Chapple.) So that I may follow your questions, would you tell me what you include as incriminated animals?

1937. (Dr. Chapple.) I am not going to name them, but I want to know the reasons that actuate the witness in giving the advice that he does. (To the witness.) You admit that you could annihilate the disease if you could destroy the incriminated animals?—Yes, if you could.

1938. But at the same time you say that the game is so valuable that you prefer that it should remain?—No; I beg your pardon, I have not said that. I made that statement quite apart from your hypothetical case—that if you could destroy all the animals you could get rid of the disease. I admit that if you could destroy all the animals you would eradicate the disease, but my reply is that I think you cannot destroy all the animals; and in my opinion there will always be a certain percentage left which will be liable to carry on the disease, and therefore the destruction will not have the effect that you assume. And there are dangers of another kind; for instance, you cannot make the destruction simultaneous over an enormous area, and you have the danger of dissemination. You may be driving infected game or fly into areas which they did not previously occupy, and so cause serious loss and damage there. I think that many practical difficulties arise as a result of any policy of general game destruction such as you are suggesting, and I think those difficulties are such that it is wiser to deal with the problem on other lines.

1939. Do you think that in the hunting of rats to exterminate plague you have the same thing, and that you might drive them to other parts where they had not previously existed?—No, it is not the same thing, because rats live under very different conditions. The conditions are not comparable on any ground.

1940. Take a populous locality where there are domestic animals and the people think the domestic animals are necessary to their life and progress. In your opinion, would it be a wrong thing to disturb the incriminated game or the incriminated fly in case you drive it to other villages?—I think there is that danger.

1941. Would you apply that to the hunting of incriminated rats which spread plague?—No, not necessarily. In certain areas it might be true, but the methods of poisoning or trapping rats are not applicable to game as a general rule. If you harass game it scatters over the country as rats do not do. The whole method would have to be different.

1942. If you allowed the villagers to hunt the game, would it disappear?—They would only kill a small percentage.

1943. Would the rest run away?—Yes.

1944. Would it take the fly with it?—I could not say; we do not know.

1945. If it did take the fly with it, would it be a good thing to do?—No, I do not think so; it would merely transfer the danger.

1946. If the relaxation of the Game Laws enabled villagers to frighten away animals which become shy when they are hunted, as I understand, and if they took the flies with them, would you think it unwise to relax the Game Laws lest the animals should go to other populous centres and carry the fly with them?—That is a danger which you have to consider, and, apart from that, there are animals which I do not think they would frighten away, and animals which are likely to remain a source of infection for a considerable time.

1947. Could they be killed?—There would be great difficulty in doing it. I would be glad to have a recipe for killing some of them now.

1948. What animals would not run away, but would remain to be killed?—They would not remain to be killed. The fact is that they are so very difficult to get, that neither commission has investigated the blood of the bush-pig, for instance, which is quite a common animal; but it is most difficult to get hold of.

1949. Is it already incriminated?—They have not examined one, but it is one of the most important animals to investigate, and the fact that it has not been investigated is, I think, a ground for criticism.

1950. You would not be as alarmed at the existence of an animal which had not been found to be incriminated as you would at the existence of an animal that had been so found?—It has not been examined, but the evidence from the other pig points strongly to the fact that it would be incriminated.

1951. But is that an argument against the killing of incriminated animals?—There are other animals that may be incriminated that are difficult to eradicate; for instance, the duiker is one of the few buck that can persist and flourish in close proximity to man. About Salisbury, in Rhodesia, duiker were more plentiful on the commonage round the town than in the open veld far from any cultivation. They are also particularly fond of the vicinity of Kafir gardens.

1952. Is that incriminated?—Yes. That applies to both diseases.

1953. And do I understand that it cannot be exterminated or driven off?—You would have great difficulty in eliminating it. Then the baboon is extremely difficult to destroy. You could eliminate it from a limited flat area, but in country specially suited to it there is the greatest difficulty in getting rid of it. Other animals of the kind that I am referring to are jackals and hyenas. I have heard them come right up to the back of an hotel in Salisbury.

1954. If you could eliminate the animals that were incriminated, would you not limit the amount of the disease even though you did not exterminate the animals you have mentioned?—That is extremely dubious. I think that if the fly's food-supply is diminished it will concentrate its attack on the animals that remain and the percentage of infection may be just as high. One cannot tell, of course, but it seems to me that if you leave infected animals in even limited numbers, and the fly persists, the risk of infection is very much the same.

1955. Do you think then that there is hope of reform along the line of attacking game?—It is a thing, I think, that should be adopted as a last resort. I may put it generally in this way; the abolition of the game means the reduction of the food supply of the fly but not necessarily a reduction of the fly itself; and it

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appears to me that if you reduce the food supply of the fly you increase the danger to man of being bitten by the fly. You increase the possibility of the fly spreading into areas which it has not hitherto occupied. On the other hand I think that if you attempt to deal with the problem on the line of leaving the game as it is and reducing the number of the flies, the risks of attack upon man and upon cattle will be diminished, because you will have a smaller fly population to feed and you will have the same number of wild animals, and I certainly think it a saner and safer method of procedure than the destruction of the game. I admit that it is possible that in certain areas game destruction may be of some value when we know more about the habits of the fly, but as a general policy I think it an error, in fact I am convinced of that.

1956. You would allow in the meantime destruction of domestic animals to go on?—That can be readily avoided.

1957. By taking the animals away?—Yes.

1958. Would you apply the same argument to rats and say that you would run a risk in exterminating the rats of allowing the fleas to feed on the human subjects? Would that be a parallel case?—No, because the flea that carries the plague is essentially a rat flea, and is only occasionally a biter of human beings. It would not live permanently on human beings.

1959. Are you against the experiment suggested by other witnesses?—What experiment do you refer to?

1960. Of fencing in a certain area and carrying on experiments there.—As I said before, I cannot express general approval or disapproval of an experiment until I know what it is intended to prove and the precise methods to be adopted. I might sympathise with the object, but consider that the methods suggested were entirely wrong. Until I knew both what it was intended to prove and what the methods were, I could not express an opinion.

1961. The only thing to be done, as far as you can see, is to try experiments for the extermination of the fly?—I think that is the first step certainly.

1962. You quote Major Stevenson Hamilton as giving an account of the existence of an area in which *Glossina morsitans* is abundant in the absence of game?—Yes.

1963. In what season were those observations made?—In the dry season.

1964. Are there not seasons in which the game are very difficult to see?—He based the conclusions of his argument on the ground that there was very little water in the area. He was travelling along the only rivers for many miles that contained water permanently, and if game were present that is just the kind of country in which one would expect to find it. He could not find game there, and he could not find even the old tracks, which every hunter knows you will find in the dry season, of game which had been present in the wet season. The natives told him that they had shot out all the big game long ago, and he had no reason to doubt their statement as every other native had a gun, but, notwithstanding that, the flies were extremely plentiful and very annoying to his caravan.

1965. Many observers have said that game can only be seen at certain seasons of the year, though it may exist in large numbers?—Yes; it would be more easy to see in the dry than in the rainy season, and he was there in the dry season.

1966. You are satisfied that the game would have been seen if it was there?—Certainly.

1967. (Sir William Leishman.) Do you advocate that further investigations should be made into the bionomics of the fly?—Yes.

1968. What do you think would be likely to prove a profitable line of investigation? If you directed such a research personally what line would you take?—I think, in the first instance, the hypothesis that has been put forward by Mr. Fiske wants dealing with above all things. Take, for instance, trapping experiments—they have been tried in German East Africa and have proved more or less unsuccessful, and I think Mr. Fiske's hypothesis would afford an explanation of why they were unsuccessful. I do not know whether the members of this Committee are acquainted with the position as to the migration

hypothesis. If it can be proved to be true it will make a tremendous advance in the possibility of coping with *Glossina morsitans*. If a sex count will give us an indication of specially favourable breeding areas, we can mark out such special areas for attack. I think that then in those areas we must tackle the fly in the first place. When you have located breeding areas, I would suggest measures of trapping the fly. Clearing the bush in those particular areas would destroy them as breeding areas. I would also suggest the trapping, if possible, of pupae. That is a mere hypothetical suggestion which I might offer. It arises out of an observation made by Mr. Lloyd, the entomologist to the Rhodesia Commission. He records that they found the pupae of *morsitans* associated with 50 different trees—I do not mean different species but different individual trees—and in every one of the cases the trees were abnormal, that is to say, they were damaged in some way or diseased. I cannot think that that is a mere coincidence, because the numbers are too big for that.

1969. Where was that?—In North-Eastern Rhodesia. This observation suggests that there is some special attraction exercised by such trees over the *morsitans* when it is ready to deposit its larvae. I cannot believe that a *morsitans* surveys a tree and notices whether it is decayed or otherwise suitable. From observations made upon the habits of insects in other directions, probably the explanation is that there is some chemotropic reaction, possibly arising through vegetable decay—some chemical substance which affects the fly and stimulates the deposition of the pupae. Now if that is true, the discovery of that substance would be an enormous advance in the practical control of the deposition of the pupae. We could have a practical test by which we could ascertain what were breeding grounds, and we could create artificial breeding grounds which could be controlled. If the hypothesis is true, it might enable us to make a very marked diminution in the fly and therefore it should be immediately investigated, I think.

1970. Would you suggest a method of marking flies in order to measure migration?—Yes, that is essential for the elucidation of the question.

1971. What method would you adopt?—I do not know what Mr. Fiske has done recently, but before he was engaged by the Sleeping Sickness Commission he discussed the matter with me and suggested an extremely ingenious idea which had occurred to him, which would mark the flies, but not affect their subsequent flight, because that is the weak spot in all the systems which have been tried up to now. Dr. Carpenter cut off a leg, and the result, as he himself said, was that a very large number of flies never flew away at all. They were so damaged that they could not have migrated. Mr. Fiske's suggestion depends on a feature of the development of the flies; the fly in the pupa stage is encased in the puparium, and that puparium has a little lid at the top of it, and in order to escape the fly blows out a bladder from a slit in its forehead and forces the cap off, and it forces its way through the superincumbent earth by a similar action. Mr. Fiske suggested collecting a number of pupae, covering them with a substance like flour mixed with various adhesive colours, so that the flies would draw into the ptilinum a certain amount of the coloured substance. In that way you could deal with an enormous number of flies with a minimum of labour, marking them with a certainty which, I think, nobody else has attained.

1972. Is there any evidence that that system has been adopted in connection with other flies?—It is an original idea of his own, and I never heard anybody else suggest it. He said when he went to the States that he would have experiments made, but I have not heard the results.

1973. You are naturally familiar with what is known as to the breeding places of other biting flies which occur in other parts of the world—what is known about the breeding places of Tabanidae?—Very little has been known up to now. None have been found in Africa south of the Sudan until quite recently. Our travelling entomologist, Mr. Neave,

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has now bred a number, including *Tabanus* and *Chrysops*. He thinks that he has some larvæ which will prove to be *Hæmatopota*.

1974. We have a lot to learn?—Yes; we know very little indeed about it.

1975. Do you know other flies which show the same peculiarity in producing their young as *Glossina*?—I think that the only ones are the *Hippobosca*, or forest flies as they are known in this country.

1976. Do they produce their young in the same way?—Yes, as pupæ. A number of flies produce more or less young larvæ, but very few deposit full-grown larvæ as does *Glossina* outside those I have mentioned.

1977. (Chairman.) What sort of fly is the *Hippobosca*?—A very hard flat fly which runs sideways.

1978. (Sir William Leishman.) Is there any other biting fly, the study of which might throw light on the problem of *Glossina*?—I am afraid not. The life history of the *Hippobosca* is very different indeed, and I think it would not assist us at all.

1979. Do you think that *Glossina* feed only on blood?—I am inclined to believe so.

1980. One witness said that he thought they fed on vegetable matter because he found starch grains from bananas on dissection?—I think it is possible.

1981. Is there other evidence of that sort?—I do not think so. That probably has been produced under abnormal conditions.

1982. If it cannot be got to drink water in the laboratory do you think it would do it in nature?—They have been seen in damp places with the proboscis inserted in the damp sand, but the experiments in the laboratory always seem to prove that nothing is absorbed.

1983. (Sir Mackenzie Chalmers.) Your principal work is to collect information from all parts of the empire bearing on entomology generally?—Yes, from all parts of the empire, or rather the world.

1984. How far has the distribution of the tsetse-fly been mapped?—To a very limited extent. We have very few really thorough maps anywhere in the sleeping sickness areas; it is possible there may be such maps, but I am not aware of their existence. Extremely few places have been what I call thoroughly well mapped, and it is a very essential point.

1985. Have they been roughly mapped?—Yes; we are continually publishing maps based on the collections of our travelling entomologists and others; but they just follow a line and along that line the mapping is fairly good, but there are vast spaces about which we know absolutely nothing. There is only one province that I am aware of that has been thoroughly well mapped, and that is the Illorin Province in Northern Nigeria.

1986. That is a very small area?—Yes, it is a very small area, but it has been thoroughly well done by Dr. McFie.

1987. Mr. Austen has a map, has he not, that is at present at Ghent?—Yes.

1988. How far does that go?—The areas marked on it are merely approximate. I might say that many of them are based on very inadequate evidence.

1989. But it is the only evidence there is up to date, is it not?—Yes.

1990. So that we do not know what are the fly areas really?—We have a rough idea of the fly areas but it is of a very vague and general kind. In order to study the habits of the insects we want precise information as to their distribution and alterations in the distribution above all things.

1991. You mean as to their migration?—Yes.

1992. As to which we have very little information at present except in certain limited areas?—With regard to Northern Rhodesia, I have a very excellent map showing the recrudescence of the fly in a limited area, subsequent to the rinderpest.

1993. How long were you in Rhodesia yourself?—About 13 years.

1994. How much of that time were you in the fly area?—Not very much. In 1894 I visited the Umfuli district, and I passed through a good many fly belts

there, and in 1895 I was residing in a fly area for about six months continuously.

1995. Have you been bitten yourself?—Frequently.

1996. With no evil results?—No.

1997. Was there any sleeping sickness?—It was not recognised, and it has never been detected since in that particular area.

1998. Those flies were probably non-infective flies so far as human beings are concerned?—With regard to what?

1999. *Rhodesiense*.—Yes, but not as regards cattle.

2000. How many entomologists are now working and reporting to your bureau?—The official entomologists are attached to the Department of Agriculture, and they deal purely with plant pests and not with matters of this kind as a general rule.

2001. There is entomological work going on, is there not, under the Bureau of Entomology?—Mr. Neave is the only man at present in the field, and he is not dealing with *Glossina* at all. Our West African entomologist is at home on leave.

2002. Then no work is being done on the lines you suggest?—Not at the present moment. Dr. Carpenter is in England at present.

2003. There is Mr. Fiske, what is he doing?—He has just arrived in Uganda.

2004. He is investigating a solitary problem out of the various ones that have to be dealt with?—It is one of the greatest importance. Mr. Anderson in British East Africa has been sent down to collect tsetse-flies in a certain place, but he is not doing research work on tsetse-flies. Dr. Carpenter and Mr. Fiske are the only two men appointed for that special work.

2005. To carry out as far as possible the investigations you suggest to us, how many men should be employed?—I would not like to express an opinion on that matter. It seems to me that the whole question of mapping out a policy of that kind should be left to a man who has had practical experience in the destruction of insects on a large scale. No such man had ever touched the *Glossina* problem until Mr. Fiske went out. I am ready to leave everything in his hands. If he came back and said to me "It is no use tackling the problem along the lines which you suggest," I should accept his verdict at once without hesitation, and I would sooner defer any definite suggestions until he is in a position to report as to what he thinks.

2006. When is he likely to make any report?—I think that rests with the Royal Society. I think they have asked him to report as soon as he has arrived at any conclusion.

2007. You have not heard from him?—I have had a private note to say that he has arrived, and that he is more optimistic than when he started as to being able to obtain practical results.

2008. But there is no time mentioned with regard to when his report may be expected?—He gave me no information about it.

2009. How long has he gone out for?—He was engaged for a year.

2010. Assuming the investigations that you suggest could be carried out, is it possible to get sufficient trained observers to carry them out?—I do not think there would be much difficulty about that. The difficulty is to get a man to organise, and I think we have got one in him. I think that to get men to work under him would not be difficult. We have plenty of good men in this country.

2011. A sufficient supply of men who would be suitable for the work?—Yes, I should say so.

2012. Now I want to go rather more into detail with regard to two experiments which have been suggested to us; there is an island, as I understand, separated from other portions of the coast where you find *sitotunga* harbouring *T. gambiense*?—Yes.

2013. There are no other antelopes there. If the *sitotunga* were exterminated from that island should we gain anything?—That would be quite a useful experiment to see whether the flies retained their infectivity, but the whole of the observations with regard to the infectivity of the fly along the lake shore are open to very

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serious criticism. The assumption is that it is due only to antelopes.

2014. You think that there are other incriminated animals to be considered?—Yes, man especially.

2015. Including man?—Yes; Captain Fraser and Dr. Duke mentioned in 1911 the surreptitious visits of natives to the proclaimed areas. They said that that would account for the continued infectivity of the fly after the area had been closed for three years.

2016. The fly drawing poison from infected man direct?—Yes; all the men were supposed to be removed, but some natives had come back surreptitiously.

2017. Were those natives infected or non-infected?—That cannot be said; but the assumption that it is necessarily the buck that kept the infection going is somewhat invalidated by those observations, especially as the observers themselves say that such visits would be sufficient to account for the continued infectivity. Then there is the question of the possibility of the fly inheriting the organism, which has not yet been conclusively disproved.

2018. There has not been much observation, has there, as to areas in which you find fly without game?—I have brief notes about four or five other cases here, which I would read out if desired.

2019. I would like to know more about that.—These are casual observations. They ought to be investigated to see what the position really is. This is from a Colonial Office Blue Book issued just lately. The acting Principal Medical Officer in Nyasaland states:—“*Glossina morsitans* has increased enormously, not only in the districts where game has recovered from rinderpest, but also in those where game is practically non-existent, and where 10 years ago it was plentiful.” That is a very definite statement with regard to increase.

2020. That area has not been mapped out yet?—No; Dr. Sander in German East Africa says in a pamphlet on tsetse in 1905 that in the area which he specially examined between Tanga and Kilimanjaro he was informed by both Europeans and natives alike that though the game had diminished to an extraordinary extent, tsetse had yearly increased in numbers, and had even spread into localities previously free. This they attributed to the cessation of annual grass fires, and he was inclined to agree with them. This shows that there is no necessary co-relation between the destruction of game and the diminution of the tsetse-fly.

2021. In many areas in Africa you find game but not tsetse?—Yes.

2022. Is work being done with regard to the rest of Nigeria now that Ilorin is finished?—Not that I know of, at least on those lines. It was done on the individual initiation of one man, Dr. McFie, and he has been transferred to the Medical Research Institute at Lagos, where he has not had an opportunity of carrying on further work of that kind.

2023. Is there sleeping sickness in West Africa?—Yes; it is very sporadic. It is scattered about in various places, but the cases are few, indicating, I think, the existence of a large immune population with the occasional breaking out of infection in susceptibles. It is very similar to the position in Nyasaland and Northern Rhodesia. To my mind the similarity is striking.

2024. In Southern Nigeria, where there is hardly any game, is there any sleeping sickness?—Yes; it has been found at Eket in the Eastern Province, and at least in one or two other places in the Cross River basin.

2025. Do you get information from the foreign bureaux at all as to what Belgium, France, and Germany are doing against sleeping sickness?—That is all sent to Dr. Bagshawe.

2026. Do you know whether any experiments have been made in game destruction by foreign nations?—No, I cannot say that I know of any.

2027. Another experiment was suggested to us with regard to *morsitans*. The Domira Road running up to the lake runs through 20 miles of fly country, and it has been suggested that five miles on each side of that road should be cleared of game and fenced, and then patrolled to keep it clear of game; and then,

of course, as the end of that experiment I suppose that men and cattle would pass along the road and we should see the results; what would you think of that?—I should have thought that the mere clearing would be sufficient without any fencing.

2028. Clearing the game I mean, not clearing the scrub?—Oh, I see. I should think that it would be cheaper to clear the scrub, and that you would obtain the same result.

2029. Do you know what kind of scrub it is?—No, but I should think it is very similar to the bush we get in Rhodesia. I may be mistaken.

2030. Is that easily cleared or burned?—Yes. It is known as orchard bush in Africa. If it was a low-lying area, it might have thick rank growth; but if it is anything like the Rhodesian bush, it is easily cleared.

2031. Is it easy and inexpensive to burn an area on each side of a road?—I do not think there is much difficulty in keeping down trees; in grass country one of the difficulties is to rehabilitate them when once they have been eliminated. There is no question that various parts of Southern Rhodesia were covered with heavy forests at one time. When these are cut down the grass comes up and the trees are kept down by the annual grass fires.

2032. After a grass fire will the fly exist?—There is the statement by Dr. Sander which I have just read. I should think it extremely improbable that fly would be found immediately after a grass fire. Burning the grass would certainly tend to keep them down very much, though a certain number would probably escape.

2033. (Dr. Chalmers Mitchell.) You have a great deal to do with English and foreign research work in the way of following it?—Yes.

2034. Now, in your judgment, with regard to man there are two different sleeping sicknesses, Congo sleeping sickness and the other, the Nyasaland sleeping sickness?—Yes.

2035. With regard to Congo sleeping sickness, what is your opinion as to what is the important reservoir?—The evidence strongly points in the direction of man himself, I think.

2036. And not animals?—It was certainly not so in Uganda.

2037. By what was it introduced into Uganda, do you think?—I should think it extremely probable that it was introduced by natives who were taken across from West Africa, possibly by Stanley's or some such expedition.

2038. With regard to Congo sleeping sickness, I gather that you think the best means of attacking it is by obtaining further knowledge of the fly?—Yes, certainly. In Uganda the fly is much more restricted in its distribution.

2039. You think that we need further knowledge as to how to trap the fly and how to destroy the breeding places?—Yes.

2040. In fact, entomological knowledge?—Yes.

2041. With regard to Nyasaland sleeping sickness, I gather that you think that the balance of the foreign and English evidence is against *brucei* and *rhodesiensis* being identical?—What do you mean by English evidence?

2042. The balance of the scientific evidence I will say.—There are certainly more competent observers who have expressed a doubt about their being the same than there are observers who have thought they are the same, taking local investigators.

2043. You think that rather more are doubtful about their identity?—Yes. I am not aware that any continental observer who has done work in the field has expressed agreement with Dr. Yorke and Sir David Bruce as to the identity of the two forms, but I may be mistaken.

2044. Now, you think with regard to Nyasaland sleeping sickness that entomological experiment is specially necessary?—Certainly.

2045. You were talking a few minutes ago about dead trees in relation to the fly breeding there?—Not dead trees, but damaged trees.

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2046. Yes, I beg your pardon. You suggested that it was probably chemotropism that was the attraction?—Yes.

2047. Do you mean by that some odour, some scent?—Yes. An experiment has been made by Mr. Howlett in India. He extracted a substance known as scatol from excrement, and by placing a few drops of that on a rag he induced various dung-feeding flies to deposit eggs on that rag. The question of what the material was or what it looked like did not seem to influence the fly in the least. It was a response to the odour of the essential substance that caused it to lay its eggs.

2048. With regard to chemotropism, we have very little knowledge as far as flies are concerned?—Very little. It requires to be investigated.

2049. Would you expect there to be other stimuli of some kind—temperature, for instance? Would they be affected by the temperature of the body?—That is possible, but I do not think it is probable.

2050. Do we know anything about it?—No, we do not know anything about it.

2051. We have been told several times that certain flies seem to prefer human bodies, and other flies the antelope, and so forth; in other words, that they exercise choice. Do you think that there is an unknown chemotropism or physical stimulus of some kind?—Do you mean species of flies when you say "certain flies"?

2052. I think it is species, but that does not matter for this purpose.—I should be inclined to explain it on those lines.

2053. So that when we talk of the choice or preferential habits of a fly it is from ignorance?—I should say so.

2054. If we knew a great deal more about the fly do you think we could provide the proper stimuli or body to which it would be inevitably attracted?—That is quite a feasible proposition and it is worth investigating, I think.

2055. I want to know how far your demand for further entomological knowledge goes.—In that direction it is unnecessary, I think, to go beyond the investigation of the factors that control the deposition of larvae. There is another factor about which we at present know nothing and which must have been observed by every man who has been through a fly belt. In passing through such a belt the flies will accompany you, and there may be a hundred or so round your carriers; then as you proceed they become fewer and fewer and at last disappear, having all returned to their original belt. Now, no real attempt has been made to ascertain what the factor is that compels them to return to those limited places, and I think that the discovery of that factor might be the key to the solution of the whole problem.

2056. Is there evidence to suggest that rapidly moving objects will attract flies more than other objects?—The evidence is very strong that they temporarily attract them. I cannot doubt it.

2057. Would you not rather expect to find flies following big game, because they were large and rapidly moving things?—No doubt they do follow for a certain period. It does not necessarily mean that they will follow the big game wherever they go. They will probably follow for a certain distance and return again. I cannot speak about that for certain as I can with regard to man. They follow man for a certain distance from their belts and return again.

2058. Like gulls flying behind a ship?—Yes.

2059. You think that probably these flies are to a large extent anchored to some particular locality by some unknown factor?—We know fairly certainly that that is so in some instances, but we want to know more about it. In other cases it is little more than a legitimate assumption at present.

2060. You propose that people should be sent to work out those special problems for us?—Yes, before any other experiment is made. I think that the true interpretation of the results will be conditioned to a great extent by our knowledge of the habits of the fly.

2061. You would not necessarily send out a systematic naturalist?—Certainly not.

2062. What training should a man have before he was sent out?—I would sooner have men who have had actual experience in dealing with the destruction of insects on a large scale.

2063. Where has such experience been gained—in India?—There are men there who have done work of that kind, and in various of our colonies, as well as in America, especially in America. It has been done to a much greater extent there than anywhere else.

2064. I gather that you think that one leader in such work, with an adequate staff of young assistants, probably with the addition of trained natives, would be a good plan?—Certainly, a very good plan.

2065. Have you any idea, roughly, what the cost of a really first-rate man would be? What sort of salary would tempt him?—It is very difficult to say. A good deal depends on the individuality of the man.

2066. These men have unfortunately a market value. Would you say, for instance, 1,000*l.* a year?—You would get quite a good man for that.

2067. Do you think that one could be obtained for less than that?—I think, certainly, for 700*l.* or 800*l.* a year.

2068. And with regard to the other men, I take it those would be young men who had been trained at one of the entomological centres at Oxford, Cambridge, or London?—That is desirable, certainly.

2069. You would expect to get those for what—300*l.* or 400*l.* a year?—Yes, but you must remember that so far as Nyasaland is concerned it will be a matter of some risk. The men have to live in the fly district. It is not a question of living outside. They have to live on the spot and take the chances, if they are really to solve the problem.

2070. That would mean longer holiday and more pay?—Yes.

2071. Then for one leader with three or four or five assistants and a few native boys, we may say roughly 2,000*l.* a year?—Yes. At all events, you could make a good start for that.

2072. Would you suggest any particular locality? They would not have a roving inquiry all over Nyasaland, but would be planted down in some particular place, I should think?—That I should leave entirely to the leader. Nyasaland obviously suggests itself, or Northern Rhodesia, as being the place where the problem is most pressing.

2073. If we had the money to send out an expedition to-morrow, so to speak, do we know enough now to start?—I should put it the other way. We know so little that it is desirable to start at once.

2074. We know where to go to look for what we want to deal with?—Yes.

2075. So that no time would be wasted, as it often is at the beginning of an investigation, in looking about?—No.

2076. We know where to find the problem which faces us?—Yes.

2077. (*Mr. Rothschild.*) I have some other questions to ask you later, but there are two or three things that have arisen out of your answers to previous questions which I will ask you about at once. First of all you were saying that the maps were very faulty. These are maps which have been supplied to the present Committee (*handing maps to the witness*). Are those what you refer to, or have not you seen those?—These are very much out of date now; that is one of the troubles. They are diagrammatic rather than specifically accurate. For instance, one of these patches here may have been based on the capture of only half-a-dozen flies, and where you have a few green dots over there the actual facts may be that the whole area is covered with fly. The maps give the barest indication of where fly were known at that time, but they are practically valueless, for example, for supplying exact information as to the movement of fly within a particular area.

2078. Another question arising out of your evidence is rather more important, I think. You said that you certainly think there is a stimulus from a damaged tree, or something of the sort, which induces the fly to deposit its pupae in that locality?—Yes.

2079. Now to take an analogous case, the various species of meat flies which deposit larvae or eggs in

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decaying animal matter. These flies are always found in enormous numbers on the South African plant *stapelia*, on the *ragfia*, and on numerous Malayan orchids, which have a scent like decaying fish or animal matter. They deposit eggs or larvae on the flowers, and consequently they perish?—Yes.

2080. Do you not think a proper line of investigation would be to try the flies with some plant or some animal odour which was similar to that of these injured trees which would attract the flies in large numbers, and so make them deposit their pupæ where you could lay hands on them?—That is precisely what I do suggest, and I think it would be very simple to experiment on material taken from trees in which pupæ have been found. It is possible that chemical analysis would very quickly reveal the actual chemical constituents responsible for the stimulus.

2081. That is what I thought. Now to come to the other points which I wanted to ask you about; first of all there is the question in Nyasaland, and various parts of Rhodesia, of the human reservoir. We have been told by previous witnesses two facts. One is that the disease takes such a very rapid course that the human element is negligible. The other was told us in answer to a question of mine following on that statement of Dr. Yorke's that he had found a boy tolerant of the trypanosome among his laboratory boys. I put the question to the witness: "If you examined the blood of a large number of natives, might not numerous instances be found of this tolerance?" I asked him whether such examinations had been made on a large scale. His answer was, to me very astonishing, that although no examinations had been made for that purpose since that discovery, the very fact that the blood of an enormous number of natives had been examined to find the few cases of sleeping sickness that had been found answered the same purpose. But as the number of trypanosomes in the blood varies at different times, do you think that that is a sufficient answer to the question?—I should say not. I consider that results obtained in that way are not necessarily any evidence against the possible existence of a considerable amount of tolerance on the part of the natives. I understand that no sub-inoculations were made, but that it was merely a question of blood examination. As regards the rapidity of the disease, I might draw attention to the fact that Dr. Meredith Sanderson mentioned in a recent paper before the Society of Tropical Medicine three cases in Nyasaland that came under his observation, where they apparently recovered after 15, 13, and 7 months, and he mentioned as the first case that of a native (in 1908) who is still alive and apparently recovered. I state that on his authority; I do not know it myself.

2082. You say that you consider these statements inconclusive from the point of view of the human reservoir, because there is, at all events, no statement in print that after the discovery of the tolerant native inoculations were made into animals of the trypanosomes in his blood?—Yes.

2083. Now with regard to the hereditary transmission of infectivity in the fly itself, we have been told that laboratory-bred flies have been fed on clean non-infected animals for periods exceeding in length the time which seems to elapse in the case of wild-bred flies biting infected animals, and which is necessary for the flies themselves to become infected. Now is it not a fact that that is not a sufficient argument, because in the first place the parent fly, from which the laboratory-bred flies were obtained, had not been definitely proved to be an infected fly?—Those experiments are really valueless from a scientific point of view. May I refer to one particular case that has been recently put forward? At the very time when it had been shown that it took 10,000 flies to produce a single infection, an experiment was put on record, as evidence of the non-inheritance of the trypanosome, in which 583 flies were bred and none of them was found infected. When from the very area from which those pupæ were taken, experiments on animals show that 10,000 flies were needed to produce a single infection, it seems absurd to suppose that you would get any

infectivity in only 583 flies of unknown parentage, so that experiment is absolutely valueless.

2084. Am I right in supposing that the only way of testing this infectivity would be to take a large number of laboratory-bred flies, which had been fed on infected animals, and which after the lapse of 30 or 35 days were made to bite clean animals? Suppose that you took those flies which were proved to have infected the clean animals which they lodged on and proceeded to take their pupæ, and bred from them, and kept those flies for the necessary length of time, and then experimented both on animals and through dissection of the fly?—That is the only way to arrive at a solution.

2085. Am I right in saying that that has never been done?—So far as I am aware, it has never been attempted.

2086. (Dr. Martin.) I quite understand your advocacy of the urgency of further investigations into the bionomics of tsetse-flies, and of course I sympathise with it, but I want to put this to you: supposing that it is decided to perform an experiment of the character generally outlined by Sir David Bruce, with which you are familiar, I believe,—?—I have never heard the details.

2087. But you have a general idea, I take it?—I do not know what the conditions of the experiment are.

2088. The general idea of the experiment was to enclose a certain area and to remove the game as far as possible from that area.—But what is it supposed to prove in that way?

2089. Never mind. I will not go into what it is supposed to prove. If I had to carry such an experiment out, and I asked you as an entomologist, from your present knowledge of *morsitans*, what precautions you would advise me to take in order to minimise as much as possible the immigration of flies from outside into that area, what would you say?—I do not see that you could take any precautions whatever to prevent it.

2090. No precautions that you can conceive would be likely to succeed, is that what you mean?—Yes; it is generally assumed, and I fancy that it was suggested by Sir David Bruce, that you would surround the area by a clear belt, but until we know whether *morsitans* is capable of crossing a clear belt of 10 or 20 miles, I could not say whether the area would be safe from immigration or emigration for that matter. I think you have to find out the migratory capacity of the fly first, before you make any such experiment. Otherwise you cannot decide what has happened.

2091. But in my case I have to do the experiment. You cannot give me any useful information. I understand, at the present moment?—All I could suggest is the marking of the flies in the area, because unless you can do that how can you check whether flies have come in or flies have gone out?

(Dr. Martin.) I am afraid I cannot regard that as useful.

2092. (Sir Stewart Stockman.) I think you said, or indicated at least, that the authorities responsible for settlement of a country should not send settlers to a country where there is fly?—I think it is an inadvisable thing to do, certainly.

2093. But you can imagine that it would be necessary to do it sometimes?—Well, possibly; but I do not know of any area in South Africa where it appears to be particularly necessary.

2094. I put to you a mere experiment. An experiment might possibly put you in a position to advise whether clearing out the game as has been suggested, would or would not make the place habitable for farming?—Yes.

2095. Would that not be rather a good thing to be able to advise? It is a question, as I daresay you know, that is always cropping up.—The experiment is of little value unless you can apply it in practice. The question is whether the conditions in your experiment are going to be such as you will get when you put the idea into practice.

2096. What do you think of it?—You might clear an area and have adjoining areas still containing game and fly. If you removed the bulk of the big game from the area and still retained the bush-pig and

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baboon and so on and then introduced large numbers of cattle, I think it would be an extremely dangerous experiment, because the fly might drift back again and live on the cattle. You have also other animals of the type of the bush-pig and baboon and they might still carry infection to cattle.

2097. Part of the experiment, however, would be to see whether the area was clean or not?—You would put the cattle in and risk them?

2098. Yes; that is how it has struck me.—I think it an inadvisable experiment, because it is practically tantamount to throwing away your cattle.

2099. That depends on the number, of course?—Yes.

2100. In reply to the Chairman you suggested that natives did not suffer particularly from *rhodesiense* because they had become immune owing to living in contact with the disease for generations?—That is a hypothesis.

2101. Another view which has been put before the Committee is that this particular trypanosome was only beginning to live in man and was finding some difficulty in doing it. Is that an impossible view in your opinion?—I think the results obtained from the men into whom it has gained entrance, flatly contradict that. It thrives considerably, to the detriment of man.

2102. Now with regard to domesticating wild animals, you include with the eland the zebra and the elephant?—Yes, and the buffalo.

2103. Up to date has it been tried and has it been a success?—I do not know that the eland has yet been domesticated, except an occasional few.

2104. Yes, that is what I mean?—I have on one or two occasions seen one working in a span of cattle.

2105. The attempts that have been made to domesticate the zebra have been rather a failure, have they not?—Yes. I did not suggest the zebra but the eland, elephant, and buffalo.

2106. The zebra seems a useful animal?—Perhaps, but if it cannot be domesticated it is not much use.

2107. Has the domestication of wild animals had a fair trial?—No, I do not think it has. Where it has been tried it has been thoroughly successful. The Belgian Government had 35 trained young elephants working on their station at the end of 1910.

2108. They have domesticated buffaloes in India, and have domesticated elephants. Can you tell us why they have never been successful in Africa to the same extent?—Because they have never tried.

2109. You really believe that they have never fairly tried it?—I am quite sure of it. I am not talking of the zebra. The zebra has been tried.

2110. (Mr. Austen.) Do you know of any published statements by a reputable investigator which tend to show that the antelope in Uganda appear to be acquiring immunity towards *Trypanosoma gambiense*?—Yes. I think that Dr. Duke's recent observations published in the reports of the Sleeping Sickness Commission show that very clearly. There were five buck which were inoculated with *gambiense* in 1910, and which by the middle of 1912 had entirely got rid of the trypanosome. He endeavoured to re-infect them, and, in two cases, he was entirely unable to do it. In the remaining three he produced temporary infection, lasting for only two months or so, indicating to my mind that those animals were not likely to prove a permanent reservoir. There is corroborative evidence in the observations made on the infectivity of flies along the uninhabited lake shore, that there has been a steady progressive diminution until the last count, which was a little higher than the one before. I have the figures here, and I find that Mr. Duke has made a small slip in his calculations. The first percentage of infectivity from November 1908 to September 1909, was .115. Another count was made between November 1909 and June 1910, by which time the percentage had been reduced to .037, or about one-third. Another count was made between August 1910 and March 1911. The percentage then was .009, or one-fourth of the previous one. From April 1911 to December 1911 the count was .014 per cent.; that is to say, about 50 per cent. higher than the previous

one, but very much below the two earlier ones. The two things together point very strongly to the view that antelope are not likely to prove permanent reservoirs of *gambiense* and Uganda sleeping sickness; at least, that is my interpretation.

2111. It is, I think, generally admitted that sleeping sickness has been introduced into Uganda by man, but it is contended that the antelope are now keeping alive the infection by acting as reservoirs. I think from what you have just said you consider the facts are diametrically opposed to that?—With the exception of two buck and one monkey on Damba Island, not a single wild animal has been found having *gambiense* in its blood, either by direct blood examination or by sub-inoculation by either the Uganda Commission or by investigators in German East Africa, so that there is a very large body of hostile evidence.

2112. We are told from time to time that it is impossible to attack the fly with any hope of success, but I take it from answers you have given to previous questions that you do not admit that?—I certainly would not admit it.

2113. If investigations such as you have outlined in your replies to Dr. Chalmers Mitchell should be undertaken, do you think it advisable to have such investigations in as many different fly areas as possible; I mean in as many different parts of Africa as possible so as to eliminate possible fallacies arising from local conditions?—Apart from the sort of central investigation I think it might possibly be feasible to second in various colonies medical officers who have been specially trained in the direction of entomological investigations to carry on work which could be co-ordinated with the work of the central body of workers. I think it very desirable that the investigation should cover as wide an area as possible.

2114. The marking of flies would be a highly important thing and must be done in a lasting manner. Do you know what was actually done in this direction in the case of the house fly investigation conducted by Dr. Monckton Copeman for the Local Government Board in this country?—No, I do not know what was done.

2115. You are not aware that in those investigations they found it sufficient to colour captured flies by means of coloured chalk powders?—No. The difficulty in handling the fly is so great that I would prefer any system that eliminated that. You cannot handle one of these flies without more or less damaging it, and as far as migration is concerned you vitiate your results to a certain extent. I would sooner use the system adopted for marking mosquitoes in Panama; that is by a very fine spray of coloured oils. They had to recover the colours spectroscopically because they were in such minute quantities.

2116. How is the colour rendered visible in Mr. Fiske's method? Does it colour the whole head or does he assume that it will colour the whole head?—I cannot say anything beyond that he made the statement that he was going to experiment. I do not know what the result has been.

2117. He has not actually tried it or similar experiments in America?—I think he did try. He was in correspondence with Dr. Bagshawe about it, and perhaps he knows more about it than I do. I do not know the details.

2118. (Dr. Bagshawe.) I do not think he tried anything before.—I do not know.

2119. (Mr. Austen.) Are you familiar with the Sebungwe district in Southern Rhodesia?—No, I have never been as far west. I know the Umfuli district only.

2120. Do you know the details of the experiment that Dr. Warrington Yorke has proposed shall be carried out there?—He proposes I understand, speaking generally, arming the natives, and killing the game, as far as possible, without any fencing, but I do not know what it is intended to prove.

2121. Does it appear to you that an experiment of that sort would be open to serious objection on the score of the probable introduction of factors which would vitiate the results whatever they were?—Everything depends on the object of the experiment, and

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unless one knows that, it is almost impossible to say whether one agrees with an experiment or not. Unless it is precisely stated what it is intended to prove by the means adopted, obviously one cannot give any definite opinion.

2122. So far as I understand it, the experiment is intended to discover whether the fly, if it remains after the destruction of the game, continues to be infective. Assuming that to be the object, what do you say?—I think that it will be vitiated by the probable continuance of animals like the bush-pig and baboon, upon which we know *morsitans* feeds greedily. The bush-pig is a very serious obstacle. In uninhabited districts they appear to be diurnal, but where harassed by man they become nocturnal in their habits. They are then very difficult even to see. They are quite abundant in the close vicinity of Salisbury. I heard only the other day from a friend of mine on a farm in Southern Rhodesia who wanted a remedy for them as they were devastating his crops. You cannot see them in the daytime where the cover is good. They do not inhabit holes, but lie up during the day in thickets and dense grass, just the very sort of place where the flies could feed on them, and if they carry *gambiense* or *rhodesiense*, they are a serious menace to any experiment that does not include them.

2123. Having regard to the locality, can you say whether there is any likelihood of the experiment being rendered useless by the incursion of other animals or other tsetse-flies from outside the area?—I think that is feasible, because the area is not very far from another fly area which is on the Umniati River. Certainly, the distance is not such as would preclude the possibility of the migration of the fly from one to the other, and it would be extremely difficult to get any really reliable results. Further, as I have suggested in the abstract of my views, we have already had an experiment of the kind in Portuguese East Africa. There you have natives armed to a very large extent, and you have an area with regard to which it has been stated that the game has been largely eliminated, if not entirely, and the investigation of an area like that seems to be a much cheaper and more effective way of settling the question than doing the work all over again for ourselves, apart from the undesirability of arming natives to any extent. If I lived in the region, I should be averse to any such proposal.

2124. Was there ever any sleeping sickness in the area mentioned by Major Stevenson Hamilton?—It has never been recognised since sleeping sickness has been investigated.

2125. So that unless the fly was actually infected with the human trypanosome now, you would not necessarily prove everything which it is desired to prove by the game destruction experiment?—No. If such animals as could be found there were found to contain *brucei* in their blood that would meet the case.

2126. As regards the *Trypanosoma brucei* but not as regards the human trypanosome?—There are a good many human beings there and if *brucei* exists in the blood of any animals there it would probably be communicated from them to man, if the organism is the same as *rhodesiense*.

2127. Yes, but not if it is distinct?—No. It would test that effectively.

2128. If they are distinct the human trypanosome need not necessarily occur in that area?—No.

2129. (Professor Newstead.) Have you any information at all bearing on the movements or migration of *Glossina morsitans*?—Extremely little. But what do you mean by "movements or migration" because the definition may be important?

2130. Seasonal movements from one locality to another, not necessarily over a very great distance, but distinct movement from one locality to another?—The evidence is conflicting. One or two people say quite positively that in the dry season the distribution of *morsitans* is considerably restricted. Mr. Neave in his paper on the subject, says that he does not agree with that, but I do not know that he has been resident long enough in a single area to give a really valid opinion. In many cases the statements that they do not

migrate or move in the dry season is based on the fact that you come across them in very large quantities in certain areas in the winter. My own view is that that is due to concentration. They have a much wider range in the wet season and concentrate during the dry season. Dr. Shireore in Nyasaland has, I think, very sensibly suggested that a careful study of the phenomenon would be of very material value; for by concentrating any clearing measures or other forms of attack on the areas to which the fly is restricted in the dry season, you may prevent their spreading over the whole area during the wet season.

2131. I was coming to that point. Do you think it really desirable that we should know something quite definite with regard to the movements of *Glossina morsitans* before such an experiment as Sir David Bruce and Dr. Yorke recommend should be undertaken?—I think that a most important point. I do not think that the experiments will be of any value until we know more about the intimate habits of the insect involved.

2132. Would you attach more importance to the removal of shade-giving vegetation than to the setting of traps for the collection of pupæ of *Glossina morsitans*?—Experience of insect pests of other kinds has conclusively shown that there is not one panacea to get rid of them, and that you have to attack them on two or three different lines, and that is one of the main objections I have to the game destruction proposal. I know that it has hitherto overshadowed everything else, but I think that it has many obvious difficulties and risks attached to it. In any case I feel very strongly that no one line will be sufficient and that we need to act on various lines. I would advocate the clearing and trapping of the flies themselves and an attempt to trap the pupæ also.

2133. But do you attach greater importance to the clearing of vegetation than the setting of such traps as have been suggested?—Well, they are in different categories. We do not know yet whether clearing kills the insect, but trapping has the great advantage that you know that you are actually reducing the numbers. By clearing you may be only driving it elsewhere. General experience shows that the fly does not care about cleared areas, and clearing is therefore likely to be of practical value. Clearing for the purposes of protection is a valuable proceeding, but clearing, as opposed to actual trapping, has the great disadvantage that we do not know what we are doing. We must find out whether clearing kills the insects or merely drives them away.

2134. If it killed the tsetse-fly both *Glossina palpalis* and *morsitans* would disappear entirely?—Yes.

2135. It has been suggested to us that it would be a good thing to introduce a natural enemy or enemies, and I take it you agree?—Yes.

2136. Would you advocate the introduction in the first place of a predaceous insect, and where would you look, for instance, for such an insect that you would hope to prey upon the tsetse-fly?—I should not anticipate much success along those lines.

2137. Would you consider the introduction of fungi as at all practicable?—I should hardly think so.

2138. Now, with regard to bacterial diseases, what do you say?—Bacterial diseases would have to be disseminated through the medium of the food; and that presents a considerable difficulty in this case.

2139. With regard to the question of birds (I refer more particularly to *Glossina palpalis*), it has been suggested that it would be an excellent thing to introduce Guinea fowl into Uganda in order to reduce the number of tsetse-flies. It has been suggested to us that these birds might prey on the pupæ of *Glossina* to a considerable extent, and would therefore be beneficial. Would you think it within practical politics to introduce Guinea fowl into Uganda which would increase sufficiently to reduce the number of tsetse-flies or make an appreciable reduction?—I thought there was already a Guinea fowl there.

2140. Yes, I know, but that is not the question exactly?—I think that the South African Guinea fowl, at least, would not live in the Uganda forest in the

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sort of localities frequented by *palpalis*. It seems to me improbable.

2141. You have referred, as also have previous witnesses, to the fact that the pupæ of *Glossina morsitans* are found (and I agree with this) largely if not exclusively (I am not sure what word you used) in, if I may use the term, rot-holes, or in natural pockets or cavities in trees. I have on a previous occasion called attention to the fact that there is at least one picture, and I think there are more in the paper which was issued or published in the Entomological Bulletin of the Entomological Research Committee, in which pupæ are shown in sites outside the pockets—in soil at the foot of the tree?—Yes.

2142. You admit that the pupæ of *Glossina morsitans* have been found in the soil at the foot of trees?—Yes.

2143. That is a point which I wanted to make clear, because I thought many might run away with the idea that the *Glossina morsitans* dropped its larvæ in inaccessible places and nowhere else. As a matter of fact, three out of four places in which pupæ have been found in Nyasaland were in the soil at the foot of the tree, and not in such pockets?—Yes.

2144. (Dr. Balfour.) Have you any idea of what is going to be the effect of railway and steamer extension in Africa on the distribution of the tsetse-fly. Is there any evidence about it at all at present?—I think it is likely to distribute them. The only case I can call to mind at present is Accra, on the Gold Coast.

2145. What has occurred there?—*Palpalis* has now been introduced into Accra by the railway.

2146. Do you think that areas in South Africa that are at present free might become reinfected when the Cape to Cairo Railway is more advanced?—It is a possibility. The eradication of the fly in various parts of South Africa has been due to factors which we cannot at present understand. In the north-west corner of the Transvaal the fly, for example, disappeared very considerably prior to the rinderpest, though there was plenty of game left. There was a factor which was adverse to them, and they disappeared even though there was plenty of food left there, so that reintroducing them might not necessarily re-establish them. Until we know the factor which is adverse to them, it is difficult to express an opinion.

2147. But it is a matter concerning which investigations should be made?—Certainly.

2148. Have you ever heard of instances of mechanical transmission of the pupæ of flies, as in the baggage of persons in caravans, and so on? Is that very unlikely?—Yes.

2149. The point was once raised by an officer in the Bahr-el-Ghazal. He thought he had evidence of it.—It would be so exceptional, I think, as not to be worth considering.

2150. As a question of historical interest, where did Hannibal get the elephants that he used? Were they not African?—It has been much debated, but the general consensus of opinion appears to have been that they were eastern. But the possibility of domesticating the African elephant has now been put beyond the possibility of doubt by the Belgians.

2151. (Dr. Bagshawe.) You said just now, as I understood you, that the first case of human trypanosomiasis was discovered in Nyasaland in 1908, and that the patient is still alive?—That is on the authority of Dr. Meredith Sanderson in the paper that he read to the Society of Tropical Medicine.

2152. That case was believed to have been infected in the Congo, as the native travelled in the Congo. There are other cases of natives living for longish periods with the disease; are you sure that they had not visited the Congo?—Dr. Meredith Sanderson does not qualify the information in that way.

2153. There are many cases of the human trypanosome having been found in Rhodesia and in Nyasaland among natives who have travelled in the Congo, and they are possibly cases of Congo sleeping sickness?—Quite so. I give for what it is worth what Dr. Meredith Sanderson says.

2154. I understood you to say that in Rhodesia and Nyasaland cattle were gradually acquiring immunity to trypanosome diseases?—I did not say in those

specific countries. I said there is evidence that they are doing so, but I did not mean specially there. I anticipate that it is going on.

2155. Is there evidence to that effect from Rhodesia or Nyasaland?—They have suspected it, owing to the origin of the disease in countries remote from fly areas. There are cases of outbreaks of trypanosomiasis in Southern Rhodesia for which no adequate cause can be given except that cattle apparently healthy have been introduced into a herd and there has been mechanical transmission. A similar case is reported by Mr. C. W. Howard from the coast belt in Portuguese East Africa. In West Africa there is a considerable body of evidence, I think, as to the existence of immune races of cattle.

2156. In Nigeria?—Yes, and the Gold Coast.

2157. We have been told by two or three witnesses that tsetse-flies do not feed at night and that therefore there is no possibility of their feeding on nocturnal animals?—That is the most misleading statement I have ever heard. It is not true that they do not feed at night, and in the second place the habits of the nocturnal animal are the essential point. With regard to bush-pig, they are nocturnal in their habits but they lie in the day just where flies are most likely to bite them. Antelopes, if harassed by men, become nocturnal, but they lie in the bush in the daytime and are bitten by flies. The nocturnal animals that hide in holes in the daytime might escape but even they may be out in the early morning and the evening when they are likely to be bitten. I think that all the carnivora of the type of civets and genets and things of that sort ought to be investigated.

2158. (Sir John Bradford.) I did not quite understand your answer as to how you discriminate between *rhodesiense* and *brucei*. I gather that one is a trypanosome affecting man and the other is one affecting cattle and game?—Yes.

2159. Did I understand you to say that you think that man is only occasionally susceptible to *rhodesiense*?—Natives, I said. I think they would acquire an immunity which I should not expect in Europeans.

2160. You think that white men would be susceptible?—Yes, I should say so. I think it is a legitimate inference.

2161. Putting aside the questions whether antelope are an important reservoir or not of the trypanosome causing sleeping sickness, do you regard them as at all a reservoir?—I think the evidence which we have rather points in the opposite direction. I think they are not likely to be at all a reservoir in areas where the human trypanosome has been thoroughly established. In Uganda undoubtedly it can be introduced into the blood of antelopes, or *gambiense* can; but the evidence shows that it is being gradually eliminated and those animals cannot easily be reinfected; therefore eventually they would not be a reservoir.

2162. You agree that in Uganda, *gambiense* has been found in the blood of antelopes in a state of nature?—Yes, on two occasions under very exceptional conditions, as pointed out by Dr. Duke himself. The conditions are very different from that of the Lake shore.

2163. I suppose you imagine that the trypanosome has been transferred to the antelope from man and not the other way about?—Yes, quite so.

2164. You have mentioned that cattle in various parts of Africa, either are resistant to the trypanosome or are becoming resistant?—Yes.

2165. Does that apply to all trypanosome infections or to particular trypanosome infections?—Well a great amount of the evidence is from West Africa and would involve quite a number of species of trypanosomes. I take it. I would not like to say precisely what trypanosomes.

2166. I want to know whether there is evidence that cattle are becoming resistant to the trypanosomes which are known to be very highly infective to them?—That again I would not like to answer offhand. In areas where *palpalis* are thick and attack the cattle in large numbers, the cattle are able to survive, but I cannot tell you exactly what trypanosome they are carrying.

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2167. That is a matter of importance?—Of course it is. They do not flourish in areas where *sub-morsitans* occurs.

2168. The whole question of whether cattle are becoming immune or not depends on which particular trypanosome you refer to. You admit that?—Certainly.

2169. The importance of it depends on that?—Certainly.

2170. Have you any information as to outbreaks of trypanosomiasis in cattle being dependent on the importation of cattle?—There is one case quite definitely stated by Howard, to which I have referred, in Portuguese East Africa.

2171. Have you any knowledge of any such occurrences in Uganda, for example?—No, I could not say offhand that I have.

2172. Have you ever heard it suggested that the presence of the trypanosome in game is dependent on the game having become infected from imported cattle?—No, I have never heard the suggestion.

2173. It has been suggested.—I have not heard it.

2174. Now as regards the supposed experiments, I understand your objections to the indiscriminate slaughter of game. Are we to understand that you object equally to an experiment conducted in a limited area to determine whether the percentage of infectivity of the fly is thereby diminished?—I think that further investigation is required before such an experiment is carried out with regard to animals which are likely to exist after attempts to destroy them, and which may still remain reservoirs.

2175. Do you not think the experiment itself is one which is quite capable of giving us a certain amount of information on many problems which at present exist?—Yes, but it resolves itself largely into the question of whether the results you will get will be sufficiently definite to make it worth the money and trouble it is going to cost.

2176. It is difficult in scientific work to foresee what the result of an experiment would be?—Yes, and therefore I think there are points which obviously will raise difficulties which should be settled before you start the experiment, such as whether there is one trypanosome or two, which can be decided by laboratory experiments.

2177. But do you, because there are obviously grave difficulties in the conduct of such an experiment and obviously many sources of fallacy for that reason, say that such an experiment is quite unsuitable and should not be entertained?—When you have an alternative method of tackling the problem, I think it undesirable that that experiment should be carried out until you have fuller knowledge on the points which are likely to make your experiment indecisive.

2178. One of the objects is to get further knowledge?—Yes.

2179. We must keep the object of the experiment quite distinct from the question of game destruction. You admit that the two things are on a different footing?—Yes, I agree; but when you have destroyed the game the interpretation of the conduct of the fly afterwards may be modified, depending on whether you know the intimate habits of the fly or not.

2180. Now with regard to *palpalis* and the Victoria Nyanza problem, do you think that the removal of the population from the lake zone has thrown any light on the scientific problem in connection with sleeping sickness, and has it been a valuable experiment?—I think so, but according to the reports of the two men I have cited the results are rather vitiated by the surreptitious return of natives.

2181. That may be, but there have been certain results?—Yes.

2182. The infectivity of the fly, for example, has altered, has it not?—Yes, very materially.

2183. Do you not look upon a limited game destruction experiment, supposing it can be done of course, as a somewhat similar experiment?—Yes.

2184. Therefore, it is possible that it might give us results at any rate of interest?—It all depends on whether, in that experiment, you are going to get conditions which it is possible for you to reproduce

if you apply the principles of the experiment to a wide area.

2185. I am not now concerned with that.—That is the only practical point at issue.

2186. There are two points, first, increase of knowledge, and also the point that certain regulations administrative and otherwise, to meet a given practical problem, and the two things are distinct?—Yes.

2187. Now I want to know whether, from a scientific point of view, you think it would be right and proper to conduct such an experiment to gain further information?—But will you tell me the nature of the experiment? So much depends on the precise nature of the experiment. Do you mean experiment to see whether the fly can persist in the absence of game?

2188. The experiment I have in mind has been mentioned several times, namely, whether the destruction of game in a limited area will affect the percentage of the incidence of infective flies.—I think you might gain information with regard to that, certainly, but I would like to know whether you are going to retain human beings there or not. To get a result you would want two experiments.

2189. You would not say that such an experiment is not one that should be made?—No, I should not.

2190. Now you have doubts as to the possible effects of game destruction, have you not, and the possible results?—Yes.

2191. Do you think that that might be used as an argument in favour of such an experiment as I am outlining?—The same objection arises again, and that is whether you can actually apply the conditions of your experiment generally. That is what I doubt, and that makes me doubt the value of general game destruction. You can kill off everything in an enclosed area with the greatest of ease; you can starve the animals out, but you might not succeed in doing that on a big scale in a broken country. The results you refer to may never be attainable in practice.

2192. I quite agree that you might draw deductions from the experiment which would not be borne out in practice.—Yes. In that way it is open to serious objection.

2193. The practical application of the experiment is open to serious objection?—Yes.

2194. Why not do the experiment?—It seems to me useless to do experiments, the results of which you cannot apply practically.

2195. One point you alluded to was that if the mammals were destroyed in any given area, the fly might persist and get its food supply from birds and reptiles?—Yes.

2196. Under those circumstances the flies would not be infective, would they?—It depends on whether man is there still. Presumably man would be there and they would attack him too.

2197. But supposing man were not there?—The whole idea of the experiment of game destruction has been brought forward because you cannot remove man and the alternative is to remove the game so far as Nyasaland and Rhodesia are concerned.

2198. You are looking at it from the human point of view?—Yes.

2199. Leaving out the cattle point of view?—Yes. I think that quite subsidiary to the human point of view.

2200. But there is no evidence that birds and reptiles are infected by pathogenic trypanosomes?—No.

2201. (Chairman.) In your experience how far are, firstly, fly areas, and, secondly, fly belts constant?—The biggest alteration in fly areas has been subsequent to the outbreak of rinderpest in 1896. There has been a very material alteration in fly areas since that, and it has been continually taking place as the fly has recovered the ground it then lost. Apart from that, I should consider fly areas to be approximately stable. The movements of fly belts within these areas are comparatively small. Sometimes the fly may be on one side of a river and next year on the other side of the river, or there may be slight general movements of belts. They are by no means fixed.

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Mr. G. A. K. MARSHALL.

[Continued.]

2202. You referred just now to the experiment made in Portuguese East Africa. Can we get access to any report of that?—Not an experiment.

2203. I will use another word. There has been a large destruction of game in a certain part of Portuguese East Africa. Can we obtain any report to show the effect of that on the fly?—We have Major Stevenson Hamilton's account to which I have referred.

2204. With regard to your suggestion that it is very expedient that entomological research should be entered upon on a large scale, is it your opinion that it should be undertaken as soon as possible, or that the report of Mr. Fiske should be first made?—I think it is advisable to await Mr. Fiske's report, because the whole mapping out of any scheme may depend very largely on the conclusions at which he may arrive.

2205. It has been suggested that there is the possibility of the fly itself being affected by some unknown

action at the rinderpest time, and that it did not disappear because of the disappearance of the game. Is the suggestion that it was some element which produced the disease itself in the cattle which affected the fly? Was the cause connected with rinderpest itself, or something independent of it connected with game?—It is mere hypothesis, but I should attribute it to an indirect effect of the disease—the fly feeding on the altered blood of the game.

2206. Do you mean some kind of poison arising from that?—Yes. I saw crowds of flies on a troop of sick buffalo at the end of 1895. The buffaloes, instead of stamping and swishing their tails and trying to get rid of them, stood listlessly, and the flies would be likely to feed on the animals that were most sick.

2207. (Sir Stewart Stockman.) Did you observe any falling off of the flies from the poisoning?—No, I did not. It did not occur to me to examine them.

The witness withdrew.

EIGHTH DAY.

Friday, 14th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (Chairman).

Mr. E. E. AUSTEN.

Dr. A. G. BAGSHAW.

Dr. ANDREW BALFOUR, C.M.G.

Sir JOHN ROSE BRADFORD, K.C.M.G.

Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Dr. W. A. CHAPPLE, M.P.

Sir EDMUND G. LODGE, Bart.

Dr. C. J. MARTIN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Mr. A. C. C. PARKINSON (Secretary).

Mr. R. B. WOOSNAM called in and examined.

2208. (Chairman.) You are Chief Game Warden in the East Africa Protectorate, I think?—Yes.

2209. How long have you been engaged there?—I have been there now for three years and two months.

2210. As regards the scientific aspect of dealing with trypanosomes and analysis, that is not the subject you have been engaged on yourself?—No.

2211. Am I right in supposing that in the East Africa Protectorate there is plenty of wild game?—A great quantity of game.

2212. And also *morsitans*?—I do not think *morsitans* occurs there.

2213. *Palpalis*?—Yes, on the shores of Lake Victoria.

2214. Does the human population live near the lake there?—Yes, one tribe, the Kavirondo, inhabit the country surrounding the lake.

2215. Has there been a large amount of sleeping sickness among the men in that district?—Very little; certainly less than there used to be. There used to be sleeping sickness in and around Kisumu itself, but the bush has been cleared a good deal, and certainly in the neighbourhood of Kisumu the fly has disappeared. A few years ago it was quite common to catch *palpalis* on the railway station and in the town itself, but I do not think the fly has been seen there now for two or three years.

2216. Have you, yourself, directed or witnessed clearing operations there at all?—No.

2217. Do you know how they were carried out?—Yes; a large number of natives were employed to cut down the bush all round the town. It was entirely cut down by hand.

2218. *Palpalis* does not go more than 400 or 500 yards from water?—Not as a rule, but I am not very familiar with *palpalis* myself.

2219. For the purpose of dealing with *palpalis* from that point of view you would only have to clear near water and not far inland?—In many *palpalis* areas the fly can be entirely exterminated by clearing.

2220. With *morsitans*, as you are probably aware, the conditions would be quite different?—Yes.

2221. Do you know when the clearing was done of which you spoke just now?—I should say between 1905 and 1909 or 1910.

2222. Was it done all over the Protectorate?—No. That was the only spot, as far as I am aware, where it was necessary, because sleeping sickness occurs only near Lake Victoria in the Protectorate.

2223. There had been none in the other areas?—There had been none, and there still is none.

2224. Was there wild game in the other districts and *palpalis* in the other districts?—As far as I know, Lake Victoria and the immediate neighbourhood of the streams running into Lake Victoria are the only places in which we get *palpalis* in the whole Protectorate.

2225. Where there was no *palpalis* no clearing was necessary because there was no sleeping sickness. Where there was sleeping sickness, *palpalis* and wild animals, clearing, as far as you can judge, got rid of the sickness?—Certainly, it has around Kisumu removed the fly, and sleeping sickness has been reduced almost to a minimum.

2226. Do any cases occur?—Yes, occasionally, but very few.

2227. You are not a medical man?—No.

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Mr. R. B. WOOSNAM.

[Continued.]

2228. You say in the memorandum which you have been kind enough to furnish to us, that trypanosomiasis is not a serious factor among domestic animals in the East Africa Protectorate at present, but is far more widely distributed than human trypanosomiasis. Fly areas are numerous, but the parts of the country which are occupied by European farmers are not in contact with these fly areas, and those animals which become infected are almost invariably in the possession of travellers, traders, or shooting parties. Has your attention been called to any large extent to the existence of disease among such cattle?—Yes. Those cases I think chiefly occur among shooting parties who have been passing through fly areas.

2229. What I wanted to ask you was whether you can say from observation of your own how long an animal which has acquired trypanosomiasis will live afterwards?—That is a very difficult question to answer, because one can never be quite sure when they have been infected. I know one or two cases of animals, mules and horses bitten on the Amala river by fly, which died very rapidly. No doubt it was a very virulent strain. But it is difficult to answer because one cannot be certain when an animal has been infected. It may be in a fly area for many months but not be infected until the end of its visit there.

2230. It has been suggested to us that you might by excluding game from or destroying game in a particular area, test the question whether the fly would, with the game, disappear, and secondly whether, if the fly did not disappear, it would cease to be infected. Have you had an opportunity of considering at all, if such an experiment was thought desirable, the manner in which it should be carried out?—I think that it could be carried out. I think that a certain amount of fencing would be necessary, and I think it would be desirable to have a certain amount of fencing if not to have an area entirely fenced. In connection with any measures taken afterwards to deal with the destruction of game on a large scale in other parts of Africa, it would be necessary to fence off certain places. It is quite impossible to exterminate the whole of the game all over the fly areas of Africa, and I think that fencing would be necessary for that reason and would be desirable. I should recommend that, if possible, an area should be found bounded by mountains on one side perhaps and possibly a river and the remaining sides or one side should be fenced. If that fence is put up in a certain way it is possible for game to jump out of the area you are trying to clear of game, but not to jump in again: this can be effected by putting the fence along streams even if it makes the fence longer, so that there is a drop outside and not a drop inside. The thing can be done. I have seen it done in this country in rabbit-warrens. A fence has been put up so that the rabbits could jump into the area and not out of it, which is the converse of what you desire in this case.

2231. What do you say with regard to a stampeding herd of animals?—A game fence must be a very expensive thing. No doubt in many instances it might be put up so that there was a river between the animals and the fence. That will stop stampeding. With regard to the Southern Reserve in East Africa it was decided to put the fence so as to leave the railway inside the game reserve. That would prevent rushes.

2232. What would be the cost of any fence of such a character as you have in your mind?—The conditions in Nyasaland, say, and in East Africa would be very different.

2233. Because of the communication?—Yes. The estimate for a fence entirely iron was 140*l.* a mile. That is the most expensive type but the most durable. It is No. 4 wire about the size of a pencil—and six strands of wire on iron posts; 17 big straining posts to a mile and 88 intermediate standards.

2234. That includes carriage?—That includes carriage to the Protectorate but not delivery up and down the line where the fence was to be put up.

2235. Perhaps something less costly might be adopted. If you had a systematic destruction of game in a particular area and no fence, would the tendency

of the game be to come back or would they be too frightened and stay away?—They would certainly come back unless it was immediately occupied by white farmers, or unless there was some form of cultivation and constant settlement. An area cleared of game naturally attracts the surrounding game into it.

2236. You think that if it was necessary to keep men out game would come back shortly?—Undoubtedly.

2237. Therefore, without a large army of men you would not keep the area clear?—No, I do not think so unless it was a curiously isolated area with a quantity of game and fly, but I know of no such area in East Africa.

2238. Have you had an opportunity yourself of observing the movement of the fly?—My own experience is that it tends to increase its area during the rainy season—not very materially—and to decrease it during the dry weather.

2239. As to its association with large game, what do you say?—In nearly all the fly areas that I know in East Africa game is numerous. There are one or two places at Kibwezi, and in that neighbourhood, where the fly exists, *pallidipes* and *brevipalpis* or *longipennis*, but game there is not at all numerous.

2240. Does the fly move with the game? Would you find fly where there was little or no game?—You may find fly where there is little or no game, but I doubt very much whether it is in any way correlated with the game. I do not think it is. In Bechuanaland, south of the Zambesi, I have passed over 200 miles of country exactly similar in every way, and with the same game, where you suddenly come to an area which is infested with *morsitans*. I have myself tried to find some explanation why the fly should be in this particular area and not anywhere else, but I cannot find anything. I must say that it is an exception to find an area like that, with country exactly similar. In East Africa a great deal of our fly is found along the courses of rivers or country in some way different from much of the surrounding country. The area of which I was speaking in Bechuanaland is most remarkable. I think Mr. Selous has mentioned it in his writings at some time.

2241. Have you formed any theory as to the food of *morsitans* in the absence of ruminants or wild animals of any kind?—It must live on some small animals, or possibly birds I should say, but I have never been able to get definite proof of that. It may exist without a feed of blood for several months, but I do not believe any blood-sucking flies can breed without having fed upon blood. That is the only thing one can say.

2242. If you yourself had to make a recommendation as to the best means of getting rid of fly from a particular district, what is the most practical suggestion you could make with that view?—If the fly was confined to anything like well-marked areas of bush or forest I would undoubtedly say clearing.

2243. Is it a practicable thing to clear a large area of forest and keep it clear?—Yes, undoubtedly; certainly far more practicable than clearing it of game. Once heavy bush is cut down it rather tends not to come up again.

2244. It is not replaced?—Certainly in most parts of Central Africa and East Africa, if you cut down entirely the forest on the banks of a river it is probable it will not come up again, or not for a very long period. It is not replaced.

2245. Do you think that the proposed experiment is not one that would be likely to lead to useful results? Would you go as far as to say that?—I feel myself that it is very doubtful whether any conclusive results would be derived from it.

2246. Would not even a negative result be of some use in advancing the inquiry?—It may be of use. I do not say it is, to find out that something does not happen. I would strongly recommend, if it is possible to carry out the experiment—if there is money to do it—doing it. As you say, even if it was only a negative experiment it would prove that it was not practicable on a large scale or was useless.

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Mr. R. B. WOOSNAM.

[Continued.]

2247. You think that such an experiment might be of assistance for the purpose of further inquiry, but your remedy is clearing?—Yes.

2248. Have you formed an opinion at all as to whether man is largely the carrier of the infection?—I think that man must be a reservoir and possibly a larger reservoir than we think. I think that when sick people are taken out of an area and put into a segregation camp it must often be the case that numerous persons suffering from the disease are not detected and are left. I think that man must be a more important reservoir than is generally imagined.

2249. From your observation, I gather you are satisfied that where the ground is cleared there will not be fly?—That is so in my experience. I have never seen fly out of bush.

2250. I want to follow that up by this: if domestic stock were kept in cleared areas there would be under no circumstances much danger of fly in the absence of wild animals being attracted to the domestic stock?—It depends on how far they are kept away from the fly area, and I think they would have to be kept further away in the rains than they would in the dry weather.

2251. If the effect of the removal of wild animals was to bring the fly to the domestic stock, the danger both to man and stock would be increased?—I do not believe that that would happen.

2252. (Mr. Read.) When you speak of bush taking some time to grow again on a cleared area you refer entirely to the East Africa Protectorate?—Yes.

2253. Where there is very little sleeping sickness. With regard to those protectorates where sleeping sickness is a serious matter, like it is in Uganda and Nyasaland, do you think the same thing applies?—Yes. I think that once an area has been cleared of bush, it is a comparatively simple thing to keep it clear afterwards.

2254. Even where there is heavy elephant grass as in Uganda, for instance?—You do not get elephant grass and bush growing together. You do not get forest and elephant grass together. Elephant grass grows by itself, and I have never seen fly personally in elephant grass alone.

2255. I gather that you think that any general attempt at extermination of the game would be futile, but it has been suggested here that there should be local extermination of game in the neighbourhood of villages. You know a good deal about game, and I would like to put this question to you, which I put at an earlier stage of the meetings of the Committee. Take the country in the neighbourhood of a village; one assumes that that country can only support a certain amount of game of all kinds, the big game, the medium game, as I will call it, like duikers, and so on, and the ground game, the smaller mammalia. If you succeeded in exterminating the big game, is it not possible that the medium game, the duikers for instance, might increase to such an extent, because there would be no competition between them and the big game which had been exterminated, as to be a very serious menace to the native plantations? I understand that they are very destructive animals, and by upsetting the balance of nature in that way, you might bring about a more serious state of things than existed before?—I do not think that that would happen. I think that the duiker and very small antelope are the most difficult to exterminate. If game is exterminated on a large scale, I do not think that any small species remaining would increase out of due proportion. Previously, I was referring to the necessity of using a fence in any experiment. What I meant was what you refer to now, and that if it was done on a large scale round villages, it would be necessary to fence the villages from large areas, over which extermination could not be carried out. I would fence off a portion of a big fly area under natural conditions, and exterminate the game in that portion, and not take an isolated fly area.

2256. Any general extermination in the neighbourhood of a village would be so costly as to be prohibitive?—Certainly. That is why I think it is prohibitive.

2257. (Sir John Bradford.) You say in your précis that in 1909 you met with several cases of sleeping sickness south of the Zambesi. Did they run a rapid course or a slow course?—I can only tell you (I had not a microscope at the time) that the two or three patients that I saw said they had been ill for one or two years. That was in 1908, and at that time *morsitans* had hardly been suspected of transmitting human trypanosomes, and I did not attach much importance to it myself. I brought back a great many flies with me, and Mr. Austen had them at the British Museum. They were all *morsitans* and not *palpalis*. I spoke to Dr. Bagshawe, and he thought it extremely unlikely that there would be a case of human trypanosomiasis, but I am satisfied now that there was.

2258. I am not throwing doubt upon it, but I want to know whether you think that the sleeping sickness you saw was in any way different from that which had been previously described in Uganda?—I saw four or five cases of natives and they said they had been ill for from one to two years. I can only give you that information.

2259. Now with reference to trypanosomes in cattle, have you from your own experience any knowledge of any sickness having been due to the moving of cattle?—Yes. A few cattle are brought down by traders from the northern parts of the Protectorate, I mean Boran and Rendile cattle. Some of them become affected and afterwards die, many die of East Coast fever, and cases of trypanosomiasis are not detected. The cattle from the north are coming from a clean area into an endemic area of East Coast fever, and the death-rate is very high in some cases, and frequently I think there may be undetected cases of trypanosomiasis.

2260. The presence of trypanosomiasis in game is looked upon by many as a serious danger to cattle?—Yes.

2261. What is your opinion about that?—I should think it has been conclusively proved that it is so. Inoculations have been carried out from game into susceptible animals. I think that sufficiently conclusive. In the case of human beings I think there is possible doubt about it. It is very remarkable that the percentage of fly infected with human trypanosomes should be so small compared with that which is said to be infected with animal trypanosomes. If the trypanosomes have been distinguished, it is curious that the fly infected with human trypanosomiasis should be only 2 or 3 per cent., while the fly infected with animal trypanosomes should be 10 or 13 per cent., while it is said that 50 per cent. of the game in the fly areas is infected with various trypanosomes.

2262. With reference to the cattle trypanosomiasis would you regard the trypanosomiasis of game as a more serious cause of cattle disease than, for example, the moving of possibly infected cattle?—Yes. I think it tends to keep up the infection in an area, whilst the moving of cattle through an area would hardly do so.

2263. I understand you look upon man as an important reservoir as regards human sleeping sickness?—I am not really qualified to speak on that.

2264. I am not going to cross-examine you about it, but I want to know whether the remarks apply to the sleeping sickness of Uganda or to that of other parts of Africa?—I meant them to apply particularly to Nyasaland and Rhodesia.

2265. You do not confine them to Uganda?—No.

2266. It has been found in Uganda, has it not?—Yes.

2267. Now, with reference to the effect of clearing on the presence of *Glossina*, do your remarks apply to *palpalis*, or to *morsitans*, or to both?—I think any fly, regardless of what the species is: if it is a well-marked area of bush or forest, clearing will do away with the fly. I have never experienced any tsetse-fly in open plain country. I have never seen it and I have seen cattle grazing in the Southern Reserve in the East Africa Protectorate within 200 yards of bush in which I know there is fly.

2268. There has been a good deal of clearing on the shores of Lake Victoria?—Yes, I believe particularly in Uganda, not so very much in East Africa.

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Mr. R. B. WOOSNAM.

[Continued.]

2269. Has that clearing led to a diminution in the number of *palpalis*?—Yes. In the case of Kisumu itself, the port on Lake Victoria on the East African side, the clearing has practically exterminated the fly round the immediate neighbourhood of Kisumu.

2270. Are you satisfied of that from your own knowledge?—Yes; there is no doubt of it.

2271. You are equally satisfied of the same fact with regard to *morsitans* in other parts?—I have never seen a *morsitans* area cleared, but I believe it would be equally successful.

2272. These remarks of yours apply to *palpalis*?—Yes, particularly *fusca*, in Uganda.

2273. They do not apply to *morsitans*?—No. I have never seen a *morsitans* area cleared.

2274. (Dr. Bagshawe.) What symptoms did the sick natives you saw show?—They were very much emaciated. I turned up an old diary to look it up. One man constantly fell asleep in camp two or three times in an hour. I do not know whether I examined them to see whether they had swollen glands or not, but blood smears were sent to Dr. Moffat and the result of the examination was negative. It is some years ago now.

2275. Could they walk?—They could walk, but they were obviously ill and torpid and in a very bad condition.

2276. Had they oedema?—Yes; all, I think, showed the oedema round the legs and ankles, but I cannot speak of anywhere else.

2277. If they were cases of human trypanosomiasis that would show that the disease has existed in man south of the Zambesi for a much longer time than is supposed?—Yes.

2278. So it is important to find out what they really were. It would be very difficult now to say for certain whether they were cases of human trypanosomiasis?—Yes, it would be very difficult. They might have been cases possibly even of malaria.

2279. In your summary you allude to the area in the neighbourhood of Kibwezi, in which there are two species of tsetse-fly, but game animals are very scarce. Were the fly numerous? It is very difficult to estimate the numbers of the fly, but can you give us an idea?—They fluctuate during the wet and dry seasons. The two species are *pallidipes* and *brevipalpis*, and *pallidipes* appear principally, if not entirely, during the rainy weather.

2280. Are they numerous enough to constitute a plague?—No. One can almost always find one or two. A few come into the railway carriages occasionally, but they are not so numerous as to constitute a plague.

2281. You say that you know of no fly areas in the East Africa Protectorate in which the game could be exterminated, except by an enormous expenditure. That is due to the large size of the areas?—Yes, and unless an experiment of that sort is carried out within a reasonable time it loses most of its value. I take it for granted that if no result comes for 20 or 30 years it is practically useless.

2282. What is the smallest fly area in the East Africa Protectorate in square miles?—It is difficult to say. Take the area on the upper part of the Tana. There is no doubt fly the whole way down the Tana though the areas are not connected. There is no doubt that a great deal of the northern part is all fly area and other places are along the courses of rivers. I would say 500 or 600 square miles—probably more than that—would be the very least.

2283. The isolated areas on any individual river would probably communicate?—Yes. I know several which appear to be isolated but which are not. There are small streams running into the big streams and you find fly all along the courses of the small streams.

2284. In East Africa is the fly, say *pallidipes*, specially associated with water?—Yes, I think *pallidipes* is. I have never seen it in very dry country. It is nearly always along the course of a stream. *Longipennis* is a fly I have seen in very dry country. *Pallidipes* is in country which is moist if not within reasonable distance of a stream. The country along

the coast of Mombasa is nearly always fairly moist and it is infested with *pallidipes*.

2285. In that respect it differs from *morsitans* which is very far from water?—Yes, I have seen *morsitans* a considerable way from water, particularly in the forest.

2286. It is very often said that the habits and habitat of *pallidipes* and *morsitans* are practically the same, but you do not think so I take it?—They are not according to my experience. I have only experienced them in widely different places. *Morsitans* I have only seen south of the Zambesi and *pallidipes* in East Africa. The country is very dissimilar on the whole. I have never seen *pallidipes* in anything like the sort of country I have seen *morsitans* in.

2287. You say further that you would recommend, if any expenditure is to be incurred, that money should be spent on clearing operations first and then on therapeutic and immunising research. Do you think that clearing operations would get rid of the fly or merely drive it away?—They would probably get rid of it altogether in the areas cleared, but there is always the danger of leaving a small area along a river bed, which is not cleared and in which a few fly exist. I think the clearing of an area exterminates the fly. The *morsitans* areas I have seen in South Africa are all in the mopani forest. It is a more practical undertaking to exterminate the trees than it is to exterminate game, although it is an enormous undertaking.

2288. If you only cleared part of the fly area would you expect to exterminate them to some extent or would they go to another part of the area?—I do not think it would have any effect on the area not cleared.

2289. I do not mean that. My point is rather this, that if you sought to exterminate fly by clearing in an area, you would have to clear the whole of the area?—Yes, I think so, certainly. I think it would be a dangerous experiment to leave any uncleared.

2290. You do not suggest an expenditure of money on the study of methods for destruction of the fly other than clearing?—No. The only method that has had any success was tried in Principe Island. Natives dressed in black and smeared with birdlime are said to have reduced the number of the fly, but it does not appear to be a very practical method. The only method I know of is the clearing of the bush.

2291. You do not think yourself that any other method is practicable?—I have never heard of one, and I cannot imagine one.

2292. (Dr. Balfour.) You have occasionally seen tsetse-flies in railway carriages you say?—Yes.

2293. Could a fly ever be transported over a long distance by train?—Yes. An instance came under my own observation. A fly was caught biting one of my polo ponies at Nairobi, which is at least 100 miles from the nearest fly area. That fly must have come in the train. There is no other way. Several flies have been caught at Naivasha station and at Nairobi station.

2294. Have you ever known an area become infested by that means—by flies being transported by trains or steamers?—No, I have never heard of it, and I do not think it is at all likely. I have constantly caught flies on the steamers on the Congo.

2295. A long distance away from a fly area?—No, in the immediate vicinity of a fly area.

2296. Will the fly follow a native boat out of the fly area?—I do not think they follow. I think it is quite exceptional if they do.

2297. That is true of caravans also, is it not—that they rarely follow a caravan?—Very rarely. I have had fly on myself and on the porters in a fly area. You may find them for a mile, but after that they disappear.

2298. (Sir Edmund Loder.) I am not a believer in a fence that will stand the rush of zebras which are being chased by lions or anything of the kind. I have seen them in my camp a hundred at a time. A stone wall would not stop the rush. Do you think a fence would?—Yes, but you must have a trench or something in front to stop the rush, and the fence must be really good. If possible it should follow the line of a river.

2299. The cost of the ditch would be extra to the cost of the fence?—Yes, and the fence has to be of a

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very expensive type. If it is put up in country in which there is good fencing wood, wood can be used instead of iron, but it means constant supervision to see that it is not burnt down by grass fires.

2300. If a fence was put up at any place in East Africa for any length on a river, there is the question of floods, which are a terrible thing, and men would have to watch to put up the fence again?—It would be very difficult. You might use the river as one boundary.

2301. In most places the rivers get very dry in some parts of the year, and if it was not fenced the animals would come through?—Yes.

2302. In flood time the fence would go, and you would have to have a gang of men at the side of the river to put it up again. No doubt it can be done?—It might be possible to have a fence which would swing one way but not the other.

2303. I have seen it tried in Scotland on a smaller scale and it failed.—You are trying, you see, in this case to keep the game not in an area, but from getting in.

2304. It can be done, but will it not be prohibitively dear?—Yes, it is certainly expensive.

2305. (Mr. Austen.) Have you ever met with a breeding place of *Glossina morsitans*?—No, I have never found the pupæ of any species of tsetse-fly.

2306. You mention in your statement a *morsitans* area in the marshes of Ngamiland, which was full of game, and you say that the boundary was most sharply defined. Game was also found outside, and there was no apparent difference to account for the restriction of the fly area?—No, except that there were possibly more buffalo inside the fly area than there were outside. They were not entirely confined to the fly area.

2307. On thinking over the case, do you consider that there is any reason to suppose that the area where you found the fly was a breeding area, while the area outside was not?—If so there is some governing factor that we do not know anything about.

2308. There was nothing to indicate it?—Absolutely nothing. The river was running through a mopani forest with marsh on both banks of the river to a certain extent.

2309. Was there anything in the shape of an excessive number of dead trees in the area where you found the fly?—No; the conditions were exactly similar. There are a certain number of dead trees. The mopani forest is about the same all over. The wood is rather like a beech wood. A number of the trees are twisted and cracked, and there are holes. I do not think you will find one area in which there are more dead trees than in another. It is about the same all over.

2310. Do you know the scientific name of the mopani forest tree?—No, I do not.

2311. You mention a place where game is very scarce though dik-dik are fairly numerous, where you find *brevipalpis* and *pallidipes*. Do you consider that the tsetse-flies feed on the dik-dik more especially?—They must, I think. There are a fair number of rhinoceroses, a few water-buck and a few lesser-koodoo. The dik-dik is certainly the most numerous species there.

2312. Do you think that the dik-dik furnish the major portion of the flies' food supply?—I would not say that. They may feed on birds or animals, but that is certainly the most numerous species of game.

2313. Of mammal?—Undoubtedly.

2314. Would it be possible to exterminate the dik-dik in a given area?—Yes. I think it could be done by trapping and game nets—by driving or even game nets without driving.

2315. Would other measures be necessary than would be necessary in the case of the larger mammals?—No. I think that trapping and driving into nets would be one of the most effective ways of clearing an area of all game.

2316. Of all animals?—Yes, and if there are isolated water holes, I would fence off the water holes so that they could not get to them, or could only get in and could not get out again. It would be a

trap. You see, they would jump in and not get out again.

2317. As regards water holes, the suggestion has been made that it would be advisable to poison them. Have you any opinion to express as to what the result would be?—Would it be possible to use a poison that could be eradicated afterwards?

2318. I have not any details.—I think it would be objectionable. I think it would be almost certain to lead to the death of a good many natives. They would certainly not stay out of the area.

2319. Would it be likely to have any effect on birds which prey on injurious insects such as locusts and things of that sort?—Not seriously, I think.

2320. It would not produce a prejudicial effect in that way?—No.

2321. But there would be a risk of poisoning the natives?—Yes, there would be a risk of poisoning the natives undoubtedly.

2322. Are you well acquainted with the eland?—Yes, I know it very well.

2323. Would it be possible to domesticate that animal?—Yes. I have seen numbers of them domesticated. They are very easily domesticated; one of the easiest of all game animals. They are one of the few animals which can be caught up to six months old and afterwards become quite tame.

2324. When they are tamed how is it possible to use them?—I have never seen them put to any practical use. Some farmers have kept them with the idea of spanning them in instead of oxen or with oxen, but they are certainly not as good and I do not think they will do the work. Some people have the idea of keeping them for milk, but they will not give as much milk as a cow obviously. Most of those I have seen have ultimately been sold to dealers, Barnum and Bailey and people of that sort.

2325. You do not think it would be possible to use them like trek oxen for haulage purposes?—No, I do not think they would ever be as good as trek oxen or do as much work. It is possible they might be kept for slaughter for meat. That is the only possible use I think you can put an eland to. For draught animals I do not think they are possible except as a fad or a luxury.

2326. Have you had any practical experience of cattle ticks?—Yes.

2327. Is it a fact that when game has been driven out from a tract of country and domestic cattle have been introduced, the cattle ticks have enormously increased in numbers in that district?—I have never seen a case of that, but I have seen a somewhat similar case which is rather interesting: I have seen a case of a man who had a large farm, a good deal of it fenced, who used to put his cattle at night into a boma or small enclosure full of mud trodden up by the cattle. The cattle were kept in during the night and let out during the day. Whilst he adhered to this proceeding the ticks undoubtedly became a good deal less. For some reason or other he allowed the cattle out at night and very rapidly, wherever they went, the ticks increased enormously. His theory was that they fell off at night in the mud and were trodden under foot when they were in the enclosure, and when they were not in the enclosure every tick that fell from an animal was able to lay eggs.

2328. I believe I am right in saying that cattle ticks are found on wild animals?—Undoubtedly, particularly the zebras. That is one of the most prolific carriers of ticks. Small gazelle and hartebeeste and such like animals carry very few.

2329. It has been suggested that when cattle ticks from force of circumstances have none of their natural hosts in the shape of game to feed upon, they attach themselves to cattle and increase in numbers, but you are not aware of that?—No, I am not.

2330. You say you think an experiment ought to be tried, but that you have grave doubts as to whether it would lead to any definite conclusions. Now why, if you have grave doubts whether it would lead to definite conclusions, do you still think it should be tried?—I think it would allay what I call the outcry of the general public, if it was tried and found either

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to be a failure or to be negative, and I think it might prove that if it was extremely costly on a small scale or impossible to exterminate game over a small area, it is far more impossible on a large scale. That is one of the benefits that would be got from it. It would be extremely difficult to exterminate everything in an area. It would have to be fairly large, say 25 miles each way, 625 square miles. It would not be much use dealing with a smaller area.

2331. Why should it be as large as that?—Do you not think it is possible that there might be movement of the fly?

2332. I think there certainly might. You regard the suggested experiment if it take place, as a sort of "sop to Cerberus," Cerberus being the general public?—Hardly. We cannot say for certain that it would be a failure, and I think in any case there might be interesting scientific results derived from it.

2333. What do you mean by the "public outcry" of which you have spoken? Is there in fact a public outcry in regard to this at the present time?—There is in Nyasaland and Rhodesia from what I read. In countries where people are in contact with the fly and where the idea is that the game is entirely responsible for the distribution of fly, and also for the disease it transmits, a large number of people are firmly convinced that extermination of game would be possible in those places, and that it would entirely do away with the fly, but they do not realise how long it would take.

2334. I take it that that is your impression from what you have read?—Yes, from what I have read and people I have spoken to. One meets a good many people coming up from South Africa and the first thing they say is "Look at your game reserve." A number of them have been opposed to it, because they imagine that wherever there is game fly will be present—I mean those people who are really thoroughly ignorant. With that class of man there is no doubt a wide feeling that game is entirely responsible for all the disease and trouble that there is with tsetse-fly.

2335. Do you know how they have got that idea?—I think chiefly because in most fly areas there is a good deal of game. That is one of the principal reasons.

2336. Yes, but I mean how have they acquired the idea that the game is a danger to their stock or to themselves?—To a great extent it is because they have been told it is a danger, and I think if the results are published with regard to pathogenic trypanosomes which have already been found in game, what I call the general public naturally will say "Here is the explanation of the whole thing."

2337. The general public gradually hears of scientific results and bases its opinion on them?—Yes, certainly. These things are published in the East African papers and many of the daily papers.

2338. Apart from trypanosomiasis in domestic animals, which is admitted to be connected with game, have you met many people from Nyasaland or Rhodesia who consider that game are responsible for human trypanosomiasis in those countries?—No; the people I have met would hardly separate the two. I think they call it fly disease, as a rule.

2339. Do they call it fly disease in men?—Some call it fly disease and some sleeping sickness. I think they generally realise that it is caused by tsetse-fly. Most of the men I have met have complained more because of its being a disease among domestic stock than a disease among human beings.

2340. Settlers in East Africa have no practical experience of the problem?—No; practically the problem does not exist. They speak of what they read and what they hear.

2341. (Dr. Martin.) You say that the settlers' opinion is that the game is a source of danger to their domestic stock?—Yes.

2342. You think that they have come to that opinion merely by being told so?—Partly by being told so, and partly because they see themselves that most fly areas are inhabited by considerable numbers of game—most of the *morsitans* area, certainly. One is bound to admit that.

2343. Do you think they are mistaken in their conclusions?—I think they are probably mistaken with regard to human trypanosomiasis.

2344. But with regard to their stock I mean?—It appears to me from experiments carried out that there is no doubt that these animals are harbouring the trypanosome pathogenic to stock.

2345. Yes, you have just told us that. You think they are not mistaken then in assuming that it is a source of danger to their own stock?—No, I do not think they are mistaken.

2346. Do you think that a clearing of half a mile would render the transport of *palpalis* negligible from your own experience?—Do you mean half a mile on each side of the road?

2347. No. Supposing you had a belt half a mile wide, do you think that *palpalis* would cross that to any great extent?—Yes. I think it might cross, but I think there would be very little danger to porters or caravans passing through that area. I think they would meet with very few fly passing through an area half a mile wide.

2348. That is not what I meant. If you had an area surrounded by a belt which is cleared by the timber being burnt and not allowed to grow up again—by periodical burning, do you think that *palpalis* would fly across it to any extent?—I think there is danger. It is too small an area.

2349. There might be danger of one fly, but do you think it a serious menace to the success of the experiment?—Yes.

2350. Would that apply to *pallidipes*?—Yes, to any fly.

2351. You think it of course with regard to *morsitans*?—Yes; I think it too narrow a strip.

2352. What size strip would give a reasonable hope of success?—What are you trying to do by the strip?

2353. Supposing you surrounded the experimental area by a cleared area with the hope of minimising as much as possible the ingress of the flies from outside to inside, what distance would give you a reasonable chance of success do you think?—Possibly a mile.

2354. I recognise that nobody has accurate knowledge with regard to this and I just want your impression.—Probably a mile.

2355. That would apply to *morsitans* as to others?—Yes, probably to all tsetse-flies.

2356. Do you believe it possible to form an efficient fence locally?—Do you mean without importing iron standards and that sort of thing?

2357. Yes.—Yes, but I think it would be far more expensive in upkeep.

2358. You are familiar with the type of country in Nyasaland?—No. I am not familiar with Nyasaland. That is the difficulty. I have never been there.

2359. I understand that the timber is small owing to the periodic burning that you get in many parts of the African Continent, that it is called "orchard bush" country, with scattered trees with grass between.—With regard to the cedar and other trees in East Africa, I believe the same species are found in Nyasaland. I should think there must be plenty of timber.

2360. I do not think that the timber is higher than 30 or 40 feet in the immediate neighbourhood. Could you make a fence of that?—No, it would not be strong enough for the straining posts. You would have to import some timber.

2361. You think that a game fence could be made locally, but on the whole it should be made of iron?—It would be so in East Africa. The transport conditions are different in Nyasaland.

2362. You think it could be done?—I think it could be done, but it would be a very expensive undertaking.

2363. I imagine it would be possible to repair the damage done by zebras about which one member of the Committee is anxious, and to drive them out even if they got in?—I am satisfied that game could be fenced out of an area or kept in. I want to impress upon you that it is quite useless to put up a fence unless it is a fence of the very first class, and it would be a very expensive undertaking.

2364. From your answer to another member of the Committee, I understand that you think that an

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experiment, if properly carried out, would lead to information of value apart from allaying the clamour that has been referred to?—Yes, I think it might lead to the discovery that the fly will disappear, or that it will become non-infective. Even if it did not, and the experiment was merely negative, the information would be of some use.

(Dr. Martin.) Any experiment may teach one nothing.

2365. (Mr. Rothschild.) You state in your summary that you think the experiment ought to be tried, so as to allay public outcry, but do you not think that if by chance a very favourable spot could be found in which to carry out your experiment, it might succeed on a small scale, and that then a further outcry would be raised in consequence of the experiment, that it must be carried on and the destruction must be continued on a large scale, and that then the whole thing would fail and cause a great deal of trouble both with regard to the natives and other people?—Yes, I think that is possible.

2366. (Dr. Chalmers Mitchell.) Apart from the possible value of the experiment, there is another question. I should be very much obliged if you would tell me how you would conduct it if you were ordered to conduct it, quite apart from whether you think it worth while. You said 25 miles each way or 625 square miles. You would take a portion of a fly area if you could not get an isolated one?—Yes.

2367. And if possible, one side should be steeply rising country or water?—Yes.

2368. And therefore you would not fence that side?—No, as a rule that would act as a complete barrier.

2369. You would have river on one side if possible?—You would have to have a really large river.

2370. Then you would be left with at least two sides probably that would have to be effectively fenced?—Yes.

2371. The mountain and the river would probably be effective boundaries?—Yes.

2372. And the other two sides which you would have fenced would be *here* (describing). Am I interpreting you rightly?—You would have to clear a belt as well as fence.

2373. That I understand. First of all you would suggest on the outside a strip of a mile wide *here*?—It ought to be so.

2374. The purpose of that would be to prevent the results of the experiments from being contaminated?—Yes.

2375. Inside the mile strip you would have your fence?—Yes; I would put it at the edge of the experimental area.

2376. How many feet high would that fence be?—It would depend on the situation of the fence. If it was put on a level with a ditch in front you would have to have at least six strands of wire practically six feet high. Eland are the best jumpers of all antelope and if you had to deal with them I would do that.

2377. How close would you mesh it?—It depends on the animals you have to deal with. If you dealt with the dik-dik and small duikers, as you certainly would have to, you would have to put sheep netting on to the wire.

2378. To prevent your fence being broken down by a stampede of animals you suggest having some sort of trap on the experimental side so that the rush is stopped before it touches the fence?—Yes. I would dig a fairly deep broad trench and throw the earth up between that and the fence. The game would get on to that and from that they would jump over the fence and that is exactly what you want. You may be able to make use of a small stream or donga so that a ditch may not be necessary on all sides.

2379. The expense of the ditch would be a considerable addition?—Yes.

2380. Do you think it would be possible to break rushes by cutting logs and putting them on rests about so high (describing)?—That would want a lot of supervision. Would it not be very liable to be burnt down by grass fires?

2381. You would not expect to have grass fires inside the 25 miles, would you?—But a grass fire may

come from the far side of the experimental area, and burn right through it and out the other side.

2382. You would have a clear strip?—But it would not be clear of grass.

2383. You would have to have a ditch, you think?—Yes, or very good fire breaks. You would have to have the ground cleared of grass and probably ploughed. Fire will work its way over areas with surprisingly short grass.

2384. That is carrying out the experiment in a first class way. Do you think that all the things you have spoken of are necessary?—You must have a fire break if you have wooden posts. Not all parts of the country are subject to really bad grass fires. It depends on the length of the grass, but in certain cases you would have to have a fire break.

2385. You spoke of an area where you found the country practically the same in every way—game and forest, and so on, one portion with fly and the other portion without fly?—Yes.

2386. Do you know anything about the distribution of the human population?—It was rather scanty. The natives cannot live far away from the rivers, because it is a very dry, waterless country.

2387. (Sir Mackenzie Chalmers.) As game ranger what is the general nature of your duty?—I patrol over the country, and I spend a great deal of time in my office.

2388. You do a good deal of game work?—Yes, a considerable amount.

2389. Do you watch the game in its distribution and so on?—Yes, or I am engaged in chasing somebody who has shot too much of it.

2390. What size camp have you—how many natives?—12 to 15.

2391. Have you ever had a case of sleeping sickness among your staff?—No; there are hardly any cases of sleeping sickness in the East Africa Protectorate.

2392. Can you clear by burning, or only by cutting down?—I think there are not many fly areas that you can clear by burning. If it was fairly dense bush, which was too big to burn, and a percentage of the trees were cut down and allowed to dry, it might possibly cause such a big fire that it would kill the remaining live trees.

2393. I rather understood that the *morsitans* areas were scrub?—Yes, in some cases they are. In Africa the word "forest" does not always mean forest, just as deer forest does not always mean forest. It may be scrubby stuff, half the height of this room.

2394. Is it fairly dense?—No. You can walk through it comfortably usually. There is no dense undergrowth in mopani forest.

2395. Could it be dealt with by burning?—No, not on the whole. I think a large portion of it would have to be cut.

2396. What is the distance from the clearing round the village that you mentioned?—At Kisumu I have seen clearing carried out. That is a *palpalis* area.

2397. What circumference round the village was cleared?—I should think two miles.

2398. Two miles away from the town?—Yes.

2399. Two miles from the centre all round?—The bush has not been entirely removed, but they have reduced the density of the bush a great deal.

2400. Are there any local cattle which are, so to speak, salted and resist the infection?—No. Where we find native cattle in close contact with the fly area, the natives are very well aware of the flies, and they know the areas exactly, and they do not take the cattle into the fly areas.

2401. It is not that cattle are resistant to the fly, but that they are kept away?—I do not think that cattle are resistant, because they tell you that if they take them into those areas they will lose them.

2402. As to the proposed experiment, you do not yourself know the Domira Road, which is a road going to the lake, and passes through 20 miles of fly area, and may become a very important road?—Is that in Nyasaland?

2403. Yes.—I have never been in Nyasaland.

2404. What is your opinion on the nature of the experiment that is suggested? It is suggested

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that five miles each side of the road should be cleared of game and fenced, and that from time to time any flies in the area should be examined to see whether they are infective or not. You would add to that yourself that not only should five miles be fenced, but that a mile outside the fence must be cleared. You have the road, and five miles off on the right you have a fence, and on the left you have a fence, and outside each of those fences you would suggest that an area of a mile or half a mile ought to be cleared?—Yes. I think that certainly ought to be done, otherwise it appears to me that after the experiment has been carried out, if it was found that the fly was still infective, you would find a certain school of people who would say that the infective flies had come from the non-cleared area.

2405. From outside?—Yes, and that would be found in the case of such a small area as 10 miles that you speak of.

2406. Would it be a test experiment in your opinion?—No, I think that the area cleared would be too small. If infected flies were still found in the area there would be a suspicion, I think, in nearly everyone's mind that they might have come from the area outside. Dr. Bagshawe was telling me recently of a theory of Mr. Fiske who thinks it possible that tsetse-flies may migrate *en masse* or to a certain extent. Information on that would be of the very greatest assistance before carrying out an experiment of this sort. It would certainly assist in fixing the size of area.

2407. An island was spoken of in which only situtunga were found and you have the infected fly and the infected situtunga. Could you not select an island?—I think you would have to find an island very considerably away from the mainland or from any other island. I am not familiar with the islands of Lake Victoria and I do not know if there is such an island.

2408. Do you think it would be a difficult thing to exterminate the situtunga from an island?—Yes. They swim and dive beautifully and they swim from one island to another.

2409. Can they swim two or three miles?—Yes, with ease, so you would have to fence the island.

2410. Another experiment suggested to us was in the Sebungwe district. There there is an isolated fly area and outside that for miles and miles beyond there are no flies, but this is a sort of island of flies that is about 2,500 square miles. Would it be a great undertaking to drive game out of that?—I think it would be an expensive one.

2411. If you were charged with the operation, what kind of staff would you require?—You would employ a good many natives. You would not require many Europeans. By fencing off the water-holes so that they cannot get any water in the area most of the game will get out of the area if they possibly can, or they must die in it.

2412. Would there be danger of the migration of game?—It would be certain to occur because as soon as game was reduced inside the area, game from outside would come in because the feeding would be better inside. In that case you would lay yourself open to the difficulty mentioned by Mr. Rothschild of the experiment being unduly successful in that one isolated area, and on that account people would cry out for it as a general measure.

2413. You think that no experiment should be carried out without fencing?—I think that fencing is desirable. That is one of the things that would have to be adopted if it is carried out on a large scale.

2414. Assuming the experiment with regard to the Domira Road was carried out (I am not saying whether it is a sufficient experiment or not), would it take long to clear the five miles on each side of the road?—I do not think it would take very long.

2415. It is said to be scrubby country. What kind of staff would you require?—The conditions might be very favourable for extermination. There might be places where game could be driven or easily trapped, but I could not answer without seeing the country. Trapping with steel traps is far more successful than pitting game. One would want certainly two or three

Europeans and 50 or 60 natives at least to get the experiment done within a reasonable time.

2416. Take an area of 20 miles long and 10 across?—I think it ought to be done within 12 months.

2417. (Dr. Chapple.) With regard to the cleared land, can you make any practical use of it after it is cleared?—If it is in a country that has sufficient rainfall, I take it, it could be used for agricultural purposes.

2418. If you cleared an area round villages, could you put the capital which you expended in clearing to practical use?—Yes, to a certain extent, but a great deal of the bush country if it is to be ploughed, has to be stumped. The trees have not only to be cut down but the stumps have to be taken out and that is one of the most expensive processes in clearing land for agricultural purposes. It will cost a great deal more to clear land and put it under agriculture than simply to get rid of the bush.

2419. If it was merely to get rid of fly you could burn down the bush, and then leave it in a rough state?—Yes.

2420. Under those circumstances could it not be put to any practical use?—In many parts of the forest in East Africa if you cut down the forest, in two or three years the ground underneath becomes covered with beautiful grass which was not growing underneath the forest originally. I do not think that that would happen in a *morsitans* area.

2421. If you felled the bush and burnt it, and scattered grass seed and put cattle there, would the villagers get any return from that?—It would depend on the country. Take the mopani country: the mopani covers the ground densely with leaves, and you never find much grass under a mopani forest. I think the grazing would be increased naturally if the bush was felled. The rainfall is not sufficient in the particular country I am thinking of for agriculture, and that is the case in most *morsitans* areas, I think. The rainfall is very deficient.

2422. With regard to the village in the instance you gave of the two miles being cleared, was any use made of the land?—Not for agricultural purposes. It is nothing but stones and gravel. It is hardly right to call it a village. It is a big port on Lake Victoria. The work was carried out to prevent disease in the town, and it is not possible to put the land to any such use as we have been speaking of.

2423. You would get some return for expenditure if you used the land for agricultural purposes?—Yes, but in the *morsitans* area the rainfall would not be sufficient for mealies or Indian corn, or anything like that. You would improve the grazing in some cases, but not all.

2424. Does the progress of settlement not require that bush should be cleared where you have villages?—Bush naturally becomes to a certain extent cleared round villages by the cutting of firewood. That is a natural process that always occurs to a certain extent, but generally the natives do not attack the big trees. The clearing round a village is not usually complete. It is only partial. They only take the wood that is easy to get.

2425. Suppose that after clearing you planted trees and grew crops. If by clearing you can drive away game and also fly, and at the same time improve your estate, and get some return for capital outlay, is not that an advantage?—I do not think that the clearing of bush necessarily means the driving away of game.

2426. I thought you said so.—No. The game may remain, but it will drive away the tsetse-fly.

2427. Will the game remain and the fly go, if you clear the bush?—It is quite possible.

2428. Do you know of instances of that?—Such an experiment has never been carried out. If bush is cleared it is probably for some definite purpose, and it is put under a crop or fenced.

2429. With regard to Lake Victoria, did the game not also disappear when the fly and the disease disappeared?—No, it remained as it was. There is very little game there indeed.

2430. The fly disappeared?—Yes.

2431. Does the fly disappear because the game does?—There are a few situtunga along the actual

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shore of the lake, and possibly water-buck, and so on, but the game is very scarce indeed round the neighbourhood of Kisumu. The disappearance of the fly had nothing whatever to do with game.

2432. Would you advise the clearing of certain areas around villages beyond the immediate necessity of the villagers themselves, in order to clear the place of fly?—That is exactly what I mean. I think that a fairly large area would have to be cleared, including the area in which they cultivate their crops, if indeed they do cultivate them there. Very often the crops are situated a long way away, where the soil is moister, and where there is sufficient rainfall.

2433. By clearing you would be paving the way for a more profitable use of the land?—It depends on the rainfall. It would not be the case in every instance. It would not be a criterion.

2434. You said you had a contract for 140l. per mile for the actual fencing. Did that include the erection?—Yes, and the transport from England to East Africa, but not the distribution of the material along the Uganda railway.

2435. From the railway station?—The material would have to be brought from Mombasa, and dropped at intervals along the line. That is not included.

2436. Could you form an estimate of what ditching would cost in addition to the 140l. a mile, and what the clearing of the mile strip would be roughly?—I could not give you an estimate of that for Nyasaland; I have not been there.

2437. Would it double the cost?—Certainly not. In that 140l. a mile you are taking the most expensive iron fence that you can possibly get; the ditch should not cost so much.

2438. A fence that you think would be effective for such an experiment as we are talking of would cost what?—Certainly not 140l. a mile. The fence I spoke of along the railway is permanent, but for an experiment, the fence need not be permanent; it should last for five or six years.

2439. Then, after five or six years, it would be wasted money?—The wire could be put to another use.

2440. Can you give us an idea of the cost of ditching?—The price of labour varies. It depends on the native labour.

2441. (Mr. Read.) It would depend on whether it was a thickly-populated country?—Yes, and on the type of ground in which the ditch was cut. Some ground is easily excavated, and other ground is not.

2442. (Dr. Chapple.) What wages do you pay the labourers?—Six to eight rupees a month in East Africa.

2443. Twenty-five miles each way; that is, 100 miles you have to fence provided you have no natural barriers. You cannot always have natural barriers, because you have to go where the fly is. You cannot induce the fly to occupy an area, can you?—I do not think so.

2444. You have to pick a fly area. You could not get them in one particular place?—I do not think it would be at all likely that that would be successful.

2445. Now, you spoke of a place south of the Zambesi being inhabited by game but no fly, and then an area with a large number of fly, but no game?—I do not think I ever said that. I said that I travelled through country in which I suddenly came to a fly area, but I could see no difference in the species of game or vegetation or other conditions. I have seen instances in East Africa of fly, but very very little game. I cannot say absolutely none, but very little.

2446. Do you know areas where there is fly but no game?—I do not know personally, but I have seen a case reported in the "Tropical Diseases Bulletin."

2447. You do not know of your own experience?—I do not know of my own experience.

2448. You said that trapping was a successful way of ridding an area of game. Do you think that you could do it by trapping, and means other than shooting?—Yes. I think that trapping and nets in certain places are very successful.

2449. You could exterminate the game then in an area without arming the natives?—It would assist to

do a certain amount of shooting, but it would not be necessary, I think, to arm a large number of natives.

2450. Is it your experience that game get very shy when you hunt them and keep away?—Yes. That is why some farmers have to resort to traps. The constant shooting makes them so wild that they cannot get near them.

2451. If around settlements you relaxed the game laws and allowed a certain amount of hunting to be done within a radius of a settlement, would you drive the game away?—Not entirely. You would reduce it by a certain amount if it was very numerous. In that area you would find people who made a profession of shooting game for the value of the hides, you would get skin merchants. As soon as it ceased to be profitable they would give it up. There would still be game there in small numbers. It takes 20 or 30 years, perhaps, before game entirely disappears before civilisation.

2452. Suppose that hunting were allowed within a radius of five miles, would it decrease the numbers, and tend to keep the game from the villages?—I think they would come into the five-mile circle at night. Game move far greater distances for water or food than people think.

2453. Would it diminish the numbers?—It would diminish the numbers.

2454. We have heard that they destroyed crops at night.—Yes; game is very destructive to certain crops, chiefly at night.

2455. The game would not be so tame or so "cheeky"?—I thought you were speaking of elephants.

2456. Yes, or anything.—At times game that can hardly be seen in the daytime comes at night to the garden and breaks down fences, especially when it is hunting for better grazing or for water.

2457. You would have to depend on the extermination of the game in the area?—Yes. If you want to clear an area in a reasonable time you must exterminate the game.

2458. Do you think that if you did that and got rid of the fly, the disease would disappear from those areas?—That is what we all want to know.

2459. I thought you said that where the game and fly disappeared, disease disappeared too?—Yes. That was at Kisumu. There was practically no game there before but there is certainly as much now as before. It would not be fair to take it as a criterion for any experiment.

2460. Is it not a pretty common experience in South Africa of the unscientific as well as the scientific investigator, that when the game and fly disappear the disease disappears both from domestic stock and other animals?—I have never heard of a case of that.

2461. In Uganda where they have cleared the people away from infected areas the Uganda sleeping sickness has been practically stopped?—It has disappeared among the people but the fly is still infected in the area from which the people were removed.

2462. Then the danger lies in the association of the game and the fly?—I do not see that exactly.

2463. If you find that the game is a reservoir of the trypanosome and that the fly is a carrier, then the existence of the incriminated game and the fly is necessary for the spread of the disease?—The presence of the fly is necessary, but we do not know that the game is the only reservoir.

2464. What other reservoir do you suggest?—I am under the impression that it was thought that when the natives were removed from the shores of Lake Victoria, that the disease would be stamped out. The natives have now been removed for three or four years but the disease still exists and the fly is still infected. Now it is thought that the situtunga is the reservoir but I am satisfied they cannot be the only reservoir. There are not enough of them to keep the fly constantly infected. There must be some other reservoir.

2465. You are not satisfied that the removal of the game and fly would protect the domestic animals in these areas?—I think the removal of fly would

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[Continued.]

certainly do so, I think we have no evidence at present to prove that anything but *Glossina* transmit trypanosome in Africa. I think that the cases must be very isolated if there are any at all.

2466. Are you sufficiently satisfied now that you would advise the clearing of areas round a settlement without experiment?—If a sum of money can be found to carry out the experiment, I think it would be a good thing to carry it out for many reasons. I would certainly start clearing operations where the disease is serious.

2467. You want the experiment, I think, more to satisfy public opinion than to satisfy yourself with regard to the necessary data?—Yes. That is an important object of the experiment because, if it is found that the cost is so great that it cannot be carried out, or that it is not worth bringing the experiment to a conclusion, that will show the public that it is not a practical measure to carry out on a large scale.

2468. From the figures you gave us, it would cost anything between 20,000*l.* and 50,000*l.* to carry out the experiment with regard to the 25 miles?—Yes, I think that probably would be so.

2469. If you were contrasting the value of game and the value of domestic stock what do you estimate the value of the game at? Which is the more valuable?—The domestic stock is more valuable always.

2470. What is the value of the game chiefly?—They supply the settlers with meat, and the revenue from game licenses is a very considerable sum. There are many farmers who like the game on their land because it keeps the grass down. It is a very considerable source of food. That is its chief value, I think. The hides are of value. In any case where the game is definitely proved to be a danger or not to be beneficial in any way, I would say destroy it if it is possible to do so; but it cannot be done except in a few cases.

2471. If the game were found to be the source of trypanosomiasis in animals, you would be prepared to have it exterminated?—Yes, if it can be done, but it is quite useless to spend an enormous sum of money on an attempt of that sort and achieve nothing by it.

2472. You think it cannot be done?—Not on a large scale.

2473. Has it not always been done where settlement has taken place?—Yes, but that is very slow. It takes a long time. I should think almost by that time, men would acquire natural immunity.

2474. You do not think that a relaxation of the game laws would be sufficient?—I think you can reduce the game, and that might reduce the percentage of flies infected, but I do not think you will entirely exterminate the game in any given area, unless definite steps are taken to do so. The removal of the game laws would reduce the amount of game, and the game will become very wild, but there will be ample game to supply the fly with food.

2475. Do you think that the settlers are against the extermination of game?—Certainly they are against it on a wholesale scale in East Africa. Extermination in certain places is a different thing, and some want it

exterminated from their own farms, but I think everyone would be against extermination of game throughout the country.

2476. Are they in favour of relaxation of the game laws or is their feeling against it?—No, the game laws are relaxed to a certain extent. A man can kill any game on his farm, which is doing damage.

2477. Is that the case in Nyasaland?—I do not know.

2478. (Chairman.) With regard to clearing round villages and keeping a clearing for the sake of the population, and possibly for agriculture, is the existing population sufficient to keep a space clear round villages?—Unfortunately I am not familiar with Nyasaland and I do not know. In East Africa the population is ample, but if they were employed on a work of that sort some measures would have to be taken to supply them with food which they at present grow.

2479. But at present is there sufficient population for the work?—Yes, speaking of East Africa.

2480. With regard to a fence, can you not conceive that some less costly fence might be erected?—Yes, but I think you lay yourself open then to having it constantly broken.

2481. I do not touch on the general question, but suppose that instead of an iron fence or a dead wood fence you had live wood. Would not live wood that would sprout be effective?—In my experience the trees which will sprout are very unsuitable for fencing and you could only use certain trees.

2482. Why are they unsuitable?—They are very soft wood.

2483. (Mr. Read.) White ants would eat through them?—Yes, and I do not know how you could attach wire. The posts must be bored so that the wire can go through. It will give with the strain; if you put up a fence and attach the wire to the posts with a staple, the wire cannot give. Sometimes the staple gets knocked out and the upkeep of the fence is enormously increased. If you bored the trees I do not think they would grow.

2484. (Chairman.) Am I right in saying that you do not think that even for a temporary experiment of this kind, anything short of the best iron fencing would be effective?—No, I do not say that. I think that there ought to be sufficiently good fencing wood found to put up a fence that will last for six years, but of course an iron fence is stronger and it is fireproof and the best results would be obtained.

2485. Would a wooden fence require much watching?—Yes, considerably more than an iron fence.

2486. Would a considerable number of men be required to look after it?—No.

2487. How many to 100 miles?—I should think four Europeans ought to be ample. They would probably have to go two together because some of these big heavy wire fences are difficult to handle. I should think four Europeans and ten natives would be sufficient for 100 miles.

2488. Dr. Balfour suggests that we should ask you to supply a diagram showing the fencing that you have been describing. Will you kindly do that?—Yes.

The witness withdrew.

Captain R. J. C. THOMPSON, R.A.M.C., called in and examined.

2489. (Chairman.) You are a captain in the Royal Army Medical Corps are you not?—Yes.

2490. You have had a good deal of experience of sleeping sickness in the Lado enclave, I believe?—Two years.

2491. Have you had experience of it in other parts of Africa at all?—No, not at all—just the Lado enclave—nowhere else.

2492. You have had to deal with the question practically there. Was there in fact much sleeping sickness there?—We found 500 cases in three years.

2493. Were they cases of *morsitans* or *palpalis*?—*Palpalis* almost entirely.

2494. Had you anything like an epidemic there?—No; it was endemic.

2495. You have never had epidemic conditions?—No.

2496. Do you consider the disease is endemic, and has it long been endemic in that country?—Yes, for about 15 years.

2497. Ever since you have known it?—Yes.

2498. Was it spreading or fairly stationary in your experience?—I think it was fairly stationary after the Belgians went.

(Chairman.) You will help us more on the clinical and medical side and other members of the Committee can follow that up much better than I can.

2499. (Sir Mackenzie Chalmers.) You have not been in Uganda?—No; I have just touched the northern part of entry but not been inside at all.

2500. You have not been in Nyasaland or Rhodesia?—No.

2501. What measures did you take to combat sleeping sickness?—We cleared a large area and tried

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to make it fly free and made a segregation camp. We patrolled the infected villages, and found the cases and segregated them and treated them as much as possible.

2502. What has been the result?—I think we have got the disease under control in the enclave.

2503. Were you successful in your treatment?—Yes, apparently.

2504. What remedies did you find best?—Plimmer's metallic antimony and salvarsan.

2505. What percentage of deaths did you have when you used that treatment; were you able to effect cures?—I think so, but it is rather early to speak about that. We have cases in most excellent condition after two and a half years after diagnosis.

2506. Going about and able to do their work?—Yes, very fit indeed.

2507. Could you still find any trace of trypanosomes?—No; we have inoculated monkeys and could not find a trace of them.

2508. In your opinion, are there two kinds of sleeping sickness? Do you recognise the Nyasaland type as different from the Congo type?—I have only seen the Congo type.

2509. It is all conveyed by *palpalis*?—Yes.

2510. And the trypanosome is *gambiense*?—Yes; I take it so.

2511. Have you made postmortem examinations of cases?—No.

2512. So that you do not know what changes there are in the body which account for death?—No. You have to be very canny with the dead. The natives would not stand it.

2513. We have not much information as to the pathological changes?—No.

2514. Are you still trying any new remedies or have you practically got into routine?—Routine practically. The antimony and salvarsan.

2515. Alternating?—Yes; five doses of antimony and one of salvarsan, and then five doses of antimony and one of salvarsan.

2516. Do the majority of cases answer to the treatment?—Yes; they seem to if they are in fairly good condition. If they are too far gone they do not. If they are in fairly good condition the great majority answer extremely well.

2517. Do you regard the *gambiense* type as a curable disease?—I cannot say that because it is too early, but we have got cases into good condition in two and a half years after diagnosis.

2518. (Mr. Rothschild.) Did you find that the reservoir of the disease was mainly human in the Lado enclave or mainly animal?—Yes, mainly human.

2519. Is big game very numerous there?—No, it is not. Elephant is very numerous, but otherwise game is not numerous.

2520. With regard to domestic animals in the enclave, did you find much trypanosomiasis?—Yes, especially amongst the transport cattle.

2521. And do you consider that that had been conveyed from the sleeping sickness cases in man, being carried by fly to the animal?—No.

2522. You consider that the trypanosomiasis of domestic animals is a distinct disease from that of men, and that it is conveyed by the fly from the wild game and not from man?—Not from man. I do not know where it was conveyed from. It might have been conveyed by other domestic animals.

2523. Had you any experience in clearing the haunts of the fly, and producing the destruction of the fly and clearing it out of the bush?—I think to drive the fly into the uncleared thick part of the bush is rendering the cleared part "fly-free."

2524. We have been told by other witnesses that *palpalis* frequents the borders of rivers?—Yes.

2525. If you cleared for half a mile back on each side of the river, would that clear the fly out—or a mile?—In the half-mile you would very seldom find them. They are driven back into the shady part, the uncleared part.

2526. Do you consider that there is so much swamp on each side of the river that it is more or less useless to clear?—You can clear a limited space. If

you want a place fairly free you can clear the banks of the river.

2527. I understand—you mean that if you clear in the neighbourhood of native villages, you do good, but that any large extent of clearing is impracticable?—Yes, any very large extent, because it would have to be kept up. Supposing it was several miles, if the growth came again, the fly would return. It is a question of labour entirely.

2528. A previous witness said that in East Africa where a piece of land is once cleared, it is not difficult to keep it cleared, and that elephant grass springs up and chokes off any attempt on the part of the scrub to reappear. That is not the case with you?—No, we have very little elephant grass.

2529. (Mr. Austen.) Do you consider that in the Lado enclave game plays a part in keeping up sleeping sickness among the human population?—No; that has never struck me.

2530. In your letter to the Secretary to the Committee, among the questions on which you say you are prepared to give evidence, you mention "Part played by game in spreading the disease amongst human beings"?—In my opinion it is negative in the Lado enclave.

2531. You have then no evidence that game plays any part?—No. The disease has spread on lines which are perfectly well understood. With regard to human communication, it has spread from a centre, and it is most easily explained by human carrying, and not by game carrying.

2532. (Chairman.) By means of fly?—Yes. The whole country is full of flies. I am speaking of human reservoirs.

2533. (Mr. Austen.) As a medical man, are you acquainted with any human disease due to an organism in which the main reservoir or an important reservoir of that disease is outside the human body?—Carried by a fly—a biting animal?

2534. Yes, carried by a biting animal.—No, I cannot think of any disease.

2535. (Dr. Balfour.) Do you know the north-western part of what used to be called the Lado enclave as well as the rest of that region?—Yes.

2536. Does *morsitans* exist there as well as *palpalis*?—Yes, *morsitans* is much more common there than in the south part of the enclave.

2537. Have cases of sleeping sickness been found there?—I only heard of four or five after the big inspection at the beginning of this year.

2538. Did those cases conform to the ordinary type?—Yes.

2539. Was there any difference?—No.

2540. You do not think that *morsitans* could have played any part in the transmission of the disease?—There is *palpalis* there.

2541. Did there seem to be any difference in the disease in the Moru district?—No.

2542. I have heard recently that there is some reason for thinking that in the western part of the Baïr-el-Ghazal the form of human trypanosomiasis is different perhaps from any form hitherto recorded, and I thought you might have found a difference in the Moru country also?—No, not clinically.

2543. Can you give us any information about the cost of clearing? You cleared a large area along the banks of the Yei?—Yes.

2544. Have you any idea of the cost?—200 labourers would cost two Egyptian pounds a day. That is the present rate of labour wages in Lado.

2545. In addition to that you cleared areas where the road from the Nile crosses streams?—Yes.

2546. Has that been effective in keeping away the fly?—Yes, just at the place where the bridge crosses the stream.

2547. Is it quite effective?—Yes.

2548. What was the diameter of the clearing? I think it is in a kind of circle?—It is about a quarter of a mile back on each side of the stream in the big ones, about 200 yards in the smaller ones, and 200 yards back from the edge of the stream.

2549. Is that effective?—Yes, but if a person goes to sleep in the uncleared part you cannot help it.

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[Continued.]

2550. If a person stood in the middle of the road would fly come to him?—Yes, if he stood and waited.

2551. Now with regard to the food of tsetse-flies, have you ever seen them feed on birds?—No.

2552. A brother officer of yours told me that through a field glass he saw a tsetse-fly feeding under the wing of a hawk. You have never seen anything of that sort?—No.

2553. Have you examined the blood in tsetse-flies?—No.

2554. Has your colleague done it?—I believe he is doing it now. He was not doing it before.

2555. Is he examining the blood of the game there?—Yes, I believe so.

2556. You have no information as to any results?—No. We found that the blood films taken from animals that had been shot were nearly all negative. A hartebeeste had trypanosomes.

2557. If you enclosed a comparatively large area, would it be possible to clear out all the game?—I do not think so.

2558. Why not?—I do not see how you could prevent other game from straying in to take the place of the others.

2559. Even if natives were kept constantly outside firing off guns and making a noise?—No. I am sure a herd of elephants would walk through.

2560. If there was a ditch inside the fence to check the charge, would not that meet the case of even a charge of elephants?—It would have to be a very cunningly constructed ditch and very large.

2561. For practical purposes you do not think it possible to keep game from coming in?—No. It would be a very serious thing from the point of view of meat hunger. If a native man has meat hunger, he will get meat even if he has to stray out of the uninfected area into the infected area to get it.

2562. (Dr. Bagshawe.) Have you any experience of the movements and migration of *morsitans*?—No. Where I have seen them they have been fairly constant.

2563. Their barriers do not shift?—The area seems to be limited and they do not migrate.

2564. Have you noticed any particular relation to big game in the area where there is *morsitans*? Are there more large animals there than outside?—No. I should say that the game is very evenly spread.

2565. Was there a patch of *morsitans* in the middle of the game area?—Yes. In the north-west corner of the Lado enclave there are an enormous number of buffalo more than there are elsewhere.

2566. You have not any definite evidence that *morsitans* carries the disease to human beings?—None at all.

2567. It is thought to be possible but you have not any definite evidence?—None at all.

2568. (Sir John Bradford.) I understood you to say that you looked upon man as the principal reservoir of human trypanosome?—Yes.

2569. Do you exclude game altogether?—No, but I have no evidence of it whatever.

2570. You have not worked on it yourself?—No.

2571. Have you made any observations on trypanosomiasis in wild animals?—No.

2572. Have you made any observations on trypanosomiasis in cattle?—Yes; transport cattle coming from the Congo.

2573. What trypanosome did you find in them?—The *nanum* and *pecorum* type.

2574. You did not find *gambiense* in cattle?—No.

2575. The only observations you made of *gambiense* were in regard to men?—Yes.

2576. Did you ever observe *Trypanosoma brucei* in cattle?—No.

2577. Passing to the clinical side of the disease in man, you said you had seen 500 cases?—500 were admitted. I have seen about 400.

2578. You had a very large experience. That is what I wanted to know. Did many of the cases run an acute fatal course?—No, none of the untreated ones.

2579. Perhaps I might put the question in a somewhat different way. Did any of these cases die after a few months illness with, for instance, epileptic seizures?—Yes, but a very few; a small minority.

2580. Some of them did?—Yes.

2581. They did not all of them run the course that one associates with the term sleeping sickness?—No.

2582. I perhaps used the word "acute" rather inadvisedly. I meant a comparatively rapid course in a few months instead of two or three years?—There were some, but they were in a very small minority.

2583. Did you observe in any of these cases any oedema?—No, not particularly; the oedema was more noticeable in the rather more protracted cases in the second stage.

2584. In your experience was the oedema a common phenomenon?—Yes, as the cases became advanced.

2585. When they became stuporose?—No, before that, when they were in the wasting stage.

2586. Did you find any other complications?—No, not in untreated people.

2587. Did you never observe opacities of the cornea or lens?—No.

2588. With reference to the clearing operations, what was the effect on the fly? Did you say that with clearing the fly diminished in number?—Tremendously.

2589. What fly was that?—*Palpalis*.

2590. Have you made any observation as regards the effect of clearing on the number of *morsitans*?—No, not at all.

2591. (Chairman.) What was the smallest amount of clearing that seemed to be effective? To remove *palpalis* from access to a road how far away would you have had to clear to make the road safe?—I should say at least 250 yards on each side of the crossing.

2592. Would that be sufficient?—Yes. It does for people just passing—crossing the road.

2593. You would not say that that would make that area safe?—No, but I think it enough for caravans just crossing the bridge.

2594. (Sir Mackenzie Chalmers.) Supposing that you were clearing round a village, what would you think would be a sufficient area to give that village real protection?—At the drinking places which women use to draw the water from the stream, never less than half a mile on each side.

2595. Did you clear by cutting or by burning?—Cutting almost entirely. We could not burn very much.

2596. Is it very dense? Could you clear rapidly?—It is extremely dense, but the natives are extremely good labourers and clear very rapidly.

The witness withdrew.

NINTH DAY.

Tuesday, 18th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.
 Dr. A. G. BAGSHAW.
 Dr. ANDREW BALFOUR, C.M.G.
 Sir JOHN ROSE BRADFORD, K.C.M.G.
 Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
 Dr. W. A. CHAPPLE, M.P.
 Colonel Sir W. B. LEISHMAN, F.R.S.

Dr. C. J. MARTIN.
 Dr. P. CHALMERS MITCHELL, F.R.S.
 Professor R. NEWSTEAD, F.R.S.
 Mr. H. J. READ, C.M.G.
 The Hon. L. WALTER ROTHSCHILD, F.R.S.
 Sir STEWART STOCKMAN.
 Mr. A. C. C. PARKINSON (*Secretary*).

Sir JOHN KIRK, G.C.M.G., K.C.B., F.R.S., called and examined.

2597. (*Chairman*.) I need not, before this Committee, ask you if you have much experience of Africa, Sir John; it goes a long way back, I think?—It goes more than half a century back; I was there in 1858.

2598. What I think I had better ask you to begin with is this: when you were there in the early days, days of great historical interest to all of us, I suppose that you and your party were in contact with tsetse-fly constantly?—Constantly. On the march we passed through areas of the tsetse-fly and then into free country, all along the Zambesi. You began about 200 miles from the coast and you met with tsetse-fly in patches all along. It was never in great areas, always in limited areas. All around Sesheke, which is a good deal beyond the Victoria Falls, and up by Linyanti you met with tsetse-fly, but always in patches, sometimes narrow and sometimes wide.

2599. Were they *morsitans* or *pallidipes* or any particular class, or did you meet all sorts?—I believe they were chiefly *morsitans*, but, of course, I am not an entomologist. I made collections then, but unfortunately they were lost in the Zambesi when my canoe was upset. That was on the upper Zambesi. I sent home certain specimens from the Zambesi and from Nyasaland to the British Museum, but I do not think any that came from the Upper Zambesi ever reached home. I believe it was *morsitans*, and there was another species which, of course, I could not identify.

2600. In those days I suppose there was always plenty of large game?—Abundance; we lived on game. We took hardly any provisions with us, and we shot for a party of from 90 to 100.

2601. Were you, your party and your men, bitten constantly by fly?—Constantly.

2602. At that time I fancy sleeping sickness was not identified, but had you any experience at that time of a disease among your men which resembled what we now call sleeping sickness?—None whatever. I have been bitten, and my men were bitten, and I have lived among natives many of whom lived in tsetse-fly areas, and also with natives who passed through tsetse-fly areas, and I never heard of any disease that they identified as caused by being in contact with the tsetse. They knew it killed their cattle, that they knew perfectly well; it was their business to know, but they never associated any disease with it.

2603. At that time I suppose the trypanosome had not been identified as the cause of that or any other illness?—Oh, no, it was unknown as far as we were concerned; in fact, I do not know when it was identified, but certainly it was never associated with sleeping sickness or with any disease.

2604. When did you first come in contact with sleeping sickness, if ever?—I never came in contact with it until as a member, vice-chairman, of the Uganda Railway Committee. After I had retired from the service I was employed at the Foreign Office,

and when the railway was completed I went out to see it and examine it. Then I was at the hospital at Entebbe and saw cases there and saw the treatment, but at that time the trypanosome had not been associated with the disease. That was just before Colonel Bruce went out; he followed me about a fortnight afterwards, but I did not meet him.

2605. Was there much sleeping sickness there?—Yes, there was; I went to one island which was almost depopulated; it had had a population of about 3,000 at one time, and there were not 300 left.

2606. Do you know what the fly was there?—I believe it was the *palpalis*; I think that has been identified. I do not pretend to know myself.

2607. But possibly from what you have heard, can you tell us?—I have always heard it was *palpalis*.

2608. You may be aware it has been suggested, especially as regards *morsitans*, that the presence of large mammals in a country where there is *morsitans* does lead to death among the domestic stock, and is likely to produce sleeping sickness in man, and for experimental purposes it is suggested that an endeavour should be made in the fly country to exclude the larger mammals and see what the result is; have you heard of that at all?—I know that has been proposed.

2609. Have you formed any opinion as to, first, the possibility of doing the thing at all and, secondly, the probability of something being learned therefrom?—I think it would be practically impossible to do it in an effective way—to include such an area in Africa in such a manner as would exclude the possibility of wild animals breaking in, or man, because you must remember that man may be as infective as wild animals, passing into the area. I do not think it would be possible to include such an area as would be sufficient to allow of any experiment, but I think the experiment has been made already. When I was at the Victoria Falls with Livingstone in 1860, on the left bank at the Victoria Falls there was no tsetse-fly and there were cattle there. On the south, on the other hand, there was a big belt of tsetse-fly so that any half-caste Boers hunting could not enter with their horses because they knew they would die; the natives knew also; they were cattle keepers and cattle stealers and they knew that they could not take cattle into that area. Now I am told in that very area where I saw the tsetse-fly, where I saw buffalo, elephant and rhinoceros abundant, there is an hotel and there is civilisation and the fly has disappeared, and there is no sign of disease among the cattle there, which shows, I think, that the experiment has been done and that somehow or other the fly and the disease in cattle have disappeared together.

2610. Is it not the experience of those who know best that where there is human settlement the fly itself almost invariably disappears?—The natives told me that if they made a village in the fly country the fly would disappear from the small area round the

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[Continued.]

village and that they could keep cattle if they kept them in compounds, but that they could not allow them to go out to graze because at a small distance, I cannot say how far, but anyhow within the distance the cattle would roam out to feed, they would get into the fly. They told me (and I only give it on native evidence) that the fly would disappear from the immediate vicinity of a native village.

2611. Then it does come really to this: it is a question of degree, but a large human population and all the appliances necessary for it does have the effect of banishing fly from that area?—That is so. That has happened in the Transvaal and it has happened in Southern Rhodesia near the Victoria Falls. With the advance of civilisation and the population of the country by white men and so on, the fly seems to disappear.

2612. With regard to the actual experiment suggested, it might, might it not, result in our learning aye or no (it might be either way) that in the absence of the larger mammals the fly either departed or ceased to prosper? It is possible that that might be learned from the experiment?—Yes, if you could carry out the experiment effectively for several years, but I think in Uganda they find that the game remains infective and the fly remains infective for a good long time after the population has been removed, so that it would require to go on for a long time before you could be satisfied. I do not see how you could protect such an area, given the conditions you have in Africa where the fly exists, because the fly does not exist near civilisation; you must go into the wild part.

2613. Still it is a possible result that you might ascertain whether or not the fly disappeared and whether or not it became either non-infective or comparatively less infective?—Yes, of course, if it could be done on a sufficient scale, but it would be rather a heavy price to pay to exterminate the game on the chance of that.

2614. I see you say in your memorandum which you have been good enough to send us that "the fly does not follow the game outside these areas" (that is the fly areas) "to which for some reason unknown as yet it is limited"; but am I right in thinking it is pretty nearly common ground that there are only certain countries or certain forms of scrub or bush in which you find fly, and if you go outside that the fly will not either follow you or remain outside; in other words, they require a particular form of shade and a particular form of country in order to prosper?—The fly is definitely limited to certain areas and what those are I do not understand; I have gone through scrub and seen them, and I have gone through mopani bush and seen the tsetse-fly; but there is some condition which I do not think we have yet ascertained, which confines the fly's existence to certain limited areas, and it is quite certain that when the game moves out of those areas the fly does not follow the game.

2615. Have you any personal experience as to whether the fly is found in large quantities where there is little or no wild game?—No, I never found it, but one must remember there are many species of tsetse-fly and we would require to study the bionomics of each species separately. For instance, there is no doubt that the habits of the *palpalis* are in many ways different from those of the *morsitans*. Probably the country which suits the *palpalis* would not suit the *morsitans*, and there are so many other species of the tsetse-fly that probably we would have to follow them out individually for each species.

2616. I might put the same question more generally have you any experience or do you know of cases at all where there have been large quantities of tsetse-fly with an absence of wild game?—No; I have never met with it.

2617. I see you suggest, for reasons which you give in paragraph 4 of your statement, that fly-infested areas should be thrown open to hunters, sportsmen, and settlers with a view to driving the fly away, but might not that involve the danger of creating or spreading sleeping sickness among the men who settle there, on the assumption which you yourself suggested just now that man himself may be a reservoir? If, before the fly was out, you put a large

population into the fly country, would not that involve a somewhat substantial risk of creating and spreading sleeping sickness among men?—In the *morsitans* districts of Rhodesia and so on I do not think there have been many cases of sleeping sickness yet, and it is not quite certain that it is the same disease.

2618. I quite accept that, Sir John, but still assuming, for the purposes of this question, man to be a possible reservoir, there would be danger in making a large settlement of men in a *morsitans* area?—I do not think you would begin by making a large settlement. I think I should admit hunters to go in and hunt; they would not be long in and they would not be liable to any risk. I and my companions have been in tsetse-fly countries and have been bitten by the tsetse-fly, and it has never done us any harm. I am speaking of *morsitans*, of course; it would not be so with *palpalis*.

2619. I gather that you have not studied the habits of the fly, and you could not help us with regard to any manner of attacking the fly?—No, I know nothing about it.

2620. There is one more point, and that is whether you have ever considered whether there are any species of birds which might be introduced which you think would tend to limit or reduce the numbers of fly?—I should doubt it very much. Of course the birds eat the tsetse-fly, but whether you would ever get birds sufficient to keep down the tsetse I should doubt very much. The birds are there now.

2621. (Professor Newstead.) I should like to ask, Sir John, whether you have ever met with tsetse-flies of any kind in scrub or in areas where flat-topped acacias preponderate?—Yes, you get them in that country; I think along the south bank of the Zambesi, halfway between Lupata and the Victoria Falls, you have pretty open scrub country, and you have lots of tsetse-fly there.

2622. You have no mopani in such country at all?—You have mopani forests there; the mopani forest sometimes comes down pretty near to the water.

2623. Has it not been your experience that where you get flat-topped acacias growing in profusion there have been few or no mopani trees at all?—Yes. I think it is usual. I found tsetse, *morsitans* chiefly, associated with rather open bush country, not very dense bush country.

2624. Such country, for instance, as you would describe as more or less park like?—Yes, a little more bush than you would call park; I do not think you find so much in the open park country.

2625. Have you ever seen tsetse-flies in the open dambo or savannah?—No, I do not remember noticing them to any large extent in the open country.

2626. May I ask also if you have ever seen tsetse-flies of any kind feeding upon wild animals—big game?—Of course you would never get near enough the wild game to see them until they have been killed; I have seen them go to the dead animal.

2627. (Sir Stewart Stockman.) I think your opinion is that the slaughtering of game is impracticable in an area of the kind proposed?—The slaughtering is practicable, but the keeping of other game from coming in again is very difficult, unless you were to fence it in.

2628. I see you suggest also that the game should be driven out?—Yes.

2629. You think that is practicable?—I believe that would be practicable.

2630. It would serve the same purpose, I suppose?—I think so. I believe that the tsetse-fly depends on the game. I learned that chiefly from being in contact with the natives who live with their cattle, and look on the cattle as their money, and look upon the cattle held by another tribe as fair game to go and steal. These men have studied the whole question thoroughly, and they know the danger of entering the tsetse-fly country. They know exactly the limits of the fly country; they know where it is, and where they can cross it at night when the fly is asleep, and they know that if unfortunately they get into the wrong crossing through that country, they will lose all their cattle.

2631. You think then it would be quite practicable to drive the game away, and test the virulence of the

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fly afterwards?—You would have to do it over a big area and then keep it from coming back again.

2632. That is why I asked.—I do not think it would be so practicable unless you fenced it in, and I do not think that would be practicable. I do not see how you could keep other game from coming back, and even if a few head came back it would vitiate your whole experiment.

2633. You suggest that reserves for game should not be made in fly areas. Would it equally serve the purpose if you made the game reserves in the places where you get the game to stay where there are no fly?—Yes.

2634. They do not specially like the places where the fly exist?—If you follow the Uganda Railway you get immense areas where there are no fly and abundance of game, so that you could easily select a large area quite suitable without entering fly country.

2635. (Dr. Chalmers Mitchell.) In your early days in Africa you told us that, of course, nothing was known about sleeping sickness and trypanosomes, but the natives understood quite well that their cattle were killed because they were bitten by the fly, did they not?—Yes, they knew that.

2636. They knew it was the fly that killed them?—They knew that, and they knew the danger to their own cattle.

2637. They knew it came from the bites of the flies?—Yes, they associated it with the bite of the fly, and they knew that they would not know their total loss until the next rainy season began. A great many would die very soon, within a fortnight or three weeks, but they never knew their total loss until later on. If the rainy season came soon afterwards they knew their total loss sooner, at least so they told me.

2638. And they knew the kind of sickness that came from the attacks of these flies as being different from other kinds of sickness?—Yes, they knew that perfectly.

2639. You are sure they knew that very well?—There is no doubt of that.

2640. So that probably, do you not think, they would have known and distinguished it from other kinds of diseases, if they themselves were dying from fly poison?—Certainly, they would have recognised it, and if they had died in any numbers they would have known the danger to man of entering that zone. They were passing through it constantly, and I never heard of any illness coming from the bite of the fly. It always seemed to us a most extraordinary thing that the fly should kill domestic cattle, horses, cows, and sheep, and not kill man or have any effect upon man.

2641. So that you are fairly clear that in fact human beings were not killed by the sleeping sickness then?—I think then there is no doubt they were not killed.

2642. (Sir Mackenzie Chalmers.) When did you first hear of sleeping sickness as a definite disease?—I heard of it in London when I was on the Royal Society Committee. We heard that sleeping sickness had been brought into Uganda and was advancing. That is the first I ever heard of it. Afterwards I knew it was endemic on the Congo and also up the Niger, and on the Gold Coast in patches, but I never heard of it while I was serving in Africa during the 30 years I was there.

2643. That would be about 15 years ago, I suppose?—It is 25 years since I was there.

2644. But you first heard of sleeping sickness 15 or 20 years ago?—Yes.

2645. (Sir William Leishman.) We have had it suggested to us by a previous witness that the great difficulty would be to deal with the bush-pig. Have you had much experience of this bush-pig?—Yes.

2646. Do you think it would be possible to drive it out of the area which was proposed to be enclosed? We have been told it would not be possible.—I do not see why it is impossible. Of course they have to be hunted; they go into holes in the ground, but they come out in packs feeding, and they are easily shot.

2647. (Dr. Chapple.) Did I understand you to say that when game moved the fly did not follow them?—Yes, the fly does not follow the game; if the game is driven out or moves away or migrates out of an infested area the fly does not follow it.

2648. The fly remains?—Yes, the fly remains in its locality.

2649. And lives on?—I am speaking of the game migrating. There may be game enough left to keep the fly going. I never heard of the fly disappearing except where civilisation has advanced. Where civilisation has advanced then the fly disappears.

2650. Have you associated the disappearance of the fly with the disappearance of the game before the advance of civilisation?—I think I should be inclined to do so.

2651. That is the important point, is it not?—Very; the natives all associate the presence of game with the presence of the fly.

2652. That is they think the fly cannot live on in the locality unless it has game on which to feed?—That is their impression.

2653. Do you know of any other diet the fly has besides the game and animals?—No; I suppose it will bite anything, it will bite man or anything which has got a skin sufficiently thin to be pierced.

2654. If it has not some animal upon which to live, there is no other diet you know of to keep it?—No, I think it depends on blood somehow.

2655. Then would you expect that the fly would disappear if the game could be driven off?—Yes, I should from what I have heard.

2656. Have you formed any estimate as to what the cost of fencing would be, or do you know of any practical fence which would be game-proof?—No, I could not suggest that at all. It would be a big job to fence in an area sufficient, and still more difficult to keep it entire without its being broken through.

2657. Do you know of the existence of any fence in any part of South Africa which has been either used for the obstruction of game or which could be so used?—I think at the time of the rinderpest they used fences to keep the infected cattle from passing through, but that is rather a different thing. That was more in the civilised or advanced country.

2658. It has been suggested that a fence might be made of the trunks of trees and debris and what not, which might keep game out or in. Do you think that is practicable?—You would require such an enormous amount. What sort of area would you propose to fence in?

2659. One area suggested was 25 miles each way.—I think that would be practically impossible; I do not know where you would get the timber in Africa.

2660. The fence suggested was a wire fence with iron standards.—I have known a rhinoceros upset an engine on the railway, so I think a rhinoceros would very soon get through a wire fence.

2661. Anyway, is it your opinion that a sufficient number of experiments have occurred in nature to demonstrate what we want to know?—I think so.

2662. (Chairman.) I do not think you yourself have examined flies to see what they feed on?—No. I made certain collections and brought them to the British Museum, but I am not an entomologist and I never went into details.

2663. You cannot say whether they feed mainly on mammalian blood or on reptilian or avian blood?—No, I never followed that up at all.

The witness withdrew.

TENTH DAY.

Friday, 21st November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Dr. A. G. BAGSHAW.
 Dr. ANDREW BALFOUR, C.M.G.
 Sir JOHN ROSE BRADFORD, K.C.M.G.
 Mr. E. NORTH BUXTON.
 Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
 Dr. W. A. CHAPPLE, M.P.
 Colonel Sir W. B. LEISHMAN, F.R.S.

Dr. C. J. MARTIN, F.R.S.
 Mr. J. DUNCAN MILLAR, M.P.
 Dr. P. CHALMERS MITCHELL, F.R.S.
 Mr. H. J. READ, C.M.G.
 The Hon. L. WALTER ROTHSCHILD, F.R.S.
 Sir STEWART STOCKMAN.
 Mr. A. C. C. PARKINSON (*Secretary*).

Sir ALFRED SHARPE, K.C.M.G., C.B., examined.

2664. (*Chairman*.) You have been good enough to send us a very full statement of your views on certain of the subjects which we are inquiring into. You, of course, have had a very large experience in Nyasaland and other parts of Africa covering many years?—I was out there, I think, for 22 years altogether.

2665. You know a good deal about game, and no doubt have come a good deal across fly in the course of your travels?—Yes.

2666. I do not remember when first the trypanosome was identified and found to be the germ which conveyed sleeping sickness. Do you remember when that was first established—about 1903, I am told?—Yes.

2667. Before that had you been in the habit of shooting in various parts of Africa?—Yes.

2668. In places where there were both fly and game?—Yes.

2669. You went, I suppose, with a good many carriers?—Yes.

2670. Were you and they constantly bitten by *morsitans*?—Yes, constantly.

2671. Knowing, as you probably do, more or less the symptoms of sleeping sickness, did any or many of your men suffer from any disease that resembled it at that time?—No, I cannot recall any case at all. I have often tried to look back and think whether I could remember anything which would be likely to be sleeping sickness, but I cannot recall anything.

2672. Afterwards when you held an official position, that of the Governor of Nyasaland for some years, you no doubt saw cases of sleeping sickness?—Yes, a few only.

2673. As a matter of fact, I believe I am right in saying that as far as was ascertained, at any rate, there were not many cases at any time?—I believe that up to the present date, up to the most recent date, the total number of cases discovered is still under 150.

2674. In *morsitans* areas in that country men engaged in their ordinary avocations were subject to the attacks of fly constantly, were they not?—Yes, constantly.

2675. But, at the same time, while men who were probably bitten very largely were not suffering to any very great extent, it was impossible, was it not, in a *morsitans* country to keep domestic stock cattle?—There are villages, especially up in North-Eastern Rhodesia, where natives used to keep a very small amount of stock in fly country, but the cattle were never allowed to leave the stockades or go farther than just round the edges of the stockades. They were not allowed to go to the country where fly was.

2676. That leads me to the next question, which is a very important one; is it your experience that, when ground was cleared round villages for agriculture or anything else, where it was cleared there were no

morsitans?—Yes, but you would find *morsitans* occasionally on the edge where the cleared country joined the bush.

2677. Yes; but substantially am I right in saying that where ground was cleared it ceased to be suitable to the fly and was not occupied by the fly?—Yes.

2678. As far as your observations go, do you think fly will follow the game for any distance, or if the game move, do the fly remain where they were or go with the game?—My view, from what I have seen, is that there is certain country which you can say is fly country, but why the fly are there one cannot say; within what you might call not a fly belt but a fly area sometimes you would find fly in some parts and not in others. Again, you might pass the parts where you found fly last month and find none this month, and all within a certain area which you might call a fly area, you might or might not find fly. Outside that area you would find no fly at any time. I do not think that the fly move outside what you may call the fly area, but what keeps it in I do not know.

2679. Have you, from your own observation, known of places where there was a large quantity of fly and little or practically no large game?—Yes.

2680. Many instances?—Yes, I might say many cases.

2681. All through this I have referred to *morsitans*.—*Morsitans* is all I know about. I hardly know anything about *palpalis*.

2682. So I understood. You know that there has been an experiment suggested as to excluding or driving out game from a limited area for the purpose of seeing whether the fly will disappear with the game, and, if it remains, whether it loses its infectivity. Now, do you think in the first place, speaking quite broadly, that you could effectively carry out such an experiment? I mean do you think that you could effectively exclude and keep out of the area the wild game—the large wild mammals?—It depends altogether on what you mean by wild game.

2683. I was going to ask about that afterwards. Tell me first about the larger wild game.—From what I have seen and heard of Dr. Warrington Yorke's views, he speaks rather vaguely of big game, and I do not know that it is at all settled yet whether he has any real basis to go upon as to what beasts or animals might act as reservoirs. Therefore, it is difficult to say. Supposing it was only one or two species of antelope, I think you might possibly succeed in exterminating them out of a given area; but if the lists were to include such beasts as bush-buck, wart-hog, bush-pig and lions, then I do not see how it could possibly be done, unless it was in a very very limited area, so limited as not to be useful as an experiment.

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2684. Would it be practicable in any case, even assuming you could go to that extent, to exterminate or exclude the smaller antelopes and small mammals?—I do not think so.

2685. You do not think it would be possible to do it?—I do not think it would be possible.

2686. And if those animals remained can you form a judgment from anything you have observed as to whether the fly would feed equally readily, I would say, on the smaller mammalia?—Well, I have seen the fly feeding on almost any kind of beast which I have killed in fly country. You see it actually on the beasts when they are dead. But I cannot say from my own experience whether they feed on birds and monkeys and—

2687. Bush-pig?—I cannot say; I cannot recollect from my own knowledge.

2688. The small antelope?—I have seen them feeding on almost every kind of antelope I have killed from time to time.

2689. It has been suggested that there would be a difficulty, because many of the smaller mammals are nocturnal in their habits; but would it be the case that an animal that was nocturnal but did not live underground would afford as good an opportunity of feeding as an animal that was diurnal, if it lay in the bush above ground?—Yes. It seems to me that the difficulty in the matter is that we do not know what beasts act as reservoirs. If it is every animal that has blood then it appears you would have to kill out every form of life before you could say that you had tried the experiment thoroughly.

2690. Are you able to form any judgment as to the cost of fencing such an area as is suggested, if it was thought necessary?—I think you would have to include the whole of a fly area. You could not take a piece out of a fly area and say we will fence in this piece, because the fly would come in from outside. You would have infected fly coming in and fly going out and getting infected outside. You would have to include the whole area.

2691. If you could get an isolated area of about five miles, what clearing of belt outside that would you think would be sufficient to exclude the entry of other fly?—You mean to clear a belt round?

2692. Yes.—I used to think that *morsitans* was never found except just on the edge of the bush, but there is one thing which rather astonished me on my last journey during last year. We found in some big flats in Northern Rhodesia fly a mile, I should think, from the edge of the bush—very little, but just sufficient to show that it did come there sometimes. That was in absolutely open country, flat marshy ground, with no grass or anything on it.

2693. Have you any views as to the possibility of doing anything by the introduction of insect-eating birds for the destruction of the pupae or the flies themselves?—No, I cannot say that I have any views.

2694. (Dr. Chapple.) You say that you found fly a mile from the edge of the bush?—Yes. I always held the view that, as a rule, you never found them more than a few yards from the edge of the bush, but on the last journey in Northern Rhodesia in one case we found them quite a mile away.

2695. Extending into the clearing?—Not many, but we saw one or two.

2696. That mile was quite clear ground?—Absolutely clear ground.

2697. They must have come, you gather, from the bush?—Yes.

2698. They would return?—Yes, I suppose so.

2699. If you were carrying out an experiment over an area of 25 miles each way (it has been suggested that it would be necessary to have an extensive area), do you think there would have to be a strip of a mile around the whole hundred miles of fencing in order to ensure that the fly from the inside of the area could not go to the bush and *vice versa*?—I think so, certainly. I had a companion travelling with me, Mr. Mount Stuart Elphinstone, and we both noticed what I told you just now and remarked upon it, because I had never seen the fly away from bush before.

2700. It might travel more than a mile, for all you know?—I cannot say.

2701. Have you anything to tell you how far *morsitans* would travel for its food?—No, I cannot say. My experience has been that as soon as a change is established in fly country and as soon as they plant cassava and maize and so on fly disappears altogether from the cleared ground. You come out of the bush; you see the flies feeding on the backs of your carriers and by time you get near to the village you observe that they are all gone. It is very noticeable.

2702. If we were carrying out an experiment then, and had a mile of cleared space around, would you think that our experiment might be vitiated by the fact that the space was only a mile, and that our conclusions might all be wrong?—I think it would be a doubtful experiment.

2703. You would not be in favour of an experiment of that kind unless you had a larger area than a mile of cleared ground?—I think I would make some investigations first with a view to testing what distance *morsitans* is ever found away from bush. It is not like *palpalis*. The only place where I have seen the latter is near the south end of Lake Albert Edward, and the *palpalis* does not go 10 yards; it lives in the bush, and you do not find *palpalis* 10 yards from the edge of the bush. You can pitch your camp within 50 yards of the edge of the bush on the Ruchuru river; you will not have a single *palpalis*, whereas the bush would be full.

2704. You would not advise the Committee then to suggest an experiment of that kind?—I think that before it was undertaken you should have some investigations made with a distinct view of ascertaining how far *morsitans* is ever found from bush.

2705. Can you tell us some way by which we could get at those facts?—In Nyasaland, I should think, the Governor of Nyasaland might put in hand something by way of having observations taken in some of the fly areas there.

2706. Can you suggest how those operations could be carried out?—He might issue instructions to all the district officers to endeavour to go through a course of observations of some particular piece of fly country. Where it borders on stations observations might be taken daily perhaps.

2707. Would it be possible in that way to discover it, do you think?—I cannot say.

2708. Has it ever been done?—No, it has never been done.

2709. Dr. Balfour suggested, at our last meeting, that there was an area 20 miles long by two wide where fly and game were to be found at the Kowalib Hills, three or four miles away. Do you know that area?—Whereabouts is the hill? I do not know the name.

2710. (Dr. Balfour.) In Southern Kordofan.—I do not know it.

2711. (Dr. Chapple.) Do you know any area whose natural conditions will enable us to make an experiment without the cost of fencing?—In Nyasaland the fly occurs as a rule following the Shire river and Lake Nyasa. It is generally a belt possibly not more than 8 or 10 miles wide, but of great length. In the Shire Highlands near Zomba would be the best place to look for an area, I should think. It is very hard to say, but there might possibly be found there a fly area not more than 10 miles in diameter perhaps.

2712. You have not fixed on any arrangement by which an experiment could be made?—No.

2713. Do you accept the fact that domestic stock cannot be kept at all in infected *morsitans* areas?—I cannot give a definite opinion. I only know that stock is not kept by the natives in fly areas except in their villages.

2714. Do you know why?—I presume because they find that they die if they take them outside.

2715. Do you accept that as the explanation yourself?—Yes, I have always considered that the explanation.

2716. Have you made up your mind as to what remedy should be adopted to prevent, having regard to the progress of settlement, the extermination of stock in *morsitans* areas?—I cannot say that I have seen

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any solution except very extensive and close settlement of natives. As the country becomes settled and the bush cleared, the tsetse will disappear.

2717-8. Have you come to a conclusion as to why the tsetse disappears?—No, I have not. I do not know why tsetse occurs. I have not been able to form any opinion as to why it is there and why it keeps to certain country.

2719. Is the progress of settlement taking place now in Nyasaland causing the fly to disappear?—It is not sufficiently close settlement to influence the tsetse-fly question.

2720. Are there areas in Nyasaland that would in your opinion be settled if it were not for the existence of the fly?—Certainly.

2721. What would you suggest then in order to encourage settlement in fly country?—I am afraid I could not suggest anything.

2722. Would you oppose the relaxation of the game laws in those areas with a view to driving off the game preparatory to clearing operations?—I think that if it was decided to make an experiment, it would not matter what you did if the object was to get rid of the game. I think you could try to get rid of it in whatever seemed the best way.

2723. Assuming that the object is not to get rid of the game at all, but to get rid of the fly, or rather to get rid of the disease, and supposing that the fly is the carrier of the disease and game the reservoir, and the removal of the game means also removal of the fly, would you not be prepared, with the progress of settlement, to give facilities for the destruction of game within certain areas around inhabited spots?—As I say, first of all we want to know what kind of game. We can talk of "big game," but what is meant? We want to know what we have to remove. If we have to remove only a few of the big antelopes, that is one thing; if we have to remove everything which has blood then I should say that it is a total impossibility.

2724. If you knew that some of the game harboured the trypanosome and some did not, what harm would there be in driving it all off within a certain radius of inhabited spots?—If we knew definitely that certain game did harbour trypanosomes and that all the rest, the bulk practically, were not reservoirs, then I think we should be quite free to do whatever we thought right—even to slaughter the game and get rid of it in whatever way we liked over a definite area.

2725. Do you think that it would be a good experiment to try by the extermination of the game or the driving off of the game round inhabited areas, to see whether or not that did not free domestic stock from disease?—Yes.

2726. You would not object to that as an experimental procedure?—No.

2727. What do you consider is the chief value of the game to the community?—I do not think it has any economic value.

2728. Has it no other value except the value of sport?—Simply the value of beasts which we can never replace if we kill them off.

2729. Do you mean value for sporting purposes?—For zoological purposes, for general interest, and so on. If the world is any the better for having animals in it, then it would be a pity to abolish them.

2730. If domestic animals are not possible in the presence of this game, would you be prepared to sacrifice the game?—I think that if it is once definitely proved that the reservoir, and the only reservoir, is certain animals in at any rate certain parts of the country where tsetse exist, and we can get rid of those animals within the areas, then I would by all means get rid of them.

2731. Is it your experience that when animals are hunted in the vicinity of villages they get shy and timid, and keep a respectful distance from those inhabited spots?—Yes, to some extent.

2732. Did you say to the chairman that you know of no area where fly exists without game?—I know of many areas.

2733. I beg your pardon, you put the converse of what I say. You know areas where fly exist and absolutely no game?—No, I would not say absolutely

no life, but I know of areas which are what we should call hopeless game countries, where no one would go to shoot because there is nothing worth looking for, and which are swarming with tsetse-fly. On the other hand I know of country excellent from a shooting point of view where you get all sorts of antelope and other game, and where you never need trouble your head about tsetse.

2734. In the other places there is a sufficient amount of game of some sort to supply the fly with food, is there not?—I cannot say. The food may be birds, and not what we should call game; life of some kind possibly. There may be snakes, lizards, birds, possibly some monkeys. I do not know.

2735. We had evidence before us with regard to sleeping sickness in Nyasaland. Do you say that this disease has not been spreading amongst men in Nyasaland?—I believe the number of cases up to the present date is something under 150. I have just received the last Diary. I cannot remember the exact number. Those are cases that have been discovered during the last six, seven or eight years.

2736. Has it not been spreading over a new area, where it was not known to exist before?—It was never noticed before, but whether it existed or not I cannot say.

2737. Is the danger in Nyasaland an increasing one in regard to both man and animals?—I cannot say. Perhaps you would get a medical witness to tell you better.

2738. You said in answer to the chairman that it was not possible in your opinion to exterminate game?—Not to exterminate every kind of game.

2739. Is that extermination not taking place in the case of settlement all over South and Central Africa?—Yes, where you get close settlement.

2740. It is possible then in the presence of settlement to exterminate?—If you get the whole of the bush down and the whole of the undergrowth down and natives settled and cultivation going on and no more rank growth and so on, then you most undoubtedly will get rid of tsetse-fly.

2741. (Sir William Leishman.) It has been suggested to us as a possibility that elands might be domesticated. Do you think that is possible?—I think they might. We have had absolutely tame eland in Nyasaland.

2742. Has that been tried on any scale?—I do not think so.

2743. No organised attempt?—No.

2744. You make some interesting remarks in your summary about your personal observations on fly. You say, for instance, that you have examined a great many personally, and never found any blood in them. —Except where they were in the act of biting.

2745. How did you judge that—simply by the distension?—By squeezing the abdomen. There was nothing visible.

2746. It is your opinion, I gather from this, that the tsetse-fly may live on something else than blood as mosquitoes do?—I formed that opinion because in some districts where flies are plentiful, I cannot understand what they have to feed on in the shape of animals of any size like even the smallest antelopes.

2747. Have you not observed them feeding on lizards?—No, I cannot say that I have ever seen them.

2748. But you know that they do?—One may have seen it, but I cannot remember ever having done so.

2749. Have any of your personal observations led you to think that they drink water?—I have never seen them drink water.

2750. Or feed on vegetable matter?—I have seen them on leaves many times. I have seen them on the ground getting up from the path as you walk along.

2751. But you have observed nothing that would make you certain with regard to that?—No, but at the same time I have not made any careful observations regarding this.

2752. With regard to flies caught in the act of biting either yourself or others, is the bite of the fly painful?—Yes, but not very.

2753. We have been told that you may see the back of a native, for instance, covered by a number of flies?—Yes. The native skin is much harder; I have

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often seen a native ahead of me with one or two flies on his back and wondered how soon he would know they were there. They stop a long time. Then he swings his hand back mechanically when he begins to feel them.

2754. (Sir Mackenzie Chalmers.) We were told that usually the flies waited until you stopped before they began to bite. Is that so?—I cannot say I have found that. I have had my shoulders badly bitten through my shirt. It is like a flea bite; afterwards you want to scratch it. I have had my shoulders raw sometimes.

2755. It is a bite producing local irritation?—Yes.

2756. You mentioned just now that you had seen the latest return of cases of sleeping sickness in Nyasaland, and that they were under 150 in number?—I think it is a little under 150.

2757. Do you happen to know whether they are all of the very severe type, or whether there are any people alive who were infected some time ago?—I cannot say, but I know there were recoveries. The very first case we ever discovered in Nyasaland recovered. When I left the man was alive. I am speaking of three or four years ago.

2758. A native?—A native. He had been working with Lieutenant Pooley, who was travelling through the country.

2759. Do you know whether he was infected in Nyasaland or had come from some *gambian* district?—I think he was infected in the Congo. I cannot say that I know of any case myself which has recovered where the disease has been actually acquired in the country.

2760. You do not know how many of these 150 who have been infected are still alive?—No.

2761. You have considered the question carefully of course, but are you satisfied yourself that the antelope and big game contain trypanosomes which are pathogenic to man?—I only know what Drs. Yorke and Kinghorn have said, and what struck me about what they said was that they had not gone nearly far enough into ascertaining what the different kinds of animals which can or do harbour trypanosomes are. They gave a list of animals of which they had found a certain percentage infected, but with regard to many of the other animals they had only tested two or three.

2762. Are you satisfied that where they have found the *Trypanosoma brucei* or *rhodesiense* the disease has been conveyed by means of fly from game to man?—I cannot say; I only know what they say. I have not sufficient medical knowledge.

2763. You have not formed any opinion yourself?—No, I have no scientific knowledge on those matters.

2764. Is it possible, do you think, having regard to the small number of cases in Nyasaland, they are cases of infection from man to man?—I have no means of judging that.

2765. You told us that in fly areas, when a village is established and a clearing made round it, you can keep cattle in safety?—As long as they are not close to the bush.

2766. In the centre of the village you can keep them safely?—Yes.

2767. Now what is a sufficient clearing to give a margin of safety?—I cannot say.

2768. In practice?—There was a very large village, Nsama, up in North Eastern Rhodesia, where they had perhaps about a dozen cattle the first time I went there. What you might call the tsetse-fly bush there came within, I should say, perhaps a mile.

2769. The cattle there were living and prospering?—Yes.

2770. There was a radius of a mile clearing all round?—Perhaps a mile, with gardens in between.

2771. Is the clearing done by cutting down trees or by burning?—Both. They fell the bush in the middle of the dry season and leave it till a little before the wet season to dry. Then they chop it up and burn it and leave the ashes to enrich the soil.

2772. Is it a very long and expensive process or not?—I do not know what the planters reckon it costs them to clear in Nyasaland, but roughly, perhaps, about 2l. an acre I should think.

2773. Do you think that a good deal might be done to protect people by impressing on them the importance of clearing round their habitations?—I do not think so, because the planters have not money to spend in anything except their own planting.

2774. I was thinking of native villages. Do they naturally clear round the native villages or do they leave a fly area close to the village?—They plant their crops all round the villages so that there is a natural clearing.

2775. I should have thought they would often plant crops on one side of the village and not trouble to clear on the other?—Sometimes bush may come up close to a village. Nyasaland is under such very excellent control now that the natives would do very much what you told them to do.

2776. Do you think further protection could be afforded by giving instructions about clearing and by encouraging clearing?—I daresay, if it was made a Government instruction and they were told to do it.

2777. If the officials were told to encourage clearing and explained to the people the advantage of it?—Yes.

2778. Where you have a clearing round a village and crops planted do the game return at night?—Sometimes they come and eat the crops.

2779. They ravage the crops?—Yes.

2780. It is not enough simply to make a clearing and drive the game out, but you have to do something in the nature of fencing?—Yes. In country where there is game the natives always have to put up some kind of protection if they are right up in the bush. They generally put fences made of logs.

2781. Do you know the Domira Road?—Yes.

2782. That was suggested for an experiment. I would like to get your opinion on the suggested experiment with various modifications that have been submitted to us. The Domira Road, we were told, will some day be a very important road?—It may be.

2783. At present you cannot take stock through it at all?—You cannot.

2784. It runs for about 20 miles through fly area?—Yes, I should think about that.

2785. It has been suggested that on each side of the Domira Road you should clear out the game for five miles.—And clear the ground?

2786. No, leave the ground untouched for the sake of the experiment, but clear the game out. Then put up a fence at the outside border. The suggestion was to have a stake fence which would be an inexpensive one. Do you think a stake fence would be effective?—First, of all, you have to clear the game out without clearing the ground.

2787. Yes, without clearing the ground—leaving the bush?—What game?

2788. As far as you can do it, everything; but, at any rate, the big game, to see the effect of getting rid of what are found to be now the main carriers?—Do you ask whether it would be possible to do that?

2789. I ask whether it would be possible to do it, and whether you could keep the area free from game by a stake fence, not a steel wire fence?—I do not think it would be possible to clear every kind of beast out (in fact, I am sure it would be an utter impossibility) up to five miles, and then put a fence.

2790. You could clear out the big game, the eland, and the wart-hog and so on?—I do not know that you could even do that if you left the forest.

2791. It is more or less open scrub, is it not?—It is open forest. It is a mixture.

2792. You could patrol that?—If you could, at any one moment, having pushed the game out, put the fence down and close it up, one might say yes, but all these things take weeks and months to do.

2793. You would have to leave gaps in the fence to drive the game through?—If you went one night without having every yard of it guarded, a beast or two might come in and the whole experiment would be worthless.

2794. You would patrol it and find those beasts probably. You would have a small hunting party, a continual patrol. That was the idea, I think.—My idea is that you cannot do it. If it were merely a

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matter of clearing the ground and then clearing every kind of small beast off it would have a better chance.

2795. You want to see whether, if game has been cleared out, the fly will disappear or become non-infective, so that you could introduce fresh animals, and there would no longer be a source of danger.—Even if you succeeded in the experiment, the fly would have access to the country outside the fence.

2796. The witness suggested, as regards that, a clearing of half a mile to a mile outside the fence. You would want to keep the natural country in the five mile area, but you would clear a mile outside?—I should think that that might be the most likely way of trying any experiment.

2797. But you very much doubt what the result of the experiment would be?—I very much doubt what the result would be.

2798. Or whether it would be worth while?—I very much doubt it.

2799. Can you give us any opinion as to the cost of the operation?—No, but I do not think the fencing would be specially costly.

2800. Two kinds of fencing have been suggested. We were told that a steel fence in the case of a railroad came to about 140*l.* a mile, but Sir David Bruce suggested that if you relied on a stake fence, you could use native labour and do it without much expenditure of money, because a lot of people who cannot pay their hut tax might work it out?—It could be done, I think, without heavy expenditure. It would have to be a wooden fence.

(Chairman.) He suggested a fence of live wood.

(Sir Mackenzie Chalmers.) He said that if you put in stakes in that country, after a short time they grow.

(Witness.) Yes; some of them do. That is not reliable, because some grow and some miss, and those which miss get rotten, and if there happened to be two together that would make room for the animals to get through.

2801. You would have to watch and renew?—Yes.

2802. You would always have to have a party going up and down?—Yes.

2803. That would not be a great expense, because you would use native labour with one or two Europeans?—I cannot say without thinking carefully what the cost would be, but I do not think it would be a great expense.

2804. He gave an estimate of about 1,500*l.*—What length?

2805. Twenty miles, and he proposed to have an area of five miles each side cleared of game.—You would not do it for 1,500*l.* You might do it for 10,000*l.* It is 40 square miles, and you could not put it at less than 1*l.* an acre for clearing the bush. That would be the outside limit at very low wages. It would cost nearly 30,000*l.* to clear the bush alone.

2806. What Sir David Bruce says in his corrected memorandum is this: "The next scheme I would choose" (that is if he cannot have the steel fence), "supposing the first was impracticable on account of its cost, would be to enclose the Domira Bay Road by a post and rail fence. This would be 20 miles long by 10 miles broad. The cost at 15*l.* a mile would be 750*l.*, clearing the tract 500*l.*, and upkeep 225*l.* equals 1,475*l.*"—Doing no bush-clearing.

2807. I do not know what he means by clearing the tract. Perhaps it is clearing outside the line of fence.—You would simply have a fence on each side, and have no clearing. It would mean 40 square miles of bush-clearing, and even at 1*l.* an acre that would be between 20,000*l.* and 30,000*l.*

2808. He puts it at 500*l.*?—He has not reckoned the cost of clearing the land of bush.

2809. So that it would be more expensive than the original experiment?—Yes.

2810. (Mr. Rothschild.) Sir Alfred, in answer to the chairman and Dr. Chapple, you said that with very close settlement the fly disappeared by itself. Now in your statement about the cost of clearing a mile outside the fence of the proposed experimental area, you show the great cost of clearing.

Do you not think that, independently of the question of game, the one practical way of getting rid of fly is by clearing away the bush?—That is the only way I know.

2811. And you specially state that you know bush areas containing fly which do not contain game? What I mean by game is antelope, pig, elephant, rhinoceros or eland.—I would not say that I know any area which does not contain some life in it. I do not know any area which does not contain some kind of small game.

2812. Small game?—I would not say that there is any area except what is occupied by natives and cultivated, where you could find absolutely nothing, not even some of the very smallest kinds of life, like the African hare.

2813. That I understand, but as far as we have been told, the animals which are to be regarded as reservoirs are the antelopes, small and big, the pigs, that is to say, wart-hog, bush-pig, and the river-hogs, and elephant, and rhinoceros, and zebra. Those are the only animals referred to in talking about clearing out the great game, as far as we have been told in evidence; this term has not been used to include hares, or rabbits, or foxes, or monkeys, or things of that sort.—They have not done anything to prove that those smaller things do not act as reservoirs.

2814. What they say is that they are largely nocturnal, or else that they are so quickly killed off by the disease that they are negligible as reservoirs.—I should not say that that was so.

2815. I want to know whether in those areas, which you say are not favourable game areas, the game that is present consists of the bigger animals. You have found fly in an area that might contain small animals like hares or monkeys, but not the big animals?—I have found frequently areas containing only very few small animals and no large animals, but plenty of fly.

2816. Do you think that clearing the bush in those areas would destroy the fly or drive it away?—Yes. Anywhere I think it would do that.

2817. (Dr. Martin.) I understood you to say that for the purposes of this experiment it would be necessary to have a large area?—No, I did not say large area. I said it would be necessary for you to have a complete fly area.

2818. That would be rather large?—It would generally be very large.

2819. Would it be useless to do it in two or three square miles?—You must isolate your area in such a way that fly does not come in and game does not come in, must you not?

2820. Certainly.—You might choose quite a small area and go to a very heavy expense in making sure by absolutely clearing the ground and keeping it clear. That is the only way I can see. If you limit your area it must be big enough for game to live in it, must it not?

2821. The game are to be kept out.—Quite so, but it must be sufficiently big to be able to make observations in, and to give some definite proof.

2822. There is nothing you know about the life history of *morsitans* which would lead you to suppose that a small area would invalidate that experiment?—No.

2823. You have not anything in view?—No, and I have no scientific knowledge of the life history of *morsitans*.

2824. (Mr. Millar.) I should like to ask you a question about the areas where you have told us there is practically no big game at all. Are these extensive areas in Nyasaland?—Yes, sometimes.

2825. Are they well defined?—Yes, fairly well defined.

2826. Could you give us an indication of any of the particular districts you have in mind in Nyasaland?—Yes.

2827. Such as?—Such a district, for instance, as the district on the Lirangwe river. It is rather a noticeable case. That is why I give it.

2828. Is it an extensive district? What area does it cover?—I only know the part of it which you cross

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going by road from Blantyre to Matope; all along the Lirangwe river you get tsetse-fly.

2829. What would that cover altogether?—It is a long drawn out line.

2830. A belt?—It runs near to the Lirangwe river. You might call it a belt in that case.

2831. Near to the river itself?—For the remaining portion of the road down to Matope you are liable to get fly anywhere. We used to say you did not get it until you got to the river.

2832. Are there districts similar to that?—Yes. There is no big game there.

2833. There may be small animals?—Yes, I have seen monkeys and baboons.

2834. Can you give me other areas?—Yes, on the eastern side of Lake Pamalombe, where I never troubled to go because there is nothing to shoot there, but it is full of fly.

2835. When you say there is nothing to shoot there have you formed any opinion as to whether game did exist there at one time, and has been shot out, or whether it is a gameless country in the sense that big game have never been found there?—No. The Yaos live all round there and they are always moving their villages. Patches have been cleared. It may have held game formerly.

2836. Is it your opinion that game has been cleared out of the country?—I cannot say. There never has been game since I have been in the country.

2837. The natives constantly move about and clear the district?—Yes, all round the country they move.

2838. Which might account for the game disappearing?—It might.

2839. Can you tell us whether there is any evidence as to the infectivity of the fly in the particular district or in any of the districts you have referred to with regard to sleeping sickness?—I do not quite understand.

2840. Can you tell us whether there have been many cases, or any cases, of sleeping sickness occurring in those gameless districts?—I notice in the last Sleeping Sickness Diary that a certain proportion of cases (I cannot say how many) occurred in the South Nyasa district, and that is the district of a portion of which I am speaking.

2841. You do not know how many cases?—No.

2842. To your mind, is there anything to account for the presence of fly in these districts in connection with foliage or otherwise, or the character of the country?—I have often tried to form some opinion as to what kind of country the fly lives in, but I have never been able to specify anything definite.

2843. As far as I gather from you to-day, you rather indicated that wherever there was bush or shelter or shade, the fly was found?—Not always. I think that elevation certainly has a great deal to do with it in Nyasaland. For instance, in Nyasaland itself I have never seen fly above 3,000 feet or at most 3,400. I do not know of any case.

2844. In these districts, where the big game is not present, is there, in your opinion, a sufficient number of small mammals to sustain the fly, assuming that the fly is feeding upon mammalian blood?—I should say not, especially in the district east of Pamalombe, which I spoke about, where you come into patches of fly so thick that you have no peace. You have to be swishing yourself all the time with leaves. All the boys do the same, and that is a country where you do not see any living beast at all.

2845. Do you know whether the fly subsists on vegetarian or other food? Have you formed any theory at all with regard to that?—No, I have not. I cannot say.

2846. With regard to the other game country you refer to along the Rukuru river, where you say game is abundant, there is no tsetse-fly at all?—No. I gave that as an instance. I have heard that there is fly down at the mouth of the river where it runs into Lake Nyasa. Dr. Laws, the missionary there, told me that he had seen tsetse there, but that is not the part I am speaking of. I am speaking of the upper valley of the Rukuru.

2847. In your opinion is there anything to account for the absence of fly? Is the elevation or the character of the country sufficient to account for it?—No. I do not know why it is not there.

2848. Is it high country?—Not very high. It is from 2,500 up to about 3,200 feet.

2849. What is the nature of the foliage and of the bush?—The ordinary kind of African Nyasaland bush. It is scattered bush with grass in between, fairly rank grass, much the same as elsewhere.

2850. Is there everything to encourage the fly which exists in the districts where the fly is to be found?—Apparently it is very much the same. I have never noticed any difference.

2851. With regard to game preservation in Nyasaland, perhaps you can speak of the present moment as well. How many game reserves are there in Nyasaland?—Originally there were three. There was the Elephant marsh reserve, the Chilwa reserve, and the Angoniland reserve.

2852. One of these has been abolished since?—The Elephant marsh reserve was abolished. First of all it was curtailed. The real fact was that people wanted to shoot the buffalo, but of course tsetse-fly was used as an argument. I cut out of it everything but the actual open marsh known as the Elephant marsh, and left simply that in. In the marsh itself there are no tsetse-fly—only when you get away from the marsh into the foot hills. That has since been cut out too, I believe.

2853. Is there a dense population round about these reserves?—No, not dense.

2854. On the outskirts?—Very few.

2855. In every case?—Only down at Chiromo, there is a big settlement there.

2856. Has the result of the enforcement of the game laws in Nyasaland been to lead to the increase of game?—Yes, certainly, game has not diminished except round settlements, and within a reasonable distance of settlements.

2857. Is there a marked increase in any district?—Not a marked increase.

2858. Round about the reserves?—In the Angoniland reserve there was certainly a marked increase of elephants. I would not speak about other game. That is because they had come over from Portuguese territory.

2859. Do you see any objection to allowing natives in districts where the game is invading the clearings and villages to have greater freedom in extermination?—No, I do not. I always told all chiefs of villages that if any game came and troubled them in connection with their plantings or anything, they could do what they liked.

2860. With firearms?—Yes, if they had firearms. A native who has a gun has to pay a licence.

2861. Would you carry that policy further and allow them to remove or drive back the game from any districts which were infested with fly round about settlements?—I do not think that a native would ever trouble to do it unless he gained some benefit.

2862. Would he not be satisfied with what he got?—You mean give them free leave to shoot game anywhere near the village?

2863. Yes, and drive back the game round about the settlements?—That would have the effect, I think, of driving the big animals back in course of time.

2864. Where the country is completely cleared, and where, as a result of the clearing all the game, both big and small, is destroyed, the fly, I think you said, is not to be found at all?—The game would no longer have any cover, but my own view has always been that the disappearance of the fly from cleared ground is not on account of the absence of beasts, but because fly requires shade. If instead of game cattle are put on, though the cattle are there the fly does not come as long as the ground is clear.

2865. You would not object to an experiment being made to exterminate the game in certain districts, would you, in order to determine the relation between the fly and the game?—No, provided that you are sure before you authorise an experiment that you know sufficient about game acting as a reservoir and what

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game do act as a reservoir. It would be useless to exterminate all the game that you could get at (the bigger antelopes) over a certain area and then find afterwards that a number of other animals—smaller animals—acted as reservoirs also.

2866. But are you not prepared to admit that in a fly area which is inhabited by big game, the big game do act as a reservoir of the trypanosome?—I accept that because we are told by men who know what they are talking about.

2867. (*Sir Stewart Stockman.*) I think you said that it would be very difficult to get rid of such things as lions and leopards?—Yes.

2868. If it were possible to drive out the zebra and the antelope, would not the lion follow? Would he not go away of his own accord?—It is not only a question of lions. There are many beasts in Africa which are extremely harmful to natives. Perhaps the most harmful beast the native has to deal with is the bush-pig. They live in holes among the rocks and in many inaccessible places, and they come down not only at night but in the day time, whenever the plantation is unguarded and they destroy the crops. The natives hate bush-pig and set traps for them, but they remain very numerous. If it had been possible for the native to destroy the bush-pig he would have done it long ago.

2869. Is the wart-hog destructive to agriculture?—Not like the bush-pig. Bush-pig come into plantations when wart-hogs will not. I do not know why.

2870. An attempt has been recently made to deal with wart-hogs and bush-pigs in Zanzibar by setting free a very virulent form of swine fever amongst them. Do you know anything about that?—No, I do not.

2871-2. With regard to the eland, we might almost assume that he can be domesticated, I think. Do you think that he can do a good day's work if he is domesticated, and plough like an ox?—I cannot say. I really have not enough to go upon. I have seen in the Egyptian tombs the carvings of not only eland but koodoo and other antelopes being milked. You see it in the Sakara tombs.

2873. It is one thing to domesticate them and another thing to say that they are good for the plough?—Yes.

2874. (*Dr. Balfour.*) Supposing that the ink-pot here was a small experimental area each side, a matter of five miles long, and the blotting paper the surrounding clear zone, and this [the table] was the bush country outside, the danger would be of fly coming from this zone into the experimental area?—Yes.

2875. Do you think that the fly could be checked by affording facilities for feeding along the margin by means of the blood tubes that Rodhain used, or by placing tethered animals at certain points?—You are assuming that the fly comes in pursuit of blood?

2876. Yes, supposing that blood is the natural food of the fly.—It is hard to say. It would be better, I should think, to do without the tethered animals, because they might draw the flies in. If the area was wide enough it might be different, but what is wide enough is hard to say.

2877. The wider it is the more expensive it is?—Yes.

2878. You cannot say whether that would be a good method of preventing the ingress of flies?—No.

2879. If you drove animals out of the area those turned out would tend to return to their old haunts, as game always does?—Yes.

2880. Some might also come in from outside?—Yes.

2881. Game usually travels along definite tracks?—You would have to have the fence on the outside of the clearing.

2882. Take an unfenced area; as a rule game travels along certain definite tracks?—To some extent.

2883. Would it not be possible by a liberal distribution of calthrops, as they are called, to prevent animals from getting into the central area? I refer to spikes of iron embedded in a lump of lead. They are very easily made. Gordon used them to check the charge of the Dervish horses in the Soudan. You would lame some of the animals, and as they dislike treacherous ground they would avoid the "doctored" areas, while you could get at any which might be

injured more readily?—Perhaps if you covered the whole of the ground you might.

2884. Suppose you specially concentrated on the game tracks?—That would not do, I think.

2885. It would have to be a large belt, you think?—Yes.

2886. Supposing you had a mixture of small pits and these calthrops. Would that be sufficient?—You might do something.

2887. You think that it would not be a radical way of dealing with the difficulty?—I do not think that it would be certain at all.

2888. It is outside the range of practical politics, you think? I take it that any experiment carried out would have to be carried out in both the wet and the dry season?—Yes.

2889. The conditions vary so greatly that that would be an essential of the experiment, would it not?—Yes.

2890. You mention much fly and little game in some places. Is that not possibly explained by the fact that the game has died off owing to infection by the fly?—I do not think so, because game do not seem to suffer at all.

2891. Not in those special areas?—No.

2892. There was no evidence of it at all?—No.

2893. (*Mr. Buxton.*) From the answer you have just given I suppose you are confident that the game itself is immune?—I should certainly say so.

2894. Even if it contains pathogenic germs it is itself immune from the disease, you think?—Yes, apparently. That is the conclusion I have come to.

2895. Do you think it is possible that human beings may have acquired certain immunity, or that some have acquired immunity?—Yes, I have thought so. Perhaps it is not much use my speaking on medical matters because I have no medical knowledge; but speaking from a general point of view, the opinion I have formed is that, as has been stated by some other people, Nyasaland sleeping sickness is not the sleeping sickness known in Uganda at all, but is a disease which has been there from time immemorial, and I think that the population is immune more or less.

2896. Would that satisfactorily account to your mind for the fact, which you have told us, that a comparatively small number of cases has been discovered over a considerable number of years?—Yes, it would.

2897. How would that affect this suggested experiment? Assuming it to be decided to have an experiment of the kind described, would you eliminate men as well as animals?—I think that before we can try the experiment, we want to know exactly what can act as a reservoir and whatever can act as a reservoir must be excluded.

2898. Assuming it to be decided, and assuming the clearance of game as far as it is possible to have taken place, I want to know whether you would eliminate men also from the inside of the experimental area?—Certainly I would if they can act as a reservoir, which I suppose they can.

2899. You would hold that they could be a reservoir, or a possible reservoir, if they remained?—If they act as a reservoir, which I suppose they must, then I think the experiment is not complete unless you take all the natives out of that area.

2900. Then under those circumstances would you not rather spoil your experiment, because you would not reproduce the condition of things which would obtain if there were a general destruction of the game?—I see what you mean—that you must leave the natives there because they will be there in future.

2901. Exactly, and would be a reservoir, or I am assuming that they would be a reservoir?—I follow.

2902. Do you know an investigator who has seriously sought to distribute the culpability (assuming they are culpable) of big game, and to distribute the responsibility among different species?—All I know is what I have heard and read of Dr. Yorke's experiments.

2903. Was not the number of each species investigated very small?—Yes; it appears to me to be quite absurdly small in some cases, and incomplete.

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[Continued.]

2904. Do you think that before game is generally condemned, we ought to know something about the relative culpability of the species?—Yes, I think we ought to know very much more than we know now definitely.

2905. (*Dr. Bagshawe.*) Supposing that Sir David Bruce's fencing scheme round the Domira Road were adopted, and it was decided to make a wide clearance of vegetation outside the fence on each side, would it not be possible to get a good deal of work done by hut-tax labour?—There would not be any trouble about getting it done, but you would have to pay for it.

2906. It could not be done without the passage of cash from the Government to the natives?—No. If you take it instead of hut tax it means you diminish the revenue coming from hut taxes. It is 50,000*l.* a year revenue from hut taxes for the whole Protectorate.

2907. That is a considerable asset to the Protectorate, I suppose?—It is nearly half the revenue.

2908. (*Sir John Bradford.*) I understood you to say that your view was that this disease in man had been in Nyasaland for a long time?—I have no medical knowledge, and therefore anything I say really has no weight with regard to that.

2909. Is there any tradition amongst the natives to that effect?—The natives all say that they know it of old.

2910. Has it an old native name?—No, I do not think it has, not that I know of. Possibly the doctors in that country may have found a name for it, but I do not know.

2911. Are the symptoms at all similar to those of any other common disease in that country?—I cannot tell you.

2912. You are quite satisfied that it is not new to the natives?—The natives have said over and over again that they know the disease of old, but native evidence in matters of that kind is not worth much.

2913. You have described some areas where fly is abundant and where game is relatively absent?—Yes.

2914. Can you tell us whether in those districts cattle are maintained and live?—No, there were no cattle in either of those districts I referred to.

2915. There are villages, I suppose?—Yes, villages but no cattle. The Yaos keep very little cattle.

2916. Why is there no cattle, what is the reason?—I cannot tell you except that they are not a cattle-keeping people. On the other hand it may be simply because of tsetse-fly.

2917. Is there reason to suppose that cattle die if they are introduced?—I cannot say because there have never been cattle there. On the west side of the Shire river near the south end of Lake Nyasa, where you get the Angoni people living lower down than usual, they had no cattle in the fly country when I first went there. Later on some of the villagers began to get cattle (when all the wars had finished) and they lost a great many cattle through tsetse-fly. That is not a game country either.

2918. I want to know if there is evidence that in a country relatively game free, and where there is abundance of fly, cattle can survive?—I know of no instance.

2919. You do not know any instance of any such country, where cattle have been known to die of tsetse-fly disease?—Yes, I do.

2920. I mean a game-free country?—Well, round about Fort Johnston at the south end of Lake Nyasa, there is a very large Yao town where they recently lost a good many cattle from fly, and some of the herds belonging to the station have been affected. They lost a great many cattle about four years ago. I told the magistrate there that it was his fault because he had let them graze too far away from the station. Where the present station is was formerly Mponda's old town. The whole country was cleared and he was turned out of the town. We had to fight him in 1891 or 1892 and finally an English Government station was built on the site of his old town, the surrounding country being still more or less old gardens. The bush has been gradually growing up in the old gardens, Mponda having settled in a different place altogether. I have thought that the cause of tsetse-fly appearing

again round about our present station is that the bush has gradually grown sufficiently to give shade to the fly.

2921. The point I want to get at is whether in these districts which you have described, where there is little or no game and an abundance of fly, there is evidence of cattle disease caused by tsetse-fly?—I cannot say that I know of any instance except in the above described case.

2922. (*Mr. Read.*) In a dispatch of the 13th July 1912 published in a Blue Book, Sir William Manning says that in October 1908 the first local case of human trypanosomiasis was reported. I have the latest Sleeping Sickness Diary. Down to the end of August the total number of cases to date is 153. In five years there have been 153 cases, an average of 30 cases a year in a population of a million, I suppose?—A little over.

2923. One would not call that serious mortality compared with the mortality from any other diseases in the Protectorate?—No.

2924. To what extent is the raising of stock an important industry in the Protectorate? As I understand, the population is chiefly agricultural. They devote most of their time to producing cotton, tobacco, maize, and that kind of thing. Is there any stock-raising industry there as an industry? One understands that stock is used for transport purposes and for food purposes, and I will come to that later, but is there any stock-raising industry there?—Yes, there is, but it is only among the Angoni. They are the bastard or mixed Zulu tribes who live in the high country—Angoniland—there is no fly there at all.

2925. From the point of view of the stock-raising industry, the fly is not a serious question?—From the point of view of native stock raising it is not.

2926. With regard to transport, as you are aware, we are doing a great deal to improve transport facilities there, extending railways, making metalled roads suitable for motor wagons, and apart from that, Nyasaland is a country in which transport is probably more easy than it is in most of these wild countries in Africa. It is a long narrow strip of country. You have a spinal railway, so to speak, and short connecting roads. To what extent then, from a transport point of view, is fly a serious question in Nyasaland?—There is a certain amount of oxen transport.

2927. Is there much of it?—Not very much of it yet, but there is a certain amount.

2928. You think that within probably a reasonable time the importance will diminish?—Yes, the importance will diminish, but at the same time I think that the fact of fly being there is certainly a great disadvantage, because it prohibits the use of oxen for transport.

2929. I quite see that.—Local transport, I mean.

2930. We have tried motor transport in other countries, in Uganda, for instance, with great success.—Yes, for long lines of transport, but I was thinking more of farmers and settlers and planters on their own places for short distances.

2931. From the point of view of food, to what extent do the natives use the milk of cows and to what extent do they eat meat?—Very little.

2932. Reference has been made to the question of relaxing the Game Laws. Under section 30 of the Game Ordinance very wide powers are given to the Residents to allow the natives to kill game and an experiment was actually carried out for 12 months in the Ngara sub-district of the Marimba district. The Governor says: "A careful record has been kept by the Resident in charge of the district as to the avidity evidenced by the natives to avail themselves of the opportunity offered, but it appears from the reports submitted each month that natives exhibit but little enthusiasm, and during the 12 months over which the experiment has extended only 48 head of game have been killed by natives." So that the difficulty rather lies with the natives than with the Game Laws?—Yes.

2933. I see again here, later on, that the Governor says: "Even were there among Nyasaland natives a much larger proportion than there are of expert

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"shots, the dense cover characteristic of this country would afford numbers of bush-haunting antelope a secure refuge for many years from a larger number of hunters than the local population of any district could produce. In proof of this I need only instance the fact that game is by no means extinct in the immediate vicinity of Blantyre, where for more than 20 years there has been a large European settlement, a number of armed sportsmen, black as well as white, and a quite abnormally dense native population." I understand that most of the cases of sleeping sickness at present come from a very limited district called the Dowa district?—Yes, mostly. I think they have had some cases outside.

2934. Yes, certainly, there have been cases outside, but I am talking of the greater number. Is game abundant in this district?—No. When you get from Lake Nyasa to Dowa, you go first of all through a bad tsetse-fly district. That is the Domira Road. Then you get out of that to the foothills up to about 2,500 feet and you lose the tsetse-fly and you get into the Upper Dowa district. From there there is no fly. Where it comes from is the piece of country which goes down to the lake.

2935. Most of the cases occur then in countries where there is very little game?—I would not go so far as to say that.

2936. You said that there was not much game there?—There is none in the upper part of the Dowa district, but in the lower part there is game. It is not a very great game district, but there is game there.

2937. Is the district a big one?—I think it is about the same size as all those other districts in Nyasaland. I am not sure of the area, but it is not very large.

2938. What size would that be?—Without really being quite sure, I should say the Dowa district might be about 40 miles by 30 possibly.

2939. 1,200 square miles?—Yes, possibly, but really I am speaking without knowing.

2940. I have just read out the difficulties in the way of getting the natives to exterminate the game. Can you suggest any means of exterminating the game in the neighbourhood of these native villages?—I do not know how it would be possible. In exterminating the game, even if you could arm the whole population and encourage them to fire off their guns whenever they saw anything, there are too many, and I do not know how they could get at many of the things like bush-pigs and leopards, as there is too much refuge for them in inaccessible rocks, and places of that kind.

(Mr. Read.) I see from the last Sleeping Sickness Diary that the local authorities are doing a great deal. Thus: "impressing on the natives the necessity of avoiding being bitten by flies," and "the necessity of clearing for a distance of 300 yards or more round villages situated in close proximity to fly, of extending these clearings in many instances so as to embrace their food gardens also, and of making clearings on each side of the roads and main paths leading from one village to another, thus uniting groups of villages, and enabling the natives to journey from one to another with commensurate safety," and so on. Those are precautions which it is suggested should be taken. This Diary is being sent to the Committee, but I thought I would call your attention to it.

2941. (Chairman.) I think I am right in saying that in answer to one of my colleagues, you said that

the clearing round villages would cost about 2l. an acre?—I cannot say definitely. I do not know what it costs the planters when they clear.

2942. I do not mind about the exact sum. When you said that, you meant clearing outside and beyond that which the villagers would naturally do for the ordinary purposes of their village?—It would mean cutting down and felling.

2943. But I mean it is in addition to the clearing that would ordinarily exist at the present time round a village?—No; it would be more or less the kind of clearing which a native carries out when he cuts down bush to plant his food.

2944. That in a sense does not cost anything?—It does not cost us anything.

2945. Supposing there was a systematic endeavour (I do not mean merely encouragement) by the Government to organise clearing to a considerably larger extent than at present round every village, and within that area cultivation was carried on and such stock as the natives have was kept, and there was not a great deal of occasion for the natives to go outside, would not that be, according to our present knowledge, the most efficacious way of keeping the native and the stock from the fly, and consequently from the risk of sleeping sickness?—Yes, I think perhaps it would be, but natives travel a great deal.

2946. (Chairman.) I am not suggesting that you could prevent that, but in their ordinary daily life they would be kept from the fly, which with our present knowledge, until we can eliminate the fly is the objective. If in addition to what I suggested, it was the custom to clear for a considerable distance on each side of much travelled roads, that would add to the security, would it not?—Yes, if the clearing was really sufficiently wide.

2947. (Mr. Buxton.) You were asked just now if you would agree that the natives should be permitted to drive off the game or to destroy the game within a mile of their villages. As a matter of policy would you be in favour of the distribution of modern arms of precision among natives?—I certainly should not.

2948. (Mr. Millar.) With regard to the experiment that was made in the Ngara sub-district of the Marimba district of opening up a free shooting area to natives, can you speak of your own knowledge of the conditions under which it was carried out?—No, I cannot. I believe that Sir William Manning was there then.

2949. With regard to whether the experiment was a success or not, everything would depend on the number of guns or weapons which the natives possessed?—Possibly, but I cannot speak as to that; I was not there at that time.

2950. (Mr. Read.) Sir William Manning in his despatch of the 13th July 1912 refers to the question of arming the natives with guns of precision and says: "If it were accompanied by the wholesale arming of natives with weapons of precision, the amount of game killed would, no doubt, be greater, but the political possibilities of any such action are so grave that I hesitate to contemplate such a measure." We must attach great weight to the opinion of the local authorities and we must observe the provisions of the General Act of the Brussels Conference?—Sir William Manning puts it very diplomatically. I should have said that I could not agree to it under any conditions.

The witness withdrew.

Mr. F. C. SELOUS called in and examined.

2951. (Chairman.) I think I may take it that there is probably nobody alive who knows more about the habits of game than yourself, in Africa at any rate. —I daresay there is, my Lord.

2952. I put that as a statement, not as a question. You have had in many parts of Africa great opportunities of watching game and their habits. Naturally you have been in contact with tsetse-fly. Am I right in

saying that the tsetse-fly which you have been most in contact with is *morsitans* and not *palpalis*?—Yes, almost always *morsitans*. I have met with *longipennis* in the northern parts of British East Africa but I have never seen *palpalis*.

2953. In what parts of Africa have you seen *morsitans* and game?—All over South Africa more than 40 years ago, on the Limpopo and all the way from the

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Limpopo up to the Zambesi and North-Western Rhodesia, in the Bahr-el-Ghazal province of the Sudan, and a little in British East Africa.

2954. Is it your experience from observations of *morsitans* that in a game country they attach themselves to the larger mammals, in preference to other animals or to man?—My experience in South Africa was quite different from my experience in the Sudan and therefore if I only gave you my experience in one part of Africa it would not tally at all with my experience in another part.

2955. Then will you tell us both please?—You have only to read the writings of Livingstone and Oswell and the early hunters and travellers to see that they first met with the tsetse-fly where the Notuwani river runs into the Limpopo, along the whole valley of the latter, and in the northern part of what is now the Transvaal, and also on the tributaries that run into the Limpopo from the north. When I first went into the interior of South Africa in 1872, the tsetse-fly was still where it was in Gordon Cumming's time. That is at Silikaa's kraal on the northern bank of the Limpopo. From there the whole valley of the Limpopo eastwards was full of fly. You could not travel with bullocks or horses in that part of the country and in many other parts of South Africa. In those days buffaloes were enormously plentiful, and in the whole valley of the Limpopo, wherever there was tsetse-fly there were always large herds of buffaloes as well. The natives became very generally armed in the early days of the discovery of diamonds. No native would come to work on the diamond fields unless he could take a gun and powder away with him, and although the Boers protested very loudly against the natives being supplied with arms, their objections were overridden, and in order to induce the natives to come to work, they were always given arms; the buffaloes got killed off or driven away very quickly. They lived in large herds along the rivers; they cannot run very fast; the natives could keep up with them and fire at them over and over again, and the buffaloes were the first of the large animals that were gradually driven out of the Limpopo valley. In 1872 there was still a lot of fly there. In 1886 I travelled along the northern bank of the Limpopo from the Notuwani to near its junction with the Tuli. There had been no buffaloes there then for several years, but all other game was still very plentiful. There were koodoos, water-bucks, zebras, wildebeeste, roan antelopes, and impalas along the river, but no tsetse-fly. In South Africa over large areas the tsetse-fly disappeared with the buffaloes, although many other species of big game long survived the disappearance of both buffaloes and tsetse-flies. The same thing happened over and over again. All along the Botletlie river in Livingstone's day there was a great deal of fly and large numbers of buffaloes. In 1878 when the Boers made their great trek to Humpata in Portuguese West Africa they still found large numbers of buffaloes along the Botletlie and tsetse-fly. The traders who went to Lake Ngami before 1878 had to trek through two belts of tsetse-fly always by night. The Boers killed all the buffaloes along the Botletlie. Nobody has ever seen a buffalo on the river since 1878 and the fly from those two belts has disappeared. The same thing has happened on the Okovango river just north of the lake. If you read the accounts of old elephant hunters you will see they found tsetse-fly and buffaloes there in enormous numbers. When Moremi, the chief of the Batawana, was driven out by the Matebele and trekked to the Okovango river, he and his people killed or drove away the buffaloes and the fly disappeared and now the Batawana have large herds of cattle. It has been my experience in South Africa that the tsetse-fly is not able to exist without the blood of buffaloes. There was a "fly" area south of the Victoria Falls near Pandamatenka and another area 12 miles in extent to the south of Daka on the old wagon road that the traders made to the Zambesi many years ago. We always had to trek through those fly belts in the night. I went up in 1874 to the Zambesi and again in 1877. In the first year there were large herds of buffaloes. I saw them myself round about waters

where the tsetse-fly was. Then the natives began to obtain firearms and in 1877 there were still numbers of tsetse-fly and also buffaloes, but the buffaloes did not live there all the year round. The fly was always there. It remained all the year round, but there was no permanent water. There were large vleis that filled in the rainy season and during the dry season dried up. During the rainy season there were always large herds of buffaloes in the district, but as the waters dried they went to the eastward. For several months in the year the tsetse-fly had no buffaloes and they lived during those months on the blood of other kinds of game, although it was scarce. They seemed always able to maintain themselves as long as the buffaloes came there during the rainy season, but as soon as the buffaloes through constant hunting no longer returned to the area the fly very soon began to dwindle, and in 1888 there was not a single fly left there although the other kinds of game were still there, eland, giraffe, roan antelope, and so on.

2956. When you and your men were travelling in that part of the country, did any of your people suffer from anything like what we know now to be sleeping sickness?—No, we never had it, living as we did months together, day and night, in parts of the country where tsetse-fly were enormously plentiful. It certainly had a certain effect on us. It made us very thin, and after three or four months we began to get weak; but we had no sickness, and I never knew one of my boys die.

2957. Your experience was different in other parts of Africa?—Yes, absolutely different. Three or four years ago I went to the Sudan and I went to the central portions of the Bahr-el-Ghazal province. Having had such a large experience with the tsetse-fly in South Africa I was very interested in the question and I made exhaustive inquiries both amongst the officers living at the different stations and amongst the natives. I found *morsitans* almost everywhere—I brought a lot home for Mr. Austen and he pronounced them to be *Glossina morsitans*. I found fly over quite a large area of the Bahr-el-Ghazal. There was in some districts very little big game. I will not say there was none, because there were a few herds of hartebeeste and a few giraffe, but there was very little big game and quite a number of tsetse-flies. I asked the natives if the buffaloes ever came out into those districts during the rainy season, and they said, no, they never came there at all. So that those flies were able to maintain themselves perfectly well without the blood of buffaloes and without a constant supply of the blood of any kind of big game. They were living on the very small antelopes, bush-bucks, duikers and oribi. I wish to make a point of this: I went to a water, 15 miles from Rumbek, a place called Beer-el-Gurud (the well of monkeys). I saw no fly there. I had 20 donkeys with me. I saw lots of fly soon after I left the well the next morning. Three days later, the officer commanding at Rumbek came to my camp and told me he had had a terrible time at Beer-el-Gurud with tsetse-flies; that the tsetse-flies were so thick that he was obliged to have water carried to his mules and donkeys at a long distance from the water. I said "That is very curious; I was there only two or three days ago and saw no fly." A little later I went back to Beer-el-Gurud, and there was a great troop of baboons there. They run in herds of 50, 60, or 100 together. There is a clump of great big trees there and there is water under these trees and the well is outside. A great number of baboons were amongst the trees—they had come down to drink and the fly was there with them very thick. I visited that place five or six times. Whenever the baboons were at the water there was the fly with them. If you saw by the tracks that the baboons had only just left there would still be a good many flies, but if the baboons had been away for some little time there was not a fly to be seen. I am perfectly certain that the flies were living on the baboons more than on anything else. Baboons are enormously plentiful in all that part of the country.

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2958. We have been told, and I would like to see whether you agree with it, that *morsitans* are never found except in bush areas, or that they require shade of a certain sort and that you never find them in open ground?—No—absolutely—they cannot live in open grass or open reed country.

2959. If they were feeding on game, they would not follow game more than a certain distance from the area in which they live, would they?—In my experience in South Africa the fly areas were always perfectly well defined.

2960. In the country which you have just spoken of was there sleeping sickness, or what we now know to be sleeping sickness, to your knowledge?—Do you mean to the south of the Zambesi?

2961. No.—There was no sleeping sickness in the Bahr-el-Ghazal.

2962. It has been suggested that as it is possible that the game forms the principal reservoir of the trypanosome that carries the sickness, an experimental area might be cleared of game for a certain time if possible to see whether the fly disappear with it, or whether it would become non-infective. Do you think it would be possible in fact efficiently to clear of game an area say of five miles across each way?—I think it would be very difficult after my experience in the Bahr-el-Ghazal. It was a simple matter in South Africa. The fly certainly was not able to live after the buffaloes had been exterminated, but in parts of Africa where that is not the case, and where the tsetse-fly is able to live perfectly well on the blood of other animals, and where, as I have myself seen in the Bahr-el-Ghazal, they live on baboons, I do not see why they could not maintain themselves on the blood of squirrels, hares, ichneumons, or any kind of mammal, however small. I do not see how you can exterminate all life in a territory 25 miles square, nor can I see the necessity.

2963. Limiting it to the antelopes and larger mammals, if they were driven out would not their tendency be to constantly return?—Yes, certainly. If you shot them the survivors would come back as soon as the persecution came to an end.

2964. (Mr. Buxton.) What you have just said is very important as bearing on the suggested experiment. One idea is that an area should be taken and the animals destroyed as far as possible, but I suppose that more would escape than would be destroyed?—Yes.

2965. In your opinion they would return, governed by the homing instinct, when the driving-out ceased?—Certainly.

2966. Unless you kept an army continually driving them out you would not keep the area free from the animals that you wanted to get rid of?—No, I do not think so. Might I ask you what use the country is going to be put to which it is proposed to clear of game?

2967. The suggestion was that an experiment should be tried to see if the effect would be that, in the first place, the fly would leave the area when the game disappeared, and, in the next place, to ascertain its infectivity. I am glad to know your opinion. You have no experience, I suppose, of attempted enclosures and the nature of a fence which would keep game out, have you?—No. All the fences that I have seen have never succeeded in keeping game out.

2968. You must have seen many native fences?—Yes. The game jump over the native fences—the clands and the koodoos.

2969. Am I right in saying that you have never seen a native fence that would keep the antelopes out?—No, they cannot keep them out; they always make fences round their cornfields, and they leave a little opening here and there and dig pitfalls in those openings. The game very soon get to know the pitfalls, and they jump over the fences. I have known natives have to sit up all the time their corn was ripening, night after night, light big fires and beat tom-toms all night long in order to keep the game out.

2970. (Dr. Balfour.) What you say about baboons is very interesting. The usual idea is that a monkey

is much too sharp for the tsetse-fly, and would catch it when it lighted upon him to bite him. Did you actually see *morsitans* feeding on baboons?—No, I did not actually see it. All I can say is that it came to the well at Beer-el-Gurud with the baboons. If a fly settles on the back of your neck you do not feel it. The first thing you know is that you get what the Boers call "stuck," and, quick as a baboon may be, I do not see how he can catch a fly on his back. That is the usual place they go for human beings.

2971. The general idea has been that the monkey is too quick for the tsetse-fly, so it is interesting to have your views.—I daresay that if a baboon saw a fly anywhere where he could get his hand he would catch it, but not if it settled on his neck or on his back.

2972. The old Boers had many ways of exterminating game and hunting it. Had they any method of laming or crippling it apart from pits with stakes set in them?—No, I have never heard of it.

2973. I thought that there might be some way of getting over the fence difficulty. Supposing you had an experimental area, could you surround it with what are called calthrops, a sort of metal porcupine that would lame the game or check a charge? Would such a method be of any value?—The Boers never lived in tsetse-fly country. They only went into the tsetse-fly areas when they were hunting elephants.

2974. Would it be a practical measure to surround a limited experimental area with a large supply of calthrops on the ground and to honeycomb the ground with small pits, not game pits but small pits to take the place of a fence, or to supplement a fence if necessary?—I think it much better to clear the area of bush.

2975. But the game from the uncleared area around would invade the experimental area. My idea is to check the game from coming back into the experimental area after you have cleared it. Could you do it by such means as I have suggested?—I do not think so. You might catch some of the herd if a large herd came, but the others would just go on.

2976. You could not lame or cripple them on a large scale to enable you to get at them or to turn them?—I do not think it possible.

2977. A few solitary animals would be turned by such an obstruction as that?—Of course, if they got into any of the traps.

2978. I do not mean traps, but the use of calthrops and small pits, making the ground treacherous and difficult?—I do not think that that would stop game.

2979. (Mr. Millar.) You said that in your experience the fly areas were well defined and did not vary to any very great extent?—That is so.

2980. Did you form an opinion as to what the characteristics of such areas were in order to enable the fly to exist?—Altitude is the first thing that counts in Africa. To the south of the Zambesi the fly is always in the low lying country; it is all along the south-east coast, and then up the valleys of the big rivers, up the Limpopo and up the Zambesi and up all their tributaries to a certain altitude.

2981. Up to what altitude approximately?—The highest is 3,500 feet. That is in the northern part of Southern Rhodesia. To show you how the fly are deterred by something, in that particular area in 1878 we made a wagon road from Matabeleland to near where Salisbury is now. We wanted to be as near the fly country as possible and to hunt elephants in the fly country. We ran our road not more than ten miles outside the fly belt all along. Sometimes large herds of buffalo would come up out of the fly area and the fly would come with the buffaloes for three or four miles, and then all the flies would leave the buffaloes and go back and the buffaloes would go on further up, always rising towards the watershed. When they came to our drift on the Umfuli River they were absolutely clear of flies. I have hunted buffaloes there myself with my horses and carried the meat back in wagons drawn by oxen and there has been absolutely no fly.

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2982. The buffalo got above the fly?—The buffalo got above the fly. The fly could not get up beyond a certain altitude.

2983. Would you mind telling us whether the three or four miles where the flies followed beyond the fly area was clear country or bush?—Exactly the same kind of country—bush.

2984. The altitude stopped the fly, you think?—It must have done. St. Lucia Bay is the most southerly limit of the tsetse-fly in South Africa. It is 28 degrees of south latitude. The buffaloes and all the game, and the character of the country, were the same north and south of the 28th parallel. As you get further south the nights get colder I suppose in winter and the fly has never come further south than St. Lucia Bay.

2985. Take for example vegetation. Has that anything to do with fly area in your opinion?—There is a certain kind of bush in South Africa where, if you get it at a low altitude and there is game, you may confidently expect to find tsetse-fly, but you also find it in other kinds of forest as well.

2986. There is nothing in the vegetation to indicate fly area?—In contiguous areas within and outside fly belts the vegetation was often identical.

2987. Are there any characteristics which in your experience indicate fly area?—No. Throughout the interior of South Africa all low areas along the big rivers were more or less covered with bush and forest and they were all more or less infected with tsetse-fly.

2988. Were there any areas where after the buffalo had disappeared the fly persisted with the other big game which remained?—Not in my experience in South Africa. As long as the buffalo were constantly present or returned every year during the rainy season the fly persisted, but when they ceased to come into these areas the fly disappeared.

2989. I am thinking of the district you were speaking of just now, that is, Rhodesia and up to the Zambesi.—There were many districts in which there were buffaloes and no tsetse-flies. The buffaloes were not in any sense bound up with the tsetse-fly. When they were above a certain limit they were absolutely free of fly.

2990. Did you form any opinion as to the life history or habits of the fly from your experience in other districts?—I noticed that you could go through a district in June or July when the nights are very cold and not see a single fly, and an inexperienced man might pronounce such a district to be absolutely free from fly, but if you went through the same country in October or November when the sun was intensely hot, you might find any number of flies. As the weather got warmer they got more and more plentiful.

2991. Have you formed any opinion as to the most suitable breeding places for them?—No. In those days I believed what the bushmen said, that they laid their eggs in buffalo dung, but we know now that they do not lay eggs.

2992. Are you in a position to tell me what in your opinion has been the effect of the modern game laws throughout the districts you have been familiar with? Can you tell me whether the effect of the restrictions imposed under the present game laws has been to lead to an increase of game?—You see I have not been in South Africa lately. I have not been there now for 16 years.

2993. You cannot speak to anything within a more recent period than 16 years in South Africa?—No.

2994. Before that there were practically no game laws in existence?—No, there were no game laws.

2995. You cannot speak I suppose of the actual cases of sleeping sickness which have occurred, except what you heard of in the early days?—If natives or even a white man died in those days we always put it down to fever or something of the kind. We had no medicine and no means of ascertaining what the illness actually was.

2996. You did not diagnose it as sleeping sickness?—No.

2997. (Mr. Rothschild.) You have mentioned several times that the fly disappeared when the

buffalo disappeared. Have you any experience of the fly disappearing through the game disappearing during rinderpest? I ask the question because we have been told in evidence that in one area where the rinderpest cleared out the game a few buffaloes remained but that the tsetse-fly all went. Have you any experience of fly going although the buffalo remained when the scrub was cleared?—I left South Africa the year of the rinderpest. The rinderpest came through the country in 1896, during the rebellion in Matabeleland, and I left immediately after the rebellion. You say that the tsetse-fly went away, but it did not go away. In my opinion the tsetse-fly absolutely died from taking the blood of diseased animals. If you take the Sabi game reserve in the north-east Transvaal in 1896 there were fly areas in which there were buffaloes. That was the only place left in the Transvaal where there were buffaloes and where there was tsetse-fly. During the hot weather of 1896 and the beginning of 1897, the rinderpest swept through that country. The next season when hunters went down there they thought the buffaloes were all gone. The fly certainly had all gone and they found no fly whatever and there has never been a fly seen there since. A few buffaloes survived the rinderpest as they have done everywhere else, and they subsequently increased in a country that was absolutely free of fly. The fly could not come in from outside, because this fly area was absolutely isolated. On the one side it had the high veld of the Transvaal and on the other side Portuguese East Africa. The whole country was free of fly and therefore it was impossible for fly to come in again from outside. I think the evidence shows conclusively that the fly died from drinking the diseased blood, and that is the reason why there are buffaloes now without fly.

2998. The second part of my question is whether you know of any area where the bush has been cleared where the fly has gone but the buffalo has remained?—There is no doubt whatever that if the bush was cleared the fly would go. The fly cannot live outside the bush. When the Boers followed Umziligazi from the Transvaal in 1840 and crossed the Limpopo they chopped their army road. It was a quarter of a mile broad and they trekked with oxen and horses through that cleared space and did not lose their cattle by fly. I only heard about it from the Boers and that was a long time ago, so I cannot say exactly the width. If you wish to open a transport road through a fly belt to make it safe for transport animals, it appears to me that the easiest and safest way is to chop a road through the bush for the width of a mile and clear it of all bush. I think you could then pass through the area without danger to the oxen.

2999. But do you think a mile would be sufficient to ensure permanently that no fly would ever wander in there?—On the Loanja, a tributary of the upper Zambesi, the French missionary, M. Colliard, made a wagon road under native guidance in 1886. He journeyed during three nights along the river through country full of tsetse-fly, where buffaloes were also very plentiful at that time, and they remained plentiful there until 1896, when the rinderpest swept through the country. Both buffaloes and tsetse-flies then completely disappeared, and I believe have never since reappeared. Speaking of his journey up the Loanja, M. Colliard has written, "How capricious this 'murderous fly' is! Can it be believed that swarms of it abound in the forests which skirt the Loanja, while on the grass islands about 500 yards off there is 'no trace of them?' These islands are sure and well-known refuges, where the Barotsi in travelling 'always pen their cattle.' That this is true I can testify, as I travelled up the Loanja with horses in 1888, moving through the fly-infested bush by night and keeping the horses on the islands in the daytime, only a few hundred yards away from the bush where the fly swarmed.

3000. (Sir Mackenzie Chalmers.) Do you think a quarter of a mile sufficient for protection?—All I can say is that those little grass islands were not more than a quarter of a mile from where the fly were in hundreds. It was the experience of the

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natives that if they travelled through the fly area during the night and kept their cattle on the islands during the day-time the fly never touched them.

3001. At what time does this fly cease to bite, as soon as the sun goes down?—It all depends. On cold nights the *morsitans* become numbed practically as soon as the sun goes down; when the nights get warmer, any time after the middle of August it will bite. If you walk through bush where tsetse-fly is it will come on you and bite you in the middle of the night at any time. It is a matter of warmth.

3002. The buffalo, I suppose, do not suffer from the fly. They are tolerant?—They do not. No wild animals suffer.

3003. Generally as regards South Africa, do I understand that you have only seen *morsitans* where there are buffaloes or the possibility of buffaloes?—Yes. I never met with *morsitans* anywhere in South Africa where buffaloes were not permanently living or where they did not frequent that part of the country during several months of the year. This does not apply to other parts of Africa. I hope you will understand that what I say about buffaloes has no application to Nyasaland nor I believe to any other part of Africa.

3004. Only to Southern Rhodesia?—Yes, to Southern Rhodesia and all that country south of the Zambesi.

3005. (Dr. Chapple.) I think you told us that in your opinion the tsetse-fly died off during the epidemic of rinderpest because it fed upon the poison of the infected animals?—Of course that is only supposition but I cannot see why else it died out. Take the Sabi game reserve area. The greater part of the animals was not affected by the rinderpest. The rinderpest did not affect all animals in Africa. It affected for instance the buffaloes, elands, koodoos and bush-bucks, but it did not affect water-buck, sable antelope, roan antelope, wildebeest, impala and many other kinds. The tsetse-flies all disappeared in a few months. After the buffaloes and koodoos had died, there was any amount of other game to feed upon. They did not die out because of want of blood.

3006. Sir David Bruce said that he had fed a lot of flies on rinderpest animals and they seemed to enjoy it.—I do not know how he would account for the disappearance of the fly from the game reserves I have spoken of. It is only my supposition. I do not know how he would account for the complete disappearance of tsetse-fly in the area. They did not die for want of blood.

3007. That is a matter of great interest.—Thousands of animals in the area did not die.

3008. If the rinderpest-affected animal did not poison the tsetse-fly, is it not possible that the tsetse-fly had some preference for the blood of the buffalo and when the buffalo left, went elsewhere in search of its favourite food or else died off?—But they would not disappear immediately under those circumstances. In countries where buffalo were exterminated the fly did not disappear immediately. It lingered for some years but gradually diminished in numbers and after a few years entirely disappeared.

3009. If there is any big game the fly has a preference for, we might select that game and attempt its extermination?—That would answer all right in Africa south of the Zambesi but it would not answer in the Bahr-el-Ghazal district.

3010. Or in Nyasaland?—Or in Nyasaland. They thrive perfectly well in countries where there is no buffalo.

3011. There is no game in Nyasaland that you know for which the tsetse-fly has a preference?—I have never been in Nyasaland, but I believe that the tsetse-fly all over Africa has a preference for buffaloes, and it becomes more plentiful where there are large herds of buffalo than anywhere else. In the Bahr-el-Ghazal between Meshra-el-Rek and Wau the tsetse-fly is only found along the road (which takes seven days to walk) in one spot, and that is in the Khor-el-Dammús—the valley of the buffaloes. There are numbers of buffalo there. But in other parts of the country I found from personal experience that the fly exists where there are no buffalo.

3012. Do you think that systematic observation carried out in Nyasaland, for instance, might determine what animals formed the favourite diet of the fly?—You found out about the buffalo, for instance.—They take blood from all other animals as well; I feel pretty sure of that. I have seen impala, for instance, feeding along the river. You could see them move every now and then as you would expect them to move if flies were on them. There were thousands of fly there.

3013. There is no hope for us along that line then?—No, I think not. I think the fly takes blood from any mammal that it comes across.

3014. With regard to arming natives to exterminate animals from the immediate vicinity of villages, is it not your experience that natives get on much better with the bow and arrow and pitting and trapping?—No. They never killed off the game in South Africa until they had got guns. They really do it much quicker with guns.

3015. Have you seen them employing their own native methods?—Yes, I have.

3016. Would the bow and arrow be of any practical use?—Yes. Some use poisoned arrows, and they are far more deadly. I have seen them shoot at a herd of game by pulling the bow with their toes when sitting down. They did not shoot at the animals really, but in the air like that (*describing*), and the arrow dropped on to some animal's back, or that was what they hoped for. Whenever I saw them shoot, the arrow did not drop on to any animal's back, but evidently that was the idea. They do not kill any great quantity of game with their own weapons.

3017. Not with traps?—They kill more with traps.

3018. Is that an effective way?—It is far more effective than their shooting, except with the poisoned arrows. The tribes that use poisoned arrows kill a lot of game.

3019. Poisoned arrows are the most effective, trapping is next, and shooting with the unpoisoned arrow third?—Unpoisoned arrows are not very effective.

3020. Would it be possible to diminish greatly, if not exterminate, the number of game round villages by permitting the natives, short of arming them, to use their own methods of extermination?—I expect they always do so. They are not allowed to have arms, but I do not think there is any law against their trapping game or even killing it with bows and arrows.

3021. I think that the game laws are sufficient to interfere with that in Nyasaland?—I do not know, I think it would be very difficult to prevent.

3022. If game around a village are incriminated, and it is known that the game harbour the trypanosome of the disease and through the medium of the tsetse-fly spreads the disease to domestic animals, would it not be possible to carry out an organised system of exterminating all game within a certain radius of the village by using the natives, by trapping and by relaxing the game laws, keeping the game at a respectful distance from those villages, thus protecting the domestic animals?—But even when there were no game restrictions the natives could not accomplish that in bush country. They will drive off the larger game, but in the Bahr-el-Ghazal for instance the natives do their best to trap and kill the smaller antelopes in any way they can, but they cannot do much. They are close to the villages all round and the natives cannot keep them down.

3023. If that relaxation were supplemented by organised Government hunting, that is hunting under Government regulation, for the purpose of driving them back, do you think a sufficient area round the villages could be cleared of game to protect domestic animals?—Usually where the natives live the area round the village is cleared of all forest and bush. They clear the land themselves for planting corn and sweet potatoes and they keep the tsetse-fly away. Where you have very poor natives who live in a few huts without much clearing, there the tsetse-fly will come into the village, but where the natives clear considerable areas of ground, as most of them do, the tsetse-fly keeps beyond the farm.

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3024. Could there be an economical clearing of areas all around these villages by the Government in such a way that the land would be so valuable afterwards that the Government could recoup itself its cost by the sale or leasing of the land on the advance of settlement? Could the land be sold or leased in the future for settlement?—I doubt it very much. If you wanted such land for grazing cattle you would need in any part of Africa which is subject to long drought at least five acres for every beast and you would have to clear a very large area to be able to run any large herd of cattle there.

3025. If the clearances are necessary, would it not be a profitable thing for the Government to clear in anticipation of later needs?—But who do you think would settle there?

3026. Either Europeans or natives, I presume.—Speaking generally, the fly country south of the Zambesi is absolutely useless for settlement. It is low and very hot country, very unhealthy for Europeans, and I do not think that any country that ever has been infested with tsetse-fly up to this day has been settled by Europeans. Take the Limpopo Valley, there has been no fly in the Limpopo Valley for 30 years, at any rate, but there has been no settlement of Europeans in the Limpopo Valley. It is low lying disagreeable country to live in, and it is unhealthy. The Europeans will first want to settle in the high and healthy parts of Africa.

3027. What remedy would you suggest for the difficulty we now find ourselves in?—I would suggest chopping and clearing. If there is a mine in the fly country that you want to work, and to which you have to carry machinery by ox transport, I would advise chopping a very broad road through the fly area, clearing it of all bush to where the mine exists. If a transport road has been closed owing to the tsetse-fly, the fly belt is probably only a few miles broad, and I think that by clearing a space a mile broad through the fly belt you could use it for ox transport perfectly well.

3028. That is half a mile on either side of the track?—Yes.

3029. Do you think that that would be safe?—In the only instance that I know of along the Loanja River with the little islands the fly never came from the neighbouring bush much less than half a mile away. The fly never came to the islands.

3030. Would you not interfere with the game along the roads?—I do not see how you are going to kill off all species of mammals in these tsetse-fly areas in Africa.

3031. Does not game get shy along the roads where it is hunted?—No, not the small game.

3032. Does the large game get shy?—Yes, but still if rain falls and grass grows over a certain area the game does not mind whether there is a road there or not. It will work down to where the grass is.

3033. If the game comes out of the grass the fly will not follow it for any distance?—The greater part of the fly country that I have been in in Africa is not dense bush; it is open forest with patches perhaps of thickish bush in it; but there is grass all through it.

3034. Now what would you do in the villages?—I would leave the natives to worry it out. They are in the same position as they have been for hundreds of years.

3035. Then you would not do anything?—No. Apparently the mortality amongst them is very small.

3036. I was thinking of domestic stock.—You cannot possibly take domestic stock into an area where there is tsetse-fly. You cannot take cattle. They would all be dead in two or three months.

3037. Then what would you advise this Committee to suggest in order to make it possible for settlement to proceed and for people to have their domestic animals there?—Well, I should like to know what evidence there is that anybody wants to settle in the tsetse-fly country.

3038. If they did want to settle there what would you say?—But surely, why should they want to settle there?

3039. We are here in order to advise the Colonial Office as to what steps should be taken in order to protect domestic animals and men from trypanosomiasis in these territories. What help can you give us? What would you suggest as to the advice we should give the Colonial Office? Would you say to the Colonial Office, "These places are not worth settling; do not bother your heads about it"? Is that your advice?—Yes with regard to European settlement. If you wish to connect one suitable area for settlement with another by means of a transport road, the best way is to clear a wide area of all bush. In Southern Rhodesia, in the Gatooma district, which used to be infested with fly, and I believe still has fly, they have opened up a lot of mines. It is very necessary that they should be able to keep the wagon roads free of fly but it is not a country that one could care to settle in unless he were particularly fond of fever.

3040. We have had evidence about places once inhabited by domestic animals and people, whence the people have now disappeared and the animals have been exterminated. Sir David Bruce told us that.—As to that I think there may very well be some misconception. In 1896 when the rinderpest swept through the country it cleared large areas which hitherto had been infested with tsetse-fly. The tsetse-fly all died, and the natives finding that the fly had gone, moved with their cattle into countries which had been previously infested with tsetse-fly. Then the tsetse-fly which had survived in certain areas began to spread and, as the game increased, gradually reoccupied all its old haunts. Naturally it came to the parts of the country where the natives had brought their cattle and it drove the cattle out. I take it that that is the explanation. Such a thing certainly never happened in South Africa.

3041. Not in South Africa?—I have no experience of fly extending its range into countries where it had never been before.

3042. You have no practical suggestion to make as to how the fly and disease can be exterminated in South Africa?—No. My opinion is that in South Africa the fly came to an end, because the buffaloes came to an end, but that state of things does not exist in other parts of Africa.

3043. You have never been in Nyasaland?—No, only just to the Loangwa River where it runs into the Zambesi. I know that it is a terribly hot unhealthy part of the country, and I cannot imagine Europeans wanting to settle there.

3044. When you were travelling through these extensive districts did you take any personal precautions against being bitten by flies?—None whatever.

3045. (Chairman.) You have not been in Nyasaland, but do you know whether the population in Nyasaland or Rhodesia are a stock-raising population? Are they agricultural or pastoral? I will put it in that way. If you do not know, it does not matter.—All African natives keep cattle if they can. There are many agricultural people who to-day have no cattle because their cattle has all been taken from them by the warlike tribes. I do not think that any natives would live in a tsetse country at all if they had not been driven in by more powerful tribes. The warlike tribes attack the weak agricultural tribes, take all their cattle and drive them here and there into the fly country.

3046. The objection to the fly country not being the fly, but the fact that the country in which tsetse-fly lives is not a pleasant country in which to live. Is that what you mean?—Yes, certainly. Speaking generally, in all my experience in South Africa, not only white men, but natives will not live in the fly country if they can possibly help it. All who live in the fly country have been driven in to get out of the way of these warlike tribes.

(Chairman.) Thank you very much.

The witness withdrew.

ELEVENTH DAY.

Tuesday, 25th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Dr. A. G. BAGSHAWE.
 Dr. ANDREW BALFOUR, C.M.G.
 Mr. E. NORTH BUXTON.
 Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
 Colonel Sir W. B. LEISHMAN, F.R.S.
 Dr. C. J. MARTIN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.
 Professor R. NEWSTEAD, F.R.S.
 Mr. H. J. READ, C.M.G.
 The Hon. L. WALTER ROTHSCHILD, F.R.S.
 Sir STEWART STOCKMAN.
 Mr. A. C. C. PARKINSON (*Secretary*).

Sir HARRY JOHNSTON, G.C.M.G., K.C.B., called and examined.

3047. (*Chairman*.) You have a long and varied experience of tropical Africa?—Yes, I think I may say that I have.

3048. You were a good deal in Africa, I suppose, before sleeping sickness was identified at all?—Yes, but not before it was heard of. I first heard it spoken of in 1882, but I went out then rather prepared to hear of it because I had been reading the works of Wynwood Reade of 1860 and the book of Monteiro—published early in the 'seventies dealing with Angola—and both of those authors had mentioned sleeping sickness almost under that name.

3049. When you first saw it recognised as sleeping sickness what country was that in?—In Loango, just to the north of the mouth of the Congo.

3050. Was that *palpalis* or *morsitans* country?—Probably *palpalis* as we now know it. I heard of it again in Angola in 1882. I did not connect the sleeping sickness with any fly. I heard of it as sleeping sickness.

3051. At that time did it assume any serious proportions in comparison with other illnesses?—No; the cases were few, but as it occurred several times amongst half-castes, it attracted even then a good deal of attention. If any of the Committee would look up the works of Wynwood Reade they would find that he wrote rather a charming story about sleeping sickness and the death of a half-caste woman more or less based on things which actually came under his observation about 1862.

3052. That was a country, was it not, where there was both game and fly?—There was not very much game even in these days in Northern Angola, where I heard accounts of sleeping sickness in 1882. In Loango there was very little game, as we term it, at all. There were plenty of big apes and small wood antelopes, but there was very little big game. There was the red buffalo, it is true. Dr. Héli Chatelain, by-the-bye (a Swiss missionary), mentions in 1894 in his "Folklore of Angola" that the "sleep sickness" carries off numerous natives annually.

3053. Since that time, have you met in your career with much sleeping sickness?—I have not come personally much into contact with it. It was only just beginning in the central part of the Uganda Protectorate as I was leaving. When I was in the Congo Free State in 1900 the Belgian officers talked to me of the spread of sleeping sickness. When I visited the Congo myself long before, when Stanley was there, I heard it mentioned occasionally as a disease, but except that I was interested in the subject on account of having read Wynwood Reade, I did not notice it very much. I first realised what an awful thing it might be when I heard of it from the Belgians in 1900. As you are no doubt aware, almost coinciding with my leaving the country of Busoga the first cases of sleeping sickness began—in 1901.

3054. Was it then attributed to fly?—No.

3055. At the time you became aware of sleeping sickness among men was there much disease called now nagana amongst domestic stock?—In Uganda there was practically nothing of the kind, not even in the frontier regions of the Congo State where they possess large Ankole cattle with long horns. I heard of no serious sickness at all. When I brought down the Ankole cattle with long horns to the coast regions on the Victoria Nyanza they all began to sicken and die, and we remarked amongst ourselves how like it was to the tsetse disease, and concurrently Sir Frederick Jackson showed me specimens of tsetse which he had just captured, and which naturally in those days we took to be *morsitans*.

3056. When was that?—In 1900.

3057. Was that prior to the outbreak of the epidemic amongst men?—Yes, quite a year before, and that was a little farther to the west.

3058. All that time, if there was sleeping sickness, it had not assumed epidemic form in Uganda?—No. Just one or two isolated cases occurred 12 months afterwards, and the bad epidemic began in 1902.

3059. After that you went away and were not there at the time of the outbreak?—I was visiting Busoga near the close of my stay in the Protectorate in the spring of 1901 and visiting the island of Buvuma. When the first cases of this mysterious malady were noticed by the natives, I took a photograph which I thought very typical of natives in the banana groves of Buvuma, and I noticed how very sluggish the people in the foreground were. As most of the people were rather wild at that time, I asked why they had been so obliging as to pose without taking notice of me. I was told that they were suffering from a disease which brought on great sleepiness.

3060. Speaking generally, when you heard of the outbreak later on in Uganda, to what source, if any, did you attribute it? I mean the outbreak among men. You have said that it was there among cattle before. I suppose that I was influenced by the mosquito theory which was well established by them, and possibly thought that it was due to infection from a germ-carrying insect. There were several theories at the time, and I may have dallied with this, that and the other, but my own leaning, I think even then, was towards infection by means of some arthropod.

3061. If the germ-carrying insect had been there all the time, how did you account for the sudden outbreak in epidemic form in Uganda?—I accounted for it at that time, I suppose, by realising that the disease existed in the border regions of the Congo State, and that the traffic we had newly opened up between that region and the Uganda Protectorate by means of roads had brought in human beings infected with sleeping sickness and that some agency had been interposed.

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3062. That is what I was coming to.—If I may interrupt to answer your previous question, I now recollect our first theory was that the infection was carried through the saliva by some of the men who were recruited as soldiers, putting their fingers into the same pot of rice or other food with the natives of Busoga. We thought that by putting their fingers to the mouth and then into the pot, the infection was conveyed in that way. We connected it with the people of the Congo State at once.

3063. To put it shortly, you thought that the epidemic, however it might be distributed afterwards, was originally brought in by man and not by game or cattle?—Yes, that is what we thought.

3064. From what you have read and learnt from personal experience, do you connect the disease closely or not at all with the presence of wild game, which we are told are a possible or perhaps probable reservoirs of the trypanosome that carries the disease?—I do not think myself that I see much connection between the abundance of certain species of big mammals and the spread of the disease, but I would not like to be dogmatic. I sometimes, in thinking the matter over in my own mind, ask myself why, for example, in Somaliland, where until quite recently big game swarmed, and again in many parts of the Sudan, where they have swarmed and swarm still, there is no sleeping sickness that we know of. I remember vividly in connection with malarial infection, that Sir Alfred Sharpe and myself laid great stress on this point. We both knew by experience the very disagreeable country between the west bank of the Lower Shire and the Portuguese region of the Zambesi. If you went overland from Blantyre in the Shire highlands to Tete on the Zambesi you would cross a strip of country particularly disagreeable to the traveller. There was scarcely any population, and no big game; there was nothing to shoot by the way, so that your porters ran the risk of being very hungry if they did not carry loads of provisions. The region was infested with tsetse-fly to such an extent that when we tried to establish native settlements there in connection with the Cape to Cairo telegraph line, even the very dogs seemed to be poisoned by tsetse, and goats could not subsist there. The tsetse forced their attention upon you because they were in such quantities that they perpetually attacked you and tried to bite you.

3065. Was it *morsitans* or *palpalis*?—Probably *morsitans*, but the region was remarkable for being swept entirely free of big game.

3066. Is the tsetse-fly entirely dependent on blood for its sustenance, speaking of blood generally?—I would not place much reliance on my investigations in that direction, but I should think, speaking in a general way, that it was mainly a blood-sucker if not almost entirely a blood-sucker.

3067. Does the fly show a preference for warm mammalian blood to either reptilian or avian blood?—I have hardly an opinion to offer. I do not think I could say. It certainly buzzes a good deal about the places where the crocodiles are basking in the sun, I have noticed that.

3068. The *morsitans*?—The *morsitans*. On the banks of the Shire where the crocodiles swarmed we have found the tsetse in great abundance.

3069. You think that that was *morsitans* and not *palpalis*?—I could not say. I only know it was a tsetse.

3070. Leaving that for a moment, it has been suggested to us that it might be useful, in order to obtain further knowledge, to make an experiment by enclosing an area and driving the game out, if possible, or destroying it. I will not go into detail about that at present, but is it in your opinion a practical thing to get game out of an area and keep it out of that area permanently?—I should think it was quite practicable within an area of not very large size to get rid of it and keep it out.

3071. Would it not be the tendency of game driven out or other game to try to come into an area suitable to it? Where there was the less game there would be the more food and the tendency would be to return, would

it not?—I have not noticed that tendency in the many parts of south and central Africa with which I am personally familiar. Take the region of the Shire, between Tete and Blantyre; it was so far back, according to the native stories, that the game disappeared from the region, that nobody could approximately fix the date; and because this district was clear of human beings there was no obstacle to the game returning and there was nothing to frighten it away, yet it never returned. It must have been there because we found skulls of buffaloes and other beasts so often.

3072. One question about the fly. Have you noticed that where ground is cleared of bush and shade the fly disappears?—Yes. In my experience that has been the one sure way of getting rid of tsetse-fly. I do not say that no fly has ever come back, but it has been the most practical way of getting rid of tsetse-fly to clear the low-growing bush.

3073. Do you consider that the cost of clearing would be very large?—It might be in some instances, but if I may speak openly and rather jump a question I should say that in no direction could money be applied with more practical benefit. In my opinion it would be a far greater step towards the extirpation of the tsetse-fly to clear regions of undergrowth and anything but short grass, and introduce, say, the *dhtib* grass of Ceylon to take its place. It would be far better to do that than to spend money on destroying the wild game, which of itself has the tendency to go away from the proximity of human interference with nature.

3074. The labour would be cheap, would it not?—I think so.

3075. I think I am right in saying that the natives themselves clear an area around every village?—Yes.

3076. Would it be very costly to double or treble the area of cleared ground round every village?—I should not think so. If you interested the native chiefs and told them the reason, a lot of the labour would probably be voluntary, because they begin to appreciate the importance of the question.

3077. In parts of the country suitable for growing crops or suitable for raising cattle, could you not increase the area of the cultivable ground without danger, and possibly be able to keep a small amount of stock with great benefit within the area? Is that possible?—I think so, from my own experience. When I first arrived in Nyasaland I noticed at Chiromo, for example, at the mouth of the Ruvo river, that it was almost impossible to land a horse or an ox. As long as you carried the beasts on boats in the river, the tsetse seldom if ever attacked them on board; they had a great aversion to exposure in the air over the water; but when landed the cattle were at once attacked, and soon afterwards sickened. When I arrived in 1891 to commence administration, we cleared a considerable area to get rid of the mosquitoes, which were an extraordinary pest at Chiromo and many other parts. We did not think so much about the tsetse. A year or two afterwards it was pronounced quite safe to land cattle and horses at Chiromo, and when they were kept within a cleared area they were seldom if ever attacked by tsetse. When I first reached Zomba, in 1891, the few planters and missionaries there said it was impossible to introduce cattle because of tsetse-fly. We cleared industriously for several reasons, one being that the bush concealed the attacks of Yao slave traders, who virtually besieged us several times at Zomba. Also I associated the bush in those days with the presence of mosquitoes. We got rid of the mosquitoes, and incidentally apparently of the tsetse-fly, and I believe that cattle have been capable of being kept at Zomba from that day onwards.

3078. Can you make any suggestions, apart from further inquiry, as to any method by which the fly could be reduced in numbers—by the introduction of enemies, or in any other way? Are you sufficiently an entomologist to do that?—I think that the first way to do it is by clearing bush in all directions. As I have said the fly detests the wind, and I would give it no harbourage or shelter; the second and almost equally important thing is the preservation of insect-eating birds.

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3079. Apart from insect-eating birds already there, can you suggest the introduction of any bird likely to survive its enemies? We are told that the pupæ of *morsitans* are placed in rather inaccessible places for birds that run on the ground. Do you think that there is any bird that could usefully be introduced that would be of assistance for that purpose?—I think there is no bird so useful for the purpose of scratching out and eating the tsetse larvæ as the Guinea fowl, which is indigenous.

3080. Take Nyasaland and Rhodesia, for instance?—There were a quantity of Guinea fowl there, but Guinea fowl are so toothsome that they are killed in very large numbers by the settlers. I would venture to direct the attention of the Committee to some reports issued by the French Administration of West Africa about two or three years ago, in which an official called attention to the increase of germ diseases which had occurred since Guinea fowl had been virtually shot out round the French settlements. He strongly advocated in his report the preservation of Guinea fowl—even the forbidding for a time the killing of any of these birds on any pretext whatever. He was a scientific observer apparently, and he pointed out in detail the way in which they were particularly useful in scratching up and eating the larvæ of the tsetse. He refers specially to the tsetse and certain beetles which damage crops.

3081. Are you sufficiently acquainted with the subject to know whether there would be the better chance of its being effective with regard to the *morsitans* than the *palpalis*?—No. I mean I could not answer that. The third method I should suggest is that which has apparently been very successful in the Island of Principe, that is employing, say, negroes to don sackcloth smeared with birdlime for the double purpose of protecting their bodies and catching the fly. They go about, and then the tsetse-fly settles on them and is caught. In one year they caught something like 51,000 tsetse-flies in the Island of Principe. That was *Glossina palpalis*.

3082. (Sir William Leishman.) Is there within your recollection any instance in which the movement of big game has been followed by a corresponding movement of the fly?—No, I cannot say that that has ever come within my experience. I have never been in a region of Africa where the big game became more abundant than previously, and where, therefore, there was the possibility of bringing fly with it. I have always been in regions where big game was lessening in numbers.

3083. You must, in your experience, have frequently re-visited a district or country which you had shot over the year before. Have you, in your recollection any facts with regard to the number of tsetse-flies in such districts revisited, compared with the number on your first visit?—As regards North Nyasa, which in 1889 swarmed with buffalo to a really remarkable degree (I have seen herds of 300 to 400, at a guess, at a time), that district was considered bad with tsetse-fly. I came back to that region several times afterwards, and as late as the beginning of 1896, when the fly was supposed to have much lessened in numbers. According to the African Lakes Company there were numerous cattle maintained for draught and dairy purposes where they could not previously keep them. But the remarkable thing about North Nyasa was in those days of the '80's and '90's, the co-existence of tsetse-fly, buffalo, and herds of splendid humped domestic cattle (the humped or zebra breed stands the fly and tick better than the straight-backed Egyptian ox). But these cattle were never allowed to stray beyond the banana groves, as their native owners asserted that the tsetse (*morsitans*) did not enter the banana plantations.

3084. Did you yourself notice a reduction in the numbers?—I cannot say that I did. I cannot say that I ever noticed it one way or the other, either as existing or being diminished. The buffalo took very little notice of human beings at that time. I wandered about amongst them for the interest of the thing, but I never noticed the fly and I was never bitten by it in that district.

3085. Do you think tsetse-flies bite at night?—I cannot say from personal experience, but I have seen it so stated by travellers whom I should credit.

3086. You yourself have not been bitten by them in the night?—No, only in the daytime.

3087. It has been suggested to us that the difficulty of driving game out of an experimental area might be that we could not get rid of the bush-pig. What do you say about that?—The bush-pig is certainly very persistent. It is a principally nocturnal beast and certainly seems to persist where most of the bigger beasts are driven away.

3088. Would the same apply to wart-hog?—It would apply less to the wart-hog, which is, I should say, more diurnal in its habits.

3089. And the duiker?—I should think the duiker would persist long after; in fact we know by actual experience that it has remained long after the big game disappeared. May I interpolate a remark about it: one of the countries I have been interested in most of recent years is Liberia. I went out twice to assist the Liberian Government in its frontier negotiations, and I went a considerable distance into the interior for that purpose. I passed through the edge of the Gora Forest, a very interesting region. I have subsequently rather promoted the exploration of it, and therefore have read many reports which have not been made public with regard to it. According to native tradition, the forest, which to the zoologist is remarkable for the number of duikers, several species of which it contains, was depopulated a long while ago, some say about 100 years, by a terrible epidemic of sleeping sickness, the remains of which are distinctly alluded to by the British and German missionaries of the forties of the last century. The sleeping sickness still lingers on the borders of the Gora Forest, and I have actually known negroes who have perished from it quite recently. That district, and other parts of Liberia, although the big game, with the exception of the elephant and a very few tragelaphs, is almost exterminated, nevertheless are celebrated for the number and variety of its duiker antelopes. They seem to have persisted where, owing to the possession of guns, the natives have done much to exterminate other big game.

3090. Are you familiar with the country where Sir David Bruce has been working recently in Nyasaland?—I have only been through south-west Nyasaland on two occasions, I think, passing rapidly through. I have never resided there as I have in other parts of Nyasaland.

3091. You could not advise us then as to whether that would be a suitable place for the suggested experiment of enclosing an area?—I should say that it would be the most suitable part of Nyasaland, because you would interfere least there with such industries as might object to areas being enclosed, cotton planting and so forth, and I think you would interfere least with the natives. It is not so thickly populated as some other parts, or I think it is not.

3092. Do you think it would be practicable to attempt to keep big game out by a fence?—Yes, I should think it would.

3093. What class of fence do you think it would be best to construct if we had to fence in an area of, say, 10 miles by 10 miles?—I should think you could not do better than get the natives to make the sort of fence that they used to make for hunting purposes, a type of fence that was very common in Nyasaland in my day. It is chiefly done by cutting down trees to a certain height and bending the branches down and weaving other branches in. What used to strike me in those days was the dread that most wild animals had of anything in the form of a fence, so that even an elephant who could crash through it without the least trouble would skirt it rather than attempt to get through. To the very suspicious big game of Africa anything out of the normal in the bending down of a branch suggests a trap. They become so suspicious about traps and pitfalls that even a fence that any determined man could push through would be enough to keep out big game.

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3094. That would not be a very expensive proceeding, I suppose?—No, it would not be a very expensive proceeding. Central Angoniland is rather well timbered with not very tall trees. It is a country of considerable vegetation.

3095. Is it a fairly dense forest country?—No, except in the hollows of the stream valleys, and so on. There is not much dense forest there from my recollection.

3096. But would not a dense forest be necessary for the fence?—No. A great deal of that part of Africa is thinly wooded, but the trees are not so far from each other, but that it is possible to make a fence from local materials.

3097. (Sir Mackenzie Chalmers.) Do you happen to know the Domira Road, which Sir David Bruce suggested was a good place for an experiment?—No, I do not think I do. I know that there is a place or region called Domira from which a steamer was named, but that is about all.

3098. You cannot tell us the nature of the country?—Is it near the Bua River?

3099. From the middle of the lake shore to the Northern Rhodesia frontier it is four treks?—That is in the direction of the Bua. I have been up into that country.

3100. Is that country very suitable for the experiment?—Yes, I should think very suitable.

3101. From your own observations and from what you have heard and read, where do you regard sleeping sickness as having originated?—It seems first of all to be mentioned by Arab writers as a disease of Senegambia, and a celebrated Mandingo king is supposed to have died from it. It is first mentioned with something like credibility by French writers in connection with Senegambia nearly a hundred years ago.

3102. That is on the West Coast?—That is on the West Coast.

3103. Do you regard it as having spread eastwards from the West Coast by human intercourse?—Yes. Missionaries say that the tribes of Central Congo have traditions to the effect that this disease has occurred in cycles, and they trace it back several hundred years in its appearance on the Congo. If there is any reliability to be placed on these very vague Arab references, it would seem to show that it existed at any rate in the 12th century. The Mandingo king died in the 12th century.

3104. It is supposed that it runs in cycles and sweeps over the population, and then it is supposed that the people who are left are more or less immune and that then a susceptible generation again arises?—I cannot express an opinion on that, but in Liberia there was traditionally a terrible epidemic of sleeping sickness a hundred years ago, which absolutely killed out the indigenous population in much of Western Liberia. Since then it has never been a serious disease in Liberia, but from that time apparently every year a few cases occur. You do not hear of it east of Cape Palmas, east of the frontier of Liberia. You do not hear of it on the Ivory Coast and still less do you hear of it on the Gold Coast.

3105. The mortality statistics of Liberia are not reliable, are they?—No, they are most unreliable.

3106. With regard to the Gora Forest, is that a *palpalis* or *morsitans* country?—I should say *palpalis*.

3107. As regards clearing round villages, I suppose that that depends very much on the character of the vegetation round. For instance, you may find a village close to the thick jungle?—Yes.

3108. Then they only clear a piece just in front of the village?—Quite so. Many of the villages in the most backward parts of Africa (and it is in the most backward parts that the fly on the whole seems to be the most abundant) prefer to hide away their villages in the bush or in the forest, and before the Pax Britannica they had not welcomed the idea of clearing away the forest or bush, because it gave such scope to their enemies.

3109. It is almost impossible to clear thick jungle, is it not, on account of the expense?—It is grievously

expensive in forested West Africa and in many parts of the Congo.

3110. You would not find *morsitans* there, would you?—No; it would be more *palpalis* or a variety of *palpalis*.

3111. I gather that you do not yourself very strongly connect the presence of big game with the spread of sleeping sickness to man?—No, from such an amount of experience as I have had I do not. I would not hesitate to condemn the big game if I was sure in my own mind that there was no other host of the trypanosome, but I have no reason to think that the large mammals are more the hosts of the trypanosomes than the smaller.

3112. For instance, where sleeping sickness has been introduced as it was in Uganda, do you think it possible that man coming from some infected neighbourhood was the original carrier of the disease, and that through infected persons being bitten by tsetse-flies the animals have now become infected with the trypanosome and carry a breed of trypanosome dangerous to man?—I should be disposed to think so, and I should like to give an instance: when I first knew the Sesse Archipelago in the Victoria Nyanza it was a very well populated region, all except the smallest of the islands, and those had been peopled to a very large extent by the water-loving tragelaph, the so-called *situtunga*. These *situtunga* were not much interfered with by the natives, because they were a totem or something of the kind. At any rate this very creature which is now thought to be one of the worst with regard to the trypanosome existed in very large numbers almost within sight of the abundant population of the Sesse Islands. In 1896, or some such year, there came along two British officers with a native following in a steam launch or a dhow, and they set to work and had a battue of these tragelaphs and destroyed, according to the native story, hundreds of them. With regard to my own testimony, all I can say is that I visited the islands and saw quantities of skulls and skeletons lying in all directions. There was no case of sleeping sickness in those days, but in 1902 sleeping sickness was introduced into the Sesse Islands, and now, I believe, there is practically no human inhabitant in the Archipelago. Those who did not die were removed. A point I would like to enforce is that although the tragelaph has since been declared to be one of the chief hosts of the trypanosome (I think its Uganda name is *nakong*, but I forget) and a *Glossina*, possibly *palpalis* existed in 1900 in the Sesse Archipelago, yet sleeping sickness was unknown and cattle were kept. You had the agent in the *Glossina* which I have seen myself on the islands and you had the tragelaph, and you had a large human population, but not a case of sleeping sickness was known.

3113. Do you know the islands on the Victoria Nyanza well? Have you been much among the islands?—Yes, I know them well on the whole.

3114. Do you know of any isolated island where you find *situtunga* and fly which could be cleared as an experiment?—If there are any *situtunga* left you would find them on one of the smaller Sesse Islands.

3115. Are those islands pretty well isolated?—Yes, they are remarkably isolated.

3116. Would it be rather farther from one island to another than *situtunga* could swim?—Yes, I should think you would find islands almost beyond their swimming powers.

3117. Would it be feasible to drive out or exterminate the *situtunga* on one of those islands? Would it be a difficult operation?—It would not be at all difficult if there were any left, but I very much doubt whether there are any left, because I am afraid that the work was done too thoroughly by the first pioneers. There might be some. There are an abundance of *situtunga* in other parts. I used to have them caught for me by natives to keep them alive.

3118. For experimental purposes we want an area in which there are both fly and *situtunga*?—Yes. There is just the possibility that you might find them still on the Sesse Islands as they were there once.

3119. Do you think you could find an island where a fresh *situtunga* would not be likely to swim to?—I would not be very positive on that score. You must

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either suppose that there have been very recent changes in the level of the lake, and that the Sesse Islands were once knit up more with the mainland, or you must credit the *situtunga* with very considerable swimming powers. Some of those outlying islands are so far removed from the central group that the natives do not like to venture so far in canoes on account of the roughness of wind and wave. The most comfortable journey I had to them was when I went on the German steamer. There was no British vessel large enough in those days. Sir Clement Hill went in that direction, and very nearly met with disaster.

3120. (*Dr. Chalmers Mitchell.*) We know, Sir Harry, that you have added very greatly to the knowledge of the geographical distribution of animals in Africa, and that you have always taken a great interest in them. —I have taken a great interest in them.

3121. Speaking of the Island of Principe, you have been there?—Yes, in 1883.

3122. Would you tell me about how big it is?—Roughly speaking, it is considerably under 100 square miles.

3123. Roughly, about the size of the Isle of Wight?—It is smaller, if I remember rightly. It is about 78 square miles, or not much more.

3124. Where is it situated?—It is situated between Fernando Po and San Thomé.

3125. Is it rather isolated?—Yes, it is rather isolated.

3126. Could you tell me what indigenous mammals there are there?—I should be inclined to say none except bats.

3127. I know that Dr. Russel Wallace says none, but since then Mr. Thomas has found some bats, and one or two small things. Anyhow, am I right in supposing that there is nothing that you would call big game there?—Absolutely nothing.

3128. No giraffe?—No, but there are some feral pigs, wild pigs, which have descended from pigs introduced by the Portuguese, and I was told at the time of my visit that there were some feral cattle.

3129. Domestic cattle which had been introduced?—Yes.

3130. No antelope?—No.

3131. No buffalo?—No.

3132. No elephant?—No.

3133. None of the things we usually know as wild game?—No. The island was thoroughly investigated by Frank Newton, whom I knew personally, a Portuguese naturalist with an English name. I once travelled with him in West Africa. The gist of his report to the Portuguese Government I think was that there were no indigenous mammals in Principe except bats.

3134. Would there in your opinion be as complete an absence of wild mammals as we should be likely to get in an experiment?—I think so, unless you chose the little island of Annabon, which is still further away from the coast.

3135. If we took a bit of land now occupied by animals, do you think that we could drive out the big game as completely as they are absent in Principe?—No, most certainly not.

3136. Would you tell the Committee whether you know anything about the history of sleeping sickness in this experiment which has been made for us by nature? Is there any sleeping sickness in the island of Principe?—It was introduced into Principe (according to what I heard in 1883), about the middle of the 19th century, and was attributed to an infection arising from workers from Angola.

3137. Do you happen to know which sleeping sickness it is, or rather, which fly it is?—The Portuguese now say it is *palpalis*. I myself at the time knew nothing about the fly and never thought about it.

3138. Do you tell us that it is now pretty well identified as *palpalis*?—Yes. The story of the Portuguese, which is set forth very circumstantially, is that there was no such thing as tsetse-fly, locally called the Gaboon fly, on Principe till about the early years of the 19th century, say 1823. When the Portuguese introduced domestic cattle from the Gaboon to Principe they did it with the deliberate purpose of

making Principe a great place of call for provisioning ships. They noticed that the keeping of cattle on the mainland opposite was precarious owing to the attacks of a poisonous fly generally called the Gaboon fly. With these cattle they seemed to have introduced the Gaboon fly or tsetse-fly. Some years went by and then they brought labourers, virtually slaves, from Angola. Then there was an attack in the 'seventies some years afterwards of a terrible sleeping sickness disease, attributed to infection from the Angola people, but how it was conveyed nobody attempted to guess. For that and other reasons the island became almost derelict. People had a kind of horror of Principe, and when I landed there I was warned of the consequences that might occur. I found scarcely a white man on the island and the island virtually ruined. Years went by and the Portuguese forgot all this and decided on doing at Principe what they had done at San Thomé, that is, to make it prosperous by cocoa, quinine, and coffee. People came there for land and again they introduced labourers from Angola or from the Congo in later years and once again sleeping sickness and its ravages were such that they attracted the attention of the whole world. Then the new Government of Portugal sent out a Commission to make a thorough inquiry into the whole business, and they have reported that the mortality from sleeping sickness, owing to the measures that had been taken, has diminished 50 per cent.

3139. There were no game to kill off?—Absolutely none. The island had only a few types of bird, the reason being that the grey parrot had taken possession of the island and had developed a local variety of very much darker colour, with an almost purple instead of a red tail, and it was so savage that it drove away birds of prey, and the few Portuguese there said that it even interfered with domestic fowls. That is so interesting that I would ask you to look at the works of Newton, amongst others, and not rely on my memory.

3140. I may take it from you that here is a case of an island without big game into which man and his domestic animals introduced sleeping sickness, probably the Congo type, and probably the *palpalis* tsetse-fly?—Yes.

3141. And, notwithstanding the absence of big game, the sleeping sickness has obtained a foothold there and has become an enormous plague?—Yes.

3142. (*Mr. Rothschild.*) You mentioned, Sir Harry, that in one place where you said there were large numbers of tsetse-fly and practically no game, there were a number of large monkeys?—Did I say so this time?

3143. Yes.—That was with regard to Loango—chimpanzees and gorillas, and so on.

3144. (*Mr. Buxton.*) You said apes?—Yes, I specially referred to anthropoid apes, but, of course, monkeys also are singularly abundant in Loango.

3145. (*Mr. Rothschild.*) Is there not a monkey in Principe which was introduced and has since spread?—I know that one was introduced into San Thomé, but I have never heard of its introduction into Principe. Newton's writings would tell you precisely. I have seen the monkey in San Thomé which was introduced, because it has become quite abundant. I think it is one of the white-nosed monkeys.

3146. One witness told us in answer to a question that I put, that hitherto in laboratory experiments it had been impossible to inoculate baboons with either the cattle nagana or the human sleeping sickness.—Yes.

3147. In your experience have you ever seen tsetse-flies feeding on baboons to any large extent?—No, I have not.

3148. Mr. Selous mentioned that up in the Bahr-el-Ghazal he found that at a certain well, when the baboons came to drink, for some hours afterwards he could not let cattle or donkeys drink because of the large number of tsetse-fly which had been carried there?—I am afraid that I have no evidence to give on that point. I have never in any way associated baboons with tsetse-fly. I think rather I should say

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[Continued.]

that in the parts of Africa that I have been in outside the forest where there were baboons, there were probably few or no tsetse. I do not much associate tsetse with elevated or rocky regions.

3149. The Chairman asked you if the introduction of any insect-eating bird beyond a Guinea fowl would be of any use. Do you not think that those mound-builders which are spread all through the East, and which do a much greater amount of scratching than Guinea fowls, might be introduced?—It would be an extremely interesting experiment to try, I think; but I think the most practical thing would be, by explanation as well as by law-making, to interest all the white and black people you get into touch with in Africa in the question, and to ask them for the time being to spare the Guinea fowl. You would have no difficulty about the natives because they never seem to kill the Guinea fowl. I would beg the Europeans for five years, say, not to kill any more Guinea fowl in order to see whether they would not increase on a very large scale in the tsetse-infested regions. It would be quite as easy and less expensive than introducing mound-builders, and waiting a very long period till they were acclimatised and increased in numbers. I have tried myself introducing exotic creatures into Africa for six years, and I have met with very ill-success. I brought pea-fowl from India to Nyasaland, but they all died out. I have introduced Indian buffalo, and even brought parrots (Indian parrots), but they have all died out. Seeing how many species of Guinea fowl there are and how rapidly they increase, I should say that it would be better to give one's time to the Guinea fowl than to introduce some foreign bird.

3150. (Dr. Martin.) I have been told by some Portuguese authorities that there are no wild pigs in San Thomé. Do you know whether that is true?—I do not think there is such a thing as wild pig in San Thomé. There is practically no mammal unless they have a rat, which is indigenous.

3151. In Principe there are plenty of wild pigs?—Yes, feral pigs.

3152. They run wild upon the hills?—Yes.

3153. I am told that that is not the case in San Thomé.—I could not say myself. I have never heard about their existing in San Thomé, but I have heard of them in Principe. I have been to San Thomé. The island is, and was at that time even, very much more opened up and developed than Principe. I have never heard of any feral pigs or cattle in San Thomé.

3154. (Sir Stewart Stockman.) I understand that if you clear the bush for farming the game goes and at least the *morsitans* goes, and if you leave it derelict the *morsitans* may come back. Is that within your experience?—That is certainly my experience. I have noticed *morsitans* or at any rate a tsetse-fly in many regions where you could see by the surface of the soil that there had been anciently sweet potatoes and maize and other plantations.

3155. There is such an area in Uganda. Have you formed an idea how long it takes the fly to come back after clearing?—Three years would bring back the bush to a great extent, I should say; but I could not say how long the tsetse takes to make up its mind to return. I should think it would not be very long in returning. It seems to detest particularly the presence of human excreta. That is a fact that the natives bring to your attention in West and East Africa.

3156. If you attacked the problem of clearing you would suggest, I take it, that it would have to be done at least every three years?—Yes, the country would have to be kept perpetually cleared as though it were farm land.

3157. Just as if it was under plantation?—Yes.

3158. Would you say clear by bush fires, for instance?—Yes, by bush fires; much as I hate bush fires, from the ruin that they have brought to the African flora, they must be looked on as an ally.

3159. A necessary evil?—A necessary evil.

3160. (Professor Newstead.) You have stated in your letter to the Colonial Office dated 17th July, that you would willingly give us information regarding the interdependence or otherwise of game and tsetse-fly. You have answered questions on that point already.

Now I want to know whether you have in your long experience in Africa met with considerable numbers of the tsetse-fly in areas which were relatively free from game—or the converse?—Yes, I have. A part which I remember as being extraordinarily bad and trying was all along the lake-like portion of the Upper Shire, where I have lived for months together. Here there was practically no big game, but the tsetse-fly was, as I say, a positive nuisance. It was perpetually biting us; and it was the same also along the south-east coast of Lake Nyasa, where you have again no big game unless you count the hippopotami in the water.

3161. Do you mean that district which lies between Lake Malombe and the lake itself?—Yes.

3162. About when was that?—Ranging from 1889 to 1906.

3163. You are aware that there is a fair amount of game present in that region now, or that there was?—There was not in my day. There might be, say, three miles back from the river, or four, but not even there have I noticed much big game as compared with the Shire Islands of West Nyasaland.

3164. You know the district south of the lake fairly well, I take it?—Yes.

3165. Could you give us any information with regard to the relative abundance of birds on the south-western shore of the lake and that region which lies immediately south of Malombe? Do you consider that there is any marked difference between the number of birds, not actual individual species?—I should think so. The birds were perhaps less prominent to the eye—less observable. The region that you first mentioned between Malombe and Lake Nyasa was so full of water-birds that it left an indelible impression on my memory. It was there that I made most of my bird studies and pictures. I found enormous numbers of white heron and pelicans and all the water birds common in Africa, and rollers and bee-eaters and so on.

3166-7. The avi-fauna south of the lake would be about the same, I take it?—I am speaking of that region. I have not noticed so many birds along the south-west coast because it is high and rocky in most places, and birds do not care about that so much as shallow riverine flat places.

3168. We have been told by a previous witness, Sir David Bruce, that in the *morsitans* areas, referring to Nyasaland as a whole, there are very few birds and that they are the kind of birds which would not lend themselves to being fed upon by tsetse-flies. I think I am right in saying (the Chairman will correct me if I am wrong) that Sir David Bruce made the remark that in the Domira Bay region there were very few birds indeed; in fact I think he used the words "a few birds of prey or hawks," or something of the kind. I want to gather from you whether there is a scarcity of birds in that particular region or not?—In south-west Nyasaland?

3169. Yes.—I should say that as compared with valley of the Shire, both Upper and Lower, and some of the flatter parts, the birds in south-west Nyasaland are much less abundant. The fact is that I know that part not so well as the others, because there was not so much to attract me as a collector. I used to send Mr. Whyte and others to places like that. Their collections from that district are very poor and those from Central Angoniland and south-west Nyasaland almost non-existent.

3170. May we conclude that birds are relatively scarce in the Domira Bay district?—Yes. By that I mean south-west Nyasaland. I would not say that they were very scarce in the valley of the Bua River. They were not in my day.

3171. Then again, Sir Harry, you refer in the same report to birds as being destructive to the tsetse-flies.—Yes.

3172. You say that they are probably the only serious foes of tsetse-flies and of other noxious insects. Have you made in the course of your long experience post-mortem examinations of birds which have fallen to your gun and kept records of those?—Yes; I have done it largely through assistants; Mr. Whyte, who worked with me for six or more years in Nyasaland

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[Continued.]

and was afterwards in Uganda, used to open the crops of almost all the birds he killed and he noticed what they fed on, and he drew my attention very often to certain birds having tsetse-fly in their crops. I have seen them hawking after tsetse.

3173. Have you actually found the remains of tsetse-flies in their crops?—Yes, or Mr. Whyte has if I have not, and I quite rely on his reports. Mr. Dogget, who was also with me in Uganda, several times found tsetse in the crops of insect-eating birds both in Kavirondo and in West Uganda.

3174. In your book or work "George Grenfell and the Congo" you state on page 552 that the small white heron or Bubulcus and the squacco heron which occur in Central Africa, live mainly on insects, and that these birds frequent herds of cattle or buffalo in order to pick off flies. Do you think it is possible that these two birds, the white heron and the squacco heron, were actually engaged in picking off ticks rather than tsetse-flies?—Well, my impression would be that they would pick off ticks, but would even more readily snap at flies. In Egypt I saw the small white heron obviously snapping at flies on the buffalo.

3175. But is not the "small heron-like bird" referred to, known to be a destroyer of ticks? It is called the tick bird in some parts of South Africa, I believe?—It probably feeds very much on ticks, but I should think that it snaps equally eagerly at flies. I should think that many of those smaller heron do not care about fish at all. If they are down at the water side it is more for the water insect and the flies than the fish. I have seen a smaller heron (I do not know its specific or even its generic name) which is more related to the green heron as they are sometimes called. I have seen it standing motionless under the mangrove roots and then snapping suddenly at flies settling on the roots.

3176. The squacco heron is an aquatic bird more or less?—Yes.

3177. You would not expect to find it in such parts or in such countries as *Glossina morsitans* is known to frequent, would you?—I should think it would be very often in the country of the *Glossina morsitans*. It is more a bush-haunting fly. My own experience is that although the *Glossina* dislikes exposing itself to the breeze by crossing water, yet I have seen it by the river side over and over again almost without number just where I have seen these birds, and I have seen bee-eaters apparently hawking after the tsetse-fly—also by the side of a river.

3178. Do you consider the common drongo of British Central Africa, *Dicrurus afer* (it rejoices in the possession of many synonyms) and another insectivorous bird which belongs to the genus *Prionops* as being likely to feed upon Diptera including tsetse-flies to a greater extent than the guinea fowl?—Do you mean as regards searching for larvæ?

3179. No, as regards the fly itself, the adult fly?—I should think it pursued the adult fly and is far more successful in its pursuit than the guinea fowl is in picking up and devouring the larvæ.

3180. Such birds as I have referred to, you would agree with me are constantly in attendance during the grass fires and they snap up almost every insect which endeavours to escape from the approaching flames?—Yes.

3181. You consider, therefore, I take it, that these two birds, and many other insectivorous birds which frequent grass fires, would play a much more important part in checking the spread of or in other ways keeping tsetse-flies down, than such birds as the guinea fowl, which could prey only upon them by searching for their pupæ or larvæ?—I am so much impressed by the French report about guinea fowl that I have assigned them in my own mind an equally important place of late. The advantage of enlisting the services of insectivorous birds is that there is no temptation to a local man, black, white or yellow, to kill them for food, but the guinea fowl is a great temptation. It is the most eatable bird in Africa. If we cannot succeed in enlisting public opinion on behalf of the guinea fowl, perhaps the best course would be to promote, as far as possible, the

multiplication of insect-eating birds other than guinea fowl.

3182. If guinea fowl fed upon the pupæ of *Glossina morsitans* they would surely also feed upon the pupæ of other insects?—Yes.

3183. Including Diptera and Lepidoptera?—Yes.

3184. Have you ever found the pupæ of other insects in their crops?—Yes, I have found the remains of beetles in their crops.

3185. But the pupæ of Diptera or Lepidoptera?—I cannot answer that. My research did not go as far as that in those days. I have found guinea fowl mainly insect-eating as compared with the francolin, which seems to go in more for seeds. I have examined their crops.

3186. Is it not a fact that the guinea fowl in Nyasaland during the dry season feed, to a very great extent indeed, upon the small corns of a liliaceous plant, which occur in great profusion in more or less park country?—Actually liliaceous, or one of the crinums?

3187. I think it is liliaceous, but I am not sure about it.—I have no experience. I would not deny the possibility. My only experience has been in finding that they seem to be mainly insect feeders.

3188. During the dry season you in travelling have, of course, observed their scratchings all about the country?—Yes.

3189. It strikes one very much as one travels through the forest. Have you ever found that they have sought for insects or for food of any kind by scratching away the earth immediately at the foot of trees?—The tame ones I have seen in Africa scratch a good deal. I do not know particularly about the foot of trees. They seem to be perpetually engaged in scratching and searching for insects in the soil. One point that I used to notice about them at Zomba, where I kept them, was this: that Mr. Whyte, who was with me for years, always referred to them as excellent gardeners. He pointed out that they did little or no harm to the garden, but were perpetually watching for grubs and insects.

3190. Would you not consider it rather a hardship to protect guinea fowl during, say, the dry season, for instance, but not at any other season, because, at any rate, in central Africa, or that portion of it which I know a little about, it seems to me that the European has to subsist very largely upon such birds? As a matter of fact, they supply the principal article of meat diet. People live, to a very large extent, on the flesh of the guinea fowl, which is most excellent, so that I feel it would be a hardship to Englishmen going out there if they were prevented from shooting these birds for the purpose of subsistence?—I think that the hardship would be trifling compared with the possibility of good that the protection might effect, and I think that the people, black, white, and yellow, in Africa must be prepared to make some sacrifices if we are to conquer in this vital struggle against germ diseases. I am sure that, as a rule, nobody sufficiently depends on the guinea fowl to be too much circumscribed in diet if he is asked not to shoot them. Speaking from my own experience, I have eaten guinea fowl flesh relatively a small number of times, and I had no reason in those days to abstain. The natives seldom touch it themselves.

3191. But before protecting these birds would you not advise that a thorough investigation should be made into the nature and extent of the food that they eat in a state of nature?—Yes; I think it quite the best plan to establish scientifically without any doubt the services that they are alleged to render. I quite agree with that.

3192. (Dr. Balfour.) Are there any guinea fowl in the island of Principe?—I do not think so.

3193. If there had been I suppose they would have kept the tsetse-fly from multiplying, if your views are correct?—If the French report I have referred to so often is true, they would have had some effect possibly, but I do not recall any indigenous guinea fowl in Principe.

3194. What distance is Principe from the mainland?—About 200 miles.

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3195. So far?—Yes.

3196. Do you know if there is sleeping sickness on the portion of the mainland opposite Principe?—I do not think there is to-day, but there has been at one time or another.

3197. Is that where the infection came from, or was it from the labourers imported from Angola?—I should say entirely from the direction of Angola and the Congo. Principe was Portuguese and the Gaboon was French, and there has been very little intercommunication between the two since the French took possession.

3198. The tsetse-fly was probably transported over 200 miles, you think?—Gaboon is more than 200 miles away—300 or 400.

3199. It must have been transported that distance?—Yes.

3200. It did not exist originally at Principe?—According to Portuguese traditions, there was no such fly until the Gaboon cattle came.

3201. Do you know anything about the artificial transportation of tsetse-fly on the mainland of Africa by steamer, train, native boat, or caravan?—I have noticed occasionally that tsetse, which appeared on a steamer or boat going up the Zambesi, was rather anxious not to leave it, because it hates to trust itself, as I have said, to air currents, and in that way tsetse may have been carried up and down the water.

3202. Considerable distances?—Yes, and George Grenfell notes how tsetse were found in his canoe where the Congo was, perhaps, 10 miles broad.

3203. He was in the middle of the river?—Yes.

3204. They do not just accompany a canoe through a fly belt, and leave it as it leaves the fly belt?—Would they follow a considerable distance?—I think they would follow the canoe till the canoe came within easy distance of the shore, and then leave it. They have, possibly, been blown out to sea as it were, in spite of all their precautions, and settled on the canoe as the only refuge.

3205. Do you think it possible that areas in South Africa, the Transvaal, and so on, which have been cleared of tsetse-fly may become re-infected, owing to greater travelling facilities?—I think so.

3206. One witness said that Acera had become re-infected in that way.—I think it quite possible.

3207. It is a danger that has to be considered, is it not?—I should think it very likely that the quite recent coming and going between Northern Rhodesia and Nyasaland proper is one of the reasons why trypanosomiasis may have been introduced.

3208. I think there are no tsetse-flies in Madagascar?—I have never heard of any being there, and it is a great cattle-breeding country. I have never been there, but I do not think it is noted for tsetse.

3209. There is a small area in Arabia near Aden where there is tsetse-fly?—Yes.

3210. What is your explanation of fly being there?—I have never thought of it hitherto, but I can only imagine that it must be possible to convey it actually on the bodies of cattle. It has been so for centuries and centuries, and cattle have been conveyed from Arabia to Aden.

3211. You do not think that at one time the continents were in communication and—?—That this is a vestige?

3212. Yes.—Under the circumstances it is more probable that it has been introduced, but if there is any truth in the latest discoveries, it is declared that they have found in a fossil condition a true *Glossina* in the south-eastern States of North America.

3213. Yes, I remember.—That opens up even wider possibilities.

3214. (Mr. Buxton.) With regard to the island of Principe, sleeping sickness is present there, but there is no big game?—That is so.

3215. Have you formed any opinion about what it is that serves as a reservoir there?—I can only imagine that it is human beings. The fly was introduced first of all with the cattle, and then they found natives of Angola with trypanosomes in their blood and they transferred the trypanosomes to other negroes not of

Angola and to whites in the middle of the 19th century, say, and caused the first terror of sleeping sickness. The island being almost derelict, no more was heard of it. Then more human beings came, and more natives of Angola and the Congo, and there was the tsetse as the mediary of intercommunication.

3216. Has there been a serious epidemic there?—Yes, it has temporarily ruined the fortunes of the island.

3217. Recording your experience of Angola in 1882, you said that the half-castes were more liable to disease than the pure natives, I think?—I did not intend to convey that they were more liable, but being persons of note, you heard of it much more in connection with half-castes than in connection with natives. There were one or two celebrated half-caste mistresses of Portuguese officials, supposed in local opinion to be beautiful women, who died of sleeping sickness, and this created a great clamour. I hesitated, I suppose, from motives of delicacy to mention that. The average Portuguese would not record the death of hundreds or thousands of negroes, but he would record the fact that the officials' ladies had died of this mysterious disease, and it is a story like that that attracts one's attention.

3218. (Mr. Read.) With regard to birds, your criticisms are directed mainly against the destruction of the Guinea fowl. You do not allege that there is any great destruction of birds for plumage purposes, do you?—Yes, I do. The Guinea fowl is a minor question in this respect. I understand the objections to preserving the Guinea fowl, though I think they might be expected to give way, but what I think should be put a stop to immediately if possible by legislation, is the destruction of any insect-eating bird not palpably good for food. Plumage should be no excuse for killing insect-eating birds.

3219. I knew you were going to speak about birds, and so I asked Mr. Woosnam, the Game Warden of East Africa, and he assures me that, with the exception of ostrich feathers, there is no exportation whatever from Uganda and the East Africa Protectorate of plumage. This afternoon I met Major Pearce, who has been administering the Government of Nyasaland, and asked him, and he assured me that there was no exportation of plumage from Nyasaland. I have seen myself a despatch from Zanzibar, which states that there is no exportation from there.—There may be no official exportation in the sense that possibly plumage traders have no specific agents there as on the Congo, in Senegambia, and so on, but a good many beautiful plumage insect-eating birds are killed by Europeans and sent home privately as presents to friends in England.

3220. Mr. Woosnam was strong on the point, and was confident that there was no smuggling.—They would be sent home by parcels post, and unless that is checked, they pass through.

3221. You know that in most game ordinances birds are scheduled, and under Schedule 1 the vulture and other birds receive absolute protection?—Yes.

3222. Under Schedule 3, four specimens of the marabout stork and the egret can be killed?—Yes.

3223. We have just decided to put those birds into Schedule 1, to protect them absolutely.—I reviewed a book a little while ago, written by a woman, of her experience in Northern Rhodesia, and she speaks half regretfully of the number of egrets that her husband killed, and she feels a little compunction about it. I thought this extraordinary, because I believe the South African Company have regulations covering that. I asked their secretary, Mr. Brodie, and he said yes, either it was forbidden or the number was limited to four; but here was the case of a man who snapped his fingers at regulations, and so far did not realise he was erring that he allowed his wife to publish news of the number he had killed.

3224. What territory was that in?—Northern Rhodesia.

3225. Do you mean Northern Rhodesia?—Yes, north of the Zambesi. The authoress is Mrs. Cecil Porch.

3226. That is under the Chartered Company, and we have no control.—I know.

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3227. In the places I refer to only four are allowed to be shot.—I was speaking just now of my own recent experience. I have often remonstrated with Europeans myself in Nyasaland and Uganda and elsewhere as to the killing of birds when they were only sending the plumage home privately—not trading in it.

3228. (Chairman.) Have you seen much nagana among cattle?—Not to a very great extent. My own personal experience has been in Nyasaland.

3229. (Sir Mackenzie Chalmers.) Mr. Buxton referred to an epidemic in Principe.—Yes.

3230. At the same time was there an epidemic of nagana among the cattle? Was the epidemic of nagana among cattle contemporaneous with the epidemic in human beings?—I never heard much about cattle dying, although the Portuguese say that the Gaboon fly came with the cattle; it is rather curious now you mention it that I cannot find any evidence that the cattle suffered particularly from association with the Gaboon fly.

3231. Was Principe a place intended for breeding?—It was intended to be used as a depôt, but when sleeping sickness and other troubles came the cattle became feral; they simply wandered away into the woods. Whether they are still there I cannot say.

3232. Is there a tsetse-fly in Southern Nigeria?—Yes, I think there is.

3233. *Palpalis*?—Yes. They might have *fusca* too.

3234. I suppose it is impossible to clear a mangrove swamp?—The villages are not necessarily close to the mangrove swamps.

3235. They are sometimes in them, are they not?—No. A mangrove swamp is an extremely unpleasant place to inhabit even for a black man. There might be mangroves in front and you might walk 200 yards or half a mile before you came to a village, but you would find quite a different soil and a different class of vegetation. It would not be mangrove that would be so much the obstacle, as the dense bush and palms and thorns.

The witness withdrew.

Subsequent to his examination Sir Harry Johnston sent in the following additional evidence:—

I.—I should like to add these few remarks to the replies I gave to Mr. Read's questions. His examination came at the very end of the sitting and I was too hurried to give careful consideration to what he said. As regards the plumage trade and its effects on the numbers of insect-eating birds in Africa, I have not in my writings asserted that that trade had agents all over Africa or that it paid any particular attention to the colonies of Nyasaland, East Africa, and Uganda. I believe I drew attention to the fact that the real ravages caused by the trade in bird-skins had occurred in the more southern parts of South Africa, in Angola, the Gaboon, and French Congo, the western parts of Belgian Congo, all the rest of French West Africa, and perhaps the British colonies of the Gold Coast, Sierra Leone, and the Gambia. That is to say, in these regions numbers of natives and a few Europeans or half-castes are at work shooting birds of remarkable plumage in order to export their skins. The greater part of this trade is directed in the first place to Marseilles and Bordeaux. I have no doubt that the fourteen or fifteen birdskin-selling firms of London are correct when they assert they have no agents and no direct relations with any part of Africa, British or otherwise. They probably receive their supplies from the French ports and Paris, Antwerp and Amsterdam, perhaps also from Trieste. But inasmuch as they and other plumage traders in Great Britain act as the means of supply of the plumage of insect-eating birds to millions of women and a few men, they are indirectly concerned with a traffic which in my opinion is altogether disastrous to agriculture and to the checking of germ-diseases in Africa. Mr. Read stated that the customs returns of East Africa and Nyasaland show no indication of the exportation of bird-skins. That may be so, but I have reason to know that many skins (insect-eating birds of attractive plumage being mostly small in size) are sent from those countries through the parcels or even the letter post. Birds are but seldom shot and skinned for purposes of gain,

but in order to make presents to friends in England, and also partly from the sheer love of shooting and "collecting" some object of great beauty.

Why, I ask, should any useful or harmless bird be killed at all except where it is needed for food? The marabout stork, for example, is a much needed scavenger and has little or no records of blame for destroying livestock. Yet it is killed, partly because it is an easy mark for a rifle, and partly because under the tail are the few delicate, filmy plumes which are much valued by milliners. Egrets or any form of white heron subsist much more on insects than on fish; in some species entirely on insects. Why should a single white heron be killed? Not even a savage would think of eating the bitter flesh. The excuse that their plumes are a decorative article of wear is no excuse at all.

I would therefore suggest that the Colonial Office, in regard to all the Crown colonies and protectorates in Africa, and the Foreign Office in regard to the Sudan, should seek the expert advice of the bird department of the Natural History Museum, and ask that a list of names might be furnished of all rare, beautiful, or remarkable insect-eating birds still found in Africa, and that this list should be sent out to the Governors of the respective countries, accompanied by instructions or suggestions that the aforesaid birds should be added to the lists of creatures under the local game law which it is forbidden to kill. But at the same time, inasmuch as it is very difficult to carry out literally such penal legislation, I would further propose that the Governors should be instructed to make a frank appeal to both the European settlers and officials and the natives of the country they administer to respect these forms of bird life which are useless for food and perfectly harmless in their relations with man; the reason given being the important part that such birds play in keeping down insect pests. As a concrete instance I would suggest that the crowned crane throughout all Africa under British influence should be a privileged bird, because it subsists chiefly on locusts, grasshoppers, beetles, and other more or less harmful insects, yet does not interfere with native or European crops. Not long ago the crowned cranes were abundant in the landscape of the Egyptian Sudan, but a dead set has been made at them by tourists of all degree, partly because they are not shy of men and are so beautiful in appearance. I was informed not long ago in Germany that the Prince of Liechtenberg in 1909 and 1910 killed or caused to be killed over a thousand crowned cranes in the Anglo-Egyptian Sudan in order that he might select 600 of the best heads of this beautiful bird, with its golden crest, white cheeks, and black top-knot, to be set up on brackets for the ornamentation of the immense smoking-room in his palace. Before the white man came on the scene the native left unharmed all such birds as I describe, or even revered them for their beauty.

II.—Looking at the whole question of sleeping sickness through the big end of a mental telescope, and refreshing my memory by reading up several works by Portuguese and other writers, including those of the late Dr. Héli Chatelain (a Swiss medical missionary who resided and worked for many years—the 'eighties and 'nineties, especially—in Angola), I view the question thus:—

Though sleeping sickness is first recorded in history as a disease afflicting parts of westernmost Africa (Senegambia), it seems by the beginning of the nineteenth century (possibly much earlier) to have established a nidus in Angola, from which it has never since been chased. In fact, reading the works of nineteenth century writers (especially those of Dr. Chatelain) one is able to recognise sleeping sickness as the standing plague of Western Congoland, and above all the northern half of Angola. It seems scarcely to extend its ravages to the south of the Benguela region or into the Nano highlands. Again and again it would seem as though this dreadful epidemic started on a fresh career from an Angolan basis and ravaged the Congo basin. The traditions of many of the peoples of

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Central Congoland are to the effect that sleeping sickness was known to their far-back ancestors and that it recurs at intervals as a devastating plague, always coming from the west. As it is comparatively a matter of no moment at the present day in westernmost Africa, it would seem that we might concentrate our attention on Angola as the great source of its recurrent devastations. I mean, if it could be stamped out of Angola it might in course of time be exterminated all over Africa. Now the part of Angola to which I allude (the northern half, and a region which I have myself visited somewhat in detail many years ago) is singularly devoid of big game, and in spite of sleeping

sickness is somewhat densely populated. Southern and south-western Angola which are comparatively free from this disease, are on the contrary—or were at the time of my long-ago journeys—one of the richest big game regions in the world, but were somewhat scantily populated, except in the Nano highlands (where big game is absent). I believe the prevailing tsetse of the northern half of Angola is a local variety or subspecies of *palpalis*. I observed tsetse frequently in Southern Angola, away from the highlands where they do not seem to exist, and they used in those days to transmit the nagana disease, so that possibly they were probably *morsitans*.

TWELFTH DAY.

Friday, 28th November 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

SIR MACKENZIE CHALMERS, K.C.B., C.S.I., (Chairman).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Dr. ANDREW BALFOUR, C.M.G.
Sir JOHN ROSE BRADFORD, K.C.M.G.
Mr. E. NORTH BUXTON.
Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODGE, Bart.
Dr. P. CHALMERS MITCHELL, F.R.S.
Mr. H. J. READ, C.M.G.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
Mr. A. C. C. PARKINSON (Secretary).

Mr. H. G. PLIMMER, M.R.C.S., F.R.S., F.L.S., F.Z.S., called and examined.

3236. (Chairman.) I think that we all in this room know your pathological work in connection with trypanosomes, and I believe you have paid particular attention to trypanosomiasis in man and animals?—Yes.

3237. Perhaps, before we go into the details which suggest themselves to us, you would kindly give us your opinion on one or two general points. We, as the Committee, are particularly investigating sleeping sickness in Nyasaland caused by or carried by *morsitans*. Have you come to any conclusion as to whether the disease carried by *morsitans* and produced by *T. brucei* or *T. rhodesiense* is the same disease as the disease caused by *T. gambiense* and carried by *palpalis*?—No. When the disease is caused by *Trypanosoma gambiense*, it does not run quite the same course. The latter is a more acute form apparently.

3238. Have you seen cases of both yourself?—Yes, I have seen cases of both—one case of *rhodesiense* and three of *gambiense*.

3239. Could you amplify your answer at all and say how far they differ and in what respects?—No, because my experience is so small.

3240. But you are familiar, no doubt, with the literature and with other people's work. Have you formed any opinion on that?—The only case of *rhodesiense* I have seen is one that was here in London. There was one case of *gambiense* which to my mind was like it, and that was the case of Captain Tulloch, who died here. That was a more acute form.

3241. Do you place any reliance on the fact that the trypanosome develops in a different way in the case of *morsitans* from the development in the case of *palpalis*?—The flies are very nearly alike.

3242. The development, as far as is known, is also similar?—We know very little of the development of *rhodesiense* in the fly, I think.

3243. Is there any way of accounting for the different forms the disease takes in the two countries?

—I think you will find the same thing in the experimental diseases evoked in animals. You get some in which it is acute, and some in which it is chronic.

3244. But there is no reason at present known to us why in Nyasaland the acute form should prevail, and in Uganda the chronic form?—In the West Coast it is still more chronic. The initial outbreak of any of these diseases is more acute, as a rule, than the later, and then it gradually becomes less and less acute, I should imagine.

3245. We have been told that up to date in Nyasaland there have only been 153 cases identified. Would you judge from that that there is not much danger of the disease suddenly fulminating and becoming epidemic?—I should think that at any moment, under the proper conditions, it might become epidemic.

3246. You do not think there is any inference of safety to be drawn from the fact that up to date the disease has been purely sporadic?—I think that it depends on the aggregation of people, and so on, very much. The population is very scattered, I gather.

3247. I do not know whether it is fair to ask you whether you have come to any conclusion as to whether the disease is spread in Nyasaland by the fly from man to man, or whether you think that it is spread from the trypanosome of the antelope to man?—I have no means of judging.

3248. Can you suggest any line of research which would throw light on that problem?—I think one has to accept very largely the opinion of the people who have worked at it out there, and all of them seem to be in favour of certain measures, of which you know.

3249. We have had a good many measures suggested to us. Which do you think is the most hopeful line in your judgment?—Most of the people who are working out there tell us that the only thing is the isolation of a certain area in which the disease could be studied under certain conditions.

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3250. It has been suggested that an area of, say, five miles on a certain road, which at present is full of antelope and fly, should be cleared. What do you think we should learn from an experiment of that kind?—I do not think we ever know what we shall learn from an experiment, otherwise it would not be an experiment.

3251. Do you think that there is any prospect of learning much from further entomological research?—No.

3252. If our funds are limited and money has to be spent, would you rather spend it on the experiment suggested by Sir David Bruce than on prosecuting vigorously any other sort of research?—Yes.

3253. I think you are one of the few people, are you not, who have treated the disease successfully?—Yes.

3254. Would you tell us about the treatment you think best?—I happen to be the person who carried out a large number of experiments, extending over many years, which were arranged and devised by a sub-committee of the Royal Society and we tried a very large number of drugs and chemical substances. It was known when the committee began to make its researches, that arsenic was the only drug which had at that time apparently had any influence over the disease. That was proved by Sir David Bruce as long ago as 1900. We tried various kinds of arsenic, and then the committee decided that we should try other things that were nearly related to arsenic chemically and we tried one of the nearest which was antimony. We found that the effects of antimony on animals were more marked and more definite and gave a larger percentage of cures than arsenic did. We used at first salts of antimony: the salts apparently are excreted very rapidly from the animal organism.

3255. Does that apply to all mammals?—It applies to all mammals. We tried the salts on human beings as well. They are so rapidly excreted that many of the trypanosomes escape the trypanocidal action of the antimony. We were getting to the end of a very large series of experiments and the thing was to find a form of antimony that would not get excreted so rapidly as the salts. In the horse, for instance: there is no antimony to be found in the horse three to four hours after the administration of a dose of a salt hypodermically. It is got rid of through the kidneys. Knowing that copper could be deposited in infinitely fine division electrolytically we tried the same method with antimony, and found that it could also be posited in an extremely fine state of division as absolutely pure antimony. We tried that under the skin and in the muscles and found that it caused very much less disturbance than the salts which occasioned necrotic changes in the tissues. Then we dared to put the solid stuff into the blood vessels, and found that by doing that no irritation was caused. The particles of metallic antimony were taken up by the leucocytes and were altered by them into some soluble form which we do not know anything about. We found that instead of its being excreted in a few hours, you could find traces of the antimony in the horse in the leucocytes for four days. No accident happened with the animals. Then it was tried on man. The Royal Society published in the spring a paper by Captain Ranken who worked with me for two years. The Egyptian army took it up, and he went up to the Lado. He has done now over 2,000 injections of metallic antimony into the vessels of humans without accident, so that from that point of view it is not dangerous as a method. The results from that I think have been better than the results from any other method of treatment which I tried during the years I worked for the committee of the Royal Society.

3256. We were told, I think, by one of the medical witnesses who came here that at present he was employing alternate treatment, arsenic and antimony?—There are a certain number of cases in which Captain Ranken has kept to antimony alone, and a certain number in which it has been given in conjunction with mercury, atoxyl and various other things which have been found to be of use.

3257. In your opinion is that treatment better than the single treatment?—I cannot quite tell yet, because it takes such a long time with these cases to know whether they are absolutely free or not. The difficulty with the treatment, it seems to me, is the question of the time at which it is begun. Captain Ranken has had to take any cases that were sent to him without picking them. The results to my mind, if you analyse them, show that if you get the cases in a fairly early stage you can cure them, but if you get them when the trypanosomes are in the cerebro-spinal fluid it is very much more difficult and there comes a time when apparently no treatment of any kind is of any avail. He has had the largest experience of this particular form of treatment of humans.

3258. You look on it, at any rate, in the chronic form, if taken early as a curative treatment?—Certainly. I have had cases under my own eyes that have been resistant to arsenic. The particular ones I am thinking of, the first cases in which antimony was used, had become resistant to arsenic and they were dying.

3259. Did they both respond to the antimony?—They both responded to the antimony and they are both living now.

3260. Do you think that any further research is required with regard to improving the form of administration or the particular kind of antimony or arsenic?—Yes. There is apparently a great deal of virtue in being able to give a form of antimony which does not get excreted very rapidly, and with regard to this particular form it takes a long time for the white blood corpuscles to turn the metal itself into some soluble form. It seems to be, as regards antimony, the best form.

3261. You do not think that much advance will be made on that?—Not as regards antimony. Better things may be found, no doubt.

3262. You think that research should continue on those lines?—Certainly, because as regards animals, you can cure 100 per cent. of rats suffering from nagana with antimony. We did, over a large series, cure practically 100 per cent.

3263. Did they recover their normal health and become perfectly well?—Yes. With larger animals the percentage was less and less, because we did not use the same trypanosome, and the animals were necessarily not of such a good type. With horses we used surra, because that is a much more fatal form in horses than nagana.

3264. Treatment hitherto has been no use, has it, in the acute cases, which they have had in Nyasaland?—This particular form of treatment has not been tried there. Sir David Bruce has had no chance of trying it.

3265. I think he has not treated any cases at all?—No, he has not treated any at all.

3266. I have had handed to me with regard to the Nyasaland type the Report of the Principal Medical Officer at Zomba: "Various treatments have in the past been given a trial:—Atoxyl, with immediate doses of Hydrarg. perchlor.; Soamin; Salvarsan; Tartar emetic; and Dye B.S." I do not know what that is. Is it a preparation of antimony?—No—an anilin.

3267. "In no case was there recovery, and in one or two instances in which some amelioration of the patient's condition was observed the improvement was merely of a transitory nature. Such slight improvement in symptoms is, however, occasionally noted in patients undergoing no treatment." They have not yet tried the pure metallic antimony apparently?—No.

3268. You have hopes from that treatment?—One can only judge from one's own experience of two cases.

3269. And from—?—And from Captain Ranken's work in the Lado which has been carried on for 18 months or a little more.

3270. Is that form of treatment sufficiently well known?—No, I do not think it is. The technique is a little difficult, but he has had no trouble out there.

3271. The difficulty of technique is the intravenous injection?—The technique is a little difficult and it has to be acquired.

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[Continued.]

3272. Are there any definite lines of research which you would suggest should be carried out?—This particular treatment is being carried out and it would be much better if it could be carried out on a still larger scale.

3273. You suggest that this form of treatment should be made known and tried extensively?—I think so, and I think that Captain Ranken who has had the most experience of it would say the same.

3274. Have you ever made a post-mortem, or do you know the result of post-mortems on sleeping sickness cases?—No. I have had many bottles of specimens of organs sent to me.

3275. What in your opinion is the actual cause of death, is it the mere mechanical multiplication of the trypanosomes, or is the action toxic?—Toxic. One uses this as a term of ignorance. It is not mechanical in the cases that I have examined.

3276. It is not mere mechanical clogging?—No, not like it is in the acute form in animals. I have not seen that in the human brain.

3277. Or spine?—No.

3278. What changes does the spine show?—You get varying amounts of degeneration. I should imagine that it is here often mechanical. You get cellular exudation round the vessels which exerts pressure, and you get atrophy of the nerve fibres and degeneration.

3279. Produced by what?—Partly pressure and partly "toxic," but we do not know what it means.

3280. Does that require further working out?—I think it all wants further working out. All our answers are in terms of incomplete knowledge.

3281. You say that you have done morphological work and experimental work on animals with *brucei*, *gambiense*, *evansi*, and *rhodesiense*?—Yes.

3282. With that group of trypanosomes?—Yes.

3283. Have you come to any conclusion as to whether the *brucei* which produces nagana in animals is one and the same with the *rhodesiense* which produces sleeping sickness in man?—The strains I have worked with were not the same.

3284. Can you distinguish them morphologically, do you think, at all?—I think so, without difficulty.

3285. Can you distinguish morphologically *rhodesiense* from *gambiense*?—I think I could with regard to the strains I have worked with myself, but I could not perhaps give guides.

3286. In the case of other people?—No.

3287. You think that you yourself would not have much doubt, if you made an examination, as to whether a man was infected with *gambiense* or *rhodesiense*?—I do not think I should have much doubt; I think they are different, at least the strains that I worked with. The only *rhodesiense* I have had is one which Dr. Daniels gave me; I kept it going some two years. I have carried it through a large number of animals. I can only judge with regard to one's own experience of these particular trypanosomes.

3288. Was the strain varied by passing from animal to animal, or did you find it constant?—All strains of trypanosomes vary enormously.

3289. As you pass them from animal to animal?—Yes, if you pass them through certain animals.

3290. Sir David Bruce told us that he had come to the conclusion that *rhodesiense* and *brucei* were one and the same?—I cannot agree with regard to the strains with which I have worked. They were certainly quite different.

3291. Another witness suggested to us (I do not know whether you have any opinion on the point) that *brucei* which gave nagana to susceptible animals might, under certain unknown conditions, change not in form but in effect, and become pathogenic to man?—I do not see any reason why it should not under certain conditions.

3292. There is much more to be learned?—Very much more.

3293. I do not know whether you can suggest any lines on which you think further research should proceed?—I personally think the morphology of these things is taken in much too definite a way, because you can produce, by passing any trypanosome that I have

worked with through various animals, things that are quite unlike the original trypanosome.

3294. In form?—In form.

3295. And possibly in effect?—By using certain chemical substances you can do away with the micronucleus of the trypanosome. You can alter its morphology so much by environment or artificial means, such as chemical substances, that it is extremely difficult to give any kind of formulation except a personal one. One knows what one has worked with, but to give any general kind of statement that would be satisfactory is very difficult.

3296. The same thing may happen in the larger laboratory of nature?—Yes, in a way it may. Another thing that is so difficult with regard to morphology is varying methods of preparation. If you want to compare trypanosomes they must be prepared uniformly. Many times I have had large numbers sent to me for an opinion to be given upon them, but it is very difficult because the methods of preparation differ from mine. You can only judge intimately about trypanosomes if you prepare them in your own way, because you can make them appear so different.

3297. (Sir John Bradford.) First of all I want to ask you a few questions based on some of the answers you have given to the Chairman. With reference to the case of Captain Tulloch, which has been mentioned to this Committee several times, I daresay you know that Captain Tulloch was under my care for a time, and so I am familiar with the case?—Yes, I know.

3298. Did you actually have specimens from Captain Tulloch's blood?—Yes.

3299. What was your opinion as to the identity of that trypanosome?—It was of the long form with a flagellum. In the specimens that I had there were never any short forms at all.

3300. For the purposes of this Committee could you give it a name?—No, but you saw them at the time, if I remember rightly.

3301. I am afraid I do not remember.—I think I showed you some of them.

3302. Yes, no doubt you did.—You might have said they were like nagana.

3303. That is one of the points I want to elicit.—They were quite like.

3304. I will put it in the form of another question. There was another patient of mine whose blood you have had many opportunities of observing who died from the typical disease?—Yes.

3305. He was one of the early cases. Now do you think that the trypanosomes in those two cases were identical?—They were quite alike. They were both arsenic treated cases.

3306. They were to all intents and purposes from a morphological point of view alike?—Yes, quite alike.

3307. Now one of these cases followed the typical chronic course, did it not?—Yes.

3308. And the other one followed the acute course?—Yes.

3309. And they both of them came from Uganda?—Yes, I believe so. I believe that Captain Tulloch was there.

3310. (Sir William Leishman.) Captain Tulloch contracted it in Uganda.

3311. (Sir John Bradford.) I suppose that it might be urged that the trypanosomes in these two cases were identical?—If you accept morphology as the only criterion.

3312. Yes, I am only on the morphology at the present time. I want your opinion on the morphological question.—I could not tell the difference. I have the specimens now.

3313. You could not tell the difference even now?—No.

3314. They were both Uganda cases. One was an acute case and the other a chronic case. As regards the cases that you treated successfully, can you tell us about the morphology of the trypanosomes in those cases?—Yes, they were both of the long form.

3315. Where was the disease contracted?—In the north of Rhodesia, I believe.

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[Continued.]

3316. May I ask, because of course none of us on this Committee has the knowledge that you have on this matter, when you say the long form, do you mean a form of *gambiense*?—There are two forms of *gambiense*. I mean the form that has a flagellum apart from the body.

3317. The long form?—Yes, with a flagellum extending beyond the body of the trypanosome. In the short form the flagellum ends with the body of the trypanosome.

3318. You recognise, then, that some trypanosomes are dimorphic and some monomorphic?—We described four forms in the *brucei* many years ago. There is more than one form in the case of every trypanosome. I should imagine.

3319. I want to avoid, as far as possible, technicalities in this matter, but some of the witnesses who have been before us have laid great stress in respect of the identity or non-identity of trypanosomes, on the question whether they are dimorphic or monomorphic. Do you accept the classification of trypanosomes that some are dimorphic and some are monomorphic?—Yes, roughly.

3320. In which category would you place *gambiense* and *rhodesiense*?—Both of them dimorphic.

3321. In which category would you place *brucei*?—Monomorphic, roughly, although it is not absolute, I think.

3322. I think I am right in saying that you and I were more or less answerable for the naming of the *Trypanosoma brucei*?—Yes.

3323. You have specimens of that still?—Yes.

3324. Would you tell the Committee whether the specimens you have exhibit two forms or one form?—We described the ordinary form, a long form with a flagellum beyond the end. We also described large forms that you find in certain parts of the body. We also described forms with granules that you find in certain states and certain animals. There was no dimorphism *qua* the flagellum, which is the point that you were speaking of, that is a short form with no flagellum beyond the body. We did not find any.

3325. Would it be correct to say that at the time of our observations that was not a point to which we greatly directed our attention?—Yes.

3326. You still have that original strain in the laboratory, have you not?—I had until recently.

3327. Can you tell the Committee whether it has undergone any morphological change?—No, it has undergone no change, either in its morphology or in its effect upon laboratory animals.

3328. At the present time does it not exhibit two forms, a broad stumpy form and a long form?—No, it does not.

3329. Do you think that it is at all likely, as has been asserted by some workers, that the trypanosome that Sir David Bruce sent home on that occasion was not the one that he had worked at in Zululand?—How can I tell? We had to accept what was given to us.

3330. You know that that has been said, do you not?—No, I did not know it had been said.

(Mr. Rothschild.) I do not quite think that that was what was said. What was said to this Committee was that what he sent home as *brucei* from Rhodesia and Nyasaland was not the same as what he sent home from Zululand.

3331. (Sir John Bradford.) That may be so, but the other has been said. I do not say it has been said to this Committee, but it has been said. (To the witness.) It is a point of some importance, is it not?—Certainly.

3332. As regards the question of the identity or non-identity of *rhodesiense* and *brucei*, certain observers have said with regard to Sir David Bruce's original work in Zululand, that although he undoubtedly intended to send home the trypanosome that caused the disease which he was then investigating, by some chance or other he sent home another trypanosome. Do you think, from your experience and work with trypanosomes, that there is any solid ground for thinking that the trypanosome that was sent home, and that you and I described, was, so to say, not the one that Sir David Bruce thought it was?—No, but

I do not see how you can tell. We had to accept the animal that was sent to us; we had no means of telling.

3333. You have worked with what is now called *Trypanosoma rhodesiense*?—Yes.

3334. Do you look upon that as different from the one you have described as *brucei*?—Certainly with regard to those in the animal that was sent to me by Dr. Daniels. I had it going for some time.

3335. Do you base that on morphology only?—No.

3336. Morphology and clinical experience?—Yes.

3337. I think you told the Chairman that you are satisfied that one and the same trypanosome will vary in its morphology as a result of having passed through different animals?—Yes.

3338. Would you give us an instance of that?—Would you name a trypanosome and the animal?—I was thinking of the most gross example, and that is the passing of nagana through a cold blooded animal, a frog, as we did years ago.

3339. Would you tell the Committee what the changes are?—The trypanosome becomes very much smaller. I am not quite prepared to give the actual measurements, but I should say roughly nearly half the size that it is in the mammal, and if it is put back in the rat again it gets very much larger than the original trypanosome in the rat. You nearly double the length by passing nagana through a cold blooded animal, but in the rat it gradually comes back to the normal size. The example that was most familiar to us was the growing of nagana in rabbits. It grows with great difficulty. You get a very chronic disease; and there the trypanosomes are not the same to look at. They are smaller and they are thinner, and still more so in the goat where they grow with greater difficulty.

3340. You are satisfied that if they are passed through animals which are at any rate not very far removed they may alter in their morphology?—Yes, they do, certainly, and by chemical means you can do away with the micronucleus.

3341. Yes, but that is another point, is it not?—It is a question of how you can alter them by artificial means. That is the point.

3342. One of the points I have in my mind is this—that some witnesses have gone so far as to throw doubt on whether the different trypanosomes in wild animals and so on are forms of one and the same. Have you any doubt that many of these, or some of these, at any rate, are specifically different?—I do not think that our knowledge enables us to say at all without comparing the effect upon animals and without a uniform method of preparation.

3343. Provided that those conditions are complied with do you think that then you can definitely separate the different forms?—I should say that if the experiments were extensive enough you could.

3344. As a matter of fact would you separate *gambiense* from *rhodesiense*?—Certainly.

3345. I think you said that you would also separate *rhodesiense* from *brucei*?—Yes.

3346. You would separate *rhodesiense* from *gambiense*?—Certainly. With regard to the effect on laboratory animals, in my experience it is quite different. Therefore, if it is the same form it is in an exalted condition. *Rhodesiense* kills all the ordinary laboratory animals much quicker than the *gambiense* that we have used.

3347. Can you tell us roughly the difference, say, in a rat or any animal you like to take?—The difficulty comes in again that the two strains of *gambiense* that Sir David Bruce sent home acted differently upon laboratory animals. With regard to the so-called *gambiense*, the form that produced exudation round the vessels of the rats that were inoculated (signs that were comparable to the lesions in humans) took very often six or eight months to kill a rat, whereas *rhodesiense* kills them in 16 to 20 days. The longest is 20 days I think, and very often death occurred within that time.

3348. In your experience with *rhodesiense*, have you had any animals survive for a prolonged period?—No, not without treatment.

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[Continued.]

3349. That is what I mean.—Rabbits even will go down under a month.

3350. From the clinical point of view in animals would you say that the clinical picture produced by *rhodesiense* resembled at all that produced by *brucei*?—No.

3351. In what way does it differ?—It differs in the spleen principally I should think. I do not think you ever get the same enlargement of the spleen in *rhodesiense* as you do in nagana, and you do not get the changes and destruction of the bone-matter that you do in nagana. Those are the only things I can think of at the present moment which are particularly concerned.

3352. I gathered from a former answer that the clinical picture, the symptoms produced by *rhodesiense*, are quite different in animals from those produced by *gambiense*—much more acute?—Much more acute.

3353. I want to know whether the clinical picture produced by *rhodesiense* is very different from that produced by *brucei*, or whether it resembles it?—It more nearly resembles *brucei* than it does *gambiense*.

3354. (Dr. Bagshawe.) You said in reply to the Chairman that you did not think that entomological research would lead to any appreciable result. What do you base that opinion on?—Since it has done so little towards getting rid of the disease.

3355. With regard to the control of trypanosome diseases in Africa, do you think it is impossible to get rid of the fly?—I do not know.

3356. What was in my mind was, have sufficient attempts been made?—That I do not know.

3357. Has the subject been studied sufficiently?—We know the habits of the fly, do we not?

3358. I do not think we do know much. We know a little about *papalis*, but I think very little about *morsitans*. My own idea was that we have a great deal to learn.—On the entomological side?

3359. Yes.—It may be. My opinion on a thing of that kind is not worth very much, because I have never been there and I do not know anything about the fly on the spot.

3360. Do you think that if we got a more complete knowledge of the habits and life history of, say, *morsitans*, we might possibly find a way of dealing with it—a practical method?—I should not have thought so, but I may be quite wrong. My work has been entirely apart from the insect side and I would not like you to attach any importance to my opinion.

3361. (Mr. Buxton.) With regard to your experiments with intravenously deposited antimony—is that the right way to describe it?—You inject it into the veins. Solid particles of antimony are taken up by the white blood corpuscles and converted by them into some soluble form.

3362. And deposited?—No. They are converted into a soluble form and then diffused through the blood stream apparently.

3363. How is the first introduction achieved? Is it simply by mechanical means?—Simply injected by a hypodermic syringe into the veins.

3364. I thought you mentioned the word "electrolytically"?—The antimony is electrolytically deposited on zinc.

3365. You said that this treatment has been tried in a number of cases of nagana, and successfully?—Yes, in animals.

3366. Was that in Africa?—No; experimental animals here.

3367. Did I understand you to say 100 per cent. of successes?—With rats 100 per cent. It went down as the animals became larger and more difficult, because you could not get the same number or quality.

3368. As regards the treatment of animals do you regard that as a hopeful line of treatment?—Yes, I do.

3369. Has it been practically tried to any very considerable extent?—It has been tried in a small way in Rhodesia. It has been tried with good results amongst the horses there, but it was done by a man who was very busy otherwise.

3370. It has not been generally introduced?—No.

3371. Perhaps not much is known about it?—No, it has never been pushed in any way. The Egyptian Government were the only people who took it up, and they sent Captain Ranken, who was working with me for a couple of years, to try the effect on humans out in the Lado. It is being tried on a fairly large scale, so we shall know whether there is anything in it or not.

3372. On the human being?—On the human being.

3373. Has it not been directed to the treatment of disease in animals there?—It has not been tried on animals at all out there in any consecutive way.

3374. No steps have been taken in that direction that you know of?—No.

3375. (Dr. Balfour.) It is a small point, but it was the Sudan Government, not the Egyptian Government, who sent Captain Ranken out.—To me it is Egypt. I did not know.

3376. I have seen Captain Ranken's work and can testify to the efficacy of the metallic antimony treatment, but it has certain disadvantages, has it not?—Yes.

3377. If you miss a vein, as even an expert worker like Captain Ranken may do, you get necrosis of the tissues?—Yes.

3378. And occasionally you get constitutional disturbance?—Yes, in a certain number of cases, especially old cases or cases in old people.

3379. Are the constitutional effects due to the toxicity of the drug, or to some special condition of the patient?—I should have thought the condition of the patient. We could kill old horses quite easily. With regard to fairly decent horses a much larger dose would have no effect on them constitutionally.

3380. When you are treating a large number of cases there is very little time to go carefully into each case?—Yes. It is only in cases in a certain stage, I think, that it can be any good. I think that there it can be of use.

3381. The other day I was asked to recommend a drug for a case of Nyasaland sleeping sickness which is in London at present. I was unable to suggest anything new and I sought advice on the subject. Dr. Dale, of Herne Hill, suggested the use of the colloidal salts of heavy metals. They have come into use in the case of silver salts in dysentery.—Yes. Sir William Crookes sent to the committee of the Royal Society some that he had prepared of silver, mercury, and several strengths of antimony. We found the silver of no avail, the mercury of no avail, and the antimony nothing like so active as the metallic form.

3382. Do you think that bismuth would give any results?—I have never tried it.

3383. The colloidal salt of bismuth would be much less toxic than ordinary bismuth?—Yes.

3384. So it would be worth trying. Colloidal salt of bismuth was recommended as a matter of fact.—My experience of the colloidal substances is that they become immediately changed in the blood so they are no longer colloidal.

3385. I do not know whether you have seen a note concerning a French preparation which has just come out, called antilueticin, used in syphilis. It is in part an oxide of antimony, a new antimony preparation which is said to be exceedingly efficient.—No, I have not seen it.

3386. So that you cannot give any opinion as to that?—No.

3387. In the late stages the difficulty with antimony and all these drugs is the lack of penetrating power. They cannot get through the spinal membrane. Now what is your opinion with regard to intrathecal injection, using the blood serum of a patient who has been treated, which presumably contains antibodies, and injecting that into the spinal canal?—I have no experience of that.

3388. Is it not a line of treatment that might be usefully applied to these antimony-treated patients?—It would very likely be of use.

3389. Is it not worth while to suggest it to Captain Ranken as a method worth trying?—Yes. I tried injecting antimony into the spinal canals of animals.

3390. Is not that dangerous?—We lost no animals. We treated one animal with very acute surra, that

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ordinary injections had not touched, for eight or nine months. It constantly got a recurrence.

3391. Would you not rather employ a serum in a human being than antimony itself?—No, but I think it quite a thing to try.

3392. Dr. van Someren tried it for some cases in 1908 or 1909 in Uganda. They were cases in extremis he admitted, and not suitable for the test. He got no results, but it might be well worth while repeating the work in the Lado?—I think so.

3393. Now, possibly, by adding something to the antimony or trypanocidal drug, it would act as a carrier to convey the drug through the spinal membrane into the spinal fluid?—Yes, if we could find something.

3394. Something like urotropin which passes so readily into the canal that it can easily be detected in the spinal fluid. Would it be possible to combine urotropin with antimony?—Quite, I should think.

3395. Would it have any action on it?—I do not think it would. Antimony is very inert as a metal. It might be tried. Both those suggestions would be very valuable to Captain Ranken. He has a large number of cases to try them upon.

3396. I wrote to him some time ago about using urotropin, but I have not heard from him. Has he mentioned the matter to you?—No.

3397. Non-pathogenic trypanosomiasis may very often exist in a cryptic form. You cannot find the parasites by ordinary examination of the blood. Do you think that pathogenic trypanosomiasis may exist in a similar condition? Have you sufficient experience to say?—I should think it could. It all depends on what value you attach to the ordinary microscopic examination of the blood. You very often may inoculate an animal successfully, but not find any trypanosomes in the blood used by ordinary microscopic examination.

3398. Is that due to the fact that there is a paucity of trypanosomes in the blood, or that the parasites are present in some special form?—That I do not know.

3399. You have formed no definite idea?—No, except in regard to the granules described by Fry. You can burst all the trypanosomes with concentrated salt solution, and the infectivity remains. Of course, there is the possibility of one or two escaping.

3400. The passage of a trypanosome through animals may result in increasing its virulence?—Yes.

3401. Do you think that such passage might alter also what you may call its "selective capacity," and that a trypanosome which had not been originally pathogenic to man might become so by having passed through animals?—I think it is quite possible, the man being in a certain given state. I suppose that all parasitism comes about in that way originally.

3402. The man would have to be in a special condition, you think?—He would have to be in a special condition.

3403. Do you think that a feasible explanation of what has happened in Nyasaland?—I should think so.

3404. Professor Minchin put forward that view and you yourself hold it?—Yes.

3405. (Mr. Austen.) Whence came the trypanosome sent to you by Dr. Daniels that you regard as *Trypanosoma rhodesiense*?—He sent me a rat inoculated from a patient here.

3406. But was the strain derived from Nyasaland or Rhodesia?—Dr. Bagshawe could tell you where the first case came from.

(Dr. Bagshawe.) That was from North-Eastern Rhodesia.

3407. (Mr. Austen to the witness.) You told the Chairman that you think that at any moment the Nyasaland or North-Eastern Rhodesian form of sleeping sickness may become epidemic. Do you regard it as a new disease?—I think that with such information as I have with regard to it, I should regard it as a new disease. As I have been saying to Dr. Balfour, if you had a man in a given condition, an animal trypanosome might possibly be able to grow in him.

3408. Would that explain the simultaneous existence of the same disease in several widely different parts of Africa?—It is not in several widely different parts, is it? Did not the Uganda disease come from the West Coast?

3409. I am not talking about the disease caused by *Trypanosoma gambiense*, but the Nyasaland or Rhodesian form of the disease. Unless I am mistaken, I believe that that disease has been diagnosed not only in Nyasaland but in North-Eastern Rhodesia, in German East Africa, and in Portuguese East Africa, and that it exists in at least four centres?—They are all in communication, are they not, by trade routes?

3410. Would you regard it as probably having arisen from one source?—It is very difficult to give an opinion on that point.

3411. You have nothing to go upon?—I have nothing to go upon. I have only worked with the disease here, and not out there. It is impossible for me to give an opinion.

3412. You have stated that you consider that *Trypanosoma rhodesiense* is a distinct species from *Trypanosoma brucei*?—The two I have had to work with are certainly distinct both morphologically and in their effects upon animals.

3413. Do you consider that *Trypanosoma rhodesiense* would always be pathogenic to man or would it only be occasionally pathogenic?—It may wear itself out like the disease has on the West Coast. All such diseases tend to wear themselves out.

3414. You must excuse my ignorance but I do not quite follow. You said, I think, in reply to Dr. Balfour just now, that if you passed the same trypanosome through the same species of animal several times you raised its virulence?—You may or may not.

3415. It does not always occur?—No, and it occurs under conditions we do not know anything of.

3416. May the virulence be diminished instead of raised by passage in that way?—Yes, it may.

3417. And in that case it may become non-pathogenic to the animal?—Yes. All these things are not certain, as there are so few facts.

3418. There is a condition of non-pathogenicity or equilibrium in the case of *Trypanosoma brucei* in the big game, or in the case of *Trypanosoma lewisi* in the rat, I take it?—Yes.

3419. That stage exists in animals however it is arrived at. Do you consider that there is at the present time any evidence of immunity, so far as you know, to trypanosomes among human beings? I am speaking of the Rhodesian form.—I have no means of judging.

3420. You do not know?—No.

3421. Do you consider it likely that if there were immunity a white man would possess that immunity?—I do not see why he should.

3422. This is what is in my mind. Assuming such immunity to exist to a partial extent, would it not be more likely to be found among people who had been exposed for an indefinite time to infection by trypanosomes, rather than among people who had been shielded from it by their geographical position?—Yes, you would expect it to be so.

3423. As you are doubtless aware, one explanation of the failure of Dr. Taute to infect himself in his well known experiment is that he was immune?—That sort of thing has been done often. Pettenkofer swallowed a spoonful of cholera bacilli. You must be in a certain state for many diseases to affect you.

3424. Two factors must be present? One is not sufficient?—Yes.

3425. We know that with regard to other diseases, diphtheria, for instance?—Yes.

3426. (Sir Stewart Stockman.) With regard to these animals that you cured, you tried reinfecting them, did you not?—Yes, some of them.

3427. With what result?—There was no protection.

3428. In no case?—No.

3429. Do you think there is any stage at which you could cure them and leave them immune?—We did not find that in any case.

3430. They were immune in no case?—No.

3431. (Mr. Rothschild.) I do not wish to labour the question of entomological research, but there is one point which arose out of an answer you gave to the Chairman. You said that you thought from what you have read of the works of others that the best method of research would be by trying the experiment of clearing a certain area of game and surrounding it

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with a fence and putting scientific workers to work to see what the result was?—Yes.

3432. Do you not think that the success of that experiment depends a great deal on our learning various things about the fly of which we are ignorant? For instance we are absolutely ignorant as to the distance the fly will travel, and it is of great importance to know what width of country outside your experimental area you would have to clear of bush to prevent outside flies from coming in and altering the percentage of infectivity; is not that so?—Yes.

3433. And there are also doubts as to whether the fly does not undertake periodic migrations for the purpose of reproduction during the breeding season, and not always to the same place. Would not those facts have to be ascertained before you could select an area and put it into a suitable condition for your experiment? Ought you not to undertake that kind of research in order to make your experiment a success?—Yes I quite agree. Judging only from what has been done up to the present time with regard to the investigation of the fly, it does not seem to me that deductions from that would lead very much further, but I may be entirely wrong, because I know nothing of the place, or the fly there.

3434. I quite understand that what has been published about the fly appears more or less to be the full extent of what we can ascertain, but as far as I know the definite bionomics published concern only the Uganda *pulpalis*, and we know practically nothing about the *morsitans*.—That is so.

3435. (Dr. Chalmers Mitchell.) In reply to Mr. Austen who was asking you about Dr. Taute's case I understood you to say that the state of the person might influence the question of whether the infection took or not?—Yes.

3436. In the case of animals, you have injected a very large number of animals with trypanosomes have you not?—Yes.

3437. Do you find they often do not take?—No, never.

3438. They take every time?—Yes. What I said referred to the initial infection, the way the thing becomes parasitic. I meant that you must have a man in a particular state (presuming that the *rhodesiense* is *brucei*) for the animal trypanosome to be able to affect him.

3439. You mean for the creation of the thing?—Yes, for the creation of the parasitism.

3440. Now that *rhodesiense* has been created, would you expect it to take or not expect it to take every time it was properly introduced into a human body?—Yes, from one's experience of animals, I should expect it to take quite readily, because it is in a very active state at the present time.

3441. In reply to various members of the Committee you have told us a good deal about your views on the morphology of these trypanosomes. I gather that you find very great difficulty, because of the different way in which the preparations have been made?—Yes.

3442. But behind that have you the opinion that they are separate although not easy to separate or that they are not separate? Have you formed an opinion?—There are certain groups that one has separated in one's mind from working with them.

3443. I know that you dislike names, but can you put *gambiense* into a group of that kind? Do you think that that is fairly separate?—I should put *gambiense* and *rhodesiense* and *pecaudi* together, but I have not anything to show that that is definite.

3444. On the whole you are disposed to think that they are separable?—Yes, I think one can separate them, having worked with them.

3445. Even although it is very difficult in a particular case—if they have been for instance over treated with arsenic and spoiled?—Yes.

3446. I forget exactly the question you were asked, but I understood you to say that your answer was modified so to speak, because the case of which you were speaking had been treated with arsenic?—Yes.

3447. That the form of trypanosome had been sophisticated?—These were cases of sleeping sickness

of a different clinical course, and in both cases they had been heavily treated with arsenic, and in both cases the trypanosomes were of the long variety rather than the short variety that you associate with sleeping sickness.

3448. Do you mean by that that the production of this long variety might possibly have been due to the action of arsenic?—No. I found them in animals in the same way without knowing why they became long or short.

3449. Then why did you bring arsenic into it at all? I do not understand.—There was nothing characteristic in these particular trypanosomes of sleeping sickness. If you had shown them to anybody he could not have said that they were characteristic of human sleeping sickness.

3450. I follow. Now, when trypanosomes are found in a wild animal (for instance we have had reports that trypanosomes have been found in a percentage of wild animals by Dr. Kinghorn and other people) have you much faith in the power to distinguish in that way and to identify the trypanosome in that way?—No, not in that way.

3451. Would you attach much value to it if you were told that a particular kind of trypanosome had been found in 15 per cent. of freshly killed koodoo for instance?—It would depend on the kind of trypanosome—a long form of trypanosome or a short. Those are the characteristics that you can instantly tell even with bad preparations, such a thing as the difference between the *T. nanum* and *rhodesiense*, for instance, one a short form and the other a long one.

3452. Take *brucei* and *rhodesiense*, for instance?—Judging from my own experience of strains I have used, I could tell the difference certainly.

3453. Take preparations from newly-shot animals in Africa?—It depends very much on the personal element. If you are working with certain strains of trypanosomes for many years, you get to know them although you cannot describe exactly by what means you get to know them.

3454. Roughly for how many years have you been working with trypanosomes?—Fifteen.

3455. And you still find a little difficulty in being quite certain?—Yes.

3456. Would you expect that a young man, however clever he was, who had been working for two or three years might be relied upon?—He would be able to tell the difference between short forms and long forms, because those are obvious to anybody, but between *brucei* and *surra*, for instance, it is not easy to distinguish at all unless you are continually looking at them.

3457. Thank you very much. Now you have for a good many of years done a large number of experiments for the Zoological Society?—Yes.

3458. I think I am right in saying that you have taken a special interest in the blood parasites, have you not?—Yes.

3459. And described many new forms?—Yes.

3460. You have been on the look out for blood parasites?—Yes.

3461. Have you found any trypanosomes in reptiles?—Yes.

3462. Have you found any trypanosomes in birds?—Yes, I have described 13 new ones in birds.

3463. I did not mean any new ones in reptiles, but have you found any trypanosomes in reptiles?—Yes, I have found in the ordinary edible frog ordinary trypanosomes and I have described them in one other frog in which they have not been described before.

3464. You also spoke (I think in reply to Sir John Rose Bradford) of having persuaded a human trypanosome to take up its abode in the frog?—No, *brucei*.

3465. Do you think it would be impossible or unlikely that in nature human trypanosomes or animal trypanosomes could effect their entrance into birds and animals and into reptiles?—They grow with great difficulty in birds and reptiles. I was only speaking of it with regard to the morphology. I could produce a

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brucei that was 40 microns in length which is more than double the usual length.

3466. Do you think there is a physiologically innate hostility between reptilian and avian blood in this respect?—The avian trypanosome would grow in a frog but not in a mammal, or only with great difficulty.

3467. Have you found any trypanosomes in mammals in the Gardens?—In one case, in a fat mouse from the Sudan.

3468. What kind of trypanosome was it?—Unfortunately the animal had been dead for some time, and I could not carry it on. It was of the long type. It looked roughly like *brucei*, but I have no evidence because I could not carry it on. I found a very large spleen.

3469. You practically do not miss a post-mortem on any animal that dies the Gardens do you?—No, I have not missed any bird, reptile, or animal which had died during the last five years.

3470. You have been particularly on the look out for post-mortem symptoms and microscopic evidence?—Yes.

3471. You have found no trypanosomes in mammals in the Gardens?—Only the one mentioned.

3472. We keep a record of the locality from which our animals come. You will bear me out when I remind you of that?—Yes.

3473. From the Sudan and the Bahr-el-Ghazal in the last five years we had 11 carnivores and three gazelles on which you made post-mortem examinations?—Yes.

3474. You found no trypanosomes in them?—No.

3475. From North-Eastern Rhodesia and Basutoland in the same time we have had two river-hog, one wart-hog, three zebra and 21 different antelopes. Have you found any trypanosomes in them?—No.

3476. From British East Africa and Uganda in the same period ten carnivores, one river-hog, two wart-hog and a rhinoceros. Have you found any trypanosomes in them?—No.

3477. I have picked out from our records only those animals which have come from areas in which sleeping sickness has been discovered. From West Africa, the Gold Coast, Gambia, Nigeria and the Cameroons, we have had three hyraxes, 37 different kinds of rodents, 55 carnivores, one river-hog, one wart-hog, two water-chevrotain and 31 different kinds of antelopes. You have found no trypanosomes or anything to suggest them in these?—No.

3478. We were in a good deal of trouble about some swine (wart-hog I think) which died of a rather mysterious disease. You were interested and did not quite know what it was at first, and you made a very careful examination?—Yes, and cultivations.

3479. And found no trypanosomes at all?—No.

3480. May I put it in this way, that if there had been trypanosomiasis or trypanosomes in any of these animals, your attention would have been aroused, and you probably would not have missed the fact?—No, I think not.

3481. (Dr. Balfour.) Did you examine the blood by cultural methods?—Yes, whenever there was any suspicion, and by inoculation. If there is any doubt whatever, one does not rely entirely on the microscope. I could give you the numbers—they are considerable.

3482. (Dr. Chalmers Mitchell.) Would you think that the effect of our climate or the effect of captivity would be likely to destroy the trypanosome?—No, I should not have thought so.

3483. It has been suggested to us, and it has been believed by many people, that the larger game of Africa form a permanent reservoir of these diseases. Speaking in a general way, do you not think it is very remarkable that in this list of animals and in all the animals that are alive coming from these parts of Africa, where trypanosomes exist, you have been able to discover no case?—Yes, I have often thought that it is remarkable that one has not been able to discover any.

3484. You take an interest when any case of an animal being very unwell happens in the Gardens?—Yes.

3485. Would your attention have been called to it, do you think, if any animal had been very unwell, although it had recovered afterwards?—Yes, my attention often is called in such cases.

3486. Acting with special consent, examinations of the blood for diagnostic purposes of living animals have been made from time to time?—Yes.

3487. So that in addition to the animals that have actually died, although opportunity has been taken of making an investigation, you have not found any cases in animals that have not died?—No.

3488. (Sir William Leishman.) The ground has been so well covered that I will only trouble you with one or two points. You have told us the strain of *brucei* with which you worked with Sir John Bradford so long ago, and you have told us the origin of it, and your opinion is that it was monomorphic?—Yes.

3489. And therefore distinct from *rhodesiense*?—Yes.

3490. Have you had any opportunity of examining any other strain of *brucei* sent from Africa?—No, except specimens from Sir David Bruce and others.

3491. From what place?—I have had constantly specimens of various trypanosomes that an opinion was wanted about, but I have never been able to experiment with them. I have had only the slides. I have only used the one strain that Sir David Bruce sent over in 1898.

3492. Have you had an opportunity of examining films made from nagana infection in Zululand?—Not lately I think. The last one was from an ox in Uganda which, you remember, was the subject of a paper by Stephens, I think.

3493. I am not clear that I remember it. Is that the strain that you spoke of as *pecaudi*?—It was a dimorphic strain which Sir David Bruce thought was identical with the nagana, but I did not agree with him. He published a paper and then Stephens published a paper, to show that it was dimorphic. He went through all the old strains of the nagana that he could get and found that they did not correspond, and that the strain from the ox in Uganda was not the same as the original nagana strain.

3494. With regard to the morphological changes, which you induced by passing the trypanosome through strange animals, by drugs and so on, you do not imply that the change in the trypanosome was permanent?—I have never found it permanent.

3495. When you put it back into the original animal would it resume its original shape?—Yes, after one or two passages.

3496. Eventually it would?—Eventually it would.

3497. There is no permanent change?—No, there is no permanent change.

3498. I wanted to make that clear. You have told us that you have no evidence of trypanosomes in antelopes at the Zoological Gardens?—No.

3499. Have you had any material sent to you from Rhodesia from wild game showing what is supposed to be *rhodesiense*?—Yes, stray specimens.

3500. Did you identify it morphologically as the same as the *rhodesiense* you had been working with?—No.

3501. Was it different?—Yes.

3502. Different in what respect?—In that it was not dimorphic.

3503. Where did you get the films from?—From Zanzibar and Rhodesia.

3504. Did they come from Sir David Bruce?—Some, from the expedition. I should not care to say anything about these films because I entirely disapprove of the method of preparation for morphological purposes.

3505. Quite so. Now have you any experience of the effect of human serum on the strains *gambiense* and *rhodesiense*? Have you done any experiments yourself?—Yes.

3506. With what result?—*Gambiense* is resistant in my experience and *rhodesiense* is not.

3507. That is what Sir David Bruce found?—I believe so.

3508. Your experience is the same as his?—Yes.

3509. Was the serum fresh serum?—Quite fresh; it was my own serum.

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3510. What proportions did you use?—About two to one.

3511. Twice as much serum as trypanosome blood?—Yes. I also heated it to 45 degrees to see if any difference was made by destroying the complement.

3512. From your general experience and knowledge of the life of the trypanosome do you think that it is possible that there is a further stage in development than any that we know of at present? I refer to pathogenic trypanosomes. Do you think that the life cycle is complete and that there are no gaps in it?—It is almost impossible to answer that with any certainty, granules being still under consideration. I would rather not give a definite opinion.

3513. Have you any general impression one way or the other?—I think we do not know enough to make a certain statement.

3514. What point would you lay the greatest stress on in establishing a species of trypanosome as a "good" species?—What do you mean? As a type?

3515. Yes, a type. What would you lay stress on?—On the morphology and the effects upon animals.

3516. In that order? You would attach most importance to morphology?—Yes, I think it would naturally come first.

3517. Would you attach no importance to the intermediary host, the fly?—I have had so little experience that I could not speak of the fly.

3518. (Sir Stewart Stockman.) I have been very much interested in the evidence brought out by Dr. Chalmers Mitchell about the absence of trypanosomes in these imported animals at the Zoological Gardens because it is rather difficult to control the importation. Is it the case within your knowledge that trypanosomes have been found in animals in other Zoological Gardens?—Very few. I know very well two or three foreign Gardens where they have practically none. In five years' examinations of blood from every animal that has died and from many that have not died, I have only found one trypanosome in a mammal. I have found lots in birds and reptiles.

3519. I ask the question because I very generously presented a camel to the Pretoria Zoo, and about a year after when somebody went round examining the blood of animals, they found the blood of this camel swarming with trypanosomes which were pathogenic to dogs and various other animals and the camel was killed. Have you made any examinations of camels?—Yes, I have.

3520. With negative results?—Yes, with negative results. I examined the animals that I thought likely reservoirs.

3521. By inoculation?—No, not all by inoculation, only those that were doubtful, whose blood was not healthy.

3522. (Chairman.) There are one or two points on which perhaps I did not quite appreciate your answers.

Perhaps you will kindly help me. You say that serum treatment may be helpful?—It should be tried, I think.

3523. Is it also prophylactic?—I do not think one can say. These things should be tried. Sir John Bradford and I some years ago tried serum treatment in nagana and found that the presence of very large numbers of trypanosomes in the blood serum of dogs did not protect other dogs when the serum was injected in large quantities.

3524. You do not think we are likely to get a vaccine?—No.

3525. One attack of nagana is not protective, I understand you to say?—No.

3526. Therefore probably one attack of sleeping sickness is not protective?—You cannot compare them. It runs a chronic course in one case and an acute course in the other.

3527. Are you satisfied yourself that true *rhodesiense* has ever been found in wild game?—From such specimens as I have had, which are not conclusive at all, I do not think so, but they have only been sent to me more or less haphazard.

3528. From personal observation you have never found any wild game with a trypanosome which you would unhesitatingly describe as *rhodesiense*?—No. All my work with regard to *rhodesiense* has been done by inoculation from a human case here.

3529. Do you know how many cases Captain Ranken has treated up to date?—I think it is over 300, implying over 2,000 injections.

3530. Of sleeping sickness?—Yes, of sleeping sickness.

3531. He is using the treatment for other diseases?—Yes, for yaws.

3532. When you find trypanosomes in a wild animal where the wild animal is tolerant, do they multiply and behave in the same way as where the trypanosome is pathogenic, or what is the difference if there is any?—I have no experience of the wild animal in Africa.

3533. Are there certain animals here which have a non-pathogenic trypanosome?—I very much doubt the non-pathogenicity of trypanosomes. I think that if you have them in the blood for some time they will produce some effects. They may lower resistance, as people say for want of a better expression. We do not know what it means at all.

3534. What inference would you draw yourself from the one experiment that Dr. Taute performed on himself?—I can draw no inference from it.

3535. He may have been lucky in what he inoculated himself with, or he may have been unusually resistant?—Yes. It has happened so with other inoculations.

(Chairman.) Thank you very much. We are very much obliged to you for the assistance which you have given us.

The witness withdrew.

Mr. J. O. SHIRCORE, M.B., B.Ch., M.R.C.P. (Edin.), called and examined.

3536. (Chairman.) You are in the African Medical Service, I think?—Yes.

3537. What was your last appointment?—At Nairobi.

3538. As what?—Medical officer at Nairobi.

3539. You have had practical experience, I believe, also in other parts of Africa?—Yes.

3540. In Nyasaland?—Yes.

3541. As medical officer, has your work been treatment or research work or both?—Investigation.

3542. Investigation on what line?—Finding cases of sleeping sickness, treating them when possible; not research.

3543. Have you come to any conclusion in your own mind as to whether the type of disease in Uganda amongst men which is carried by *palpalis* is distinct from the disease in Nyasaland which is carried by *morsitans*?—Yes.

3544. Would you kindly tell us why?—The Uganda disease at the onset is usually an insidious one. It is very seldom an acute one. The Nyasaland

disease is invariably acute. The course of the disease is usually years in the Uganda disease. In the Nyasaland disease it is a matter of a few months, on an average four and a half. Interstitial keratitis, that is, opacity of the cornea, is unknown, so far as I am aware, in the Uganda disease. In the Nyasaland disease, 17 per cent. of the cases have opacity of the cornea. Then in the Uganda disease trypanosomes are not always present in the blood. In the Nyasaland disease they are always present in the blood and in a definite relationship to the stage of the disease. Treatment in the Uganda disease is of great value, but in the Nyasaland disease, so far as natives are concerned, it is of no use whatever.

3545. What treatment have you found successful in cases of the Uganda disease?—Atoxyl and soamin.

3546. Soamin being almost the same preparation?—Very nearly the same. It is less toxic, I understand.

3547. We have heard of a treatment to-day by the intravenous injection of metallic antimony. Have you

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tried that at all?—I have given tartar emetic intravenous injections.

3548. Not actual metallic antimony?—Tartrate of antimony.

3549. What do you say about that treatment?—I should think that further experiments might be done with it. It might be very valuable. Certainly after the injection into the veins the trypanosomes are removed from the circulation for a matter of perhaps 10 days, after which they gradually return.

3550. How many cases have you treated or seen in Nyasaland of the Rhodesian type?—About 44.

3551. Have you made any post-mortems or not?—No.

3552. The natives have great objection, I suppose?—I do not think so.

3553. Do you think that anything would be learned from post-mortem examinations?—I do not think so. We know already the type of morbid anatomy found in trypanosomiasis. I do not think it would help us very much.

3554. Do you think that the morbid anatomy would be the same in the one type as in the other?—Yes.

3555. The disease is much more rapid in the one case than it is in the other?—Yes.

3556. That might to some extent alter the pathological appearances?—Yes, it might.

3557. Has your work been confined to sleeping sickness or have you general charge of the health of the people?—General charge of the health of the people.

3558. I suppose that through tropical Africa the death-rate is very high or higher than the European standard at any rate?—I am not prepared to give any opinion on that.

3559. You could not say?—No.

3560. You have not the data?—No.

3561. Let us take Nyasaland. What are the main causes of mortality?—Malaria, tick-fever, smallpox, pneumonia, and venereal disease—syphilis, and yaws.

3562. Yaws is a distinct disease from syphilis, is it not?—Yes. I think that those are the chief causes of mortality.

3563. Could you point out any disease as being the most serious cause of mortality?—I should say malaria.

3564. I suppose you could not give anything like a percentage?—No, I could not.

3565. We have been told that up to date only 153 cases of sleeping sickness have been recognised in Nyasaland. Therefore, the percentage of deaths from sleeping sickness compared with the percentage of deaths from other diseases would be infinitesimal?—Yes, I quite agree, so far as we know.

3566. From your own experience and your own work do you think there is danger in Nyasaland or in Rhodesia of that type of sleeping sickness becoming epidemic?—It is a very difficult question to answer, but I should think sleeping sickness is increasing to a certain extent.

3567. Increasing?—Slightly.

3568. The carrier of the disease no doubt is a *Glossina*?—Yes.

3569. What in your opinion is the reservoir from which the *Glossina* draws the trypanosome?—Game and to a certain extent I should say human beings, but only to a very slight extent.

3570. The cases are few in number. You do not think they can all be accounted for by infection from man to man *via* the fly?—No.

3571. You regard game as the chief reservoir?—Yes.

3572. You have not done any microscopical work on game, I suppose, and on the trypanosome in game?—No.

3573. You have considered the question of helping us by practical suggestions for dealing with the disease, and I want first to ask you your opinion of an experiment which has been suggested in the way of clearing a certain space of wild game, and then seeing what the effect is on the infectivity of the fly, and whether the fly follows its favourite food. What is your opinion about that?—I should think that if a

certain space was cleared of game, there is no doubt at all that a good many of the fly would die, because they would have nothing else to feed on. Of course, remaining fly would to a certain extent spread.

3574. The fly would migrate, you mean?—Would migrate.

3575. Would you regard that as a source of danger to human beings?—Yes, if there are human beings living within a reasonable distance of the fly.

3576. If the large game alone was dealt with what do you think would happen to the fly?—I do not think they would migrate to the same extent. They would feed on birds or smaller animals.

3577. Have you any reason to believe that they would cease to be infective when that was done?—Yes, from what one reads I should think so.

3578. What is your opinion about an attempted extermination of game?—I think it impossible.

3579. Will you kindly give us your reasons?—You might apply it to a certain limited area, but to apply it to the whole of the tropical area is utterly impossible.

3580. The whole of fly areas?—I refer to the fly areas. It is quite impossible, I think. I have done quite a lot of travelling in Africa, and a fair amount of shooting.

3581. How many years have you been there altogether?—A little over five years.

3582. You do not look upon that as a remedy then?—I do not.

3583. What do you say about clearing operations round villages, and on the sides of roads?—I think it is very important.

3584. Can you give us any experience that you have had in that way?—In a district in Nyasaland we cleared 200 yards very nearly round all the villages within the fly area, and in camping in these villages the fly was not evident as before. In fact, there were extremely few.

3585. What do you think would be an efficient clearing; could you put any limit?—A mile and a half to two miles would be quite good.

3586. Is that a very expensive operation?—I do not think so. The natives of the villages would perform the clearing.

3587. Do they work quickly and well?—Yes.

3588. Is it easy to get them to clear?—Yes, very.

3589. If you explain to them the reasons, do they appreciate them?—Yes.

3590. Supposing a quarter of a mile was cleared, would that have a considerable effect, do you think?—Yes, I think it would have quite a good effect.

3591. Do cattle live in the cleared villages?—Not oxen, but sheep and goats. Cattle only live at a village on the lake shore, which is several miles from the fly area.

3592. With regard to removing healthy natives from fly areas, are you referring to *moritans* areas?—*Moritans* areas. It is a difficult thing to do sometimes, because there is no suitable country to put the natives in. There is not a sufficient water supply.

3593. Native huts take some little time to put up. I suppose?—No, not very long. That is the least trouble.

3594. You would not press that?—No; that should be the last means.

3595. What do you say about segregation of the sick?—I think that is important.

3596. Is it only important when there are fly, or do you think there is any means of communicating sickness from man to man without fly?—Only important when there are fly.

3597. You do not think that mere contact is a source of danger?—No, I do not think so.

3598. Have you any opinion as to what work could usefully be done in dealing with the fly itself?—Measures should be taken to limit its spread and for its destruction.

3599. Have you any practical hints to give us as to how those desirable things can be accomplished?—Yes. I have some idea that fly are congregated at certain centres during the dry season, and one

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might attack these centres in some way, to destroy either the fly at the breeding places or render the places unsuitable for them to live in.

3600. Does that apply to *palpalis* or to *morsitans*?—*Morsitans* entirely. I have not had very much experience of *palpalis*.

3601. Do you think that entomological research into the bionomics of the fly is a hopeful proceeding?—Again it is very difficult to answer. I prefer more practical methods, such as clearing and destruction of the fly or destruction of the breeding places.

3602. We do not know the breeding places of the *morsitans*, do we?—No, but if you have fly living for nearly four months in one position alone, I take it that they breed there too.

3603. When you speak of fly living for months in one place, do they shift after the rains or before the rains?—As the country gets more suitable after the rains have commenced or when the rains are commencing, the fly start spreading. In the meantime these are the only suitable places for them to live in, and they live there for a matter of three months.

3604. Are they massed, so to speak?—Yes, and then they start spreading at the commencement of the rains and during the rains.

3605. Do you think that those homes, even if they are not breeding places, might be dealt with?—Yes.

3606. In what way?—By burning, by destruction, the trees being cut down and allowed to dry, and when the grass and trees are all dry there should be an extensive and thorough burning of these places.

3607. Have you ever, in your travels, found a place where there was fly but no game?—No, never.

3608. You have always found the two associated?—No, I have found game without fly.

3609. Outside the fly areas?—Yes.

3610. But in the fly areas, I mean?—There are some places in fly areas where you find game but you do not find fly; places which are not suitable to the fly, or places where, during the dry season, the flies have retired to—their summer resorts.

3611. Is there anything in the external appearance of the country or the vegetation which accounts for that?—Yes, open grassy plains and undulating country above the immediate level of the places where fly are found, and rocky barren country during the dry season.

3612. (Dr. Chalmers Mitchell.) When you told the Chairman a few minutes ago that in your opinion game was the chief reservoir and man only an occasional reservoir of trypanosomes, were you thinking of the Congo type or of the Nyasaland type?—The Nyasaland type.

3613. Not the Congo type?—No.

3614. Going back in your mind, would you make any distinction between them in that matter?—Yes. It is rather a difficult question to answer. I should say with regard to the Congo type that the reservoir, to a great extent, is man at the present moment.

3615. You really meant in the case of the Nyasaland type when you answered?—Yes.

3616. You said that you had done a good deal of shooting?—Yes.

3617. You have been in the fly belt a lot?—Yes.

3618. Are you seriously alarmed for your own health?—No.

3619. I take it that you would not sit and lounge about on the lake shores in Uganda?—I would not sit and lounge about in the area where the Nyasaland disease is.

3620. Have you been bitten often?—Not very often, because I am very careful not to be bitten.

3621. You think it is really a serious risk?—I certainly do.

3622. You know that the older travellers were bitten in very large numbers, and did not bother about it at all?—Yes.

3623. What is your explanation?—I think they were very lucky.

3624. Only luck?—Only luck.

3625. We have had one or two travellers who have told us how in the early days they were bitten and bitten and bitten until they got ill merely from loss of blood, and how men under their charge

were bitten and the cattle died all the time, the men not worrying about themselves. Naturally, no one likes being bitten, but though they knew it was dangerous to animals, they did not think it the least bit dangerous to themselves.—In Nyasaland there are a good many people who have been bitten over and over again by large members of flies and have never got the disease. Then Dr. Roux came there. He was only shooting three or four days in a place called the Dambo Patsanjoka; on the outskirts of the Dambo Patsanjoka there are a considerable number of flies. He was there with two other Europeans, and he got sleeping sickness almost immediately—after 3 or 4 days. He was a missionary and he came from South Africa to visit some friends just outside the proclaimed area.

3626. Had he porters with him?—He was shooting across country, and they went along the road.

3627. He had not porters with him?—Not during the shoot. He left the porters.

3628. Which type of the disease was it he got?—The Nyasaland disease.

3629. Was he killed?—Yes, he died. Then there were the cases of Dr. Hardy and Dr. Greathead.

3630. There are a good many people who have not been killed?—Yes.

3631. In Uganda it was rather different, was it not?—Yes, as far as the natives were concerned.

3632. They practically always took the disease if they had the chance?—I am not prepared to say that. I should say that a great many of them contracted it. There were probably many more risks there than we have in Nyasaland, and more intense infection. The natives go to watering places when they need not necessarily go into the forest.

3633. You are medical officer, and your duties are fairly heavy and absorb most of your time, I take it?—I was medical officer of the district as far as sleeping sickness investigations were concerned, and to a certain extent I had to look after the general health of the people, but that was not really the object. I was there for sleeping sickness work during this particular period.

3634. You had no time practically for research?—No, and I was not there for that purpose. Sir David Bruce was working in the same district on that line of work.

3635. So far as your opinion on game being a reservoir is concerned, on what is it based?—It is based more or less on research work which has been done, and furthermore on my particular experience in this district.

3636. I do not quite understand why you consider that game is a reservoir?—In my opinion, the nearer you get to game in fly areas, the larger the number of cases you come across. In my histories here of several cases which I have jotted down for the purpose, there has been always a history of a visit or a journey undertaken through fly and game areas before the people got sick—very shortly before.

3637. How soon have people been sick when they have been bitten by flies?—Sometimes 4 to 12 or 13 days. There is an incubation period, I think, of 4 to 12 or 13 days.

3638. I am very much interested in what you say; will you go on?—Then another reason is that no children have been found infected. If there were reservoirs immediately around the villages or in the villages, children ought to be infected. Then 65 per cent. of the males, and only 34 per cent. of the females, taking the number of cases that I noted up to the time I left, were infected. Practically half only were females. The males are more energetic; they like shooting game, and they go into fly areas and game areas, and therefore I think they get sleeping sickness more frequently than the females.

3639. Interrupting you for a moment, according to the figures you have given, the people who go away a bit from the villages, the active males and so forth, get sleeping sickness more often?—Yes.

3640. The history, so far as I have followed you, only applies to their going into the fly country?—Yes,

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3641. I want to know whether your opinion, that the game, apart from the flies, are a danger, is based on your own work or the very valuable work of Sir David Bruce?—I have taken it from the work of Sir David Bruce and of Dr. Yorke.

3642. (Mr. Rothschild.) I was struck by the fact that there is one single case on record of sleeping sickness caught in Uganda which ran an acute course. Do you think that that was due to the infection being carried by a different fly or because the patient was in a very sensitive condition at the time of infection?—I should think that if it was the Uganda disease and the patient was in a very delicate state of health before he got it, it might run an acute course. It is certain, I suppose, that it was not a case of disease due to *moreitans*?

3643. The case died in London, and we only know that he contracted it in Uganda. He possibly was in a delicate state of health when he contracted the disease.—I should think it quite possible to have acute cases of Uganda disease, very similar in type, as it were, to the Nyasaland disease. There are already these differences—for example, opacity of the cornea and the trypanosome always present in the blood, and a sleeping sickness stage which is very rare in the Nyasaland disease, whereas it is common in the Uganda disease, and so on.

3644. (Mr. Austen.) Do you consider that game is the only possible reservoir in an area in which you get game and *Glossina morsitans* together? Apart from the question of a limited reservoir in human beings, is there nothing else besides big game?—I do not think so.

3645. Nothing at all?—Not that I know of.

3646. No smaller mammals, bats for instance?—Not that I know of—I could not say.

3647. You cannot say as a fact that there are no smaller mammals?—I take it there must be some undoubtedly. I do not say whether they are reservoirs or not; I do not know.

3648. You have not made any investigation on that point yourself?—No.

3649. Do you regard sleeping sickness in Nyasaland as an old-established or a new disease?—I regard it as an old-established disease.

3650. Indefinitely old?—Yes.

3651. As old as the people perhaps?—Yes.

3652. Then if that is the case it would only be natural, from what we know of other diseases, to expect to find a high degree of immunity among the native population, I take it?—Yes, that is the natural conclusion one would come to.

3653. I think you told the Chairman or another member of the Committee just now that you consider the disease is slightly increasing?—Yes, I think so. You might conclude that, but I do not know whether you would be right.

3654. So far as you are yourself personally acquainted with the conditions and with the statistics, you consider that the annual return of cases is rising?—Yes, I think so undoubtedly, according to the statistics.

3655. May that not be merely due to the fact that the search for the cases is more thoroughly conducted?—Yes, that may be so.

3656. Have you paid special attention to the bionomics of *Glossina morsitans*?—Yes, to a certain extent.

3657. Have you found the breeding places in Nyasaland?—No.

3658. You have no idea where the fly breed?—I believe they breed at these particular centres that I have described, because they stay there for such a long time. One knows that they breed quite freely in the laboratory, and I see no reason why they should not breed there.

3659. So far as you are acquainted with *Glossina morsitans*, is there any seasonal difference in the numbers that you find?—Yes, there is seasonal difference in the numbers in certain areas and in the spread, the diffusion, of *morsitans* throughout the district. There is a decided seasonal difference.

3660. Are you able to form any opinion as to whether the breeding season is the dry or the wet

season?—I should think both, but I am inclined to think the wet season possibly more than the dry.

3661. Does not that then rather tend to diminish the importance of these concentration areas, if I may call them so, in that you would have breeding going on in the areas to which the fly retires in the dry season, and also in the area outside?—Yes, but if these places are the sources from which the fly proceeds to other secondary centres, I take it that if you destroyed the primary spots the secondary centres would cease to exist.

3662. What is your opinion as to the possibility of exterminating *Glossina morsitans* in the sleeping sickness areas in Nyasaland?—In some places it would be very easy and in others very difficult.

3663. I think you have no experience with regard to North-Eastern Rhodesia?—No.

3664. Then I may take it that you consider that to attack these areas to which the fly retires in the dry season would be the entomological method of proceeding most likely to yield valuable results?—Yes.

3665. Are you familiar with the details of the proposed experiment in game destruction?—Yes, more or less.

3666. Can you give the Committee your opinion as to its desirability or value?—I think the result of the experiment would undoubtedly be what the experimenter desired.

3667. I am sorry to hear you say that.—I mean the direct result of it; but with regard to whether it is going to serve ultimately any useful purpose, I do not think it will. I mean to say that in a certain definite area you could undoubtedly destroy the game if you choose to spend enough money to do it. You would undoubtedly diminish the infectivity of the flies because of the game destruction, but I think it is impossible to apply that throughout all the fly areas and all over large stretches of Africa.

3668. Then if I may call for the moment the sum of money which would be devoted to this experiment the "candle," you would consider the game not worth the candle?—Yes, I think that is so.

3669. If you were given, say, a relatively small sum, 5,000*l.*, to expend in combating the sleeping sickness problem in Nyasaland, would you tell the Committee what you would consider the best way to expend it?—I would firstly attack the fly centres in the way indicated. I would perform clearing round villages and at special areas along roads, so as to break up fly belts. I would also have natives or animals covered with sticky substances as has been done in some places and paraded about to catch any flies immediately around villages, and at the same time I would supply certain of the villagers with rifles—muzzle-loaders—to shoot any game within a radius of, say, a couple of hours' march from their villages so as to keep the game away. I think one ought to combine every possible method one has at one's disposal to combat fly and sleeping sickness. I do not think that any one method is of very much value, except perhaps if one could really attack some of the fly centres and destroy them, as I think could be done; but you could not do it always everywhere all over Africa. You would have to choose your place and deal with each separate place entirely on its own merits.

3670. I take it from that answer that you are decidedly in favour of entomological investigation in this connection being pushed to the utmost possible extent?—Yes, as far as the destruction of fly goes.

3671. You think that there is a decided possibility of definitely advantageous results being obtained from an attack on the fly?—Yes, I think so, and also one would attack the game; but wholesale destruction I cannot see the value of, really.

3672. You would agree that to attack the fly is the really radical method of procedure?—Undoubtedly. Either of them is really radical. If you have the fly without the reservoirs then the fly is harmless, but if you have the reservoirs then you must do away with the fly.

3673. But is it not possible that if you destroy one reservoir you may create another, whereas if you destroy the carrier you have destroyed the evil, root

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and branch?—Yes, I quite agree. The carrier is undoubtedly the most important. Then you would minimise any risk that there might be.

3674. (Dr. Balfour.) You have told us the experience you have had of the Nyasaland form of the disease. What is your experience of the Uganda form?—I have seen several cases at Nimule and between Koba and Wadelai. I have already stated what I think the differences are.

3675. Do you think that the Uganda form is getting milder from what you know?—I am not prepared to say that.

3676. You cannot tell?—I was there very nearly four years ago.

3677. You have no recent personal experience?—No, not personal experience.

3678. Do you think that any form of drug treatment is reliable in either form of the disease?—Yes, there have been very good results with atoxyl, soamin, and tartar emetic.

3679. In which form of the disease?—In the Uganda form of the disease.

3680. What about the Nyasaland form?—I have tried atoxyl, I have tried soamin, and I have tried tartar emetic, but I have not succeeded in doing any good whatever.

3681. You have never cured a case?—No.

3682. Have you ever in the late stages tried intrathecal injection, any spinal injection of the blood serum of a patient treated with some drug?—No, I have never tried that. There was a certain amount of difficulty with injections in the ordinary way, and it would have been perhaps a little difficult to persuade them to allow a lumbar puncture to be performed.

3683. Are trypanosomes rare and scanty in the blood of patients in the Nyasaland form in your experience?—They are invariably present.

3684. But are they numerous or otherwise?—It depends on the stage. During the early part of the disease there may be one or two in the whole preparation. Later on there may be one or two in each field of the microscope, and later on you might have eight or nine.

3685. They are numerous?—Very numerous.

3686. In the late stage there would be a distinct likelihood of the patient infecting a fly?—Yes.

3687. In the early stages too?—Yes, I think so. When one thinks of one trypanosome in one drop of blood and remembers that there are 14 lbs. of blood in the body very nearly, there may be probably 10,000,000 or more trypanosomes floating about.

3688. There is a distinct likelihood?—There is a distinct likelihood.

3689. Do you think that the disease is much concealed in Nyasaland by the natives?—Yes, it has been.

3690. I understood you to say that you think it is likely to spread?—I think it is likely to spread; there is an indication. We are finding more cases. It is true that the natives have hidden cases, but the finding of cases seems to be much easier now than it used to be.

3691. With regard to the incubation period, how long is it before a man has to lie up after he has become infected, as far as you can tell?—Almost immediately.

3692. In a week or 10 days the symptoms are so bad that he has to lie up?—Yes. I have a case here which I will give you if you like. A man went in June to a village. After being there a week he felt ill and was laid up for five days. He returned on the sixth day to his own village and from that time to the end he was laid up.

3693. Is it true both of the European and of the native that he has to lie up soon?—Yes, I believe so.

3694. You said that there is not much evidence of man to man infection in Nyasaland?—No, there is not so far as I can see.

3695. The distribution of cases does not suggest it?—No, the distribution of cases does not suggest it.

3696. Have you traced any connection between cases in different parts of the country; that is to say

cases being brought into contact, one case from one part of the country and another from another? Has there been any connection between them?—Almost the whole of the villages have had cases. I have a map which you might have a look at. You can form some opinion from it. It has underlined in red all the villages in which cases of sleeping sickness have been found.

3697. They are scattered about?—Yes, they are scattered about.

3698. That is very interesting. Can you yourself speak the native dialect or language there?—Yes, but I am not an expert at it.

3699. Have you got much information out of the natives themselves?—No, I am afraid they are rather dull.

3700. And I suppose that, like other natives, they are not very reliable?—They are reliable, but as regards finding out whether it is a new disease or an old disease or anything of the sort, the natives do not pay much attention to these things. They have never thought of it; they are only just beginning to realise that the disease is a definite disease.

3701. Is the fly specially abundant where most of the cases have been found?—Yes, the map shows that very distinctly.

3702. Do you know of any case that has recovered?—No.

3703. Whether treated or not?—No.

3704. We were told about one case of tolerance in a native servant. That was one of Dr. Yorke's servants. Have you any similar experience to record?—There was one case where the early stage of the disease seemed to be rather prolonged, that is to say the trypanosomes were present in the blood for a long time without showing any definite increase.

3705. Was that a typical Nyasaland type?—Yes.

3706. The disease varies in type?—Yes, but not very much.

3707. Now have you any views about segregation camps?—I think if we perform proper clearings around the villages, there is no reason to have any segregation camp. You might have one or two cases in a village at one time. If the village is well cleared I do not think there is any danger.

3708. Have you tried what is called the serum reaction in testing the effects of human serum on the *Trypanosoma rhodesiense*?—No.

3709. Nor *gambiese*?—No. I made no attempt to do any work of that description, because my time was fully occupied in going round the country and finding fly areas and cases of sleeping sickness, and Sir David Bruce was there for the purpose of doing the work you refer to.

3710. Is Sir David Bruce's theory in accord with the facts observed by you?—His theory as regards what facts?

3711. As regards the question of *rhodesiense* being identical with *brucei*. We know that you agree with him that the disease is distinct from the Uganda disease.—Yes, the actual disease. I am not referring to the organism at all.

3712. Have you any opinion to offer as regards that?—No, I have not.

3713. You cannot say?—No.

3714. How do you account for the disease being so acute if man is resistant, because there is an idea that man is probably resistant to the disease?—It is possible that some of these people get infected after a great deal of fatigue, after a severe journey or a long shooting trip in the district, or they may be in a slightly bad state of health before going out or something of the kind. There may be something of that sort; I think that there is some slight reason to suppose that.

3715. Do you think also that it might be due to the fact that the trypanosome had been increased in virulence by having passed through a man or through an animal?—I do not know; I could not say.

3716. Have you any reason to suspect that *fusca* or *pallidipes* has anything to do with the spread of the disease?—No.

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3717. You spoke of the case of Captain Tulloch. In connection with the acuteness of the disease in Uganda do you think that climatic conditions might cause a difference in the acuteness of trypanosome infection?—I do not think so.

3718. Captain Tulloch came home to this country. —I do not think that climatic conditions would have any effect whatever. When a sick man was removed to a healthier place, he might temporarily benefit, but it would not have any effect on the actual course of the disease itself.

3719. (Dr. Bagshawe.) I would like to ask you some questions first of all about this map which you have handed us. This includes, I take it, part of the proclaimed area or perhaps the whole of it?—It is actually the whole of what was the proclaimed area when I was there.

3720. There are areas marked in red numbered 1, 2, 3 and 4.—Yes.

3721. What are those?—Those I consider are primary fly centres where the fly congregates during the height of the dry season when they are not found in other places.

3722. You would only find fly in the height of the dry season within those red patches?—Yes.

3723. What do you suppose is the reason of that?—There is a lot of game near the areas. There is moisture in the areas, there is shade, and there is forest of a type which fly like. I think those are the main reasons.

3724. I suppose that most of the country is subject to grass fires in the dry season?—Yes.

3725. Would the grass fires affect the primary centres?—Yes. There is no very high thick grass there so that the fires do not burn very speedily.

3726. They burn less speedily in the primary centres than elsewhere?—Yes. There is short grass under the trees.

3727. So that the flies would get some protection?—Yes.

3728. What is the part of the map marked blue?—Those are plains with short grass.

3729. And no scrub?—Very little. There are isolated places here and there, perhaps.

3730. Would there be any fly there under any circumstances?—I have not seen any.

3731. What is that (pointing to the map)?—That is the lake shore. There is sand beyond that, and a slight amount of scrub between the blue and the lake shore.

3732. So that over three months in the year there is no fly over all this neutral area?—Hardly any.

3733. Am I right in supposing that that is the Domira road?—Yes, about there. It starts at Domira Bay.

3734. A line five miles north and south of the Domira road would come, according to the scale, about here and here (pointing to the map), and would include one of the primary centres—a comparatively small one?—Yes.

3735. For three months in the year, if you had an enclosed area here, a good deal of it would be bare of fly?—Yes.

3736. Do you think that an area enclosed by a fence five miles north and five miles south of the Domira road is a good place for the suggested experiment of game destruction in a localised area?—Between the two lines that you suggest you would get very few game at all.

3737. And in some parts of the year not much fly?—Not much fly.

3738. Then in that case it would not be a good place?—I do not think so. The game is chiefly north and south of that.

3739. Could you suggest a better place?—If you really want to destroy the game you could easily destroy it below that—taking the southern line and taking the large area below that.

3740. Supposing that you were given, say, 5,000*l.* and told to use that to enclose an area with a serviceable fence, you would want an area which would include a

great deal of game and fly?—I have already got a scheme for that purpose.

3741. May we see that?—This is almost purely diagrammatic (handing in diagram to the Committee). The fence would have to be at least 70 miles in length to enclose completely all that should be enclosed.

3742. Would that include more than one of these primary centres?—It would include practically the lot of them. There are three. That scheme was submitted some time ago merely as a suggestion, and the idea was to cut off the game from water supplies entirely.

3743. This proposed area has a sort of tongue running up to the north of the Domira road. The bulk of it is south and it includes primary centres 1 and 2.—Yes.

3744. Are there any natural features that would enable you to make your fence?—I suggested that some of the trees actually standing in the area should be used as uprights.

3745. Is that practicable?—That is practicable to a great extent. I should say very nearly for three-quarters of the 70 miles.

3746. How would you describe that particular part of the country? Is it open forest?—Yes, it is open forest, and in some places thicker forest. The trees are pretty close together, and one could easily construct a fence, I think, to go round that particular part, with the exception of near the lake shore, where in some places one would have to carry stakes and put them in.

3747. You consider, as I understand it, that five miles north and south of the Domira road is not a good place?—You would not find much fly, nor would you find much game there. You might travelling through on one day notice a number of flies, but if you were constantly travelling you would not find much fly always, and you certainly would not find much game.

3748. Taking this area marked No. 3, what would its size be, how many acres or square miles?—I should say about five square miles.

3749. Do you think it would be of any use to enclose that, or would it be too small?—If you wanted a place where you have game and fly, the better place to enclose would be No. 2 I think, because there you undoubtedly have large numbers of game.

3750. In that case you would be dealing with a part of the fly area if you enclosed No. 2?—Yes.

3751. In the wet season how would you prevent flies coming from No. 1 and No. 3?—You would have to clear extensively between those areas.

3752. What clearing should you think necessary?—I think two or three miles ought to be a very good amount of clearing.

3753. That is to say a belt two or three miles wide all round?—Yes, with no connecting forest whatever in between. The whole must be destroyed.

3754. What do you think would be the cost of making such a clearing?—Very little indeed. One would need about 50 men working there for a fortnight or a month cutting down trees. That is all. They would cut them down and leave them lying till they dried, and when they were dry the grass and the trees would all be burned.

3755. It was suggested to us by an authority who came from Nyasaland that the cost of clearing scrub in this part of Nyasaland would not be less than 1*l.* an acre.—Well, I have the opinion of several of the settlers and planters in Nyasaland, from whom one has enquired into these things from time to time, and they say that land required for planting (when you have to use very thorough clearing methods for planting tobacco or cotton for instance) requires one man per acre per month. One man can get one acre cleared if he works a month. His wages are 5*s.* or 6*s.* a month. I would undertake to clear some of these areas such as I have described in a month with 50 people, or to do it quickly say 100 people. That is 25*l.* These statements apply only to attenuated forest connections between the forested areas which contain the fly-centres.

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3756. Would that include food and everything?—Yes. The men would shoot game and live on the game. Some of the men would work for less if they were certain of getting a certain amount of meat. The men need not be paid in cash, because their hut taxes can easily be remitted.

3757. It means a loss of revenue?—Yes, it means a loss of revenue, but not actual cash paid out.

3758. It comes to just the same thing?—I do not know.

3759. Now, to go to another part of the subject, if your idea about these primary centres is correct that the flies collect there in the dry season, it seems practicable to clear these patches of forest, to cut down the wood, and eventually burn it?—Yes.

3760. Taking this area No. 1, what would the cost of that be?—It is an extent of $3\frac{1}{2}$ miles by very nearly 2 miles. It is about 5 square miles. If you had five or six hundred men working on that for a month and a half or 2 months, they should be able to cut it all down. It is merely cutting down. You do not want to uproot anything.

3761. Supposing it were practicable to deal in that way with areas Nos. 1, 2, 3 and 4, would you expect the fly to disappear?—I would expect the whole distribution of the fly in the district to be very different. Perhaps they would not entirely disappear.

3762. They would be very much diminished at all events?—They would be very much diminished.

3763. Do you think that that would be better than the game experiment?—Yes, I do.

3764. And you think it would cost less?—Certainly, I think it would cost less. If it is purely an experiment, one must do one thing or the other, but as regards prophylactic measures in the district, we ought at the same time to keep the game away and destroy it immediately around habitations and villages.

3765. To turn to the difference between Congo sleeping sickness and Nyasaland sleeping sickness, when you were in Uganda did you see any acute cases?—No, I did not see any acute cases.

3766. You told us you did not see many cases.—I did not see very many.

3767. It is not always easy to say when cases have become infected, but what was the longest course that any case took in Nyasaland as far as you know?—The longest course in one of my cases was six and a half to seven months. As a rule it is only a matter of four and a half months; I saw a boy who died, I am convinced, inside of two months.

3768. There was the case of a European in London who lasted for three years. Do you attribute that to the resistance of Europeans or to treatment?—To treatment. One gets these cases more or less advanced in the native and they have a certain amount of objection to treatment, because, I believe, of their mental condition.

3769. With regard to interstitial keratitis, I think you said you had not heard of a case occurring in the Congo form of sleeping sickness?—No.

3770. There has been one recorded in Senegal. I do not know that it is of much importance, but it might just be mentioned?—I did not know that.

3771. You said that if you made post-mortems in the case of death from each form of the disease, you would not expect to find much difference. You would expect in the Congo form surely to find brain changes much more often than in the Nyasaland form?—I have seen two cases of definite sleeping sickness in Nyasaland, one of which lasted very nearly three weeks and the other a fortnight. One was a father and the other a son. The strange thing is they both got it one after the other, and they both had a definite sleeping sickness stage. I believe the brain changes in this type would resemble those produced by the Congo disease.

3772. To go back to the areas, I do not think we arrived at the sum of the cost of clearing one of these areas. Can you give an estimate?—About 150*l.* to 200*l.* for area No. 1.

3773. You said you think the disease is spreading in Nyasaland, and at the same time you think that it has been in the country a very long time?—Yes.

3774. If it is an old-established disease what do you think is causing it to spread?—There is a larger population in this area than there has been. The Angoni, who are now settled in some parts of this area midway between the lake and the hills, were not settled there many years ago. They lived on the hills and the Yaos kept them back. Under our rule they have come down and settled very peaceably; they have opened up roads and travelled about and carried the fly. With a larger population and more opening up of the country, sleeping sickness cases have been observed more lately than they had been before.

3775. These particular inhabitants would be comparatively new to the fly area?—Yes.

3776. So that you might say that the disease was very old in the country but not old to those particular inhabitants?—I think you might.

3777. I have a question here which I was asked to put to you: supposing there was a lump sum placed at your disposal, in what order of importance would you place the following: (1) clearing of vegetation; (2) entomological research; (3) medical research; and (4) the proposed experiment of game extermination?—I do not quite understand what is meant by "entomological research."

3778. I think it means the study of the life-history and habitat of *morsitans* with a view to find means for its destruction. It would include the study of these areas to see if they were breeding places or not.—I think that would be the most important of the lot—entomological research as regards these particular centres and the clearing of these centres. With regard to medical research I think we have had a good deal of that.

3779. Where would you put the proposed experiment?—In Nyasaland.

3780. But I mean with regard to order of importance. Would you give it primary importance or secondary importance?—I mean this proposed experiment of game extermination in a limited area?—I would say that it was of secondary importance.

3781. (Chairman.) Have you any fear at present of the disease becoming epidemic in Nyasaland?—No, not epidemic exactly—not a sudden widespread epidemic.

3782. You do not think that it is likely to become like it is in Uganda?—No, I do not think so.

3783. Now a question on the map. Take, for example, fly area No. 3. You find the fly massed there. Is that state of things constant from year to year, or does it represent only one year?—This only shows as far as my experience goes. The probability is that it is a constant area, but I cannot definitely say so. I believe that these are constant areas.

3784. When the rains come and the fly begins to move, how far off do they get from these centres?—They get right away down *here* (pointing to the map).

3785. They move five miles away from the original centre at least?—Yes.

3786. And probably more?—They cannot very well move more because *this* is raised country. It is at a higher elevation than the rest. They cannot come *here* because *this* is all sand, and *that* is all sand.

3787. In *this* country they can shift four or five miles?—Yes. The clearings round *these* villages keep them away to a certain extent, but they get along *here* and *there* and start meeting *there*. If you walked along *here* you would probably not see a single fly.

3788. The destruction of vegetation as a purely prophylactic measure for these villagers is what you would suggest?—Yes, if you do not wish to spend too much money on it. *These* are all places where sleeping sickness has been found.

3789. If you could destroy the fly areas, there would be no objection to leaving the game?—No objection at all.

3790. If you isolate a game and fly area and then drive out the game, it is important to find out what happens to the fly, whether it follows its favourite food or whether it remains behind and becomes less

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infective?—Yes, but that is a very difficult thing to do. One of these islands would be as good a place as anything else I should think.

3791. (Dr. Chalmers Mitchell.) As you have here four localised areas of fly and as you would have for the experiment in regard to game to clear an area outside your fence, what would you think of the idea

of spending money simply on clearing out the foci of the flies?—I should think that undoubtedly these centres are the places to clear out instead of making more or less irrelevant clearings with a long fence. That is my idea.

(Chairman.) We are much obliged to you for your evidence.

The witness withdrew.

THIRTEENTH DAY.

Tuesday, 2nd December 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

SIR MACKENZIE CHALMERS, K.C.B., C.S.I. (Chairman).

Dr. A. G. BAGSHAW.

Dr. ANDREW BALFOUR, C.M.G.

Dr. W. A. CHAPPLE, M.P.

The Rt. Hon. the EARL OF DESART, P.C., K.C.B.

Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODGE, Bart.

Dr. P. CHALMERS MITCHELL, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Sir STEWART STOCKMAN.

Mr. A. C. C. PARKINSON (Secretary).

Lieut.-Colonel A. W. ALCOCK, C.I.E., F.R.S., I.M.S. (retired), called and examined.

3792. (Chairman.) I think that you are lecturer on Entomology to the School of Tropical Medicine in London?—Yes.

3793. I do not think you have had any personal experience in Africa?—No, none whatever.

3794. But I have no doubt you are acquainted with the literature on the subject?—Yes, I think so.

3795. What we wanted to ask you to-day was, if you could suggest to us from your general knowledge of entomology, what would be profitable lines to follow in working out the problem before us?—Of course, there is no doubt whatever that there is plenty of evidence and plenty of illustration that if you can get hold of the carrier and destroy it you can suppress the disease.

3796. Yes.—But in all the instances in which that course has actually been followed, exact information has been possessed as to not only the habits but the complete natural history of the insect. In the case of *Glossina*, I still think that we are very much in want of exact observation about its habits, particularly in respect of the places where it pupates. I was struck by this in reading an account of *Glossina palpalis* in a report by Dr. Carpenter, who was working on one of the islands of the Victoria Nyanza. He stated definitely that in the case, at any rate, of *Glossina palpalis*, that species appeared to have certain places in which it had a sort of what may be called a predilection to pupate. I think that is one of the points that should be attended to. There is another point that struck me. Of course, I do not know that this is worth anything, but looking at *Glossina* as an insect it possesses in the third joint of its antennae a most extraordinary sensory organ. That organ is present in both sexes, equally developed. There is nothing like it in any other insect with which I am familiar, and I have examined a lot of insects for the purpose of comparison. I have a sort of notion that that must have some influence, and perhaps an important influence, on the life of the fly. We do not know anything about the function of that organ. I think, in a general way, that what we want now is careful and prolonged observation of the entire natural history of every species of *Glossina*.

3797. Including, for instance, *G. fusca*, which has not yet been incriminated?—Yes, I believe that the

thing should be looked at from every point of view. I do not believe in taking a narrow point of view at all.

3798. You mean that examination of one of the species may throw light on something which helps with regard to the others?—Yes, you may stumble upon something.

3799. To go into detail a little more, how would you set about getting this information? What sort of plan of campaign would you adopt?—I would like to see a little money spent on it first. I do not think you can have anything done without that.

3800. I am afraid not.—I would like to see two or three biologists, not necessarily entomologists, but men who take a wider view, with really trained intelligence, engaged in just simply observing and experimenting with *Glossina*.

3801. That would include laboratory experiments and field work?—Yes, precisely—actual field work.

3802. Is it possible, do you think, to get men of that kind? Are they available?—They would have to be very carefully selected.

3803. The field work would be work of some danger because they would have to work in "fly" areas?—It is possible to get enthusiastic men; such men do exist. I do not believe that the Darwins and Wallaces have completely died out. There must be many young men, men of zeal, who would face anything.

3804. You would want a little more than zeal?—Of course you would—men of adapted taste and trained intelligence. There must be some.

3805. Can you help us a little further? Supposing that you sent out three men, what instructions would you give them? What lines would you start them on?—If I could select the men and were perfectly sure that they were the right sort, I would not give them too much instruction. I would not mind them blundering a little bit.

3806. You would let them loose, so to speak?—Yes, I would let them loose.

3807. If you sent out, say, three men, do you think they ought to be sent to some place to work together, or would you choose areas with different characteristics?—I should like them to be in communication with one another, because there is no doubt if you have three good men that they will all have different ideas, and there is nothing like getting those ideas rubbed together.

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[Continued.]

3808. Would you start them with one fly first, for instance, in a *morsitans* area, and then move them on to a *palpalis* area, and then on to a *fusca* area?—I am not prepared to answer a question like that offhand, because I have not thought over the detail. I have only considered it in the time I had this morning. I did not know until this morning what sort of questions would be put to me, and I came here rather earlier than I anticipated, so that I have not had much chance of looking anything up.

3809. It is rather hard on you, but when you get your proof of evidence, if anything in the way of detail occurs to you, would you kindly fill it in?—I should be most happy to do that.

3810. Or add a short memorandum, whichever is convenient to you?—Yes. I must say that, like Snug, I am rather slow in taking up my part. I should much prefer to have an opportunity of reflecting on what I say.

3811. Do you think that what I may call entomological investigation is likely to lead to helpful results?—Yes, I do. I think that it is at least as likely as any other method which has been proposed, and at least as easy as any other method which has been proposed. I would even go as far as to say that it is, perhaps, more hopeful than any of the methods which have been proposed.

3812. Have you formed this opinion from your knowledge of what has been done in other spheres of entomology? Can you give us any analogies?—Analogies as to the value of entomological research?

3813. Can you tell us of cases where the bionomics of a disease-carrying insect have been investigated, and the result has been a successful dealing with the insect?—I should think that the case of the whole series of events in the history of Panama is a sufficient answer to that. Two diseases certainly have been suppressed for a time, and probably will continue to be suppressed so long as people pay sufficient attention to them, simply from our knowledge of the part played by a specific insect and of the history of that insect.

3814. You refer to *Stegomyia*?—Yes, and the larvæ. Then, again, Havana is a notorious case. That was one of the hotbeds of yellow fever. I have not had time to look up the exact date, but some years ago, though not very long ago, they practically exterminated *Stegomyia* in Havana, and they say they have not had any yellow fever since.

3815. They did it by finding out the breeding places?—Yes. They exterminated *Stegomyia* in the adult and in the larval stage as far as I can remember. Then there is the case of New Orleans. They say there that since they have destroyed *Stegomyia* they have not had yellow fever. Then, again, there is the case of Port Said. At certain seasons of the year dengue fever was rather prevalent, and that has been connected with *Culex fatigans*, the common house mosquito, and it is said by one of the Ross's that since the *Culex fatigans* was destroyed these outbreaks of dengue fever have not occurred. Then, in South Africa, though I have not paid particular attention to the details of the literature, I know that where they have the East Coast fever in cattle, by destroying the ticks or by protecting the cattle from the ticks, and particularly from the tick larvæ, they have controlled the disease. I cannot think for the moment of any other cases, but those seem to me to be concrete examples of the success of the application of entomological knowledge, of course in combination with other knowledge.

3816. Do you think that anything can be done in the way of protecting cattle, for instance, by dips? Do you think it is likely that any dips are to be found which even temporarily would protect cattle passing, say, along a road through the fly area? Is that worth investigating?—I think that everything is worth investigating. If you turn the cattle into fly traps, or do anything of the sort, it is worth while. The problem is a serious one. It is no good not doing things because they seem to be ridiculous. I should try everything.

3817. Are there instances in the case of other carriers where the use of a dip prevents the carrier

from feeding?—Yes, in the case of ticks; but those are stationary parasites. The insect is actually a parasite adapted for the particular station, and if you make that station uncomfortable for it by a dip, you, so to speak, get it all the time.

3818. In the case of a biting fly, do you know of anything which will put the fly off?—You can keep mosquitoes from biting you, certainly. I should think there are things which would keep flies from biting man.

3819. Do you think that that is a line worth investigating?—It is quite a subsidiary line, I think.

3820. Does that come into entomological work or not?—Yes, that would be entomological work—actually trying what things are distasteful to an insect.

3821. By trying and trying you think one could possibly find something distasteful to the insect and not destructive to cattle?—Yes, I think so. I certainly would not despise that suggestion at all.

3822. What you consider most hopeful is finding out the breeding places of the fly with a view to seeing whether it can be attacked there?—Whether it can be attacked there, and also seeing if you can find out if there is anything that attracts the fly, and it seems to me that you might take advantage of that to attract the fly to definite places which you might police. That is the idea that has always been in my mind since I read the report of Dr. Carpenter on the pupation of *Glossina palpalis* in this island in Lake Victoria.

3823. Within your knowledge, is there any other instance of a trypanosome developing inside a fly in the way that we are told that a trypanosome develops inside the *Glossina*?—There is a trypanosome that develops (not a human trypanosome) in South Africa. I believe it is very pathogenic to domestic animals, and it develops in the same way in one of the horse flies—one of the Hippoboscidae.

3824. That fly has never been incriminated as regards man?—It is not known to be incriminated as regards man, and my own experience of those flies is that they do not bite man. They rather stick to their animal hosts.

3825. With regard to cattle, those flies have to be taken into consideration as well as the *Glossina*?—Those flies have a very wide distribution, and the problem is not confined to Africa, either in its investigation or in its application.

3826. (Dr. Chapple.) What points do you think you would require to prove by further entomological research? What things that we do not now know do you think we ought to be able to prove?—That is rather a difficult question to answer. We do not know so very much after all. We know a good deal about *Glossina palpalis*, we know something about *Glossina morsitans*. A lot of random observations have been collected, but I do not know that we have any exact knowledge such, for instance, as we have of the common insect pests of agriculture, or even such insect pests as the house-fly or the flea—the common human flea. I cannot say that I expect any particular thing in that way from investigation, experiment, and observation with *Glossina*, but we should get to know all about it. We should get to know if it pupates in any special places, and that would be most important knowledge. The fact that it has not been discovered is nothing. I could give plenty of similar instances. I was for four years surgeon naturalist on the marine survey ship "Investigator." That was when I was a young man and as keen as most young men are and fairly well trained. I paid attention to special points, but never could get the information that I wanted. Remember, I wanted to find out the development of a group of crustacea stomatopods. I looked for an example with eggs, but I could not find it. I used to break coral rocks to pieces to catch the stomatopod but never got one with eggs, but the other day a man had the luck to stumble upon one with eggs and to find out what I had been wanting to find for years. The same way with the robber crab: I visited an island in the Bay of Bengal, where the *Birgus latro* lives, to ascertain where its

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[Continued.]

larvæ could be found. That was not known in those days. The *Birgus latro*, I ought to explain to the Committee, though no doubt many of them know, is an enormous hermit crab adapted for life on land. It is supposed to eat coconuts, and it is a most interesting creature. Although it is a thorough hermit crab, it is absolutely adapted for life on land. I wanted to know something about the *Birgus latro* and where its larvæ could be found, but I could never get one with eggs. Somebody else by a bit of luck later found out all about the *Birgus latro*. The fact that two or three men have been out to Africa with the intention of examining species of *Glossina* without much result is nothing. You may observe for years and years and get no results, and then suddenly just by a happy chance find out something worth knowing.

3827. I understood you to say that the first thing you wanted was money, and, secondly, a zealous expert?—Pardon me, if you pin me down to anything, I would say a dozen zealous experts.

3828. Given an unlimited supply of money and an unlimited number of experts, what instructions would you give them? You would obviously have to tell them what you wanted to know?—I should presume that they knew something about the problem, or else they would not fulfil my condition at all. They should be trained.

3829. The end that we have in view is to stop the spread of sleeping sickness, of trypanosomiasis amongst animals, and trypanosomiasis amongst men in Nyasaland. Now, if we were to recommend the Colonial Office to spend money, the first thing they would ask is, "What do you want to find out?" Will you tell us for our guidance what specific proposition you would want to prove in order to enable you to advise something along the lines of preventive measures in Nyasaland?—That is a terrific question to answer offhand. I could answer it if I had some time to do it, but there are a lot of things to find out. One thing that we do not know anything about is the possible increase or decrease in virulence of the trypanosome on passing through specific tsetse-flies. There has been practically no experimental work to that end. This is rather outside my line, because although I am a general zoologist and have done something besides entomological work, I am now devoting myself entirely to entomology from the practical standpoint of medicine. Down at the school—and everyone there knows it—some years ago there was a case of sleeping sickness in the hospital, and rats infected from that case direct while it lived used to die in a few days. That strain has been kept up in rats, and Dr. Wenyon says that it takes three months to kill them now with the same strain that has been passing through rats. We have an analogous case in the virus of smallpox, whatever it is. It is known that in the cow it is almost nothing. In the human being it is a serious disease. That is one aspect of the case. There are all sorts of aspects of the case. I would not justify myself in any way if I were making a request. I would say that the sum of 100,000*l.* would be very well spent merely as a business proposition.

3830. Do you think we know enough now to give any practical advice whatever along the line of prevention?—Yes, I think we know something. From the entomological standpoint, you mean?

3831. From the standpoint of the prevention of the spread of the disease?—I would rather confine myself in talking about prevention to the entomological standpoint. We know something, I think, but whether it is applicable to the conditions in Nyasaland I do not know, because I have never been there, and I do not know anything about it. All the information collected by Dr. Bagshawe and most clearly put together in his pamphlet, as to what is known about sleeping sickness in relation to *Glossina palpalis*, shows that you can control that species, at any rate to a certain extent, by clearing and generally bringing the land under cultivation.

3832. This is my last question: pending further investigation, are there any practical steps that you think ought to be taken for the prevention of the

spread of the disease or for its elimination?—I think that what we want is more observation and experiment.

3833. You can advise no practical step to be taken pending that further investigation and experiment?—No, I would not go so far as that. I would say let us take such practical steps as can be taken. I am not in favour of doing nothing just because you cannot get a policy of perfection ready to hand. Life is a compromise after all.

3834. (Sir William Leishman.) You gave just now an illustration when the Chairman asked you of another disease or other diseases which have been controlled by a true knowledge of the habits of the insects, and you confined yourself principally to the mosquito, did you not?—Yes, principally.

3835. Does anything else occur to you in the way of similar knowledge in connection with an insect which produces its young in the same way as *Glossina*?—That is the difficulty. I do not know of such a case.

3836. Is that peculiar method of pupation confined to *Glossina*?—No it is not so confined, even among flies. There is another considerable family in which it is the ordinary method of reproduction.

3837. You do not know of any pests that have had knowledge applied to them in this way—pests on plants or insect pests of the sort which distribute their larvæ widespread in different places?—There are lots of cases known among arthropods of not precisely that method, but of methods where the young is nourished and even attains its adult form inside the mother. There are some of the mites, for instance, where that occurs.

3838. Where that does occur, do you know of attempts made to destroy the pest on a large scale, which is the sort of problem we have to face with *morsitans*?—I quite realise that that is the difficulty.

3839. You do not know of any case?—No, I do not know of any case.

3840. With regard to this peculiar development of the sensory organ which is found in the antennæ of the *Glossina*, you said, as I understood you, that you had no idea having regard to similar organs in other insects what the function was?—If you go back to first principles altogether, no one can have any idea, but what I said was that no one has any idea what use these organs are to *Glossina*. That is what I intended to say. I would not like to open up any large metaphysical problems.

3841. I did not mean that. Have you done work yourself by testing the olfactory sensation, for instance, of an insect?—No. I have not with regard to insects.

3842. It has been suggested to us that some of the *Glossina* may be attracted to certain particular sites to lay their pupæ on account of some curious smell or odour which may reach them. Have you anything which might throw light upon that?—No. I would merely point out that you have two facts—that they are attracted (if this is a fact), and that there is certainly a sensory organ whose function must be very powerful in some way.

3843. It is possible that the sensory organ may be an olfactory organ?—Yes, it is quite possible that it may be.

3844. It should be possible to determine that by experiment?—I think that is one of the things that might be determined by experiment.

3845. It would be necessary to mark the *Glossina* in some way if one was studying the flight?—Certainly it would be.

3846. If you had to tackle it, how would you mark such fly as *Glossina* for identification?—Well, I think I should look at the experiments that have recently been made by several men working for the Local Government Board to find out what they have done.

3847. In regard to house-flies?—Yes, in regard to house-flies. I believe that it is quite easy to mark them so as to recognise them afterwards.

3848. It is not a thing which you have studied personally?—No, I have not studied it personally.

3849. How would you personally group *Glossina* together with regard to their habits? What is an

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[Continued.]

appropriate grouping? We know that *palpalis* differs very much from *morsitans* in its breeding places and in its fondness for water and so on. I should be very glad if you would tell me what other *Glossina* you would group in the same way, either on the *morsitans* side or on the *palpalis* side or any other grouping that you suggest?—From the point of view of suspecting the fly?

3850. Yes.—I should look upon the very common fly called *Glossina tachinoides* as merely a variety of *palpalis*, and I should think the distinction between *Glossina morsitans* and *Glossina longipalpis* and *Glossina pallidipes* is very slight. They are specifically distinct, but the distinction is very slight indeed.

3851. *Fusca* you would put in a different group?—Yes, I should think so, morphologically certainly.

3852. But you have no personal knowledge of the bionomics beyond that?—No.

3853. (Dr. Chalmers Mitchell.) Following what you said to Sir William Leishman about these curious sensory organs, how would you proceed—by snipping them off I suppose. Could you do that in the case of flies?—It upsets an insect a good deal to have its antennae snipped off, but it can be done and it recovers afterwards.

3854. How would you proceed?—I do not want to commit myself. I would like to have a little notice of that question. Please understand that I came here rather under protest, not having any first-hand knowledge.

3855. Anyhow, if you had time to think it over, do you think you could devise a way of throwing out of gear those sensory organs?—Yes. Plenty of similar experiments have been made. I would look them up.

3856. Having got flies with the sensory organs thrown out of gear by some method, you would then compare their response to different stimuli?—I have simply thrown out the suggestion and not reflected on it. It is rather too much to ask me what I would do. I should certainly look up what has been done before I answered.

3857. I understand that it is impossible to give in detail how you would carry out a piece of research like that. The general idea would be, I suppose, to establish some method of comparison which might put you on the track of a bait or lure for a fly with regard to a particular locality?—Yes, that is the sort of idea I have in my mind.

3858. By making use of flies in the normal condition and in the abnormal condition with the sensory organs thrown out of gear you might get on the track?—I should hope to see if there was a track.

3859. Do you happen to have worked upon or followed closely the history of the trypanosome inside the fly?—Not in a fly. I have worked with the common rat trypanosome in the rat-louse.

3860. But not in flies?—No.

3861. (Mr. Rothschild.) In reply to Sir William Leishman and the Chairman, you mentioned the sensory organ and you stated it was present in both sexes. Therefore, it is evidently employed in some of the functions of the life of the fly, apart from sex. I happened this morning to be at work with some sensory organs on the antennae of some moths from New Guinea, and these appeared to be entirely confined to the male sex. I am not referring to the ordinary sensory branches of the antennae. These were big organs situated about half-way down and consisted largely of scent-producing scales, and evidently were destined either to attract the female or to discover the female. Do you consider that in the case of *Glossina* this organ, being present in both sexes, is meant for the purpose of discovering something outside the fly itself?—I should hardly think that it has anything to do with any specific sexual function, because as far as I can make out (I do not get the very best preserved material, but I have examined the thing very carefully) it is identical in both sexes, equally well developed and apparently composed of the same elements. I should say that it is something that is essential to the fly, both sexes alike.

3862. Supposing that a young and ardent biologist is detailed to examine into a history of the fly, do you not think that he ought to test this organ by seeing

whether it is used by the insects for discriminating odours specially?—Certainly. I think that would come obviously within the purview of his investigations.

3863. The question of the attraction of the colours seems to play a certain part in the life history of the *Glossina*, because on the islands of Principe and San Thomé they have captured tsetse-flies by hanging out black cloths.—Yes.

3864. Do you think that has anything to do with the fondness of the insect for the buffalo, and that the moving black cloth has somewhat the same appearance as the hide of the buffalo, or do you think that it is merely the colour attraction?—So far as I have thought of that subject at all, it seems to me that many of these blood-sucking insects have a preference for dark colours. House mosquitoes certainly have a preference for a black coat and dark clothing; certainly sand flies do, both the *Culicoides* and the *Phlebotomus*. The preference for dark clothing seems to be something of almost wider application than to *Glossina*.

3865. You also told Sir William Leishman that you did not know any other insect among the Diptera which produces its young in the same way as the *Glossina*. Do not all the Hippoboscidae produce their young in the same way?—Pardon me. The question, as I understood it from Sir William Leishman, was whether there is any other insect that is known to harbour a trypanosome pathogenic to man. I have already mentioned that there is a *Hippobosca* well known to infect cattle in South Africa, and likewise a large family of the Hippoboscidae produce their young in the same way, as you say. I am afraid that I did not express myself quite clearly.

3866. (Sir Stewart Stockman.) I think you suggested the advisability of trying dips and dressings for keeping insects off?—I did not recommend it; I said I should not despise any method.

3867. I suppose you would agree that that has been very freely tried in connection with a great many pests?—Certainly, stationary pests.

3868. To keep off other pests?—I am not aware of it. To keep off *Simulium* and *Tabanus* and things of that sort.

3869. The maggot-fly, for instance?—Yes.

3870. Would you agree that there has been rather a failure in that respect?—I do not know anything about it.

3871. It has been before us for years, and almost everything has been tried; and with regard to keeping biting insects off, we have to admit that it is rather a failure. I want to know if you have any opinion on the matter?—I know that you can keep mosquitoes from biting you in India.

3872. Successfully?—You certainly can. I used to make a mixture myself for keeping off mosquitoes. It was a great favourite with the ladies in Calcutta—essential oil and something bitter.

3873. I have never kept them off by means of dressings.—Some people are more attractive than others.

3874. (Sir Edmund Loder.) I would like to ask you a personal question. I would like very much to have a recipe to keep mosquitoes off. I am horribly bitten by mosquitoes.—I will certainly give you that with great pleasure.

3875. Is there anything that really does it?—I think, in fact I know, that a proper mixture of turpentine spirit and quinine mixed with a little something to make it smell nice, like oil of bergamot and, of course, water, makes quite a nice wash that certainly the *Culex fatigans* in Calcutta does not like.

(Sir Edmund Loder.) I have made myself smell very nasty several times, but the mosquitoes have bitten me very hard.

3876. (Dr. Balfour.) Have you ever studied the natural fungus diseases of insects?—No, I have not. I hope to do so now that we have an insect farm fitted up at the school, but so far I must plead ignorance.

3877. I suppose it is one of the lines that you would recommend in the study of *Glossina*?—No, I should not have thought of it. From the habits of the *Glossina*, I do not see how you are to get the fungus to attack the *Glossina*.

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[Continued.]

3878. Suppose you fed the flies artificially, as the Belgians have been able to do, and seeded the blood tubes with the spores of a fungus, do you think one fly could spread the disease to another? A witness suggested that it might possibly spread by the sexual act. Do you know of any analogous case that might throw light on this question?—I do not quite see how it would spread. The answer that I prefer to make is, that all these are questions that I think could be answered if we only knew more about the insect.

3879. It might be attended to among other points?—Yes, certainly. I would attend to every point. Every line of investigation that can be thought of and that is at all reasonable I would follow.

3880. You have mentioned the Hippoboscidae. We know that in some parts of Northern Africa and in India Tabanid flies convey the trypanosomiasis of mammals, or we believe they do, as apparently there is nothing else that can do it. There used in India among natives to be a prevalent idea that the *Tabanus* was the cause of surra. I have not paid much attention to that.

3881. It is admitted in Algeria, the Sudan, and India that the surra of camels can be carried by these flies?—I did not know whether the *Tabanus* was excluded from other flies.

3882. Have you any idea what rôle that kind of fly may play in *Glossina* areas as carriers of the trypanosomes of domestic animals?—It is only possible to answer in general terms that almost any blood-sucking fly would be capable of mechanical transmission.

3883. That is what I wanted to know. Do you think that it would only be mechanical transmission? Is it possible the trypanosomes might undergo cyclical development in these flies as they do in the *Glossina*?—I do not see any impossibility about it.

3884. Do you think it is another line which should be investigated?—I think that in this particular instance we must be looked upon as being fairly on the broad road, and that we can exclude other flies in the case of this particular trypanosome.

3885. But with regard to the other trypanosomes of domestic animals?—I am entirely opposed to shutting my eyes to the possibility of infection.

3886. Do you think that these Tabanidae possibly might play a rôle in connection with the pathogenic trypanosome that we are discussing? Have you ever met anything that would lead you to suppose that they may?—No, I cannot say that I have.

3887. (Dr. Bagshawe.) There is one question I would like to put to you, but perhaps you would not consider it quite suitable. It has been proposed to make an experiment in local game extermination in Nyasaland in order to study what effect that has upon the fly. If this experiment is properly done, it will obviously cost some thousands of pounds. I would like to know whether you think that results commensurate with the expense would be attained, or do you think that better value would be got by spending that money on entomological research?—That, again, is a difficult question. I must say that I like to see experiments performed. I do not think one can do any harm by experiments if they are done by trained people and properly done. Although I think that at present we want to have really accurate knowledge about the flies, at the same time I think that the other experiment is all right; I can see nothing against it on any ground except the expense. It is a little bit outside my line, but I have heard recently—and no doubt the Committee are equally familiar with it and will pardon me for mentioning it—that on the island of Formosa the Japanese dealt with their wild men by encircling them in a line of live wire. I think that that would be a very cheap way of isolating your experimental area, and would be better than a fence. If expense is the only point, there must be a source of electrical energy in the African continent where *Glossina* occurs. You could get any amount of electricity, I should think, fairly cheaply. So far as I can express my own

opinion, I should like to see some sort of experiment as an experiment. If expense is the only objection, then I think it would be just as well to inquire into the expense of that operation of the Japanese. The man who told me about it and who has recently come back from Formosa, says that it costs very little and is very efficacious. Incidentally he mentioned that although it was meant to keep the wild men out it killed the deer and things wholesale.

3888. But we still have the difficulty of the movement of flies from one place to another?—That would have to be part of the experiment. You would have to stop that. You would have to take measures to prevent flies getting into that isolated area, but I assume that that can be done if you have a sufficiently broad belt of clearing, and if that belt is patrolled in the same way as they patrol the forest clearings in India.

3889. What do you imagine the breadth of that belt should be?—It is generally understood, I think, from your own observations on this point that a *Glossina* can fly more than a mile, so that it should certainly be not less than a mile. It should be somewhat more than a mile in breadth.

3890. You are aware of Mr. Fiske's ideas? Mr. Fiske from a study of the literature has arrived at the opinion that probably flies leave the islands in the Victoria Nyanza and possibly reach the mainland, and I have figures in my possession which would very much support that hypothesis. Supposing that were proved it would show that *palpalis* could go four or five miles, I think?—If that were proved, then I should suppose that that would make the expense of any experiment of the kind proposed absolutely prohibitive.

3891. How did that paper strike you from your entomological knowledge?—Well, I am afraid I am getting now to this: that I rather distrust too much *a priori* work in anything, and in zoology, I think, it is more than in most subjects rather out of place. I think it is much better in the case of *Glossina* to get to know something more about the habits and natural history of the insect. I should be rather afraid of speculations which are only founded on insufficient data. I look upon them with a certain amount of suspicion, because if they are not justified they rather discredit scientific evidence in general. I feel very strongly on that point.

(Chairman.) Thank you very much.

The witness withdrew.

Written note sent in by the witness subsequently to his oral evidence.

With reference to Questions 3808–3810: on consideration, I should like to see four men appointed—two to work together in West Africa, and two to study *G. morsitans* together in Nyasaland.

There is a great advantage in having men at work in couples, provided that one is officially senior to the other. A man working alone, far from any possible assistance, is often—as I know from my experience in the R.I.M.S. "Investigator"—distracted as new opportunities suddenly appear when his hands are already full. Besides, there is the stimulating effect of discussion.

The men should be good all round—not merely entomologists. I should prefer them to be young doctors with a University degree and a strong liking for natural science. I am a firm believer in the solid advantages afforded by a medical education. Moreover young doctors can be left to take care of their own health.

The only instructions that I would give them are (1) that they should make an exhaustive study of *Glossina*, in the field, in the laboratory, and in the insect-house, but particularly in the field; and (2) that they must resolutely ignore all other biological problems, however interesting, unless they have some reasonable bearing on the ultimate problem of the control of *Glossina*.

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[Continued.]

Mr. G. GARDEN, M.R.C.V.S., called in and examined.

3892. (Chairman.) I think that you are a veterinary officer in Nyasaland?—Yes.

3893. For the whole of Nyasaland, or have you assistance?—The whole of Nyasaland. Until a few years ago I have been the only veterinary officer there.

3894. How many years have you had in Africa?—About 12 altogether.

3895. Most of it in Nyasaland, or not?—In South Africa, West Africa, and Nyasaland.

3896. How long have you had in Nyasaland?—Three years.

3897. Have you been studying trypanosomiasis in animals?—Not minutely. My work has prevented me from doing so. I have been engaged in veterinary administrative duties and have not had the opportunity of studying that disease minutely.

3898. Not of doing research work?—No.

3899. What generally is the nature of your duties?—Travelling about, diagnosing cases of disease, and in general dealing with outbreaks of disease.

3900. Do you take any active steps for dealing with disease? Do you order the destruction of animals or moving of animals or anything of the kind?—Chiefly the moving of animals from infected areas or from disease.

3901. I think you can give us some information about the Blantyre-Zomba Road?—Yes.

3902. Have you found disease there?—Disease is very prevalent there.

3903. Is that the ordinary type of nagana?—I would not like to say. I have referred that to Sir David Bruce, by forwarding to him small animals inoculated from cattle suffering from trypanosomiasis.

3904. What do you say about the amount of tsetse-fly where you find disease?—Where the disease is found on the Blantyre Road tsetse-fly has not been found, except in one instance where two flies were found on the verandah of one estate.

3905. But do you mean that the country round is free from tsetse?—No. From 10 to 15 or 20 miles distant tsetse abound.

3906. Were the cattle taken into the tsetse district, do you think, and did they get infected there, or how?—No. It is my opinion that the tsetse-fly have been brought to the cattle—stray flies.

3907. Stray flies so few in number that they would hardly be noticed?—That is so.

3908. You do not think there was any other carrier of the disease except tsetse?—As to that I would not like to give an opinion; I am doubtful.

3909. Do you think it is possible?—I think it is possible, once the disease is there. I am inclined to believe that direct inoculation by other flies is possible.

3910. At any rate, you were struck with the curious absence of fly from the places where you found a considerable amount of disease?—Yes, that is so.

3911. What do you say about the presence of tsetse-fly, speaking of *morsitans*? You are speaking of *morsitans* all along, are you not?—Yes.

3912. What do you say about the presence of *morsitans* along the Shire watercourse?—That is where I maintain infection comes from to the Blantyre-Zomba Road.

3913. How far would that be away?—The river winds considerably and so does the road. It would average 18 to 20 miles from the road to the river.

3914. Are there no tsetse-fly nearer than that?—Not that I am aware of.

3915. Have you been much over that ground?—Yes, I have been over it a good deal.

3916. Was your attention called to the fact that tsetse-fly were not found there, or were you studying whether tsetse-fly were found there? Was your attention alive to the absence of the tsetse-fly?—Yes.

3917. And were you investigating that?—Yes. That is one point that has struck me all along since I have gone there to work, the absence of fly in one's travels through a district where the disease is present among cattle.

3918. The animals not having been moved?—The animals not having been moved except working on

the Zomba-Blantyre Road, or grazing on the side of the road, probably 4 or 5 miles from the road.

3919. Now you suggest increasing our knowledge by certain field experiments?—Yes.

3920. Where would you carry those experiments out?—In the district between Zomba and Blantyre.

3921. What would they consist of? What would you suggest doing in practice?—In the first place I would like to prove the points where infection can be contracted, and this could be done by placing several herds of cattle at different points along the road and keeping them under observation for a certain time to see if they become infected.

3922. In what you call practically fly-free areas?—Yes, in supposed fly-free areas.

3923. May I ask you this about fly-free areas: as you pass from the road through the fly-free area towards the fly district, do you begin to find scattered flies or do you come upon flies, so to speak, in a mass suddenly?—I have never experienced them in a mass except near the river. You come into them gradually. The Government entomologist also has found them in considerable numbers down towards the Shire river.

3924. But the entomologist has not found them on the Zomba-Blantyre Road?—No.

3925. And he has looked for them?—He has looked for them, and I have looked for them there.

3926. At different times of the day?—Yes, at different times of the day. I have had boys out with nets and so on for months at a time.

3927. And never succeeded in catching fly?—Never succeeded in catching tsetse-fly in that particular area.

3928. Your experiment there would be to put two or three herds of animals at different places along the road?—Yes.

3929. Would you have those animals watched to see if any fly came?—They could be watched, but I doubt if it would be practicable.

3930. What is the question the experiment would answer?—I would wish to prove the points along the road at which infection can be contracted, so that we could in the meantime advise stock owners to avoid those points while working on the road with their cattle.

3931. Would that in any way give you the distance of safety?—I think it would.

3932. What do you say about an experiment that has been suggested to us by a good many witnesses—that of destroying game in a particular place?—The next thing that I should like to see carried out in the same local area would be the destruction of game and then observing if the disease would disappear from the Blantyre Road.

3933. Would you give us a few more details? How would you carry out that experiment?—It would require to be thought out a little better, but roughly by a general relaxation of the game laws. I would permit the natives to destroy game.

3934. On either side of the Blantyre Road?—On both sides of the Blantyre Road. I would permit them to destroy game without fear, and probably a few capable natives would be armed with rifles. I would also allow Europeans free shooting in that area. I am confident that if they could not exterminate the game altogether, they could keep it from approaching the high lands.

3935. As regards this fly-free area on each side of the Zomba Road, do you find game in the fly-free area, or not?—Yes, you find it there, but not in great numbers. They approach there at certain seasons of the year.

3936. But at other seasons of the year, are the sides of the road free from game?—They are practically free from game, except the smaller antelope, the ordinary duiker, reed-buck and bush-buck. At certain seasons of the year I have known of eland within two miles of the road, 19 miles from Zomba, and I have also known of buffalo within three miles of the road and 2½ miles from Zomba.

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[Continued.]

3937. Do you think it is possible that the fly from the fly areas are carried by or follow the game into the fly-free area?—It is my opinion that they follow the game up towards the hills, and then attach themselves to natives and are carried further on towards the grazing ground of cattle.

3938. You think there is a human carrier from the game to cattle?—To the cattle areas.

3939. You have a doubt in your mind as to whether the disease, having now been introduced, may not be spread otherwise than by the bite of the tsetse-fly?—I am doubtful on that point.

3940. You do not think that for the purposes of the experiment you suggest, it will be necessary to fence out cattle and have a clearing outside the fence?—I do not think so. I think that fencing would be out of the question for that part of the country.

3941. For what length does the Zomba-Blantyre Road run?—The distance from Zomba to Blantyre is 41½ miles.

3942. On each side is there a fly-free belt?—There is a supposed fly-free belt.

3943. And beyond that do you find the fly all along?—No, not so much on the west side of the road. It is more on the Shire watercourse.

3944. (Chairman.) I will not ask you about the dipping experiments you suggest because I will leave that to Sir Stewart Stockman.

3945. (Mr. Read.) What is the height above sea level of the Blantyre-Zomba Road, roughly, at its highest point?—It would vary from 2,600 to 2,800 feet above sea level. I cannot vouch for the accuracy of that.

3946. Do you think that the altitude has anything to do with the scarcity of tsetse-fly?—I am inclined to believe that altitude has something to do with the scarcity of tsetse-fly.

3947. This particular road is being metalled, is it not, at the present time, so as to make it suitable for motor traffic?—Yes.

3948. Coming to more general questions, I think that we have a fairly clear idea here as to the position with regard to human beings. I think that most of the Committee are convinced that the mortality is extremely small, as at present ascertained at any rate. Now can you tell us something about the mortality among stock? In the first place, is Nyasaland a stock-raising country or is it, as we rather understood from Sir Alfred Sharpe, a country where a certain amount of stock is kept in a comparatively small district, the Angoni district, while in the rest of the Protectorate the people are for the most part agriculturists. Is that an accurate description?—I would not like to agree. I believe that the northern portions of Nyasaland, more especially Mombasa's country, North Nyasa and part of the Marimba district, are quite cattle districts.

3949. Is stock kept there at the present time?—In great numbers.

3950. Is there any fly there?—Yes, there are fly all along the watercourses leading down to the lake.

3951. In those districts, is the fly a distinct hindrance to the development of the stock industry?—It is in parts of the district.

3952. Is it a serious hindrance?—Yes, I would class it as serious inasmuch as I am aware of several chiefs who have shifted large herds of cattle numbering, probably, two or three hundred head, in search of better farming ground towards the borders of Northern Rhodesia. Probably seven or eight years ago, they shifted them, and then the ground was free from fly and the cattle multiplied and did well; and now I am aware of several herds having been completely blotted out through tsetse-fly. They cannot keep cattle in these parts now.

3953. With regard to the southern part of the Protectorate, what is the position there? That is, I take it, for the most part agricultural. I refer to the lower Shire districts and that country.—I would place it on a different level to the northern districts with regard to cattle raising. Cattle there are of more value than they are in the northern portions. On account of the scarcity of labour the planters are all com-

mencing to take out implements and work their estates with cattle. Cattle are not bred in large numbers in the southern districts, but they are of great value in the southern districts for agricultural purposes.

3954. Has the position with regard to stock generally throughout the Protectorate grown worse in recent years; I mean to say, has some great and sudden calamity come upon the people with regard to their stock, or have they been merely subjected to the ordinary drawbacks of keeping stock in tropical countries?—Citing one or two instances near headquarters, Zomba and Blantyre, some six or seven years ago cattle transport used to be plied between Zomba and Blantyre on a large scale, but now you can scarcely get anybody to put cattle on the road. They are afraid to go through, because they will lose a few of them every other journey. One planter, Mr. Hooker, who used to reside in Zomba, in the space of three or four years lost between two and three hundred head of valuable cattle on the Zomba-Blantyre road. I would not like to give the exact number. The African Lakes Corporation, the biggest trading company in the country, have lost a great many. They are now unwilling to put their cattle on the road. I am frequently asked by the African Lakes Corporation, Limited, British Central Africa Company, and other trading firms if they should risk working oxen on that road.

3955. Can you quote any other instances?—Magomero, the A. L. Bruce Estates, Limited, and Mr. Willard James of Blantyre.

3956. In all these cases there has been a somewhat serious loss of cattle?—A considerable loss.

3957. You are well acquainted with Southern Nigeria. You were there for some time?—Yes.

3958. Were you in any other parts of West Africa?—No, in Southern Nigeria only.

3959. How does the position in Nyasaland compare with the position in Southern Nigeria? I had an impression that they have practically no stock at all in the latter country. Is that correct?—No, they have a large number of stock in Southern Nigeria.

3960. On the coast?—Not exactly on the coast, but a little way inland.

3961. Would you say the position there was better or worse than in Nyasaland?—I would say that it was worse in Southern Nigeria than in Nyasaland.

3962. It has been urged that the game laws should be relaxed so as to allow the natives greater freedom in the shooting of game. I have turned up a dispatch published in a Blue Book which I have quoted from before. I see that Mr. Harcourt wrote on the 6th September 1912 to the following effect: "The depredations of big game to which Sir D. Bruce refers in the fourth paragraph of his letter also show that the natives require some further liberty in the matter, and I consider that they should be allowed to use the weapons which they now possess in killing game in infected areas. I request that you will take steps to give them the necessary permission." Have you any reason to suppose that the permission has been withdrawn?—I do not think so, in the infected areas.

3963. In the infected areas the natives have permission to shoot freely?—I understand it to refer to human trypanosomiasis, and the area is limited to the Domira Bay area only.

3964. The Dowa district?—Yes.

3965. There I am told that not only natives, but Europeans, too, are allowed free shooting—that anyone can go in and shoot as much as he likes?—I do not think that is so, but I would not like to be positive on the point.

3966. I asked Major Pearce about it and he told me so. However, he will be coming before the Committee, and we can ask him about that. At any rate, the permission, so far as you know, has not been withdrawn?—It has not been withdrawn with regard to human trypanosomiasis.

3967. (Dr. Bagshawe.) Have you noticed any seasonal differences in the number of mousitans in the fly area, which I understand is below the Zomba-Blantyre road?—No, I could not say I have. I have

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[Continued.]

gone through fly belts at all seasons of the year, and I have met with fly to my mind, in equal numbers.

3968. With regard to trypanosomiasis in stock, are there any other serious diseases of stock in Nyasaland?—We have got East Coast fever in the northern districts of the Protectorate, and ordinary redwater. Apart from that I do not think we have any other serious disease.

3969. What I really wanted to ask you was, which disease you consider the most serious of cattle diseases?—I would consider trypanosomiasis the most serious.

3970. You know what is called the Domira road, the road which goes from Domira Bay towards Fort Jameson?—Yes.

3971. Are you well acquainted with it?—I cannot say that I am well acquainted with it; I have travelled over it, but I am not well acquainted with it.

3972. How many times have you travelled over it?—Only once. I cannot say that I am well acquainted with that area, because there are no stock down that way to take me there. I have been over it; that is all.

3973. You could not tell us whether there is much game to be met with on each side of that road?—No, I could not. I could tell you only from reports.

3974. Not from your own knowledge?—Not from my own knowledge.

3975. What have you heard from report?—That game is plentiful.

3976. And fly plentiful also?—Yes.

3977. (Dr. Balfour.) In the particular area of which you spoke, where you were unable to find tsetse-flies, were your investigations carried out during both the dry and the rainy seasons?—Yes, during both seasons.

3978. So that fly were absent during both seasons?—Yes.

3979. Have you any idea as to the part that other biting flies, apart from *Glossina*, may play in transmitting the trypanosomiasis of domestic animals in Nyasaland?—No, I would not like to say.

3980. You have no evidence at all upon that point?—No, I have no evidence.

3981. (Sir Edmund Loder.) You suggested putting cattle along the road to see whether they got bitten by fly or got disease. If the experiment was carried out it would not carry the Committee any further with regard to the rest of Africa, but would only be an experiment for that one road or that one district?—If you wished to go further I would give you more ground to work on.

3982. The idea is to find the infected places in order to warn cattle people to avoid them. But that is only a local thing?—Yes, but I would not like it to stop at making sure of the points of infection.

3983. It would not help us much, would it?—I cannot say.

(Chairman.) You mean that it is a practical experiment, not a scientific experiment.

3984. (Sir Edmund Loder.) Yes, for that road. (To the witness.) Now, with regard to shooting down the game, no doubt the natives would make a good hole in the game if they were allowed to; but how would you prevent their coming back again? As soon as the game got scarce the natives would not care to go on hunting them?—If you allowed free shooting there, as soon as an animal was seen by the natives it would be hunted.

3985. Black, white, or any people will shoot if they can get a day's shooting; but when people have to go four days without getting anything they tail off, and the animals would come back and the experiment would be spoilt?—The natives would not get tired; they are always ready to hunt buck.

3986. You are of opinion that it could be kept cleared?—I think that it could be cleared in that area.

3987. I think not. When game gets scarce people leave it alone, and there are always some left and that will vitiate the experiment.—I do not think so.

(Sir Edmund Loder.) Your opinion is just as good as mine. Let it stand.

3988. (Sir Stewart Stockman.) In answer to the Chairman you said that possibly trypanosomiasis might

be spread by direct contact. I take it you meant that it could be mechanically carried by other flies?—Yes.

3989. Not by contagion?—No, but by other flies.

3990. You know there is an opinion that the fly follows game. What is your opinion about that?—My own opinion is that it does.

3991. With regard to *morsitans* would you say that if you drive away the game the fly will go too?—I am inclined to think that it will.

3992. Can you cite any cases, say in Swaziland or any of the places you have been in, where it happened that the game went and the fly followed?—No, except in the Lobombo range down on the flats, when the game were blotted out during the early rinderpest days.

3993. When you were in Africa the rinderpest days were over practically?—Yes.

3994. Is this within your own observation?—No, it is not within my own observation.

3995. It is still the same?—Yes.

3996. Could you amplify your report with regard to the trypanosomiasis on the Namadidi estate?—It commenced about two years before I went to the country, 1908 or 1909. There were only a few cases to commence with and those only among the work oxen working between Zomba and Blantyre. The medical officers prepared blood smears and declared the disease to be trypanosomiasis. On my arrival I carried out a large number of examinations and corroborated their opinions.

3997. Did this go on increasing from week to week as it were?—It went on increasing. At first it only attacked the work oxen. Then in 1911 it attacked animals that had been bred and born on the estate and had never been off the estate—cows and heifers.

3998. Was it the continual supply of flies brought up, in your opinion, that kept this up?—Yes. That is how the infection was carried to the estate, in my opinion.

3999. Carried by natives?—Yes. There are regular footpaths up from the Shire river right to the estate, and on the verandah of that gentleman's estate the two tsetse-flies that I spoke of were caught.

4000. Did any of the natives, as far as you know, contract sleeping sickness?—Not that I am aware of.

4001. Now with regard to dipping experiments, altogether you have had plenty of experience of dipping to destroy pests like ticks. What is your opinion about dipping (every kind of material has been tried I think) as regards keeping off flies or ticks?—I really cannot say that I have an opinion to offer on it, but I should like to see it tried.

4002. Do you think it is hopeful?—I think it is hopeful.

4003. No doubt you have carried out many dippings in Africa for tick diseases. Do you think they keep them off for any time?—Yes.

4004. How long do you think they are kept off?—Ticks?

4005. Yes.—In a very thick area it did not keep them off longer than three or four days.

4006. If dipping was to be a success you would want to dip your cattle every three or four days?—Yes.

4007. That, I take it, is possible in some of the districts?—Yes.

4008. Now there is one other matter I want to ask you about, and it is this: We have had witnesses here who suggest, probably rightly, that the resistance of natives to sleeping sickness is due to generations, as it were, of exposure to the disease. I want to know whether you know of any native herds of cattle which have acquired immunity to these diseases?—We had an outbreak among the Public Works Department oxen in 1911. Eighteen animals were proved by microscopic examination of their blood to be infected. Fifteen of the eighteen died, and, so far as I am aware, the other three are alive to-day and look healthy.

4009. That is an example of individual insusceptibility, as it were, but do you know any herds of cattle that are immune to trypanosomiasis?—No.

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4010. Do you know herds of cattle that are immune or salted to redwater?—Yes, and I have known of herds comparatively immune against East Coast fever.

4011. But not against trypanosomiasis?—No.

4012. With regard to this experiment, how far from the fly area is the road that you say cattle get infected upon?—It would vary from 18 to 20 miles.

4013. You say that the game come up to the high land. How far do they come?—They come up within 4 or 5 miles of the road sometimes, on to the ridges.

4014. If the fly gets on to the road then it travels at least 5 miles without any game?—Yes.

4015. Do you think that is possible?—I think it is possible. I have carried fly myself on my back for a greater distance.

4016. But I mean the fly without anything to carry it?—I would not like to say that it would travel that distance by itself.

4017. You cannot explain how it came on the road?—No, except following natives.

4018. (Mr. Rothschild.) I heard, of course, what you answered to Sir Edmund Loder about the object of the experiment. I think I understood from your answer to the Chairman that one of the chief things you wanted to find out by putting the cattle along the road was how far the fly migrated of its own accord and, therefore, how far away from that portion of the road you would have to keep the cattle, or lead the cattle for safety. You thought that the fact of finding out the distance the fly travelled would be one means of knowing what portion around the experimental area ought to be cleared?—No, I do not think I said that, or if I have communicated that view I would like to withdraw it. It was not with a view to clearing at all.

4019. Perhaps clearing was the wrong word. I understood you to mean that by placing the cattle at different distances along the road you wanted to find out how far the fly flew?—No, that was not my point. It was merely to prove where they got infected. I would not confine them to one place; that would not be my intention. I would have the herds placed at known distances from the Blantyre-Zomba Road, and after one point had been proved infectious or otherwise, remove the animals to another point.

4020. You told us that you were in Southern Nigeria. Were you ever in any other part of Nigeria? Do you know Bornu at all?—No.

4021. I know that they keep a peculiar breed of cattle up there, and I did not know if you knew whether they were proof against tsetse-fly or not.—No, I am afraid I do not know.

4022. We have been told by one of the witnesses that the fly area along the Shire river was practically free from game. From your experience do you know if that is so or not?—It is not practically free from game. Game is probably comparatively scarce in the area between Zomba and Blantyre down towards the Shire river, but you have specimens of buffalo there, eland, a few sable, and the other smaller antelope.

4023. (Dr. Chalmers Mitchell.) You are rather in favour, I gather, of allowing the natives to shoot freely in the fly-infested districts?—Yes.

4024. The object of that being to clear away the game. I had a letter three or four days ago from a highly-placed official in Nyasaland (I cannot go further than that), and he told me that it was heartbreaking just now to see the natives lying round the waterholes and shooting down the eland, and practically touching nothing else. The object of my saying that is to ask you whether you think if the natives were allowed to shoot they would really scour out the game or only pick out the big animals?—To my mind they would clear it out of that area under consideration.

4025. The big things and the small?—Yes.

4026. You have been in Nigeria as well as in British East Africa, and so forth?—Yes.

4027. And part of your work has been to diagnose disease in animals, has it not?—Yes.

4028. Would you mind telling me how you diagnose trypanosomiasis in cattle?—The usual procedure is to draw blood from the ear. In some cases I have taken it from the jugular vein, and if I have my microscope

with me while I am travelling, which I very often have, I examine the fresh blood and also prepare a number of blood slides and fix them in the usual way by putting them through alcohol. I stain them with what I take to be the most common blood stain giemsa. Sometimes I have used Leishman, but chiefly giemsa. I examine these with first a low power, and then the 1/16.

4029. Do you go through all that process to see whether an animal is ill with trypanosomiasis or not?—Not always; sometimes the trypanosomes are very numerous and fresh blood examination is sufficient.

4030. No, but what I mean is that you do not diagnose the disease without the microscope work?—Never.

4031. You cannot do it by clinical symptoms?—I have never done it and I would not like to.

4032. All that you see is the presence of the trypanosome?—Yes, the presence of the trypanosome.

4033. Have you worked with trypanosomes enough to distinguish between the alleged different kinds of trypanosome?—No, I have not. I have not had that opportunity.

4034. So that when you have diagnosed trypanosomiasis in an animal all that you mean is that you have found trypanosomes?—Yes, I have found trypanosomes.

4035. Have you noticed any difference between the trypanosomes in different animals?—No, not in cattle.

4036. Have you adopted the same course with regard to pigs?—I have not had an opportunity of examining many pigs out there at all.

4037. Rats?—No.

4038. None of the laboratory animals?—No.

4039. In reply to one of my colleagues you said that you thought individual cattle were immune sometimes, or that at least they recovered?—Sometimes recovered.

4040. Was that in Nigeria or in Nyasaland?—In Nyasaland.

4041. Did they ever recover in Nigeria?—I was never stationed long enough in Nigeria to watch whether they recovered or not. I was travelling all the time I was in Nigeria.

(Sir Stewart Stockman.) They do recover.

4042. (Dr. Chalmers Mitchell.) With regard to swine of any kind, do you know whether they recover?—I cannot say that I know of swine having had the disease in Nyasaland.

4043. Can swine be taken along the infected roads? It is not supposed to be a danger to them, is it?—No, it is not recognised as a danger out there.

4044. Dogs take the disease, do they not?—Yes.

4045. Do they ever recover?—I cannot vouch for one having recovered, but I have heard of cases.

4046. Generally they die?—Generally they die.

4047. In Nigeria?—In Nigeria and Nyasaland.

4048. Sometimes cattle recover. It is doubtful if dogs ever do, but can you tell us about any other animal?—I cannot give you instances of other animals at all.

4049. Sheep?—No.

4050. Have they always died, as far as you know?—As far as I know they all died.

4051. Goats?—I have not been called out to many goats, so I could not speak on that.

4052. I merely want to know what animals are not always killed. You have given me some instances. So far as you know, occasionally, though rarely, cattle survive, and that is about all?—That is about all.

4053. (Sir William Leishman.) Have you any idea of the incubation period of trypanosomiasis in cattle?—I have not any distinct notion. I inoculated two old cows taken from a clean area. I inoculated them with blood direct from an ox suffering from trypanosomiasis, and in that case the incubation, if my memory serves me rightly, was 36 days.

4054. When you examined, for instance, a herd of cattle suspected of trypanosomiasis and found trypanosomes in a certain number, were they all sick animals or were any of them apparently healthy, taking any series of observations?—When an outbreak occurs in a herd or a team of oxen, usually you get five or six

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of them going down at one time. Some of them will die quickly, and others will linger on for some time, and then probably you will get no more infected in that individual herd if they are kept away from a fly area.

4055. Do you sometimes find trypanosomes in the blood with very few symptoms, and even the animal getting well afterwards?—Yes, I have found that.

4056. It may be in apparent health and yet be carrying trypanosomes?—I would not say that, if you take the temperature into consideration. You will usually find the animal has a high temperature.

4057. You said just now that trypanosomes are sometimes abundant in the blood?—Yes, very abundant. Sometimes they are rather difficult to find in the blood during the latter stages of the disease.

4058. In an ordinary blood film can you find them without trouble on searching for a minute or two?—Yes.

4059. (*Dr. Chapple.*) What would you say is the first clinical symptom that an ox will show?—I should say a high temperature.

4060. How soon would you expect to find a rise in temperature after the bite of an infected fly or after inoculation?—As to that I could not say except just speaking in a practical way with regard to dealing with a herd of cattle when I am called out. On examining the animals I will probably pick out one that I think looks slightly watery about the eyes and feverish-looking and take its temperature. You will usually find that the temperature is raised, and on examining the blood you will find the trypanosomes freely and easily.

4061. Have you no idea what time has elapsed between the inoculation and the first clinical symptom?—No. In my work I have not had the opportunity of following it.

4062. What were the 36 days of which you spoke?—That was a distinct inoculation.

4063. Do you mean the period between the inoculation and the first symptom?—From the date of the inoculation to the date on which I found trypanosomes in the animal's blood.

4064. Did you find trypanosomes in the animal's blood before it manifested any rise of temperature or watering of the eyes?—No, not before—after.

4065. The rise of temperature was the first manifestation that the animal had the disease?—That was the first manifestation that I got.

4066. Subsequently you found the watering of the eyes and the trypanosomes?—Yes. Later, if the disease lingers on for two or three months, which it does sometimes in animals, you usually get the superficial lymphatic glands standing out distinctly.

4067. During the 36 days were you examining the blood every day to find trypanosomes?—Every day.

4068. And there were none?—There were none. I mentioned 36 days from memory. It is in one of my reports. I think it is 36 days, but I would not like to pledge myself.

4069. Is it not important to know what the first clinical manifestations of the disease are in order that you may exterminate an animal which you know is harbouring trypanosomes in its blood, and therefore likely to be a source of danger to its neighbours?—Yes.

4070. You said that you found two flies on the verandah on one occasion. Were those flies seized and examined to see whether they had trypanosomes or not?—They had been kept for some time before they were presented.

4071. Did you examine them to see whether they were infected?—No, I did not.

4072. Did I understand you to say to the Chairman that one of your functions was to keep the public quiet?—To travel through the country wherever I am called and tell them what their animals are suffering from, and advise them what to do with them—either to shift them or do something else with them, and in that way keeping them quiet. In other words, the calls on the services of one veterinary officer for the purpose were more than I could well attend to, but I did my best.

4073. I misunderstood you. It was not to allay their fears with regard to danger to their stock?—Not so much in that way.

4074. Or to meet their protest against anything that was done or anything that was neglected in a public health way?—No.

4075. You gave the names of a number of firms and individuals who had suffered from loss of cattle?—Yes.

4076. Have you formed any notion of what the general public opinion amongst these interested parties is?—Yes.

4077. What is their general view?—They would be strongly in favour of having the game cleared out of all areas in proximity to civilisation. They have, I might say, been asking for it for a long time.

4078. For how long?—I would not like to give a fixed term.

4079. Is it a matter of years?—Yes, it is a matter of years.

4080. Is that the universal opinion among those who have suffered from this terrible disease?—Yes, in the southern districts I would say.

4081. Do you think that their opinion is well founded?—Yes.

4082. Do you think that if the animals were hunted they would get shy and depart from the vicinity of populous parts?—They would.

4083. Would that apply to all wild animals?—Yes, I think so.

4084. Do you think that if natives were allowed to shoot they would select those animals that were profitable to them and neglect those animals which, though incriminated, might still remain a source of danger?—I believe that if left to themselves they would take the bigger animals first, undoubtedly, but after they had exterminated the bigger animals they would tackle the smaller ones.

4085. Are the smaller animals profitable?—Yes, for food purposes.

4086. Are there any incriminated animals that you know of that would not be hunted by the natives if the game laws were relaxed?—No, I am not aware of any.

4087. Do you think that the whole source of infection would go if you relaxed the game laws in such a way as to permit both whites and natives to hunt these animals in the immediate vicinity of populous parts?—I am inclined to think so.

4088. You think that with the animals the fly would go too?—Yes.

4089. And therefore the source of danger to cattle?—I believe so.

4090. (*Chairman.*) Have you ever found a fly area where there was no game?—No, I cannot cite one.

4091. As regards this fly-free area on each side of the Zomba Road, do you know whether the game have been examined and whether they contained any trypanosomes?—No, I am not aware of their having been examined.

4092. We have been told by one or two witnesses that at any rate in South Africa the fly seems specially to follow buffalo, and to disappear when the buffalo disappear. Have you any experience on that point?—No, I have not had experience in that line.

4093. Have you heard that theory advanced?—Yes, I have heard it, but I have no experience in that direction myself.

4094. If that was correct it would be sufficient to exterminate buffalo and the fly would then disappear?—Yes.

4095. But you have no evidence bearing on that?—No.

4096. Have you tried any treatment of animals suffering from trypanosomiasis with success?—No, not with success.

4097. You have tried various forms of treatment?—I tried one treatment in Southern Nigeria with no success.

4098. What part of Southern Nigeria?—The experiments were carried out in Lagos.

4099. Was the trypanosomiasis that you came across in Southern Nigeria exactly similar in type to

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the Nyasaland type?—Well, I would not like to say so. Apart from the experimental treatment my work in Nigeria was almost identical with my work in Nyasaland.

4100. You did not go into that particular question then?—I did not have the opportunity of going into it.

4101. You do not know whether there was the same carrier, whether it was *morsitans* or *palpalis* in Nigeria?—Mr. Austen, I believe, knows something about the flies I encountered there. I could not name them now. I sent home a new one, I believe, to him. I came across several kinds of fly there and sent a considerable number of them to Mr. Austen for identification.

4102. Are they all carriers of infection?—I would not like to say.

4103. You did not take stains from them and examine them?—No.

4104. When an animal dies you say that very often the trypanosomes disappear, or tend to disappear, immediately before death?—They tend to disappear. They are usually more difficult to find in the later stages.

4105. I suppose you make post-mortem examinations of some of these animals?—Yes, I have made many.

4106. What do you attribute death to—is it by poisoning or the blocking of particular veins or what?—I would not like to express an opinion. I would be inclined to think that death was due to a toxin, a poison of some sort, but I do not know.

4107. There is no means of finding out, is there? There is no experimental test for that?—Not that I am aware of.

4108. (*Earl of Desart*.) Dr. Chapple used the expression "incriminated animals." What do you yourself understand by that exactly? May I put it in this way to you: do you mean by "incriminated animals" those in which you have found trypanosomes?—No, I would use it in a broader sense, and would include animals in which you do not find trypanosomes.

4109. But when you are talking of trypanosomes, do you mean the trypanosome that produces trypanosomiasis in cattle, or do you include the trypanosome that produces trypanosomiasis in man, or are you distinguishing between the two?—I have not attempted to make a distinction between them. The trypanosome in cattle, so far as I am aware from conversation with Sir David Bruce, is *Trypanosoma vivax*; but that is his classification, it is not my own. I have never attempted myself to classify the trypanosomes I have looked at through the microscope.

4110. When you adopt the expression "incriminated" you do not necessarily mean incriminated as regards producing trypanosomiasis in man? It may be so, I agree, but not necessarily?—Not necessarily.

4111. Naturally your duties relate to the conveyance of trypanosomiasis to cattle, and how it can be met?—Yes.

4112. You are speaking, I understand, of that?—Yes.

(*Chairman*.) Thank you very much.

The witness withdrew.

FOURTEENTH DAY.

Friday, 5th December 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

SIR MACKENZIE CHALMERS, K.C.B., C.S.I. (*Chairman*).

Mr. E. E. AUSTEN.

Dr. A. G. BAGSHAW.

Dr. ANDREW BALFOUR, C.M.G.

Mr. E. NORTH BUXTON.

The Right Hon. the EARL OF DESART, P.C., K.C.B.

Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODGE, Bart.

Dr. C. J. MARTIN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Mr. A. C. C. PARKINSON (*Secretary*).

Mr. J. H. ASHWORTH, D.Sc., called and examined.

4113. (*Chairman*.) Will you tell us what office you hold in Edinburgh?—I am Lecturer in Invertebrate Zoology and also in Medical Entomology and Protozoology in the University.

4114. You have not been in Africa yourself, I think?—I have not. I depend for my information regarding local conditions on reports and on the men who come to my course who have been in sleeping sickness areas.

4115. Are they frequently coming back?—Yes.

4116. Do they go through post-graduate courses?—Yes; they attend my course or work in the laboratory—not always on a specific course, but on those flies or protozoa of importance in medical work in which they may be specially interested.

4117. You have been studying the question of sleeping sickness for some time, have you not?—I have been keenly interested in it since I began the course for medical officers about 8 years ago. Naturally one meets a good many men in Edinburgh

who are interested in the subject, and their questions cause one to look up critical points from the reports and memoirs.

4118. We shall be glad of your skilled opinion on certain points. First of all as regards *gambiense*, what have you to say as to the animal reservoirs of *gambiense*?—You mean the animal reservoirs as contrasted with the human reservoirs?

4119. Yes. Are you satisfied that either domestic stock or wild ruminants are true reservoirs of the *gambiense* fatal to man?—Well, that seems clear in one (at any rate) of the islands in Lake Victoria, but I am not convinced that it is the case elsewhere. For instance, there are stretches of the Congo where game is scanty, and where the human host appears to be sufficient. I recall one case of which I heard from a missionary.

4120. A medical missionary?—He had medical training, but he was not a graduate. He said that on the Lulunga, a tributary of the Congo, there was a

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small settlement of some 400 people in which he stayed or visited, and that there were usually about a dozen obvious cases of sleeping sickness. I directly asked him about the game, and he informed me that game was scanty, in fact practically absent in that area, and he attributed its absence to periodical flooding. I asked him about fly, and he said that in the settlement itself on the north side of the Lulunga fly was scanty, but across the river was another village where there was sleeping sickness and where fly was common. The people in the first village became infected by going to buy food at the second village, as far as I can make out. Some may have acquired, and in fact probably did acquire the infection on their journeys up and down the river, but the rest apparently acquired it in the second village.

4121. What is the fly in that case—*palpalis*?—*Palpalis*. I asked my informant how far away would be the game, and he said on the higher ground some 10 miles away.

4122. Did that apply to both the villages?—Apparently. They were both in the low-lying area.

4123. Fly was found in the second village?—Yes.

4124. Although there was an absence of game, there was a good deal of fly?—Yes, fly was said to be common.

4125. Are you satisfied from your examination of the subject that the bite of a fly is the only way by which man can be inoculated?—From time to time I have inquired from those attending my course what were the conditions with regard to fly, and if *Glossina* was fully accountable, and I have not heard of any case in which other blood-sucking insects appear to be involved.

4126. But apart from blood-sucking insects, is the disease in any way communicable?—I have not heard that it is, apart from the suggestions by Koch and others to that effect.

4127. Is there evidence which tends in that direction?—No clear evidence has come to my knowledge as the result of inquiry.

4128. As far as you know, and you have made considerable inquiry, wherever the disease has appeared the sick man has either been in a fly area or in a place where he may have come into contact with fly?—Yes.

4129. And as far as the evidence goes the only fly incriminated is some form of *Glossina*?—Yes, that is the case.

4130. Now you say something about the islands in your proof. You say, "On the depopulated island of Damba, however, the game reservoir is evidently much more efficient."—Yes.

4131. "—and its destruction in this or in any other area in which that reservoir proves to be similarly efficient becomes a matter for serious consideration." Can you describe to us more about the island of Damba, its size, and so on?—As far as I can find out from measurements made on the only map at my disposal, it is about six miles in maximum length and some three miles in maximum width. Its shore is indented so that the area is less than one might perhaps expect from those measurements.

4132. How far is it from the shore, do you know at all?—I would not like to say definitely.

4133. I am told that it is about five miles from the shore. Do you think anything would be learnt from exterminating the game from the island of Damba? I assume that there are both fly and game in Damba.—Yes. I should be inclined to suggest for consideration the carrying out of an experiment on such an island as Damba if it is not considered too large, in order to ascertain definitely if when the large ruminants are destroyed (I understand there is only one species of ruminant there) the small animals can act as reservoirs.

4134. What is the ruminant there?—It is Speke's tragelaphus.

4135. That would not be difficult to eliminate, you think?—I am not sufficiently familiar with the local conditions to say that. Those who know the island can give the Committee authoritative information on that point.

4136. Assuming the larger game to be exterminated, what should we learn from the experiment?—As I have already mentioned, we might learn if the smaller animals would act as reservoirs. There is apparently a varied fauna of small animals according to Dr. Carpenter's report. There are small rodents, the names of which, however, he does not mention, bats, otters, cats, monkeys and birds.

4137. With the exception of the monkeys are there no animals in which trypanosomes would be found?—I am not aware that the other mammals have been examined from that point of view.

4138. Since man has been cleared out of the island has the game been examined, and is it clear that the *gambiense* trypanosome is still in the game?—Four examples of Speke's tragelaphus were examined, and in at least two I understand *gambiense* was found.

4139. Who examined them? Were they examined by experts?—By Dr. Duke and Dr. Carpenter, who should be regarded as experts in the matter.

4140. Yes, certainly. Four animals were killed, and two were found to be infected?—Yes, I understand that the blood of the first two was pooled, and the blood of the second two was pooled before both lots were injected into susceptible animals, and therefore we cannot get any more definite information as to the exact number infected. At least two were infected.

4141. Was nagana introduced into the animals into which it was injected?—I understand that the monkeys injected became infected with *gambiense*.

4142. Is there anything else to learn from experiment?—Yes, I think so. One might get some knowledge as to the length of time that the fly remained infective, assuming that the smaller animals did not become reservoirs.

4143. Do you think we should learn whether if the big game was removed (big game are perhaps the most tasty food for the fly) the fly would migrate in search of better food?—If the island is five miles from the shore, I see great difficulty in the fly migrating five miles over water.

4144. Do you think that that is beyond the flight capacity of a fly?—No such flight over water is recorded, and it is likely to be well beyond the range of the fly.

4145. That is one of the things that we particularly want information upon, is it not?—Yes, you would need to work out the range of flight of the fly on some other area than this.

4146. At any rate it would prove whether fly can or cannot fly five miles?—Truly, if the fly remained in undiminished numbers that would be evidence that that there was no migration.

4147. As regards these flies, supposing they are deprived of their food what happens? Does the fly die in a few days if there are no animals on which it can feed?—They have been kept alive under laboratory conditions for thirteen days without food. That is the maximum period that I recall. Experiments on flies indicate that they must be fed on blood if they are to be kept alive longer.

4148. Do you think that if, experimentally, warm-blooded creatures on which they feed, could be cleared from that island, you would have at any rate an enormous mortality among the flies, and they would be driven to cold-blooded animals, if any?—There is a good deal to do to clear off the warm-blooded animals from an island such as Damba. I should think there is a considerable ground fauna, e.g., rodents. There are also birds on which the fly would probably feed if pressed.

4149. Are you satisfied that birds have never hitherto been proved to be a reservoir for trypanosomes?—There is no record of a bird found naturally infected with *gambiense*. It has been shown only comparatively recently that they are even susceptible—by experimental work in Paris.

4150. Does that relate to particular classes of birds?—Fowl.

4151. Has the wild bird ever been successfully inoculated with trypanosomes?—I do not recall any cases.

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4152. What are your practical suggestions as regards human reservoirs?—I regard the human host as in many places a very important one, almost to the exclusion of any other host. If I understand rightly the conditions in that settlement on the Lulunga, that is a case in point, and Dr. Stohr's report on sleeping sickness in the Katanga also indicates the importance of the human host in the area.

4153. I forget for the moment; are those *palpalis* or *morsitans* areas?—*Palpalis* in both cases. In the Katanga *morsitans* occurs, but *palpalis* is evidently the primary factor. I think that as long as infected natives exist in such areas as those two I have mentioned, and so long as the fly has the opportunity of acquiring an undoubted human strain of trypanosome and becoming in due course infective to human beings, there seems no reason under those circumstances why the disease should not continue.

4154. What practical measure do you suggest?—The best one seems to be to remove from access of the fly the infected human hosts.

4155. That is to say, whenever a man is infected he must be moved right away from the fly area?—Or the community in which he lives should be isolated by a sufficient clearing around it which the fly will not cross.

4156. Are there any data at present by which we can determine what is a sufficient clearing? Is there any evidence?—Well, certainly the evidence has varied.

4157. It has varied very much.—But I suppose that if you could clear only two or three hundred yards you would greatly diminish the risk of spread of the infection.

4158. Do you think that as a rule the fly would not cross except when carried on a man's back, or something of the sort, would not voluntarily cross 200 yards of clearing?—Two or three hundred yards is a reasonable clearing to make to begin with. I would not like to say it will be always sufficient.

4159. Do you think there is evidence pointing that way from inquiries you have made?—Yes.

4160. Assuming that round these villages game was driven off, what do you think would be the effect of that? Would the fly follow the game or would other things happen?—I am afraid that we have no data on which to base a definite statement. One can only suggest that if the game is driven away the fly may develop its following habit, and may be impelled to make more frequent attacks on man.

4161. Do you think there is much hope of dealing practically with sleeping sickness by working at the bionomics of the fly, or are they too obscure?—I think that one hopeful feature is to see if we can find the enemies of the fly.

4162. Yes.—Dr. Bagshawe and Dr. Carpenter have both recorded the finding of pupa cases with a small hole in the side, the contents having evidently been devoured by some parasitic insects, and in view of the fact that in one locality the fly seems to have very largely diminished in number, possibly through the attack of some such parasite, it is desirable to make further attempts to find that parasite.

4163. This is a parasite that attacks, not the fly, but the pupa?—Yes, the pupa.

4164. Can you give us any hint as to how that examination should be carried out?—Well, the only possible way of doing it, I think, is to collect large numbers of the pupae and breed them out, and see if other insects than *Glossina* emerge. If any Chalcid or Tachinid emerged, then you would need to take careful note of that, and try to import some similar insect to help in the process of destruction.

4165. Is it possible to start a breed of insects of this kind which feed on others?—It may be possible to import a breed.

4166. I mean starting them in a new place, bringing them from an old place. Would that be successful, do you think?—It has been done under similar circumstances elsewhere—in America for instance.

4167. That is what I wanted to know. Can you give us an instance?—Yes, I think I can. The yellow scale insect in America, for instance, is stated to have

been rendered almost harmless, owing to the accidental introduction of its Chalcid parasite from Japan.

4168. What did the yellow scale insect destroy?—It was on orange and similar fruit trees.

4169. How was this parasite introduced?—As I understand, it was an accidental introduction in the first instance.

4170. Was it done by nature and not by man?—By accident in the sense that it was brought over from Japan without any special intent to attack the yellow scale.

4171. Was the parasite cultivated in any way?—I cannot give you the details, but they could be obtained, and most certainly full particulars are available regarding the more fully worked out case of the gypsy-moth and its Tachinid parasite.

4172. You think that that is one hopeful means of attacking the *Glossina*, or possibly hopeful?—It seems to me a hopeful factor in the bionomics to investigate.

4173. That only applies to *palpalis*?—One enemy, if one not too specialised could be secured, might perhaps serve for *morsitans* also, but that remains to be found out, of course.

4174. What practical step would you take to work out that problem?—I should suggest that large numbers of the pupae of *Glossina* and allied Diptera be collected and bred out with the view of finding if other insects also emerge. The endeavour would then be made to obtain elsewhere allied parasites, and to ascertain if they would attack the pupae of *Glossina*.

4175. Would you send a special man to conduct that work, or would any well educated entomologist be able to undertake it?—I think it is a problem which a well-trained entomologist would be able to undertake.

4176. Leaving *palpalis* for the moment, and coming to *morsitans*, are you satisfied yourself that the *rhodesiense* and the *brucei* trypanosomes are one and the same?—I am not satisfied that they are the same in their action on man, although they appear to be morphologically indistinguishable. As you are perfectly aware, no doubt, the question of species in regard to trypanosomes is in an extremely involved condition. As a matter of fact, it is regarded at the present day as a difficult zoological problem to formulate a satisfactory definition of a species in many cases, and therefore to distinguish species in such critical cases as we have before us; this difficulty reaches its maximum in such low organisms as parasitic amoebae, spirochaetes, trypanosomes, and so on. In the case of amoebae we have the cysts to help us with their special morphological characters, but we have no such help with the spirochaetes and the trypanosomes. We have only the asexual phases which present such extremely similar characters. The spirochaetes of different species causing human relapsing fever are not distinguishable morphologically. But, after all, the criterion of a species is that it should be distinguishable morphologically by some character which we have been accustomed to call its specific character, and when we go beyond such a character we immediately arrive, as I think, on uncertain ground. The animal reactions in such organisms, for instance, are liable to be inconstant. Recently, Sir David Bruce has shown in Nyasaland that the *Trypanosoma pecorum* derived from cattle did not establish itself in rats, but a strain of the same species derived from wild game did establish itself in rats. Animal reactions, therefore, with regard to whether it will or will not establish itself in rats, are not constant enough to distinguish this species. Such resistance to its establishing itself is met with, it seems, on the part of *Trypanosoma gambiense*; in fact, there is not only resistance to establishing itself in man but also in the fly, and there is some difference apparently in this respect between individual strains or races. May I recall to the Committee the large number of positive results obtained by Miss Robertson when she used a particular monkey, number 199, if I remember rightly, to feed her flies. That strain apparently could establish itself in the fly more readily than others.

4177. Where is Miss Robertson working?—In Uganda.

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4178. She experimented with *gambiense*?—Yes. Returning to your previous question, it seems to me that our present knowledge of the relationship of *rhodesiense* to *brucei* is inadequate and conflicting, and it enables us to say that the facts indicate that *rhodesiense* found in man may be regarded as a strain, or perhaps more than one strain, of *brucei* which has established itself in man in Rhodesia and Nyasaland, where foci of human trypanosomiasis exist. That, I think, is about as far as the evidence would permit one to go.

4179. We cannot say positively that the *brucei* which is found in wild game and which produces nagana in a susceptible animal is the true *rhodesiense* which produces sleeping sickness in man?—I do not think you can, because there are strains of *brucei* which appear not to be able to establish themselves in man. I need only remind the Committee of the strain employed by Dr. Taute in his remarkable experiment upon himself.

4180. Do you think he was very lucky in the strain he chose, or do you think he is peculiarly resistant?—We cannot argue the point from one experiment, I am afraid.

4181. That is unfortunately an experiment which cannot be repeated, because volunteers are not likely to come forward?—Evidently he had very great confidence, otherwise he would not have been disposed to try the experiment. I think the report of Dr. Fleming on trypanosomiasis in Southern Rhodesia is rather suggestive in this connection. May I quote one sentence from the report?

4182. Please.—He states, "Though infected sheep, goats and dogs were found fairly generally distributed throughout the various villages in the fly belt (and two infected water-buck were also recorded), it is a matter of interest to note that almost all the human beings found to be infected were either resident within, or had previous to the period of infection visited, a certain small area of country on the Busi river. It seems difficult to believe that the trypanosome infecting man in this particular part of the district is the same as that found in sheep and goats in other parts of the fly area." A little further on in the report he says: "The further results of examination of the trypanosome isolated from various strains obtained from men, goats, and dogs in this area show that in each case the trypanosome was identical and is morphologically and clinically allied to *Trypanosoma rhodesiense*." That report I think indicates that although a trypanosome indistinguishable from *rhodesiense* exists over the whole area examined, it is apparently only on a small stretch of the Busi river that the strain has established itself in man, and that other human beings have been infected with this strain only, and not with that strain universally present in animals in the area.

4183. We have had this theory suggested to us, that the normal *Trypanosoma brucei* is not pathogenic to man, but that under some curious and at present unknown conditions a strain may develop which is pathogenic to man. What have you to say about that?—Well, we know little about the causes which may bring about a particular strain, for instance, a strain of *brucei*, which could overcome the resistance in man and establish itself in man. We know very little, in fact, about the differences in virulence and how they are brought about in trypanosomes.

4184. But, acting rather in the dark and acting on probabilities, would you advise driving away the game from any inhabited area where there was fly?—Well, of course our present insufficient knowledge on this matter renders our views on the relative importance of the game reservoirs liable to a considerable margin of error. For instance, we have the view of Dr. Taute on the one hand that the game strain does not take in man, that the game does not play the part assigned to it, and it is quite likely that that may hold for the area in which Dr. Taute works, and for other areas; then there is the directly opposite view—

4185. Taken by Sir David Bruce?—Yes, taken by Sir David Bruce and by Drs. Kinghorn and Yorke, that the game strain is pathogenic to man. That does

not appear to be the case with the animal strain in the area examined by Dr. Fleming to which we have just referred. The state of affairs in Rhodesia seems to be that of the *brucei*, widely distributed in animals, one or more strains in certain foci have taken in man—the strains to which we have given the name *rhodesiense*.

4186. (Dr. Bagshawe.) With regard to the destruction of Speke's tragelaphus, how would you prevent fresh animals from reaching the island when all the present animals had been destroyed?—I should prefer to leave that question to be answered by those who know the local conditions. I am not aware how far these animals can swim, for instance, and those who know the local conditions could tell you much more authoritatively what you want to know. It would be necessary to prevent others getting there by some form of patrol or something of the sort.

4187. I believe there is evidence that they swim several miles—I do not know how far—and are also found on the mainland, so that they could easily cross five miles. They must be kept out in some way?—Yes, and if they gained access at all they would need to be killed off immediately and their blood examined in order to ascertain if the experiment had been vitiated.

4188. There is one other point I would like to mention with regard to the parasite that attacks the pupa. That parasite was found in the Belgian Congo. It is described as a species of *Conostigmus*?—I am afraid I do not know it.

4189. (Dr. Balfour.) Have you yourself carried out cultivation experiments with pathogenic trypanosomes?—No.

4190. Have you any view as to whether cultivation is likely to be a valuable method of differentiating species?—The morphological characters that one would have to go upon in cultured trypanosomes seem to be fewer than in the ordinary blood forms, and therefore it does not seem likely that the cultures would help much.

4191. What is your opinion regarding the cross inoculation method—that is to say, inoculating an animal with a species of trypanosome and allowing it to become naturally cured, or curing it and then inoculating it with another species, and seeing if it is immune or not?—That, so far as *gambiense* and *rhodesiense* are concerned, if I recollect rightly, has only been carried out in one direction—that is by getting an animal immune to one strain of *gambiense* and inoculating it with a strain of *rhodesiense*, but it has not been done in the reverse direction, has it?

4192. Not so far as I know. Is it worth while trying it?—If we may judge from animal reactions in other organisms, particularly spirochaetes, I should say that the method is inconstant, and therefore not highly reliable. Take, for instance, Darling's work on spirochaetes in the Panama Canal zone. He showed that immunity against one strain does not necessarily protect against another strain of the same species from a different human source in the same locality.

4193. That is true of Nicolle's work in Tunis too?—Yes. Nicolle and Blaizot have reached a similar conclusion.

4194. You regard spirochaetes as closely allied to trypanosomes?—Not at all, but that is one of the few cases available as an analogy.

4195. With regard to the life cycle of pathogenic trypanosomes in man and animals, do you think that it has been sufficiently worked out?—I think there is sufficient knowledge of *gambiense* for practical purposes (I realise that the recent work on granules, &c., should be followed up), but so long as the sexual phases remain undiscovered the life cycle remains theoretically incomplete, but it is to be remembered that the sexual process in many flagellates appears to occur, if at all, only rarely, and such processes are known in certain flagellates which live either in fresh water or in those which are parasitic in the gut of animals, and that sexual process occurs usually before these emerge into unfavourable conditions. In the case of trypanosomes the organism is always under cover of one or the other host, and in both, if it establishes itself at all, it appears to find conditions

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under which it can multiply apparently with great ease. It is to be expected, therefore, that the sexual process, if it takes place at all in trypanosomes, will be found only at very considerable intervals. I would not, if I were a protozoologist on the spot, care to sit down with the one aim of finding the sexual phases.

4196. Not at all?—I consider it would be likely to be a very long task and, judging from the observations on trypanosomes already published, perhaps not of great practical importance.

4197. How do you explain the occurrence of cryptic trypanosomes in animals—non-pathogenic trypanosomes in cattle in this country and in Germany, in Algiers, and in Holland? Indeed in various countries it has been discovered. Is it due, do you think, simply to the paucity of trypanosomes in the blood serum, so that the infection cannot be discovered by ordinary examination methods, or do you think there is some concealed form present—that is to say, some stage not recognised—a granule stage or some other stage?—I have not worked on that problem, so I would not like to offer an opinion.

4198. You do not feel competent to give an opinion?—No. I have had very little experience with granules, and I have not given close attention to the cryptic problem.

4199. I understand you to say in your proof that in dealing with the pupae of tsetse-flies you would suggest tarring?—Yes, for *morsitans*, but I should suggest clearing the areas where *palpalis* is known to deposit its pupae.

4200. What led you to suggest tarring?—Of course that is only a suggestion. Attack on *morsitans* seems to be very very difficult at the best. The only clue that we have to a means of attack, as far as I can see, is the statement of Mr. Lloyd, who says that pupae were found either in abnormal or injured trees in hollows, and therefore if one could render those hollows untenable for the larva and subsequently for the pupa, one would be destroying a certain number of breeding places at present existing. This could not be carried out over very wide areas, but when clearings are being made around villages the hollows likely to shelter pupae could be made untenable in future in and near that area. Of course, it is a very tedious task, and perhaps not of great value, because the larvae may be dropped by the fly over a wide area, especially in the wet season.

4201. You know of no similar method in the case of any other insect?—No, but *Glossina* is a special case.

4202. There are so few insects that pass through the stage that *Glossina* does?—Yes, the larva is not open to attack; the pupal stage is practically the only one assailable.

4203. Now as regards the yellow scale insect of which you spoke to the Chairman, at what stage in its life's history did the Chalcid parasite attack it?—I am sorry to say I have not the details in my mind. I have not worked on it, because I have been particularly concerned with blood-sucking insects. I am not a general entomologist, my time available for work on entomology has been devoted to the medical aspect, so the scale insects are on a side track which I have never followed.

4204. (Sir Edmund Loder.) You say you think 200 yards enough clearing for the fly. Did you think of the wind blowing hard? It would not want much to blow a fly across, would it?—No, that is true, although I have been informed that 200 yards clearing proved satisfactory in parts of the Congo.

4205. You would have to make it a little bigger?—Yes, I suggested 300 yards.

4206. With regard to your island that you talked about to one of the Committee, do you know of red deer swimming from Skye to the mainland? If they swim across that distance I should think that such an aquatic animal as the situtunga would swim further than red deer can?—Could you not get an island further from the shore?

4207. That is another point. I am asking the question because you suggested that island.—I suggested it simply because that is the one place from

which animals infected with *gambianse* are definitely recorded.

4208. I know that red deer swim a long way, and I should think situtunga would swim further, and therefore the experiment might be made null and void in that way.—Certainly, unless some means could be devised to keep them out. I suggested that island because if you are going to do anything with such an experiment you must begin where the fly is already infective, and where you have some index on which to work, and that is one of the few places where we do know that the fly is infective, or at least was infective when last examined.

4209. (Mr. Austen.) You stated in reply to the Chairman that on Damba island in Lake Victoria, which has been depopulated, game in the shape of the situtunga appears to be the most efficient reservoir. Do you think that that assumption harmonises with the evidence that we have from the mainland of Lake Victoria, with reference to the artificially inoculated antelope, that the infectivity of those antelope appears to show a tendency to diminish in time?—Well, if the situtunga are not the reservoir on Damba, it seems very difficult to suggest what is. That is the only answer I think that I can make to your question, but of course one realises the force of your observation.

4210. You are familiar with Dr. Duke's published statement, I take it?—Yes, in the sleeping sickness report—you mean with regard to the infectivity of the fly on the mainland and similar observations.

4211. I am referring to his experiments with the experimental antelopes which were artificially infected by Sir David Bruce and his collaborators, and to the fact that Dr. Duke stated that subsequently he found that those animals appeared to show a diminution of infectivity towards the fly, and that antelopes which had once ceased to be infective were only with very great difficulty, or not at all, capable of being re-infected. That scarcely harmonises with the assumption that an animal like the situtunga could remain perpetually a main reservoir of the parasite?—You remember that there is the new generation always to count in these matters, and the new generation would be susceptible, I presume.

4212. Do you mean a new generation of the game?—Yes, of the game. The young animals if much bitten by infected fly would become infected, and so you would still have a chain capable of carrying on the infection.

4213. Are you aware of any disease of man due to an organism in which the main reservoir is not in man, but in some other animal?—You mean a protozoan blood-disease of man?

4214. Yes, a protozoan disease of man.—No, I am not aware of one. Does such a case occur to you?

4215. I am asking you because I want to know.—I do not recall one.

4216. The Committee has been informed by one witness, who has had considerable experience in tropical Africa in studying trypanosomes, that the big game are a "poisonous and dangerous crowd," and that it would be "as reasonable to allow mad dogs to exist in the vicinity of English villages" as to tolerate the presence of wild animals near settlements in tropical Africa where trypanosomiasis and tsetse-flies both occur. Do you agree or disagree with that statement?—I should want to know which locality was referred to before I replied to that question. It is not a question to which one can reply in general terms. For instance, take the Lake shore, in regard to which *gambianse* area many observations are available. I should say that there the human population was the main reservoir of the disease, and that the animals were very secondary. We must, I think, admit that when we examine the figures for the degree of infectivity of the fly on the Lake shore, because the removal of the population without, so far as I am aware, disturbing the game, in fact giving the game more favourable conditions, reduced the figure of infectivity of the fly to less than one-eighth its previous amount. A factor which when removed reduced the infectivity of the fly to such a great degree is the important factor. The other is the secondary factor, but one would have

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to be convinced that the continued infectivity of the fly on the Lake shore was not due to casual visits of infected natives before concluding that the game constitutes this factor. If it is proved that natives are not visiting the shore and that infection of the fly is really due to the game, then the game is a factor to be considered.

4217. Are you aware that Dr. Duke said in one of his reports that surreptitious visits of infected natives would be sufficient to keep up the infectivity of the fly in certain cases on the Lake shore?—Yes.

4218. If you were asked to construct a table of relative deadliness towards human beings in the case of different animals, is there any case in which you would put big game in Africa on a level with mad dogs?—That is, would I regard them as equally deadly? That depends on the view one takes of the relationship of *rhodesiense* and *brucei* upon which we have so little data. Certain foci in Rhodesia are the only areas in which it is possible, or perhaps one or two of the islands, where the game and the fly are both abundant, and in constant association.

4219. I do not want to deal with possibilities, but with the evidence that we have at our command at the present time.—But the evidence is incomplete and therefore one cannot make a positive assertion.

4220. But the positive assertion has been made, and I want to know whether you would support that positive assertion, and whether at the present time you would regard big game as being as dangerous to human beings as mad dogs are?—Not until you can assure me that *rhodesiense* and *brucei* are one and the same in their reaction to man.

4221. You stated in reply to the chairman that the disappearance of *Glossina palpalis* from one locality may have been due to a parasite. I am afraid I do not remember the case to which you were referring. Would you mind telling the Committee?—I had in mind the shore near Mpumu where the fly boys used to go to collect pupae. The statement in the report was, I think, that they found it was scarcely worth while going to that place any longer in order to collect the pupae, and it was suggested that their diminution may have been due to the occurrence of this parasite.

4222. I was not sure whether you were referring to that place or not. Now you suggested, I think, to the chairman that indigenous enemies of *Glossina* might be used to attack the fly if they were properly protected and cultivated, but does not the present predominance of *Glossina* over the native parasites show that the fly has beaten such enemies?—Yes, at present. On that account I also suggested the introduction of allied parasites.

4223. Am I not right in thinking that in cases where parasites have been used successfully to combat an insect pest, those parasites have not been the endemic parasites, but parasites introduced from some other country?—The two cases I have in my mind are both of that nature.

4224. Introduced parasites?—Yes.

4225. Therefore I take it that if we are to expect anything from an attack on tsetse-flies by the agency of other insects, we must find those insects elsewhere than in tropical Africa?—Yes, but if the parasite in the pupa could be discovered, it might give us a clue (that is the point I was trying to make) which would be likely to be most useful.

4226. You were not relying on that particular species?—No.

4227. Are you aware that it was stated by one of the Belgian investigators, I think, in his description of the parasite of which the name was given by Dr. Bagshawe, that it was so rare that they only found one case in 400 pupae of *Glossina palpalis*, and that it could not possibly be of any importance in reducing the numbers of *Glossina*?—I have not seen that report.

4228. He said that, did he not?—I presume it is in the last Belgian report?

4229. It was published, I think, about a year ago. If you were given a sum of, say, 5,000l. to deal with this problem, can you tell the Committee the main lines on which you would proceed?—I think that ought to be left to someone with complete knowledge of local

conditions. I do not think I should care to state anything in regard to that, because you no doubt have had before you, or will have before you, people who know the local conditions, and who could indicate in what manner that money could be used to the best advantage, especially in the way of measures (clearings or removal of the population in the worst areas) to prevent or greatly diminish access of the fly to infected and also to non-infected human beings.

4230. Without referring to any definite locality, could you not state in general terms the lines on which you consider, with the information at your disposal, the problem should be approached?—I can only further suggest experimental work on an island, if a suitable one not too large in area is available for the purpose. I think that is the best suggestion I could make as regards a *gambiense* area, but I would not like to suggest further work in a particular district in the *rhodesiense* area, because I am quite unfamiliar with the local conditions.

4231. No, I am not asking you to suggest a district. With regard to the so-called *Trypanosoma rhodesiense*, are you prepared to put the different projects such as the entomological one, the medical one, and the proposed big game experiment in order of importance?—I feel that so long as we are faced with the difficulty of the relationship between *rhodesiense* and *brucei* we have a crucial point on which there is wide divergence of opinion, and therefore much depends on what view one takes on that point on which the evidence is so incomplete and conflicting. Until you can solve that point you cannot definitely place the game project in its relative position. Meantime, while research on this point and on the enemies and migrations of the fly is proceeding, I should suggest, as I have already done, that every effort be made to remove, by clearing or otherwise, the human hosts from access to the fly, thus withdrawing from the fly so many carriers of the human strain of trypanosomes.

4232. Do you consider it possible for anyone to determine the human trypanosome in Nyasaland and in Rhodesia at sight in the blood of an animal?—No, I do not think it is possible to determine at sight, because, as I understand, *brucei* (using the word in the sense in which it is used by Sir David Bruce in his report) and *rhodesiense* are indistinguishable morphologically.

4233. Then I take it you would agree with the conclusion of Dr. Taute at the end of his paper, when he says that no one is entitled to say that a trypanosome in a wild animal or in a tsetse-fly which looks like the so-called *Trypanosoma rhodesiense* is in fact that trypanosome, unless it actually produces trypanosomiasis in man?—I should say that it was by no means certain to be that trypanosome.

4234. One cannot say positively that it is. Would you agree with that?—Yes, I should agree with that.

4235. (Mr. Rothschild.) I have only two or three questions to ask you, and I will begin with the last question of Mr. Austen. It has been said by one witness here that the strain or species of trypanosome which Sir David Bruce identified as *brucei* in Nyasaland was not the *brucei* which he sent home in the dog from Zululand, and according to the French and Belgian authorities the true *brucei* from Zululand is more or less monomorphic, that is to say, it is always a short non-flagellate form or a long flagellate form, while the Nyasaland one is almost always polymorphic, and you find short and long forms in the same animal. Have any of your observers reported that to you?—No. I have had very little experience of those two strains, as a matter of fact. My chief observations in my own class on trypanosomes of Rhodesian origin have been made largely on films of *rhodesiense* obtained from patients and laboratory animals inoculated from them in this country.

4236. But the French report on the Katanga expedition goes so far in order to distinguish the two as to rename the one form "dimorphon"?—It is a question of nomenclature largely, is it not?

4237. The whole is a question of nomenclature in one way, but my reason for asking you the question

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was to try to bring out if any recent observers had informed you that they had come across strains of monomorphic pathogenic trypanosomes in Nyasaland and Rhodesia, where sleeping sickness is, because in Zululand where the true *brucei* is more or less monomorphic there is no sleeping sickness.—I have no information on that point.

4238. Now it is generally assumed, and I think from your answers to the Chairman you agree, that sleeping sickness was brought into the Victoria Nyanza district and Uganda generally, from the West Coast through human agency: at least there was no report of sleeping sickness until the caravan routes were more or less frequently used. Do you not think that as we know that when the human beings on the island became infected during the great epidemic, the source of the infection of the situtunga on the islands and various other supposed cases of infection of animals on the main land was from human beings *via* the fly, the cause of the infection being kept up is that the animals are no longer so tolerant as those animals are on the mainland, and that, therefore, we have fewer cases of infection, while on the islands the animals never having been infected before were affected to a much larger extent?—I should think that the human factor was a very important one in the introduction of sleeping sickness into those islands. If we are to judge from the one island where we have definite history of the part played by the human host in introducing sleeping sickness (that is the island of Principe) the human host there apparently was the factor in introducing sleeping sickness into that island, and I should think it is very probable that the same was the case with regard to the lake country. The history of the spread of sleeping sickness in the islands of Lake Victoria, though not worked out in sufficient detail to say with certainty, indicates that, *e.g.*, in the Sesse Islands the human factor (there being much communication with infected islands further north) was very important.

4239. At all events, we have no information of sleeping sickness having been recognised in Uganda and the Lake district before Stanley's various expeditions—that is so, is it not?—Yes, that is so.

4240. And therefore it is more probable that the infection was brought by human beings and passed on to the animal, rather than that the animals were infected from the beginning, or otherwise we should have heard of sleeping sickness before?—Yes. That, I think, is the suggestion which is also made in Dr. Stohr's report on the Katanga region. He indicates that the malady was probably introduced by infected soldiers and porters entering the area in question. So I think it is probable that something of the same kind happened in the Uganda region.

4241. The only other question I would like to put to you is this: Do you not think that before definitely undertaking an experiment, either on the islands or in a *morsitans* area, the main question to be found out is the distance that the flies under ordinary circumstances can fly or wander, because otherwise you would not be able to say what width of country would have to be cleared of scrub outside your experimental area?—Yes. That is an important point, certainly. You would need to do that, otherwise there would be the risk of the experiment being vitiated.

4242. (Dr. Chalmers Mitchell.) In connection with your duties in Edinburgh you have tried to keep abreast with the work on sleeping sickness generally, have you not?—Yes.

4243. Not relying upon any particular authority?—Quite so. It has been my aim to see as much of the literature as was possible, having regard to the fact that this work on insects and on protozoology forms less than half of my University duties.

4244. Now with regard to the mode of getting the infection of sleeping sickness, what do you gather from what you have read as to the chance of an animal or a human being contracting the disease if blood with trypanosomes in it is injected direct?—It depends on the species of trypanosome and on the animal.

4245. You cannot take man, but what animal would you take?—I would take tame rats.

4246. Would you mind telling me what is the probable chance of infection with the different species?—Take a rat. With regard to *Trypanosoma lewisi*, if you take a tame rat you get infection almost every time.

4247. Almost every time?—I should say so. That is the common way of keeping the strain going in the laboratory. I would like to say that I have not had a great deal of experience of that myself, because we have not made a practice of keeping strains running for very lengthened periods.

4248. *T. lewisi* is relatively harmless to the rat, is it not?—It is generally regarded as harmless.

4249. Then with regard to *gambiense*, what do you say?—I have not had experience of running a strain of *gambiense*. I have never kept it going in the laboratory for various reasons, *e.g.*, safety in class work.

4250. Nor *rhodesiense*?—Nor *rhodesiense*, no strain pathogenic to man.

4251. *Brucei*?—It is some years since I have had *brucei*. We have worked largely with *evansi* and with the trypanosomes that we get in rats and rabbits, and sometimes in frogs.

4252. But these trypanosomes are not pathogenic to man?—No.

4253. You have not had an opportunity of working with the fly, of course?—None whatever on living specimens.

4254. From what you have read would you tell me generally what happens to the fly with regard to different species of trypanosomes?—That is to say, if the fly feeds on an infected animal what is its chance of becoming infected?

4255. Yes.—With *gambiense* it apparently varies considerably in different strains and at different periods. If we may take Miss Robertson's work, which is the most complete account of such researches, her highest percentage of infected flies was somewhere about 15 per cent. when she was using a strain apparently highly favourable to establishing itself in the fly. The percentage is, however, usually much less (about 3 per cent.).

4256. Do you know any corresponding work with other species, or other names, I will say, if you are not certain about the species?—I understand that Drs. Kinghorn and Yorke have somewhere about the same percentage (*i.e.*, about 3 per cent.) with the *rhodesiense* strain—speaking from memory.

4257. What do you mean by saying that the fly becomes infected?—I mean that the trypanosomes have succeeded in establishing themselves in the gut, and in multiplying there in such numbers as to be able to retain their hold on the host.

4258. Do you think that the flies are infective in the same way that a dirty knife would be infective? If you dip a lancet into serum containing trypanosomes, and then scratch a person with it, it is possible that you may convey the infection in that way?—Yes.

4259. What is your view about the flies in that respect?—My view of the fly is that the mechanical method of transmission to which you have just referred, is of very slight importance in practical work. I should say that the cyclical method of transmission, that is to say, the parasite passing through a cycle of changes (I am referring now to *gambiense*) terminating with stages occurring in the salivary glands of the fly, is the important factor in the transmission of *Trypanosoma gambiense*.

4260. Never mind which is the more important. Would you think that they would all be capable of occasionally infecting direct?—I dare say they may be capable, but I should think that the first feed, in which the proboscis becomes contaminated, and the second feed, are generally so far apart that the infection would not be handed on.

4261. About how long do you think would be far enough apart? Do you mean one feed must be following within a few minutes of the other, or hours?—Within a very few hours, at any rate.

4262. Ten, twelve, or twenty-four. Perhaps you do not know?—I do not know definitely, but I should think probably three or four hours at most.

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4263. The important thing is the biological establishment of the trypanosomes in the body of the fly?—In the alimentary canal of the fly.

4264. Is that proved for *gambiense*, do you think?—I should so regard it.

4265. Is it proved for any other trypanosome?—I think I should regard it as clear for the *rhodesiense* form.

4266. What sort of evidence is there?—Well, the evidence is that in both species the trypanosome succeeds in establishing itself, multiplies, and if the temperature be suitable the final stages are found in the salivary glands of some of the flies, from which they can pass to the new host.

4267. Is there any difference in the behaviour inside the fly of different species of trypanosomes?—Both those two that I have named behave similarly.

4268. Do the other alleged species behave in the same way?—Not all.

4269. Can you give me any exceptions?—They do not all, for instance, reach the salivary glands.

4270. Which do not?—I do not remember all the species at the moment.

4271. What does *brucei* do?—If I remember rightly, *brucei* does affect the salivary glands.

4272. *Brucei*, *rhodesiense*, and *gambiense*?—Yes.

4273. Do all of them behave in very much the same way, or can they be distinguished in the fly?—I have not had the opportunity of examining preparations from the fly, but I should say that it would be difficult to distinguish them, because, as I have already said in answering Dr. Balfour's question, the morphological characters become less helpful as compared with those of the blood trypanosome.

4274. So, from an examination of the fly, you would not expect to be able to distinguish between the three species?—No. I think, as far as I can recall the evidence, that it would be extremely difficult to distinguish the three species in the fly.

4275. There are a number of other alleged species or a number of other trypanosomes to which names have been assigned. Do you know the relation of any of them to the fly?—No, I am not familiar with those that are not particularly connected with the human disease. Necessarily for want of time I have had to restrict my inquiries to the latter.

4276. Then you could not tell me if it would be possible to distinguish between these three pathogenic to man (as they are said to be, or possibly one) and the other ones found in animals?—I know there are some differences. I know, for instance, that some of those in animals do not affect the salivary glands of the fly. That might give a possibility of separating them. I do not profess to have studied the details regarding the changes of these animal trypanosomes in the fly.

4277. (Sir William Leishman.) I would like to ask a question or two following from one you have just been asked about the development in the fly. Do you know Sir David Bruce's classification into three groups of the trypanosomes he has been dealing with, as recorded in various papers?—I am not familiar with all the details of the species.

4278. I want to know whether you agree with it. He classifies them into what he calls the *brucei* group, including *brucei*, *gambiense*, *evansi*, and *equiperdum*; and the second group is the *pecorum* group, including *pecorum* and *simia*; and the third group is the *vivax* group, comprising *vivax*, *caprie*, and *uniforme*.—I have not followed that in detail.

4279. He attributes much importance to the changes in the fly about which you were asked just now. I want to know whether from your general knowledge you agree with the classification as being a good one?—I would not like to say, because I have not worked on that point carefully.

4280. You were mentioning some time ago the difficulty of ascertaining good points of specific difference in trypanosomes?—Yes.

4281. Sir David, in his classification, lays great stress on different development in the fly according as the trypanosomes develop in the intestine only, the intestines and labial cavity, or the salivary glands. Is

that a good point of distinction?—If it proves to be constant I should think it would be a useful character, but you would need to examine several strains of each species, because you might find divergence in the behaviour of different strains.

4282. He has examined a number of different strains?—Yes.

4283. Now, with regard to the clinical differences between the two types of sleeping sickness, I would like to ask a question.—I would like to say that I have not paid any attention to those. I have had no experience of clinical differences.

4284. I understood that you had been collecting information from your pupils?—But on the zoological side. I am a zoologist, not a medical man.

4285. You told us that you attribute great importance to the human reservoir in the case of Uganda sleeping sickness?—Yes; in the cases I have heard of in the Congo region and even on the lake shore.

4286. You state in your summary, as regards the Nyasaland form, that man as a reservoir is probably by no means a negligible factor. The reasons you give for that are: "The time of the duration of the disease in man (a few months at least) and the fact that infected cases were found which exhibited no obvious signs are sufficient to show that man as a reservoir is probably by no means a negligible factor." We have heard of one such case—what were the other cases?—Drs. Kinghorn and Yorke record that a native who proved to be infected with *rhodesiense* was apparently in a state of good health a year later, and they state that "many of the cases complained of no subjective symptoms of the disease when diagnosed, and presented very few objective signs." Dr. Fleming records a case in Southern Rhodesia of a native who was infected, on the Betsi River, and was laid up for a week, and who a long time afterwards was found in apparently good health. I think the report says that there was nothing the matter with him, although trypanosomes were present in his glands and peripheral blood.

4287. Was it not possible that these cases had contracted the Congo infection—that they had visited the Congo and had contracted the infection there? That was the explanation of some similar cases. You do not know whether that was the explanation?—No, I have only the authority of Dr. Fleming's report to go upon. He says the trypanosome was of the *rhodesiense* type.

4288. You know that the disease runs an extraordinarily rapid course in *rhodesiense*?—Yes, usually.

4289. The patients get extremely ill, and are knocked over very rapidly, and are, therefore, very likely in the course of two or three days to lie down and not be much in the open. Knowing that, do you think they would still be likely to form the most important reservoir or a very large part of the reservoir?—Did I say "most important"?

4290. You say in your proof "by no means a negligible factor"?—Yes. I would retain the original phraseology and say: "by no means a negligible factor," because although these people are in the neighbourhood of their huts, I understand that in parts of Nyasaland and Rhodesia the huts are actually in the bush, and therefore within easy range of the fly, and if a man moves from his hut, he is in danger of acting the part of a reservoir from which the fly can draw; so that even if he is out a very little, there is an opportunity of fly becoming infected from him. I draw attention to that side of the question in order to bring to notice the human hosts, so that care may be taken to isolate them.

4291. Would you admit then that there was a difference in degree in the part which human cases are likely to play as reservoirs in the two diseases?—I should be inclined to, I think. In the *gambiense* area the disease does not seem to be so acute, and the people might move about more freely, but all this on clinical work I put forward with much diffidence, because I have had no experience on the spot.

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4292. Can you tell us anything about the question of the disparity of the sexes of the fly that has often been reported? Have you any information on that subject?—No, I am afraid not.

4293. (Chairman.) You mention the infection of the antelope by trypanosomes. I do not know whether it was *gambiense* or *rhodesiense*.—The antelope on the island?

4294. No, artificially as a laboratory experiment. The antelope in those cases, I understand, are tolerant but not immune—that is to say, the trypanosome develops?—Yes, I understand so.

4295. When you inoculate with the same trypanosome an animal that is not tolerant, and the disease develops, does the trypanosome develop in exactly the same way?—I have no experience of that subject.

4296. Is anything known about that? If you select a tolerant animal and a non-tolerant animal, and to use a popular phrase they both "take" when inoculated, is the development the same?—I do not know. May I say that there is one point which has not been touched upon, I do not know whether it is a point of any importance, that is the hereditary infection of tsetse-flies.

4297. I should like your evidence with regard to that.—Might I venture to add a note on this point?

4298. Yes.—There is no positive evidence that such hereditary infection occurs. The British Sleeping Sickness Commission fed some 1,800 flies hatched in laboratories on healthy monkeys with negative results, and later 400 others were fed. I think that in both series the number of flies is not sufficient to enable one to say absolutely that hereditary transmission does not occur, for it is quite possible that the same number of wild flies from an infected part of the lake shore might not have contained an infected example. But I think there is clear indication from other sources that hereditary infection is not very likely to occur. Taking the account of Miss Robertson of the development of trypanosomes in the fly, it seems that the entire development is passed in the alimentary canal and the associated salivary glands, and that the trypanosomes do not require to pass into the body cavity, and therefore cannot reach the blood, the ovaries, nor the larva in the uterus. In the search for the track of infection of the salivary glands, the fluid in the fly's body cavity (that is the fly's blood) would no doubt be inspected in the very frequent dissections by Sir David Bruce and Miss Robertson, but there is no record of the finding of any trypanosome in that fluid. In fact in *Glossina* the parasites do not seem even to attach themselves to the wall of the gut, much less make any attempt to pass through the wall. In those cases already known, where hereditary infection occurs, it is known that the organisms pass soon after entry into the mid-gut of the insect very

through the gut wall into the body cavity. For instance, ticks infected with spirochaetes produce infected eggs and infected offspring. It has been shown that there is a rapid passage of spirochaetes through the wall of the gut in *Argas*. Similarly, in lice, the spirochaetes rapidly leave the gut for the body cavity. In the case of ticks infected with *Babesia* the organism rapidly makes its way through the gut wall. Then there is the classical instance of noseema, the organism of pébrine of the silkworm. That also passes into the body cavity and the blood stream, and thus to the ovaries. If we look at trypanosomes in insects, even in the case of the *Trypanosoma lewisi*, according to Professor Minchin, the trypanosomes enter the epithelial cells of the stomach and multiply there. They pass back into the gut again and not into the body cavity. There is no tendency in the case of the trypanosome in *Glossina* to leave the gut, and as it is definitely known that the organism keeps strictly within the alimentary canal it may be presumed that hereditary infection is not likely to occur.

4299. All the witnesses, I think, agree with that view.—If hereditary infection should occur, it would, I think, be rare.

4300. (Sir William Leishman.) In connection with that, supposing there is a granular stage of the development of the trypanosome, necessarily one might have to modify one's views?—Yes, one might.

4301. There is certain evidence, is there not, that there is such a granular stage?—There are granules but as far as I know, the fate of such granules has never been traced.

4302. We have had it stated here that trypanosomes containing granules can be broken up by hypertonic solution and the granules set free and that the fluid that contains no trypanosomes produces infection. That is stated on the work of Mr. Plimmer and Major Fry. It wants confirmation, I agree, but if it was confirmed, it would change one's views?—Certainly, one would have to ascertain the behaviour of the granules in the fly. Is that work published?

4303. I am not sure whether it is published or not, but the statement has been made. Do you think it possible for the trypanosome to live during the pupa stage of the fly as a trypanosome?—The only evidence we have bearing on that question, I think, is the hereditary infection that has been described with regard to crithidia in the puparia of the sheep ked. Assuming the accuracy of the account of the hereditary transmission of crithidia in the sheep ked the flagellates pass through the gut wall and into the eggs, lose their flagella, become rounded and divide, giving rise to ovoid bodies, which become flagellate only after the first meal of blood.

(Chairman.) We are much obliged to you.

The witness withdrew.

MR. SHEPHERD NEAVE, M.R.C.S., M.R.C.P., called and examined.

4304. (Chairman.) Have you been in Africa yourself?—Yes, but not lately. I was in Katanga for a year and a half. I have been in Africa before and since, but I have not been there on the sleeping sickness question for some five years.

4305. Five years ago were you in Uganda?—No, in Katanga, and I came through North Eastern Rhodesia afterwards.

4306. Was sleeping sickness rife at that time?—Yes. I had to delimit the area of the fly, or rather the locality of the fly, in Katanga; that had never been done.

4307. Which fly was it there?—*Palpalis*.

4308. Have you had any experience of *morsitans*?—Yes. I was very interested in the matter because there had been caravans passing and infecting the fly. One could trace how the fly had got infected. The caravans that travelled over the *morsitans* area had never succeeded in infecting *morsitans*.

4309. Only *palpalis*?—Yes, in that particular locality and that particular altitude.

4310. What was the way in which you set to work? What was your *modus operandi*?—Do you mean generally speaking?

4311. In Katanga to delimit the fly locality.—I travelled up the streams, or rather I went down the streams and travelled up them until I could find no more fly, and it was very interesting to find that the fly all stopped in about the same latitude, and so on.

4312. Were you able to judge how far the fly shifted from season to season?—No, I was not, but that has been done since.

4313. Were you concerned with the treatment of the disease or not?—Yes, to some extent. I had not all the time that I should have liked, but we set up a camp for treatment. What I did do I found very interesting, but I should have liked to do more.

4314. Did you find any mode of treatment successful at that time, or has treatment improved since?—There have been a great many other things tried since, but I do not know whether they have been very much more successful. What struck me about treatment was that

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intraspinal injection appeared to be quite possible, and I started with that.

4315. Intraspinal injection of what?—In my case the two important things were soamin and Afridol blue. They neither of them did any harm and they may have been of use.

4316. Did they do good?—I do not know, but the important thing is first of all to know that they did no harm. Where you get the second period of the disease, the nervous portion of the disease, it is probably entirely due to the organism getting into the spinal fluid and the brain, so that if you get a means of attack there, you possibly may have a chance of preventing the death of the patient for a period. Both these drugs were very easy to manipulate and certainly did no harm. Other drugs of more value might well be tried afterwards.

4317. Did you try them in the early stage as a prophylactic or when the spinal marrow was already affected?—Always when it had been affected.

4318. With what results?—I thought it delayed the thing, but my difficulty was that I had to leave my patients in the hands of somebody else and I got only the history afterwards.

4319. The Uganda type was a long-continued disease, was it not, about two or three years?—Certainly two or three years was a common length.

4320. Have you any opinion on the question whether the Nyasaland type is a distinct disease from the Uganda type?—I suppose that, in the Nyasaland type, the Loangwa Valley type would be included?

4321. The *rhodesiense* carried by *morsitans*.—There are two different trypanosomes probably.

4322. Producing a different type of disease?—Probably, but it is only an opinion on the present facts.

4323. What is your opinion about big game as the reservoirs of either the *gambiense* or the *rhodesiense* type?—I think the important thing is to take the *rhodesiense* type. The *gambiense* type, I think, should be considered quite separately; it is not of so great importance with reference to the question of the game. I do not think it is proved that the game carries the same trypanosome as kills man. The work of Drs. Kinghorn and Yorke certainly, so far as it goes, tends to show that, but it is not scientifically proved, in my opinion.

4324. Where, in your opinion, does the missing link occur?—The argument that it is proved relies on the morphology and upon the pathogenicity of the disease. Morphology is hopelessly deceptive with regard to trypanosomes; while the series of experiments that were made in the laboratory as to pathogenicity is hardly sufficient to prove the case. These experiments tend to show that it is so, but to my mind they tend to show also the necessity for further experiment.

4325. They raise a probability, but nothing more. Would you put it as strongly as that or not?—I do not know whether I ought to introduce these words.

4326. Use your own, please.—I think they tend to show what they wish to prove. I would prefer to put it that way.

4327. The line of argument is this, is it not? If you have a man suffering from *rhodesiense* and introduce the trypanosomes from him into a susceptible animal, and if you take big game which is apparently healthy but contains similar trypanosomes and introduce them into a susceptible animal, the two susceptible animals develop the same kind of disease or nagana?—Yes, within a little. When you say the same kind of disease, you mean the same symptoms?

4328. The same symptoms.—Yes.

4329. That raises an inference, does it not, that they are both poisoned from a similar source?—To some extent, but the same symptoms occur with a great many different diseases, and from many different causes. I am not referring only to these particular symptoms, but symptoms in general as well.

4330. The two diseases in this case run very much the same course, do they not? It is not merely a question of symptoms, but they run practically

a similar course with a similar ending?—So I understand, but at the same time some animals are infectable by more than one kind of trypanosome, and those diseases run practically the same course.

4331. So you think the evidence is not conclusive that the trypanosome found in game is identical with the trypanosome which causes sleeping sickness of the Nyasaland type?—I do not think it is proved.

4332. The only possible way of testing it is by way of an experiment like Dr. Taute performed on himself?—Exactly, and I think that is going on now in nature in large measure.

4333. Will you develop that a little?—Experiments made in the laboratory are never so satisfactory as those that are made in nature, and for some years now the Loangwa Valley has had the conditions existing that you have been mentioning. The result of the whole thing is, with regard to *morsitans*, that there are supposed to be 1 in 500 infected, and with regard to buck, the figures are variously stated as 16 per cent. and 35 per cent.; nevertheless Dr. May's last journey showed fewer cases than there were before in regard to humans.

4334. What is the inference you draw from that?—That you are experimenting there on the very set of facts that Drs. Kinghorn and Yorke say produce the disease in man. You have the supposed trypanosome in the game; you have *morsitans* and you have man, and man is being bitten by the *morsitans*, and the *morsitans* is biting the game, and therefore you ought to have an increase in the number of sleeping sickness cases.

4335. I suppose that another possible explanation would be that you have used up your susceptible material, and have now got to the more resisting material?—Yes, I think that is a possibility. That is a theory certainly.

4336. What do you think as to the likelihood of the disease spreading or becoming anything like epidemic?—That is a very interesting question and very difficult to give an opinion about. I think it is quite possible that Sir David Bruce's opinion is correct, and that this is *brucei* that we are dealing with. Another opinion is that *brucei* may be considerably altered by climatic surroundings. Well, if that is the case it is not likely to spread except in similar climatic surroundings. There is talk, I understand, now in Southern Rhodesia, of an apparently similar trypanosome under similar circumstances. As far as I can make out from the reports (with regard to Nyasaland, I am not quite so sure) there is always a river connected with the matter. It has never been proved that the disease has been conveyed to man by *morsitans*, but apparently it occurs in much the same climate and it occurs by a river. I think those things are practically undoubted. If the present places where it occurs are limited, in all probability the spread will be limited.

4337. You think that there may be undiscovered *palpalis* in these places?—No, I do not say that at all.

4338. Then I am afraid I do not quite understand the inference we are to draw. You say that *morsitans* has never been proved to carry sleeping sickness to man?—No, it has never been proved.

4339. When you find sleeping sickness in a *morsitans* area, where do you think it is derived from?—That is the puzzle. I say there is quite a possibility that it is not conveyed by *morsitans*. I have quite an open mind on the subject of *morsitans*. I do not for a moment say definitely that it is not conveyed by *morsitans*.

4340. No, but can you suggest to us any alternative carrier or means of conveying it?—It is very hard. The first four people infected all complained of the terrible bite of some insect quite different from *morsitans* and they attributed it to that. I do not know the value of that but still it did occur. The number of white men that we have had infected is not really very large.

4341. Were you down in the neighbourhood for a time?—I came through it.

4342. Can you speak with authority about the special bite?—No, I am not an authority on that.

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I only take what was written on the subject by these four people.

4343. What do you say as to the experiment of game clearing?—I am all in favour of it, if it is sufficiently local and, we will say, is done by means of a fence or something of the sort.

4344. I am afraid that you can hardly give us any estimate of what it would cost in any particular place?—Well, on the flat, most of the railways cost from 4,000*l.* to 5,000*l.* a mile. A fence is a very different thing from a railway. I do not suppose that a fence would cost more than a quarter of that to erect.

4345. Taking it at 1,000*l.* a mile, what would be a sufficient area?—Roughly speaking, four square miles would be a very efficient experiment. It would cost about 16,000*l.* to put up the fence and probably about 1,500*l.* a year to look after it. And then there is the expense, of course, of the scientific portion of the experiment. It is the vaguest idea, but you ask my idea.

4346. When you were in Africa working at your problem no question of such an experiment had been raised?—No.

4347. You had no occasion to consider it what I may call responsibly?—No.

4348. Putting aside the question of cost, what do you think we could learn, if we could exclude game from, say, a four, or five, or six-mile square area?—There would be a good deal to learn, but I should be sorry to prophesy what you would learn. When you are experimenting in one way you often learn things that you never started upon at all, and there is all that probability and likelihood, and it is excessively tempting. Taking four square miles of *morsitans*, I suppose there would be one or more hundredweight of fly in that area. According to the present theory those hundredweights, or that hundredweight of fly would have been made by the metabolism of blood, and instead of the blood you get the fly. It has never been proved what the fly really feeds on for certain—whether it does feed on anything in addition to blood or not, and there would be a great opportunity of learning that if the fly remained. If the fly did not remain there would be all sorts of arguments in respect to its connection with game.

4349. Have you ever in your experience found any area in which there was fly and no game?—Yes. When I say no game I have not been able to prove it, but I had not the opportunity of shooting any.

4350. No appreciable amount of game?—No.

4351. Does that apply to big game or to smaller animals?—I should say that it applied to the big game. The fly area is of a patchy nature almost anywhere.

4352. Were you engaged on *morsitans* work or only on *palpalis* work in locating the flies?—I had to pass through *morsitans* areas, and I was jotting down the areas occupied by *morsitans* as well, but that was not my first object.

4353. Did you do it for more than one year in succession?—I had two rainy seasons and a dry season.

4354. You cannot tell us at all whether the fly is constant from year to year in particular patches?—All the evidence goes to show that there are places where it is not, and that the actual boundary of the fly area varies considerably.

4355. One witness said that before the rainy season the flies congregate, so to speak, in small patches, and then when the rains come they spread over the country?—Really!

4356. Yes. You have had no experience of that kind?—No, I have not.

4357. (Sir William Leishman.) In your work in the Katanga had you any opportunity of observing *pallidipes* and *fusca*?—I found some *fusca*.

4358. Can you tell us anything about their habits from the result of your observations?—I am afraid I could tell you very little. They were very sparse and I generally caught them in the evening.

4359. Do *fusca* bite at night?—Well, I think it is quite likely, but from my own observations I could not tell you. They did not bite me.

4360. And in the Loangwa Valley have you come across them?—No.

4361. Have you any reason to suppose that they play any part in the spread of sleeping sickness anywhere?—I have no positive evidence to that effect.

4362. (Dr. Chalmers Mitchell.) Can you tell me what you think is the bait for a fly; I mean to say, why does a fly go to a black man or an animal, and so forth?—It is very difficult to say; the first thing, I think, is the movement of the animal. The fly takes advantage of the animal moving to settle in the right place, and he has a way of moving himself sideways before he bites in a way that does not tickle and does not irritate the skin. He seems to be able when once settled to move in a peculiar way, so that you know nothing about the fly till he is actually in the process of biting, and I think he gets more easily in that position by reason of the fact that the animal is moving along at the time. Then with regard to *palpalis* it is curious that the way to catch a fly that my niggers eventually adopted was to sit with their feet in water, and the fly nearly always came near the surface of the water where their feet were, and they got more *palpalis* in that way than in any other. I do not know what it means at all.

4363. Was it a question of odour?—Well, the foot ought to have been protected by the water in this case, you see. It might, though, have made the water smell as well as the foot.

4364. When you are coming up to an area where there are flies, how soon do they come out on you?—Do you mean how near the area?

4365. Yes.—That is difficult. I know the way in which they follow you. I had an interesting fact come under my notice with regard to that in the Sudan once. I was looking for *morsitans*, and the first *morsitans* I found I found in the evening when we came on the edge of a wood about a mile away from a river. I caught it trying to bite me quite in the dark, which is very unusual. I bottled him and waited till the morning. Then there were plenty of them, and having found them, and it being before breakfast, I thought there was lots of time to catch them. We walked six or seven miles down the river. They followed us all the way down to a village seven or eight miles off. They followed me into the hut where I was to have breakfast. I had to catch a few, as that was part of my business, but I put it off till after breakfast, thinking I could catch them. I sent a nigger out after breakfast to catch some. He came back and said, "I cannot see any." I said, "Nonsense; go out again." He went out and came back with the same report. I went out myself. There were none and I had to go right back seven or eight miles to the same spot before I could catch any flies.

4366. Do you say that they had been buzzing round you, so to speak?—Yes, they followed our caravan all the way.

4367. They were not settled on the caravan or anywhere?—I should think that many of them settled at times, but as soon as they proceeded to bite they were probably driven off.

4368. What do you suppose happened? Do you suppose that when they had had their morning's ride on the caravan and saw you settled at breakfast they flew back again?—I do not know what became of them; it may be so. They disappeared; I could not go further than that.

4369. How do flies pass their time?—Well—

4370. Let me explain.—I understand your question and it would be a most interesting one to answer if it were possible. I never had time on these occasions to go into that subject or I should have done. A man who could sit down and find how flies passed their time would be getting very valuable information indeed. If a fly is supposed to be composed entirely of blood, it must spend a good deal of its time looking for it. It is known that in captivity flies require to be fed very frequently, and if that is the case in

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nature too, their time must be spent as a rule in looking for blood.

4371. What I really wanted to get at, to see if you could help me with it, was this: Are they sitting in wait in the brushwood or anything till a source of blood comes along or are they out questing for it?—I have seen them sitting on the path, on the hard earth, and I think that they are all looking out for something moving. I think that anything moving attracts them.

4372. How far off? Have you any idea?—No, I am afraid I could not tell you that.

4373. (Mr. Rothschild.) In answer to Dr. Chalmers Mitchell's questions you stated just now that you certainly found the fly followed you on one occasion at least seven or eight miles?—Yes.

4374. And in answer to the Chairman, you said that you very much approved of the experiment of enclosing a certain area and driving out the game?—Yes.

4375. Now in order to prevent such an experiment from being vitiated, is it not most important first of all to find out the distance over a cleared area of bush that the fly will travel naturally, and not assisted? Supposing that you cleared 200 or 250 yards round the fence, and flies from beyond you could continually come in and out, that would as regards the percentage of infectivity entirely vitiate your experiment. I think that people who have been in fly areas are agreed that if you clear the ground of bush, the flies move away out of the cleared area. Now, supposing the fly could only travel one mile, if you cleared of bush a mile and a half round your fence and you found that the fly could only fly naturally and unassisted for a mile, you would be safe from invasion from without?—Yes, but I am thinking the position where a fly will follow an animal for the distance that I have stated. If you had an impermeable fence—the real difficulty in my mind is how far you can make it impermeable to the game—but given an impermeable fence, if the fly lives on blood, and blood only, you will not find many inside the fence if there is no blood for a sufficiently long time. That is the first point of all.

4376. I understand that perfectly, but what I was leading up to was this: we have been told that the only animals which come into account as reservoirs are the so-called large game and the smaller ungulates such as duikers and bush-buck, and that the smaller rodents and smaller cats and rabbits and hares do not come into account at all. I am supposing that although you may be able to clear out an area of five square miles of big game and small ungulates, you would always leave a large number of small rodents and cats and various other animals, which you could not exterminate without burning the bush right out, which you do not want to do, and therefore the fly would always have a supply of warm mammalian blood. The only question which arises is, whether it would accommodate itself to that form of blood or follow the big game, and if it remained, how long would the same proportion of infectivity remain. In order to study those questions, supposing the fly remained, you must have sufficient cleared space round your experimental area to prevent flies coming in from outside?—I think it would be a great advantage if you did clear your ground certainly, but other game coming up to your fence might very well bring the fly with it if it did come up to the fence over a cleared area, which is doubtful.

4377. I fully appreciate that, but the chances are that if you cleared the game out of an area, and made a fence and cleared the bush away, the game would not advance over the cleared area but would be suspicious?—Although I have no experiments to go upon, I very much doubt the likelihood of *morsitans* flying a great distance on its own account without being induced to do so by following game. I very much doubt their migrating without reason. They do migrate to some extent, I believe, but it would not be very rapid.

4378. You think that, unless they saw moving prey, they would not undertake long flights, and you have no experience of their undertaking long flights in

search of prey?—No, not on their own account without seeing it.

4379. (Mr. Austen.) Have you ever found pupæ of *Glossina morsitans*?—No, not that I have proved to be *morsitans*.

4380. You do not know of your own knowledge where the fly breeds?—No.

4381. Can you offer any suggestions as to the most hopeful means of attacking *Glossina morsitans*?—Well, the only way that I know of for certain by which *morsitans* has been driven out is by civilisation. The other way that has been spoken of is the rinderpest that swept over the country. With regard to those two last things, I very much doubt the connection.

4382. I do not quite follow you.—I mean I very much doubt the connection between *morsitans* having disappeared and rinderpest having arrived. The only way in which we know of *morsitans* disappearing is by civilisation, and there is a coincidence with regard to rinderpest and *morsitans* that *morsitans* disappeared about the same time in some places.

4383. When you say that *Glossina morsitans* has disappeared owing to civilisation, what do you regard its disappearance as being precisely due to in that case?—I think it is an enigma. Really I do not know. I think it is very difficult to say. Of course the first thing you think of is food and shelter, and whether there were plenty of animals for them to have gone on feeding on, especially considering the very vigorous way in which they breed and increase.

4384. Many people consider that the admitted disappearance of *Glossina morsitans* subsequently to the advance, or concurrently with the advance of civilisation is necessarily due to the disappearance of big game. I take it that you do not exactly agree with that?—No. I mean I do not consider that the thing is proved.

4385. Do you consider it possible or likely from your experience, that *Glossina morsitans* could be effectively attacked by the introduction of insect enemies from elsewhere, if we could find such enemies?—It is an excellent theory, but I very much doubt it. I think that the surroundings of *morsitans* at the present time are such probably that no such insect could survive or no such parasite could survive. I suppose it is a parasite of some kind that you mean.

4386. Either a parasite or a predaceous insect.—If such a predaceous insect exists, I should think we should have found such an animal actually at work now. The odds are that if you imported one from elsewhere the surroundings would not be sufficiently favourable. Those are only guesses—those are only opinions—ideas.

4387. Insects have been imported before now from one country into another country in order to deal with an insect which was a pest in the second country, have they not?—Yes. All these things are worth trying. An experiment would be most interesting.

4388. You stated to the Chairman that *Glossina morsitans* had never been proved to convey sleeping sickness to man. Has *Glossina palpalis* ever been proved to convey sleeping sickness to man?—Yes, I think so, practically.

4389. Not by direct experiment, of course?—No, not by direct experiment.

4390. How then—by a series of coincidences?—Yes, and by the fact that people who have been in a *palpalis* district and have been known to have been bitten with *palpalis* have had sleeping sickness, and those that have not been bitten have not had the disease. I am talking of *gambiense* now.

4391. I gather that you consider that the evidence with regard to *morsitans* and the human trypanosome in Nyasaland and Rhodesia is not so complete as to enable us to say positively that *Glossina morsitans* actually conveys the disease?—Exactly.

4392. (Dr. Balfour.) You told us how flies will follow caravans for long distances, and other witnesses have told us how they may be artificially transported by trains, by native boats, and possibly even by steamers. One witness, indeed, told us that a place which had been free of fly became reinfested. Now, these things being so, and it being possible that

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countries now free of fly might become infested with fly again, would you agree to the proposition that if this game experiment were carried out it would be quite a legitimate thing to ask such countries to contribute towards the expense?—I am very much of that opinion. When I was coming home I foresaw a good deal of these questions, and I tried to get people to see it, especially in the southern part of Africa generally, and I saw various officials at the Colonial Office, and all the rest of it, but I quite failed to get any enthusiasm.

4393. In other words, it is not a problem only affecting sleeping sickness areas or areas with tsetse-flies in them, but any country which might harbour tsetse-flies if introduced there?—Most certainly, and even beyond that the whole thing is so absolutely knit together in Africa. If you are going to kill off your black men, you will kill off labour, and no part of Africa does not require labour.

4394. The whole of Africa is interested?—The whole of Africa is interested.

4395. And might well be called upon to support the experiment?—Certainly, I think so.

4396. When you were in the Sudan you initiated some interesting experiments as regards serum treatment of trypanosomiasis, which, unfortunately, did not get very far, but that was not your fault?—Yes.

4397. Have you done anything more in that respect?—No. I have often thought of it and wished to do so.

4398. What are your views on the point? Do you still think it of some value?—The question of treatment, prophylactic or otherwise, is, I think, besides the question of medical treatment of the spinal canal, an important one. The other point is the serum treatment. We know that one method of differentiating trypanosomes one from another is by means of serum that has acquired a certain amount of immunity. You may give that serum an artificial immunity by infecting the animal with a particular trypanosome, but at the same time you may also have an animal that is immune to a particular trypanosome. For instance, take humans. Humans are immune to nagana, and it is well known that the treatment of trypanosomiasis with human serum has a definite effect upon it. Now we know that the big game does not suffer from *gambiense*, and therefore the treatment of a monkey, say, with the serum of antelopes, I think, is a most interesting experiment, and might lead to something in regard to the treatment of man afterwards.

4399. (Chairman.) Has it been tried?—You tried it, I think, Dr. Balfour?

(Dr. Balfour.) I had some serum sent to me by Dr. Sheffield Neave from water-bucks and animals in a trypanosomiasis area, and I carried out a few experiments on monkeys and gerbils with, I cannot say very definite, but rather interesting, results. Unfortunately it was very difficult to keep the serum sterile and it became infected. Further experiments would thus have been vitiated and I had to give them up. It was very difficult to get the serum. You have to carry apparatus, and if you shoot an animal you have to rush up as quickly as possible and draw off the blood. So you will see that it is a very difficult thing to carry out properly. It had a distinct effect on the trypanosomes, I may say, but I do not think any animal was cured of trypanosomiasis by such injections as I gave.

(Chairman.) Was it a constant effect; had it the same effect on all animals?

(Dr. Balfour.) Yes, as far as I remember.

(Witness.) The thing is a little complicated. It would make the experiment still more interesting if one had only the opportunity of carrying it out on the spot. It is complicated by anaphylaxis and other things.

4400. You have found a line of treatment?—I have found a line of treatment.

4401. That should be carried out on the spot?—Yes, quite so. That is my point.

4402. Could it be done to some extent in Zoological Gardens?—All experiments are valuable,

and the Zoological Gardens can give you some results, but until you get results one would hardly like to prophesy what will happen. No doubt doing it on the spot would be far better.

4403. Game countries would be ideal places?—Yes, game countries would be ideal places.

4404. (Dr. Bagshawe.) There are one or two points that arise out of your answers. You said that so far as Rhodesia was concerned, all the cases of human trypanosomiasis were associated with the Loangwa River, the Busi River, or other rivers?—Yes.

4405. What is the height of Fort Jameson above Loangwa Valley?—Something considerable. I could not tell you at the moment as I do not carry it in my head.

4406. Is it not the fact that two or three cases have been found on high ground near Fort Jameson?—Yes, but those that I have read about are so few, and being so few I have always thought there was an excellent chance of their being imported and not having been infected on the spot.

4407. What is the height of Serenje?—I could not tell you in feet.

4408. That is on highish ground, is it not?—On higher ground, certainly.

4409. There have been at least two cases which have been stated to be infected near Serenje, and in one instance it was said that that was the only possible place.—Is that so?

4410. Yes, so I do not think myself that it holds good that all these infections have been contracted in the river valleys. Now you advised an experiment of game extermination in an area of four square miles?—Yes, I suggested it.

4411. You suggested that that area would be large enough?—I hoped it would. I was thinking of the money to a large extent. One would like to increase it.

4412. If you suppose that that piece of blotting paper represents a fly area, is it not the case that you may find fly at one time in this portion of the area and at another time somewhere else, say, there, and another time there, that they move about in the area?—Yes; discoveries of fly are of that nature. As to the moving about, I leave that to be discovered and proved.

4413. If you made your enclosure of four square miles at this corner, it would be rather unfortunate if, as soon as you had spent 1,000*l.* a mile on your fence, all the fly disappeared into another part, would it not?—The great thing about the enclosed area is to have no game in it, to have none of the things that are supposed to attract the fly. Having found out what is going to happen, then you will know more about the fly. Given the game, the fly are supposed to move about. Now, if there was no game would they move about like that? That alone would be a fact of value to prove.

4414. I think there is a good deal of evidence that their movements are not always related to the movements of the game.—Quite so. Experiment would prove that, at any rate.

4415. Supposing you put up your fence, and before you destroyed the animals all the flies disappeared, you would not be able to prove much?—If there was no fly when you had put up your fence you would prove nothing.

4416. Yes.—It would be necessary to find a place with plenty of game. Suppose that when you brought the fencing materials into the four square miles, the game remained but the fly disappeared? Is that your question?

4417. It would spoil the experiment would it not?—Yes, but it would be a very curious thing to happen. Do you not think so?

4418. No, I do not think so at all.—Really.

4419. What in your opinion is the presence of *morsitans* determined by?—I have no opinion on that at all. It is one of the most puzzling things there are. It is attracted by game in motion, and it occurs in certain sorts of localities which are often rather hard to define.

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4420. But is its presence determined by the presence of game or the presence of a particular kind of vegetation?—Vegetation is a point, I think, and game is another point. I do not know whether it would be determined by either by itself.

4421. You have not yourself made any observations of the migrations or the movements of tsetse-flies, seasonal or otherwise?—No; I have only observed the fact that they are very much more active and that there are many more of them at one time than at another. I have never been able to find out if it is seasonal.

4422. Have you known these flies bite at night?—I have been bitten at night.

4423. By *morsitans*?—Yes, by *morsitans*.

4424. You caught the fly and determined what it was?—Yes. The first fly I found in the Sudan was one biting me at night.

4425. What time was that?—The sun had gone down. We had had a long trek and were just camping.

4426. (*The Earl of Desart*.) I would like to ask you quite a general question. I did not hear what you said to the Chairman, but he has been repeating some of it to me. I understand you have very little doubt that nagana in cattle may be conveyed to cattle from the reservoir of wild game by the fly?—I have always held that opinion.

4427. Do you hold the same opinion about trypanosomiasis in man being conveyed from the same reservoir?—I do not think there is any proof to that effect.

4428. I was going to follow that up, if you had thought that possible if not proved, by asking how you account for the fact that whereas you cannot keep cattle at all in a fly country, man relatively suffers

very little.—That is what I was arguing as showing that the thesis laid down has not been proved. The thesis laid down is that the game contains this trypanosome that is fatal to man, and that it is conveyed to man by *morsitans*. I say that that is going on in the Loangwa Valley every day and yet man has not been infected in larger numbers, but probably in less. That was the result of May's last expedition.

4429. If the wild game does harbour the trypanosome which we call *rhodesiense*, which is a human trypanosome, then man must either be resistant or have acquired some form of toleration?—Quite so. We are now talking of the black man, of course.

4430. Yes. That is another theory?—Yes, that is another theory that is quite possible.

4431. Are you yourself hopeful about the results of entomological research? I am asking you quite generally.—You mean in reference to what?

4432. The bionomics of the fly.—That we shall discover the bionomics?

4433. Are you hopeful about the results of research in that direction?—That we shall learn how to stop the disease or cure the disease?

4434. How to destroy or eliminate the fly.—I am not at all hopeful. The only way in which we have ever heard of it being got rid of hitherto is by civilisation, and there is the possibility that rinderpest had something to do with it.

4435. Local clearing removes it from the place cleared?—Yes, I believe it does, and it has to have certain kinds of vegetation and climate and so on.

(*Chairman*.) Thank you very much.

The witness withdrew.

FIFTEENTH DAY.

Tuesday, 9th December 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

SIR MACKENZIE CHALMERS, K.C.B., C.S.I. (*Chairman*).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Sir JOHN ROSE BRADFORD, K.C.M.G.
Mr. E. NORTH BUXTON.
Dr. W. A. CHAPPLE, M.P.

Sir EDMUND G. LODER, Bart.
Dr. C. J. MARTIN, F.R.S.
Dr. P. CHALMERS MITCHELL, F.R.S.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Sir STEWART STOCKMAN.
Mr. A. C. C. PARKINSON (*Secretary*).

Captain C. M. DREW, R.A.M.C., called and examined.

4436. (*Chairman*.) I think your service is the Royal Army Medical Corps?—Yes.

4437. And you have been on duty for some time in Africa?—Yes.

4438. How long?—I was five years in the Egyptian Army, and I have been with the Sudan Sleeping Sickness Commission for three years.

4439. That is to say, for three years out of that time you have been on sleeping-sickness duty?—Yes.

4440. What has been the nature of your duties generally?—Administrative and clinical.

4441. That is to say, you have treated cases?—Yes.

4442. In what parts of Africa?—I was first in the Western Bahr-el-Ghazal; I did about seven months there, and then I was in the Lado Enclave, and then I went back to the Western Bahr-el-Ghazal afterwards.

4443. Have you seen the two forms of sleeping sickness, the sleeping sickness produced by the bites of *morsitans* and the sleeping sickness produced by the *palpalis*?—No, I have only seen the sleeping sickness of *palpalis*.

4444. The trypanosome has been the *gambiense* in that case?—Yes.

4445. You have not come across the *rhodesiense* personally?—No, I have not.

4446. From what you have heard and read have you come to any conclusion as to whether there are two distinct forms of sleeping sickness?—No, I cannot give any opinion on that; I have only seen the *gambiense* trypanosomiasis.

4447. You are acquainted with certain *morsitans* areas, I imagine?—Yes, both in the Lado Enclave and the Western Bahr-el-Ghazal; that is the Southern Anglo-Egyptian Sudan really.

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[Continued.]

4448. In the areas where you observed *morsitans* there has been no sleeping sickness?—In the areas where I have found both *palpalis* and *morsitans* in the Lado Enclave I have found sleeping sickness. In the Western Bahr-el-Ghazal where there are *palpalis* and *morsitans*, I have found no cases which I think have been infected in the Western Bahr-el-Ghazal.

4449. They have come in from areas where the *palpalis* has been found?—Yes, from the French Congo.

4450. And the disease is of the typical Uganda type?—Yes.

4451. Have you examined the cases with reference to the particular trypanosome found in the sleeping sickness cases?—No, I have not; I simply made a clinical diagnosis of the cases by gland puncture.

4452. What do you obtain from that?—I simply find the trypanosomes and make the diagnosis.

4453. Does that show exactly what trypanosome it is or not?—No, it does not; it simply shows that it is trypanosomiasis.

4454. As regards particularly *morsitans*, did you find *morsitans* in any place where there is no big game?—I have found *morsitans* in two places in Africa, in the Lado Enclave and in the Western Bahr-el-Ghazal. To take the Lado Enclave first, the Belgians when they were in occupation of the Lado Enclave indiscriminately shot out the game; they sent the soldiers out and shot out the game. The result is that in the districts where the game has been shot out you find very few *morsitans*. In the districts where there is game you find *morsitans*. *Palpalis* seems to me to be irrespective of the presence of game.

4455. In those districts where the game has been shot out is *palpalis* as numerous as ever?—Just as numerous as ever.

4456. Have you any opinion upon this point—one or two witnesses have suggested it to us—that *morsitans* particularly follows buffaloes?—Yes, I think so. Take the Western Bahr-el-Ghazal. I was there in 1910 and I asked a native chief why there were no tsetse *morsitans* fly and he said, "Long ago the buffalo were here, and the game, and now all the natives here carry firearms and all the buffalo have been shot out and frightened away and there are no tsetse fly now." I saw the same man again three years afterwards, and he told me exactly the same story.

4457. Do you think in those areas the smaller game has been shot out as well as the bigger?—No, you will find smaller game like oribi and the dik-dik.

4458. Has any examination been made of them to know whether they contain trypanosomes?—I do not think so.

4459. As regards *palpalis* do you find that they are pretty generally spread?—Wherever there are suitable conditions. I have always found it wherever I have been in the Southern Sudan.

4460. Suitable conditions being what—with regard to streams of water and so on?—You want a stream with sloping banks and shade.

4461. Did your observations lead you to tell at all how far away from a stream *palpalis* will thrive?—Once I was camped about a quarter of a mile from a river and they came right up to where I was, following water-carriers, and they have come, perhaps, half a mile as well; they have followed me half a mile.

4462. In anything like numbers?—Just one or two. I was looking for them or I would not have found them.

4463. In your experience do they wander that kind of distance unless somebody comes from a *palpalis* area and, so to speak, brings them with him?—I do not think so. I think they simply follow something they want to feed on. They would not leave the river bank unless there was some prey for them.

4464. Do you know anything about the breeding places or where they deposit their pupæ?—I was with an entomologist and he found them in the dry sand about 30 yards from the bank on a river called the Yei in the Lado Enclave. That is the only time I have seen pupæ found.

4465. Were they numerous?—He found three.

4466. In your administrative experience what do you say is the effect of clearing round villages?—On the river Yei in 1911 we did a large clearing of about, roughly, three miles long on the banks of the river Yei, which was the centre of the sleeping sickness district in the Lado Enclave. This place was swarming with tsetse-flies before the clearing was begun. After the clearing was carried out—we cleared about 50 yards from the bank, and three miles along on each bank of the river—we seldom saw a tsetse-fly; they practically disappeared. In the house I only saw them perhaps if there had been people crossing the river.

4467. (Mr. Buxton.) You are speaking only of *palpalis*?—Yes. Perhaps if a native came from the bush where the place had not been cleared, you would find a tsetse-fly following him, but the clearing got rid of the flies in that particular part. I put on the backs of the labourers, when they were clearing the place, blue cloths smeared with some gummy substance to find out if it was the case that really the flies had disappeared. There were, perhaps, 100 labourers and in one evening I could only find one tsetse-fly on the backs of these labourers.

4468. (Chairman.) Do you think that the sticky stuff puts the fly off at all?—I think the smell attracts the fly.

4469. What substance, as far as you know, was it that attracted the fly?—It was a proprietary preparation that I got sent to me called "stickphast." I do not know what it was made of.

4470. Still it has a scent or smell?—Yes.

4471. Do you think that people who necessarily have to go through fly belts could protect themselves in that way at all by wearing, say, some cloths on their backs, those cloths being smeared with some material of that sort?—I do not think you could protect yourself from the bites of tsetse-flies; I do not think it possible if you are in a fly area.

4472. Have you been bitten yourself?—Yes.

4473. I hope with no ill result.—I had a party of nine men with me when I was in the Lado Enclave and we were going through all these districts and I have since heard that three of those men have got sleeping sickness.

4474. Is that entirely of the *gambiense* type?—Yes.

4475. You attribute all those cases to *palpalis* and not *morsitans*?—*Palpalis*, yes.

4476. In any *morsitans* area have you seen any clearing done?—For *palpalis*?

4477. No, for *morsitans*?—I have not.

4478. Hitherto in your opinion it has not been necessary?—Not for *morsitans*.

4479. *Palpalis* I think you told us, seemed to be equally numerous whether there was game or not?—Yes.

4480. Is it known at all on what *palpalis* depends for its livelihood?—I think they feed on monkeys and small buck if there is not any big game, because there are always a certain number of small buck about and monkeys.

4481. Have you ever seen them feeding on reptiles of any sort?—No, I have not.

4482. Your administrative duties consisted in what—in the supervising of the clearing?—In going round visiting the villages, collecting the infected and segregating them, and doing clearing.

4483. Was there much difficulty in segregating them or were they amenable?—At first we had a good deal of difficulty because the people went into the bush when we went to the villages, but when they got accustomed to us going round we had no trouble at all. I know of one case the last time I was out in the Western Bahr-el-Ghazal where a man had a little girl slave infected with sleeping sickness and when he heard I was coming along he took her out and left her in the bush to die.

4484. Was the body ever found?—No, she was abandoned in the bush. She was a young girl under 12 years of age.

4485. And she was never found?—She was never found.

4486. As your work is better understood you find less difficulty?—When you get to know the language

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and customs—one has to go round at certain times in the year to get the people in the villages—you find that ultimately you will see every man, woman, and child if they get to know you properly.

4487. What did you do? Did you have a segregation camp?—Yes, in the Lado Enclave we had a segregation camp at Yei and we brought all the patients from that small area infected with sleeping sickness and kept and treated them there.

4488. I suppose if those camps are perfectly free from fly their relations can visit them without restriction?—This camp was quite free from fly; we never found a fly in the camp and we put no restrictions on the movements of the relations coming and seeing them.

4489. The object of the segregation camp is to keep the patient away from fly?—Yes.

4490. How many cases of sleeping sickness have you found?—I have seen about 16,000 people and I have got 250 cases of sleeping sickness, roughly.

4491. In a comparatively small area?—I saw 9,700 people in the Lado Enclave and I got the 250 cases there. In the Western Bahr-el-Ghazal, the last time, I saw 6,702 people and I got 10 cases of sleeping sickness.

4492. As regards those cases did they all run a somewhat lengthened course, or did you have any what I might call malignant cases and early death?—One woman came in and she had a little boy who was infected with sleeping sickness; I examined this woman one day and I was pretty certain that she had sleeping sickness, but I could find no trypanosomes; I examined her again another day and then another day, and I meant to have examined her the next day but in the night she died of an epileptoid attack. We get cases like that which end in an epileptoid attack. On the other hand, we get cases which seem to run a very chronic course. I had one case in the Western Bahr-el-Ghazal where the man must have had sleeping sickness for 12 years if his story was true.

4493. Were there no symptoms?—No symptoms at all, simply glandular enlargement and the presence of trypanosome.

4494. Would that man be a carrier and a source of danger to others?—I think he would be.

4495. With regard to those cases which have continued, so far as they have come under your notice, are they treated cases or untreated cases?—Untreated cases.

4496. What do you find has been the effect of treatment?—The only effect I have seen on cases is this, that it gets rid of the trypanosome for a certain time, but if you treat cases with atoxyl I must say I cannot see it has any effect as far as curing is concerned.

4497. It postpones the severer part of the illness?—Yes. Then after treatment sometimes these patients get epileptoid attacks and die. I tried a man with arseno-phenyl-glycin and trypanosan. He was to get three doses, but after he got his third dose he died the next day.

4498. Did you attribute that to the sleeping sickness or to the effect of the drug?—He had an epileptoid attack; it may have been the elimination of the toxin from the effect of the drug.

4499. For instance, if you gave this drug to a healthy person it would not produce epileptoid attacks, would it?—No.

4500. Therefore, it is the drug *plus* the disease which produces this condition?—Apparently.

4501. It may be by killing the trypanosomes and intoxicating the blood; is that your theory?—I think that is so.

4502. Have you tried any other arsenical remedy?—I tried salvarsan.

4503. That is arsenical?—Yes. I have also tried antimony, but my experience of antimony was not so good because I gave it in rather a concentrated suspension; I gave one grain in 10 c.c. of water and I did not find that was the slightest good. I saw it given to several cases by one of the officers with me and they seemed to do well; the trypanosomes disappeared.

4504. Do you know anything about Captain Ranken's work?—I was with him; he came up with me.

4505. How long have you been home?—I have been away from that place for two years now.

4506. Have you followed Captain Ranken's work?—I have read about it.

4507. Could you tell us the general result?—I cannot tell you anything recently about it; I can only tell you about his first attempts at it.

4508. We were told by a witness the other day that what he found most useful was an intravenous injection of metallic antimony?—That is what he was giving when I was there.

4509. And he was giving it intravenously?—Yes. I started giving it, one grain in 10 c.c. of water, but apparently there was not enough fluid, and he gave it in, perhaps, a pint of water or half a pint of water and injected very slowly. Taking the blood of the patient three hours afterwards you could see the trypanosomes disappearing.

4510. From what you have read and observed, is that a permanent death of the trypanosomes, or, as in the case of the atoxyl, will they reappear?—I think, myself, they will reappear in time.

4511. But it may produce a longer period of freedom from trypanosome?—That is so.

4512. You have not heard of any cases which you could call cures by this treatment?—No, I have not.

4513. Have you any suggestions as to what further experiments and treatment should be tried?—No. I have not done any research work on treatment at all.

4514. I mean have you any suggestions as to the lines on which research work might proceed?—No, I have not.

4515. (Sir John Bradford.) You have seen between 200 and 300 cases of sleeping sickness, I gather?—Yes.

4516. Could you tell us roughly the number of acute cases—cases which run an acute course?—Out of 250 I had 40 deaths, I think; they ran an acute course, just came in, and died in about a fortnight to three months.

4517. Does that apply to untreated cases?—Both.

4518. How many untreated cases have you seen run an acute course?—I mean the cases which just come into the camp and perhaps die in a week; they get one injection of atoxyl and then die. I do not mean continuous treatment.

4519. Had you reason to think those people had not been infected long?—I really could not say about that.

4520. From your clinical experience did they come in in an early stage of the disease?—They came in in what we call the secondary stage.

4521. They were not in the early febrile stage?—No, not that stage.

4522. I want to limit the question to untreated cases, if possible; as regards untreated cases running an acute course, did you see any cases of keratitis?—No, I never saw that.

4523. You have never seen keratitis at all?—I have never seen that amongst my patients in the sleeping sickness camps.

4524. Did you have any cases with oedema?—Yes, oedema of the eyelids was very common.

4525. Were those early cases or late cases?—I think otherwise they were quite fit; they were patients just going about doing the ordinary work of a native, and I simply noted them by the condition of their eyes.

4526. Did you see any cases of oedema of the limbs?—Not in the stage in which they were going about.

4527. Only as a late symptom?—That is so.

4528. As regards the cases which ran a chronic course, did they give the typical clinical picture associated with sleeping sickness?—In the cases which run a chronic course some of those cases go about for a year, and these cases presented no clinical symptoms at all. The only way they would be found out would be by palpation. Otherwise they have no symptoms.

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4529. To recur to the acute cases for the moment, did you see any cases where there were epileptiform attacks in your patients?—I have mentioned one woman who came in apparently quite well; she was carrying a small boy, and he had sleeping sickness, and I thought the mother had it too. I did a gland puncture on three successive days, and I was going to examine her on the fourth day, but she died in the middle of the night with an epileptoid attack.

4530. (Dr. Bagshawe.) There is one point in your statement I would like to ask a question about. You say "On a march of 131 miles in one of these districts I found only one tsetse-fly and saw only one herd of 'hartebeeste.'" What season of the year was that, do you remember?—That was in May.

4531. Was it in the dry season or the wet season?—The rains had been on for about two months.

4532. It was at the season of the year in which, as a rule, you find tsetse-fly?—Yes.

4533. With regard to your 250 patients could you tell us what was the usual age? Did they vary?—They varied. I had one patient who must have been 80, and another a few months old.

4534. What was the average age?—The average age would be between 20 and 30. Taking my last 10 cases which I got in the Western Bahr-el-Ghazal they were all men between 20 and 30 years of age, and none of the women were infected.

4535. What proportion of children did you have?—I got no children in the Western Bahr-el-Ghazal. In the Lado Enclave I found out of 250—perhaps 15 children.

4536. That is rather a small proportion?—Very small.

4537. How do you account for that?—I cannot account for it.

4538. In what way do you think, as a rule, the people there get infected? Do they go to the river to fish?—They go down and fish at certain times of the year and live on the river banks where the tsetse-fly is very common, and I think that is how the disease is spread.

4539. The children go as well?—Yes.

4540. But they do not seem to get infected?—We never found many of them infected.

4541. You cannot suggest any reason for that?—No, I cannot.

4542. Did I understand you to say you did not get any women?—In the Western Bahr-el-Ghazal we got no women at all, except when I examined 26 people who came from the French Congo and I got one woman amongst them and one little girl.

4543. Can you suggest any reason for that?—I think the reason why there were no women infected in the Western Bahr-el-Ghazal is because the disease is not endemic in the country yet; I do not think the flies are infected there.

4544. Do the women in that part of the country carry most of the water?—Yes.

4445. So that you would expect them to be infected?—If the disease was endemic I think you would find women infected as well.

4546. But in the part of the country where the disease is endemic, do you get many women patients?—Just about equal numbers.

4547. Did you make any examination of game?—No, I did not.

4548. (Mr. Buxton.) I understood you had made a good many clearances near the villages?—Yes.

4549. Were those clearances very costly? In the first place what was the nature of the bush there?—It consists of big trees and thick undergrowth on the river banks.

4550. Gum trees or thorns?—Ebony trees—not thorns—and a tree like a lemon tree; I believe it is a species of lemon tree growing on the banks of the river. Then you have great big trees, something like oak trees at home. The cost, of course, depends entirely on what is the rate of payment of native labour.

4551. You told us you cleared, for instance, a length of two miles and approximately fifty yards

wide or something of that sort; am I right?—Fifty yards on each side of the river bank.

4552. Could you give us what that particular area cost to clear approximately?—The monthly cost to keep it up was the expense of 100 men working at a penny a day, that is 8s. 4d. a day.

4553. After it was first cleared, you mean, in order to maintain it?—Yes. I should think the clearing altogether would cost about 200l. to 300l.

4554. According to my arithmetic that area would be about 40 acres. It was 50 yards wide and I understood you to say two miles long.—Three miles.

4555. I do not want to pledge you, but I wanted to get some idea of the cost.—The original cost would be between 200l. and 300l.

4556. (Sir Edmund Loder.) Just going on with the clearing, you cleared 50 yards; I suppose that was the heaviest jungle actually on the bank?—Yes.

4557. And you did not do more because behind that it was not so thick?—Yes, you come to grass behind.

4558. That happened really to be the width of the bank?—Yes.

4559. You said that in some districts there was both *morsitans* and *palpalis*, but you put down all the sleeping sickness to *palpalis*?—Yes.

4560. How do you know that *morsitans* do not help?—In the Western Bahr-el-Ghazal it certainly cannot, because the fly is so common there that if *morsitans* did produce sleeping sickness everybody would be affected with it; you cannot walk through the place without being continuously bitten.

4561. You acquit *morsitans* of any evil effect?—So far as I know.

4562. Not to domestic animals?—It kills all the domestic animals; all donkeys and mules ultimately die of *brucei*.

4563. I think you said something about the clearing not being any use for *morsitans*?—It is no good, because they do not live on the banks of a river; there is nothing to clear for them.

4564. And they fly further?—I always find them in the grassy areas which have become dried up.

4565. You think clearing is no use against *morsitans*?—I do not think it is a practical idea at all.

4566. You talked about the ages of the natives. Do you find that they know their ages? Most of the natives I have been amongst do not know. Do you guess it?—No, I ask them questions; for instance, if they remember Gordon.

4567. It is very vague?—Yes, and then I go by their teeth.

4568. Do you not think the men go more into the woods where the fly would be likely to be, and into the jungles than the women and children? Is not that one of the reasons why they get it more?—The men travel much further, and that is why in the Western Bahr-el-Ghazal there are only men infected, because they travel to the French Congo, but so far as the fishing expeditions are concerned I think you have more women than men.

4569. (Mr. Anstey.) What is the history of sleeping sickness in the Bahr-el-Ghazal?—In the Western Bahr-el-Ghazal the first case was found or suspected in October 1909, and ultimately I diagnosed it in January 1910. In the Lado Enclave it already existed. When we took over from the Belgians in 1910 they had found a few cases of sleeping sickness, but it has always been in the Lado Enclave, of course; it came there from the Belgian Congo.

4570. Then the disease has been introduced from the Lado into the Bahr-el-Ghazal?—It is not in the Southern Bahr-el-Ghazal; in the Western Bahr-el-Ghazal it will be introduced, if steps are not taken, from the French Congo, not from the Belgian Congo.

4571. But it has been introduced now into the Southern Bahr-el-Ghazal?—I believe they got one case at a place just on the frontier, but he had come, I believe, from the Belgian Congo, and had been staying there.

4572. Where were those 250 cases you said you found?—In a small area in the Lado Enclave, which was round the centre of the Belgian Administration.

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4573. That was 250 cases in 16,000 people, I think you said?—In 9,000 in this particular part.

4574. That would be roughly about 2·5 per cent.?—Yes, it worked out at between 3 and 4 per cent.

4575. How many cases have actually been found in the Bahr-el-Ghazal at the present time?—In the Bahr-el-Ghazal itself there have been 10 cases. I found 10 cases just now, and there were two cases found in 1910, five in 1911, nine in 1912, and five in February 1913, but these had all been travellers coming from the French Congo—French Congo natives coming across and being found out, as they have to undergo a medical examination when they come into our territory.

4576. I suppose the number of cases, or the data available, are not sufficient to enable you to say whether the disease is actually on the increase or the contrary among the natives in the Bahr-el-Ghazal itself?—In the Western Bahr-el-Ghazal I do not believe it exists at all; it is not endemic there yet. In the Lado Enclave it is, and they still get a certain number of cases. The place was thoroughly gone over and all the cases were brought in, but they still find one or two cases whenever the inspector goes round the villages.

4577. With regard to the areas where you find *G. morsitans*, I understood you to say that you found it in grassy places. Is there no bush there at all?—Of course there is bush round about, but I think it is in the dried-up valleys that the *morsitans* breed, because when you come to these places you always find far more *morsitans* there than you do in the ordinary bush.

4578. You find far more *morsitans* in grassy places where there is no bush?—Yes.

4579. You have not, I think, actually found the pupae?—No.

4580. Would it be possible to destroy those grassy areas by burning?—That is quite simple.

4581. So that if you are correct in thinking that the flies breed there one might have some chance of eradicating them by attacking them at their place of origin?—I think it was pointed out by Dr. Pearson that the *morsitans* fly disappears or gets few in number at the beginning of the dry season, when the bush fires are going on.

4582. As to the alleged association of *morsitans* and buffalo, have you actually observed any instances which would tend to support that belief?—There is a big, inhabited area in the Western Bahr-el-Ghazal and there is a tremendous lot of buffalo there, and that is where the *morsitans* is mostly found, or found in the largest numbers. I have been near a herd of buffalo, and I have seen more *morsitans* than I have seen when ordinarily going along the road. In the Lado Enclave, again, the *morsitans* is found in the same area as the buffalo.

4583. Could you describe the characteristics of that district from the point of view of vegetation?—No, I could not; I could easily get it for you, but I could not give you the names of the trees.

4584. Is it grass or bush?—It varies, grass and bush and streams; to the ordinary observer there is nothing peculiar in it compared with any other part of that country.

4585. At any rate it is not a specially grassy area such as you have just described?—No.

4586. Have you had any opportunity of observing whether tsetse-fly settle by preference on a black man rather than on a white man?—Yes, I think they do, and I have seen it with a white mule and a dark-coloured mule; the white mule gets bitten far less than the dark-coloured mule.

4587. That is what I thought. Then it is possible that the mere colour of the buffalo may have something to do with attracting the fly?—I should think so.

4588. Have you made any experiments in trapping, apart from the use of sticky cloth?—No.

4589. I think you said that when those pupae were found on the Yei river an entomologist was with you; was that Mr. King?—Yes.

4590. Did he satisfy himself as to the species to which those pupae belonged?—Yes, he said they were *palpalis*.

4591. (Sir Stewart Stockman.) I think you said that the Belgians had driven the game out of one area, and that the *morsitans* had followed the game?—Yes.

4592. Do you know if there was any history as to whether that has made any difference in the incidence of sleeping sickness after the game had gone?—I think they had just got the same number of cases of sleeping sickness as they had before.

4593. Were there any domestic stock there?—Very little.

4594. Do you know if they live?—The cattle do not live there.

4595. I take it they die from trypanosomiasis?—I do not know. I take it they get bitten by other biting flies, *Chrysops* and *Tabanus*.

4596. You do not know that they die from trypanosomiasis?—No.

4597. (Dr. Chalmers Mitchell.) I think you said that you had pretty well cleared out the Lado district of sleeping sickness?—We have examined the whole district and gone round it, and eliminated the sick as far as we could.

4598. But you still found two or three cases?—Yes.

4599. What is your idea as to where those cases come from?—There is a main road there, and those Uganda porters who originally brought the disease into that part of the world always camp in the beds of the streams, and the flies that get infected from the native travellers coming along the road still keep up the infection.

4600. You think it is brought in by human beings?—Yes.

4601. Not by game?—No, I think it is human beings who brought it there.

4602. You mentioned *brucei*; what do you think *brucei* does? You mentioned *T. brucei*; what do you mean by that?—I take it that is the trypanosome which infects donkeys and mules and kills them.

4603. Killing anything else?—That is all I know of.

4604. You think it is not dangerous to human beings?—I do not think it can be, as far as my experience goes in the Bahr-el-Ghazal at any rate.

4605. Why do you think it is not dangerous?—Because if it was infective there would be far more sleeping sickness.

4606. There is no doubt but that the fly is infected trypanosomes?—It must be.

4607. You are quite certain of that?—I have only read about it.

4608. You know it is killing the animals?—Yes, I know that.

4609. And that it is biting human beings?—Yes.

4610. And it does not kill the human beings?—That is so.

4611. One other question. Have you had a chance of watching the flies yourself much?—I have watched them when I have been trying to trap *palpalis* and when I have been examining a stream for *palpalis*.

4612. What do they do most of the time?—You might sit for 20 minutes in some places and then suddenly one flits down, and if you are with a black man it prefers the black man. It always goes for the black man first.

4613. Flits down from where?—Comes out of the leaves.

4614. You think it has been most of the time sitting on the leaves?—Yes, waiting for food.

4615. Do you think they fly out to meet food?—No, I think they wait for it, because I have never met *palpalis* flying away from a river unless there has been somebody just come from the river or they have followed me myself.

4616. What do you think attracts them? Do they see you?—I cannot say; I could not answer that question.

4617. You have not happened to think of it?—No.

4618. You do not know whether it is smell or sight?—No.

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4619. We have been told several times that moving bodies attract them?—I have always sat down and waited for them to come.

4620. And they come to you?—They ultimately come to you.

4621. Just as much as if you are moving about?—Yes.

4622. That is *palpalis* you are talking about?—Yes.

4623. Now as to *morsitans*. Has it the same sort of habits?—I have only seen *morsitans* when I have been on the march, and when I sit down I get just as many coming round me as I do when I am marching.

4624. They buzz about you just the same?—Yes, if I am in an area where there are *morsitans*, I have never seen any difference whether I am marching or sitting down.

4625. You think they are flying about looking for you, so to speak, or do you think they are lurking like the other fly? I take it you think *palpalis* on the whole lurks quietly in the trees until it has been disturbed by something?—Yes.

4626. And then it comes out and settles?—Yes.

4627. Does *morsitans* do the same sort of thing?—I think it must do the same thing, because you suddenly come into an area where there are *morsitans*, and then you suddenly find yourself covered with them.

4628. You do not see them coming flying to you?—No.

4629. (Chairman.) Only one question. Was your work solely confined to sleeping sickness or were you

in charge of the general health of the district?—It was principally sleeping sickness. I also did S.M.O. for military duty but that was only nominal.

4630. Could you give us any of the current diseases in the area where you find sleeping sickness?—Amongst the natives I saw yaws and syphilis, and they got various types of fever, of course indefinite fevers. Those are the commonest kinds of disease there.

4631. I do not know whether you could say at all what you think is the most severe cause of mortality?—Sleeping sickness is in the Lado Enclave at the present time.

4632. It is more fatal than any other disease?—I think so.

4633. (Mr. Buxton.) What are the other, what you might call deadly diseases, which are answerable for a large number of deaths?—In the Lado Enclave I never met any disease which was severe—I mean a disease which caused a lot of deaths—like sleeping sickness.

4634. Do you have bad epidemics of smallpox?—No, there is no smallpox or plague or cholera—none of that at all. They get dysentery at times.

4635. Does that destroy many lives?—No, it does not kill very many—not like sleeping sickness. We have no statistics available about the numbers of people infected by those diseases because we have just taken the place over, and they are only beginning to come into the hospital.

4636. And does the same thing apply to the Bahr-el-Ghazal?—Yes, much the same, only I never saw yaws there. I have seen more dysentery there than in the Lado Enclave.

The witness withdrew.

Major F. B. PEARCE, C.M.G., called and examined.

4637. (Chairman.) What is your present appointment?—My present appointment is Deputy Governor of Nyasaland, but I have been transferred recently; my actual appointment is Resident at Zanzibar, but that is a new one.

4638. In connection with sleeping sickness, your work was done as Deputy Governor in Nyasaland?—That is so.

4639. As Deputy Governor did you travel much through Nyasaland?—I always made one annual inspection. Of course, I have been 18 years there, so that I know the country pretty well.

4640. Before that were you District Commissioner, or what?—No.

4641. You have been the whole of that time Deputy Governor?—I was originally appointed as Assistant Deputy Commissioner in 1897 under the Foreign Office, and then the titles were altered to Governor and Deputy Governor, and I was promoted to Deputy Governor when the alteration in the constitution of the Protectorate took place.

4642. What number of District Commissioners have you under you?—Speaking from memory, there are about 35 District Commissioners altogether. They are called Residents there.

4643. Civil District Officers?—Yes.

4644. You are in communication and touch with them, of course?—Intimately, yes.

4645. What can you tell us about trypanosomiasis in Nyasaland?—Of course, I cannot speak scientifically at all.

4646. As an administrator?—I simply speak as a very ignorant layman. Reports of all kinds have passed through my hands, and I have tried to keep up with the latest developments. That is my only knowledge as regards the matter. The first case I remember was discovered in 1908, when a native was placed under observation and treatment, and, curiously enough, he is perfectly well to-day, at least he was when I left.

4647. Was it reported to you as an undoubted case, or was it a doubtful case?—I believe there is no doubt at all, because the trypanosome was found.

4648. Do you happen to know whether the trypanosome was of the Nyasaland type or the Uganda type?—That I cannot speak definitely upon.

4649. Do you think that the disease had existed before then in Nyasaland, and had not been identified?—Of course that is only a matter of my opinion; I really have no intimate knowledge at all.

4650. Have the natives any description of the disease?—I have never been able to get any satisfactory replies from a native on that point. They are not good, of course, at describing matters as regards sickness and so on.

4651. Do you speak the language?—Yes, there are three or four languages there, but I speak the main language, Chinyanja.

4652. They have not themselves identified it and got a name for it?—Certainly not, as far as I could make out.

4653. What do you say with regard to the present conditions in the infected area?—I left in October last. We had one seriously infected area on the coast of the Lake by Domira Bay. That has always been regarded as an infected area. There have been isolated cases found elsewhere; for instance, two in Blantyre hospital, but probably they had come down from the infected area. The first case was not found in the infected area; it was found more to the north, at Nkata Bay. At that time we thought it was an imported disease from the Congo, and we took precautions accordingly to prevent immigration into the Protectorate.

4654. Your medical officers have now come to the conclusion that it is a special type of disease peculiar to Nyasaland and Rhodesia?—So I understand.

4655. And that it is carried, probably, by the *morsitans* fly, not the *palpalis*?—We have no *palpalis* there as far as our investigations have shown, and the country has been pretty well looked over in that respect.

4656. In the many years you have been in Nyasaland has the *morsitans* increased? Has it spread in fresh directions?—I have never really been able to make up my mind, one way or another, with regard to that point, because people talk rather loosely sometimes about the spread of tsetse. There are so many factors, I think, which might lead one to suppose that it has spread, and again perhaps that it has not spread.

4657. Will you indicate some of those?—One thing is, of course, that the country has been opened

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up, and people travel a great deal more than they did before. They feed their cattle now where, 10 years ago, there was nothing but wild animals and bush. The consequence is that they penetrate into remoter valleys and wilder country in search of fodder, and they meet with tsetse-fly, which may, as far as I know, have always been there. I know there was a case at Fort Johnston which rather bears upon that point. I always thought it was rather a typical case of what might happen; I think fly has spread in certain parts. Fort Johnston is a great cattle place for Nyasaland; there is a large Government herd there, and they thrive exceedingly well there. About two years ago the cattle began to die, and they were examined, and it was found they were dying from tsetse. The cry immediately arose, of course, that the fly was spreading, whereas investigations proved that native settlement had increased round Fort Johnston, and the bush and jungle had been pushed further back, and to feed the cattle the native herd had to go further afield into areas where there was fly. When the native cowherds were forbidden to go to those areas the deaths amongst the Fort Johnston cattle ceased.

4658. That is interesting. What area round Fort Johnston was cleared? What is the protected area?—I do not think it is cleared. The formation of the vegetation is not forest immediately round Fort Johnston; it is what one would call grass plains with scattered forests more or less, and it is not until you come to the forest that you meet the tsetse. It is more an open area.

4659. In this open area you do not find the tsetse?—That is so; exactly the same as Chiromo.

4660. It is a natural clearing?—Yes, but at the same time natives have settled in the forest adjacent and cut down and cleared a certain area, and the consequence is that the country has been more or less deforested, apart from the natural vegetation.

4661. Do you know of any instance in a fly area or touching a fly area where clearing has been made and protection has been attained?—No, I cannot say I do. Do you mean on purpose?

4662. Yes; for the purpose of getting rid of the fly?—We are clearing around the villages in the infected area now, but clearing in that part of Africa is such a gigantic task, because, unfortunately, the things grow up again.

4663. I suppose you cut down trees and then burn them in the dry season?—Yes.

4664. And then you have to reburn them next year?—They start springing up directly the rain starts. We get five months' rain there.

4665. When you have cut down the trees and burned them, will the fly come back into that area until you burn again?—I could not say that; I do not think we have ever made definite experiments with regard to that point.

4666. What do you say as to the increase or decrease of game where there is disease? In areas where sleeping sickness exists, do you connect the sleeping sickness with the game?—Of course, it is connected in a way, I suppose.

4667. It is suggested by some and denied by others that the wild game constitute reservoirs from which the tsetse get the trypanosome which is fatal to man. —There is no doubt that is the case, of course; it has been proved, as far as I can make out from reports.

4668. It is generally accepted in Nyasaland that game is a reservoir?—So I understand from the scientific reports, but of course there are parts of the roads, for instance, between Zomba and Fort Johnston, from which you may derive a total misunderstanding as to the position of the tsetse-fly. You can go down that road in July, as I did last year; I specially looked for tsetse, as a matter of fact, and camped two nights on the road, and I could find none at all. Sir David Bruce came out and went down in December and he was almost eaten alive. So that a man who went down in July would say that there was no tsetse-fly, and another man would go down in December and say that the place was swarming with tsetse-fly, and both would be right, but they might come to quite wrong conclusions.

4669. Have the movements of tsetse-fly been watched? Do you know whether they spread from centres or originate from other districts, or what?—I really cannot say for certain, but I know this, that it is supposed that owing to the more frequent intercourse between natives, the fly follows the natives and enters into areas where perhaps fly had not been before. Of course, 10 years ago, the natives were very much restricted; they never left their homes, but now there are roads, and natives ride bicycles, and other people ride motor-bicycles, and so on, and if there is fly it is much more likely to spread.

4670. There is peace in the land, and that makes a difference?—Yes. These natives like to walk about and travel.

4671. And now they can do so with safety?—With absolute safety.

4672. What practical steps have been taken by the Government with regard to checking the sleeping sickness?—The first step was the proclamation of the infected area and the prohibition either of natives entering it or leaving it. Then the restriction as regards shooting game was withdrawn from that area and is still in abeyance; anyone, European or non-European, can go and shoot as he likes there.

4673. In the proclaimed area?—In the proclaimed area. Then, there have been medical officers stationed in the sleeping sickness area for special duty, finding out cases and dealing with them.

4674. Treating them and segregating them to some extent?—We tried segregation at first, but it was a failure, simply because the natives, when they heard the doctor coming, hid their sick in the bush.

4675. What is done now?—It is much better now; they do not hide their cases. If a native likes to go to the segregation camp at Ngani he can; his family is allowed to go there with him, he is given a house and food, and made as comfortable as possible. If he does not want treatment he need not have it. If he does not want to go he is allowed to stay in his village.

4676. Is he allowed to stay in the village if there are fly about?—Yes. There are fly in all the villages where there is sickness.

4677. How far is it the opinion of your staff there that the cases of sleeping sickness are carried from man to man or from animals to man?—I am afraid that is rather a medical question I should not care to trench upon.

4678. There is no general opinion among your officers?—No.

4679. Has the result of the free shooting in those areas of game been to reduce it a good deal or not, or rather, I should say, free destruction, because they may trap as well as shoot, I suppose?—They may do anything they like.

4680. What is the result?—There are two free shooting areas, one where there is no fly but plenty of game, and there is this infected area where there is fly and game. In the first one I think only 48 animals were killed altogether.

4681. That is where there is no fly?—Yes. There are plenty of places in Nyasaland where there is game and no fly.

4682. I did not know that in those cases the people were allowed to shoot?—They were in this instance because this was proclaimed a free area for this purpose.

4683. To deal with sleeping sickness?—No, for the purpose of testing whether the natives by themselves could clear the area of game.

4684. And they have cleared out 48 animals?—Yes.

4685. Now let us come to the fly area: what is the result there?—That was only proclaimed last year and I have seen no returns of how many animals have been killed there.

4686. I suppose it is rather difficult to get returns; a native will often kill an animal and not make a return?—It is in the sleeping sickness area, because it is a pretty big area, and the native cannot count very well, and he forgets whether he shot an animal last month or did not. In the smaller area the permission was given on the understanding that he would give a

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report of the game he killed, and in consequence we got a return.

4687. Do you think there is much destruction of game in what I might call the infected area?—I should doubt whether game would ever be cleared by that means. Of course it largely depends on the tribes. Some tribes go in for hunting more than others, and they probably would clear a district of game if they were allowed to. On the other hand, some tribes are not hunters and they have not got guns, or, at least, not many of them.

4688. What do you say as to the possibility of a general destruction of game in fly areas, throughout the fly areas of Nyasaland and other parts of Africa?—In all fly areas?

4689. Yes.—I should think it is impossible.

4690. You think it is quite impossible?—I should think so; five months of the year it is raining, with the grass 10 feet high.

4691. The most that can be done, you think, is to clear areas immediately around villages?—Or move the people.

4692. And as regards roads, do you think it possible to keep the roads cleared in any way?—No, I should not think so; they would have to be cleared during the rainy season, just when the native will not work. He has to get out into his own fields, growing his own crop for next year, and he will not turn out during the rains.

4693. What is the time when you can get native labour?—In the dry season, when it is not particularly wanted, unfortunately.

4694. When he has nothing better to do?—And then he has to find money for his tax; but directly it rains, in October, when he has to start hoeing, he must get back to his village if he is to have any food next year.

4695. That cannot be interfered with, of course?—No, not under the present circumstances.

4696. There are no what I might call free labourers—outside labourers?—No, only prison labour, and that would go no way.

4697. Before we leave the question of game destruction, what do you say about arming natives as a question of administrative expediency?—Of course, I should be very sorry to see it in a way.

4698. And there are plenty of ways of killing game without giving natives arms?—Yes. They are bad shots, too; giving them arms would not necessarily mean that they would kill much.

4699. And, on the other hand, they might kill things which were not game?—Yes.

4700. Besides, an armed population is always a danger, is it not?—There is bound to be a danger, but perhaps not so much in Nyasaland as elsewhere.

4701. However, on administrative grounds that is a remedy that you could not recommend?—I should not recommend it; it might be done in areas, but I should be sorry to see that.

4702. I suppose what could be done would be that you would have a certain number of trained hunters and let them shoot?—Yes. But of course, everyone who shoots in an infected area runs the danger of getting sleeping sickness. I have always hesitated myself in sending down magistrates and other white people to these areas because it is a danger they run hourly. There was a white missionary came up to shoot from South Africa, and he got bitten the very first day he went out shooting in this area, and he was dead three months afterwards.

4703. The type of disease in Nyasaland is a very severe type, is it not?—Yes, very severe.

4704. And death usually results in one or two months?—In a very short time, I believe.

4705. An experiment has been proposed in the way of selecting an area and destroying the game or driving out the game therefrom and keeping the game out in order to see what effect that would have upon the movements of the fly or on the infectivity of the fly. Would you give us your opinion on that?—I suppose I can refer to my papers?

4706. Certainly.—I made a few notes.

4707. If necessary, when you come to correct your proof, you can add anything which may be essential.—I do not know whether these maps will interest you; they are the latest maps with regard to the infected area in Nyasaland, showing the distribution of fly and the cases which have been discovered during the last two years.

4708. Can you spare them?—Yes. They give rather a good idea of the state of affairs (*handing in the same*).

4709. Will you now give me your opinion about the experiment?—It is only my opinion. It appears to me that if an experiment was initiated it would be shown whether the existence of tsetse depended on the presence of game. It could be shown whether if the larger game is removed the tsetse disappears. It could be shown whether, if the larger game is removed, the tsetse finds that the smaller mammals and birds are sufficient for its perpetuation. It could be proved whether tsetse will persist without any animal life, and whether the introduction of domestic stock into the area will cause a recurrence of tsetse after the game has been removed. Also whether the reintroduction of game into an area would cause the recurrence of tsetse.

4710. How about man? In the experiment you contemplate would man be excluded from the area?—As a matter of fact probably man would be included, but he could always be removed if it was required.

4711. Can you suggest any area which would be suitable where at present the game are infected and the fly are infected?—Nyasaland is about the worst country in the world for such an experiment, owing to the fearful expense of carriage.

4712. That is just what I want to hear about.—To everything we get up there, all our indents, we have to add 50 per cent. on to the cost price.

4713. To get them up country?—To get them up to the Government headquarters in Zomba even.

4714. And then beyond?—Beyond you would have to add more. The carriage is on men's heads practically after Zomba.

4715. Have you thought of any area which would be fit for the experiment at a reasonable expense?—My own idea was just north of Domira Bay, utilising the shore of the lake as one of the sides of the area, so that you would save a quarter of your expense.

4716. Is that an area in which there is both game and fly?—Yes.

4717. Is that an accessible place?—It is. That is one of the reasons I thought it would be rather suitable, because Domira Bay would be rather a good landing-place where steamers always call.

4718. So that European observers could be there?—That is so.

4719. Is there any fly-free area where the observers could camp?—Oh, yes. On the lake shore. There is no fly on the lake shore. It is only when you get inland.

4720. You have no *palpalis* there?—No.

4721. Could you give us any idea as to the probable expense of such an experiment? It would depend on what size of area you wish to enclose?—Exactly.

4722. Do you suggest any particular size of area?—Personally I thought 100 square miles; but I am not a scientist.

4723. That would be 10 miles each way?—Yes.

4724. Could you give us any estimate of the expense of that?—My own idea was that if it was larger it would be difficult to overlook, and if it was smaller it would probably not give a fair indication of what the real results might be. For 30 miles of fence, that is to say, an 8-foot steel fence with barbed wire, the lower 4 feet being covered with wire netting to prevent the exit and ingress of smaller animals, the figures given to me by the Director of Public Works—they are not my own figures—

4725. Is that Mr. Binnie?—Yes. That would cost 15,000*l.* erected.

4726. That would not include what has been suggested to us as a further branch of the experiment, that half a mile round the area should be cleared?—If half a mile was cleared it would be a big job, but my own idea was that 100 yards should be sufficient.

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4727. Would that be a very expensive and troublesome job?—No, I should think it would be included in the 15,000*l*. You are bound to start cutting down trees the moment you start erection.

4728. We had a suggestion from Sir David Bruce of altogether a different kind of fence, namely, a rail fence; what do you say about that?—I should think it would be rather unsatisfactory in a place like Nyasaland.

4729. Do you think if you had a third-rate fence, and had a small shooting party continually harrying this area, that would clear out your game?—With a third-rate fence you would always have animals coming out and going in of their own accord.

4730. Even if the area was thoroughly shot week by week?—You would have bush fires endangering the fence constantly. This is my idea.

4731. In fact you do not think that with a cheap rail fence you would get a fair experiment?—I should not be content with the results myself, I must own, as there would be so many openings for mistakes to be made; bush fires would occur and elephants would come trampling down the place, and that would not be found out for a week afterwards. My idea was, that if the thing was to be done at all it should be done well.

4732. You do not, I imagine, suggest that Nyasaland should find the money?—No.

4733. It would be an experiment which would be for the benefit of all *morsitans* countries quite irrespective of Nyasaland?—It would. If it was really cleared and freed of tsetse, the country would score in having an area free of tsetse. It could then be leased for various purposes.

4734. As regards the scientific results, they would be shared by every other country where *morsitans* was found?—Yes.

4735. (Dr. Chapple.) Did I understand you to say that 15,000*l*. just covered the cost of the fence?—Erected—yes.

4736. How many miles of fence?—Thirty.

4737. That is three sides, leaving the fourth side with the water?—Yes.

4738. If that were wire would it be elephant-proof? You mentioned elephants just now coming against a rail fence, but would an elephant be obstructed by a wire fence?—If they determined to go through it, they would, but my idea was that in the 100 yards cleared area there would be every three or four miles native patrol huts, who would thereby, by keeping up fires and so on and having general supervision over the fence, prevent elephants approaching the fence. They could be linked up by telephone if necessary.

4739. You would require that patrol for the protection of the fence?—Yes, and for repairing and so on, and seeing that the fire did not sweep up to the fence.

4740. Supposing the game came into that cleared area, would the fly from the enclosed area not go over the fence for the sake of getting its diet and therefore vitiate the experiment?—I do not suppose one could keep the fly in the enclosed area even with wire netting, but it is 100 square miles and a great portion of the area would not be open to that possible source of mistake.

4741. The object of driving out the game in an experiment of that kind is to take the game away from the fly, is it not, so that the flies which you are going to examine have not the infected game to feed upon; is not that one object?—To take the game away from the fly—that is the eventual idea.

4742. If the fly could go through or over that fence, might it not reach the game in the vicinity of the fence outside?—Yes, on the edges that would certainly be a risk, I think.

4743. Then what is the use of your experiment?—One is not confined exactly to the border of the fence.

4744. If you assume that the fly for 100 square miles is free to roam over that area and you have game coming up to the fence on the outside —?—I do not think they would come across the cleared area.

4745. Would not the game come along that cleared area grazing?—They might, of course, but with the

patrol men constantly on the alert I should think they would not.

4746. If they did, would it not vitiate the experiment?—Besides, game largely feed during the dark, very early dawn, when the fly is not supposed to be particularly vicious.

4747. If you have one source of error in a scientific experiment of that kind, may your experiment not be entirely useless?—I suppose it may be, but I do not know about "useless."

4748. Supposing you drove off your game like that and kept it outside the fence and yet your fly remained infective inside the area, what guarantee would you have that the fly had not gone a mile or two or five miles for its food beyond the fence?—One reason would be (I do not profess to know much about the tsetse-fly) that the fly keep very much to their own areas.

4749. That is, if the food is there?—I should not like to say. I think the tsetse is very mysterious. I do not profess to speak definitely as to whether they are dependent upon blood food or not, but I know areas where there are always tsetse exactly in the same spot.

4750. Presumably their breeding places are there and their food is there?—Yes, whatever their food may be.

4751. If you take the food away is it not likely that they will go some distance for it? We had evidence the other day, I think, that flies would travel five miles.—That is, following a man on a bicycle, probably.

4752. That is one thing, but we had evidence also that they went from an island to the mainland, a distance of five miles.—That is not in Nyasaland; probably that is *palpalis*.

4753. Have you any evidence to indicate how far flies would go, either for their food or for other reasons?—No, none at all, except that I know they do follow cyclists.

4754. Would it not be essential to determine how far flies would go before you would engage in an experiment which would cost you, for fences alone, 15,000*l*.?—Yes, I think it is highly desirable.

4755. Do you not think that until we have that information it would be unwise for us to advise that 15,000*l*. should be spent on a fence?—I should like to see something done about the matter; that is all. I am not a champion, particularly, of this experiment, but I have seen the spread of sleeping sickness in Nyasaland, and very little has been done about the matter. The experiments which have taken place, so far as I can make out, have not led to very practical results; they may be very satisfactory from a scientific point of view, but from an administrative point of view they are not. Having the experiment of an enclosed area seems to me, at any rate, a step in the right direction to determine various points. It may be (as I say, I do not speak as a scientist) that there may be very strong objections on points such as you have raised, but at least it seems a step in the right direction to determine something with regard to tsetse and sleeping sickness.

4756. We are anxious to know, if we are to advise upon an experiment, that some practical result or some practical information for our future guidance would come of it. Would it not, in your opinion, be essential that if you had one fence to fence in an area of 100 square miles such as you suggest, so as to get a sufficiently wide area beyond the travelling capacity of the fly or the travelling habits of the fly, you should then have another fence beyond that distance keeping the game from coming in, and making a Rotten Row of this cleared area?—Of course, that would preclude any possibility of mistake, certainly.

4757. Is it not essential, if you have a scientific experiment, that you should preclude any possibility of mistake?—I think so.

4758. You talk about the spread of sleeping sickness; do you think it is spreading rapidly over any large area?—Certainly not rapidly.

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4759. It is spreading, though?—It is rather difficult to say whether it is spreading or whether we are finding cases where they already existed.

4760. Still, it is being discovered in new areas?—Yes.

4761. Sleeping sickness among the natives you mean?—Yes, in certain areas.

4762. Is trypanosomiasis among the domestic animals also spreading; that is to say, is it found in new areas now where it was not found before?—I do not know whether any particular definite investigation has been made with regard to domestic animals in Nyasaland from that point of view. All our investigations have been confined to human beings, and, of course, where the fly is there are not many domestic animals, because they are only in areas where the fly does not exist.

4763. Is that because it is impossible to take them into areas where the fly exists?—Yes.

4764. Do you know of any prosecutions of natives for violation of the game laws in Nyasaland?—I cannot recall any special ones. Europeans and natives occasionally are prosecuted.

4765. Do you think that the administration of the game laws in any way restricts the natives or the whites from the killing of game in the immediate vicinity of populous centres where the villages are?—Not connected with the infected areas.

4766. Any of the areas?—Of course, people are not allowed to shoot without a licence other than in infected areas.

4767. Does that restriction lead to an increase in the amount of game and an invasion of these areas by the game?—I do not think so, because as regards Europeans, if the restrictions were withdrawn it would not make much difference, I think, simply because they do not want to go and shoot females and so on. As regards natives, of course they might hunt a little more if the restrictions as regards game were withdrawn.

4768. Do you think the proximity of the game to these places increases the danger both to man and animals of trypanosomiasis?—I suppose if the game is a reservoir it is bound to more or less, but that only applies, of course, to the infected areas.

4769. In those infected areas do you think the driving off of the game from these villages and from settlements would decrease the danger? Naturally that will follow from your last reply.—Yes, I suppose it would. The fly itself seems to have in a great measure approached the villages in a way which a few years ago it never did. For instance, Dr. Hearsey, the Principal Medical Officer, when he came back from a trip through the infected area, just before I left, said that as he was looking at a sleeping-sickness patient in a village he was actually bitten by tsetse. There cannot be any game within, probably, a mile of a village or so because the grain fields are round the villages.

4770. What do you infer from that?—I do not know whether I make any particular inference from that, except that perhaps they have changed their habits up to a certain point and found it easier to prey on domestic stock and human beings than on game, in the same way as is often the case with lions, for instance. There are areas in Nyasaland where lions do not care for game, and the villages are stockaded, and these lions have apparently a passion for human food, and they simply raid these villages.

4771. Do the lions bring the flies in, do you think?—Oh no, because they come at night.

4772. Have you any practical suggestion to make along the lines of preventive medicine? I am sorry to say I did not hear your main evidence, but apart from further research and experimentation, do you think we have sufficient data to take any active steps along the lines of prevention?—No, I do not think we have sufficient data.

4773. What more do we want to know?—We want to know (at least I think so) a great deal more about the habits of the fly, first of all. It is not even decided whether they demand blood food.

4774. Surely that is sufficiently established, is it not?—I am not a scientist, and I do not know whether

that is the case any more than in the case of the mosquito; I believe it is said to be essential that they should have blood food, but I do not suppose it is possible for mosquitos all to satisfy their craving in that direction.

4775. Have you had any experience of the effect of clearing on the driving off of the fly?—Only round villages in the sleeping-sickness area.

4776. Has that produced any result?—It is rather too early to say. Unfortunately, even if you clear round a village you do not prevent the natives being bitten, because a great many villages have no fly in them, but the natives will go into the forest to get wood, and they travel from one village to another through forest belts.

4777. Do you think from your knowledge of the country there it would be possible to do an amount of clearing round villages and to produce an economic result, that is, that the land would be increased in value to the extent of the cost which you laid out in clearing it?—No, I do not; it would be impossible, I think.

4778. (Mr. Rothschild.) Dr. Chalmers Mitchell, who had to leave, asked me to put the following questions for him. When were the restrictions on shooting game first imposed in Nyasaland?—The original game regulations?

4779. Yes.—I cannot really tell you, but there have been game regulations ever since I have been in the country, and that is since 1897.

4780. Have you heard that when they were first imposed they were popular or unpopular?—I do not think people really minded, but I suppose all restrictions are unpopular, more or less.

4781. Was there a special outcry for their repeal before Sir David Bruce and Dr. Yorke suggested a new reason for repealing them?—I never heard of any such appeal.

4782. I should like to put this point to you. I think it is generally established both among scientific men and the public in the fly areas that the wild game is a reservoir as regards trypanosomiasis in domestic animals and stock, but you said just now in answer to the Chairman that you thought it had been scientifically proved also that the game was a reservoir for the sleeping sickness in human beings. We know that Dr. Yorke and Sir David Bruce have insisted upon that point, but there have been a great many doubts thrown upon it, and a large number of witnesses before this Committee have said there were doubts about it. Did you draw your belief that it was proved from the evidence of anybody else but Sir David Bruce and Dr. Yorke?—No, I had specially in my mind Sir David Bruce's observations on this point.

4783. Do you know the opinion of the German scientific staff on this point?—No.

4784. As regards the experiment itself which is proposed, do you not think it is absolutely essential before committing oneself to an outlay of a sum like 15,000*l.* for a fence to know exactly the distance that tsetse-fly will travel? I do not mean in the pursuit of a man on a bicycle or of a casual traveller but on ordinary occasions, so that you could make such a clearing round the fence as to prohibit outside flies from coming in, because, after all, that is the principal point?—Yes, but I am not quite clear whether it would matter whether flies came in or not.

4785. I think it would matter very much, because if fly came in from the outside they might have been fed on infected game, and you could not prove anything with regard to the infectivity of the fly if you knew that fly came in from the outside continually.—No, but if the game disappeared you would be able to say whether the fly were there after the game disappeared.

4786. If fly was continually passing in and out, and it remained after the game, you could not find out whether the infectivity of the fly was greater or less after the game disappeared.—Certainly, the infectivity of the fly might be obscured, but as regards whether the fly disappeared when the game had been destroyed, I do not see that it matters whether fly go in or whether fly come out.

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[Continued.]

4787. If you only limit the result desired from your experiment to that fact I quite agree, but I believe that you also want to find out, supposing the fly does not follow the game, what the result of getting rid of the game is, because if it was possible to exterminate all the game it would be no use, as regards sleeping sickness, if the infectivity of the fly remained just the same.

4788. (*Dr. Martin.*) Do you happen to know what the cost of clearing per acre would be in that area near Domira Bay which you have mentioned?—I cannot give you actual figures; I do not think it ought to come to more than about 11. per acre.

4789. Simply chopping down the trees and waiting until the dry season and burning would cost as much as 11. an acre?—I think so.

4790. Could you give me approximately the cost per ton to carry stuff from Zomba up to Domira Bay?—I am afraid I could not give you details as regards the cost of carriage; it depends largely on the season. If you bring it up from Zomba during the season when the river is available it could be carried up in barges at no expense practically at all. If, however, the river is not deep enough it would have to be carried overland by natives and the cost would be quadrupled.

4791. You think it would be at least four times the cost, but it depends on what the value of the stuff is?—Yes.

4792. Do you happen to know why game laws were introduced into Nyasaland?—I can only guess, of course. I presume it was to prevent the extermination of game generally, and to regularise the possession of firearms by natives.

4793. Should I be right in supposing it was principally to increase the amenities of life there for the white man?—I should not think so. I do not say it has had the opposite effect, but where there was game 10 years ago round Zomba there is no game now, so that it has not had the effect of increasing the game round these centres.

4794. It may have delayed the dissipation of game, may it not? Do you know any other reason beyond what you have mentioned?—No, no special reason.

4795. Supposing a settler takes up 1,000 acres in some outlying parts of Nyasaland, what restrictions are legally placed upon his shooting in the neighbourhood, if he wants to, in self-defence?—On his own land?

4796. First of all, he may shoot on his own land?—Yes, but he has to take out a gun licence, which costs 10s.

4797. That permits him to shoot on his own land?—Yes.

4798. What about the immediate neighbourhood?—He would have to take out an ordinary game licence.

4799. Is that a very costly matter?—21. is the cost of the game licence.

4800. Does that permit a man to shoot as much as he likes?—Not elephant, of course.

4801. Barring elephant?—And rhinoceros and the various rarer species. He would have to pay 10l. if he wanted to shoot an elephant.

4802. (*Mr. Austen.*) Do you regard sleeping sickness in Nyasaland as an old or a new disease?—Really I should not like to say one way or the other; it is more or less a technical question which I have not formed much opinion about. My own impression is that it has been imported.

4803. From your extensive knowledge of the country do you consider it possible that the disease may have existed in Nyasaland before the first recorded case—that is, the one in 1908?—It is quite possible.

4804. My next question is one I have been asked to put to you by a member of the Committee who has had to leave (*Mr. Buxton*). Have you proved to your satisfaction that sleeping sickness in Nyasaland has actually increased since 1908?—Of course we know now of a great number of cases; it has increased in that respect.

4805. Is that an actual increase, or is it merely due to the more efficient search for cases?—I should say to the more efficient search.

4806. So that there is no real evidence that the disease now in 1913 is any more extensive than it was five years ago in 1908?—Absolutely no evidence—no proof whatever.

4807. Can you tell the Committee the number of cases recorded last year, 1912?—It is on the map I have handed in; there were 46 according to this map in the infected area.

4808. (*Chairman.*) And how many deaths out of that?—I am afraid I have not got that, but they are practically all dead.

4809. (*Mr. Austen.*) How many were there in 1911?—Thirty-five.

4810. So that although there is an appreciable difference between these two numbers, that need not necessarily indicate that there were actually more cases of the disease in 1912 than in 1911, but simply that more were discovered?—Yes; the natives do not hide their sick now.

4811. Is it true that owing to the game laws in Nyasaland game has increased to such an extent as seriously to ravage the crops of natives and European settlers?—I should not say seriously; of course all the fields there are unfenced, and antelope do come in sometimes and nibble the leaves of the corn and so on. But the chief damage to gardens is done by pig and porcupine at night.

4812. Is that by bush-pig?—Bush-pig.

4813. Those animals are very difficult to clear out of a district?—Almost impossible. Of course the natives have only to go to a magistrate in order to get a gun and ammunition if their fields are being harried at all by antelope and baboons.

4814. They are not prevented from doing that?—They are encouraged; there is a very large number of rifles loaned to these natives whenever they want them, as a matter of fact, and for which they pay nothing.

4815. And a European settler could shoot whenever he wanted to on his own land?—Absolutely; that is always understood. If he shot an elephant he would not get the tusks, they become Government property; in fact there was a case just before I left.

4816. The inhabitants of Nyasaland at the present time, whether European or native, have really no grievance on the score of the game laws so far as their crops are concerned because they can remedy the matter themselves?—No, I should think they have none, considering that they must expect to meet with wild animals in a country like Nyasaland.

4817. Have the natives who have not guns facilities for dealing on their own account with animals which may make incursions into their crops? They make traps, game pits, and so on, and they hunt tribally and with dogs and nets.

4818. Do those traps and pitfalls afford an efficient means of protecting the crops?—They do, because they fence their fields and make a pit in the pathway, and then when the animal comes at night to eat the leaves it must keep to the road and it falls in. It is very dangerous, however, and it had to be stopped on the main routes.

4819. Dangerous to human beings you mean?—Yes, because they fall in themselves.

4820. Are you familiar with the details of Sir David Bruce's proposed experiment on the Domira Bay Road?—He only mentioned it to me casually in conversation that it was his idea (at least I understand so) to fence both sides of the road, and to clear the road.

4821. For a length of 20 miles, I think?—He did not specify to me any particular length.

4822. From the lake to the foot-hills?—Yes, through the fly belt.

4823. Have you formed an opinion as to what the cost of that would be on Sir David Bruce's suggested lines? He proposed, if I remember aright, to construct a fence of native timber, which would sprout when he put the stakes in; could you give the Committee any idea of what the cost would be likely to be from your experience?—Without any wire at all?

4824. Without any wire at all.—It would not cost very much.

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[Continued.]

4825. I think, if I remember rightly, the ground so inclosed was to be cleared, and a belt on each side of the fence outside the fence was to be cleared also. —How broad would it be?

4826. Five miles on each side of the road.—That would be 50 or 60 miles of fence. I should think that such a fence of 50 miles (I do not profess to speak accurately) would cost from 2,000*l.* to 3,000*l.* and not more.

4827. If the inclosed area had to be cleared and a belt on each side had to be cleared too, can you say how much, in addition, that would cost?—I am afraid I cannot give you that detail except very roughly; it would cost a good deal, of course. The clearing is an annually recurring thing; you are never free from it, you are always cutting down.

4828. I think you told a member of the Committee previously that the cost of clearing was about 1*l.* an acre?—Yes.

4829. Would that apply to this particular locality?—Yes; probably a little less; but wages have gone up the last year.

4830. Roughly, may we base calculations upon that—clearing at 1*l.* an acre?—Yes.

4831. You are familiar with that stretch of road, I suppose?—No, I have never traversed it, but I know both ends of it very well. I was at Kasu with Sir David Bruce the year before last, and I visited the Domira end last year, and went up it some distance.

4832. My memory may be wrong, but I have some idea that Sir David Bruce said it would not be necessary to fence the further end, the inland end. —No.

4833. Is that owing to the configuration of the country?—The fly ceases, I presume.

4834. Could you keep game out if you did not fence it?—No, you could not do that; you would have to fence it as far as game is concerned, I think. There is no natural configuration which would prevent game entering.

4835. Otherwise your experiment might be vitiated in that way?—Yes.

4836. Although you have never actually traversed that road, or had practical experience of it, can you tell us anything as to the amount of game which is present on that stretch of road and in its vicinity?—I know very well at the start of it there is plenty of game because I was there myself.

4837. At the lake end?—Yes. That is the non-forested portion. Then you get to the forested portion with the fly. Then you get to the inhabited portion, and then the inhabited portion becomes denser until you get right to the western end.

4838. I asked that question because the Committee was told by a previous witness that, in his opinion, the Domira Road would not be at all a good locality for this suggested experiment because of the fact that there was really very little game there.—As I say, I have not traversed the road myself, so I would really only be speaking at second hand if I gave any opinion about it, except that I know at the lake end for the first four miles there is plenty of game, because I have seen it myself.

4839. And there is no fly there?—That is so.

4840. That is all you can say of your own knowledge?—Yes.

4841. Have you personally studied the habits of *G. morsitans*?—Oh, no.

4842. But you have, I take it, observed the fly in nature?—Yes, I have observed it.

4843. Have you ever found *G. morsitans* in grass country where there was no bush at all?—No, I must say I have not.

4844. Can you describe briefly the sort of country in which you do find *G. morsitans*?—I have always noticed them more in light-forested country near where it is shady and where there is a good deal of undergrowth.

4845. Something in the form of wooded vegetation is always necessary, I take it?—Yes.

4846. Have you ever observed areas in Nyasaland to which *G. morsitans* retires in the dry season, so that these areas form centres from which the fly spreads

again during the ensuing wet season?—No, I have never made any such personal observations, but I have seen it in reports which one of our medical officers has sent in.—Dr. Shireore.

4847. You cannot confirm that from your personal observation?—I cannot.

4848. Could you suggest any methods which would be likely to offer a reasonable hope of eradicating *G. morsitans* in Nyasaland?—I do not know about a reasonable hope. There has been an experiment made this year. It was suggested that if the bush was burned systematically and wholesale at a given time rather than piecemeal, the fly would be eradicated from that area. The whole country is burned when it gets dry; the fires start about August and go on regularly until the rains in November. At present any native or any man who wants to shoot simply starts these fires on his own initiative, and it was suggested that if a systematic burning of the bush was undertaken the fly might be destroyed.

4849. Would that be practicable throughout the whole protectorate?—It would be very difficult to do, but it was done in a small experiment this year just before I left. There was one District Resident there who was very anxious to try it and he was allowed to try it. I have heard since, though, that the result of his experiment (whether it was the result or not may be open to question) had been that the whole country had been infested with mice and rats.

4850. I do not quite see the connection; was that as a direct result of the burning?—No. Instead of the burning going on over a long period these rats and mice were not killed off by fire as they would be in normal years, and they got to such an enormous number owing to not being burned out that they have run all over the country.

4851. Why could they not have been burned out when the fire took place?—I suppose they took refuge in the villages; they distributed themselves throughout the country, because one does not burn round the villages.

4852. Could not the rats and mice take refuge in the villages at any time?—Yes, but they never got to such numbers, because they are burned out normally before they can propagate themselves to such an extent.

4853. I do not quite follow why they were not burned out in this case.—I know nothing about the numbers of mice myself; I simply tell you what I was informed, that the result of this experiment of postponing the bush fires has been that the country has been overrun with rats and mice.

4854. Do you know of anything to show that *G. brevipalpis*, a species of tsetse-fly which one used to call *G. fusca* in the Elephant Marsh, conveys trypanosomiasis to man?—I have no knowledge whatever.

4855. You do not know of anything which might indicate that?—Nothing. Of course they are very scarce.

4856. I think they are local, but they are fairly numerous in North Nyasa?—They are more there than elsewhere, but they are rather exceptional in the country as far as I know.

4857. You know of nothing that indicates that there may be a carrier other than *G. morsitans*?—Personally, I know nothing about that.

4858. (Dr. Bayshance.) I should like to get from you some information as to the density of the population in the proclaimed area of Nyasaland. I think you said it was 40 miles by 50?—That was only the result of a very brief look at this map which I had made, and I should not like it to be taken as accurate in any way.

4859. About how many square miles do you think it is approximately?—About 2,000.

4860. Something like 2,000 square miles, you think?—Roughly.

4861. What is the population?—75,000; that is of the whole district, of course.

4862. That is the whole of the proclaimed area?—No.

4863. It was the proclaimed area I wanted to know about, its area and its population.—I am afraid

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[Continued.]

I have not got the figures in my head as regards that, except that the number of huts shown in that area on my map is about 1,800, which would mean a population of about 5,000.

4864. I read a report about two or three years ago in which the population of the proclaimed area was put down at about 15,500.—That is so, but since then the proclaimed area has been reduced in size. The original proclaimed area, when we were very nervous about the whole matter, was a very large bit of country, but now the area has been reduced.

4865. What I wanted to get at was this: is it a sparse population or a thick population for Africa?—I should think sparse.

4866. What is the size of the villages—how many huts would form a village?—About 10 or 14 huts; they are scattered, of course.

4867. What would be the average distance between villages?—They would occur in clusters; probably the villages would straggle along the road in small hamlets. They might be a few hundred yards apart and they might be a mile.

4868. My object in asking is this: as far as Congo sleeping sickness is concerned it has been noticed that where the population is very dense, as it was on the north shore of the Victoria Nyanza, that form of sleeping sickness has become epidemic and has killed a great many people; on the other hand, where the population has been thin the disease has spread very much more slowly and has never got much headway.—Yes.

4869. It is conceivable that the reason why human trypanosomiasis has not spread in Nyasaland is because the population is so scarce and scanty?—I should think that is very probable; it is very scattered. There are no dense populations there.

4870. The only other point I wanted to put to you was this: in your summary (No. 12) you say, "Removal of native populations to non-infected areas": is that feasible in Nyasaland?—I had in my mind the area on that sketch map I handed in. There was only a matter of 1,800 huts with a population of 5,000, and there would be no difficulty in moving them, as far as I know.

4871. They could be moved; but could they be moved to areas where they would be contented, where they could cultivate the land?—Yes. They would have to be paid for it, of course.

4872. Would it be possible to induce them to go? Would they go willingly?—Yes. I think if it was properly managed; if they were given plenty of notice, and allowed to harvest their crops, and were paid a small compensation of 10s. or 15s. a hut, I think they would go.

4873. That seems to me rather an important thing, if the people in Nyasaland who are living in fly areas could be moved into non-fly areas for the time being?—That could not be done.

4874. You mean only areas which are not considered to be infected?—It could not be done in non-infected areas because that would mean a wholesale removal of population, but it could be done in this way: we have only one infected area now in Nyasaland marked on that map, and if it came to a point they could be moved without much difficulty, but it would be impossible to move populations simply because they lived in fly areas.

4875. They would be simply moved into other fly areas which it is believed are not infected; was that your meaning?—Yes, those 1,800 huts could be moved.

4876. But they would be moved into a different part of the fly area?—No. We would move them away to where there is no fly.

4877. But it could not be done on a bigger scale?—No, because the fly belts extend right from the north of the country down to the south.

4878. In that case it would only deal with the problem so far as these particular natives are concerned?—Yes, where there is sleeping sickness now; all our cases come from that small area marked on the map.

4879. Cases recently have been reported from other parts of Nyasaland, from the Marimba district and South Nyasa.—I know.

4880. And it has been suggested that cases have not been reported there previously because they have not been looked for.—That is very possible; of course they have been looked for, but the natives have been nervous about showing their sick. Now they are gaining more confidence, and probably do not hide them, and cases are being found.

4881. So possibly infected cases will be found in Nyasaland wherever there is fly?—It is possible, of course; I do not know; personally I think not myself, because I cannot conceive that they have not been found up to now. It is not from want of looking. Most of the fly belts have been most carefully examined and special medical officers have been detailed, and have spent months doing nothing else.

4882. You do not think the failure to find these cases may be due to the fact that in the early days a great deal of stress was laid on enlargement of the lymphatic glands in the neck, and in this form of sleeping sickness it appears you very often do not get that?—Possibly.

4883. It appears to me that might account for those cases having been missed in the early days.—I think that is quite possible, but I cannot speak from any medical knowledge in the matter.

4884. (Chairman.) There are only two questions I have to ask you. You told us about this burning experiment carried out by the district officer; what was his name?—Mr. Ritchie.

4885. You told us the fact with regard to mice and rats, but not the result as regards the fly?—It has only just been finished.

4886. And we have not got the results?—No; as a matter of fact, it had not started when I left Nyasaland.

4887. And we have not yet got the results?—No.

4888. What is the total revenue of Nyasaland which is available? I do not mean for sleeping-sickness purposes, but what about is the annual revenue of Nyasaland?—It is about 100,000l. or 120,000l.

4889. With a total population of what?—1,000,000 natives, roughly, and about 700 Europeans.

4890. What sum does the Nyasaland Government itself spend on sleeping sickness, do you know? What does it cost the Government?—There is 5,000l. at present for Sir David Bruce's Commission.

4891. That is paid entirely by Nyasaland?—Yes, and of course, there is the expense of the extra medical officers and equipment, which goes against the ordinary medical vote. I cannot speak from memory now.

4892. And I suppose the expenditure in segregation camps where you tried them?—Exactly.

4893. Do you know at all whether the general mortality (I do not speak about sleeping sickness) in Nyasaland is high or low? Is anything known?—There have been no investigations in that way; we have no actual knowledge about that at all—about native deaths or mortality or birth rates.

4894. Do we know anything about the main diseases in Nyasaland—what are the main causes of mortality, or is that unknown as yet?—That is more or less known, I think.

4895. Can you tell us what the Government consider are the most fatal?—Speaking as a layman, I fancy that pneumonia is largely the cause of death and pulmonary diseases of various kinds. Smallpox used to be.

4896. Malaria?—I do not think there is much malaria. There is dysentery, of course, in the rainy season.

4897. At any rate the deaths from sleeping sickness are only a very small proportion of the mortality at present?—Very small indeed.

4898. (Dr. Chapple.) Did I understand you to suggest that the right to kill game on one's own land was a sufficient protection from depredations by these animals?—For Europeans?

4899. A sufficient protection to those who owned the crops. Would it not be necessary to have the right to kill beyond your own land in order to prevent animals coming in, or would you have to wait until the animals came in before you would have the right

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[Continued.]

to kill them?—I think it would be quite fair to wait until the animals came in.

4900. Is that sufficient protection for crops under any circumstances?—If not, the man ought to fence his land, like they do in every other country.

4901. If you have no fences—assuming the absence of fences—would it not be necessary that you should have the right to kill animals approaching your land and not be forced to wait until they actually appear on your land before you can shoot them?—I should say not.

4902. You think it is a sufficient protection if they have the right to kill them when they come to the land?—I think that is a very fair provision for protection.

4903. You said that it cost £1. an acre to clear the land. I have asked you whether land would be of any economic value if you cleared it. What would be the value of land round these villages? Could you clear the land round the villages at the cost of £1. only per acre, and if so, would it not have some value if that was the only cost?—It depends on what you are to do; if you are to stump it and make it ready for leasing it might have some value, but the difficulty in Nyasaland is that there is so much land and so few people who want it. We want immigrants to come and take the land.

4904. If you had a little settlement at any particular spot and could clear half a mile round in all

directions would that be any protection, do you think, from game and fly, and if so, would it have any economic value if it were cleared as a business proposition?—None, I should think.

4905. Even although it only cost £1. an acre to clear?—Simply because there are no people who want the land.

4906. If it only cost £1. to clear and would keep away the fly and game from that area, would it not be a sufficient protection against disease, apart from its business value?—No, because the native gets bitten not in his village or fields, but when he goes into the forest to collect firewood, or travels from village to village.

4907. That is so far as the native is concerned, but what about his cattle and stock?—They do not have cattle in the fly areas.

4908. I understood you to say they did not have cattle because they could not; but if you had cleared an area like that would they not be able to keep cattle?—I should think not.

4909. That would not be a sufficient protection?—They would have to go a long way for feeding purposes, in dry weather especially; there is no grass or anything, and they have to resort towards the rivers where there is green fodder, and the consequence is that you could not confine cattle to an area round a village.

The witness withdrew.

SIXTEENTH DAY.

Friday, 12th December 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (*Chairman*).

Mr. E. E. AUSTEN.

Dr. A. G. BAGSHAW.

Dr. ANDREW BALFOUR, C.M.G.

Sir JOHN ROSE BRADFORD, K.C.M.G.

Mr. E. NORTH BUXTON.

Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Colonel Sir W. B. LEISHMAN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Sir STEWART STOCKMAN.

Mr. A. C. C. PARKINSON (*Secretary*).

Mr. J. O. SHIRCORE, M.B., B.Ch., M.R.C.P. (Edin.), recalled and further examined.

4910. (*Chairman*.) Dr. Shircore, some of us, including myself, were unfortunately not able to be here the last time you were good enough to come. We have all read your evidence with a great deal of interest, because you have thrown a great deal of light on a very important matter, which has helped us very much, but we asked you to be good enough to come back here, I think, more especially on the subject of those concentrated fly resorts to which you referred. You gave us some maps and stated that at certain seasons you found large quantities of fly concentrated at particular points, and that in the wet season they spread over a wider area. I understood you to say that at certain seasons you might reasonably expect to find them at particular places, and if that were so it might afford facilities for getting at and destroying the fly. I should like to ask you on that point, and I do not propose to touch on the other points which you have dealt with at all, whether you think that those, what I have called concentrated fly resorts, are constant, or that they may be the accident of a particular season. I do not know on how many occasions you had the opportunity of observing them, and seeing that they were always in the same place. —I think they are constant areas. I have noticed the

same thing in Uganda as well as in Nyasaland. I am referring only to *morsitans*.

4911. *Morsitans* in Uganda is really only found in a limited area at all, is it not?—We have more or less the same sort of areas in the Nile Province of Uganda as we have in Nyasaland.

4912. In these particular places which you indicated on the map which you were good enough to furnish you have seen them only in one year?—Yes.

4913. And you infer from experience elsewhere that having found them in that way it is probable that that resort is constant?—Yes.

4914. When you find them there (let us assume that) what measures do you suggest might be adopted for the purpose of destruction, and in giving your answer take one season with another?—Before the dry season actually commences, that forest connections between these primary centres and the rest of the forest in the area be destroyed, and when the dry season starts these primary centres might then be destroyed. Then towards the end of the dry season they should be burned.

4915. Then you would in the wet season have an opportunity of judging whether fly was found there, or

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[Continued.]

to anything like the same extent as it had been previously?—Yes.

4916. If you burned all these areas, resorts, or whatever you like to call them, you would then produce the result that they would be quite unsuitable for fly in the future?—Yes.

4917. Do you assume from that argument as a probability that these places are the breeding places?—Yes, I believe they are breeding places; I cannot conceive of the idea that fly should live at one place for three, three and a half, or four months during the year and not breed there.

4918. Because if they were not breeding places, of course the only effect would be that they would create new resorts for themselves; you would destroy the particular hatch of that year, and wherever they bred they would find some other place to go to, but the assumption is involved (and I quite follow it; I think it sounds reasonable) that the place where you find them is where they breed and where they remain until the wet season, when they spread?—And they also remain there during the wet season; that is to say, these are areas for fly during both the dry and wet seasons.

4919. Have you yourself formed any judgment as to the distance *morsitans* will travel from countries suitable to other countries suitable over an area that is not suitable?—I would put it at about three or four miles, that is, with continuity of forest.

4920. You put it as high as this, that if you wanted, for instance, to make a particular road safe to man and animals moving along it, you would have to clear an area of from three to four miles on each side of that road?—No, for three or four miles along continuity of forest, along suitable country.

4921. That is not quite the question I put; what I meant was, how far they would fly from bush country which suited *morsitans* to bush country which suited *morsitans* over an open area which was not suitable?—I do not think they would go at all of their own accord.

4922. But how far do you think they would go over unsuitable country in any circumstances?—Not very much over a mile; a few might be carried, but they would not migrate of their own accord. They might be carried and follow game or man.

4923. With regard to the question of the clearing, supposing you decided on spending a large sum of money in fly countries clearing round villages and roads where there was considerable traffic, do you think as regards clearing round villages it would be within practical politics as a matter of expense to clear sufficient to make countries which were not suitable for cattle suitable for cattle, and to make the transit to those countries safe over the roads? I mean is that altogether too large a thing, or is it a conceivably possible thing?—It would have to be very efficient; I mean there would have to be constant care exercised in these clearings. You would have to clear it season after season, and of course those are very difficult matters in Africa. You would clear it once, and you would find as soon as the rain started grass and scrub growing up, and then you would immediately have to attack it. Such efficiency, as far as I know, does not exist out there.

4924. We had, I think, some evidence (whether it was a particular class of forest or vegetation I do not know) that if you burned and destroyed it, it did not spring up again very quickly, but that it was killed by other growths which sprang up?—Vegetation springs up pretty quickly. Inside of two seasons you would have a fair amount of scrub and forest, not trees grown again, but sprouts from their trunks and so forth.

4925. Then it would be a constant expense, but would it be a very great constant expense?—No, not if properly attended to at once, at the very start.

4926. And that, if it should be carried out, as the result would, within those limitations, certainly be effective?—Certainly, I believe so.

4927. That, I think, is one of the things one may take as a proven fact?—I think so.

4928. So that if you cleared a large area around villages the only human population remaining in

danger would be those who went abroad for various purposes into the bush?—Yes.

4929. And those who remained in the villages would be safe; so to follow that up quite logically the only danger of infection of the villages would be that which was brought down by man?—Yes, but I do not see the danger of infection in that way if the villages are cleared and there are no fly there, and the infection is brought there.

4930. I mean by a man who goes into the bush?—Yes, he brings back the disease, but he cannot spread it.

4931. That would be a small thing?—Yes.

4932. There are many questions which I could put on your former proof, but I think that will be sufficient for the present purpose.

4933. (Sir John Bradford.) Only one question. As regards the observations on those localised fly areas, I am not quite certain whether your remarks are based on one season's observation or many seasons.—One season in Nyasaland, and, of course, my experience to a certain extent in the Nile Province of Uganda.

4934. Of course, in another year?—Yes.

4935. So that that rests on two years, we may say, but not two years in the same place?—Two or more, I should say. A man constantly travelling through fly areas gets impressions which he cannot always recall to mind as referring to any particular season.

4936. What I mean is, do you know of any single area which has been observed in two consecutive years?—Not that I know of.

4937. (Dr. Bagshawe.) There is just one question I want to put; it is on your proof if you have got your last evidence before you (*the witness's previous evidence was laid before him*). It is question No. 3760, and the question was, "Taking this area No. 1, what would the cost of that be?" And your reply was, "It is an extent of $3\frac{1}{2}$ miles by very nearly 2 miles. "It is about 5 square miles. I would say one man to "an acre a month."—I have deleted that; I corrected that. I think I rather under-estimated it at that particular moment.

4938. We are neither of us able to calculate very quickly in public. It works out to a much higher figure, does it not? Five square miles according to my reckoning is over 3,000 acres.—Yes, 3,200, I think.

4939. So that a couple of hundred men working for a month and a half or a month is an underestimate?—Yes. I should say about 500 or 600 men.

4940. (Mr. Buxton.) Have you had any personal experience in making clearances?—In so far as clearing round villages for a short distance is concerned, yes.

4941. Supposing you make a complete clearance of an area, how long is it before it again attains such density of shade, and so on, as would make it an acceptable refuge for the fly?—I should say two seasons.

4942. Could you give us any idea of the length of the shoots which occur? My own experience in this country is that if you cut down a healthy and vigorous bunch of gorse it makes 18 inches the first year. In your experience of these African growths could you give us any idea? I do not know what the plants are.—One cuts down certain of these trees, and the upright trunks which were left, if not cut right down to the earth's level, start shooting, and in a couple of seasons, I should say, one would have a bush which would be 6 feet or 7 feet high.

4943. And that would be such as would be attractive to the fly?—Yes, they would get shade; they would have something to sit on and so forth.

4944. (Mr. Austen.) Are the primary centres the only localities in the proclaimed area in which the conditions you have indicated as regards vegetation and humidity during the dry season obtain?—Yes, I believe so.

4945. Assuming those localities to have been destroyed, what chance do you consider there would be of *G. morsitans* establishing itself elsewhere?—Very little chance; they would only have very small isolated areas. You see, in these centres the water is present all the year round. There are other centres

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where water is present for a certain length of time after the dry season has started, but they are so small that *morsitans*, I believe, abandons them to a great extent.

4946. I understand that the extensions would have been destroyed before the primary centres?—Yes.

4947. So that the fly would not travel that way?—No, they would not have any continuity of forest to travel along.

4948. Would there be a possibility of their travelling, so to speak, by a worse road in some other direction?—On one side you have the lake and on the other you have large plains. I refer to area No. 1.

4949. Quite open country?—Open country, and unless you have some one travelling across there from some of these centres the fly would not go across by themselves, at least I do not think so.

4950. Would the primary centres after the first burning have to be cleared annually?—I do not think so. I should think one would have destroyed a considerable proportion of the flies, but one might do it every other season, and if, of course, in the first instance one cut down the trees right to the roots to the earth's level, it would take a very long time to grow up again, and then you would only have short grass left.

4951. The roots of the trees would not sprout?—They would, but it would take them a long time to sprout anything like sufficient to provide shade and protection; and then another point is that I believe you would destroy such a considerable number of flies that there would be very few in some of these centres to create any sort of spread.

4952. The slow breeding of the fly, of course, would help you in that respect?—Yes.

4953. Do the natives in Nyasaland grow bananas?—In a few districts, but they do not grow bananas to any great extent, not the same as Uganda.

4954. I am not aware whether banana plantations afford cover for *G. morsitans* as they do for *G. palpalis*?—I have tried to find the pupae and I have failed near banana plantations or in banana plantations.

4955. Have you found the fly in the plantations?—Very few.

4956. (Sir Stewart Stockman.) Are these concentrated areas bush?—Light forest.

4957. A witness gave it as his opinion that the flies rested on the trees and dropped down on their prey.—Very likely.

4958. Is that your experience—that they come down on their prey from the trees?—I have seen flies sitting on grass, on stones, on trees, and on the trunks of trees.

4959. Do you think that bird-liming or spraying with poisons would be any use as a practical measure?—I do not think so.

4960. (Mr. Rothschild.) I only want to ask you one or two questions, and the first is: In these concentrated areas have you noticed at all if they would be more difficult to clear, or less difficult than the general run of the country through which *morsitans* spread in the wet season?—More difficult to clear.

4961. One witness said that if the bush was properly cleared in a *morsitans* area the chances were that elephant grass or other tall grasses would grow up and effectually suffocate any young bush which was likely to come up; have you noticed that?—I should think that is very unlikely.

4962. You consider that the only method is by first cutting the bigger trees down to the earth, and then systematically burning every other year?—Yes.

4963. (Sir Mackenzie Chalmers.) Only one question as regards those concentrated fly areas; are they sufficiently well marked off from the surrounding country by the type of vegetation and characteristics?—Yes, one or two of them are very distinctly marked; they have dambos on one side, that is, plain country, and then thin attenuated forest leading off to the rest of the forest in the area.

4964. Then are the places differentiated by anything which makes the fly choose them?—They all have certain characteristics such as moisture, shade, and wild game on which the fly feed.

4965. I suppose it is not known at present whether the fly pupates all the year round, or only pupates when they are concentrated in these masses?—I could not give any opinion on that; I should think they pupate all the year round.

4966. (Sir William Leishman.) Is game any more plentiful in these concentrated areas than in the rest of the fly country?—Yes.

4967. All the year round?—Yes.

4968. Is the game similarly attracted, do you think, by water, as you think the fly is?—Yes, I should think so.

4969. What is the nature of the water there? Is it in ponds or does a river run through it, or what?—It is extensive marsh which gradually dries up until at the height of the dry season except for one or two places it dries absolutely, and then the game go down to the lake past area No. 1, I think, on the map. There are two or three rivers, one at Rifu called the Nyungwe, and the other the Chigolo river, about two miles north of Domira Bay; here there are little pools of water cut off from the lake by sand bars when the lake has gone down during the dry season, and numbers of game come down there at night to drink—buffalo, eland, and elephant.

4970. Do you think there would be any possibility of dealing with fly in these concentrated areas by drainage, or trying to diminish the water supply in any way? Is that a practical possibility?—I should think it would be very difficult to drain.

4971. Because it is too marshy to clear, do you mean, or is the extent too great?—It is rather lower than the rest of the country round about; it is in the middle of a saucer-shaped sort of hollow. With regard to area No. 4, water and moisture are supplied by the Lingadzi river.

4972. So that you get no outfall?—You get no fall.

4973. Have you noticed in the proclaimed area any other areas of the same character as regards moisture, shade, and game without apparently showing concentration of fly?—In Nyasaland?

4974. In this proclaimed area.—Those are the only areas where you have all these combinations present—the red areas.

4975. So that wherever you find these factors of moisture, game, and shade, you always find concentration of fly?—In otherwise suitable country, that is low-lying country not raised very high above the surrounding country, yes, those are about the only situations you find them in, where you have these conditions present.

4976. I should be very glad if you would give me some details of how you discovered these concentration areas; what was your system—what brought them to your notice?—I merely observed them during my travels. I found that the fly were always present in those areas when I travelled round the district, and in certain seasons I found flies elsewhere, and then they gradually diminished and disappeared, but they were always found in these situations, and it appeared to me that there probably was some explanation of this, I thought over the matter, and the result is that article I wrote.

4977. It was entirely your own personal observation of these areas?—Yes.

4978. Did you not employ boys to go out and search in different directions and make regular examinations at intervals?—No.

4979. How often did you go yourself through these areas?—I have done practically 15 miles a day for several months in that district.

4980. How many times, for instance, say, in a month, did you visit any one particular area which you have marked red there?—I should think I visited some of these places twice a month; I used to do a figure of 8 loop round that area every month, and then be ready to start off and find or examine any cases of sleeping sickness that were reported to me before the people had the chance of dying.

4981. Was your experience greater in these red areas marked there than in the areas outside them, for instance, or was your attention concentrated on them?—One used to enter these areas to search for

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pupæ, to supply meat to the camp, and also to shoot game for the purpose of sending blood to Sir David Bruce and making slides.

4982. So that you think your control observations of the area outside the concentration areas is sufficiently good?—I think so. I would like to call attention to the map when stating that I have visited every village indicated several times, and have camped at many. My record of work was returned in daily diary form in my official monthly reports.

4983. You were asked just now whether you thought the flies pupated in these areas, and you said they did. I am not quite clear about that. Do you think it is because they are in these areas that they do so?—Yes.

4984. That there is something attractive in that place which makes them tend to pupate in that place more than in any other?—No, I would not say that; I merely say that because the fly are situated there for a matter of three months, in my opinion they must pupate during these three months.

4985. Not because they are there?—Not because of the country being in some way suitable for pupation purely and simply; it may be so, but I have no knowledge on the subject.

4986. (Chairman.) I would just like to follow that up. You have yourself searched for pupæ?—Yes.

4987. We have been told that the pupæ are generally found in forks or holes in trees; have you found them near or far above the ground?—I have never found them at all.

4988. Assuming that to be correct, if you destroyed these supposed breeding areas in that way by cutting trees down and so on you would destroy all the pupæ that were in existence at that time?—Yes, I should think it is very likely one would destroy a considerable proportion of them.

4989. Can you tell me this with regard to the expense rather; are these places heavily timbered, not in the sense of the growth being thick, but is the timber heavy?—In Nos. 1, 2, and 3 they are not very heavy, but north in that area No. 4, at Lingadzi, the timber is rather heavy.

4990. If I followed the evidence you have given, you find these places which they like for their own sake, but do you think, game or no game, they would be in those areas, or that they require both the attraction of

the place and the attraction of game?—They require all the conditions which are there.

4991. (Mr. Rothschild.) May I ask another question? You said in answer to the Chairman that the first operation you would advise would be to separate or segregate these concentration areas from the rest of the forest?—Yes.

4992. By that do you mean to cut an open space and to clear an open space about a mile wide to separate the two?—Yes.

4993. And you would only start to clear the concentration areas after you had separated them at least a mile from all surrounding fly areas?—Yes.

4994. By fly areas I mean areas capable of holding fly at one season or the other.—Yes, but I would like to point out one thing, that most of these areas have a large plain or large open space on one side at least, so that it becomes a matter more or less of ease to separate them from the general forest areas in the district. For instance, there is Patsanjoka dambo here, the lake here and the extension of forest comes to here (indicating by a rough sketch). This is sand and this is dambo, and there is the Lintippe river down here. Here is also sand. So that you really only need to cut off the connection there and here to separate that area from the rest.

4995. You would clear at least a mile in those two portions?—Yes, a mile north-west and a mile, say, south-west. That would sever that entirely from any forest connections and put one mass of forest on one side. On one side you have a hill and on the other you have a wide dambo extending perhaps in places three or four miles wide. Clearing the connections does not really mean that you have to clear for a mile or two miles right round a certain specified spot.

4996. That I understood, but what I wanted to get you to say was just what you have said, that it merely means cutting a belt on each side.—Wherever the area is connected to some other forest area.

4997. Which of the areas are you referring to?—Rifu; I think it is called No. 1 in the map.

4998. (Dr. Balfour.) Do you think the presence of water in those areas in itself attracts *morsitans* to them apart from the question of the water also attracting the game?—No, I think all these conditions have to be present.

4999. Have to be together?—Yes, have to be together.

The witness withdrew.

Professor H. MAXWELL LEFROY, F.E.S., F.Z.S., called in and examined.

5000. (Chairman.) We are extremely obliged to you, Professor Lefroy, for coming to help us to-day. Among other problems which we have to deal with, and perhaps the most important of them all, is the possibility of being able to eliminate or deal with the fly so as to render it non-infective. I understand from the letter you have been good enough to write to the Secretary that you have not yourself personal experience of the tsetse-fly?—No, I have none.

5001. Your very large experience has been in dealing with insects in other parts of the world, but you have read, I presume, a good deal about the tsetse?—Yes, I have read a good deal of the literature.

5002. Among other things I have no doubt that you have read that they deposit their pupæ largely in holes in trees and not, as a rule, or not always, in places accessible to birds or anything which might be destructive of them; and what we should be very glad of is any suggestion for dealing with the breeding places. The conditions are different as regards *G. palpalis* and *G. morsitans*, not only the conditions of breeding, but the conditions under which the flies themselves live, and if you could make any suggestions to us as to getting at the fly in that way we should be extremely obliged?—It seemed to me from the literature I have read that attention in this problem had been more focussed upon the trypanosome than upon the habits of the fly itself. The fly, of course, is difficult to deal with for the very reason that it does not deposit eggs or larvæ, but deposits a full-grown

larva which immediately becomes a pupa and so is to some extent protected. But the fly itself is an active thing. It has fairly complex sense organisms and instincts, and it seems to me that it was possible that a method of dealing with the fly might have been obtained if a very close study of the fly itself had been made from a rather broad point of view. We have, for instance, to quote a specific case, a fly in India which is extremely troublesome to fruit, and by a purely chance discovery it was found that the flies over a large area could be attracted to one particular spot by the mere exposure of citronella oil. Further investigation has shown that that is due to the fact that the male recognises the female to some extent because she apparently gives off small quantities of this oil, and so you can attract surprisingly large numbers of the males of this species by simply putting out citronella oil. It is apparently true that for some fly species there is some specific compound which is given off by the females with which you can attract the males. In Pusa, where we have no other means of dealing with this fly, we now cope with it quite successfully with little trouble by simply putting out sheets of tanglefoot smeared with citronella oil.

5003. What was that for?—*Dacus persica*, a fly which attacks fruits, peaches, mangoes and other forms of fruit. It seemed to me that study directed to that point might possibly yield some method of trapping tsetse-flies on a large scale.

5004. That suggestion is of great interest. Taking the particular habits of this fly, is there any particular

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suggestion you would make, not with regard to attraction and destruction of the fly, but with regard to the finding and destruction of the pupæ?—I should imagine that practically nothing was possible. This is a case where it seems to me local knowledge is everything, and that a person who had never seen the puparium or the fly could really not express any opinion. When I first went to India we were totally ignorant, for instance, of where *Tabanus* bred. *Tabanus* is one of the commonest blood-sucking flies in India, and they occur in great numbers, and one would have thought it was impossible to live in the country and study insects without knowing where the larvae were. When we had once found one larva a person with a little experience could go walking across country and say, "There you will find *Tabanus* larvae." It may be the same with *Glossina*. It has been so with a good many insects. It is just that kind of personal experience which is needed.

5005. When you were successful in the experiment you have told us about, and attracted by this particular method males to a particular spot, of course the males met with a disappointment—they did not meet the females?—No, they were trapped.

5006. What was the effect on breeding? Were there sufficient males left elsewhere to continue the breed or did it destroy the fly practically?—It wiped out the fly practically.

5007. So many males were attracted to this disappointing spot that the females were left alone?—Yes, and practically from our point of view as a pest the thing went off; there were a few, but very few, left.

5008. From what you have read about tsetse should you say that they would sustain their existence without, I will not say an adequate, but without a fair supply of blood food?—I should think they would sustain existence for a fairly long period.

5009. And breed?—I do not think they could breed—no.

5010. Still, I have always been a little bit puzzled with this point, and perhaps you could tell us about it: there are many insects which always get blood when they can, and as far as one can judge must have very few opportunities of getting blood, and yet they subsist in very large numbers. How in a case of this kind (if I am not wrong in saying that there are such cases) would you explain that?—It is a fact, I believe, that that is so, for instance, with some mosquitoes, but the mosquito deposits eggs. The substance from which those eggs are formed is laid up in the pupa before the fly emerges. Many insects do not feed at all in the adult stage, but still are able to deposit eggs, the material for which has been laid up in the larva stage. *Glossina* do not do that; the egg and larva have to be entirely provided for by the parent, and therefore if blood or food is unobtainable it seems clear that the development inside the fly of its larva is impossible. But the case of *Glossina* is in that way unique as opposed to the mosquitoes and other insects which simply deposit eggs. The adult has to provide for the whole nourishment of the larva inside it for the whole of its life.

5011. Would you go so far as to say that if flies were unable to get blood, or a very small proportion of them alone got it, they would of themselves disappear in the absence of breeding power?—I should say it was probable, but there is no statement you can make generally in entomology which is not contradicted somewhere or another. General statements in entomology are hopeless things to my mind. It seems probable.

5012. Have you ever had any experience of an experiment to endeavour to attract flies generally to a particular place in the hope that they might breed there and that you might have access to their breeding places in that way by creating as it were artificial breeding places? You see what I mean?—I see what you mean exactly. I have had no experience except in the case of individual species of which we knew the tastes fairly well. This summer I wished to get a large supply of a maggot for a particular purpose, and in thinking it over and watching the insect

a bit I exposed what I considered was an adequate material to attract it to lay eggs and I was entirely successful. I had no reason to think the insect was in the neighbourhood, but I was able to get a large supply of the eggs deposited under conditions in which they were able to live, but then I knew more or less what the tastes of the insects were. I dare say that would be possible with *Glossina* if one was able to make a close study of it, but it would not be possible to do it on any general principles.

5013. Have you by any chance read a paper by Mr. Fiske on *Glossina*?—I do not think so.

5014. (Sir William Leishman.) One of the things we should wish to study would be obviously the fighting of the tsetse-fly. Could you give us any suggestions as to the way in which that might be tackled? It would involve marking a number of flies, I suppose?—Probably, I think, yes. It is apparently possible to mark flies and let them loose and identify them after considerable periods. A good deal of work has been done with that in England; Mr. Howlett and others have done work in connection with that with regard to the fly plague at Norwich. One has to catch the flies, and one can paint them on the dorsal surface of the abdomen or the thorax and release them without interfering with them. I should think that would be necessary.

5015. That would be better than clipping off a bit of leg or something like that?—I think so.

5016. Have you made any experimental study of the sense organs in the antennæ of insects?—Of insects generally I have.

5017. Might I ask on what lines the experiments you made have been?—In studying the sense organs there are two lines of work which, as it were, come together at a point. One is the minute study of the structure. One can get clues from studying the structure minutely, and by "minutely" I mean to say we get a section and study it in the finest possible detail in the hope of finding evidence of what may be sense organs. The other line which leads to the same point is the close observation and study of the insect itself. An observant study of the insect by itself frequently is somewhat barren of results, I think, unless it is worked in with the study from the structural side. Of course, the study from the structural side alone is comparatively useless and you must do with it the study of the habits. By working systematically with those two things I think one can get some idea of what the insect's mind is doing, what it is thinking to some extent, what its instincts are, how far they are instinctive things, and how far purely reflex things, and so on. The really important thing in that is the minute and close study of the one species. If I wanted to tackle an insect pest in India, I used to go myself to the place where I could study it most easily, and the only thing to do is, as it were, to absorb oneself and steep oneself in the insect and try in a way to feel what the insect itself feels like and so get an idea—it may be wrong; but you get eventually a sort of picture in your mind of what the insect is doing and why it does it. That is, I think, the root of the whole of successful entomology at the present time.

5018. It is possible, I suppose, to study the effects of various stimuli on the fly?—Yes, you can study those correctly, chemical stimuli and the effect of light, wind, and heat.

5019. And smells?—Yes. I think the study of those simply as mechanical things is not as fruitful as the more personal and perhaps less accurate observation.

5020. You told us an interesting experience about finding the *Tabanus* larvae in India; what sort of situations were these larvae found in?—*Tabanus* larvae are found in small pools which contain a considerable amount of fallen leaves and similar rotting material, and, as a rule, they are not exactly under trees, but they are in small open spaces surrounded by trees very frequently. Frequently there is fairly coarse vegetation round the pools. Now those kind of pools I think are distinct from, for instance, a large pond or lake or river, or a pool in which there may be no rotting

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vegetable material, but say large quantities of pond weed. One gets to distinguish different kinds of pools and ponds, and to get a rough idea of what is in them. It is in that way that, having once found the particular kind of pool in which *Tabanus* larvæ bred, one was more or less able with certainty to say, "You are likely to find *Tabanus* there," and find it and go to another pool and say, "You will not find them there." It is difficult to describe, because it is in one's own personal experience.

5021. Is *Tabanus* purely a blood-sucking insect? Have you any reason to think it feeds on anything except blood?—I have no reason to think so; I think it is purely a blood-sucking insect, so far as my experience goes.

5022. In your experience do blood-sucking insects drink water at all?—I cannot say in free life: in captivity they do, some of them anyhow.

5023. Which ones, may I ask?—I am afraid I shall be unable to recollect, but I am thinking of the *Tabanids* of the genera *Tabanus*, *Pangonia* and *Chrysops*.

5024. Perhaps you might include any others you think of when you get your proof?—I could do that more easily, because I shall have to think back.

5025. Have you any evidence at all of any seasonal concentration of flies which you have studied in certain areas?—I do not entirely understand the question.

5026. The point of my question is whether flies which you find at one time of the year showing a certain distribution, at another season of the year may tend to congregate definitely in one particular place, and then again to spread themselves away from that centre. We have had such a thing described to us in connection with the tsetse-fly.—I can only at the moment think of one case which was observed, and which Mr. Howlett and myself considered so extraordinary that we looked out for it, and paid special attention, and that was the case of a species of *Ceratomyza*, which is one of the smaller flies which we found one day in very large quantities on a particular tree. It was a very large pipul tree, and we found very large quantities indeed. That phenomenon we saw, perhaps, for 24 hours, certainly not longer than 48 hours, and then the flies disappeared, and could be found one or two here and there scattered over the whole place. We have observed that I can certainly say in two successive seasons; I do not know about more. I should have to find out from Mr. Howlett, who particularly studies these things. That is the only case I know of that comes anywhere within that.

5027. Had you noticed any physical features about that locality or about the weather on the occasions you observed it?—No, we were unable in any way to account for it; it seemed to us an extremely remarkable phenomenon, and Mr. Howlett paid more attention to it than I did, and, at the end of it, I think I can say that he was unable to find any one factor which could have influenced those flies in coming to that particular spot at that time.

5027A. (Sir Mackenzie Chalmers.) Was it always a pipul tree?—Yes, it was the same pipul tree in the same spot.

5028. How long have you been working in India?—I was there 10 years.

5029. Could you give us any instance by way of analogy in which disease in man or animal has been attacked through the insect carriers successfully? You have mentioned one, could you give us any others?—A case in which what has been done?

5030. Where the insect has carried disease, and where the incidence of that disease has been attacked through the insect.—My special business in India was not connected with that; Mr. Howlett, my colleague, was entirely in charge of that. The case I gave of *Tabanus* is not a case of carrying disease; it was a direct pest.

5031. Could you give us any other instance (it does not matter whether disease is carried by the insect or whether the insect is a direct pest) where insects have been successfully attacked?—I had to do with a number in India, and perhaps the most striking case we had was the case of bollworm in cotton, an insect pest which attacked cotton in the year 1905 in the

Punjab, and which occurred over an area of 750,000 acres scattered over a larger tract of country. The outbreak was entirely unforeseen and unexpected, and our attention was not drawn to it until the crop was coming into flower, because it is only then the pest begins. I went up to see it, and before going to the spot I saw the Government of the Punjab at Lahore, and from all the papers and records we had before us we came to the conclusion that it was nothing to do with insect pests, but to make the matter clear I went on the spot, and directly I got there I found it was entirely due to this bollworm. We could do nothing that season; the cotton crop was in flower, and there was practically no crop left. We lost the whole of the crop. After carefully studying it on the spot and going about and thinking it over and sitting in cotton fields, I eventually went back to the Government of the Punjab and suggested that three things should be done. One was the destruction of the cotton plant in the winter, the second was the growth of a particular plant which we thought would be more attractive than cotton to the bollworm, and the third was a hazardous thing. I suggested that the outbreak was due to the fact that in the preceding winter there had been an extensive frost, and that this frost had destroyed the parasite which normally keeps in check this bollworm. We found the parasite was absent in that season, whereas we knew it had been present in the previous season. I suggested we should map the line down the country to which the frost had extended, and below that line introduce the parasite again into the Punjab if we could. Those things were done the following season; they were done partly by the district officers, and partly by direct appeals by leaflets. One was a complete failure; we found that the plant we wished to grow as an alternative food plant was a garden plant, and could not be grown in the fields, and although an enormous effort was made to distribute seed, and practically every village in the country got seed, we could not grow it. The other two were completely successful. We restored the cotton crop; there was nowhere a loss of more than one quarter in that year, and we restored the normal condition of things. Now, it was not clear how far that was due to the parasite or to nature, but fortunately for us an area in Scinde, which comes under the Bombay Government, had not been treated; the Bombay Government were not confident that we could do anything, and nothing was done, and the following year in Scinde the cotton crop was again practically a total loss, and the parasite was shown to be absent. In the third year we re-introduced the parasite into Scinde and restored normal conditions. I think that is about the most striking case I have had personally to deal with. We gambled, of course, on the cold winter and the parasite business, but I think we were successful. Now whenever there is a cold winter we do not wait for the cotton crop to be destroyed, but we run in parasites the following season from parts of India in the south where we can get them.

5032. Is there any other analogy you can give us?—My whole business in India was this sort of thing, and I could take up the time of the Committee for a long period going over it.

5033. That was a very striking case?—Yes, I think it was the most striking case of the kind I have had personally to deal with. We have to do these things, and one has to study the thing and try to find the weakest spot in the insect, and go for it.

5034. I suppose you do not know if there is any likelihood of any parasite being found which could successfully prey upon the *Glossina*?—I think it is extremely improbable that there is any direct parasite, but it seems to me likely that there is something which preys on the tsetse-fly itself, not by simply parasitising it, but preying upon it, catching it, and killing it.

5035. Not an insect, but a bird you mean?—Quite possibly an insect.

5036. That would be a matter for observation?—Local observation and study.

5037. (Dr. Chalmers Mitchell.) Supposing you had the duty of trying to deal with the tsetse-fly question in

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[Continued.]

Africa in the same way as you had to deal with insect pests in India, precisely what steps would you take now.—Do you mean at once?

5038. Yes.—I think I should inquire as to where I could most easily see large quantities of tsetse under natural conditions.

5039. That you could be told at once.—I would then go to the place, and I know no other method but to sit there and observe and study the insect and think about it, and, as it were, steep oneself in the tsetse-fly until one began to see a way out. In my experience there nearly always is a way out; it may be something you dream about at night or it may be something that comes to you from direct observation, but sooner or later, if you think enough about the thing, it seems to me you get a remedy. Frequently the remedy is impracticable on account of cost; that is the bugbear of this sort of thing; one has often to begin to think about cost, but that is the way I would proceed first, and after three months', or four months', or six months' experience of the insect itself, I think one would be able to see how one could best proceed further.

5040. Of course, you have been 10 years in India, and you are now in London very fully occupied?—Yes.

5041. Supposing instead of it being your duty to go yourself you had to select some person to send, do you think it would be difficult for you to find such a person?—I think extremely difficult.

5042. Why?—In my experience the people who care for that sort of thing are extremely few. It is not pleasant, and I think many people think that it is in some way undignified to sit down and study one small insect, and it has been my practical experience that it is extremely difficult to get men who will do that kind of thing in that particular way.

5043. There are a great many people, as you know, who are what is called trained entomologists?—Yes.

5044. I do not want you to make any criticism upon people in your own line of professional work, but do I gather from you that you would not have much confidence in the average trained entomologist?—I should not for this particular thing; I think that it would be extremely difficult to get somebody who thought of it and got at it in that sort of way.

5045. Would it be necessary for such a person to be what is called a trained biologist? Supposing you were to get a man with a turn for natural history and general zoological information, do you think that sort of type of man would be useful?—He might be as useful as a highly trained entomologist or a specialist in entomology. I think it is a question of personal characteristics more than training or anything else.

5046. If you widened the sphere of choice in that way do you still think it would be practically impossible to select a suitable person?—I do not think it would be impossible; I think someone with a wide knowledge of men who was able to have a large number of men to select from might be able to do it. I had in India a large native staff; I had over 100 trained men, many of them with university degrees, and out of those I can think of in the whole of my experience I have only come across two men who had that particular kind of brain organisation or whatever it is.

5047. May I put this question to you? You are not very much surprised, considering the rarity of the kind of power of observation which you wish, to find that great strides have not been made on the entomological side yet with regard to *Glossina*?—No, I am not.

5048. But it does not affect your confidence in the possibility of proceeding to attack the disease by that method?—I have formed the opinion that the insect has not been sufficiently studied from that point of view. As I said at the beginning, I think attention has been focussed upon other points, and that that particular point has not been gone for.

5049. Your experience would generally lead you to believe that if you could get the right type of man it is more probable than not that the weak point in the life history of the insect could be found?—I think so.

5050. (Mr. Rothschild.) I have only one or two questions to ask you. The *Glossina* has a very

peculiar sensory organ on the antennae, and therefore at first sight what you stated to the Chairman about the attraction to the males of this other fly would seem to apply, but unfortunately this organ (and apparently entirely the same organ) is found in both sexes. Now I, in the course of purely cabinet work, have come across two or three very extraordinary cases. There is a certain genus of Oriental moths, of which there are some 20 or 30 species, and they in both sexes have practically the same form of antennae, but there is one species not very different from the others which has an enormous sensory organ on the middle of the antennae of the male. In the case of that moth we should naturally look out at once for some special attraction, supposing it to become a pest, but in the case of the *Glossina* the fact of the creature possessing in both sexes the sensory organ seems to show that this sensory organ plays a decided part not so much in the sexual life of the creature as in its food?—Yes.

5051. But we are also told that the tsetse-fly follows moving bodies rather than stationary bodies in search of food, and that it prefers a dark colour?—Yes.

5052. That seems to show again that the sensory organ is not one connected with food, which narrows the question down to whether it is to find a suitable locality for the process of breeding on the part of both sexes. Do you not think that if either a trained or an untrained entomologist were sent out there he should be instructed rather to study the thing from that point of view first, because it is evident that as we know what attracts the *Glossina* from the point of view of food, and as we know that its sensory organs can have nothing to do with sexual search, because they are in both sexes, it seems to have something to do with the searching out of locality, and therefore might lead one more quickly to the breeding places?—Yes. I think that is possible, but if an entomologist were sent out to study it, it would probably be better not to direct his attention to any one particular point. I mean that would be my own feeling.

5053. You mean to say that if you could find a competent man with the characteristics you have detailed to the Committee, you would say to him, "Now you go and study that fly," and not tell him anything about it?—Exactly.

5054. You think that is the most likely way of producing a result?—I would go further than that, and if I could select a man who had never read a line about *Glossina* I would prefer him.

5055. Then, again, in answer to a question you stated that you did not think it very likely that one could find a parasite which would destroy *Glossina*. I think Mr. Austen knows or, rather, has received a report, that from a pupa of the *Glossina* a small Bombyliid fly has been raised. Do you not think from experience in other cases that, although it might not be so easy to find a native parasite under its own name which would destroy it, if you found a parasite of the same genus, or closely allied to the one that inhabits it at present, and introduced that from somewhere else, you would have a greater chance of success?—*Glossina* is so peculiar in being the only pupiparous genus in the family that I should say the chances of success were infinitesimally small.

5056. Supposing you discovered there were parasites which lived on Hippoboscids?—It is possible it would be worth while introducing it. I would rather seek for the parasite of some other pupiparous insect. I believe parasites on Hippoboscids are not known to occur, but I speak entirely from memory.

5057. I do not know of any occurring, but I merely thought of them as breeding in the same way as *Glossina*.—As being the most likely thing? I think that is quite likely.

5058. (Mr. Austen.) You told us, Professor, of a very interesting experience of yours in connection with trapping a fruit fly. Have you had any similar experience in connection with traps for blood-sucking flies in India?—No, I have not. The only trap for blood-sucking flies I know of is the mosquito trap, which was discovered in Pusa, and which is not based upon that, but is based on the liking of the mosquito for a dark place, and in certain respects not dissimilar to

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5059. That, I think, is shared by all blood-sucking Diptera, is it not?—I do not know—by a good many, anyhow; I am not certain.

5060. Did not Mr. Howlett make some interesting observations in connection with the attractions of human sweat on mosquitoes?—Yes, I believe so; and Mr. Howlett has done a lot more work on the *Dacus*, his idea being to distil the female *Dacus*, and to discover what the predominant odour was, analyse it chemically, and then reproduce it by a suitable mixture of oils, and so get at the method of attacking and destroying any other fly. I believe fruitful results are coming from that.

5061. As regards enemies of the adult fly, I think you said you would be inclined to expect better success from them, or more assistance from them, than from actual parasitic enemies of the immature stages?—Yes.

5062. Since it is necessary, in dealing with a noxious insect on parasitic lines, to go to another country where the noxious insect, or something like it, exists, but is not injurious owing to the fact that it does not exist in sufficient numbers; and since we know in one case that a certain wasp has been found to provision its nest with *G. palpalis*, would you suggest, or do you think it would be likely that any good could be attained by searching for some predaceous wasp, in some other country, which is known to prey on a blood-sucking fly, and introducing it into tropical Africa?—I think very valuable results might come either from a wasp (I did not know there was one) which behaves like that with *Glossina*, but I should have thought a wasp like that which stocked its nest with large Diptera was less likely than one of the Asilidae or other predaceous flies which do sometimes seem to specialise on particular groups.

5063. My next question has reference, in the first instance although not altogether, to a point of terminology. Since a certain word has been used it seems advisable to ask a question on it. *Glossina* is really *larviparous* and not *pupiparous*, is it not? It is the Hippoboscidae that produce living pupae?—Yes.

5064. Therefore, so far as it goes, that gives us perhaps a little better chance, or, at any rate, renders such an insect, although only for a brief period, more open to attack than if it actually extruded a pupa, as is the case with a Hippoboscids?—That is so.

5065. I do not know whether my next question is worth anything, from the trapping point of view; *Tabanus*, to which reference has, I think, been made, is not entirely a blood-sucking fly, is it?—I know no other food that *Tabanus* usually takes.

5066. Have not observations been made which show that both sexes suck honeydew, for instance, and has the opinion not been expressed that species of *Tabanus* are probably, at any rate, capable of going through all their stages without a meal of blood?—I did not know that any Tabanid except *Pangonia* would take honeydew or liquid; I have seen *Pangonia* take liquid, but I would have thought they would have gone through the whole of their stages without blood.

5067. *Pangonia*?—Any Tabanid.

5068. Do you mean without taking any food at all in the adult stage?—Yes.

5069. (Chairman.) Do you mean any fly or *Pangonia*?—Any Tabanid; I should have thought they would have gone through their stages without blood, but I do not pretend to know the habits of Tabanidae thoroughly or to know all about the literature.

(Mr. Austen.) I asked that question because observations have been made in the United States, which seem to show that Tabanidae suck honeydew and other things, and that females as well as males may pass through the period of adult life without tasting blood.

5070. (Dr. Balfour.) What was the nature of the parasite of the bollworm of which you spoke?—It was a Braconid.

5071. It was an insect?—Yes.

5072. Have you studied the natural fungus parasites of insects much?—No.

5073. So that you are unable to say whether their artificial introduction into *Glossina* would be a likely method of dealing with the latter?—From my experience, the fungus parasites of insects are

useful, so far as I know, in only two groups, the Aleocharids and the Coccids, the scale insects, and so on, and very great use is made of fungi, but from 13 years' experience of the tropical fungi, at all events, I should have said that there was not much hope of their being successfully used. It is a point one would have to keep in mind, but I should not anticipate very much from it.

5074. In the case of locusts it has been a failure?—Yes, it has been a failure, but owing to a curious reason. There was a locust fungus sent out from the Cape some years ago as being locust fungus, and it was used without success in a good many parts of the world, including India. It was discovered some years afterwards, only about 1906, that they had sent out the wrong fungus, that the fungus which kills the locusts was not capable of cultivation in an artificial medium, and they had been sending out the *Mucor* which lives on the dead locusts.

5075. With the real locust fungus, how did transmission take place from locust to locust?—In this particular case I could not tell you; I have not sufficient knowledge. There is another fungus which has been recently used, and which has come from somewhere in America—Central America or South America—and which has been reported to be used with great success. It has been cultivated in Paris and distributed from there, but this has happened since I left India, and I have no personal experience of it.

5076. Could you conceive of such a fungus parasite being transmitted by the tracheal system to flies?—That is possible.

5077. I have read somewhere or another of a German or Austrian method for dealing with moths—having a bright light attracting them, and then a sort of wind shaft, which sucks them down into a petrol tank.—Yes.

5078. Supposing you were able to put into force the imaginary female trick in the case of *Glossina*, would it be possible with such a strong-flying fly to use that wind shaft effectively?—I think quite, if you could find any means of attracting them.

5079. Would it be a better way of dealing with them than by tanglefoot?—I could not say.

5080. The last question I wish to ask you is one which I quite realise you may not be able to answer. In blood-sucking flies, do you think there is sufficient difference in the internal anatomy and secretions of the male and female to prevent a protozoal parasite like a trypanosome passing through its cycle in one, say the female, and not in the other, say the male?—I could not tell that.

5081. (Chairman.) I take it that generally your view is that there are a number of possible methods of attack which, as far as you know, have not yet been tried in the case of *Glossina*?—I think so.

5082. And that you suggest as the right type of person to deal with the problem a young and enthusiastic man who happens to be a good observer, I imagine?—Yes, that is what it comes to.

5083. (Sir Mackenzie Chalmers.) From your knowledge of other insects do you think it is possible that any dip is likely to be discovered for cattle which would have a repellent effect on the *Glossina*?—I think it is extremely likely. There has recently been an outbreak in New South Wales of a blow-fly which attacks sheep. That blow-fly is a very important pest of sheep, and I was recently asked whether I would go out to Australia for a period of years to study it. The whole of the preliminary arrangements were made and we had planned out the line of campaign, and that campaign was that two chemists and two entomologists should go to Australia and sit with a large supply of flies and sheep and study the psychology, as it were, of the fly and the effect of dips, and I have very little doubt that at the end of two or three years we should have got a dip which could be applied to sheep and which would prevent that fly coming near the sheep.

5084. I suppose a dip which would apply to sheep or cattle could be used to some extent as an ointment applicable to man in a fly area?—To some extent,

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yes; possibly it would be so unpleasant that as a practical thing it would not amount to very much.

5085. What happened? Why did you not go to Australia?—The negotiations are not yet finished; that is the present position.

5086. (Sir Stewart Stockman.) You think that a good dip might be found?—I think so.

5087. I take it that you do not agree that a good dip exists at present for keeping insects off for any lengthened period?—Not for any period, but I think one could be found.

The witness withdrew.

SEVENTEENTH DAY.

Tuesday, 16th December 1913.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (Chairman).

Mr. E. E. AUSTEN.
Dr. A. G. BAGSHAW.
Dr. ANDREW BALFOUR, C.M.G.
Mr. E. NORTH BUXTON.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Dr. W. A. CHAPPLE, M.P.
Colonel Sir W. B. LEISHMAN, F.R.S.
Dr. P. CHALMERS MITCHELL, F.R.S.
The Hon. L. WALTER ROTHSCHILD, F.R.S.
Mr. A. C. C. PARKINSON (Secretary).

Mr. A. E. HORN, M.D., B.Sc., called in and examined.

5088. (Chairman.) I understand that up to 1910 you were much interested in and had a good deal of experience in sleeping sickness?—I worked for six months on the Gold Coast on sleeping sickness alone.

5089. In what part of the colony was that?—In what is called the Volta River district.

5090. Is that a *palpalis* district or *morsitans* district, or both?—It is both; they have both *palpalis* and *morsitans* there.

5091. Did you meet with much human sleeping sickness there?—No. I was working there for six months examining cases over a district of about 350 square miles, and I only found five cases of sleeping sickness.

5092. Do you think from your knowledge of the natives and your observation that there were likely to be many cases of which you would not be aware?—Yes, I believe there would be.

5093. I suppose it is a question you cannot possibly answer, how much you ought to multiply your five in order to approximate to the truth?—I could hardly answer that, but they gave me no help in finding cases; one had to examine every one who was sick in the hope of finding cases.

5094. Was there much mortality there from general causes?—Nothing very much.

5095. You could not get at it in any way of that sort? What would you say was the most prevalent disease among the natives in that part?—There was nothing very striking at the time I was there, and I do not think the mortality could be ascribed to any one disease in particular.

5096. As I understand from your memorandum (tell me if I am right) the natives were able in a sense themselves to diagnose sleeping sickness in their own persons, were they not?—Yes, they were.

5097. And from what you say I infer that they did not look at it as a very fatal or serious disease?—That is so.

5098. I should like to follow that up for one moment. In that belief do you consider that they were in any sense justified?—It is very difficult to say, because they showed me so many cases in which they had excised glands from the neck, and they said that these were cases of sleeping sickness which were now cured. They then presented no signs of sleeping sickness, and I could not judge as to whether they were correct in what they said, namely, that they had suffered from sleeping sickness.

5099. Had they a specific name for sleeping sickness of their own?—I do not think they had; I did not gather the name, at any rate, amongst the different tribes.

5100. Was there any other disease at all prevalent which would affect glands in a similar manner, so that they might confuse one illness with another?—There was a good deal of common dirt; they were not cleanly people as a general rule; they had a good deal of glandular inflammation round the neck, for instance, and the children suffer from *pediculi* and various diseases.

5101. Would your general conclusion be that there was not on the whole any very large quantity of sleeping sickness in that district, or would your experience not be sufficient to say that?—I think there was not a very large quantity; at any rate, it was not in an acute stage; there was no epidemic.

5102. If we are rightly informed (and I have no doubt we are) *gambiense* conveyed by *palpalis* is a disease that may incubate for something like years before it develops into any acute form. Can you speak about that at all?—I have no experience of that.

5103. With regard to *morsitans*, on the other hand, we are told the disease runs its course very swiftly and is very acute. In the districts where you yourself found sleeping sickness, what were the relative proportions of the two flies? You see what I am driving at?—Quite; I found about equal quantities mixed together; in some places along a certain road one would meet with *palpalis* and *morsitans* in practically equal quantities, and the same throughout the villages.

5104. If I follow your memorandum rightly the only trypanosome you yourself found was *gambiense*?—Yes.

5105. You did not find what we call *rhodesiense* or *brucei*?—No.

5106. And you found that (I am not quite clear from your memorandum) in man?—That was in man.

5107. Did you make any experiment on game?—No, I did not.

5108. Was there much game in that country?—Very little there.

5109. There was some?—A few small antelope; I practically saw none.

5110. Were there any of the larger mammals?—Very few indeed. I saw one or two antelope up to the harnessed antelope, but nothing more than that. There

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[Continued.]

were bush cow (buffalo) about, but I never saw them; they were in very small quantities and in parts to the north of the district I was working in.

5111. If I followed you quite rightly, so far as that part of the world is concerned you would look on man as the reservoir rather than the wild game?—Yes.

5112. Have you anything to say about domestic animals?—I did not examine the domestic animals; I could not say.

5113. Are there cattle in that country?—There are goats and pigs, not cattle.

5114. Is that because cattle cannot live there?—Probably it is; I did not inquire. I imagine they would not be there owing to the large numbers of tsetse-fly.

5115. I see you refer to Dr. Gallagher's finding large numbers of trypanosomes which were thought to be *gambiense* in the blood of domestic pigs.—Yes.

5116. As far as that goes one may fairly assume that the domestic pig was tolerant to a trypanosome which might be *gambiense*?—I am not sure, because at the time Dr. Gallagher found these there was a good deal of sickness and the pigs were dying off rather quickly. I believe he ascribed their death to the presence of the trypanosome in the blood. There was a kind of epidemic among the pigs at that time.

5117. Were those trypanosomes found in living or dead pigs?—They were found in living pigs, I believe.

5118. And pigs were largely kept, I understand?—Yes, I think so, but that was in Southern Nigeria, which is a different part of the country.

5119. But still it deals with the point I am on just now: where you found those conditions, would it not be an inference that the pig was tolerant to this trypanosome, and I assume it to be *gambiense* for the moment?—Yes.

5120. If the pigs were alive and you found *gambiense* in the blood of the pigs, the assumption would be that the pigs were tolerant to the *gambiense*?—Yes, except that those pigs were obviously ill at the time.

5121. But they were tolerant for a period, and during that period they would form a reservoir?—Yes.

5122. You have not at all considered the question as regards Nyasaland and Rhodesia?—No, I have had no experience of that.

5123. You have no doubt heard the suggestion that some experiment should be made, on the assumption that wild mammals form a reservoir, by their destruction in a selected district; have you anything you would like to tell the Committee about the effect of such an experiment, or what we might learn from an experiment of that sort?—I can hardly say with regard to that place, but I do not think it would be very effectual in West Africa, because I do not think we get the game in most places in such large quantities and so near the natives as is apparently the case in East Africa.

5124. You yourself from your personal knowledge cannot speak with regard to the presence of the human trypanosome, whether it be *gambiense* or whether it be *rhodesiense*, in wild game?—No.

5125. Have you had opportunities at all of observing the movements or the habits of the fly, either or both?—No, except during this period when I was examining sleeping sickness cases in the Volta River district. They were very prevalent at one time, and about a month later they were very much fewer in number.

5126. How does that stand in relation to the wet and dry seasons?—I think at the time of the heaviest rains in August they are very prevalent, and at the end of September, as the rains begin to go, they become fewer.

5127. Were those large areas or limited areas where you found them at times in large quantities?—Perhaps two or three days' travelling—40 to 60 miles.

5128. There they were thick at certain times?—Yes, and very few at other times.

5129. (Dr. Bagshawe.) I should like to ask you to what you attribute the small incidence of human

trypanosomiasis in the Gold Coast?—I could not tell you exactly that it is a small incidence. There is a small appearance of illness, but it is quite possible that there may be an immunisation. There may be infection of the native who does not show the illness, having become immune to it.

5130. If that were the case would you expect to find trypanosomiasis more in children or more in adults?—I think perhaps more in children. The excessive presence of trypanosomes perhaps might not necessarily produce illness. I should expect to find it more in children.

5131. I have some statistics here from the Gold Coast by Dr. Kinghorn. When he was there he analysed apparently the age distribution of 89 cases, and he says that of those 89 cases 79 were over 11, he calculates, and only 10 were 10 years of age and under, a very small number.—Yes, in my experience I found five cases of sleeping sickness, two of whom were children, and in those I found the trypanosome. The other three were older people and there were no trypanosomes to be found, but they presented all the clinical signs of sleeping sickness.

5132. You think the disease may be commoner in children than is supposed, that is to say, that children may harbour the trypanosomes more commonly than is disclosed?—I think that is so.

5133. Would you not expect them to present signs of illness more than the adults?—I think they are possibly confused with ordinary malarial fever. The native does not perhaps analyse them more than that.

5134. You think it is likely that there are many cases of fever in children which possibly are due to the trypanosome, but which are held to be fever due to other causes?—Yes, malaria, for instance.

5135. Todd (of course it was not the same colony) found very few cases in children, and none in persons over 40 years of age. Are you aware that Todd suggested that that might be due to immunisation?—Yes.

5136. What do you think of that?—I think it is quite likely.

5137. You think children are just as subject to acquiring infection as the adult natives?—I think so.

5138. You cannot give us any opinion as to the average duration of cases in that country?—I am afraid I cannot say anything exact, but the children I saw got rapidly ill, more ill, and died shortly after I left the country, I heard.

5139. You speak in your summary of tsetse-flies being spread by the formation of new railways?—Yes.

5140. Could you give us any names?—I think Accra, the capital of the Gold Coast, is a case in point. About seven or eight years ago it was practically free from fly up to within 18 or 20 miles of the town, and the ponies lived there quite well, but I hear, although I have not seen it, that since the railway was built from Accra into the country they have quite a number of fly in the town itself.

5141. Has there been any disease amongst the domestic animals?—I believe the ponies will not keep so well, but I cannot be certain, as I have not been there since myself.

5142. You have heard that the fly has come into the town?—Yes.

5143. And do you think that the fly will breed there, or is it that only stray specimens are brought in?—I can hardly say that. I imagine they are not breeding very much. I do not think there would be any facilities for their breeding.

5144. Have you noticed in that part of West Africa any seasonal differences in the distribution of the tsetse-flies, *morsitans* or *palpalis*?—Beyond what I have just mentioned, that towards the end of the rains they were fewer in number; I have not noticed more than that.

5145. It has been suggested with regard to some tsetse-flies that in the dry season they become limited to certain areas and in the wet season they spread.—I imagine that is true.

5146. But you have not noticed anything of the kind?—No; certainly in the dry season in travelling up in the Northern Territories in the Gold Coast it is

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chiefly near watercourses and small connections of water where you are troubled with the fly; when you are away from these sort of places you do not see them so much.

5147. That is *palpalis*, is it?—It is both *palpalis* and *morsitans*.

5148. (Mr. Buxton.) It is difficult for a layman like myself to understand how the railways could act as spreaders of the fly; have you any hypothesis about that? In what way would it operate?—It is a nearly closed carriage passing through a fly district and containing people; the fly finds out those people, gets in there and is carried on by the train mechanically from one place, its home perhaps, to another town very much further out. I think that constantly happens.

5149. And you think that would be sufficient to account for it?—I think so. There are several trains a day and probably several flies getting into each carriage.

5150. Have you any experience of a change or growth in the breeding places of either of these flies, either *morsitans* or *palpalis*?—No.

5151. Because unless their breeding places are handy they would find a difficulty in retaining their hold on that town of which you spoke, would they not?—Yes, I think probably outside most of the towns there are more or less facilities for the flies to breed. There is bush there and damp.

5152. That is *palpalis*?—Yes, and probably *morsitans* also.

5153. What is the population of this area? Would it be about 20 miles square, or less?—The Volta River?

5154. Your area.—In which I was working at sleeping sickness?

5155. I understand there are 350 square miles in it; that would be rather less than 50 miles square?—Something less than that.

5156. Have you any means of knowing what the population there would be?—I am afraid not; I can hardly say what it would be.

5157. I asked the question, because I want to know what proportion these five cases you found bear to the total population. We have had figures in other cases, and one wants to know whether this area of yours was less productive of the disease in proportion to the population than others.—I could hardly say what the population would be. It is very concentrated in some of the towns, and then one might go miles and not meet anything at all.

5158. However, you do not know?—No.

5159. Did you ever hear in the course of your communications with natives of any tradition of an epidemic in the past or at any time?—Never; I inquired for that and I could get no information.

5160. I understand you think the small number of cases is due to a certain immunity of natives?—I do.

5161. Have you formed any opinion in your own mind how that immunity originated? Is it because of the slow killing off of the susceptible natives? or take the animals which are immune, what is your theory about the acquired immunity?—I think it is really the survival of the fittest, probably some of the natives who were early infected died and some have been able to resist the infection.

5162. It was a partial resistance, you think, and it became a survival of the fittest?—Yes.

5163. If that is the case you think that immunity would be inherited to some extent from generation to generation?—Yes, I should believe so.

5164. (Dr. Balfour.) Do you think that the transportation of flies by trains would be greater in the case of trains carrying live stock, horses and cattle?—Yes, I imagine it would be.

5165. Have you any experience of that?—I have no knowledge of that.

5166. Have you any evidence to offer as regards similar transport by steamers?—No, except travelling up the Niger; I travelled a good deal there and one certainly gets a large number of flies at certain places on board which travel with you.

5167. How far do they travel?—We kill them as soon as we can, as a matter of fact, but I think probably they would be with you the greater part of the day.

5168. Do you think they would go beyond the area of the fly belt?—I think so.

5169. And they might be carried into new regions in that way?—Yes, I do not think they would travel far.

5170. Do you think it is possible that seagoing vessels would carry them also?—Such boats as go up the Niger might.

5171. Do you think it is possible that they would carry them along the coast from port to port?—That is possible, but I imagine they would probably not go out to sea.

5172. I see that an observer in the French Congo has raised the question of the transmission of sleeping sickness there by sexual intercourse, the point that Koch paid attention to a good many years ago; have you any evidence of that occurring on the Gold Coast?—I never found any evidence of that or anything suggestive of it.

5173. (Mr. Austen.) In the district with which you are acquainted, do you consider that *G. palpalis* is dependent upon big game for its continued existence?—No, in this Volta River district there was very little big game and there were certainly large numbers of *palpalis*. I think it was probably the domestic animals and the natives.

5174. Do you consider that in that district *G. morsitans* is dependent on the big game for its continued existence?—I do not think so; it appeared to me that they took every opportunity of attacking natives and, of course, Europeans.

5175. It has been suggested that the proposed experiment would at any rate show whether or not *G. morsitans* is dependent on big game for its continued existence, but I take it from your answer that in the Volta River district at any rate that question is already answered in the negative, and that the assumption that where *G. morsitans* and big game exist together the disappearance of the big game necessarily entails the disappearance of the fly, is not borne out by facts?—That is my experience in the Volta River district.

5176. Have you found the breeding places of either species—*G. morsitans* or *palpalis*?—No, I hunted a good deal while I was there and I could not find any larvae.

5177. Have you had any experience in trapping flies of either species?—No.

5178. Were you able to make any observations on natural enemies of either species?—No, nothing that I can quote. I looked out to see if I could ascertain anything, but I had nothing to report afterwards.

5179. (Mr. Rothschild.) I have not got much to ask you, but in the district where you worked had you any experience of clearings round villages?—Yes.

5180. Did you notice if the flies did not frequent the clearings there; in other words, whether the clearings of the bush made any difference?—It did make a difference as far as one was able to clear, but it was very hard work. If one could get a clearing to something approaching 100 yards, there was quite an appreciable difference in the number of fly within that cleared area.

5181. In those villages where you say the pigs and goats were kept, were they allowed to go into the bush where the fly was, or were they kept strictly to the cleared areas?—I think they were allowed to go anywhere.

5182. (Dr. Chalmers Mitchell.) When did you come to know of the distinction between *morsitans* and *palpalis*? I take it you are not an entomologist by profession?—No. Before I went out there I had a course in entomology at the School of Tropical Medicine.

5183. Could you tell whether a fly was *morsitans* or *palpalis* without catching it and examining it closely?—No, I did not trust to that; I caught them. One was able to inspect them on one's clothes.

5184. Is there much difference in their habits?—I could tell practically none from what I saw of them, but

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I was not studying them from the entomological point of view, of course.

5185. I understand, and if I may say so, your evidence is a little more interesting from that circumstance. Practically speaking, it was an identification by the methods of systematic zoology, not from the naturalist's point of view?—Exactly.

5186. Following up that it is hardly necessary to ask you this: I suppose the natives would not distinguish between *morsitans* and *palpalis*?—I do not think so; I never found that they did.

5187. The natives, I suppose, knew that bites from these flies were harmful, or did they?—No, they did not appreciate that.

5188. Not even to animals?—No, I do not think so. They are not an agricultural people, at any rate they do not keep herds of cattle. I think those natives who do quite appreciate that the tsetse-fly is harmful to the cattle.

5189. You told us that the cattle were not an important thing there.—No.

5190. So that practically speaking the natives do not associate the presence of flies with disease at all.—No.

5191. Did you yourself notice any difference in the habits of the two flies?—No, I do not think I did, except that I thought I was able to distinguish the bites of the two flies. I could always tell if I was bitten by *morsitans*; I felt it immediately. With *palpalis*, however, I have several times been bitten and perhaps I have moved my arm round like that (*indicating*) and I have found a big spot of blood and a *palpalis* dead, which had been feeding although I was perfectly unaware of it. It seemed to bite more quietly. That is the only thing I noticed.

5192. Was the one fly or the other fly more addicted to watercourses?—Not that I noticed. Of course, this place was very much intersected with watercourses; it is a pretty high forest and there are numerous small watercourses running along it.

5193. So that I gather you would not rely very much upon any distinction—you have no evidence as to any very great distinction between the flies?—No, I have not apart from the systematic examination of them.

5194. You spoke of finding a particular trypanosome. How did you identify it? Did you identify it out there?—I identified it out there by my knowledge of the trypanosome which I had studied at home before.

5195. I understand, but do you mind telling me what precise course you followed? I think you said you thought you found *gambiense*.—Yes.

5196. Do you mind telling me exactly how you identified it? Do you remember?—No, I cannot exactly now; I have not got my notes and I have not worked at trypanosomes for three years.

5197. Did you do it by microscopical examination or by experimental tests?—By microscopical examination.

5198. And not by experimental tests?—No, nothing experimental.

5199. Where did you find this trypanosome, in your patients only or in animals?—In the patients, nearly always in the gland juice.

5200. You did not examine animals for it?—No.

5201. Then with regard to your friend who had been examining it in pigs, his attention was called to the matter because the pigs were dying of an acute disease?—Yes.

5202. It was not that he found it in a stock of pigs?—No, I think not; it was just in these pigs which were sick.

(Chairman.) May I suggest a question which seems to come appropriately now? The words in your proof are "indistinguishable from *gambiense*." I would like to know if there was anything distinguishable morphologically or in any other way.

5203. (Dr. Chalmers Mitchell.) (To the witness.) I gather that it was morphologically?—So far as I know, but I have not discussed the subject with Dr. Gallagher; I have simply seen his report in the Southern Nigerian Annual Report.

5204. But your own identification of the trypanosome was what we may call morphological; it was under the microscope?—Yes.

5205. I would be very much obliged if you would describe briefly when you had a case of sleeping sickness how you diagnosed it? I am not talking now of examining the juice from the glands, but taking the clinical signs.—I think, on the whole, the patients seemed very ill; there were tremors, there was wasting, and there was a certain history of lethargy which might be more or less apparent, and possibly some fever. The history of that being continued for some time, a matter of some months, that generally put me on the track, and then I examined the glands and the blood.

5206. I had the impression (I am not a medical man and I may be wrong) that the lethargy of sleeping sickness is generally towards the end of the case?—Yes.

5207. Had you a history of long illness before? I suppose you could not get a very good history?—You could not, but there is generally a history of some months' suffering. That is as far as the native mind would go back.

5208. Am I right in supposing that most of those cases you had died subsequently?—I heard they all died afterwards.

5209. Fairly soon after?—Fairly soon after, I should think within six months after I left.

5210. So that you would rather expect from what you know that you were seeing the terminal stages?—Yes, with the exception of two young children; they died afterwards, but they were certainly earlier stages than the others I found.

5211. Those were without the symptoms you have described?—They did not have so much lethargy, but they suffered from fever and wasting and illness generally.

5212. Did you try any treatment?—Yes, I tried atoxyl, and it had no effect over there; one could not give it systematically, and I had to be travelling round examining the whole district.

5213. You mean you could not give it systematically, because you did not have the opportunity?—That is so; I particularly had to travel and inspect the whole district and see what the amount of sleeping sickness was.

5214. (Sir Mackenzie Chalmers.) I see you say there is little wild game with the exception of a few small antelopes; what big game is there at all?—Some buffalo, hartebeeste, and antelope.

5215. But very few?—Very few indeed in this district.

5216. Are the smaller antelopes duiker?—Yes, duiker chiefly.

5217. Do you know whether any of those big or small antelopes have been examined by anybody to see whether they contain trypanosomes?—I do not think much has been done in West Africa as far as I know.

5218. Did I understand you to say that there were any cases in Accra itself of sleeping sickness?—I think they have had them there, but they have been chiefly imported from other parts.

5219. Because all the land round Accra is cleared?—Yes, practically cleared; it is not a forest country just there.

5220. So that they were either imported flies or imported cases, you think?—I think they were imported cases.

5221. When you get away from the town are the villages often in thick jungle or always in cleared places?—Frequently in thick jungle; the high forest may grow almost up to the very outskirts of the village.

5222. With very thick undergrowth—jungle?—Yes.

5223. So that it is very difficult to clear round them?—Extremely difficult and to keep it clear too.

5224. You can hardly do it?—It is almost impossible. We have tried it in certain places.

5225. There is only one thing further I wanted to ask you. Did you come across the German Sleeping

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Sickness Commission?—Yes, I went across and saw them.

5226. Do you know what steps they were recommending?—They had themselves a camp which I visited, and they had something like 60 or 80 cases there.

5227. They had segregated them?—Yes. They are very anxious to work in conjunction with us on the Gold Coast, and I drew up a scheme and discussed it with them, and they agreed to it, but I do not know what steps have been taken since. I sent the scheme home.

5228. Have they been successful in their treatment of those cases at all?—They said they were; they were using arseno-phenyl-glycin, and they were getting quite good results at the time.

5229. In the way of prolonging life or actual cure?—I think actual cure; they claimed that cases would come back six or eight months after treatment and they would find no trypanosomes and the cases apparently quite well.

5230. Do you know any other suggestions they were considering for dealing with the disease besides segregation of the sick?—No, I think they are doing clearing round villages, but as far as I know they were not transporting villages.

5231. (Sir William Leishman.) In your investigations could you tell us anything about the comparative numbers of *moritans* and *palpalis* in the districts where you found the two flies?—They were practically equal, I think, as far as I could see.

5232. In any places you visited did you always find the two varieties?—I was in the Volta River district.

5233. Is that country fairly moist the whole year round—plenty of water supply?—I think so, it is forest country and very moist in the low-lying parts.

5234. Did you notice that one fly seemed more associated with water than the other?—I did not notice that.

5235. But there is, I take it, no dry expanse of country?—There is none in the Volta River district.

5236. With regard to the very interesting fact you told us about the greater severity of bite of the *moritans*, how many times did you observe that, roughly?—I should think perhaps I have observed it from 15 to 20 times in the case of *moritans* and fewer times in the case of *palpalis*. Perhaps four or five times I have killed a *palpalis* by chance, which I have not known has been biting me.

5237. So that your general experience of the bite of *moritans* was more painful?—That was my experience.

5238. Did you ever hear anyone else say that?—No.

5239. Did you ever inquire of the natives themselves whether they had been specially bitten by flies—the cases of sleeping sickness I mean?—No, I do not know that I did.

5240. They never told you about having had very severe bites?—No. I do not think they distinguish between the two kinds in the least.

5241. In your own case it was exceedingly painful at the time, I take it. Did it result in inflammation afterwards?—Not more markedly in the one case than in the other, I think.

5242. Do you think the natives are likely to conceal the disease to any great extent?—Yes, I think they are very nervous and suspicious. They always used to run into the bush when I first went there, but they afterwards got more confidence and then they brought out their sick cases. I think when they got to know the doctor they would help him more than they did at first.

5243. Of course you only saw a very small proportion of the inhabitants of the place?—I think towards the end I saw certainly most of the sick.

5244. In this country you visited you were asked about the population; you could not give us a fraction for those you saw?—I should say not more than one-tenth, probably less.

5245. So that it is quite possible that there may have been a greater proportion of sleeping sickness

than you found?—Yes, but I do not think there is anything in the nature of an epidemic.

5246. I have heard it said by men from the Gold Coast that there is probably more sleeping sickness than comes to light. Is that your own view?—Yes, I think so.

5247. Turning now to the question of a comparison between this sleeping sickness which you are familiar with and that we are learning about in Nyasaland, you have not, of course, seen any cases from Nyasaland?—No.

5248. But I suppose you have read about them, and you know the clinical distinctions which have been described?—Yes.

5249. In your experience of the type you are familiar with have you come across any very acute cases which have run a very short time and then died?—No.

5250. What was the average length?—I could hardly say that, because it is difficult to say exactly when the case began, but after I had seen the cases I do not think any of them lasted more than six months. How long they had had the disease I could not say.

5251. Not in any single case could you fix the length of it?—No, I could not obtain the information.

5252. With regard to individual symptoms, were oedemas a common feature in the cases you saw?—No, I saw no oedemas.

5253. You saw oedema in none?—Not in any of them.

5254. Interstitial keratitis?—I did not see that.

5255. No instance at all?—No, of course I only saw five cases the whole time I was there.

5256. Did you not get clinical information about other cases from any of your colleagues?—No, there was no other doctor in the district; I was sent out there specially for it. There had been none there for many years.

5257. At the termination of these cases, did any that you saw die go into a comatose condition?—I did not see any of them die, but from what I heard afterwards with regard to the three cases, apart from the children, certainly one of these cases did become comatose and die. I did not see that myself.

5258. From your own experience of these cases and from what you know and what you have heard about the Nyasaland ones, would you regard those two diseases as the same or distinct?—I should think distinct, clinically distinct.

5259. With regard to the frequency of the trypanosome in the blood that you tested, was it scanty or plentiful?—Scanty rather.

5260. In an ordinary thin blood film would you have to hunt for some time before finding the trypanosome?—Yes; of course, by good fortune you might just light on one in the early part of the examination, and probably you would not find any more until after a quarter of an hour's hunting, or possibly not in a quarter of an hour. One was more successful with the gland juice than with the blood.

5261. Did you examine the cerebro-spinal fluid?—No, I did not.

5262. Did the natives tell you anything about excision of glands as a method of curing the disease?—They said that was how they cured the disease; they said they recognised the disease and were able to cure it in the younger children by excising the lymphatic glands in the neck.

5263. Were you able to get any satisfactory information about that from any native or to see any cases in which that had been done?—Yes, many cases.

5264. Which had scars in the neck where the glands had been excised?—Yes.

5265. Did you examine any of these natives?—Yes.

5266. And found no trypanosomes?—No sign of trypanosomes.

5267. Were you satisfied by your cross-examination of them that they had been cases of sleeping sickness?—I do not think they had been such at all from their description.

5268. You are rather sceptical about it?—Yes.

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5269. The Chairman tells me that you have already gone into that, and that the natives had a special name for it.—Yes, I could not say the name; there are different tribes in different parts and they have different names.

5270. (Dr. Chapple.) Is there much cervical gland tuberculosis with which this disease might be confused?—I never saw any tuberculous glands there, but the condition of dirt in which they live is rather marked; the children have *pediculi* and sores in their mouths and on the head, and so on, and there were also cases of inflammation of the glands. They had got quite a number of cases of the glands inflamed and enlarged, and I imagine when these people thought they had cured cases of sleeping sickness by the excision of the glands it was such cases they had been treating.

5271. They were evidently old cicatrices?—Yes.

5272. Did you find the flies, both *palpalis* and *morsitans*, in and around the villages?—Yes.

5273. Are there cattle?—Very few cattle there.

5274. Are there any cattle at all in these villages?—I cannot remember that there were—chiefly goats and pigs.

5275. Do they die?—I do not think so. There are sheep, too, of course, but not much cattle.

5276. Do none of the domestic animals ever get trypanosomiasis in those villages?—No, they do not suffer from it, so far as I know. I did not examine any sheep or goats, however.

5277. Are there dogs?—Yes.

5278. Would the fact that these domestic animals lived there at all not indicate that there was no trypanosomiasis communicated to them from these flies?—I should think they probably became infected with trypanosomiasis, and were immune to the disease. They probably would show trypanosomes in their blood.

5279. Do you know that for a fact?—No, I do not; I did not examine them.

5280. Do you know of any cases in which trypanosomes have been found in the blood of domestic stock, although they have shown no symptoms of the disease?—I cannot quote any case from my own knowledge.

5281. Are there any districts of which you are aware in that locality which are known to the natives as fly districts and therefore dangerous to stock?—I think the whole place is really a fly district; you can hardly say that one place is more full of fly than another in the Volta River district; almost in every place there is fly.

5282. The natives seem to have no dread of fly districts?—No. They do not have droves of cattle. They may have a stray cow occasionally.

5283. Still they have some domestic animals which are susceptible to trypanosomiasis apparently?—I do not know that they have; I did not see them there.

5284. Have you examined any of these flies to see whether they were infective or not?—No.

5285. Do you know of any locality where there are fly and disease but no game?—There is practically no large game in this Volta River district; there is some very small game, and that is the nearest I can quote to that.

5286. Still there is none you know in which the game is entirely absent?—No.

5287. Do you see any danger from the spread of the disease in the Volta River district, or do you think it is of little consequence?—I think it is of little consequence there except as regards the infection of Europeans, perhaps, who might go there.

5288. Do you think Europeans are more susceptible than natives or less so, or is there no difference?—I think they are more susceptible.

5289. To trypanosomiasis?—Yes.

5290. Do you know of any cases of trypanosomiasis in whites in your district?—I have met cases, not from this one district, but I have known two cases at least of Europeans who have suffered from trypanosomiasis, one from the Gold Coast and the other from Southern Nigeria.

5291. Who have suffered and recovered?—And recovered.

5292. Did they recover under treatment?—Yes.

5293. What?—I believe atoxyl in both cases, but I am not sure in the second case, because I think there was some antimony treatment, too. He was in the School of Tropical Medicine at Liverpool.

5294. Have you tried salvarsan?—No, I have not.

5295. Do you know of its being used?—I have heard of its being used, but I have had no experience of it.

5295A. Do you know with what success?—Very little, I believe, so far as I have heard.

5296. You answered Mr. Buxton with regard to immunity. Did I understand you to mean that susceptible infants and children died off with the disease, and left only those who were insusceptible?—Yes, partly insusceptible; they were susceptible, but survived; that is to say, they had sufficient powers of resistance in them, so that they did not die but survived.

5297. Do you think that infants then are more susceptible than adults?—I should think so.

5298. And that those that are not only susceptible to the disease, but perhaps are susceptible to it in a virulent form, die off?—Yes.

5299. And leave only those who recover?—Yes.

5300. Do they recover from the presence of an antitoxin being set up, or from hereditary immunity?—I imagine hereditary immunity has a good deal of effect in those cases.

5301. Have you had experience of any other district in Africa where sleeping sickness prevails?—Yes, I have seen at the top of the Northern Territories in the Gold Coast occasionally a sporadic case occurring, but nothing of any importance.

5302. Do you think there is any possibility of doing anything in a preventive way to cope with the disease or not?—It could be done, but I am not quite clear that it is really worth the trouble; it depends on how far it is incumbent on us to work in conjunction with the Germans, who are working in Togoland, which is co-terminous with the Gold Coast.

5303. Have you any suggestion to make of a practical kind which would aim at preventive measures?—I drew up a scheme in which I suggested that the natives should be examined at the borders when crossing between Togoland and the Gold Coast, and that those should be given a certificate that they had been examined, and they should then go on to the next post on their round and be examined there, or their certificate countersigned, and that this thing should be available for a month; so that one would be able to trace the movements of these people and keep them under observation, segregating and treating all cases of the disease. I drew up a full scheme and sent it in.

5304. That is a scheme which applies to that particular district only?—More than that—to the whole of the country between the Gold Coast and Togoland along the Volta River.

5305. Would you treat all cases of sleeping sickness found coming into the district where these flies exist, lest the infection should spread?—No, I do not think so, because they have the infection brought there in large quantities without having a serious effect on the people.

5306. Still you think the effect would be more dangerous to blacks than to whites?—I do not think the introduction of one fresh known case of sleeping sickness would make it very much more dangerous. It is simply that the scheme was drawn up, as I was told in my instructions when I went there, with the possibility in view of working with the Germans.

5307. Do you think that the spread of settlement then is not in any way interfered with by the existence of this particular disease?—I do not think it is.

5308. (Chairman.) There is just a question or two I have to ask you. You said the country was full of small streams, but is there any bush country upland at any considerable distance from any water?—Not there, I think, because there are so many small streams; there are constant rivulets.

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[Continued.]

5309. Have you formed an opinion with regard to the habits of *palpalis* as to the distance they will move from water?—I have not any exact idea as to how far, but I think it moves within a quarter of a mile roughly of the water.

5310. Can you think of any part of your district where it was more than a quarter of a mile from any water?—I do not think there is any place more than a quarter of a mile from water.

5311. You spoke about the fly being round villages; do not the natives of that district clear round their villages?—No.

5312. They let the bush come right up to the village?—Yes.

5313. Have you seen villages where there was much clearing round them?—Not very much clearing, but a certain amount.

The witness withdrew.

Mr. MERCIER GAMBLE, M.D., B.Ch., called in and examined.

(He submitted some photos and a map.)

5318. (Chairman.) We are very much obliged to you for attending here to-day. I understand that you are connected with the Baptist Missionary Society, and that you have had experience on the West Coast?—Yes, at San Salvador, Portuguese Congo.

5319. As I understand from the proof you have been good enough to send to us, you have seen a good many cases of sleeping sickness yourself?—I have.

5320. What is the fly in that area?—*Palpalis*.

5321. You know, of course, by reading what has happened in Uganda. Has any experiment of that kind been tried where you were, that is say, moving the population back a certain distance from the river?—None whatever.

5322. They still inhabit the neighbourhood of the water and fish in the water all the time?—Yes.

5323. Is there much sleeping sickness there?—There is a good deal.

5324. Can you give us any idea at all of the population in that district?—25,000.

5325. Can you form any idea of the number of cases which have been reported, say, in the last two years?—I have examined 408 people for sleeping sickness.

5326. That is 408 separate cases?—Yes.

5327. All identified clinically as sleeping sickness?—No; 230 were positive results out of 408 persons.

5328. In how long a period is that?—From October 1910 to December 1912.

5329. That is just over two years?—That is so.

5330. The figures here are a little difficult to follow, as they represent consultations really and not cases. You put the total for 1911 at 3,799, and for 1912 at 5,269 attendances.—Those are attendances for treatment.

5331. Those would include all the attendances in every case of sleeping sickness, and the examinations for cases which turned out not to be sleeping sickness, would they not?—No, those are all attendances of the sleeping sickness people for treatment.

5332. You have given us the numbers, and that is the material point. Do you think there are many cases you do not hear of, or that the natives are willing to come to you and others when they are ill?—There are no others; I am the only one. There are 25,000 people in the district, and it takes eight days to cross one way and seven days to cross the other, and I was the only medical man there as far as I know.

5333. Then you must have the largest practice of anybody in the world, I should think.—When they come to me they come voluntarily, and they submit to examination, and a certain proportion stop the whole time. Out of the 230 I have already mentioned 199 stayed with me for various periods of treatment, and some of them for 12 months.

5334. What I wanted really to ask you was this: Do you think, speaking generally, that the natives when taken ill would desire to come to you, or that there would be any desire to conceal the disease if they had it?—Those who were not under our influence

5314. Have you seen clearings sufficiently to judge whether fly will or will not come into a cleared area?—Yes, it depends rather on the width of the area, but clearing certainly acts as a deterrent to the fly, a width of 50 or 60 yards.

5315. Do you think if you had a large area cleared you would find any fly at all? Apart from the spread of the fly, would you find fly really at all in a large cleared area?—You would find it much more rarely, but it follows people across a cleared area.

5316. I understand that, but apart from the flies which follow an actual person on his clothes or on himself in some way, would there be fly which would come from the bush into a large cleared area?—They might occasionally have a stray fly, but I think it would be practically clear.

5317. I gather you have been bitten very often?—Yes.

as a missionary society might go to the native doctors, and they put pepper in their eyes to keep them awake.

5335. Does it not come to this, that there must be many cases in that district of which you never hear?—Certainly.

5336. Do they themselves recognise sleeping sickness as a separate disease?—Yes, they do.

5337. They have got a name for it?—They have.

5338. And treatment of sorts?—Yes.

5339. Is there much mortality from other diseases in the part of that country which you know?—No, it is the most severe chronic disease, that is to say, it shows the highest mortality from a chronic disease. Pneumonia would show the highest mortality from an acute disease.

5340. I am not sure that I appreciate that answer.—I should say that of diseases which run a long course sleeping sickness gives the highest mortality.

5341. That is in proportion to the cases, or is it all the cases?—All the cases in the country.

5342. I want to be clear about that. Do you mean that if you were asked what was the principal source of mortality in your district you would say sleeping sickness?—Only with the modification of chronic diseases, because pneumonia causes the most deaths of the acute diseases. The two principal sources of death are pneumonia and sleeping sickness.

5343. Malaria?—No, no deaths as far as I have seen.

5344. Have you studied the habits of the fly at all?—No.

5345. Is there much game in the district to speak of?—I do not think so. I have brought some photographs for you to see, and the grass is 15 feet high with us and sometimes 20 feet, and it is exceedingly difficult to find game; the natives very seldom bring it into the town.

5346. Are there many of the larger mammals?—There are buffalo.

5347. Many?—I cannot say about many; I do not think so. They very seldom bring them in. There are roan antelopes and horned antelopes and an antelope which has horns something like the South African koodoo—I do not know what that is.

5348. Are you an expert on trypanosomes?—I know nothing about them except in diagnosing my cases.

5349. You would not be able to say anything about trypanosomes in wild game?—I have never been near a wild animal, and I very seldom see them; the water-buck is the other one.

5350. (Dr. Chapple.) Is the fly very numerous?—Not in San Salvador, but in the district it is—not in the town itself where I have been. I have got photographs there to show how it extends 80 miles to the south and 100 miles to the east. The photographs show the rivers on which I found it.

5351. Did you find the fly confined to water-courses?—Yes.

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5352. And not to be found far from them in the bush?—I have not travelled very much, and my travelling has been fast from one station to another in a case of illness. By "fast," I mean 90 miles in four days. So that I have only seen the flies just in passing. I have not been wandering round the country; I have been 14 months at a time sleeping in my own bed in my own house.

5353. Do you know of any locality where the fly is found without the presence of game?—I cannot say, I do not know.

5354. Have you any experience of *morsitans* at all?—No, I have never seen one. Of all the flies that I send to South Kensington none have been identified as *morsitans*.

5355. Have they been identified as *palpalis* in each case?—Invariably.

5356. Do these cases of sleeping sickness you saw all follow an approximately typical course?—Yes.

5357. And are all chronic?—No, I have seen some acute cases.

5358. How acute?—As far as I know, they have died rapidly after they have been identified as sleeping sickness; that is all I can say.

5359. You could not say how long it had existed before you saw it?—No.

5360. Is there nothing to indicate, in the clinical signs, how long this disease has existed?—Not that I can speak of. The case I am referring to is a man who died in 11 days after I had punctured one of his glands, and found it swarming with trypanosomes.

5361. Are people of all ages equally susceptible?—Yes. I have some children of 27 lbs. in weight under treatment, and also 33 and 36 lbs. There is a photograph there of a boy who weighs 42 lbs.

5362. Does it seem to follow a more acute course in children than in adults?—I do not think so.

5363. Do you think that all children are, more or less, affected at some time?—No, I have no reason for thinking that.

5364. Do you think that all who are infected show signs of the disease?—I have got one case which, in 1909, received five successive doses of four grains of atoxyl. That is a woman who is alive and well—and so well that she got married in May 1913. I have another one who had 50 grains only of atoxyl, and she is doing fairly well. These were all cases which refused treatment; and then I have followed them up and found out that they were still alive.

5365. Do you find any cases which recover spontaneously?—No.

5366. Do you think that in cases where no treatment is given the disease is invariably fatal?—No, I think it quite possible that some have recovered. For I remember the history of some of the early cases of whites before Dr. Broden and Sir Patrick Manson were on the track of trypanosomes in the Congo.

5367. Do you think there is any possibility of children getting a mild infection and thereby acquiring immunity?—Can I answer your question in this way? that I have only found one case with trypanosomes during treatment and that case died.

5368. What do you infer from that?—That they die.

5369. That is an obvious inference?—I mean to say that they do die if they are infected except in these rare cases that recover. I have not heard of a case having trypanosomes in the blood and going on as a healthy individual.

5370. Take an analogous case. We know that there are many people who are immune from typhoid because they have contracted it early in life and acquired an ontogenetical immunity; do you think it possible that the absence of epidemics in many of these cases may be due to the fact that everyone has got more or less inoculated some time or another with a small dose of the trypanosomes sufficient to be overcome by the native resistance of the body and then enjoy immunity thereafter?—I do not think so, because I think I should have fewer cases coming to me for treatment if that was the case.

5371. Have you had any experience, either personally or by reading, of any heightened susceptibility

of whites to sleeping sickness?—I have only diagnosed one white man as having sleeping sickness. There is a very small white population where I am. He came from 100 miles to the east.

5372. Did he recover?—He is under a professor at Lisbon. I do not know whether he has recovered or not, but I heard that he was still alive.

5373. Have you often been bitten yourself?—I have been bitten only once.

5374. Do you take any personal precautions against being bitten?—I live in a mosquito-proof house; that is the only precaution. There are very few tsetse with us.

5375. And then, when you are travelling at the rate of 90 miles in four days, do you take any precautions?—I sleep under a net at night.

5376. And what precaution during the day?—None.

5377. Is there anything you can apply to the skin to keep them off?—I have not been in a district so badly infected that it was necessary, or at least, that made me think of it. We have citronella oil on the station, and the ladies use it for keeping off the mosquitoes and the other small biting insects.

5378. Are the natives themselves afraid of the bite of the fly?—They hardly appreciate that it is the cause of sleeping sickness, but I have been trying to teach them.

5379. Is there any trypanosomiasis amongst domestic stock?—I cannot say. No cattle will live with us. We have tried donkeys and mules, both from the Canary Islands district and from the Congo Belge, but they have invariably died. The cattle live 150 miles in the interior to the east, and they are brought down and killed soon because the natives realise they cannot keep them. Sheep barely exist with us; goats thrive.

5380. Are they native goats?—Yes.

5381. Not imported goats?—One man has imported them from the Canary Islands.

5382. Have they lived?—They have not thrived.

5383. When they die do they die of trypanosomiasis?—I could not say.

5384. Do the natives know themselves that the stock cannot live in a fly district?—I do not know that they associate it with flies.

5385. Have you tried salvarsan?—No, I have not, it is so expensive.

5386. Has it been tried, do you know?—Not in my district. It has been tried by the Belgians.

5387. Is there much tuberculosis, or any?—Very little.

5388. Is there much syphilis?—Yes, but not primary; it is all tertiary, I think.

5389. It must be primary in some cases?—I see very few of those cases. The sort of syphilis we get is the big ulcers from the size of a cheese plate to a soup plate.

5390. Do you see much hereditary syphilis?—Yes, I do, all the hard palate gone.

5391. (Sir William Leishman.) How long have you been in that part?—I went out in October 1907, and stayed two years. I returned in 1910 and have stayed three.

5392. Was sleeping sickness in full blast when you went out there first?—Yes, I believe it has been there since 1882; I have evidence from 1882.

5393. Is it increasing or steady or falling?—I think it is steady.

5394. Do you still see as many fresh cases as you did two years ago?—I am seeing more.

5395. Because you are more popular; is that it? Can you diagnose by puncturing the glands?—I do.

5396. Do you put much faith in the enlargement of the glands? In what proportion of the enlarged glands you puncture do you get trypanosomes?—There were 408 people I suspected.

5397. Because of their glands?—Because of their glands. There would be certain of the people I have seen in whom I thought it was due to other diseases, dirty heads, and the usual thing. Of those 408 cases, 230 were positives, although I made 513 examinations of those 408 people.

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5398. That is, a little more than half of them were positive?—Yes.

5399. As to the symptoms of the disease did you ever see interstitial keratitis in any of your cases?—No.

5400. You never saw a case at all?—There was one man, but I did not think that was anything to do with it.

5401. Are the oedemas common?—Yes, oedema of the face is common.

5402. Any other part of the body?—Not particularly, except in the advanced cases, when they look something like cases of chronic Bright's disease, very heavily swollen all over—hopeless cases.

5403. Were you able in any of your cases to ascertain when the case started?—When they begin to notice symptoms, if they are the intelligent people, I can get a fair history of the disease, but in the early stages that precede the symptoms it is impossible.

5404. In such cases as you get information about on that point, what was the average duration of the cases from that time till death or recovery?—I should say three years.

5405. You mentioned one acute case; have you seen any other acute cases besides that?—Very very seldom.

5406. Any in which you could be sure that the course has really been a short one?—None, because I do not know when they started.

5407. In the last stages of the disease do you usually get comatose symptoms?—No, I do not.

5408. That is not common?—In some cases, but it is not usual. Some die suddenly. I have had one case of one day's headache and then death.

5409. Do you get much mania?—No, very seldom.

5410. You have heard about the clinical features of the sleeping sickness which is now being found in Nyasaland?—Yes, I have.

5411. Contrasting that with your own experience of Congo cases, would you regard these as the same disease or varieties of the same disease or different diseases?—I should call them different diseases, because I have got what I think are certainly apparent cures which are not occurring in Nyasaland, where it is invariably fatal.

5412. Would you found your opinion on anything else except the fact that the one is always fatal and the other sometimes recovers?—No.

5413. That is your ground?—Yes, that is my ground.

5414. Do the natives ever speak of having been severely bitten by tsetse-flies? Do the cases of sleeping sickness speak of that?—No, I do not think a patient has mentioned it.

5415. You mentioned in your summary that you have collected a large variety of blood-sucking arthropods.—Yes, 50, and over.

5416. Did you study them in any way yourself?—Just a little, and then passed them on to the Imperial Bureau of Entomology.

5417. Do you happen to know which were the chief ones, generally?—The *Tabanus congolensis* and *claripes*, with *Stomoxys*, *Stegomyia*, and *Culex*.

5418. Is *Tabanus* common?—Both *Tabanus congolensis* and *claripes* are; those are the only two which are really common with us. The others are a rarity, one in a month.

5419. Does the *Tabanus* bite the natives?—Yes, they tell me so.

5420. Do you think it is a painful bite?—Yes, they tell me so.

5421. Have you any reason to suspect it is in any way connected with trypanosomiasis either of man or stock?—I have no reason for thinking so, but I do not see why it should not prove to be so ultimately. I have a list here.

5422. You need not give us that. Are ticks common?—Some of them are, on the dogs especially.

5423. Are human beings bitten by ticks much?—Not with us, but at Kibokolo-do-Zombo, 100 miles to the east of us, they are fairly common.

5424. Have you any tick fever there?—I have never heard of one of our white people having that fever.

5425. (Sir Mackenzie Chalmers.) The only treatment you have tried is atoxyl, I think?—I use atoxyl, soamin, tartar emetic, and by the mouth perchloride of mercury, urotropin, or one of its synonyms and arsenic.

5426. Did you ever get unpleasant symptoms?—Some people are very intolerant of arsenical treatment. I have had *herpes zoster* several times.

5427. No dangerous symptoms?—Eye symptoms are the chief dangerous ones.

5428. We were told by a witness the other day that there was great hope of a treatment by intravenous injection of antimony; have you heard anything about that?—I have been practising that since January 1912.

5429. Pure metallic antimony?—Not metallic.

5430. That is being tried now; you have not heard of that?—No.

5431. I was going to ask this question: Assuming that to turn out to be a successful treatment, is the technique very difficult or of any?—I do not know anything about metallic antimony.

5432. Is the technique of intravenous injection difficult, or would anyone in Africa be able to do it?—My record was 10 in 40 minutes, but it needs a little practice, and smart assistants.

5433. That is at the end of the practice which makes perfect?—I had to do 36 in one day.

5434. Intravenous injections?—Yes, when I ran short of atoxyl.

5435. We were told by another witness that on the Gold Coast the natives have a theory that excision of the glands is a cure, and that he found a good many cases where there were cicatrices in the neck. Is that idea prevalent where you were?—I have read of it, but I have never heard of it on the Congo.

5436. You did not find it amongst the natives?—No, I have only heard of it from further north in West Africa.

5437. Have the natives any specific name for the disease?—Referring to the sleeping stage—*mekwende* or *manimba*.

5438. Which means to sleep?—Yes, unnatural sleep.

5439. In your part of the world they recognise it as a distinct disease, do you think?—Yes.

5440. Could you get any history from them as to how long they have known it?—No, I could not, except that I know it was there in 1882. I have not had that from the natives.

5441. I was wondering whether any of the old natives would talk about it and say that they remembered it in their childhood.—No.

5442. Do you think it probably has existed there for generations?—In association with 1882 I heard that there was an epidemic of it and that it came from the south-west. Whether it existed before that time or not I do not know. The district I am referring to was hardly occupied before 1878.

5443. Has it been repopulated?—I mean occupied by Europeans; it has an old civilisation, but Europeans have occupied it since 1878.

5444. The native treatment, I think you said, consisted of pepper in the eyes?—Yes, and there are other things.

5445. Various necromantic proceedings, I suppose?—Yes, and leaves.

5446. Have you made any *post-mortem* examinations?—Only one.

5447. Have the natives any objection to it?—I think they would have, for I have not been there long enough.

5448. I was wondering whether the *post-mortem* examination showed at all what was the proximate cause of death, whether it was some toxin or whether it was mechanical blocking of the veins, or what?—I should think it is the effect of the trypanosomes on the brain.

5449. The toxic effect?—Yes, and the changes there.

5450. (Dr. Chalmers Mitchell.) I think I heard you say you contrasted sleeping sickness as you know it with a disease like pneumonia, calling the first a chronic disease and the second an acute disease. I wish you would tell me this: Do many of your sleeping sickness

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patients die of an acute disease? Do they pick up and die of an acute disease?—No.

5451. Do you think that is because they are protected in any way?—No, I do not think so.

5452. About how many cases do you think you have had of sleeping sickness? You have had many hundreds, have you not?—In this last period?

5453. Yes.—199 submitted to treatment.

5454. That is quite enough, thank you; but among those 199 cases I gather you have never had a case of death which you did not attribute to sleeping sickness?—That is so.

5455. You say that your chief method of diagnosis is by the puncturing of the glands and getting the trypanosomes?—It is.

5456. How do you identify the trypanosomes?—By the microscope.

5457. What power do you use?—A sixth.

5458. Do you simply try the blood film?—Fresh gland juice.

5459. You have not had time to make yourself an expert in the different kinds of trypanosomes?—No, I have not.

5460. You simply diagnose it as a trypanosome and see the trypanosome?—Yes, and I make a point of not treating anybody until I have diagnosed it.

5461. (Mr. Rothschild.) I have only one question to ask: Have you found in going round the country in any of the villages or centres where the country surrounding has been more or less cleared for cultivation or other purposes that the fly is absent?—I do not think there is any part where it is clear.

5462. That is to say, that the bush comes right up to the villages?—Yes. There are elaborate regulations by the Portuguese which I showed to Dr. Bagshawe at one time, but they are still regulations.

5463. The natives do not pay much attention to them, you mean?—There is no means of putting them into execution whatever, financial or otherwise.

5464. (Mr. Austen.) It was not from your district, was it, that natives were introduced into Principe Island to work on the cocoa plantations?—Close to it, and now they are recruiting in the town. They were recruiting 12 months ago for the coast towns which supply the islands, and to replace those men they have been recruiting with us by force. They say that forced labour is legal, and the last mail I received news that they were asking for 1,500 to 2,000 men, women, and children, perhaps men with their women and children, to work on the islands.

5465. In your district?—In the town itself.

5466. Are any steps taken to ensure that the people who are sent to the islands are not suffering from sleeping sickness?—If there is, it is done nearer the coast than where I am. I have not seen a medical officer there doing sleeping sickness work at all.

5467. Are pigs kept in your district?—Yes, in the district, but not much in the town; that is merely a social question.

5468. They are kept in the district where sleeping sickness occurs?—Yes.

5469. And you told us that goats thrived?—Yes.

5470. Have either of those animals, pigs or goats, ever been examined to discover whether they may serve as reservoirs for trypanosomes?—I do not think so; I never heard of it.

5471. Would it not be worth while (in this question I am not casting any reflection on you) as a general measure?—Most decidedly; there are a good many other things which ought to be done there, but I am the only man, and I have a good deal of work to do.

5472. It is quite possible that they are acting as reservoirs, you think?—Yes.

5473. You are aware of what has been stated with regard to Principe?—Yes, I believe they have done a good deal of slaughtering there.

5474. But no measures of that sort have been taken in your district?—No.

5475. You told us there was very little game?—I think that is a fair way of putting it.

5476. So that you would not say that in your district *G. palpalis* is in any way dependent upon big game for its continued existence?—That follows from

what I have said—yes—but I have not travelled sufficiently really to be able to speak on that point. I am sitting down, as the natives call it, in San Salvador, working morning, noon, and night at the dispensary, 28,000 attendances in a year.

5477. So that you really cannot say definitely from your own experience and observation whether the fly in your districts subsists so largely on the blood of natives that the blood of game is a factor of small importance?—I do not think I have had the experience to answer that question. I do not think there is much game, because of the food that the natives have. They eat white ants, and if there was enough meat, they would not bother with white ants. They are hunting day after day, and bring in nothing. There is always a hullabaloo if anybody has shot something. The game must be rare for them to make such a fuss about it. That is why I say it is rare. Then I know about the grass being 15 feet high. I have heard of Mr. Larsen. I have met him. He is a professional elephant hunter, and he has left the country in disgust, because the grass is so high that the elephant is on top of him before he can shoot it, and then it is like shooting the roof of a house, and the bullet slides off the elephant's forehead. I have had a number of people brought into the hospital tossed by buffaloes, and some shot by their friends, and it is all due to the long grass. The buffalo has a habit of dodging round to the old track and waiting in the long grass, and when a man comes along following it, it tosses him. The buffalo waits alongside the path he has already travelled. That would not be possible if the grass was not long. Then, again, the way they hunt game is that they set fire to the grass, and as the fire approaches the game they stand on each side of the valley and fire. They miss the animal, as a rule, and hit each other on the opposite side of the valley, and then come into the hospital.

5478. And that is where you come in?—Yes, that is where I come in, and that is why I say game is scarce. They do not bring it in sufficiently often, nothing like what we hear of with regard to South Africa. I have seen only two antelope, I think, in the little travelling I have done, and I have not seen a buffalo or an elephant. I have seen very little game. When I travel I travel with a company, and I have not had the opportunity of seeing, perhaps, but still, I am not a hunter.

5479. Am I right in thinking that, although game is apparently scarce (we will leave it at that), *G. palpalis* is not scarce?—I think not.

5480. Is it plentiful?—Yes, I think so.

5481. Have you ever found it in this elephant grass of which you speak?—No, except near the rivers.

5482. Always in shelter of a woody, bushy nature?—Yes, except for the occasional ones following people who come up to San Salvador, perhaps six in two years.

5483. You have told us already that you have not made any entomological investigations into the life history of the fly; you have not time?—No.

5484. So that it is no good my asking you whether you have found their breeding places?—I have not found them. The nearest thing to that was at Kibokolo, to the east of San Salvador, where I noticed a number of tsetse in our houses, so I went to the rivers and inspected them; on one stream there were very few, and on the other stream there were a great many; and I recommended that the trees should be cut down. I have not heard the result of that—whether there was any benefit, but that is the nearest I came to them.

5485. Have you a marked dry season?—We have, from the 24th May to the 5th of October.

5486. Have you at any time observed anything which would seem to indicate that in the dry season *G. palpalis* retires to certain centres, from which it spreads out again when the wet season returns?—I think that applies to all flies at San Salvador, not only *Glossina*, but especially to the *Stomoxys*, and also to the *Tabanidae*. It is in the wet season, from October especially, that the *Tabanidae* come in, but still they continue on until May. I wondered if it was that the

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floods really did some damage to the fly in the later seasons.

5487. Have you actually observed such centres?—No, it is a hilly, undulating country, like a gigantic Salisbury Plain, and we are just on the top of one of the hills, so that there is no water close to us, and there is no water resting there half an hour after a thunderstorm.

5488. If a proper entomological investigation were undertaken in your country from that point of view, do you think it would be possible to find these centres if they existed?—Yes, I do.

5489. And having found them, would it be possible to deal with them by clearing and burning them?—If the money were available, but the country I am in—

5490. That is another matter, of course. Sir William Leishman has already asked you practically this question, but I would like to ask you again: Have you any reason to think that sleeping sickness is conveyed in any other way than by the bite of *G. palpalis* in your district?—I have no reason for thinking so, but there are flies there, especially *Stomoxys*, which possibly might do it.

5491. You cannot say whether the existence of sleeping sickness in any way coincides with the area of distribution of *G. palpalis*?—No, I have not travelled sufficiently.

5492. (Dr. Balfour.) I notice that you used soamin as well as atoxyl in the treatment.—Yes, I do use them alternatively.

5493. How does it compare with atoxyl?—I think they have the same effect; I see no difference provided they are both fresh.

5494. One is no more beneficial than the other?—I have not noticed it, but I have not treated any single patient with only one. I alternate them.

5495. Do you think you get a better effect by the alternative treatment?—I try to; I think so.

5496. You lay great stress on the drug being fresh?—I do.

5497. Where do you obtain your drugs—from England?—The Belgian Government have been very good to me. I am living in Portuguese country, but the Minister of the Congo gave me permission to go to the Tropical School at Leopoldville, and, as the result of that, I have had donations of atoxyl sent to me, and just recently the Portuguese have begun to send it to me. The Governments do not supply me with sufficient.

5498. The atoxyl comes from Europe, and arrives in perfectly good condition for you to use?—Yes.

5499. It is perfectly safe to use?—Yes.

5500. Have you ever had cases of blindness resulting from its use?—Yes, that was when I returned in 1910; I had several cases, and I am inclined to think that the atoxyl had been kept in Africa for some time, and certain of the bottles I rejected because the drug was brown. I had about six cases of eyes affected about February 1911.

5501. Do you think it is light or heat which chiefly affects the drug, or both?—The heat, I should think.

5502. Do you keep it in the dark?—Yes, invariably I keep it in the dark; I cannot change the heat. Taking those bottles which came to me, they had been kept in boxes, and therefore it would be heat more than light which would affect them. I keep it in the dark, wrapped up in its original paper, and put it in a cupboard.

5503. The antimony you have been using recently was, I suppose, probably tartar emetic?—Yes, it was.

5504. Did you find that effective also? What made you use antimony in preference, let us say, to atoxyl or soamin?—The shortness of the atoxyl.

5505. Only that?—That was what started me, and since then I have been using it alternately with atoxyl, sometimes giving them in alternate months, giving a patient two grains two days a week for a full month, or sometimes dividing the month and giving half atoxyl and half antimony.

5506. Are you pleased with the mixed treatment by atoxyl and antimony?—I am. There were 65 people received treatment in 1911 and 38 are going on nicely;

there were 103 in 1912, and there are 81 going on all right.

5507. That is for over a year?—Yes, and in October and December 1910 (I had not tried antimony then) of 31 there are 20 all right.

5508. Might I ask you whether you use urotropin?—Yes, I have been using it since January 1911.

5509. What led you to do that?—Because we are told it passes into the spinal canal.

5510. That is why you used it?—Yes, I used it when I had to leave the station and the nurse was left behind. I had prepared it on purpose and left her with this so that she could go on with the treatment, and I have always either that by me or perchloride of mercury, or arsenic, to keep the people secure if I happened to be called away professionally or in periods of rest. I began when I first went out in 1909 to give them perchloride.

5511. You did not give them urotropin at the same time as your trypanocidal drug?—No, I have not tried that.

5512. Have you given urotropin as a trypanocidal drug? Do you know of its having any trypanocidal action?—No.

5513. Have you ever noticed any difference in the trypanosomes when you have given it?—I have only once found the trypanosomes in the patients when I have given the atoxyl.

5514. You have never given urotropin alone?—No.

5515. So that you cannot say whether when given alone it has any action on the trypanosomes?—No.

5516. You get advanced cases in the third stage of the disease?—Yes.

5517. Have you ever tried intraspinal injections of serum from your treated cases?—No.

5518. Have you found anything to benefit these cases in the third stage?—I have not.

5519. They always die, I suppose?—I have done spinal puncture with those which seemed to be not getting better and they have all died; there has been leucocytosis and they have invariably died. I have not done it in cases which I thought were getting better, but in cases which I thought were getting worse.

5520. You say that the disease in Angola has been present from 1882?—Yes.

5521. Do you think it was probably there before that date?—I cannot say.

5522. There is no history that it was introduced from outside?—There is a rumour that it came from the south-west, an epidemic from Loanda district.

5523. Then I suppose you know nothing of the history of the tsetse-fly in the area? I suppose the fly has probably always existed there?—I should say so.

5524. The natives have no information regarding it?—They have got a name for it and they have got a name for *Tabanus*, and the intelligent native will distinguish between the two, but the unintelligent one says it is a fly.

5525. There is nothing to show that the fly and the disease came together?—I know of nothing.

5526. (Mr. Buxton.) I understand you to say that in your opinion the game is not very numerous?—That is so.

5527. If they were numerous would you not, although you could not see the animals themselves, see game tracks?—Yes.

5528. Did you look about for game tracks?—I did. I have seen very few. The main tracks have been on the frontier river between the Portuguese Congo and the Belgian Congo. There is a photograph of the river in that little book. That is the place where I have seen most of the tracks, but very seldom at other parts. There are sometimes the tracks of buffalo on the hilltops, but you see that the roads would keep to the more healthy places and the drier parts of the ground, whereas the buffaloes are down in the swampy long grass. In the early morning we might see them, but our travelling time, as a rule, is not that when the animals are moving about.

5529. You do not, as a matter of fact, see them?—I have never seen a buffalo or an elephant, and only two antelopes.

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Mr. MERCIER GAMBLE, M.D., B.CH.

[Continued.]

5530. (Dr. Bagshawe.) I am not sure if you have told us already what the population of your district is?—25,000; it is a district eight days the one way and seven days the other.

5531. How many square miles would that be about?—Twenty miles a day is good walking.

5532. Is it a dense or a sparse population for tropical Africa?—It is the only part of tropical Africa I know.

5533. You have no standard of comparison?—No.

5534. How many deaths from sleeping sickness do you think occur in a year in that population, not only in your own hospital but in the whole district? Could you give any estimate of the numbers?—No, there is no Government work; we have tried to do it through our evangelists bringing in the numbers, but the information is not such that I could tell you. I do not know. The population is only calculated from the hut tax, and the hut tax is not universal.

5535. You could not give that information for any other disease; you could not say, for instance, how many deaths from pneumonia there would be in the year?—No. I do not know what is going on; and a man dies and is buried, and that is the end of it. The Government there is only in its infancy really, things are not done as they might be done.

5536. You could not say what is the principal cause of mortality?—The principal cause of mortality in the district is sleeping sickness, I think.

5537. Among your 199 cases what proportion was there of children under 12?—Out of the 1910 series there are five children well, one ill, and four died. Out of the 1911 series there are 13 children well, two ill, and six died.

5538. I mean as compared with the adults, what are the comparative numbers? Would half be children, and half adults, or would the adults out-number the children?—The adults out-number the children.

5539. By much?—It is more likely to be one-third of children, one-third of men, and one-third of women,

that is about it, so that the women and children are greater than the men.

5540. You think they are all equally liable to infection?—Yes, except that now the men are getting better clothed than the women, and are more protected. The women and children go to the farms, they are the gardeners of the country, and there again they are more exposed because the farms are frequently near the water, although not always. In passing to and from the farms they are exposed. The farming of the women and the clothing of the men gives the women and the children a worse chance. The children are taken by the women to the farms, so that naturally the cases of the women and children are greater than of the men.

5541. Is there a large child population? What would be the average number? I suppose it will be difficult to say. What would be the average size of the family?—Two or three amongst the polygamists and a larger family amongst the monogamists.

5542. Something like three to a hut?—Yes, the Christian villages are noted for their populations. That has been said, and I think it is true, that if you get to a Christian village you will find more children running about than you do with the polygamist heathen.

5543. (Chairman.) There was just one thing I wanted to follow up with you. You said, I think to Sir Mackenzie Chalmers, that you were very familiar with intravenous injections. Do you know what metallic antimony is?—Yes.

5544. And you know more or less its effect?—Yes.

5545. If, I will not say an unskilled person, but an insufficiently skilled person, made a mistake and did not do that operation correctly, the consequences would be very serious, would they not?—I expect they would be most unpleasant to the patient. If a solution of tartar emetic gets astray a blister results, so I find my way with normal saline, and then fill up with the active drug, so as to avoid serious consequences.

The witness withdrew.

Mr. F. O. STORR, M.B., B.CH., called and examined.

5546. (Chairman.) I understand you have had very considerable experience of sleeping sickness, both where it is conveyed by *palpalis* and where it is conveyed by *morsitans*?—Yes.

5547. And you have had experience of the form of illness which follows, or which we treat as due to *morsitans*—*rhodesiense*, and that due to *palpalis*—*gambiense*?—Yes.

5548. Have you formed the opinion that they are distinct diseases?—They are distinct diseases clinically, and the distribution is very different.

5549. The course of the disease is different?—It is much more acute in the Rhodesian disease.

5550. But you consider they are diseases of the same family?—Yes. I saw one *rhodesiense* case in which the man was sleeping, only one case—and it is probably rare for them to live long enough to sleep.

5551. In the one case we have been told the trypanosome may remain in the blood for years before the disease reaches an acute form, but with regard to *rhodesiense*, it runs a very swift course, and the man may die very quickly?—Yes.

5552. Have you had opportunities of studying the habits of both flies at all?—Yes, but I have studied *palpalis* more.

5553. *Palpalis* is very frequent on some of the islands on the lake, is it not?—My experience of *palpalis* was in Katanga. I only paid a visit of a week to Victoria Nyanza.

5554. Have you formed any opinion as to the distance *palpalis* travels? We know that it does not go far from water. What distance does it travel, say, along water?—I think a mile away from bush you do not find it much.

5555. In open ground?—In long reeds. If there are reeds in the dry season you do not find much fly a mile away from the bush. Where there is a good bank with trees growing on it you will find lots of *palpalis*, and a little lower down the stream you come to tall

reeds, possibly the height of this room or half that height. There you find *palpalis* in the wet season, but not in the dry season. I should say a mile away I could tell the people that they were pretty safe in the dry season.

5556. Do you connect *palpalis* noticeably with the presence of game or the presence of the larger game?—No, there is always game in Katanga everywhere.

5557. I will put the question in a slightly different form: Have you had opportunities of examining that game?—No.

5558. You have not examined them for trypanosomes at all?—No.

5559. Have you ever observed fly in large quantities where there was little or no game?—No.

5560. The two things are more or less co-existent in your experience?—I am saying that of either kind of fly, but I do not think that *palpalis* is particularly connected with the game in Katanga, because it is just as easy to find it close to a village as to find it away from a village. If you find a big village, you may be pretty certain that game never goes to drink in the middle of the village. The village may be a mile long, and you will find *palpalis* all along that village.

5561. Would there be domestic stock in that village?—Not much. They have a few goats.

5562. Do the fly feed greedily on goats and domestic animals?—I have not noticed; I do not think so. I think their food is people near the villages, but what it is away from the villages I do not know.

5563. You know, I suppose, the history of the epidemic in Uganda pretty well?—Yes.

5564. To put it quite shortly, have you formed a conclusion as to how the epidemic came into the country in the first instance? Was the *causa causans*, the trypanosome, conveyed, do you think, from game animals, or man?—Into Katanga?

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[Continued.]

5565. Yes.—It was brought in by infected soldiers from the northern part of the Congo.

5566. Speaking generally, do you think with regard to *gambiense* that men or animals form the larger reservoir?—I think man, and for this reason, that the *gambiense* sickness spreads rapidly in big villages while in small villages it does not spread nearly so rapidly. Where you find a small scattered population the sickness does not sweep through it so quickly as where you find a big population.

5567. Have you examined many flies microscopically?—No, that has not been my kind of work.

5568. Do you think that the *pulpalis* when near water will feed on reptiles, birds, and other things as readily as it would on warm-blooded animals?—I do not want to answer that question; I really do not know.

5569. May I leave *pulpalis* for a moment? You have also experience of *morsitans*?—Yes. May I recall my answer to your last question? I have seen a few *pulpalis* in a place where there were certainly no animals, because there were very steep rocks on both sides, a gorge, but there were plenty of those cormorant birds which hang out their wings to dry. I did think they were probably feeding on the cormorants.

5570. We have heard that before, but as to reptiles you cannot say?—No. Dr. Carpenter in Uganda showed me a reptile which he says they feed on.

5571. With regard to the *rhodesiense* conveyed by *morsitans*, have you formed any judgment as to what the main reservoir of the trypanosome may be?—I think it is the wild game, particularly water-buck.

5572. On what do you found that?—I examined the blood of five water-buck, and I think we found trypanosomes in four—it may have been three—out of the five.

5573. That leads up to the question of what the trypanosome may be. Do you think the trypanosome you found in the wild game is the trypanosome of man, *rhodesiense*?—As you see it in a fresh preparation it looks the same, that is all I can say about it.

5574. There are, as you know, various views, one being that *brucei* and *rhodesiense* are the same, and another that *brucei* is the one which produces nagana in cattle, as distinguished from *rhodesiense* which produces sleeping sickness in man, and a third is that *rhodesiense* may be a strain of *brucei*?—Yes.

5575. If the trypanosome is morphologically like *rhodesiense*, or *brucei* is *rhodesiense*, how do you account for the fact that in fly countries domestic animals cannot be kept at all, and that man, certainly in parts of the country in Rhodesia and in Nyasaland, suffers very slightly?—I cannot account for it.

5576. There are a good many possible hypotheses?—I cannot account for it. Man seems to be very hard to infect. There are up and down the country different herds of cattle which are immune to fly.

5577. Is that in particular places?—They are probably the remains of certain herds. An Arab called Chiwali was knocked out by the Belgians, and they collared his cattle, and some were taken to the Luapula valley and some to Lukafu, and both herds are still existing, but they are very small and suffering from in-breeding.

5578. Is it not the general experience that if you bring cattle into a fly country they almost all die?—Some do not die.

5579. Can you tell me whether the survivors have been examined—has their blood been examined?—No, I cannot say.

5580. Have they been examined as to whether trypanosomes were in their blood and whether they were tolerant to them?—No, I cannot tell you that.

5581. It has been suggested, following your theory of the game being the principal reservoir of the disease, that some moderately sized area should be cleared of game and the game kept out of it, and it is hoped that results might follow either as regards the disappearance of the fly or the disappearance or modification of the infectivity of the fly; have you considered the feasibility or the possible results of that experiment?—I

wanted the administration of Southern Rhodesia to do it.

5582. Could you suggest the sort of area, or could you name an area which you think would be suitable?—I suggested the Sebungwe area.

5583. Would that be within reasonable limits as to cost?—That was an area where you could have a square with a side of about 60 miles, roughly speaking, but I am afraid it would not be so favourable now, as they have moved the people out of it. I believe Dr. Yorke talked to you about the Sebungwe area.

5584. If I recollect aright we heard something about Sebungwe before. The advantage of that is that it is in a small fly area separated from another fly area by a considerable distance of open country?—About 60 miles.

5585. Is that quite clear?—Yes, I have been there.

5586. So that I suppose you would say, from what you know of the fly, that there would be very little danger, supposing one lot of fly went out with the game, that another lot would come in from anywhere else?—There is no danger of that; the only conceivable danger is driving infected game into the other fly area.

5587. But you do think that the experiment, if the fly went away when the larger game were driven out, would be entirely falsified if other fly came in?—There is no danger of that.

5588. Is that an area which has, what I might call, any natural fencing round it, such as water?—No, there is no natural boundary to it.

5589. If you in some way drove the game out, you would have to provide for keeping them out by a fence or something else?—Yes.

5590. There is no natural obstacle to their coming back again?—No, none at all; there are no big streams at all.

5591. What did you say the size of that was?—About 60 miles square. It is more a figure of that shape (*indicating*) than square, but the side would be about 60 miles. It is a rhomboid, I think; it has an acute and an obtuse angle in it.

5592. When you were making the suggestion to the Government of Rhodesia, did you give them any estimate of what that would cost?—I suggested their giving out two or three guns to each headman; and that would cost the price of loaning a hundred guns.

5593. But when you have done that?—I should give them cartridges. The people have now been moved out of the area, but my suggestion was that they should give out guns and cartridges, and make them account for the cartridges.

5594. Are there many people living in that area?—Yes, I examined 2,300—you might say 3,000 people.

5595. Is there much sleeping sickness there?—I found one-half per cent. of all the people infected, and, as it is a disease mainly of adult males, that means a good deal more than one-half per cent. of the men, possibly 2 per cent.

5596. If you tried this experiment, would you move all the people from that area?—No, I should make them stop.

5597. Might you not, game or no game, continue the infectivity of the fly by man?—It is very poor fun for the fly biting people.

5598. In what way? I should have thought it was very poor fun for the people.—He very seldom succeeds in getting a meal off you.

5599. We have had it in evidence that the natives are rather indifferent to the fly, and let it sit on them.—In our country the native generally cuts a green branch and keeps dabbling at his back. I have seen a fly sitting on a native and he had not a good time at all; he kept on jumping off and coming back again.

5600. I would rather go back upon what I asked you. Did you, when you were considering this, limit the expenditure to turning the game out, because when you have turned the game out you have to keep them out? You would kill a good many, but you would not kill all?—The people keep on with their guns, and the less game there is the keener they are.

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[Continued.]

5601. Might not the result be to kill a large proportion and by constant disturbance to drive out others, and when the danger had ceased, apparently by the game having gone, they would return to their old feeding grounds or water holes?—If you stopped shooting them they would return, but if the people kept on using guns, when they have finished with the game going in big herds and easy to shoot, they will turn their attention to the single animals, which are harder to shoot.

5602. What I mean is this, that if the game was driven out and kept on trying to return, you would still keep up in that area a reservoir by which the fly might be infected to some extent?—I think myself you do not find fly unless there is a good deal of game. I do not think that an occasional bush-pig or a duiker or anything like that keeps up fly. The fly does not fly about looking for food; it sits on a game track waiting for the animal to come.

5603. We are told that one infected antelope or rather a tolerant antelope has an enormous number of trypanosomes in its blood.—No.

5604. You think not?—That is not my experience and I do not think it is the usual experience. They need a good deal of looking for.

5605. Then the question I was going to put to you does not follow: Would not a small number of game be sufficient to infect almost as large a number of fly as a larger herd of game if that was so?—Where this sleeping sickness is there is as much game as I have ever seen anywhere, an enormous quantity like you have on the plains in East Africa. In Lomagundi, where there is only a moderate quantity of game, the people keep their dogs and goats, but they cannot keep cattle.

5606. Do you think the dogs and goats are tolerant and can form a reservoir of trypanosomes?—Some dogs may be; the dogs in Sebungwe are not, because they all die.

5607. What about the goats?—They seem to have a certain amount of immunity because they die in the rains like cattle do, and they do not die during the dry season.

5608. Is it immunity, or what we call tolerance? Have there been experiments made to see whether a goat in good health might contain trypanosomes in its blood?—I have not made that experiment myself; I went for poor-looking goats.

5609. Sheep and pigs have both been suggested as animals that might be in good health and yet tolerant to the trypanosomes.—I have seen one goat in particular that was in very good health (this was not in Sebungwe but in the Lomagundi district) and I found trypanosomes in this animal and I sent him down to Salisbury. There is no doubt it was the same animal, and in Salisbury he was examined several times by Mr. Bevan and he could never find trypanosomes.

5610. Would this be an accurate representation of your view as regards *morsitans* and trypanosomes conveyed by *morsitans*, that game, in your opinion, are the principal reservoir, and that man himself and certain domestic animals, goats, sheep and pigs, may be reservoirs? Does that correctly represent your view?—No, I do not think that man himself is of any importance as a reservoir, because as soon as he is sick he sits in his house and never gets bitten again. You do not find man with this Rhodesian disease walking about in the bush, or very rarely.

5611. Not in the early stages even of the disease?—I cannot say about being in the very early stages; the only cases I have seen in the early stage had a temperature of 105 the first day.

5612. I understand that your suggested experiment did not involve any large expenditure on fencing, or any artificial methods of excluding the game after they had been driven out?—No, it did not.

5613. (Dr. Bagshawe.) I would like to ask you in what parts of Africa you have met with sleeping sickness; what was the first part of Africa in which you saw it?—Katanga.

5614. How long were you there studying sleeping sickness?—I was there 3½ years, and one year I was working on a mine seeing some sleeping sickness; the rest of the time I was working on sleeping sickness.

5615. So that you were working for 2½ years in the sleeping sickness area?—Yes.

5616. Were you in the Government service?—No. I was only six months in the Government service; the rest of the time I was on my own.

5617. You were working on it because it interested you?—Yes.

5618. After that where did you next see it?—I saw two cases at Fundu in Northern Rhodesia.

5619. That is on the Zambesi.—I do not know whether those were the *palpalis* disease or the Rhodesian disease; they might have been mild cases of the one, or severe cases of the other.

5620. What were you doing at Fundu at the time?—Examining the natives going south to the mines.

5621. Were you examining them for trypanosomiasis?—Yes.

5622. And you met with some cases there?—Two cases.

5623. After that you went to Southern Rhodesia?—Yes, I spent three months in Sebungwe and three months in Lomagundi.

5624. Lomagundi is somewhere north of Hartley, is it not?—Yes.

5625. I would just like to show you this map, published by Dr. Fleming, of the Sebungwe fly area (*laying same before the witness*). I want your opinion on that map. It shows the Sebungwe fly area with very definite borders and then the Mafungabusi fly area about 25 miles away. Have you been in the Mafungabusi fly area?—Only on the edge of it; I came into it from the east, and I did not go to the western edge of it.

5626. Do you think that plan is about right?—It gives it a very ample boundary to the eastward; it is about right.

5627. And the edge of the Mafungabusi is about right?—I have never seen this map before, and I did not know at all that the Mafungabusi fly area went so far west; if it does it is correct. I was given to suppose that the Mafungabusi fly area was just at the Sanyati River there.

5628. Supposing the areas to be correctly represented there, do you think there is any likelihood of intercommunication between these two areas of flies? There is a river, which seems to connect them.—I have heard of a transport rider who was followed for 23 miles by fly, but that is a record; 23 miles is more than a day's journey, and I have never heard of fly following more than a day's journey. This man was going at top speed to get through the flies, and he drove his cattle 23 miles at a stretch.

5629. You told us that game were extremely numerous in the Sebungwe district; what did they consist of?—Eland, zebra, and sable; those were the three most common, and then sassabi and water-buck.

5630. Anything larger?—There were lots of elephants and a few rhino up and down.

5631. Any buffalo?—There are herds of buffalo; I did not meet them, but there are two or three, I forget which, herds of buffalo, and they have well-known limits. The natives and officials know how they go up and down between different pools.

5632. With regard to the infections that were contracted in the Sebungwe district, from what I have read I have come to the conclusion that most of the infection was contracted on the Busi River; that is a small part of the area?—That is correct. You may say, generally, that in this eastern half there is more game and more fly, and in this western half there is less game and less fly, and along the Busi there is game and people and fly. There, there is plenty of game and plenty of fly but no people; there, there is plenty of people but very little game and very little fly, and the reason why they have most sickness in Busi is because there is both game and fly and people.

5633. And that is only part of the area in which you do get these three elements in abundance?—Yes.

5634. But if the infection comes to man from the game one would expect them to be infected at any part of the fly area, would not one?—I reckon there is much more game there. That is flat country; there are

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[Continued.]

dambo on it and game thrive, but here there is very little dambo country and it is more stony, and there is nothing like so much game. The bulk of the infection is here (pointing on the plan).

5635. You think that in order to get infection of man you must have a lot of game?—That is my impression.

5636. You have studied *morsitans* in this Sebungwe area?—Not much.

5637. We were told by one of the witnesses that the fly in dry weather is confined to areas from which it spreads in the wet season: have you noticed anything of that kind?—I did not notice that in Sebungwe because I was not particularly studying the fly, but in Lomagundi I had been told that by Mr. Jack, the entomologist, and going there in the rainy season and comparing it with the dry season, I confirmed Mr. Jack's observations. He observed both in the rainy season and the dry season, and I confirmed what he observed in the rainy season. I was not there in the dry season myself.

5638. If that were the case it would account for the very conflicting accounts of the fly areas which one meets with in the literature?—Yes.

5639. In some cases the areas would have been visited in the dry season, and in others in the rainy season?—That is right.

5640. You think that is probably correct?—Probably correct.

5641. It has been suggested that this is the time when you might attack the fly with some hope of success; that is to say, if the fly is limited to comparatively small areas in the dry weather you might cut down its shelter, you might cut down forest and burn it at the end of the dry season: do you think that feasible?—No. I think it would be a very large piece of work even in the dry season, and it is not a matter of cutting it down, but keeping it down. I think it would be a very big job. It is not like *palpalis*; the belt is much wider than the *palpalis* belt.

5642. Have you found any pupæ of *morsitans*?—I found one batch of 40 pupæ.

5643. Of *morsitans*?—Yes.

5644. Were they all in a small area?—They were under a clump of big trees in an area about the size of this room.

5645. What were the trees?—There were two fig trees, but the others, consisting of two or three, I did not know.

5646. Did you find the pupæ close to the roots or some distance away?—Within a yard of the roots, but they were in places where they could easily have been scratched up by game birds. They were not right among the roots as I have heard; they were right out in the open as regards birds scratching.

5647. Were they occupied pupæ or were they shells?—40 occupied pupæ.

5648. But many old pupæ shells?—No, the great bulk were occupied.

5649. Did you breed any of them out?—I sent them down to Salisbury, and a good number were bred out.

5650. And they were all found to be *morsitans*?—Yes, Mr. Bevan bred them out.

5651. Have you any idea of the conditions which determine the places where pupæ are deposited?—No, that is the only time that I found pupæ, and I looked under all the other trees and failed to find them. I think the reason I found several of them in that one place was that that was the only favourable place just there, but I may be wrong.

5652. They have been found by some people in hollow trunks and damaged trees?—I have never found them there.

5653. And it has been rather concluded that that is the place where they can always be found, but that is not so, according to what you tell us?—That is not where I found them, but you will find a report from Mr. Lloyd, of Northern Rhodesia, who is an authority, and has a lot of trained natives.

5654. Has he found many?—He can find them now without fail, because he, like Dr. Carpenter, has a lot of trained boys.

5655. Has he found any special condition which determines their presence?—They are always near shade, I believe, and near a stream bed. They are not always in deep shade. He has found them exposed to the sun, but that was in the winter.

5656. They are always near water?—No, the stream may be dry, but they are near a watercourse or stream bed.

5657. I think you told the Chairman that the Congo and Rhodesian diseases are clinically different?—Yes.

5658. Would you expand that a bit and tell us in what the differences consist?—The Rhodesian disease is much more acute. I saw one man with Rhodesian disease who was well and came to the place applying for a job, but all the others were obviously sick and quite obviously suffering from fever. They all complained of pain in the legs, and they mostly had oedema of the feet, whereas in the other disease you go to the village and line up the people and examine them.

5659. What were the symptoms in the ordinary sleeping sickness?—The first case I saw I said to him, "How long have you been ill?" and he said, "I am not ill." That is in the Congo sickness. These other people are all ill and die in a few months, two or three, as a rule.

5660. Did you see any cases of interstitial keratitis in either form of the disease?—No, I did not see keratitis.

5661. How many cases did you diagnose in the Sebungwe area?—Eleven.

5662. I think that is all that has been found there?—No other doctor has been there before or since. Mr. Taylor was found before in England.

5663. You mentioned one man in the Sebungwe area who came to you; what became of him afterwards? Did he get sick afterwards?—He went down to Bulawayo and there he was not sick at all, and they allowed him to go home, but whether he has been traced or lost sight of I do not know. They once found trypanosomes in his blood in Bulawayo.

5664. Did you diagnose your cases by examination of the blood or by gland puncture?—All except one by gland puncture, and that one was a baby.

5665. Do you think you get as much enlargement of glands in the Rhodesian disease as in the Congo disease?—No, I think you get less enlargement of glands on the whole, but the trypanosomes are much easier found in the glands.

5666. In which form?—In the Rhodesian disease. It is very easy to find the Rhodesian disease by gland puncture; you always find it in a minute. In the Congo disease I have found a case where the man was examined by three different doctors, and the last one examined him five times and found the trypanosomes at the fifth attempt. Yet we were all certain he had them really, but could not find them.

5667. How many cases did you diagnose in Katanga?—Some hundreds.

5668. Four figures perhaps?—Perhaps four figures.

5669. Did you watch the course in all those?—No, I watched the course in perhaps 40 or 50.

5670. What do you think the average duration of the disease is about Katanga?—So far as I remember, we had 80 cases which I managed to trace, and at the end of three years half were dead and half were alive.

5671. So that half the cases had survived more than three years?—Yes.

5672. Were any of them pretty well then?—They were said to be; I did not see them all. As a general rule those who survived were fit and well. It is quite a common thing to have carriers walking about with the sickness. One of my carriers got to the sleeping stage all of a sudden while working for me.

5673. To come back to the Sebungwe area, I think from what you said to the Chairman you rather share the views of Dr. Yorke. You have read Dr. Yorke's papers?—I have read a paper of Dr. Yorke in the *Lancet*, and I think it quite correct.

5674. You agree with him that it would be a good plan to take the Sebungwe area, and kill off the game and see what the result was?—I think it would be more difficult and less valuable now that the people are

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gone. When the people and the domestic stock were there you could test the effect on the people and the domestic stock; now there is nothing to test it by.

5675. How would you test it by the effect on the domestic stock, because I believe there was not any; would you introduce any?—There was; people had big herds.

5676. In the Sebungwe areas?—Yes, big herds of sheep and goats.

5677. But no cattle?—There was only one man on the edge of the fly area who thought he was outside the fly who had a few cattle, but I expect he will lose them.

5678. These big herds of sheep and goats were living in the fly?—You might say they were dying in the fly; they had a good deal of sickness, particularly the sheep.

5679. Were they dwindling in numbers?—Yes, at some villages they had lost all or driven them all away, but the goats do better than the sheep.

5680. Did the natives say this was a new phenomenon, the sheep and goats dying off?—Yes, new.

5681. Formerly they had been able to keep them?—You know the rinderpest came in 1896, and in the rinderpest the fly was almost blotted out; there was only a little area left when the white man came after the rinderpest where there was fly. The people say that before the rinderpest they had fly and the goats did not die, but the sheep have come in since the rinderpest, that is to say, they are new to the country.

5682. And now that the fly have spread since the rinderpest they are killing these animals?—Yes. As to the goats, I do not know whether they are the descendants of the original goats or a different lot.

5683. And now that the population has been removed I understand you have changed your mind and that you would not recommend that area?—I still think it would be a useful thing to do as an administrative measure, but it would not be so interesting scientifically. I think ultimately that is what they will do.

5684. You think that that would be a better experiment than taking a smaller area and fencing it?—I have not thought about fencing a small area. The question is whether the fly would disappear from a small area if there is fly all round. Supposing you had a small area with a breeding place in the middle of it, I think the fly might still go there to breed, and then you would have to destroy the breeding places, and then you have introduced a new factor.

5685. In answer to the Chairman you said something which interested me, to the effect that the disease had spread rapidly in certain villages in Katanga which were large, but very slowly where the villages were small.—I think that is correct.

5686. Do you think that is general, that you get a rapid spread of the disease in a big population and a slow spread in a scattered population?—I mentioned that observation of mine to Dr. Hodges, and he was quite clear that the same happened elsewhere; he compared Uganda and the Nile province, and in the Nile province he said it was a kind of sporadic thing because of the small scattered population.

5687. (Mr. Buxton.) I understand you are in favour in any such experiment of retaining the people?—Yes.

5688. Supposing the game were all destroyed and the people remained, your theory is that the infectivity of the fly would cease?—Yes.

5689. Supposing it did not cease, there would be a strong presumption that man is a reservoir?—Well, before accusing man I should accuse the domestic animals.

5690. A domestic animal might probably also be a reservoir?—A much more probable reservoir than man, to my mind.

5691. I should like to ask you about these sheep; were they brought in since the rinderpest?—Yes.

5692. And they are now dying?—Yes.

5693. May that have been because, unlike some domestic cattle, they have not been long enough exposed to it to become immune?—I should think that is so, certainly.

5694. You would recognise that it is possible for animals to become immune in course of ages?—Yes, it has been proved to be so in the case of cattle, but it is only a certain degree of immunity.

5695. Have you formed any opinion as to how they originally became immune? We are told that the animals are immune and tolerant, are they not?—If you mean that they get the disease and get better of it, I do not know.

5696. I mean that they may have in their blood the trypanosome and yet have no apparent disease?—It is certain that cattle in Katanga may have trypanosomes in their blood, but probably not *brucei*.

5697. I am speaking of wild animals at the moment.—Wild animals certainly have trypanosomes and no apparent disease.

5698. In some way or another they have acquired that immunity in the course of ages?—I dare say, I do not know; they may or may not. As they became animals the trypanosomes became trypanosomes; the things developed together. You are going back to geological ages, are you not?

5699. You say that man seems very hard to infect?—Yes.

5700. Does it occur to you that it is possible that in those regions a considerable proportion of human beings may have become immune or practically immune?—No, because white men seem no easier to infect than black men. I could not put it statistically, but I know lots of white men who have worked about these dangerous countries and not been infected.

5701. How do you account for man being so hard to infect? How is it that with all the conditions of a very enormous epidemic we did not get it there?—I cannot account for it; it is very difficult to explain to my mind.

5702. Immunity or partial immunity would account for it, would it not?—Yes, certainly it would, but there is a difficulty about it to my mind. I am talking to clever people and I am just a general practitioner, you understand. The difficulty is that if man is partly immune, why does he get it so terribly badly when he gets it?

5703. You think some natives have only been recently introduced like the sheep?—The natives themselves get it and die right away.

5704. You do not know but that the grandfather or grandmother of that particular native was a slave raided from some other country?—That is quite possible.

5705. There are many ways in which the introduction of fresh blood might take place which has not had time to get immune?—That is quite possible.

5706. (Dr. Balfour.) With reference to an answer you made to the Chairman, how much blood would you say a tsetse-fly sucks up at one uninterrupted meal, roughly? He sucks up a good quantity anyhow, does he not?—Yes.

5707. What is your experience as regards the number of trypanosomes in the peripheral blood of wild game; say, in a slide, if you take a small drop and smear it out, how many trypanosomes will you find, as a rule, in an infected animal?—I am not the person to address this question to, Dr. Balfour, because I have only found them twice, but I have searched, searched, searched.

5708. You found them in water-buck?—Yes, and I have searched, searched, searched. Dr. Kinghorn and Dr. Yorke found them in 100 or 200. You should address the question to them.

5709. Even although there were only a few in the blood, what we call a few, still the tsetse-fly would become readily infected?—Yes.

5710. Do you think the Katanga disease is the same as that in Uganda?—I should think so.

5711. And with the same carrier?—Yes.

5712. As regards sick natives going into their houses, do they not lie outside their houses?—Yes, but in the village.

5713. Do not the flies go into the village?—Quite rarely; there are clearings around the village, so that there is the clearing and there are fires, and very likely the dung.

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5714. As regards the Sebungwe area, supposing you had been able to carry out an experiment there, with the people and domestic flocks and herds, and supposing the tsetse-fly had not gone away, would there not have been rather a risk of the people and the stock getting more infected than before? Would not the fly probably have turned their attention to the people there?—If the fly did not disappear, I dare say it would bite people more.

5715. So that there is a bit of a risk in carrying out the experiment with the people in the area; you might have to take that risk, but still it exists?—Yes.

5716. That is part of the experiment; you do not think the fly would be infective?—I believe that as the game disappeared the fly, even if it remained, would lose its infection.

5717. You do not know how long the infectivity lasts, or could you say definitely how long the infectivity lasts in this Rhodesian infection?—I should say probably, not definitely, during the life of the fly.

5718. How long does the fly live?—It is not likely to live round the seasons, is it?

5719. That is what I want to get at.—You know the experimental work; it is between 200 and 300 days.

5720. You cannot draw conclusions from a captive fly; it may live considerably longer in nature, may it not?—It may.

5721. You do not think there would be very much risk, as far as you can tell, in leaving the people in the area?—No, I do not think so; you could always move them when you wanted to.

5722. That being so, is there any smaller area like the Sebungwe area which might be a kind of ideal area with all the necessary conditions?—Sir William Milton suggested another area.

5723. Where was that?—Just east of Chechenini Hill in Lomagundi, but I am not satisfied myself that that area is isolated.

5724. Have you been there?—Yes, but I have not been round it to see that it is isolated.

5725. Would it be an area worth exploiting?—The people there cannot keep cattle, but to the best of my belief they have dogs and one or two goats, and there is no human sickness there.

5726. Who suggested it?—Sir William Milton.

5727. Does he know all the local conditions there?—I do not think he does know the local conditions particularly well.

5728. He has never been round it?—Not recently anyhow.

5729. It might be worth investigating as a suitable area?—It might.

5730. Is it a large area or a comparatively small one?—It is not so large as the Sebungwe.

5731. Is it half as big?—I should say it was one-fourth of the area.

5732. Would it be more easily managed than the Sebungwe area would be?—If it is isolated.

5733. It is no use unless it is. Is it an area you might isolate artificially if it was not naturally isolated?—No, that is out of the question.

5734. (Chairman.) Did I understand you to say there was no sleeping sickness there?—In this area in Lomagundi I could not find any sickness.

5735. (Dr. Balfour.) Then it would not be suitable in that way, would it, for the experiment?—No, the only animals you could test with would be cattle.

5736. It would only be of value from the point of view of the domestic stock?—To the best of my belief (I have not my notes) they had goats there. They had no dogs, but that was because of the taxation.

5737. They had no cattle, because cattle would have died there; was that it?—They had four head of cattle, two calves and two mothers. When I was there the two mothers were dead and one of the calves infected.

5738. With what trypanosome?—With a trypanosome, but I did not get the strain down to Salisbury. I inoculated a rat, but the rat escaped.

5739. Was it like *rhodesiense*? Do you think it may have been *rhodesiense*?—Yes, just looking down the microscope it was the same kind of trypanosome.

5740. That does not help us very much?—No.

5741. How many inhabitants are there in this area?—There is one chief, and I forget if there are two or three little villages. There are two or three moderate-sized villages.

5742. A few hundred people?—300 people, or thereabouts.

5743. Do you believe that both diseases, that due to *T. gambiense* and that due to *T. rhodesiense*, are solely due to the bites of infected *Glossina*?—I have seen places in Katanga where it is difficult to account for it by *Glossina*; I have seen one or two villages where it is difficult to account for it. The enormous mass of the villages are all near *Glossina*.

5744. What may have been the cause in those villages; have you any idea?—No. My suspicion is that it was *Glossina* all the same because the people go off in search of fish; they desert their villages and go fishing.

5745. When we went out together to Africa some years ago you had an idea of getting boys well trained in Uganda to look for pupae?—I gave that up and went off to Sebungwe.

5746. You say that Mr. Lloyd has trained boys. He was very successful in finding breeding places?—Yes, he told me he had a good many trained boys. You will get a report from him pretty soon.

5747. I suppose his operations have not resulted in any diminution in the number of tsetse-flies?—No.

5748. From what you said I also gathered that gallinaceous birds are probably not very much use, because I suppose you have plenty of Guinea fowl?—I have never found them of any use at all. In places where there are lots of *palpalis* there are also plenty of Guinea fowl.

5749. You say you found pupae in a place where Guinea fowl could easily have got at them?—That is an exceptional observation, I think.

5750. You have no observations on a large scale?—There was a curious thing there which might account for it. As soon as I started scratching I got sore eyes, and my boys pointed out to me that there was a cocoon of one of these poisonous caterpillars there. Possibly that had the same effect on a bird, that as soon as he started scratching he got sore eyes.

5751. Keeping the birds away, you mean?—Yes.

5752. You did not think it attracted the tsetse-fly to deposit its larva there?—It is possible.

5753. You never found it in any other place?—That is the only place where I have ever found pupae.

5754. You do not know if Mr. Lloyd has found that poisonous caterpillar?—No.

5755. (Mr. Austen.) You mentioned having seen one man with trypanosomes in his blood, who subsequently went to Bulawayo, and you said you then lost sight of him; how long was he under your observation?—I observed him and sent him down to Bulawayo to be under observation there; he was there about three months and then he clamoured to be sent home, and they sent him home; whether they have kept him under observation or not I do not know.

5756. During the whole of that time he developed no active symptoms of disease?—No, I am told he was well all the time.

5757. Would not he have been a reservoir if he had been in places where he was liable to be bitten by *Glossina morsitans*?—Yes, he might have been.

5758. Do you suppose that case is unique?—No, I have heard of another case like it in Nyasaland.

5759. Dr. Yorke mentions one case which apparently was similar in Northern Rhodesia?—I came home with a man from Nyasaland, who told me of a case of the same kind. He was not a doctor.

5760. Here we have got three cases; might there not be 30, or 300, or 3,000 similar cases?—There might be 30.

5761. You would go as far as 30?—Yes.

5762. Have you any reason for fixing the limit at 30?—It is a very severe disease, and I think there might be these cases. I examined very carefully all the people who had enlarged glands; but if you think that people frequently have the sickness without an affection of the glands, it would be very hard to say. Some

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people say that the glands are no good in diagnosing the sickness.

5763. Did you examine the whole of the people in the Sebungwe district, so far as it was possible to do so?—Yes, but a good many were away from the district working in the mines and with farmers; there is always a shortness of men compared with women, but the presumption is that all the men who were away were healthy.

5764. They were well enough to go away at any rate?—Yes.

5765. If any of these people had had trypanosomes in their blood without having enlarged glands, must you have found them?—I examined about a hundred people right through, examining their blood microscopically, and I did not find anything, and life was not long enough to do that, and I gave it up, and only examined those who either looked sick or had fever or had enlarged glands. I examined those who either looked out of sorts or had any fever. I took their temperature. If they looked ill or had fever, I examined their blood even if they had no glands.

5766. I think you said, in answer to another member of the Committee, that one reason which made you inclined to doubt that human beings could be reservoirs was the fact that people who contracted the disease got it so extremely severely?—Yes.

5767. I am not a medical man myself, and I ask for information; is that out of harmony with what we know as regards immunity in other diseases? Is it not a fact where you have immunity, whether artificially acquired or not, that people who are not immune to a certain disease get the disease very severely, whereas the immune escape?—I do not know.

5768. Taking the mere fact that a man dies of typhoid fever, for instance, is that any proof that it is impossible that there can be any typhoid carriers?—No, but the typhoid carrier is a man who has had typhoid; very likely he has had severe typhoid, but he goes on having typhoid without knowing it. He is not a man who has always had typhoid without knowing it.

5769. But I ask about the possibility of a man having trypanosomes in his blood without knowing it.—It is a possibility.

5770. What I want to get at is this: is the fact that non-immunes get the disease severely a proof that there is no such thing as immunity or tolerance?—No; it puzzled me.

5771. I thought you rather put it to another member of the Committee (Mr. Buxton) that it was—that the fact that the people who got the disease got it very severely seemed to suggest that other people could not be carriers, and I did not quite follow that. You did say that, that the people who got the parasite were killed off?—It is a very puzzling subject; I am puzzled, too.

5772. Have you made a special study of trypanosomes?—No, only of people.

(Chairman.) I think what the witness said was in answer to me. I asked if he thought that man was a reservoir for *morsitans*, and he said, "I do not think so, because man dies so quickly; he gets into his hut and has not the opportunity of spreading it, because no fly gets at him."

5773. (To the witness.) Is not that correct?—Yes.

5774. (Mr. Austen.) You told us you found what you considered to be *T. rhodesiense* in four water-buck out of five?—Yes.

5775. May I ask how you determined these trypanosomes to be *T. rhodesiense*?—I cannot tell you. Mr. Bevan did all the work.

5776. He is responsible for the identification of the trypanosome?—Yes.

5777. Do you know anything as to the distance that *G. morsitans* will fly across the open? You have told us that in one case it followed a transport rider for 23 miles?—Yes.

5778. What I want to get at is the distance across the open which you think *G. morsitans* might fly in search of food without actually accompanying an animal or man?—I have not the faintest notion,

but I do not think as a rule it does fly. I think it sits where it is and waits for food.

5779. It does not cross the open?—I think it crosses the open and flies away to the breeding place when it wants to breed, but people who have transport work to do have told me that the flies that bite you when you are walking on a path are either sitting on the grass or the sand waiting for you to come, and when disturbed by you they get up and bite you. There may be a place where there is plenty of fly; you go out from your camp in the early morning without any flies following you and you sit down, and you may sit till doomsday, and I think it very unlikely that the fly will come and bite you.

5780. Do you mean that they will attack moving bodies rather than a stationary one?—They are stationary themselves, waiting for the moving object to come; that is the way they hunt.

5781. They are attracted by moving bodies?—I do not know whether they are attracted by moving bodies or whether they are attracted by the smell of blood, but they attack people as they come along or animals as they come along.

5782. What I wanted to get at was this: in the case of the Sebungwe area, where it is suggested an experiment should be made, do you really consider this area sufficiently isolated to obviate any risk of incursions of fly from outside?—Yes.

5783. Only one other question: I think you said that *G. morsitans* is not likely to bite natives very much because the natives would hit at the insect and knock it off, but surely the fly does bite human beings?—Yes, but I do not think it would keep up its numbers by biting people if game failed. It is very dangerous for the fly to bite a person.

5784. The very existence of these cases of *rhodesiense* disease is clear proof that the fly bites people?—Yes, but it is not clear that the fly gets any blood out of them.

5785. There are cases, which have been recorded by Mr. Selous and others in the case of white men, showing that perhaps only one bite out of ten causes pain and the other nine may pass unnoticed. That has been recorded, and I can give you chapter and verse later on for it.—I should think that is an exaggeration.

5786. It may have been only a personal experience?—I have not tried it with *morsitans*, I have tried it with mosquitoes, and with mosquitoes the majority of bites cause pain.

5787. The idiosyncrasy of the person varies very considerably?—Yes, but a fly bite is much more painful than a mosquito bite.

5788. (Mr. Rothschild.) Do you not think, as the fly, to a certain extent, follows game, there would always be the risk of a few animals coming back into the area and bringing fresh fly in?—I do not quite follow you, sir.

5789. You suggested that we should try an experiment in the Sebungwe area by employing natives to shoot or drive out the animals by shooting?—Yes.

5790. Animals always like to stop near where they can get water as well as grazing, and if their main water supply was inside the area a few would always try to get back, and would they not bring fresh supplies of fly with them?—I can answer that in two ways. First of all, there is no fly outside, so that if they came in from outside they would not bring fly with them. Secondly, there is excellent shooting country along that river Tana, and there is the Zambesi. It is only in that corner, which is rather stony, where there is not much game. There is excellent game country on three sides of the area with lots of game in it.

5791. Where there is no fly you mean?—Yes.

5792. (Dr. Chalmers Mitchell.) You are quite convinced that in the *morsitans* area the wild game are the chief reservoirs?—Yes.

5793. So much so that I am right in thinking you are not quite in favour of the experiment, but you would rather advocate the destruction of the game as an administrative measure. I think I heard you say that?—I would much sooner see it done in a scientific

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way first, in case I should be wrong, because it would be an awful pity to have killed all the game.

5794. You are not quite convinced, then, that you are right?—I do not know what being "quite convinced" means; I would give big odds in a bet.

5795. What does your conviction, so far as it goes, depend upon?—Wherever I have seen fly myself there has been plenty of game, and I only know one competent observer who says the opposite, and that is Major Stevenson Hamilton, and that was an isolated observation.

5796. But with regard to the particular trypanosome, you have not had time to be an authority upon trypanosomes yourself?—No, I am not an authority on trypanosomes.

5797. Then it turns on the identity of the human trypanosome with the trypanosome you find in the wild animals, does it not?—I do not quite follow.

5798. Why do you accuse the game? Because you find trypanosomes in them; is not that so?—Why do I accuse the game of causing the sickness?

5799. Of being a reservoir of the sickness?—Yes.

5800. Because trypanosomes have been found in them?—Yes.

5801. What trypanosomes? You do not yourself claim to be an authority on the particular trypanosome?—No.

5802. Upon whom do you depend?—I depend upon Sir David Bruce and Dr. Kinghorn.

5803. You are aware, of course, that certain people do not agree with them?—Yes.

5804. So that really your belief that the game ought to be destroyed depends upon your confidence in Dr. Kinghorn and Sir David Bruce?—No, there is another thing outside; I think the tsetse itself would disappear.

5805. So that the fly would go and it would not matter much; anyhow there would not be any chance for the trypanosome if the fly went?—I have got two things to say; first of all, that the fly would disappear, and if it did not disappear then I rely upon Dr. Kinghorn that the trypanosome would disappear even if the fly did not.

5806. You know, of course, that in many parts where the Congo disease is prevalent there are no big game?—No, but in the Congo disease I think it is the people themselves who are the important reservoir.

5807. I gather that from you, but I want to come back again. If you kill off the game in Nyasaland you still have the fly left outside, have you not?—Yes.

5808. And then you get back to the position that you are in in some of the Congo districts; you have the people and the fly?—No, because on the Congo the people stand all day by the water; the women are pounding grain and the children playing about, people are bathing and the men are fishing. They do their transport by canoe very often as well. The people stand all day long by the water and the fly is limited to the water. In Nyasaland the women and children spend most of their day in the village and the fly does not go into the village; the fly is outside in the bush.

5809. Only one other question. I gather you have no theory to account for the number of people who have been bitten by *morsitans*, which at the same time is biting and killing pigs and cattle, and so forth, and not being killed themselves?—No, I cannot account for that at all.

5810. Would it not be explained if it so happened that Dr. Kinghorn and Sir David Bruce were wrong in their identification?—I do not quite follow that myself. How would that explain it?

5811. It would explain why people may be bitten constantly by flies which at the same time were killing animals, if the flies were carrying a trypanosome which was fatal to the animals but not to human beings?—Yes, perfectly.

5812. Therefore really it turns upon whether Sir David Bruce and Dr. Kinghorn are correct in supposing that the trypanosome is the same?—Yes.

5813. So that really it turns on the correctness of their judgment?—Yes.

5814. (Sir Mackenzie Chalmers.) As regards the Sebungwe district, do you think it could be cleared of

game by keeping a small hunting party harrying them continually?—No.

5815. The population have been removed and you cannot rely on them, but do you think the game could be cleared if you started a hunting party and kept harrying it?—No. I should put on one hundred or a couple of hundred black police with two or three officers.

5816. And then get up a small party and keep it clear; you would require that, would you not?—I should think you would require a pretty big party to keep it clear.

5817. You would require a couple of hundred black police and three white officers?—At least, I think.

5818. Then without any fencing, do you think it could be practically cleared?—I would like to consider that a bit more.

5819. You suggested the Sebungwe area for an experiment, but you suggested it when it was inhabited?—That is so.

5820. Now, what is your opinion?—I think it would be more difficult and less satisfactory.

5821. I understand you to say you have never seen a *morsitans* area in which there was no game. —No.

5822. Have you examined, or do you know, if the smaller game have been examined to see if they are likely to harbour the pathogenic trypanosomes?—What kind of animals?

5823. The duiker and dik-diks. — The duikers have been examined, surely.

5824. And they harbour the pathogenic trypanosome?—I forget.

5825. Would they not be very hard to exterminate from any area?—Yes, they would be very difficult, but there are comparatively few of them.

5826. Still, so far as they remained, they would be a possible source by which flies might be infected?—I think myself that the fly disappears if you clear out most of the game without clearing it all out.

5827. You think they would go to a new good feeding ground?—Yes. In Lomagundi, where I was, they showed me places where there was quite a fair amount of game left, and the fly is gone, or, if not gone, very nearly gone.

5828. You think they followed to the place where game was really thick?—No, I think they have died; I do not think the fly goes about looking for food at all.

5829. You think, if you got out most of the big game from this place, the flies would starve and die?—Yes.

5830. As regards the cases you have seen of Nyasaland sleeping sickness, have you made any *post-mortem* examinations?—No.

5831. Have you noticed at all what we have been told, that *morsitans* specially followed buffaloes?—I am quite clear that wherever I have been *morsitans* has not followed buffaloes, but it was a very curious thing that in Sebungwe, talking to old chiefs of the old days in the times before the rinderpest, they always connected the two together.

5832. You think that no longer holds good?—It does not hold good now.

5833. (Dr. Chapple.) Why were the natives taken away from the Sebungwe district? Was it because of the disease?—Yes.

5834. What preventive measures would you suggest for the control of sleeping sickness in Nyasaland? Have you made up your mind on any scheme?—No, I cannot suggest any except clearing off the game. You must have local knowledge before you can make suggestions of that kind.

5835. That is the only thing you know; you do not know any way by which the fly themselves might be driven off beyond driving off the game?—No, but Mr. Lloyd and Mr. Jack are really the men who know more about the entomological question.

5836. You cannot make any suggestion as to what further line of research we should follow?—Go on studying the fly and its breeding habits particularly.

5837. Do you think that, as far as you know, we have enough information to justify us in attempting

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[Continued.]

game destruction in the vicinity of populous centres?—With a view of saving the cattle?

5838. Yes.—Yes, I do.

5839. With a view to saving man?—In populous centres? You do not get those centres, and so the question does not arise.

5840. In populous centres in Nyasaland?—I could not tell you; I do not know Nyasaland. I thought you meant towns where the workpeople were. The Sebungwe area has a very small population.

5841. Do you see any other hope of limiting the spread of the disease by any prophylactic measures whatever or by medicinal measures?—No, I do not.

5842. Or by anti-toxins?—I do not know.

5843. Do you think the disease is spreading in Nyasaland?—I do not know Nyasaland at all; I have never been there.

5844. In your own area of which you speak?—I was the first man and also the last in Sebungwe, and I do not know whether it is spreading or not. I do not know if they have sent a man in this year or not.

5845. Do you think the matter is sufficiently urgent to justify immediate measures?—No, they have moved the people, and there is no immediate urgency now.

5846. (Chairman.) If entomological research was successful in getting rid of the fly, there would be no possible object in destroying the game, would there?—No.

5847. I wanted just to understand how far you take it; it may be that I have misunderstood one of your answers, but do you go so far as to say that now, on the knowledge we have, all game ought to be destroyed in all fly countries?—It all depends on what your object is, does it not?

5848. I do not know. You recommend the destruction of game so as to get rid of the disease both in cattle and in man, and I only wanted to know how far your recommendation went.—I think if you want to introduce white settlers and cattle into the country, you ought to destroy the game. I should be very sorry to introduce white settlers and cattle myself.

5849. That does not get us very much further forward. Have you formed so strong an opinion as

to the connection between game and fly and the disease of man and beast that you would recommend, in any fly country that is settled or likely to be settled, that all game should be destroyed? Do you carry it as far as that?—Yes, I would.

5850. Do you think that is feasible?—Yes, I do.

5851. I have asked you the question as I have asked it of other witnesses. There is only one other thing; I was very much interested in what you said to Dr. Bagshawe about this discovery of pupæ. We know that in your view game and fly are always associated; that is your view?—Yes.

5852. Are you aware either from what you know yourself or from what you have read whether the breeding places of fly are generally discovered in a game country?—I saw Mr. Lloyd the other day, he came through my place, and he has lately been discovering that the breeding places of the fly are always close to game tracks or else to paths, and I think to paths because they are also game tracks.

5853. So that in the view of both of you the breeding places of fly *prima facie* appear to have some connection with the presence of game?—Mr. Lloyd's view is that the young fly will starve unless it gets blood soon after its birth. Mr. Lloyd is the chief entomologist in Northern Rhodesia.

5854. (Sir Mackenzie Chalmers.) Is he at home now?—No, he is out there.

5855. (Chairman.) There is just one other thing. I dare say you have read that certain observers have said that they have known of countries where they have found fly in large quantities where there is no game. Your view, as I think you said to Dr. Chapple, is that with the absence of game the fly would die?—Yes.

5856. Do you reject their evidence on the ground that their observation must have been mistaken and that there must have been game?—Major Stevenson Hamilton is a man I have a great opinion of, but I cannot understand his evidence that there was no game or traces of game where he found fly. It worries me.

The witness withdrew.

EIGHTEENTH DAY.

Thursday, 5th February 1914.

At the Privy Council Office, Whitehall, S.W.

PRESENT:

THE RIGHT HON. THE EARL OF DESART, P.C., K.C.B. (Chairman).

Mr. E. E. AUSTEN.

Mr. E. NORTH BUXTON.

Sir MACKENZIE CHALMERS, K.C.B., C.S.I.

Dr. W. A. CHAPPLE, M.P.

Colonel Sir W. B. LEISHMAN, F.R.S.

Sir EDMUND G. LODER, Bart.

Dr. C. J. MARTIN, F.R.S.

Dr. P. CHALMERS MITCHELL, F.R.S.

Professor R. NEWSTEAD, F.R.S.

Mr. H. J. READ, C.M.G.

The Hon. L. WALTER ROTHSCHILD, F.R.S.

Mr. A. C. C. PARKINSON (Secretary).

Mr. LL. LLOYD, B.Sc., called in and examined.

5857. (Chairman.) You have been good enough to furnish us with a paper indicating the points upon which you are going to give evidence. Would you tell me when you went out?—I went out to Northern Rhodesia in July 1911 to join the Luangwa Sleeping Sickness Commission.

5858. You came home quite recently?—Yes, quite recently.

5859. As I understand, the purpose, or at any rate the primary purpose, of your going to Africa was to study the habits of the fly?—That is so.

5860. Incidentally you may have formed opinions on other points, but that was, at any rate, the primary object of your going?—Yes.

5861. I do not propose for the moment to deal with question 44, where you talk about an experiment,

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[Continued.]

but to deal with the question as regards the fly. Other members of the Committee can ask you about other matters. I understand that you have been successful in discovering breeding places of the fly?—Yes.

5862. In many instances?—The total number of places in which pupæ were found is about 200.

5863. When you say that breeding places have been found, do you mean that you have found a large quantity of pupæ in the same place or pupæ here and there, just one or two or more?—I have found as many as 150 in a small hollow of a tree.

5864. All in one hollow?—All in one hollow, and under certain fallen trees over 100.

5865. Have you found varying quantities, but large quantities together, as a rule?—No; large quantities in places which are close to the paths and the fords, but scattered generally through the bush, only in small numbers.

5866. What do you call a small number?—From one to ten.

5867. Is it usually, or was it invariably, the case that you found the larger quantities near the roads or paths?—Taking an area of about two square miles, it has been repeatedly and systematically searched for pupæ, and the relation between the breeding spots and the paths and drinking places of the game is apparent. 174 breeding places were found in this area. Of these, 123 places yielded less than 10 pupæ, 404 in all, an average of 3.25 pupæ per position. I have here a diagrammatic map of the area (*handing a map to the Committee*). These positions will be seen from the map to be generally scattered through the bush with no special relation to the paths. 38 positions yielded from 10 to 50 pupæ, 831 in all, an average of 22 per position. These positions are all within 150 yards of a native or a game path, with the exception of four, which yielded 10, 13, 20, and 32 pupæ respectively, and they are in close relation to some salt licks. These salt licks are very much haunted by the game animals which come to scrape away the soil and lick it up. 13 other positions yielded an average of 68 pupæ; nine of them are on the paths, and four within 100 yards.

5868. The black dots show where you found large quantities, and all the black dots I may say appear to be very much in the neighbourhood of paths, as far as I can make out. What was the nature of the bush or the grass close to the paths where you found these?—The bush throughout the area is the same well-wooded country and very scanty grass.

5869. Shade and comparatively light grass?—Yes.

5870. Are the trees large or small?—They are medium-sized trees up to two feet in diameter—mostly about a foot in diameter.

5871. Are there villages in this area which you have marked? I cannot quite see from the map.—No, there are no villages in the area. It is only two square miles in extent.

5872. Are the paths and roads you have marked much used by natives?—They are not much used by natives now, except amidst the gardens. They are paths which lead to villages which are now deserted.

5873. You would not say that there was a constant passage of moving objects up and down?—They are very much used by the game animals.

5874. Is it a country in which there is a large quantity of game?—Yes, there is a considerable quantity of game in the area.

5875. What does it consist of?—Rhinoceros, eland, the large antelope, and very numerous pig.

5876. Is there much small game?—Yes, a considerable number of pig and duiker.

5877. And the smaller rodents?—Porcupine are fairly numerous.

5878. Rats, rabbits, or anything of the kind?—They are not more numerous than usual.

5879. Is there much water there?—There is a permanent stream bounding the area.

5880. You were out there, I suppose, both in a dry and a wet season?—Yes.

5881. Coming now to the fly, did you notice a considerable difference in the quantity of fly in the dry and the wet seasons respectively?—On the high

ground in Rhodesia there is very little fluctuation in numbers. They appear to be rather more numerous towards the end of the dry season than at any other time of the year, but in the Luangwa Valley, where the conditions are different, being very much hotter and drier in the dry season, there is a considerable reduction of the fly towards the end of the dry season.

5882. As far as your observations go, would it represent what you are saying if it were put in another way; that there was concentration in the dry season and wider distribution in the wet season, or would it be that you really think the numbers of the fly are different? You see what I mean?—Yes.

5883. If you do not know, never mind. The question arises out of something we have been told by another witness. Now, putting aside for the moment the difference in the number of fly, owing to seasonal variations, do you think, judging from what you observed during the period that you were there, that the fly are always in that area—are they constant there?—Yes.

5884. Have you examined any other areas besides this one?—I have travelled through the whole of Northern Rhodesia practically, the Luangwa Valley and the high ground of North-Eastern and North-Western Rhodesia.

5885. Have you there pursued the same searches for breeding places?—I have not succeeded in finding any in the Luangwa Valley, but in both North-Eastern and North-Western Rhodesia breeding places have been found.

5886. I suppose that with the experience you had gained you rather hoped or expected to find under the same conditions the breeding places of this fly? I do not know whether the same conditions exist there.—I think that in a place like the Luangwa Valley, where the ground is very much cracked in the dry season and also where the fires are more severe when the dead logs are burned up, pupæ would be rather more liable to be found in the cracks of the earth than on the surface of the ground and in holes in trees.

5887. Did you in fact find them in cracks of the earth anywhere?—I have not done so yet.

5888. With the experience you have acquired, should you expect, if you and others had a free hand, that by constant search you would be able, more or less, to find the breeding areas of this fly, the *morsitans*?—Yes.

5889. And in that case to destroy them in the breeding places?—I have a suggestion which may be applicable to that. It may be possible to turn to account the habit of the fly of breeding close to paths and fords. The experiment would consist of the removal of all potential breeding places within 200 yards of a piece of road. These would be replaced by dry logs of wood slightly raised and coated on the lower side with bird-lime. The logs would be placed every 25 yards, and at varying distances from the path up to 100 yards. When the female flies searched for hiding places or for spots in which to breed, it is thought that they might be caught by this method. The experiment might be commenced on a very small scale and only extended if likely to be beneficial.

5890. In answer to me just now, you said you found a very large quantity in a little hollow. Were there fallen trees and branches over that hollow? Were there the usual conditions that you have described?—A photograph of one of the hollows is here (*handing a photograph to the Committee*).

5891. When you found the pupæ, did you notice in any or many of them anything to show that there was any parasite that preyed on the maggot or larva?—A number of empty cases which have been found are bored by a small hole like a pin-hole, which indicates that a small hymenopterous parasite has emerged.

5892. That inference is entirely derived from the fact of finding the hole?—Yes.

5893. Nothing more?—No.

5894. You did not get it closer than that?—No, not with that parasite.

5895. Now on another subject, have you watched the habits of the fly as to feeding?—I have carried

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[Continued.]

out some experiments on the comparative value of mammalian and avian blood in the laboratory.

5896. That was not quite what I meant. I meant have you noticed the fly in nature to see whether it appears to follow wild game? We know that it bites game and man. Does it follow either for any distance?—It certainly follows both man and game for considerable distances.

5897. Have you noticed at all whether it bites at night?—I have found it on one occasion fully fed inside my mosquito curtain in the morning. The fly must have fed during the night.

5898. I think you say somewhere in your statement that flies are unable to breed on a diet of reptilian blood. On what do you base that statement?—On experiments in which I fed the flies on chameleons, and I found that they died off very quickly, and none of them showed any signs of becoming pregnant.

5899. In how many cases did you make that experiment?—That was done with 52 female flies.

5900. From your observations should you consider that the prosperity and the breeding power of the flies is mainly dependent on mammalian blood?—The fly is ready to feed on any small mammal or bird, but the question whether it is able to do so or not depends on the agility of the small animals. These small animals and many of the nocturnal birds sleep during the daytime in just the places which the tsetse fly haunts, and when the animals were asleep the fly would have an opportunity of feeding on them, just as the mosquito will feed on man when he is sleeping; it rarely feeds on man when awake, but it is quite a common experience to find them in the morning fully fed. In the same way the tsetse will probably feed on small animals while they sleep.

5901. Am I right in saying that according to your observation you will not find any fly in cleared ground, whether naturally cleared or artificially cleared?—They are not seen, as a rule, in country where there is very little bush, and in cases near mines where the timber is cut down to be used as fuel the fly disappears from the area. This has been the case with the Bwana M'kuba mine close to Ndola in Northern Rhodesia.

5902. Take the ordinary clearing round a village, would you find fly coming to the clearing and coming up to the houses or not?—It would accompany men.

5903. I do not mean that. We quite understand that, but would you find it normally living there?—I do not think that, as a rule, it is found in the villages unless it has been recently brought there.

5904. Can you form any judgment at all from your observation as to the distance a fly will travel?—I do not mean when carried on a man's back, or by some other means, but of its own accord either in going from place to place or in following food—the distance from the bush to the clear ground I mean?—There is no experiment upon which any statement could be based.

5905. That point, in your judgment, has not been reached yet?—It has not been reached.

5906. I understand that in your view trapping, so far as it has been tried, has not been effective?—It has not yet.

5907. (Dr. Chapple.) Have you any evidence to suggest that flies return after they have followed man or animals for any considerable distance to their original haunts?—There is no such evidence to my knowledge.

5908. You have no opinion therefore on that point?—I do not think that they would return.

5909. Unless they were conveyed back in the same way?—Unless they were conveyed back in the same way.

5910. What does that suggest would happen to the fly? Would the fly die?—Of course, one could not make any statement about that without experimental evidence. Very little experimental work has been done with this particular tsetse-fly.

5911. Then upon what evidence do you base your suggestion that it will not return?—It would be unusual for an insect to do so. If the fly is carried out of one area into another area in which the physical conditions and the fauna and flora are

precisely the same, there is no obvious reason why it should return.

5912. Have you positive evidence that it does not return?—I have no evidence on the matter at all.

5913. It is an opinion based upon what?—On logic.

5914. Would your experience in other departments of research not lead you to believe that a fly that had wandered like that would not return to its companions, or at least to its breeding place?—As I say, there is no evidence on the matter at all, as far as I am aware.

5915. Still I understood you to say that they did not return?—I think I said that it would be improbable that they would return. I also said that I had no evidence on the matter.

5916. I do not think you said it would be improbable. If you had a cleared area round an experimental spot would you expect those flies to cross that cleared area in search of food, or for any other reason, apart from being conveyed by men or animals?—That could only be ascertained by experiment.

5917. There is no evidence that you know of?—None that I know of.

5918. You spoke about some villages now deserted. Do you know why they are deserted?—That is simply in connection with the habits of the Awemba natives in their agriculture.

5919. They have not been deserted because of the fly?—No; they move the village every three years.

5920. Have you any practical scheme in your mind which might be followed in the direction of controlling either the tsetse or the disease or exterminating the disease?—Apart from some experiment in which the fly should be compelled to attempt to support itself on a fauna of small animals, something might be done by the fencing experiment or by an alternative experiment which could be carried out in a large cage.

5921. Has any attempt been made to carry out that alternative experiment?—Not yet.

5922. Has any similar experiment been carried out that would serve as a model?—No, no experiment on a large scale has been made with this fly at all.

5923. Have you any evidence about the migration of flies, about the spontaneous migration of flies from one place to another?—In the Luangwa Valley towards the end of the dry season they appear to move down to the main streams, and in the wet season they disappear from that particular area close to the streams.

5924. Do you think they alter their breeding places from place to place?—At that time of the year in the Luangwa Valley apparently very little breeding goes on. In the laboratory we could not get our flies to breed towards the end of the dry season.

5925. Are the pupæ easily detected when you are going in search of them?—Very easily.

5926. Is there any evidence of imitation of their environment?—Apparently none. One point which is common to all the breeding places that have been found is that above each there is some relatively dark spot in which the female fly could hide when pregnant.

5927. Does she seem to take no precautions in the direction of hiding the pupæ?—There appears to be no care for the young. They are frequently dropped on to ground so hard that it is impossible for them to burrow. They then lie quite exposed to the surface, and frequently where any scratching animal could find them.

5928. Is there any colour imitation?—There is none.

5929. What should you say we have still to discover in order to justify our taking any practical steps along the line of prevention?—We need more information on the breeding places, more information on the food of the fly, more information on the parasites, and on a number of smaller side issues, such as the distance which the fly will move on its own initiative.

5930. Do you think we could take no step along the lines of prevention pending the discovery of those things?—There is nothing that can be recommended yet except experiment.

5931. (Sir William Leishman.) Have you worked with the trypanosome yourself at all?—I have not done so

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[Continued.]

5932. Have you made breeding experiments with the fly?—Yes.

5933. Did you send out fly boys to collect flies?—Yes.

5934. You were speaking of the disproportion of the number of female flies compared with the number of male flies. In some of your observations in your breeding experiments did you find an equal proportion hatch out?—Yes.

5935. You confirm the results of other discoveries on that point?—Yes.

5936. Did you or your fly boys ever encounter a larger number of males than females?—Yes. It is invariably the case in any indiscriminate catch of flies that the males are largely in excess of the females.

5937. You have not found the converse?—I have not.

5938. With regard to these breeding places which you have described to us, did you ever find a female fly in the sheltered spots you have described?—The flies are frequently seen to come out of these places as one approaches them—out of such a position as the burrow of a wart-hog or hollow in a tree. It is a very common experience to find large numbers of flies coming out of such places as they are approached.

5939. With regard to this experiment which you suggest of a fly-proof cage, would you tell us more in detail what you mean? What would such a cage be; how would you construct it to resist the weather? I take it that it has to stand for two or three years, has it not?—Yes. The dimensions of the fly-proof cage would be 100 yards long. It would be constructed of wire gauze or netting.

5940. Could you tell us more than you have included in the printed summary which we have of what you have before you? The details of structure are what I am interested in.—The details of structure have been discussed to some extent between Dr. May and myself, but we have not yet prepared any definite statement on the matter.

5941. In general, from the results of the discovery of the breeding places of *morsitans*, are you hopeful that we shall be able to destroy the fly on such a large scale as to control trypanosomiasis by tackling it on that side?—It is quite possible that that may be done.

5942. Do you regard it as probable—are you hopeful?—There is no analogy upon which one can base a statement on the matter.

5943. We want a great deal more information, you think?—Considerably more on every point.

5944. (Sir Mackenzie Chalmers.) Am I right in saying that at present you are the only investigator who has investigated *morsitans*, or has any other entomologist worked with *morsitans*?—There are two others, Mr. Dolman and Mr. Eminson, in North-Western Rhodesia.

5945. How long have they been working?—They have been working since February last year.

5946. Have you compared results with them at all or not; have you had the opportunity of discussing points?—We have met upon one occasion and discussed matters to some extent, but their observations have been on just general lines.

5947. Where have your observations been confined to—Southern Rhodesia, or where?—All in Northern Rhodesia.

5948. You have had three years there, have you not?—Yes.

5949. Can you form any rough estimate as to the size of the fly area in Northern Rhodesia? If we are to attack the fly in the way you suggest is possible, one rather wants to know what area has to be attacked?—Almost the whole of North-Eastern Rhodesia is at present infested with fly.

5950. It is all fly area, so to speak?—Almost the whole of it.

5951. In some places, of course, thicker than others?—Yes.

5952. Practically there are hundreds of square miles of fly?—Yes.

5953. And all over that fly area they would be breeding, I suppose?—Yes. North-Western Rhodesia is not nearly so heavily infested.

5954. But still you find the pupae of fly every where?—The fly frequents such districts as the Kafue district in North-Western Rhodesia.

5955. Take the Luangwa Valley—I am rather ignorant of the geography—what is the size of that valley?—It is 500 or 600 miles long.

5956. How broad?—The broadest place would be 60 or 70 miles.

5957. Is the whole of that fly-infested? I mean if one set to work to clear out fly-breeding places would one have to deal with the whole of that area?—Yes, practically the whole of it.

5958. During your investigations have you come across any places where you have found fly in any numbers without game or without big game?—Not in my experience.

5959. You always found the two together?—Yes.

5960. Are there any places which you know of which are free from game?—The game animals exist practically through the whole of Northern Rhodesia. In the fly area close to Broken Hill there is not very much game, but still it does exist there.

5961. Are there buffalo there or not?—There are buffalo there.

5962. One or two people have told us that fly are very apt to follow buffalo especially. Have you seen anything confirming that?—There seems to be no connection between the fly and the buffalo, or between the fly and any other particular species of animal.

5963. Or any food?—Or any food.

5964. Small or great, it makes no difference?—No.

5965. Have you been able to find out yet, taking the places where flies are thick, whether those places remain constant from year to year, or would you find them thick in one part one year and thick in another part in another year?—In the little experience that I have had they appear to be the same year after year.

5966. They are not like the natives, who change their place of abode every three years?—No.

5967. You told Sir William Leishman that it is much more common to catch male than female flies?—Yes.

5968. Do you attribute that to the fact that the numbers are different or that the female is more shy and harder to catch?—It is apparently connected entirely with the different habits of the sexes. The male accompanies the moving animals in large swarms in the hope of finding mates. The female, on the other hand, directly it has fed retires to some hiding place.

5969. She stays at home?—Yes.

5970. (Dr. Chalmers Mitchell.) Where does impregnation of the fly take place; do you know?—I have seen it occasionally in nature on animals and on man, but that cannot be considered a usual thing.

5971. Where do you think they meet if the females are generally in dark places and the males generally following man or animals? Where do you think the sexes usually meet?—I think they would usually meet around some animal. When the females come out of their hiding places to feed they would be found by males, and impregnation would take place probably after feeding.

5972. The female going out for food would also get something else?—Yes. Dr. Kinghorn has observed in the laboratory that impregnation usually follows feeding.

5973. Do you know the period of gestation?—It varies a good deal according to the temperature. I have known it as long as 20 days and as short as 12 days.

5974. I am afraid I do not quite remember the anatomy of the tsetse-fly, but is the female impregnated once or repeatedly?—Repeatedly; there is a provision for keeping the male element alive for some time in the fly.

5975. So that after one impregnation she could go back to her dark place and continue to produce at intervals her progeny without further impregnation?—I am not aware of any experiment that has been made which would show that. I am not aware of any experiments as to how long the male element would remain alive inside the female fly. She would have to emerge repeatedly to take food.

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[Continued.]

5976. You say that impregnation generally takes place, so far as is known, after feeding?—Yes.

5977. Do you suppose that parturition has any relation to that?—I think not.

5978. I notice in your very interesting specimens that the young that are born are of quite different sizes.—That is apparently due to the different food upon which they were bred. One series were bred on fowls and ducks. The others were fed on goats and monkeys. Flies fed on goats and monkeys were, on the average, 4 per cent. longer than those that were fed on the birds, and were greater in breadth.

5979. Compared with the naturally found ones the feeding of which you did not know?—They are smaller on the average than those. The length of the ones that were bred on the mammalian blood is roughly 5·8 millimetres, and the average of those found in nature is about 6·1 millimetres (*the witness produced a specimen box*). The ones that are found in nature are those at the bottom on the right-hand side.

5980. Do you think that you could infer from your catches of the progeny in nature what the food of the mother had been? Is it certain enough for that?—No, it is not nearly certain enough for that, because the result was only given on the average measurement of a number. Some of those bred on the birds were quite as large as the largest bred on mammals.

5981. You merely think that on the average they are smaller?—On the average they are smaller.

5982. (*Mr. Rothschild.*) First of all, when these breeding places were found did you or your associates try to find out if there were any particular conditions connected with them other than what the Chairman asked you? I mean, were the hollows in the trees in or near which they were found hollows of a particular tree, and were they produced by a special kind of decay, so that we may assume that there was some special odour connected with them?—There appears to be no association with any special kind of tree nor yet with any decay. They are frequently found, for instance, in the hole of the wart-hog quite apart from a dead tree, or in a cave in an ant-hill.

5983. But the hole of the wart-hog would have a very strong odour, at least to human nostrils. Might there not be some connection between the breeding places of the fly and strong odours, so that you could impregnate your bird-lime in the experiment which you suggested with some such odour?—I think not. The majority of the pupae, for instance, were found under dead trees. In many cases the trees were rotten, but, as will be seen from the photographs, in many places they are perfectly dry and not rotting in any way.

5984. In your printed evidence you suggest a certain fence, and you say that this fence would have to undergo constant supervision, because it would be approached and perhaps more or less injured by the big game from the outside. As far as I can understand, the experiment is designed to prove two things: first of all, whether the flies stay after the big game has been driven out, and supposing they do stay, whether the absence of the big game produces a difference in infectivity. Do you not think that such an experiment would be entirely vitiated by the fact that game animals on the outside of your experimental area might be continually bringing tsetse-fly alongside of the fence, and they would get in from outside even if you established by another experiment the distance the fly would normally fly and make a clearing of a larger space round your fence? The mere approach of a wild animal to your enclosure might bring fresh tsetse-fly in?—Yes, it is certainly a possible flaw in the experiment. The area which has been suggested in North-Eastern Rhodesia in the Lukasashi Valley is surrounded by very bare hills upon which, in the dry season at any rate, there is no food for game animals. If the fence were built along the edges of these hills game animals would not be likely to haunt the edges of the fence except where it crossed the stream.

5985. (*Dr. Martin.*) You are aware that there is some sort of experiment in contemplation, are you not?—Yes.

5986. I want to ask you a question which I think you will very likely feel you cannot answer, and if that is so, please leave it. It is assumed that it will be necessary to leave an area inside the fence which shall be cleared in order that flies from outside may not get into the experimental area. We have had various estimates given us of the width of the cleared area. Can you form any opinion as to what width would be reasonably safe so as to render the visits of flies from outside reasonably negligible?—No, I can give no such opinion.

5987. Could you form an opinion as to whether it should be 50 yards or a mile?—It would be very difficult indeed to say without definite evidence.

5988. I quite understand that. In North-Western Rhodesia, close to the Nkala Mission, there is a small plain which is just about a mile or so in breadth, and on one side of it there is a large quantity of fly; on the other side there is no fly, and the natives keep very large numbers of cattle which apparently never suffer from trypanosomiasis, so that apparently the fly does not cross that particular plain. I have not seen the place myself; I have only had hearsay evidence on the matter.

5989. (*Professor Newstead.*) How wide?—About a mile.

5990. (*Mr. Austen.*) You spoke of two workers in North-Western Rhodesia, Mr. Dolman and Mr. Eminson. By whom are they employed?—By the Chartered Company.

5991. With what object?—Following up the investigations of the bionomics of the *morsitans* fly.

5992. With a view to finding some means of getting rid of it?—Yes.

5993. Therefore the Chartered Company are fully alive to the importance of getting rid of the fly, if possible?—The Chartered Company are employing three men with that end in view.

5994. Including yourself?—Including myself.

5995. With regard to this area that you have suggested here, the Lukasashi Valley, you give the dimensions, and you say that it would have to be fenced, and the fence, I gather, would have to be from 52 to 60 miles long and 10 feet high, I think?—That is my estimate.

5996. What would be the approximate cost of such a fence in that position?—I think that Dr. May, with whom I have been in consultation, has prepared an estimate of the expenditure necessary.

5997. You have not the details yourself?—I have no details with me. I could merely give you the details which, of course, he will give himself.

5998. You say in your summary, I notice, that two villages would have to be removed from the area. Why would they have to be removed?—The question is whether they would be exposed to very grave risk if they were left among large numbers of hungry fly. In that particular area there is undoubtedly sleeping sickness, and if the fly, as it most probably would, commenced to feed on man, then they would be in danger of getting the disease. On the other hand, if they were moved the experiment would give us no evidence as to what would be the attitude of the fly, deprived of what is possibly its main food, towards man.

5999. No evidence of what would be the effect of the fly upon man if the villages were removed?—Yes.

6000. Has *Glossina morsitans* increased in numbers in North-Eastern Rhodesia in your experience in recent years?—Apparently it is continuing to spread. There is one small area over which it spread a distance of seven miles in the two years with which I was familiar with that area.

6001. That is within your own knowledge?—Yes.

6002. Has it actually increased in numbers in any area with which you are familiar?—No.

6003. The numbers appear to be roughly constant?—Yes, the numbers appear to be constant.

6004. You said, I think, in reply to another member of the Committee, that in the laboratory you could not get your flies to breed towards the end of the dry season. Which do you regard as the chief breeding season in nature—the wet or the dry?—On the high ground of the plateau the breeding season in nature is

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the warmer part of the dry season. There the temperature in the shade rarely rises above 90 degrees. In the Luangwa Valley the temperature frequently rises to 108°, or even more, in the shade, so that the climatic conditions are very different and the breeding habits in nature would probably be different. I have no evidence of what happens in nature in the Luangwa Valley, but on the high ground on the central plateau the breeding is almost confined to the warmer part of the dry season. The evidence is not yet published, but it is available.

6005. Do they breed in the wet season at all to any extent?—In searches in December and January and on to April I only found two live pupæ in nature. Those were found towards the end of January when the rains had been on six weeks.

6006. Only two living pupæ?—Yes.

6007. I gather from that that so far as your experience goes, the main breeding season at any rate is the dry season?—Yes.

6008. When the flies approach watercourses, as you say they do, perhaps with the game as the dry season advances, are they breeding then?—That I do not know.

6009. But have you found their breeding places near water at that time of the year?—I have had no opportunity of working in the Luangwa Valley since I began to understand the places in which the flies breed.

6010. Have you ever found *Glossina morsitans* in any area in which game was so scarce as to be practically non-existent?—No.

6011. You are aware, I suppose, that other observers have made statements to the effect that they have found *Glossina morsitans* in numbers in practically gameless country?—Yes.

6012. But that has not been your experience?—No, except this area near Broken Hill, where game is certainly scarce and fly is numerous. I have passed through that area and there is evidence of the presence of big game, but the game animals themselves are very rarely seen. That may be partly because they are extremely shy and partly because of their scarcity.

6013. Are the game animals in the area relatively scarce as compared with other areas do you think?—They certainly are. The place is close to a large settlement and a considerable amount of shooting goes on over the area.

6014. How about the fly in the area; is that relatively scarce as compared with other areas of the same natural physical character?—It is said to be amongst the thickest fly in Rhodesia at one point.

6015. The fly is thickest in Rhodesia there, although the game animals are scarce?—That is the statement which has been made to me. Dr. May will give you definite evidence on that as he is familiar with the area.

6016. Does *Glossina morsitans* feed readily on man or reluctantly?—Very readily.

6017. Have you noticed any difference in the degree of avidity with which *Glossina morsitans* will feed on a man and, say, a water-buck or any other big game animal?—When an animal has just been shot and men are standing round it, the fly is more attracted to the animal than to man. One is rarely bitten when standing over an animal recently killed, although the fly keeps feeding on the animal. That is the only evidence I could get on the matter.

6018. From what you know of the bionomics of *Glossina morsitans*, do you think it possible that this fly in course of time, by dint of necessity, that is to say, by force of having to feed on a certain animal, or a certain group of animals, because those animals are most numerous or most accessible in its district, acquires a natural predilection for those animals which might again disappear in course of time in favour of another animal or another set of animals if the first set of animals were removed?—That I do not know.

6019. I put that to you for two reasons, one of which is that it has always seemed to me that the alleged partiality of *Glossina morsitans* for the African buffalo

in Africa south of the Zambesi was probably due to the fact that the tsetse-fly and the buffalo were thrown together by force of circumstances. They both lived in the same places, and as Mr. Selous has said, the buffaloes were more numerous than all the rest of the game put together, and therefore by constantly feeding on the buffaloes the flies in those districts perhaps acquired a special predilection for that kind of blood; do you think that that is likely?—I do not think so. It is very difficult to say whether they have a preference for any one animal over another, except in the instance I have given of the antelope and man.

6020. I am not suggesting that they actually have an inherent preference, but that the force of habit during a certain time amounts to something, and that when the habit is broken by the snapping of one link in the circle, the habit may change and they may acquire a predilection for something else. You have not any observation to support that?—I have no observation to support that.

6021. Have you ever known *Glossina morsitans* to imbibe vegetable juices?—We have observed it in the laboratory to settle and thrust its proboscis into a ripe water melon, but by dissecting the fly afterwards, I was able to find no trace of the vegetable organism inside.

6022. So that the action of the fly in that case may have been merely occasioned by curiosity?—It may have been or it may be that in some way it can suck off a little moist air from such a position. It will apparently feed on a piece of wet blotting-paper in the laboratory, but if the fluid is stained and the fly is afterwards dissected, no trace of stain can be found inside. There, again, it looks as if the fly were able to suck up the moisture in the air rather than any actual fluid.

6023. You have no evidence that the fly normally feeds on anything but blood?—None.

6024. You say in your summary that the effect of undigested avian blood on pregnant flies caused their offspring to be smaller than usual?—Yes.

6025. Were the larvæ viable? Would they produce flies?—Very frequently a very small pupa and a deformed adult is produced.

6026. But can you say whether abnormally small larvæ, due to the mother having been feeding on bird blood, would give rise to a perfect fly?—Yes. There are instances in the box of pupæ which I pass round of deformed and very small pupæ which did produce perfect flies. The very small ones would produce perfect flies, but the deformed pupæ would produce deformed flies.

6027. You have got perfect pupæ, although small, from flies feeding on bird blood?—Yes, as small as 5 millimetres in length.

6028. Have you carried those on to the next generation, and ascertained whether or not the resultant flies could breed?—I have endeavoured to do so, but I only got one healthy larva in the second generation with the birds and one aborted larva. In the corresponding series which were fed on mammalian blood, there was no breeding at all. Those fed on birds were fed again with bird's blood; those fed on mammals were fed again on mammalian blood. In the mammalian series no breeding at all occurred. In the avian series, two flies became pregnant, one produced a healthy pupa and one produced an aborted larva which died. That was probably due to the effect of the season. At the time the experiments were made it was very cold indeed in the laboratory, and breeding amongst the wild flies was at a minimum also, so that would probably account for the mammalian ones not breeding.

6029. Do I understand that you carried the avian fly to the third generation? The fly which had fed on bird blood produced a living larva which in its turn gave rise to a fly?—Yes.

6030. So that so far as your experiments go (of course they are not complete, and they need to be carried further) we have no reason to suppose that *Glossina morsitans* would die out if the whole of the mammals in its area were removed, and the flies were henceforward restricted to a diet of bird

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blood?—That would really depend upon their ability to feed on birds, which is a very important factor. There is no doubt that they do, to some extent, feed on birds, but whether they could take their entire diet from birds or not is a different question. A bird is able to reach with its beak any part of its body, and when alert would undoubtedly pick off any tsetse that commenced to bite it, whereas the antelope is not able to knock the fly off from any part of its body.

6031. May not a bird be bitten and fed upon while asleep?—Certainly. As I said earlier in my evidence, that probably occurs to some extent.

6032. But at any rate, so far as your own experiments and observations go, a diet of avian blood on the part of *Glossina morsitans* would not necessarily lead to, if I might say so, race suicide?—It would not, so far as my experience goes.

6033. (Sir Edmund Loder.) All the country you were in, in Rhodesia, had some sort of game in it I suppose?—Yes.

6034. So you are not able to answer the question whether the fly can exist in country where there is no game? That did not come under your observation?—No.

6035. It has been suggested that clearings round villages and roads would assist in keeping flies from the inhabitants. Is it your opinion that it would help?—I think that some experiment should be carried out in clearing. One is hearing at present a great deal about it, but there is absolutely no evidence available.

6036. I suppose there are some villages where, not for this purpose, but for other purposes trees have been cut down; and is it not known whether the flies come into those villages as much as they do when they are able to hide amidst trees? Villages always are in some amount of clearing and trees hardly ever come quite up to the village. The fly rarely gets into the villages at all in any case. It is possible that it is the smoke in the village which keeps it out. There is something in the village which makes it unattractive to the flies.

6037. You spoke just now about a place with a mile of clearing where it was apparently quite safe?—Yes.

6038. So apparently less than that would be something and less than that would do something less. You have no idea of what number of hundred yards would be really of any value?—No.

6039. A mile you think is sufficient?—A mile apparently is quite sufficient.

6040. (Professor Newstead.) Was your camp actually in fly area?—Yes.

6041. Did you clear the bush or surround the camp?—At the Ngao Camp we had a clearing 120 yards long by 70 yards broad, and the houses were in the middle of it.

6042. Did the fly come into that area?—Frequently.

6043. Following yourself or your boys or voluntarily crossing it?—Last year it frequently came into my house and quite on its own initiative. I have had as many as seven or eight in an afternoon come in when I have been there.

6044. Would such days be bright sunny days or dull and rainy?—At that time the weather was bright and sunny. The fly is not very active in dull weather.

6045. You said that tsetse-flies rarely come into native villages, and you said also that you thought that possibly the presence of smoke (from burning timber I take it) might have some detrimental effect on the flies or might act as a deterrent?—Yes.

6046. Would you not think it was due to the absence of suitable vegetation?—It is a very difficult matter to say exactly what it is that keeps them away.

6047. There is a certain amount of land under cultivation, is there not, in the immediate neighbourhood of the villages?—Yes, that is usual.

6048. And the crops under cultivation afford a totally different shade-giving vegetation from that which you would get in a forest?—It would certainly be a different quality of shade.

6049. What is the exact extent of area over which you have continually conducted your investigations regarding the bionomics of *Glossina morsitans*?—At Ngao the work was confined to an area of some

25 square miles around the camp—about five miles in each direction.

6050. You personally conducted the major portion of the investigations, I take it?—Yes.

6051. That is to say, you did not rely upon the evidence which the boys brought into camp?—No; I never do under any circumstances.

6052. Have you noticed that *Glossina morsitans* is markedly gregarious during the dry season, and that during the rains it distributes itself over a much wider area?—I have not found it to be so.

6053. Not in any portion of the particular area in which you have conducted your investigations?—No; it has not been my experience at all.

6054. What was the relative number of flies present in any given area—I speak more particularly of those areas in which you found pupae?—In the particular piece of country in which I found most of the breeding places the fly was very dense indeed—exceptionally so.

6055. Could you say roughly how many tsetse-flies were present at a time?—No, that would be quite impossible.

6056. Would you say four or five surrounding one of your boys or would you put the number at 40 or 50?—It varies so from day to day that it would be quite impossible to say. I have not seen them in that area as described in some areas like a swarm of bees for instance. We did not get that in the area.

6057. Could you give us some idea as to the largest number of flies that were present in any one spot? Did you find that they were more attracted by khaki than by any other coloured garment?—Yes, they appeared to be so. They appear to be more attracted to a khaki jacket than to a black one, and white they seem to dislike.

6058. White acts more or less as a repellent to them?—Yes.

6059. I want to get from you, if it is possible, some information regarding the relative abundance of the flies in this particular area, because as one travels through a fly area one notices day by day that there are a certain number of flies present on your porters or on your men or on your companions. Is it not possible to say whether there were 4 or 5 or 20 or 30 or 40?—I have made no notes on the matter and therefore I cannot make any statement, because my recollections of different days confuse themselves in my mind. I remember days when they were extremely numerous, and other days when very few were seen. It depended upon the weather conditions.

6060. I am very anxious to get some information on that point, because when I traversed a portion of Nyasaland myself I found there that the maximum number of flies present on one person at different times was about eight or nine, but rarely more than twelve. Would you consider that a fair average number of flies?—That would be about an average.

6061. Did you find pupae in districts where you got such a high average more abundant than in districts where the average was lower?—My continuous work in searching for pupae has been confined almost entirely to this one small area of two square miles about which I have spoken. I have found them also on the Kafue River, where the fly is much scantier. I was accompanying Mr. Eminson.

6062. There were two or three flies present at a time?—Yes, it was a very common experience on a suitable day to see no fly at all.

6063. You have found pupae of *Glossina morsitans* in a large number of different places. They occurred in varying numbers, I think you said, and you found an average number of 3.25 present on each of these various sites?—Yes. But that number referred to 123 positions generally scattered. If those near the paths, &c. are included, the average of the whole is about 12.

6064. I think in your earlier experience you also found a number of pupae of this particular species of tsetse-fly in a rot hole which was of a rather peculiar form. Unfortunately I have not with me the picture, but you will probably recollect it. It is a cup-shaped cavity fully exposed to the light, so far as one can judge from the photograph, and during rains it would

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form a natural sort of rain gauge, and would hold up the water, I take it, would it not?—Yes.

6065. Did you find pupæ in that particular rot hole during the rainy season?—No. I searched that particular cup several times last year and found no more pupæ in it but a number of empty cases.

6066. You would infer from that, then, that the larvae were dropped in that particular recess during the dry season?—I should imagine so, because in the wet season quite a pool of water was formed inside it.

6067. And naturally, of course, the pupæ would die if they were placed there?—Yes, they would.

6068. Did you find any pupæ at all in places which would be accessible to gallinaceous birds?—Yes. In some cases the larvae are dropped on to ground which is extremely hard and the larva in its efforts to find somewhere to hide has been known to creep a distance of 18 inches away from the shelter of the tree. Then again some of the trees are raised as much as two or more feet above the surface of the ground, and any bird would be able to scratch below them.

6069. From your experience, you, I take it, assume that a number of individual flies would deposit their larvae in one spot?—Yes.

6070. The pupæ you find in such places are not the progeny of one particular individual?—No.

6071. Did you collect those pupæ?—Yes.

6072. Did you breed out the flies?—I bred out some of them, not all, because I had to leave the place in which I was working and go to another part of the country, so that I could not follow the work up entirely.

6073. Did they hatch out more or less simultaneously or did they continue to hatch out over a relatively long period?—It would have been over a long period. I had many of them under observation for one month and they had not hatched out.

6074. Some had not hatched out then?—No.

6075. What is about the average period?—I did not follow that work out with these particular pupæ collected in nature long enough to make an estimate.

6076. You made some reference to parasites. When you had those naturally found pupæ in the laboratory, did you find any parasite hatch out from them?—Yes; one Bombyliid fly emerged.

6077. But no Chalcidid parasite?—No.

6078. Do you think that shade is necessary for the pupæ?—I think not for the pupæ, but the fly only rests in shady positions. They are frequently in such places that for some hours of the day they get the direct sun. The general forest is of a shady nature, but the individual pupæ may be in what one may describe as sunny positions.

6079. (Mr. Buxton.) I would like to have your idea a little more about the practicability, from your investigations, of exterminating the insects over a certain area. What is the area shown by this plan of your's?—That is about two square miles in area.

6080. From your experience, if you were set to do it, do you think you could exterminate the pupæ laid there at any given period?—It would be possible, I

think, to destroy all the breeding places in that area but it would mean an immense amount of labour.

6081. Do you think that carefully as this plan is prepared by you, you may have missed some breeding places?—I think it probable that I have only found a small proportion of those that actually exist, because a tiny little branch may shelter a number of pupæ.

6082. Having carefully marked the spots you could exterminate, I suppose, the pupæ found there?—They could, of course, be collected by hand, but the number which I found in July and August, which was about 700, must have been very small compared with the number which were actually in the area at the time.

6083. If it would be so difficult in a small area such as that, it points to the extreme difficulty of general extermination, does it not?—It would be extremely difficult.

6084. (Chairman.) You say, if you remember, in your memorandum rather positively, "Flies are unable to breed on a diet of reptilian blood." Is it not a matter of experience that any experiments with captive flies as to breeding must be highly fallacious?—I think it may be so, but reptilian blood seems to be entirely unsuited to the fly compared with other bloods.

6085. Why?—Because of the rapidity at which they die off if ever fed on reptilian blood.

6086. I follow that, but do you feel quite as sure as you suggest here that that would happen in nature, and that they would die as speedily on reptilian blood?—Apparently they would, if confined to that diet.

6087. I do not say they may not, but do you really feel confident that that which you found happened in the laboratory could at all assuredly be said to be likely to happen in nature? Can anything of that sort which happens in captivity be taken as a guide?—Not with absolute certainty. The conditions of nature, of course, are different from those in the laboratory.

6088. Rather on the same ground, I would like to ask you a question about your alternative experiment of a fly-proof cage—could you really draw inferences with any safety from what happened to a fly living under such very artificial conditions?—The cage would, of course, be very large.

6089. That does not quite meet my point. There are many things in the natural life of a fly (we may know them or may not know them) which one would expect to be very different from life in a fly-proof cage. I only want to know whether you really think, assuming certain things to happen, when an experiment is tried, you can rely on the results. I want your opinion about it, that is all.—Perhaps if the details of the experiment were more fully known it would answer the question. The experiment consists of two parts. In the first part the flies are enclosed in the cage with a fauna of small animals and birds. In the event of their disappearing the experiment would be repeated with the introduction of larger animals.

6090. I follow that, but could you with artificial conditions like that rely upon what happened as indicating what would happen in nature?—I think so, if the fly disappeared at first and with the introduction of the larger animals was able to breed.

The witness withdrew.

Mr. AYLMEER MAY, M.D., called in and examined.

6091. (Chairman.) You have been good enough to send us from time to time reports, and we have read them with great interest. They are of great importance on the question we have to consider. I propose to put a few general questions to you, and any other questions will be asked by other members of the Committee. You have now had a very large general experience of the incidence—I will not say the spread—of sleeping sickness?—Yes, since 1909.

6092. Am I right in supposing that in the district in which you are the fly you have to deal with is *moritans* and the trypanosome *rhodesiense*?—We have had both varieties of sleeping sickness to deal with. The *gambianse* is finished; the measures taken have proved successful and there is no more of that disease.

The only active disease at present is the *moritans*-transmitted disease.

6093. Am I right in saying that the recent reports that you have been good enough to furnish relate to *moritans* and *rhodesiense*?—Principally.

6094. You have watched it very closely for a long period. Do you think it—the disease itself in man—is increasing, stationary, or decreasing?—It is slightly inclined to decrease. The number of cases found this year have been less than the number found last year.

6095. There is no very substantial change, as I understand?—No. The proportion to population this year is less than 1 per 1,000. It was slightly over 1 per 1,000 last year.

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[Continued.]

6096. What is the population on which you are basing your calculation? I would like to get it on the notes.—Approximately 100,000 people. The population actually individually examined amounts to about 40,000 people.

6097. The population in the country in which you have searched for sleeping sickness is about 100,000?—Yes.

6098. Do you think from the habits of the natives that many cases are concealed from you?—No, I do not think that any cases are now.

6099. If we are rightly informed, the disease that arises from *morsitans* and *rhodesiense* is a very virulent disease, in which symptoms of great violence appear in a short time?—Yes. Medical examination is only practicable once a year in the dry season, but the census returns which are made every month by the different district officials account for all deaths which take place, and it is possible to judge roughly how many have happened as the result of sleeping sickness.

6100. Although you have only examined 40,000 people my point is, that if there had been 60,000 or 80,000 cases would you have heard of many or all?—Every one of them.

6101. Therefore, the whole population does form a guide?—Yes.

6102. What is the whole population of Rhodesia?—Northern Rhodesia?

6103. Yes.—I think it is something under a million natives.

6104. Do you have reports from there as to the spread of sleeping sickness?—Yes, from every district.

6105. Do they confirm the view that you have expressed?—Yes. I do not know if it is understood that *morsitans* areas do not cover the whole of Northern Rhodesia.

6106. No; I quite follow that. Taking that population, can you give me at all in round numbers the number of cases that you have detected of sleeping sickness?—*Morsitans*?

6107. Yes.—About 120 since the start of the disease.

6108. Since when?—Since the disease was first discovered.

6109. What was that date?—1909.

6110. That is $4\frac{1}{2}$ years. Then by what figure should you multiply that number to give us an estimate of the amount of disease, including cases of which you do not hear?—I think you would be within the mark if you multiplied it by three, perhaps.

6111. How does that mortality compare with that from other local diseases?—It has been very much less than the mortality from smallpox for the last two years.

6112. Is it a country where there is much malaria?—Yes. Every native is infected, but it is very difficult to give any estimate of what mortality occurs as the result.

6113. Is there much syphilis?—Yes, in places.

6114. From what you have learnt and from what you have seen, do you think the disease is a new disease, or has it been endemic there for years?—I think it has been endemic for years.

6115. Under what conditions, if any (of course you cannot prophesy) do you see any probability of epidemic conditions arising?—I can see none.

6116. You cannot suggest any particular circumstances that would be likely to lead to an epidemic form?—I fancy that the present state of the disease is due to the relative immunity of the population. If that should break down no doubt there would be an epidemic. I do not know what would break it down.

6117. Do I understand your answer to me that you are of opinion that a large number of men are in fact infected, but no evil results follow?—I do not know; I am not able to say.

6118. So far as your experience goes, have you found cases where there was no disease, but there have been trypanosomes in the blood?—No, never.

6119. That is probably because you have not looked for it. You would not look for that, would you?—I do not quite follow the question. Do you mean that

the human being can carry the trypanosome without showing signs?

6120. Or without acquiring the disease?—I know of no such case; I do not think that is possible.

6121. I am really following up your own answer. You said you thought that there was comparative immunity, and if you acquired immunity you resist that which would cause the disease. Do you mean that men are largely infected, and that no consequences happen? It might be that or it might be that men were not infected at all?—I think it is possible that they may be infected, but not to an extent which is demonstrable. An infected fly may possibly bite an individual and the immunity acts almost instantaneously and prevents the contraction of the disease. I do not think there would ever be a stage when the trypanosome would be in the circulation with no disease resulting.

6122. Generally on that head, I take your view to be that the disease does not loom very large at present in the bill of mortality?—No—very very small.

6123. To the best of your judgment it is an old disease and is endemic?—Yes.

6124. With regard to the cause of the disease when it arises, that is the conveyance of the trypanosome to man, you have yourself worked with trypanosomes, have you not?—Yes.

6125. And no doubt you have worked with the trypanosome which I will call for the moment *brucei*?—Yes, but not to any very great extent.

6126. You have worked with the trypanosome that we call *rhodesiense*?—Yes.

6127. Can you distinguish between the two in any way?—No.

6128. Either morphologically or experimentally?—No.

6129. Have you experimented with both?—I cannot say that I have personally, but I have seen experiments carried out by Dr. Kinghorn.

6130. Speaking quite broadly from what you have done yourself and from what you have learnt, do you consider that the two trypanosomes are identical?—*Rhodesiense* and *brucei*?

6131. Yes. Are they the same in your opinion?—Yes, I think so.

6132. You think they are the same. I put it to you as it is a question which has been very much discussed; they being the same, would your theory of immunity alone be sufficient to account for the fact that where wild game are found tolerant of this trypanosome (speaking broadly for the moment), practically domestic stock are always destroyed and man suffers hardly at all?—I do not quite follow you.

6133. There is a trypanosome which, if it be *brucei* vel *rhodesiense*, would produce nagana in animals and sleeping sickness in man. Where you find that trypanosome in large quantities in the blood of wild mammals and you find that cattle and domestic stock always died and that man hardly suffers at all, is the doctrine of the comparative immunity of man sufficient to account for that great difference?—I think so.

6134. You think it is sufficient?—I think so.

6135. You are, no doubt, aware of Dr. Taute's experiment?—Yes.

6136. I suppose you say that an experiment on one man cannot be conclusive?—It cannot.

6137. In a sense, is not that experiment in the manner I have indicated being constantly repeated, as men are constantly being bitten by flies with regard to which there is the same likelihood of infectivity as with regard to flies that bite domestic stock, while domestic stock die and man suffers not at all? Would not that primarily (there may be other explanations) point to the trypanosome of wild game being a trypanosome pathogenic to cattle but not pathogenic to man?—Yes, I think so. The proportion of flies infected has been demonstrated roughly to be 1 in 500. One does not know how often a man is bitten, but nothing like as often as the ordinary domestic animal.

6138. Is that so?—I do not think that any individual who can protect himself is bitten to the extent that the domestic animal is.

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[Continued.]

6139. Am I right in supposing that the domestic animal, except when travelling, is generally kept in a comparatively cleared area?—Yes.

6140. And is it also the case that men have constantly to leave the cleared area and work in the bush on their business and are more exposed?—But they adopt to some extent preventive measures against the bites.

6141. Do the natives?—Yes, they all swish the flies away, which is the only preventive measure to adopt.

6142. How far does that take us? Is the native very seldom bitten?—I did not say he is very seldom bitten, but he is not on a parallel with the domestic animal, which may be bitten a hundred times in an hour, whereas if a native was bitten 10 or 12 times a day that is about as much as he would be.

6143. Does that apply to domestic stock tethered?—No, it would not.

6144. You are aware that an experiment has been suggested to test mainly the relations of wild mammals and fly—putting it quite shortly. I will give you the details of the experiment in a moment. The main object that everybody must have in view would be the elimination of the fly?—Yes.

6145. That is the carrier without which the disease could not be conveyed?—Yes.

6146. Are you of opinion that such an experiment as is suggested, if a suitable place could be found, should proceed or that we should first follow further entomological research as to the bionomics of the fly?—I am of opinion that further work should be done on the life history of the fly before the experiment be attempted.

6147. On the other hand, to put it in another way for the moment, might not the results of the experiment (I do not say they would) be such as to be of assistance to entomologists embarking on further inquiry?—Yes.

6148. There are arguments both ways, I suppose?—It would be the more expensive way of going about the necessary work in connection with the bionomics of the fly.

6149. From what you have learnt so far of the results of entomological research, are you yourself hopeful of facts being ascertained that might lead to the possibility of the destruction of the fly?—Moderately hopeful.

6150. Has that side of the work, in your opinion, been sufficiently developed so far?—I do not think it has. I do not think anything has been done really except on a very small scale in Northern Rhodesia as regards *morsitans*.

6151. Should you be hopeful of really valuable results if considerable attention were systematically devoted to that side, and the representatives of foreign governments in East Africa consented to work on similar lines?—I should.

6152. It would be desirable, would it not, that any entomological research should be over as extended an area as possible, and naturally that as many people as possible should be employed on the work?—Yes.

6153. Next to the elimination of the fly, its banishment from inhabited villages, roads, passes, and fords is the best thing, with present knowledge, is it not?—Yes.

6154. As to clearing (I will limit myself to *morsitans*) having regard to the habits of the fly in the bush, do you think that it could be effectively done firstly, over a considerable area round villages; secondly, by roads, passes, and fords and places frequented by people or cattle? I am not asking for the moment about the flight of the fly but as to the actual labour, and the possibility of clearing to any large extent. Would it be beyond possibility?—I think that clearing, if everything is favourable, if expense is not a consideration and labour is plentiful, is an effective measure, but it is a measure which can only be taken over the enormous areas with which we have to deal at enormous expense

with a very great amount of labour, and it has to be done at least two or three times a year to be effective.

6155. As often as that?—Certainly twice a year.

6156. Would that be the case if, after clearing, there was burning?—Possibly after a thorough clearing by stumping and burning it would not require further attention for a year, but after that I think you would have the same trouble and you would have to go through the same process twice every year.

6157. You would not like to commit yourself to a cost per acre, I suppose?—No, I should not; it varies so much with the price of labour.

6158. Then as to clearing by-roads, because that is a matter of great importance, do you think it would be possible, so to speak, to schedule main roads and make all stock travel by those roads, when cleared, as a matter of administration?—I think matters could be improved by doing it, but I do not think it can be guaranteed that there would be no fly and no infection. The fly would be very much reduced.

6159. So far as funds were available, am I right in saying that, on present knowledge, firstly the destruction of the fly, secondly, clearing, would be the best course to pursue, and so far as the experiment might assist towards further knowledge the experiment would be of great value? Is that fairly correct?—I do not quite follow the question.

6160. I mean an experiment to test the connection of fly and game in a selected area by exterminating or expelling game?—And clearing at the same time?

6161. I am taking the three. The first object is to get rid of the fly?—Yes, that is the first object.

6162. Pending that, clearing would be of the greatest value, with our present knowledge, would it not?—Yes.

6163. Then as regards the experiment, if it contributed to that knowledge should it not be undertaken as soon as may be?—Yes, certainly.

6164. At the same time?—Yes.

6165. (Mr. Buxton.) I would like to ask you a question about the immunity or partial immunity. Would that be the same sort of immunity as human beings who have been inoculated against smallpox have? Is it in that sense that you use the word?—I use the word as a conjecture altogether. The exact nature of immunity against trypanosomiasis is not known, I think.

6166. Exactly; but do you look upon it as analogous to the immunity which, apparently, the game animals enjoy?—Yes.

6167. In their case it is proved that they carry *brucei* in their blood?—Yes, quite so.

6168. That the trypanosomes apparently thrive?—Yes.

6169. Then in your opinion there must be some difference between the action of the blood upon the trypanosome in the case of man and in the case of animals, because I think you said that you did not believe that the trypanosome could exist after a human being had been bitten by an infected fly?—Without his showing the disease?

6170. Yes, without his showing the disease.—I do not think so.

6171. Do you draw a distinction between the effect on an immune, or partially immune, man and a game animal also immune?—Yes. There must be that distinction. I do not know what the nature of the immunity in either case is.

6172. Is it possible that the human beings who are bitten are exactly in the same way immune? What are the facts upon which you found the distinction that you draw? Have you examined the blood of healthy natives to such an extent that you can confidently say that there are no trypanosomes in them, and that they only exist in diseased cases?—I can confidently say that in no case in which I have found trypanosomes is the disease absent, and I have examined thousands of natives' blood.

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[Continued.]

6173. I think you said that 40,000 in the area you are dealing with have actually been examined?—Yes, this year.

6174. What is the nature of that examination?—Palpation principally (gland palpation) and all doubtful cases have their blood examined.

6175. (Professor Newstead.) Have you had any personal experience at all with regard to clearing?—Yes, with regard to clearing for *Glossina palpalis*.

6176. Have you had any experience at all with regard to clearing for *Glossina morsitans*?—Very small experience of artificial clearing, and a large experience of natural clearing with regard to *morsitans*.

6177. By natural clearing what do you mean? I do not quite understand.—Take a country in which bush is present. For instance, you may have two or three square miles of open country absolutely free from *morsitans*, and absolutely surrounded by bush and thick *morsitans*.

6178. *Morsitans* is absent in the open area?—*Morsitans* is absent in the open area.

6179. You have done some clearing in the *morsitans* area?—Yes, in the immediate vicinity of European settlements and native villages.

6180. Did you find it necessary to clear a piece twice a year?—Quite necessary.

6181. What was the nature of the vegetation of that particular area?—The ordinary bush that occurs all over Northern Rhodesia.

6182. What are the kind of trees, for instance, that preponderate in a typical fly country?—They are very varied, I am afraid. I do not know their botanical names.

6183. Or their native names?—No.

6184. You have cut down different kinds of trees?—Yes.

6185. And the result has been that the second growth which has arisen from the stumps has afforded sufficient shade for tsetse-flies?—Yes.

6186. And you have found it necessary to repeat the clearing?—Yes.

6187. That is in typical forest country?—Yes.

6188. The type of vegetation is markedly different from that which you get in a *Glossina palpalis* area?—Yes.

6189. You probably know that the vegetation grows much more rapidly along the river courses in *palpalis* areas after it has been cut down, than is the case in *morsitans* areas?—I do not know that it is so. At the beginning of the rains, vegetation starts. I think it grows as quickly on the river bank as elsewhere or vice versa.

6190. (Sir Edmund Loder.) You have been treating the natives, I suppose, who have been ill from sleeping sickness?—Yes.

6191. Have you had any success or any cures?—Not with *morsitans*-transmitted disease, we have never had a recovery from this type of trypanosomes.

6192. We have been hearing about some newer treatment up the Nile and in the Lado district. Have you tried any of those?—We have tried atoxyl and salvarsan (606).

6193. But you have found nothing of any use?—We have had encouraging results to some extent but no recoveries.

6194. With regard to *palpalis* have you got on better?—Yes; that yields very readily to atoxyl treatment.

6195. You make great distinction between the two?—There is a very great distinction.

6196. (Mr. Austen.) Referring to your examination of the 40,000 natives, I gather that in the cases where you found gland enlargement you invariably found trypanosomes of some kind?—No. Gland enlargement is simply looked upon as an indication for further examination; the great majority of gland enlargement is due to other causes.

6197. You told us that you never found trypanosomes without finding the disease?—No.

6198. In how many cases did you examine the blood of natives?—I personally did not examine the

40,000 natives this year. That is the result of the work which has been done in the area this year.

6199. By your medical officers?—Yes, by various medical officers.

6200. In how many cases out of the 40,000, has the blood actually been examined? Can you give the Committee any idea?—Roughly in about 20 per cent. I gave a report just now to the Secretary which shows.

6201. That leaves 80 per cent. in which no blood examination has been made?—Yes.

6202. I gather that you consider that the bulk of the population is probably immune. That being the case why should not the immunity (the Chairman put something of this kind to you) be of such a nature that human beings are actually tolerant to the trypanosome and maintain the trypanosome in their blood without contracting the disease, just as wild animals can have trypanosomes in their blood without contracting disease?—In the work which has been done in the last seven or eight years I think that we should certainly have found a good many cases of trypanosomiasis in healthy natives, if they were carrying trypanosomes and not showing signs of disease.

6203. There has been a sufficient amount of blood examination to lead to that conclusion?—Yes, blood examination is constantly going on.

6204. Dr. Yorke, I think, found a native in whom trypanosomes were present in the blood, but who did not develop the disease. He was perfectly well, was he not, a year later? Dr. Yorke mentions that in his final report published in the "Annals of Tropical Medicine" of Liverpool University?—I do not know that case.

6205. You have not met with any similar cases?—Never. Has Dr. Yorke followed up that case? I am rather under the impression that every native who has been diagnosed as carrying trypanosomiasis is either dead or dying.

6206. That is what Dr. Yorke said in the first instance, and in his last report he mentioned the other case.—Cases of the disease in the early stages have been found in which there is very very little sign of illness, but they have got rapidly worse, and in no case have trypanosomes been found without either at the time, or subsequently, the disease being very evident.

6207. Dr. Yorke himself says that the man was to all appearance perfectly well a year later. He says so in practically so many words.—I am sorry to say I do not know that case.

6208. In the summary with which you supplied the Secretary I notice you say, "the question being still unsettled as to whether the 'fly,' game and human trypanosomes are identical, though similar morphologically" and so on. You further say, "pending the discovery of a means for the identification of the trypanosome the destruction of infected stock is considered advisable." That is dated the 31st January. Have you changed your opinion since then, because I gather from what you told the Chairman just now that you consider that *Trypanosoma brucei* and *Trypanosoma rhodesiense*, so called, are one and the same trypanosome?—That means that *rhodesiense* is not to be identified. If that trypanosome is called by two different names, *brucei* and *rhodesiense*, and there are no means of distinguishing which it is, then the *rhodesiense* cannot be identified.

6209. I do not quite follow your point, I am afraid. I have not the slightest doubt that you fully realise that it makes an enormous amount of difference whether we are dealing with a trypanosome which is present practically in the blood of every game animal, or with a trypanosome which perhaps occasionally occurs in a buck here and there, but is far less common than the ordinary game trypanosome?—My point is that under certain conditions that trypanosome may be pathogenic to human beings and under certain conditions non-pathogenic, but there are no means at present of discovering which the trypanosome is or whether it is pathogenic to human beings or not.

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[Continued.]

6210. To follow out that line of thought we thought we might reduce the total number of actual species of trypanosomes to two—perhaps *gambiense* and *brucei* shall I say—which would include all the others in their respective groups?—Yes.

6211. And the other names of trypanosomes would merely be applicable to phases. Is that what you think?—Yes, that is what I think.

6212. There is a *Trypanosoma equiperdum* which produces dourine, a disease not acquired through the agency of a biting fly, though the trypanosome causing it is morphologically indistinguishable from *Trypanosoma brucei*?—Yes.

6213. Is not that rather an awkward point with regard to your opinion?—I do not know that it is awkward. We must admit that we know very little about the trypanosome.

6214. I quite agree, but at the same time we want to clear our minds as far as possible, because if we consider that there is in this case only one trypanosome which is generally present in the blood of big game and may at any moment become lethal to man, though usually it is innocuous, then it is a far more serious thing than if we have two trypanosomes, one of which is innocuous to man and another, a far rarer one, is harmful?—That is the serious view of the case.

6215. Since we are now dealing with a disease which has been found in half-a-dozen different localities in Africa, is it not somewhat curious that that should be due to the trypanosome which we have known—I was going to say all our lives, but at any rate for the last 18 or 19 years, as the *Trypanosoma brucei*?—I think that considering the extent to which the disease has been found up to the present, it might easily have escaped notice.

6216. In different localities?—Yes.

6217. You think its occurrence in Southern and North-Eastern Rhodesia, Portuguese and German East Africa and Nyasaland is due to the fact that it has been looked for?—Yes. There are greater facilities for finding it.

6218. Yes. In the old days of the pioneer and the elephant hunter the man who went into elephant country for the purpose of money making carefully calculated the cost, because he knew he would have to go into fly country. He reckoned on losing all his horses, all his trek oxen and his dogs, and on getting a certain amount of ivory, and he balanced the two; but he never reckoned on losing his own life?—He sometimes lost his life from fever which may have been due to trypanosomes.

6219. I agree.—He did not recognise the fact that the fly was a danger to him.

6220. There was no general mortality amongst those men?—There was possibly or probably just as much as there is from trypanosomes in human beings at the present time.

6221. You think so?—Yes.

6222. Surely the older elephant hunters and men of that time were more exposed to being bitten than the modern settler, were they not?—I do not think very much more. Everybody naturally takes means to prevent being bitten if possible. It is disagreeable. The incidence amongst Europeans at present, considering the number of Europeans who run the risk of infection, is extremely small.

6223. Mr. Selous did a certain amount of his hunting clothed in a shirt?—Yes. You must remember that the disease, owing to climatic conditions, as is proved by Dr. Kinghorn, is not transmitted during the season in which most of the hunting is done.

6224. In the cold weather?—In the cold weather. One may say with a certain amount of conviction that you are perfectly safe not to contract sleeping sickness in fly areas at a certain altitude and that is where most of the hunting is done, during the months when most of the hunting is done you can go with almost perfect safety on to the plateau of Northern Rhodesia between June and August and hunt and be bitten, but not contract the disease.

6225. I do not want to pursue the subject any further, but I will ask you one more question with regard to it. You say that this trypanosome which has brought this Committee into being is really after all nothing else than the *Trypanosoma brucei* with which Sir David, Surgeon-Major Bruce as he was then, worked in 1895 in Zululand. It is that trypanosome which here and there sporadically, or in certain seasons, acquires properties which are pathogenic to man. That is your view?—Yes, that is my view.

6226. Mr. Lloyd said you could give us some particulars about the cost of fencing the area in the Lukasashi Valley to which he drew our attention. He describes the area as being about 20 miles long and 6 to 10 broad, and he says that there ought to be a stockade 10 feet high round it. That would mean 50 to 60 miles of stockading?—The estimate must be very approximate. I put it down in my report at 50l. a mile.

6227. Three thousand pounds. What would be the material of the fence?—The ordinary pole fence with one strand of wire.

6228. Local wood?—Local wood.

6229. Would that include trenching or digging of any kind?—No, 50l. a mile would not. A somewhat modified form of fence has been suggested with a trench to prevent the animals returning into the area. Trenching would be the most expensive form.

6230. Mr. Lloyd said you could give us some particulars about the Broken Hill district in which he said that big game, as far as he knew, was relatively scarce, although the fly was quite common. Can you tell us anything about that?—Yes. I can give two instances of very thick fly in the absence of game. Broken Hill is probably not the best of them. The other is a traction-engine road which runs from a copper mine called the Kansanshi Mine in North-Western Rhodesia into the Belgian Congo to the railway. It is 80 miles long. I travelled over it about six weeks ago. There was no evidence of game of any sort or description anywhere near the road, and there was no day on which fly was not very plentiful and very vicious. No case of human trypanosomiasis has as yet been traced to this area. The altitude of this district is 5,000 feet and upwards.

6231. That is in North-Western Rhodesia?—It extends from Rhodesia into the Belgian Congo.

6232. Are you sure that there was no game there?—I have my own observation to go by. I hunted very hard for it, and I have the report of all the people. It is a road on which there is a good deal of traction-engine traffic and native traffic. I questioned everybody who knew the road; with the exception of one reed-buck, which had been hunted on to the road and killed on the road by wild dogs, no game has been seen on the road or near it for two years.

6233. Are there any small mammals, duiker or pig, or things of that sort?—I think I saw one duiker, but practically no evidence of game; no spoor.

6234. That is very interesting. With regard to the Broken Hill district are the conditions very similar?—With the object of clearing a road which was wanted for native traffic, we had recently in the Broken Hill district two extensive game drives embracing a very large area, and it was very thoroughly done. The result of one was a duiker, and the result of the other was two pigs in thick fly.

6235. What fly?—*Morsitans*.

6236. The fly there is quite as numerous, I take it from your statement, as it is in other areas where game also exists?—No. At Broken Hill it is not very numerous. It is not as numerous as in many other places, but it is quite enough to be a trouble and a danger to domestic stock.

6237. What do you consider is the value of the game to Rhodesia? Do you consider it an asset, and if so, in what respect?—Financially an asset?

6238. An asset of any kind. You say here, I notice, "The general extermination of game is considered to be neither feasible nor desirable." Therefore you

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[Continued.]

consider the general extermination of game would be undesirable. Would you mind telling the Committee why you think it would be undesirable?—Well, I consider it would be a pity to lose it without any benefit resulting.

6239. I am very glad to hear you say that—I cordially agree—but I hoped you would point out some substantial reasons for keeping the game.—I do not know that it has any money value beyond a certain amount of commerce it brings into the country in the way of shooting parties and the sale of shooting licences and that sort of thing. I do not know that it has any other commercial value.

6240. It has been suggested that it might be possible to utilise the eland as a draught beast. Do you think that that is feasible?—Yes, I think so. As a matter of fact at Kalomo they had or have a pair of eland which were quite useful.

6241. It has been urged that it is economically unsound to replace the local mammals by imported mammals which are naturally not so capable physically of existing in those localities as the mammals on the spot?—I think the eland is the only animal which could ever be turned into a draught animal.

6242. It has been done by the Belgians with elephants, has it not?—Yes, but elephants are very scarce.

6243. (Mr. Rothschild.) You said in answer to the Chairman that you had made use of salvarsan and atoxyl, and that there was very little result except in the case of *palpalis* disease. We have had some foreign answers to sets of questions which the Committee sent out in which it is said that in the *palpalis* disease in the Belgian district, they had had much better results with a preparation of iodine, and that they had had very serious after results with the use of atoxyl. Have you heard anything about that?—No, I do not know that. We have had no bad results from atoxyl; in fact we have had very good results. I know nothing of the iodine treatment; at least, I have not tried it.

6244. Now with regard to the experiment of fencing in a certain area, do you not think that the experiment would be quite vitiated, however wide an area you cleared, from the fact that you could not prevent big game animals from approaching the fence from outside and so bringing in outside flies?—It would depend on the extent of the area included in the fence. If the fly is dependent on the game, but it will go away again, I think, with the game.

6245. But we have been told that it would be very difficult to clear any fairly large area of small animals, such as duiker and pig, and that even if we kept most of those out, there are all sorts of small mammals which sleep in the daytime in exactly the places where the flies would be able to bite them during their sleep?—Quite so. That is, I think, one of the fallacies about the experiment.

6246. (Dr. Chalmers Mitchell.) I think you said something about having to multiply a number by three.* Did you mean that only about one in three of the probable actual cases were discovered? It is a guess, of course?—We all realise that there may be cases that have not been discovered, and as an outside figure I think that you are perfectly safe in multiplying the discovered cases by three in order to get the total infection.

6247. Now the population of 100,000 is, of course, the population in the fly area?—It is the population in the fly area which is immediately under observation at present.

6248. You have had both *palpalis* and *morsitans* in your areas?—Yes.

6249. How do you distinguish between them?—Do you mean clinically?

6250. When you have a case how do you know which it is?—We presume if it occurs in a *morsitans* area that it is *rhodesiense*.

6251. You have had both in your district, have you not?—The *palpalis* disease has been confined to the River Luapula and Lake Tanganyika.

6252. You have no *morsitans* there?—There is no *morsitans* in immediate contact with the river or lake. In the country in which the *rhodesiense* disease is found there is no *palpalis*.

6253. Have you any evidence of any kind—I gather you have not—of the attenuation of the disease in the last three or four years? I mean to say is each case just as serious now?—Yes.

6254. That is what I gathered. Now you have been pressed a good deal on the question of the possible identity of *rhodesiense* and *brucei*, and you understand why we have been pressing you?—Yes.

6255. Sir David Bruce and Mrs. Kinghorn and Yorke take one view, and pretty well everyone else takes another view, about the identity of the two. You are rather inclined to take Dr. Kinghorn's view?—Yes.

6256. That I follow. Then you speak of the immunity of man to *rhodesiense* or to *brucei*?—When I speak of the immunity of man I put it forward as a conjecture as to the reason why, under very favourable conditions, the disease is not spreading.

6257. That is exactly what I understood from your answers, and I simply wanted to confirm it.—There is no evidence for the immunity beyond Dr. Taute's experiments.

6258. But that was the other way?—Dr. Taute infected himself with *rhodesiense*.

6259. That is the point, did he? He infected himself with a thing which he took to be *brucei*, and which he believed to be *brucei*.—He infected himself with a post-nucleated trypanosome which he found under the conditions in which *rhodesiense* is found.

6260. Under the conditions in which *brucei* is found?—Quite so—under the conditions in which both are found.

6261. You understand I am only trying to put before you the point (I think it is as clear in your mind as it is in ours) that immunity is not a scientific theory, but is a possible explanation, if it is true, of the difficulty of the position?—Quite.

6262. It is not a scientific theory at all?—Exactly.

6263. There is in fact no scientific evidence that man is immune to *rhodesiense*?—No, none that I know of.

6264. Thank you very much. I believed that that was the view that you were taking, but I wanted to be quite sure of it.—Exactly.

6265. (Dr. Chapple.) Did I understand you to say that you have solved the problem with regard to *gambiense*?—In so far as we have no continuation of the disease.

6266. How do you account for that?—By the movement of the population.

6267. You have taken them away from the infections?—The country in which infection is possible has been depopulated.

6268. The infection still remains?—The fly still remains.

6269. Not the infection?—I am not in a position to say that. We have no means of finding out.

6270. If you sent the people back would you expect them to be reinfected?—Yes, I should.

6271. Do you call that a satisfactory solution of the problem?—I am sorry if I said that we had arrived at a solution of the problem. What I meant to say was that, as far as the disease is concerned (*gambiense*), we have no further trouble.

6272. But still you are not content with that as a solution surely?—No. From the point of view of repopulating the country, if that be necessary, further work must be done with regard to it.

6273. Do you think that the *palpalis* area is increasing and may follow the population which you have withdrawn?—No, it cannot possibly do that, because the population has been moved to districts in which *palpalis* life is not possible.

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[Continued.]

6274. You have taken it away from water?—From all big waters.

6275. Is not that a considerable sacrifice to make?—It cost money originally, but the natives at the present moment are very much better off than they were before.

6276. Will it not be necessary, sooner or later, to re-inhabit these areas?—It would be advisable if it could be done with safety.

6277. Do you see any way in which it can be done with safety?—Do you mean, do I see any means of exterminating the fly?

6278. Yes.—I see no means of exterminating the fly.

6279. With regard to immunity, what is your theory as to the tolerance of native game to trypanosomes? Is it an immunity which has been engendered by long contact with the organism? I am speaking of *morsitans* now.—I am afraid I cannot say. I do not know what the immunity is due to.

6280. The game are tolerant; the domestic stock are intolerant?—Yes.

6281. What is your explanation of that fact? You say that the disease is endemic, that immunity has arisen or tolerance from long contact with the organism in the one animal, and that another animal which has never in its past racial history been in contact with the organism, is not immune because it has not been in racial contact with the organism. Now, is not that why domestic stock is not immune but game is?—Possibly that is the reason.

6282. Do not those same conditions exist with regard to the black and the white?—You mean with regard to human beings?

6283. Yes. Taking phylogenetic history the black has existed in contact with the organism, and the domestic stock has not, and the European has not?—But we have no evidence of the disease being different as regards immunity in the native and in the European.

6284. But would you not expect to find it?—Arguing from the point of view that you put to me one would expect to find that the native was more immune than the European.

6285. And yet you do not?—Not so far as one knows.

6286. Then how does your theory of immunity explain the susceptibility of domestic stock, and the insusceptibility of the human being?—I do not know that it is a theory, I put it forward as a conjecture that there may be immunity. I cannot theorise on it because I do not know what the factor is that produces immunity to trypanosomes whether it is an anti-body or whatever it may or may not be.

6287. Apparently there is no absolute immunity because some, both black and white, contract the disease in a very virulent form?—Yes. We have no evidence to show that some wild animals do not die from the disease.

6288. Is there any evidence that they suffer from the disease?—There is no evidence either way, as far as I know.

6289. There is positive evidence that they can carry the trypanosome and live?—Yes, but it may be the survival of the fittest.

6290. What proportion of flies in infected areas has been found to be infected?—One in 500 with the trypanosome which is called *rhodesiense*.

6291. If stock are bitten frequently and the human animal is bitten infrequently, is it not due to the fact that there is not very high infectivity among flies? Might not the relative insusceptibility of the human being be due to that fact?—That may be so. They are certainly very much less exposed to the danger of infection than the domestic animal is.

6292. Have you known of an experiment in which a non-infected fly has been fed upon a case of human trypanosomiasis in a *morsitans* area and that fly then has been fed upon domestic stock and produced trypanosomiasis in the animal?—If you change the domestic stock to experimental animals I know of an instance of a fly fed on an infected human being and

afterwards fed on experimental animals transmitting the disease.

6293. Is not that strong presumptive evidence, if not absolute evidence, that the diseases are the same—that the trypanosomiasis in the animal and the trypanosomiasis in man are due to the same organism?—In the particular instance to which I refer the animal died. It was a pathogenic trypanosome.

6294. And it was a trypanosome pathogenic to man?—Yes.

6295. The man died?—The man died.

6296. Is not that strong presumptive evidence that it is the same organism?—As?

6297. Is it not strong presumptive evidence that *brucei* and *rhodesiense* are the same organism?—Yes, I think they are. Had that infected fly bitten a domestic animal instead of an experimental animal, I do not know what would have happened. Experimental animals are used because one knows what the reactions of certain trypanosomes are in their case. If a domestic animal had been bitten, instead of an experimental animal, for instance a cow or a horse, one does not know that the result would have been pathogenic. One can only presume.

6298. Has that experiment not been performed to your knowledge?—Not directly with flies. *Rhodesiense* has been proved to be pathogenic to domestic animals by direct inoculation.

6299. You spoke of a road 80 miles long where there were flies and practically no game?—Yes.

6300. What do the flies feed on there?—My theory about it is, that in the absence of game, flies will feed on human beings, and will look for human beings as a source of food supply.

6301. Is there a sufficient traffic of human beings along the road to supply them with food?—Yes, there is a very extensive traffic. All the carrying for a rather large copper mine is done along that road partly with traction-engine transport, and partly by native transport.

6302. Do you think that a certain amount of clearing on either side of that road would give human travellers freedom from being pestered by the fly?—I think it would decrease the amount of fly to a very considerable extent.

6303. Mr. Rothschild asked you about the danger in an experiment of having an area a mile wide around the experimental area traversed by game bringing flies up to the experimental enclosure. Would the experiment not also be vitiated by the flies from the enclosure going out and biting the game in the cleared area?—Yes, quite. If we presume that the removal of the game is not going to remove the flies, it would be vitiated.

6304. What is your opinion about the removal of the game? Do you think it would remove the flies?—I think if you removed the game and the population the flies would leave the area.

6305. (Chairman.) Is it the case within your knowledge that domestic goats and sheep are in many instances tolerant of trypanosomes?—In very very exceptional cases goats and sheep are found in fly areas.

6306. They would not have acquired immunity by long association with trypanosomes?—As a rule they are infected and they do not live long.

6307. We have had some evidence on that point. That is your experience?—Yes.

6308. What about domestic pigs?—I do not know of any instance of domestic pigs in fly areas.

6309. (Dr. Chalmers Mitchell.) Do you mean that they are always killed?—They are very very seldom to be found in native villages.

6310. (Chairman.) I do not quite understand your position at the moment. In the report you were good enough to send dated the 31st of last month, you refer to an experiment, and in paragraph 3 (c) you say: "No importance is attached to the estimation of the infectivity of the fly, since there is at present no known method of identifying the trypanosome." I do not quite reconcile that with what you have said

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[Continued.]

about *brucei* and *rhodesiense* being the same.—My point is this—that one knows that *brucei* is common in nature, and one finds a trypanosome called *rhodesiense* which is not common. Presuming that they are originally the same trypanosome, the difference in that one is pathogenic, and one apparently is not, must be due to some phase, and I feel now that that phase or that stage in the life of the trypanosome in which it is pathogenic to man, cannot be distinguished from that in which it is not pathogenic.

6311. Take the two theories—the theory of the relative immunity of man and the theory of some particular form in which it becomes pathogenic. Is it not an equally simple explanation, and perhaps a more simple explanation, that the two trypanosomes are different altogether? It accounts for it equally well, does it not, one being the human trypanosome and the other being the nagana trypanosome?—But one ignores altogether in that the fact that they are morphologically, and in many other respects, very similar.

6312. I speak under correction, but morphologically you would bring in many other trypanosomes which clearly are not the same trypanosome and would you not equally make *gambiense* and *rhodesiense* and *brucei* all the same on a morphological basis?—I do not know how we are going to say that any one of these trypanosomes, which is morphologically similar, is not the human trypanosome.

6313. I do not say so, but we have had some evidence upon it. However, I will leave it. Now do you attach any importance at all to direct communication through *Stomoxys*?—In the human disease?

6314. In communicating the disease from man to man, from game to game, and cattle to cattle?—You are speaking of trypanosomiasis generally?

6315. Yes.—The disease has almost been proved to be transmitted mechanically by *Stomoxys* in cattle.

6316. That would be under special conditions?—Under special conditions.

6317. Would you regard it as a serious factor?—No.

6318. One general question: what do you consider, in the best of your judgment, is the main reservoir for the trypanosome that conveys the disease, certainly to cattle, possibly to men?—The opinion I have formed since we have found that the disease has not spread, and is not spreading, is that man is the chief reservoir of the trypanosome which is pathogenic to man.

(Chairman.) Thank you very much.

The witness withdrew.

Further Note furnished by Dr. May, with Special Reference to Question and Answer No. 6318.

The distribution of the disease lends some support to the view that the disease is transmitted from man to man. Since the discovery of the presence of human trypanosomiasis in the absence of *G. palpalis* in

Northern Rhodesia it has been frequently found and reported on that the disease seemed to follow the movements of natives. The majority of cases have been found in the vicinity of main traffic routes (game is generally less abundant in the vicinity of these routes than in many other localities and under equally suitable conditions where the disease is absent or very rarely found), thus lending some support to the view that it is transmitted from man to man rather than that game is the chief reservoir. This occurrence of the disease along the main traffic routes is so marked that before the discovery of the presence in game of a trypanosome morphologically identical with that found in the human disease, it led to the opinion being advanced that the disease had been carried along these routes from *palpalis* (*gambiense*) areas (Luapula and Tanganyika), and that the increased virulence evidenced in these areas was the result of changed surroundings.

In the Bangweolo District of North-Eastern Rhodesia, where four cases only of the disease have been found since 1909, all these cases have been found within a small radius of the village in which the original case occurred, thus showing the presence of infection over a considerable period without a tendency to spread beyond a limited area, although game and fly are very plentiful.

There is a marked tendency for the disease to recur in villages where it has been previously found rather than in neighbouring villages where apparently equally suitable conditions exist (game and fly being equally distributed). This applies particularly to the villages on the Lower Luangwa.

There seems to be good reason to think that the disease has been known to the natives for a very considerable time (in most localities under the name of "Chilolera"); if this be so, and the source of supply and means of transmission of the organism be what they are supposed to be, it seems inconceivable that some circumstance (such as famine, &c.) should not before now have broken down the immunity or interfered in some way with whatever factor it is which has prevented the spread of the disease. On the other hand, if this trypanosome (or the particular phase of this trypanosome which is pathogenic to man) be transmitted only from man to man, it is quite conceivable, taking into consideration the scattered nature of the population (which in the Luangwa Valley fly areas is less than one to the square mile) that the disease might remain for many years, if not indefinitely, endemic; slight local exacerbations from time to time being accounted for by some such circumstance as increased opportunity of infection for the fly.

The fact that a greater number of cases of the disease have been found in a very much smaller area in Nyasaland than in Rhodesia would bear out this view, owing to the much greater density of the native population along the shores of Lake Nyasa, and consequent increased opportunity for inspection and transmission.

APPENDIX C.

Part I.—Written Statements by Foreign Experts.

DOCTEUR G. BOUFFARD, ÉCOLE D'APPLICATION du SERVICE de SANTÉ des TROUPES COLONIALES, MARSEILLES.

TRYPANOSOMIASE HUMAINE.

*1°. J'ai étudié la maladie du sommeil dans les territoires de la boucle du Niger, principalement dans la région de Bamako, sur les bords de la Volta Noire, du Bani et de ses affluents, le Banifing et la Bogué. Je me suis livré à ces recherches de 1906 à 1910; c'est pendant la saison des pluies de 1907, de juin à septembre, que j'ai parcouru les principaux districts infestés de glossines, réputés inhospitaliers et rendus désertiques par la crainte qu'à l'indigène de contracter la maladie en voyageant sur ces fleuves, ou en habitant dans leur voisinage immédiat.

2°. Le but de mes recherches était de faire une enquête administrative sur la diffusion de la maladie, les ravages qu'elle pouvait y causer, d'identifier morphologiquement et expérimentalement les trypanosomes en jeu.

3°. Ces régions sont réputées, depuis le début de notre conquête, pour être des centres endémiques de trypanosomiase. Elles auraient été autrefois dévastées par d'importantes épidémies de trypanose et la plupart de ces vastes gîtes à tsé-tsé, que constituent les rives boisées des grands cours d'eau, sont aujourd'hui désertiques. Les cas de maladie du sommeil sont devenus de ce fait relativement rares et on ne les observe que dans de petits centres endémiques, créés par des gîtes permanents à glossine, de petite étendue.

4°. Les cas que j'observais en 1906 provenaient du district de Kati, province de Bamako; on trouve dans cette région un petit cours d'eau à végétation touffue, où les glossines abondent. Les indigènes connaissent très bien la maladie, mais ne l'identifient que lorsque le patient est à la phase terminale, au stade hypnose. Ils isolent le malade et lui assignent dans le village une case éloignée des autres.

J'examinai en 1907 deux autres trypanosomes provenant du village de Koulikoro sur le Niger; comme ces indigènes n'étaient dans le pays que depuis deux ans, et habitaient antérieurement la province de Sikasso où la maladie existe, j'ai cru qu'ils s'étaient infectés dans leur pays d'origine.

A. Leger, observant, en 1911, trois autres cas de trypanose dans le même village chez des autochtones n'ayant jamais voyagé, a serré le problème de plus près et a découvert dans le voisinage de l'agglomération un gîte permanent de glossines, sur le bord d'un ruisseau très fréquenté par les indigènes. Des recherches minutieuses lui firent découvrir quatre autres porteurs de trypanosome. On trouvera dans le Bulletin de Pathologie Exotique de 1912 l'étude de ce petit centre endémique de maladie du sommeil.

Dans la boucle de la Volta Noire, dans les provinces de Koury et de Boromo, je rencontrais deux cas typiques, au stade sommeil.

Je n'en ai point trouvé sur les rives boisées du Bani et du Banifing; je dois reconnaître que la population indigène a fui les abords de ces fleuves. On n'y rencontre que quelques villages, perchés sur des hautes berges dénudées, où les glossines sont absentes. Les pêcheurs qui les habitent, pratiquent la pêche au flambeau; le jour ils restent au village, dormant ou s'occupant de menus travaux manuels; dès la tombée de la nuit, on voit leurs barques sillonner le fleuve. Je n'ai trouvé chez eux aucun cas de trypanose; leurs ganglions étaient normaux. Ils connaissent fort bien la maladie, et citent certains villages voisins qui ont dû être abandonnés par suite d'une forte mortalité attribuée par eux à l'hypnose.

* The figures at the beginning of the paragraphs refer to the questions in Appendix A.

5°. Il est impossible d'avoir une opinion sur le nombre de trypanosés; le médecin n'a connaissance que des cas graves, à échéance fatale; ils sont d'ailleurs relativement rares, puisque les statistiques du service de santé de la colonie n'en mentionnent que 5 à 6 par an. Il faut reconnaître que le service médical n'est encore qu'à l'état embryonnaire dans les provinces à tsé-tsé; les administrateurs ont fait quelques enquêtes sur l'existence de la maladie dans ces régions; elle serait relativement peu répandue.

6°. La durée moyenne de la maladie est d'environ trois ans.

7°. L'affection paraît localisée en certains points où elle serait endémique; aucune poussée épidémique n'a été signalée par le service médical.

8°. Les seules recherches systématiques entreprises dans les provinces à tsé-tsé sont celles que je fis en 1907 dans le Bani et la Volta Noire.

9°. Je pratiquai l'examen direct du sang périphérique des suspects, ponctionnai les ganglions, quand leur hypertrophie le permettait, et trois fois je fis des ponctions lombaires.

Quand un suspect est évacué sur une formation sanitaire, la ligne de conduite à suivre est sans contredit l'inoculation à l'animal sensible; le procédé est évidemment impossible à employer au cours d'une enquête exigeant de fréquents déplacements. Le meilleur réactif animal, le seul, pour ainsi dire, en Afrique Occidentale, où le cobaye ne s'infecte qu'exceptionnellement, est le *cercopithecus pathas*. Une injection intrapéritonéale, ou sous-cutanée de 10 c. cubes du sang du malade permet d'assurer le diagnostic; la période d'incubation varie très peu; elle est de 10 à 16 jours. Cette espèce de singe est très répandue dans le pays, et on s'en procure très facilement.

10°. Dans les territoires de la Volta Noire, les indigènes se laissent très difficilement examiner, et j'étais accompagné dans mes tournées par 8 agents de police qui, avec beaucoup de peine, arrivaient à me présenter 5 ou 6 suspects; à l'arrivée du médecin les habitants des villages fuient dans la brousse; on en ramène bien quelques uns, mais ni la douceur ni la force ne réussissent à les convaincre de l'intérêt qu'il y aurait pour eux à nous montrer leurs malades. Les administrateurs, plus au courant que nous de leurs habitudes, prétendent qu'ils cachent leurs hypnosés.

Dans d'autres provinces, en contact depuis plus longtemps avec l'élément européen, on vous amène volontiers les trypanosés, qui ont été isolés le plus souvent dans une case du village, et on accepte volontiers l'évacuation du malade sur une formation sanitaire.

11°. Je n'ai jamais observé qu'un seul cas de trypanose au début, le mien; mon sang devait être très peu parasité, puisque de nombreux examens, à l'état frais, d'une goutte de sang ne m'ont point permis de déceler de parasites. Trois mois après le début de la maladie, j'ai pu infecter un *cercopithecus pathas* avec 1 c. cube de sang.

A. Leger, dans le foyer endémique de Koulikoro, trouve de nombreux trypanosomes dans le sang d'un indigène en apparence en bonne santé. Le cas doit être plutôt rare et je partage l'avis de la majorité des médecins qui ont étudié la maladie en Afrique Occidentale Française et qui considèrent la *très grande rareté trypanosome dans le sang* comme la règle.

Tous les malades que j'ai examinés étaient à la deuxième période de leur affection; je n'ai trouvé de parasites ni dans le sang, après centrifugation ou à l'examen direct d'une goutte de sang, ni dans la pulpe

ganglionnaire. Deux fois je les ai vus très rares dans le liquide céphalo-rachidien.

L'inoculation du *cercopithecus pathas* restera pour les formations hospitalières la seule épreuve pratique qui donne un diagnostic sûr et relativement rapide pour une maladie à marche lente.

12°. Chez ces singes j'ai observé fréquemment une sorte de périodicité dans leur parasitisme sanguin. Certains jours les trypanosomes sont assez nombreux; le lendemain on n'en rencontre que difficilement un ou deux à l'examen direct d'une goutte de sang; puis ils font totalement défaut pendant huit à dix jours pour réapparaître ensuite en assez grand nombre. Je crois que cette périodicité existe chez l'homme; les deux faits suivants sembleraient le prouver. Deux Européens ayant contracté la maladie dans la boucle du Niger ont été soignés en France. L'un d'eux, qui ignorait le caractère de sa maladie, vient en 1908 à l'Institut Pasteur, pour que l'on examine son sang au point de vue hématozoaire du paludisme; Monsieur Leger y trouve des trypanosomes; le malade revient le lendemain; on centrifuge dix c. cubes de son sang sans qu'il soit possible d'y déceler un seul parasite.

Chez moi-même, en 1910, on prélève deux fois, à cinq jours d'intervalle, 10 c. cubes de sang que l'on centrifuge avec résultat négatif; trois jours après, sur un frottis de sang prélevé au moment d'une légère hyperthermie (37°5) on trouve un trypanosome.

Cette périodicité ne peut qu'accroître les difficultés du diagnostic; je ne crois pas qu'il y ait, certains jours, stérilité absolue du sang, mais seulement très grande rareté dans le nombre des parasites, que, seule, l'inoculation au singe pourra déceler.

13°. Les indigènes sont souvent de bons observateurs; les plus intelligents, les notables, prétendent que l'on ne contracte la maladie qu'en saison des pluies. C'est l'époque de l'année où les mouches sont extrêmement nombreuses, sont très voraces et où, en ce qui concerne les trypanosomiasés animales, on les sait expérimentalement les plus infectées. Il doit en être probablement de même pour *Trypanosoma gambiense* et ma conviction absolue est que la trypanose se contracte principalement de juin à novembre.

14°. Je n'ai jamais observé l'opacité de la cornée.

15°. Le trypanosome humain de la boucle du Niger est morphologiquement identique à *Trypanosoma gambiense*. Expérimentalement il s'en éloigne un peu; il est peu ou point pathogène pour le cobaye et les petits rongeurs, rats et souris. J'ai eu, entre les mains, trois parasites isolés directement de l'homme. Avec 5 c. cubes de sang d'un cercopithecus présentant des parasites nombreux je n'ai pu infecter le cobaye ni le rat gris; l'injection était faite dans le péritoine.

Mes recherches épidémiologiques me permettent d'affirmer que ce parasite ne peut être transmis que par *Glossina palpalis* et *Glossina tachinoides*. Pratiquement *Glossina morsitans* n'est pas en jeu.

16°. Morphologiquement le trypanosome de la boucle du Niger me paraît identique à *Trypanosoma gambiense* du Congo. Il s'en éloignerait expérimentalement par son absence de virulence pour le cobaye.

17°, 18°. Je n'ai, sur ce sujet, aucune expérience personnelle.

19°. Il existe au Soudan Français de petits centres endémiques de trypanose humaine, comme ceux de Kati, de Koulikoro, où, sans aucun doute, le gros gibier sauvage fait défaut; mais dans tous ces centres on rencontre des tsé-tsé.

20°. La transmission directe exige un parasitisme abondant dans la circulation périphérique; comme le fait est exceptionnel, il est probable que, sur les territoires du Niger, le principal agent de propagation de la maladie est la tsé-tsé.

21°. Les autres insectes hématophages ne me paraissent jouer qu'un rôle secondaire dans la transmission du *Trypanosoma gambiense*.

22°, 23°, 24°, 25°. Aucune opinion sur ce sujet.

26°. Dans les centres endémiques, où les gîtes à tsé-tsé sont de minime étendue, et souvent très éloignés des immenses gîtes constitués par les galeries forestières, je crois que l'homme est le seul réservoir à virus.

Dans les vastes gîtes du Bani et de la Volta Noire la glossine ne peut s'infecter sur l'homme qui a fui depuis longtemps ces régions inhospitalières.

La preuve est faite du danger de la navigation sur ces fleuves en pleine saison des pluies pendant les mois de juillet et août; on a huit chances sur dix de s'y infecter. Il faut donc que la glossine trouve un réservoir à virus ailleurs que chez l'homme; c'est très probablement dans le sang d'un animal sauvage qu'elle va puiser le germe infectieux.

27°. Je n'ai jamais observé de cas de trypanosomiasé héréditaire chez la mouche.

28°. Si l'on admet que *Trypanosoma gambiense* du Congo est identique au trypanosome humain de la boucle du Niger, on est obligé de reconnaître une différence dans leur virulence. Il est certain que le trypanose évolue plus lentement, et est moins grave chez le Soudanais que chez le Congolais.

TRYPANOSOMIASES CHEZ LES ANIMAUX DOMESTIQUES.

29°. Trois trypanosomiasés se rencontrent dans les zones à tsé-tsé de la boucle du Niger. La plus répandue est indiscutablement celle qui reconnaît comme agent pathogène, *Trypanosoma cazaloui*; je ne connais point de régions à glossines qui en soit indemne; elle a été observée et étudiée pour la première fois par Cazalou en 1904. Par ordre d'importance nous donnerons la seconde place à *Trypanosoma pecaui* qui étend ses ravages tout le long des grands fleuves de la boucle du Niger, le Bani, et la Volta Noire, et de leurs nombreux affluents. Cette trypanosomiasé paraît s'être cantonnée dans ces importants gîtes à tsé-tsé; elle n'a pas encore été domiciliée dans les gîtes du Niger et de ses affluents.

Trypanosoma dimorphon est beaucoup plus rare; et pratiquement je le considère comme peu dangereux.

Il parasite surtout les chiens et s'observe assez rarement chez les bovidés et les équidés.

Le domaine géographique de ces redoutables trypanosomes est celui des mouches qui les convoient, *Glossina palpalis* et *tachinoides*. *Glossina morsitans* est fort rare; aussi les considérations qui vont suivre ont-elles trait aux deux premières espèces, en considérant la troisième comme jouant un rôle très effacé dans la transmission des trypanosomiasés.

Je ne reviendrai point sur les recherches de Roubaud et de Bouet et les miennes, déterminant l'aire de distribution des glossines dans la boucle du Niger; je n'insisterai pas sur les conditions de température et d'humidité, qui cantonnent ces mouches dans telle ou telle région, les rendent infectieuses en certains points, inoffensives en d'autres; je ne classerai point les gîtes en temporaires ou permanents, pour l'unique raison, qu'au point de vue épidémiologique, ils ont tous deux la même importance et que certains gîtes temporaires peuvent parfois être plus dangereux que les gîtes permanents dont ils sont issus. Je me contenterai de passer en revue et d'étudier les causes de contamination de l'animal domestique dans la colonie. Les conditions de l'infection peuvent être variées; elles relèvent toujours de deux sources: 1° la glossine, second hôte du trypanosome; 2° les autres mouches piqueuses; principalement les stomoxes, qui ne sont que des agents de transmission directe de la maladie. Mon opinion sur le rôle comparé des glossines et des stomoxes dans la transmission des trypanosomes animaux a été publiée dans une note à la Société de Pathologie Exotique en 1912. Les tsé-tsé demeurent certainement l'agent de transmission le plus redoutable puisqu'elles sont seules capables d'entretenir des centres enzootiques vrais et qu'on les trouve toujours à l'origine des épizooties.

La répartition géographique de ces mouches dans la boucle du Niger permet de distinguer deux variétés de gîtes: 1° les galeries forestières qui s'étendent le long des fleuves sur des centaines de kilomètres; 2° les petits cours d'eau dont le lit est enfoui sous une végétation touffue. Les premiers sont des gîtes continus, tributaires les uns des autres où il serait bien difficile d'expérimenter; les seconds au contraire sont généralement isolés, sans aucune relation entre eux, et c'est avec eux que je proposerai certaines expériences qui me paraissent intéressantes.

Etudions maintenant avec plus de détails ces deux variétés de gîtes.

I. *Les grands gîtes*. — Ce sont des sources permanentes d'infection; leur accès est, en toute saison, fort dangereux; il suffit en saison des pluies d'y laisser séjourner une heure un animal sensible pour être certain de le parasiter. Les indigènes connaissent bien l'inhospitalité de ces berges; aussi tiennent-ils leurs troupeaux éloignés de ces régions maudites, qui sont devenues complètement abandonnées. Ces galeries forestières, qui longent les cours d'eau et abritent un nombre formidable de glossines, se dressent partout comme une barrière infranchissable aux troupeaux en transhumance. Les franchir, c'est vouer le bétail à l'infection certaine, c'est voir en quelques semaines fondre de superbes troupeaux. On ne pourra éviter, quelle que soit la rapidité avec laquelle on fasse franchir la rivière aux animaux, *Trypanosoma cazalboui*, qui, inoculé à quelques bœufs, trouvera dans le stomoxe un agent merveilleux de diffusion. L'abondance de ce parasite dans le sang périphérique facilite singulièrement la transmission directe par les insectes piqueurs, hôtes habituels des troupeaux. Les éleveurs conservent toujours le bétail dans les pâturages éloignés des gîtes à glossines; ils doivent renoncer à les faire voyager et ils ne devront tirer profit de cette source de richesse que lorsque l'administration leur aura créé des routes sanitaires, comportant de larges déboisements aux points de passage des cours d'eau.

Tous les grands cours d'eau de la boucle du Niger sont infestés par *Trypanosoma cazalboui*; *Trypanosoma pecaui* ne paraît pas exister dans la vallée proprement dite du Niger; et cependant on y rencontre assez souvent des équidés qui ont été infectés sur les bords du Bani. *Glossina palpalis* ou *tachinoides* des affluents du Niger, en amont de Bamako, seraient-elles incapables à cultiver ce virus? Je n'ai aucune opinion à ce sujet, mais on peut préparer toute une série d'expériences faciles à faire et qui peut-être confirmeraient les vues de Roubaud, et établiraient nettement qu'il existe bien des races géographiques de glossines. *Trypanosoma pecaui* a élu domicile dans tous les formidables gîtes constitués par la Volta noire, le Bani et leurs affluents. Pour s'en convaincre il suffit, comme je l'ai fait, d'emmenager sur ces cours d'eau pendant deux à trois jours quelques chiens pour les y voir tous se contaminer.

De tous ces grands gîtes le plus intéressant est indiscutablement le Banifing, un des principaux affluents du Bani; il est infecté par *Trypanosoma gambiense*, *Trypanosoma pecaui*, et *Trypanosoma cazalboui*; il est tellement redouté des indigènes que les pêcheurs eux-mêmes, si peu soucieux de leur santé, refusent d'y naviguer. Je l'ai parcouru sur une cinquantaine de kilomètres; un silence impressionnant règne le long de cette infernale rivière où les tsé-tsé s'y voient en quantité incroyable. Jamais mes matelots n'avaient été autant piqués et ils me suppliaient en fin de journée de les ramener sur le Bani.

Bien que l'indigène ait fui ces rives inhospitalières depuis de nombreuses années, elles n'en demeurent pas moins un foyer permanent de trypanosome humaine, au même titre que les îles du lac Victoria, signalées par Bruce et ses collaborateurs, dont les glossines sont encore infectantes malgré la disparition de l'homme depuis plusieurs années.

Où est le réservoir à virus? Dans le sang de quel animal les tsé-tsé trouvent-elles *Trypanosoma gambiense*? Voilà un point qui me paraît encore fort obscur et je ne suis nullement convaincu, malgré les intéressantes découvertes de ces dernières années, que ce soit les antilopides qui entretiennent l'endémicité de ces centres.

Dans les régions qu'arrose le Banifing, la saison des pluies dure environ cinq mois; elle commence en avril et, dès la fin juin, les nappes d'eau stagnantes sont suffisamment répandues dans la campagne pour que le gibier sauvage ne se trouve plus dans la nécessité, comme en saison sèche, de venir s'abreuver au fleuve. Souvent, sur de très grandes distances, l'accès de la rivière est rendu fort difficile, sinon impossible, par les inondations qui isolent, pour ainsi dire, les gîtes à glossines.

Or, c'est précisément à l'époque où les antilopes se tiennent éloignées des cours d'eau que les gîtes sont en pleine prospérité, que le nombre des glossines atteint son maximum, et que leur pouvoir infectant est le plus

considérable. Où donc, à ce moment-là, la tsé-tsé trouve-t-elle le sang nécessaire à son existence? Peut-être chez les oiseaux, mais ils sont plutôt rares; il est probable que la majorité se nourrit aux dépens des hôtes permanents de la rivière, les hippopotames et les caïmans. On les y rencontre en très grand nombre; et j'ai traversé des biefs, où vivaient de véritables troupeaux de 10 à 15 hippopotames. C'est, avec le caïman, l'unique habitant vraiment sédentaire de ces vastes gîtes à glossines; pourquoi n'entreprendrait-on pas quelques recherches en vue d'élucider, une fois pour toutes, le rôle de l'hippopotame dans la conservation des trypanosomes humaines ou animales. L'expérience n'est pas impossible et je dirai plus loin comment je la conçois. Elle me paraît bien plus difficile avec les caïmans, sans cependant être irréalisable.

Les partisans de l'antilope, réservoir à virus, m'objecteront que les glossines, pouvant vivre six mois, s'infectent naturellement en saison sèche alors que le gibier sauvage doit obligatoirement venir chercher de l'eau à la rivière.

Mais, alors, comment se fait-il qu'en mai et juin l'accès du fleuve soit peu dangereux pour les animaux domestiques, et qu'il faille les y envoyer boire plusieurs fois pour les infecter alors qu'un seul voyage est suffisant en juillet et août? C'est, me répondra-t-on, qu'en plein été, le nombre des mouches s'est considérablement accru et que les animaux sont plus piqués qu'en mai et juin. J'accepte cette manière de voir, mais elle n'explique pas pourquoi le pourcentage des glossines infectées est bien plus élevé à l'époque où les antilopes n'ont plus accès au fleuve depuis deux mois?

Je tenais à présenter ces observations, parce que je reste convaincu que l'antilope n'est pas un réservoir à virus de première importance, et qu'elle ne joue aucun rôle dans la conservation du virus dans les grands gîtes à tsé-tsé.

En résumé, il existe dans la boucle du Niger certains cours d'eau entre autres le Banifing qui sont désertés depuis longtemps par l'homme et qui persistent à être des foyers à *Trypanosoma gambiense*. L'infection paraît certaine pour ceux qui s'y aventurent en pleine saison des pluies, au moment où les glossines y pullulent et sont le plus infectantes. C'est l'époque où le gibier sauvage n'a plus accès au fleuve et où les seuls animaux sédentaires que l'on y rencontre sont l'hippopotame et le caïman. "La tsé-tsé est la mouche de l'hippopotame" dit l'indigène. Que l'on démontre donc une bonne fois que ce pachyderme ne peut pas être un réservoir à *Trypanosoma gambiense*, et l'hypothèse de l'antilope reprendra alors toute son importance. Je ne mets point en doute l'infection naturelle de l'antilope *Trypanosoma gambiense*; ce parasitisme n'a nullement lieu de nous surprendre, puisque expérimentalement tous les bovidés sont sensibles à ce virus. Mais que ce sang si peu infectieux, où les trypanosomes sont extrêmement rares, soit capable d'infecter les glossines dans la proportion de 5 à 10 pour cent, j'en doute un peu.

II. — *Les petits gîtes*. — A côté des vastes gîtes, que sont les galeries forestières, et qui sillonnent tous les territoires de la boucle du Niger d'importants foyers endémiques de trypanosomiase, existent une succession de petits gîtes permanents, isolés les uns des autres, et constitués par les rives boisées de petits cours d'eau, tributaires des fleuves et rivières aux berges nues. Distants de plusieurs kilomètres les uns des autres, ces réservoirs à glossines sont généralement indépendants; ils ne sont pas tous infectés; mais dans la plupart y vivent des tsé-tsé convoyant *Trypanosoma cazalboui*, *pecaui* ou *gambiense*. Je n'ai jamais rencontré ensemble ces trois parasites. Dans la vallée du Niger ces gîtes sont surtout des foyers de *Trypanosoma cazalboui*; quelques uns sont à la fois infectés par le *Trypanosoma cazalboui* et *Trypanosoma gambiense*. Dans la vallée du Bani et de la Volta Noire ces gîtes sont principalement infectés par *Trypanosoma pecaui*; quelques uns sont des foyers à *Trypanosoma dimorphum*.

Les foyers à *Trypanosoma cazalboui* et *Trypanosoma gambiense* de la vallée du Niger me sont mieux connus que ceux du Bani et de la Volta Noire; c'est d'eux qu'il sera question dans les considérations suivantes.

Dans la plaine de Bamako se dresse une nouvelle ville européenne, capitale de la colonie du Haut Sénégal et Niger, qui groupe autour d'elle une série de villages noirs d'une population d'environ 10 mille habitants. La vallée, large d'une dizaine de kilomètres, est riche en pâturages et de nombreux troupeaux y vivent en de parfaites conditions. Cependant de temps à autre quelques épizooties viennent surprendre l'éleveur et décimer son bétail; j'ai eu l'occasion d'en observer quelques unes et je puis affirmer qu'elles relèvent toutes de *Trypanosoma cazalboui*. L'indigène mène paître ses bovidés en aval de la ville, sur la rive gauche du Niger aux berges nues, et qui, sur près de 15 kilomètres de distance, ne reçoit aucun affluent. L'état sanitaire de ces troupeaux est toujours parfait; la maladie ne s'y déclare que s'ils ont accidentellement fréquenté les pâturages en amont de Bamako; là les bergers ne trouvent d'abreuvoir pour leurs animaux, qu'à l'unique cours d'eau infesté de glossines, appelé le Faraco. Il arrive assez souvent que l'entrée dans le troupeau d'un ou de plusieurs bœufs infectés soit suivie d'une poussée épizootique sérieuse; l'agent de transmission en jeu est alors le stomoxe.

La diffusion de la maladie par cet insecte est facilitée par l'extrême richesse en parasite du sang périphérique de l'animal malade.

Il existe donc près de Bamako un gîte permanent à glossines palpalis, centre endémique de *Trypanosoma cazalboui*, formé par les rives boisées d'un petit cours d'eau, le Faraco, dont la largeur moyenne ne dépasse guère cinq mètres. Ce gîte présente certains caractères, communs à tous les gîtes, aux grands comme aux petits. Les glossines y deviennent très rares en saison sèche, de décembre à avril, pour se multiplier dès les premières pluies en mai, et atteindre leur chiffre maximum en juillet, août et septembre. Leur pouvoir infectieux évolue parallèlement; il n'est jamais nul, puisqu'en pleine saison sèche on peut encore infecter des animaux très sensibles comme les zébus, à la condition toutefois de les y conduire boire pendant plusieurs jours de suite. Il est à son maximum en pleine saison des pluies, où l'infection éclate après un voyage unique.

En saison sèche les glossines sont cantonnées dans les zones les plus touffues de la rivière, dont l'accès sur la majeure partie de son parcours reste sans danger aux animaux domestiques. Dès le mois de juin la répartition n'est plus la même et la tsé-tsé se rencontre tout le long du cours d'eau; c'est alors que les bestiaux qui vont y boire contractent *Trypanosoma cazalboui*.

Le Faraco est distant d'une dizaine de kilomètres de toute autre rivière à glossines; c'est un gîte complètement isolé sur lequel il serait facile d'expérimenter. Dans ce foyer de *Trypanosoma cazalboui* les mouches s'infectent principalement sur l'animal malade. On sait que cette trypanosomiose a une évolution subaiguë chez les bovidés, et chronique chez les équidés. La race zébu, et d'autres races de la boucle du Niger, importées dans la vallée de Bamako pour les besoins de la ville, ou pour l'exportation sur d'autres provinces, sont très sensibles à ce virus; en pleine épizootie on voit la maladie évoluer en un mois et le sang périphérique être extrêmement parasité. C'est avec ces animaux et avec des moutons également très sensibles que j'ai pu expérimentalement infecter des glossines dans la proportion de 25 à 40 pour cent. Il est donc tout naturel de penser que les tsé-tsé trouvent des conditions parfaites d'infection dans les troupeaux que les bergers ont la mauvaise habitude de mener boire au Faraco, et que l'existence d'un autre réservoir à virus est peu probable. D'ailleurs on ne rencontre point de grandes antilopes dans la vallée; on en voit de la petite espèce, de la taille d'une chèvre, qui ne viennent à la rivière qu'en saison sèche. Quelques chasseurs m'ont procuré plusieurs fois l'occasion d'examiner le sang de ces animaux; je n'y ai rencontré aucun parasite. Inutile de dire que dans ces étroits cours d'eau ne vivent ni hippopotame, ni caïman.

La preuve de l'animal malade, seul réservoir à virus, infectant les glossines du Faraco pourrait être facilement faite, en surveillant ce gîte dont l'étendue est d'environ dix kilomètres; on en interdirait l'accès à tout animal domestique pendant toute une année de

juin à juin, et si en juillet et août suivants les mouches restaient infectantes, on serait, je crois, en droit d'affirmer l'existence d'une source d'infection chez le gibier sauvage.

Tant que cette preuve ne sera pas faite je persisterai dans mon opinion que les petits gîtes à glossines sont des foyers de trypanosome entretenus par la fréquentation d'animaux domestiques porteurs de germes.

Quand ces gîtes sont infectés par *Trypanosoma gambiense*, ce n'est point chez l'animal sauvage mais bien dans le sang de l'homme malade que la glossine puise le virus.

L'histoire du centre de Koulikoro, étudié par A. Leger, vient appuyer ma manière de voir. Trois indigènes sont en état d'hypnose évidente; on les conduit à ce médecin, qui fait une enquête sur la cause de leur contamination et découvre un gîte à glossines palpalis dans le voisinage du village; il examine le sang des indigènes qui ont l'habitude d'aller, chaque jour, faire leurs ablutions près de ce gîte et trouve quatre porteurs de trypanosomes; le sang de l'un d'eux est même assez riche en parasites. C'est évidemment, sur eux, que s'infectent les glossines. Il eût été cependant intéressant, non point de détruire ce gîte, comme on l'a fait, mais de le conserver, en interdisant l'accès à l'indigène, afin de se rendre compte, si, après un an, les glossines étaient encore parasitées par le trypanosome humain. Dans l'affirmative l'origine animale du virus devenait alors probable.

D'autres petits centres endémiques d'hypnose, parfaitement isolés, semblables à celui de Koulikoro, existent certainement dans la colonie; on pourrait en rechercher et expérimenter sur eux.

Conclusion. — Il existe dans la colonie du Haut Sénégal et Niger deux variétés de foyers endémiques de trypanosomiose animale et humaine; ceux qui sont constitués par les importants gîtes à glossines des galeries forestières, et ceux qui, indépendants et isolés des grands gîtes, sont disséminés par-ci par-là, le long des petits cours d'eau, à végétations touffues, mais tributaires de fleuves et rivières à berges nues. Les premiers sont indestructibles. Bien que l'homme les ait fuis, ils restent infectés et très probablement le réservoir à virus doit être le sang d'un ou de plusieurs animaux sauvages, qui ne me paraissent pas encore suffisamment déterminés.

Ce sera sur la destruction de ces porteurs de germe que devra s'orienter la prophylaxie, si toutefois l'homme est capable de les atteindre. Les seconds sont bien moins dangereux; il est facile de les détruire par le débroussaillage; il serait même urgent de prendre cette mesure dans les territoires du Haut Sénégal et Niger, où ces gîtes jouent un rôle important dans la conservation et la diffusion des trypanosomioses animales.

30°. 31°. La trypanosomiose animale la plus répandue est la "Souma" dont l'agent étiologique est *Trypanosoma cazalboui*. Elle est enzootique dans toute la boucle du Niger, et détermine de graves épizooties dans les troupeaux, principalement chez les zébus qui représentent l'espèce de beaucoup la plus sensible.

Cette trypanosomiose infecte naturellement tous les bovidés; elle revêt chez eux une allure rapide, tuant en trois ou quatre semaines, parfois en huit jours; la mortalité fort élevée atteint 90 pour cent.

L'évolution lente est exceptionnelle. Certaines espèces, telle que le petit bœuf bambara, sont considérées, par beaucoup d'auteurs, comme beaucoup moins sensibles. Ce n'est point mon avis; ces animaux se sont toujours montrés expérimentalement très faciles à infecter et la maladie a évolué rapidement chez eux; j'ai pu étudier l'infection naturelle chez des génisses contaminées au laboratoire par des stomoxes; ces animaux sont morts après huit jours de maladie. Leur résistance n'est donc qu'apparente et leur acclimatation, en pays réputé inhabitable aux autres bovidés, relève de certains facteurs qui m'échappent. Je croirais volontiers que les raisons principales de cette immunité se trouvent être tout simplement les soins particuliers dont les entourent leurs propriétaires dans les colonies côtières, et l'absence en ces régions d'équidés que je considère comme le plus dangereux des réservoirs à virus.

Les chevaux et les ânes, en effet, sont beaucoup plus résistants au *Trypanosoma cazalboui*; chez eux

la maladie revêt toujours une allure chronique; elle ne tue qu'à longue échéance, après deux à trois ans et plus d'infection, et l'équidé, au cours de sa trypanosomiase, héberge souvent dans la circulation périphérique de nombreux parasites.

J'ai parcouru 180 kilomètres en six jours avec un cheval, en apparence de bonne santé, et dont le sang fourmillait de trypanosomes. Les ânes peuvent aussi être fortement parasités, sans cesser de fournir le sérieux effort que réclament d'eux les marchands ambulants, et les cultivateurs. Les équidés sont donc de dangereux porteurs de *Trypanosoma cazalbei* et ce sont eux, à mon avis, qui disséminent la maladie, et jouent un rôle prépondérant dans sa propagation.

La plupart des épizooties ont, donc, comme point de départ soit la fréquentation de cours d'eau infestés de glossines, soit, dans les régions indemnes de tsé-tsé, l'introduction dans le troupeau d'un animal, bovidé ou équidé, très parasité, dont le sang très infectieux favorise la transmission directe du trypanosome par les stomoxes.

Les glossines des petits cours d'eau s'infectent vraisemblablement en prenant leur repas sur des animaux domestiques malades; le rôle du gibier me paraît secondaire. Je n'ai point particulièrement étudié le degré d'infection des grands gîtes; aussi je n'émettrai aucune opinion sur l'origine de leur contamination.

Le débroussaillage des petits gîtes, l'embouteillage des troupeaux et des équidés en transhumance, évitant l'infection par transmission directe des troupeaux autochtones, l'abattage de tous les porteurs de germes, me paraissent être les bases principales d'une prophylaxie sérieuse.

Dans le Haut Sénégal et Niger *Trypanosoma cazalbei* est si rare chez les ovidés que j'ometts à dessein d'en parler.

Trypanosoma pecaui est moins répandu; il est extrêmement rare dans la vallée proprement dite du Niger; mais en revanche il a élu domicile dans les vastes gîtes à glossines du Bani, de la Volta Noire et de ses affluents.

Il parasite naturellement les équidés, les bovidés et les chiens. Équidés et bovidés réagissent différemment, mais à l'inverse de ce qui se passe avec le *Trypanosoma cazalbei*. Ici l'animal sensible est le cheval; c'est chez lui que la maladie évolue assez rapidement, en trois à huit semaines; la mort est la règle.

Rapidement l'animal est hors d'état de travailler, et de fournir les plus petites étapes. Son sang, riche en parasites, le rend dangereux pour ses voisins d'écurie, ou de pâturage; mais il n'est point capable de propager au loin la maladie.

Les ânes paraissent moins sensibles, et ceux que j'ai pu étudier dans la vallée de la Volta noire, au poste de Boromo, étaient en parfaite santé. Un léger larmoiement m'avait incité à examiner leur sang et j'avais eu la surprise d'y rencontrer de très nombreux parasites. Les propriétaires refusaient d'ailleurs l'abattage, prétextant les excellents services que ces animaux leur rendaient. J'eus l'idée de les faire marquer au fer rouge, et j'ai su qu'un an après mon passage ces ânes étaient encore très bien portants; j'ignore s'ils étaient encore porteurs de germes. Je reste convaincu que ce fait n'est pas exceptionnel et je ferai volontiers jouer un rôle prépondérant à ces équidés dans la diffusion de la maladie en pays d'élevage.

Les bovidés sont peu sensibles; leur parasitisme se traduit par quelques rares trypanosomes dans une goutte de sang; leur rôle est donc négligeable dans la transmission directe par les stomoxes et autres insectes hématophages. Seraient-ils de dangereux réservoirs à virus ou s'infecteront les glossines? C'est peu probable, parce qu'ils ne fréquentent point les galeries forestières et qu'il est toujours expérimentalement fort difficile d'infecter des tsé-tsé en les nourrissant d'un sang peu trypanosomé.

Parmi les autres animaux domestiques très sensibles à *Trypanosoma pecaui* et qu'un parasitisme périphérique très intense rend dangereux, je citerai le chien, qui n'a jamais pu s'acclimater dans les foyers enzootiques à *Tr. pecaui*. C'est chez lui que la maladie évolue le plus rapidement; il est enlevé en sept à huit jours. Très

recherché par les tsé-tsé qui le piquent beaucoup plus volontiers que l'homme, il est harcelé par ces insectes, et se jette à l'eau plutôt que de rester dans la barque; j'ai dû attacher les trois que j'avais emmenés; cinq jours après le départ, ils étaient tous parasités.

Équidés et chiens sont donc les seuls animaux sensibles dont j'envisagerai le rôle dans la propagation de cette trypanosomiase.

Ils s'infectent toujours dans ce que j'ai appelé "les grands gîtes." Comme ils ne les fréquentent qu'accidentellement, que la plupart d'entre eux meurent très vite et n'ont pas le temps d'aider à la diffusion de la maladie par transmission directe, comme les régions les plus infectées, où l'animal sensible contracte à coup sûr la trypanosomiase, sont parfaitement abandonnées, il paraît évident que le principal réservoir à virus n'est point, comme pour *Trypanosoma cazalbei*, l'animal domestique malade, mais bien un animal sauvage. J'ignore à quel genre il appartient, si c'est un antilopidé, un saurien ou un pachyderme. Une étude serrée du problème dans les galeries forestières du Banifing pourrait peut-être trancher la question.

La prophylaxie découlera de la connaissance précise du ou des réservoirs à virus. L'abattage de l'animal trypanosé reste toujours une excellente mesure. On y adjoindra le débroussaillage des rives limité à 500 mètres pour permettre de franchir sans danger les zones dangereuses.

Je n'ai rencontré le *Trypanosoma dimorphum* que chez les chiens de la vallée de la Volta Noire (voir "Bulletin de la Société de Pathologie Exotique 1912.") C'est un parasite peu répandu dans les territoires que j'ai parcourus; il serait plus fréquemment observé chez les bovidés, les équidés, et les chiens des colonies côtières. Chez les chiens de la Volta la maladie revêt une allure chronique, cachectisante qui contraste singulièrement avec l'évolution rapidement mortelle chez ces animaux de *Trypanosoma pecaui*.

Trypanosoma dimorphum parasitait des chiens qui vivaient au voisinage des gîtes à glossines et étaient certainement piqués par ces insectes. Je n'ai point observé la maladie chez les bovidés ou les équidés.

32°, 33°. Je n'ai eu qu'une seule fois l'occasion de faire des recherches sur le degré d'infection d'équidés dans les écuries des haras de la colonie, sises à Koulikoro. Plusieurs étalons étaient morts de *Trypanosomiase cazalbei*; je voulais purger les écuries de tout animal malade encore peu parasité dont l'examen direct du sang ne décelait aucun parasite, mais dont le sang pouvait recéler d'un moment à l'autre de nombreux trypanosomes et favoriser la transmission directe du virus par les insectes hématophages autres que les tsé-tsé.

J'eus recours à l'inoculation à un animal sensible, le mouton à laine. C'est d'ailleurs la seule méthode recommandable; l'examen direct ou après centrifugation du sang donne des résultats insuffisants.

Pour éteindre la maladie dans ces écuries éloignées des zones à tsé-tsé, mais où le stomoxe suffit pour entretenir la maladie, il est nécessaire de dépister les porteurs de germes afin de les abattre aussitôt.

Les animaux hypersensibles sont:—

Pour *Trypanosoma gambiense* le cercopithecus pathas.

Pour *Trypanosoma cazalbei* le mouton à laine et le boeuf zébu.

Pour *Trypanosoma pecaui* le chien et le chat.

La maladie est fatale pour tous ces animaux.

TRYPANOSOMIASES CHEZ LES ANIMAUX SAUVAGES.

34°. Les animaux sauvages sont communs dans toute la colonie; les grandes antilopes ne se rencontrent que loin des agglomérations indigènes, dans les vastes plaines désertiques, au voisinage des rivières à tsé-tsé. La petite espèce, de la taille d'une chèvre, est plus répandue, et vit dans les plaines cultivées, près des villages.

35°, 36°. Personnellement je n'ai jamais chassé le gros gibier, mais des amis m'ont adressé des frottis de sang d'antilopes de la grande espèce, bubale, koudou; ces frottis, très épais, étaient prélevés aussitôt la mort de l'animal; je n'y ai jamais vu de trypanosome.

J'ai pu sur le Bani me procurer trois caïmans vivants; leur sang était stérile.

37°, 38°, 39°. Aucune opinion.

40°. Pendant la saison sèche, de décembre en mai, les animaux sauvages sont obligés d'aller boire à la rivière. Ils la fréquentent deux fois par jour et, puisqu'ils sont expérimentalement sensibles aux trypanosomiasés, ils doivent certainement le contracter au même titre que l'animal domestique que l'on infecte facilement en saison sèche, si on lui fait fréquenter des cours d'eau à glossines.

Quel est celui d'entre eux qui devient réservoir à virus, c'est-à-dire contracte une maladie chronique, ne le tuant qu'à longue échéance, et présente dans son sang des parasites en assez grand nombre pour infecter les glossines? Je le crois encore inconnu pour tous les trypanosomes pathogènes. Serait-ce l'antilope pour *Trypanosoma gambiense*? c'est peu probable, et je répète que les principaux foyers de trypanose humaine dans le Haut Sénégal et Niger sont représentés par les vastes gîtes à glossines des bords de la Volta noire et du Banifing, qui cessent d'être fréquentés par les animaux pendant les quatre mois de l'année où les tsé-tsé sont le plus infectants. Il est bien difficile d'admettre qu'en octobre le cinq pour cent environ des glossines, infectées naturellement par *Trypanosoma gambiense*, ait puisé le virus dans le sang des antilopes, aux mois de mai ou juin.

Puisque ce centre endémique de trypanose est déserté à la fois par l'homme, les animaux domestiques et le gros gibier, n'est-il pas logique de songer à éliminer, comme réservoir possible à virus, les hôtes permanents du fleuve, hippopotames et caïmans, avant d'entreprendre de coûteuses et difficiles recherches sur les antilopes.

Passons en revue, maintenant, les différentes mesures qui permettraient d'espérer voir disparaître de la bouche du Niger certains foyers de trypanose et nous feraient mieux connaître les sources d'infection des grands gîtes à glossines.

DE LA PROPHYLAXIE DES TRYPANOSOMIASÉS.

41°. — I. Maladie du Sommeil.

1°. *Petits foyers d'endémicité.* — En vue de leur disparition nous conseillerons le débroussaillage, relativement facile, des gîtes permanents à glossines, fréquentés par l'indigène, et très probablement infectés par l'homme au début de sa maladie.

L'isolement du malade sera nécessaire pour éviter la transmission directe par les insectes hématophages. Le rôle du stomoxe dans la diffusion de *Trypanosoma cazalbei* dans les troupeaux éloignés des zones à tsé-tsé, laisse supposer qu'un ecto-parasite pourrait jouer le même rôle chez l'homme. Les flagelles sont indiscutablement beaucoup plus rares, à la deuxième période de la maladie, dans le sang du trypanosé du Niger que dans celui du trypanosé du Congo; mais ils peuvent s'y montrer par intermittence en nombre suffisant pour rendre le malade dangereux pour les personnes qui cohabitent avec lui. Pourquoi un pou, une punaise, ou un moustique ne joueraient-ils pas le rôle de la lancette infectée? Ce n'est pas évidemment une certitude, mais soyons en la circonstance aussi sages que les indigènes, et isolons comme eux les trypanosomés. Ce but est d'autant plus facile à atteindre que son application ne peut soulever des difficultés.

Cet isolement ne s'adresse malheureusement qu'aux trypanosés évidents, qu'à ceux que l'on conduit aux médecins, ou qu'une hypertrophie ganglionnaire, seul symptôme visible, aura permis de découvrir. Ils représentent toujours la minorité des porteurs de trypanosomes; ceux que nous devons considérer comme les plus dangereux, ceux qui, parasités depuis peu, restent valides échapperont le plus souvent à nos investigations. Ce sont ceux-là qui constituent un redoutable réservoir à virus; ce sont eux qui, fréquentant les petits gîtes à glossines, les infectent. Il n'y a rien à faire contre eux, puisqu'ils demeurent introuvables, et c'est pourquoi la seule mesure efficace doit viser la destruction de l'insecte, deuxième hôte du parasite. Elle est très réalisable et il suffit de débroussailler les petits cours d'eau. On les asséchera, m'objecteront certains coloniaux. Peu importe, l'eau ne manque point dans les vallées, où il est facile de creuser des puits. Dans le gibier seul pourra en souffrir; il émigrera.

Débroussaillage et isolement des malades, voilà les deux moyens que je préconise contre les petits foyers.

Une expérience, qui ne modifierait point la base de cette prophylaxie, mais dont le résultat serait fort intéressant à connaître et riche en déductions, pourrait être entreprise dans ces petits foyers d'hypnose. Son but serait de savoir si l'homme malade est l'unique réservoir à virus où s'infectent les glossines, ou bien s'il en existe un autre en dehors de l'homme. On rechercherait tout d'abord un foyer, dans le genre de celui qu'étudiait A. Leger à Koulikoro, c'est-à-dire nettement isolé et indépendant des grands gîtes. On en interdirait l'accès aux indigènes par une surveillance, qui pourrait coûter, au maximum, 3,000 francs; les glossines prendraient leurs repas sur les animaux sauvages, fréquentant le gîte. Si après le laps de temps, que les entomologistes reconnaissent suffisant pour le rajeunissement du gîte, les nouvelles générations étaient infectées par *Trypanosoma gambiense*, on serait en droit de conclure ou bien que la maladie est héréditaire chez la mouche, ou bien qu'il existe un réservoir à virus chez l'animal sauvage. On saurait, de la sorte, s'il est possible d'assainir un gîte infecté par le simple éloignement de l'homme.

2°. *Dans les grands centres endémiques* de trypanose humaine, le long des rives boisées de la Volta noire, du Bani et du Banifing, l'hypnose règne en maîtresse. L'homme a fui ces régions, et l'expérience que nous voudrions voir tenter pour les petits gîtes est ici réalisée depuis plusieurs années. Et cependant les glossines continuent à transmettre la maladie du sommeil, et l'accès, en juillet et août, de ces gîtes est pour l'homme la certitude de s'y infecter.

Le trypanosé ne joue donc aucun rôle dans le maintien de l'endémicité dans ces régions. Avant d'accepter les vues de certains savants, qui veulent faire jouer un rôle important aux antilopes, et qui n'ont encore apporté aucune preuve indiscutable de ce rôle, avant d'entreprendre avec ces animaux des expériences très coûteuses et fort difficiles, il me paraît logique d'éliminer tout d'abord, comme réservoir à virus, les hôtes permanents des fleuves, l'hippopotame et le caïman.

Les expériences me paraissent plus faciles qu'avec l'antilope et elles sont réalisables assez économiquement dans les régions que j'ai parcourues.

Le Banifing et la Volta noire représentent les gîtes les plus infectés par *Trypanosoma gambiense* et *Trypanosoma pecandi*. Puisqu'ils sont à la fois désertés par l'homme et les animaux domestiques, on aura l'avantage de rechercher par la même expérience un réservoir possible pour deux virus différents.

Des mœurs de l'hippopotame un seul fait est à relever, c'est qu'il ne quitte jamais le fleuve le jour. Sa chasse, du lever au coucher du soleil, n'intéresse que celui dont le but est de donner de la viande fraîche à manger aux indigènes. Le pachyderme frappé à mort, coule et ne revient flotter à la surface de l'eau que sept à huit heures après sa mort. C'est un peu tard pour que l'examen du sang ou son inoculation à l'animal sensible donne des résultats certains. Pour connaître le degré d'infection de ce pachyderme, il me paraît indispensable de le chasser de nuit, de l'abattre assez loin du fleuve, lorsqu'il pait en toute tranquillité. Ce serait donc la chasse à l'affût, celle mettant à la disposition du médecin un animal qui vient de mourir, qui me paraît préférable.

L'expérimentateur, dont le laboratoire ambulatoire peut être installé à quatre ou cinq kilomètres de la rivière, dans une zone indemne de tsé-tsé, aura à sa disposition un nombre suffisant de *Cercopithecus pathas* pour *Trypanosoma gambiense* et de chiens pour *Trypanosoma pecandi*. Il est incontestable que l'examen direct d'un frottis épais pourra parfois faire découvrir des porteurs de nombreux trypanosomes; mais l'inoculation à l'animal sensible reste le meilleur moyen de déceler un parasitisme latent, susceptible, par intermittence, de poussées infectieuses avec parasites nombreux dans le sang. Ces poussées existent chez l'antilope, qui peut devenir périodiquement capable d'infecter les glossines. Mais son rôle reste bien effacé, puisqu'elle vit pendant six mois loin des rivières à tsé-tsé.

Un capitaine a pu tuer 18 hippopotames en deux après-midis; on peut donc espérer abattre en quelques nuits une douzaine de ces pachydermes.

Si leur sang est stérile, je crois que l'on peut définitivement les considérer comme ne jouant aucun rôle dans l'infection des *glossines palpalis* ou *tachinoïdes* par *Trypanosoma gambiense* ou *pecaudi*.

Les caïmans sont également piqués par les tsé-tsé. Frappés au cerveau par la balle d'un adroit tireur, ils meurent sur place. Ils sont très abondants le long de la Volta Noire, où il serait possible d'étudier leur parasitisme.

Ainsi donc, sans vouloir affirmer qu'il est très facile d'expérimenter sur ces deux hôtes permanents des fleuves à tsé-tsé, je considère que les recherches sont possibles sans que l'on soit entraîné à de grosses dépenses. Cette étude devrait être entreprise en pleine saison des pluies, en juillet et août.

La destruction de ces animaux s'imposerait, s'ils étaient la cause principale de l'infection des tsé-tsé. Celle de l'hippopotame est très facile; je n'en dirai pas autant du caïman; il n'est cependant pas impossible d'en diminuer considérablement le nombre.

II.—*Trypanosomiasis Animales.*

Beaucoup de centres enzootiques à *Trypanosoma cazalboni* sont entretenus par les glossines s'infectant sur l'animal domestique malade; ils se rencontrent d'ailleurs dans des régions cultivées où le gros gibier fait défaut.

La mesure prophylactique qui s'imposerait serait évidemment l'interdiction absolue aux bergers de conduire leurs troupeaux aux rivières à tsé-tsé; elle est d'application difficile et il sera souvent plus facile d'obtenir du village le débroussaillage du cours d'eau. Si le travail est trop considérable, on pourra se contenter de créer des clairières assez vastes d'environ 500 mètres de long sur chaque rive, en marquant avec des bambous au milieu l'endroit où les animaux peuvent aller boire.

Dans les régions d'élevage du cheval, dans les écuries de l'administration qui abritent toujours d'assez nombreux chevaux, dans les haras, on ne se mettra à l'abri de la contagion directe par les stomoxes qu'en évitant le contact avec tout équidé malade.

On sait que ce *Trypanosoma cazalboni* est peu virulent pour le cheval, et ne le tue qu'à longue échéance. Il faut systématiquement, par les procédés indiqués plus haut (examen du sang, inoculation au mouton), rechercher l'animal malade et l'abattre sans la moindre hésitation. Cette résistance si différente des équidés et des bovidés aux trypanosomes du Niger, qui rend le cheval réservoir à virus par *Trypanosoma cazalboni*, et le bovidé foyer d'infection à *Trypanosoma pecaudi*, entraîne naturellement à préconiser la ségrégation au pâturage de ces différentes espèces domestiques. Elle conduit également à imposer des gîtes d'étapes, isolés du cheptel local, aux troupeaux et aux équidés en déplacement.

Ces mesures rendraient indiscutablement de grands services à l'élevage, déjà très prospère dans la majeure partie de la boucle du Niger. L'administration estime que l'on pourrait, sans danger pour l'avenir de l'élevage, exporter chaque année 200.000 têtes de bétail. Malheureusement ces riches pâturages, sont, au Nord, à l'Est et à l'Ouest, limitrophes des régions sablonneuses, désertiques, et l'exportation des troupeaux sur pied vers le Sud se heurte à une barrière actuellement infranchissable, à un lacs de rivières à glossines, source d'infection inévitable par *Trypanosoma cazalboni* ou *pecaudi*. Cette voie n'est possible que si l'on crée des routes sanitaires, traversant les cours d'eau en des points débroussaillés, et si les colonies côtières n'importent que la quantité d'animaux strictement nécessaire à leur consommation courante. On a eu le tort d'amener en Côte d'Ivoire et en Gold-Coast des troupeaux de 3 à 400 têtes, qui, ne pouvant être immédiatement sacrifiés, étaient rapidement décimés dans ces régions très irriguées, où les tsé-tsé se rencontrent partout. L'exportation devrait donc se faire par petits paquets d'une cinquantaine de têtes, qui seraient livrées à la boucherie en une dizaine de jours au maximum. L'éleveur soudanais pourrait de la sorte trouver sur les marchés de la côte un écoulement fructueux de ses importantes réserves de bétail.

42°. Aucune mesure prophylactique n'a été prise dans les districts que j'ai visités. Celles que j'ai préconi-

sées en 1910 n'ont pas encore été mises en pratique. Elles ne pouvaient viser la disparition d'une endémie, entretenue par de vastes gîtes à tsé-tsé indestructibles, infectés par la présence d'un réservoir à virus animal inconnu. Leur but était plus modeste; il s'adressait simplement aux cas de transmission directe par les ecto-parasites, poux, puces, punaises ou les moustiques, et cherchait à les éviter par le dépistage des malades et leur isolement. Les petits foyers endémiques s'éteindraient naturellement, quand on voudra bien débrousser les petites rivières, ce qui est facile. Les grands gîtes infecteront un certain nombre d'imprudents qui s'aventureront chez eux; comme ces trypanosomés sont en petit nombre, il est sage de ne proposer à leur sujet à l'administration que des moyens économiques, permettant d'éviter les dangers de leur cohabitation avec des personnes saines. C'est évidemment une prophylaxie qui ne s'adresse qu'à une cause d'infection bien rare. Aussi la recherche des trypanosomés, en vue de leur isolement dans une hyposerie, était-elle confiée, dans mon projet, à des médecins vaccinateurs, qui, tout en rendant le grand service d'immuniser les populations contre la variole, pouvaient, grâce à un petit matériel bactériologique de la valeur de 1.000 francs environ, examiner le sang et le suc ganglionnaire d'individus suspects de trypanosomie. Tout examen positif entraînait l'évacuation immédiate du malade sur une hyposerie centrale, sise au voisinage du Chef-lieu de la province, et où résidait un médecin.

L'indigène déserte ces régions maudites, qui, aujourd'hui, sont totalement dépeuplées. C'est la seule mesure rationnelle applicable dans ces immenses foyers où la mouche est à l'abri de nos coups, et où l'on trouve une source d'infection encore inconnue.

Entretienons l'indigène dans cette sainte terreur des galeries forestières, conseillons-lui de s'abstenir d'aller au fleuve, et préparons-lui des gués assainis par un déboisement partiel mais suffisant pour lui permettre de sortir du cercle dans lequel l'enferment quelquefois les rivières boisées.

Atoxiler toute la population, comme le conseillent les Allemands au Congo, ne peut être considéré comme une mesure efficace que si l'on admet un seul réservoir à virus, l'homme. Or je suis avec les nombreux observateurs qui sont persuadés de l'existence d'un autre réservoir que l'homme.

43°. Je considère comme possible et même comme unique moyen d'assainissement de certains foyers isolés, le débroussalement des rives des cours d'eau.

Il est nécessaire de confier à des hygiénistes, parfaitement au courant de la question des trypanosomiasis, le soin d'étudier la répartition géographique de tous les centres à trypanose, et les mesures applicables à chaque foyer.

Une mesure générale, facile à prendre, consisterait dans le débroussalement de tous les points infectés de glossines, où l'indigène va puiser son eau d'alimentation, va laver son linge, franchit les cours d'eau, où vont boire les animaux domestiques. Ce déboisement doit être important, et s'étendre sur 500 mètres de large. Il serait peu coûteux puisqu'on l'imposerait aux habitants des villages, tributaires en eau de ces rivières.

L'extermination du gibier dans la région que je connais me paraît impossible; je ne veux même pas la discuter.

L'indigène a fui depuis longtemps ces grands foyers de trypanose; il n'y a donc aucun effort à faire pour maintenir les villages dans une zone saine.

La ségrégation des malades est très facile; mais elle n'est appelée à ne rendre que de bien minimes services. Je ne la considère comme n'atteignant que les malades à la deuxième période, c'est-à-dire ceux qui ne sont que, par intermittence et très rarement, fortement parasités. Le trypanosé dangereux échappera toujours à nos investigations; car il est réservoir à virus à une période de sa maladie où aucun symptôme morbide n'attire l'attention du médecin.

La destruction du bétail infecté s'impose.

La lutte contre la tsé-tsé me paraît bien difficile; je ne vois pas de moyen plus facile que le débroussalement pour la faire disparaître.

44°. Une destruction du gibier ne peut être tentée que lorsqu'on aura apporté des preuves certaines de

son rôle dans la conservation et la propagation des trypanosomiasés. Cette preuve est encore à faire pour les virus de la boucle du Niger.

Les faits, connus jusqu'à ce jour, prouvent tout simplement que les antilopes, les sangliers sont sensibles aux trypanosomes pathogènes pour l'homme et les animaux domestiques ; mais on n'a pas encore le droit de conclure qu'ils sont des réservoirs à virus, jouant un rôle important dans la propagation de la maladie.

J'ai indiqué plus haut que des recherches pouvaient être entreprises dans le but de préciser le rôle de l'hippopotame ou du caïman ; leur exécution, grâce à l'aide du gouvernement local, ne serait pas très coûteuse. Je crois qu'avec une centaine de mille

francs on paierait les frais d'une mission d'études, comprenant deux bactériologistes, qui séjourneraient 18 mois dans les territoires à tsé-tsé de la boucle du Niger, dans ces intéressants foyers à *Trypanosoma gambiense*, à *Trypanosoma pecaui*, et à *Trypanosoma cazabou*.

45°. L'atoxyl me paraît être le médicament de choix de *Trypanosoma gambiense*.

Je n'ai aucune opinion sur l'agent chimique actif contre le *Trypanosoma rhodesiense*.

Je n'ai eu que des résultats négatifs dans mes essais de traitement de *Trypanosoma cazabou* et de *Trypanosoma pecaui* avec l'atoxyl, les couleurs de benzidine, l'orpiment, l'arsénophénylglycine.

DOCTEUR AYRES KOPKE, ÉCOLE DE MÉDECINE TROPICALE, LISBONNE.

Mes investigations sur la maladie du sommeil ont été toujours faites sur le point de vue clinique. Ce fut spécialement chargé de l'observation des malades, des études anatomo-pathologiques et des investigations thérapeutiques, que j'ai fait partie de la première mission portugaise qui a étudié la maladie du sommeil au Principe et à Angola en 1901 et 1902. Après cela a été créée l'École de Médecine Tropicale de Lisbonne et, comme professeur de cette École, je suis allé en mission d'étude à S. Thomé et à Angola en 1904. Depuis cette date et jusqu'à présent j'ai toujours eu à l'Hôpital Colonial de Lisbonne, annexe à l'École de Médecine Tropicale, des malades trypanosomés, venus pour la plupart de l'île du Principe et quelques autres aussi de Angola (régions des Dembos, Dondo, Zenza, Libollo, Cazengo, Ambaca, Massangano, Muxima, Golungo, N'Dala-Tando) et du Congo portugais, de façon qu'il y a déjà à peu près 13 ans que sans interruption je suis chargé de soigner des cas de maladie du sommeil.

Vue la provenance des malades, ils étaient tous porteurs de infection par le trypanosome gambiense et je n'ai jamais eu l'occasion d'observer des personnes infectées par le virus rhodesiense ; mes conclusions sur la thérapeutique se rapportent par conséquent seulement aux cas de la première catégorie.

Comme ma pratique sur la maladie du sommeil a été faite surtout au point de vue clinique, je me rapporterais dans cette communication principalement au sujet 45 des questions que vous avez posées.

De mes observations et des cas publiés par d'autres auteurs, je conclus que l'atoxyl et ses dérivés (arsénophénylglycine, salvarsan, néo-salvarsan, galyl, ludy) peuvent guérir les malades à la première phase de la maladie, c'est-à-dire avant que les flagellés aient envahi le liquide céphalo-rachidien. Peu de mes malades ont été dans ces circonstances parce que généralement ils arrivent à Lisbonne avec plus d'un an de maladie et pour la plupart ayant déjà les trypanosomes sous les méninges. Quelques-uns sont sortis de l'hôpital colonial, annexe à notre École de Médecine Tropicale, encore vivants ; je citerai les suivants :

No. XLIV. — Nègre ; trypanosomes dans le suc ganglionnaire, pas de trypanosomes dans le liquide céphalo-rachidien. Traitement par l'atoxyl terminé le 4-XII-1906. Après la cessation des injections il est resté en observation à Lisbonne jusqu'au 22-XI-1909, et après cette date il est parti pour S. Paul de Loanda, où il a vécu jusqu'à présent, étant employé comme servant dans le laboratoire de bactériologie et par conséquent soumis à l'observation de Mr. Correia Mendes. Il n'a présenté après la terminaison du traitement aucun signe suspect, n'ayant pas eu de probabilité d'être réinfecté, vu qu'il a toujours résidé, après son départ du Principe, dans des régions indemnes de trypanosomiasé. Par conséquent la guérison de ce malade se maintient déjà il y a plus de sept ans.

No. LXIII. — Mulâtresse ; trypanosomes dans le sang, ponction lombaire négative. Terminaison du traitement le 21-V-1908. Sans symptômes jusqu'à sa sortie de l'hôpital le 22-VIII-1909, plus de un an après. Elle est partie pour l'île de St. Iago, Cap Vert.

No. LXXVI. — Nègre ; trypanosomes dans le sang, pas de trypanosomes dans le liquide céphalo-rachidien. Traitement terminé le 16-I-1909. Parti pour l'île de

S. Thomé le 9-III-1909. Il était encore vivant et sans avoir eu de symptômes suspects le 20-III-1910, dernières références que j'ai pu obtenir jusqu'à présent.

No. LXXVII. — Nègre ; diagnostic fait au Principe par l'examen du sang ; il y fut traité par l'atoxyl. Arrivé à Lisbonne le 7-X-1908 ; les examens du sang et la ponction lombaire que je lui ai faits, ont été négatifs. Je ne lui ai administré aucun traitement. Parti pour l'île de S. Thomé le 7-III-1909. À S. Thomé il n'a pas eu de manifestations morbides ; examens du sang et ponction lombaire négatifs. Encore vivant et bien portant le 20-III-1910.

No. LXXIX. — Nègresse ; examen du sang positif fait au Principe ; elle y fut traitée par l'atoxyl. Arrivée à Lisbonne le 7-X-1908 ; je n'ai pas trouvé de trypanosomes par l'examen du sang, mais une ponction lombaire exécutée le 1-XII-1908 a démontré l'existence de trypanosomes. Traitée à Lisbonne par l'atoxyl jusqu'au 28-I-1908. Partie pour l'île de S. Thomé le 7-III-1909. Elle était encore vivante le 23-III-1910, mais avait eu des symptômes morbides ayant nécessité la répétition du traitement par l'atoxyl.

No. LXXXI. — Nègre ; traité au Principe par l'atoxyl. Arrivé à Lisbonne le 21-X-1908 ; ponction lombaire positive le 3-XI-1908 ; terminaison du traitement à Lisbonne le 5-II-1909. Parti pour S. Thomé le 7-III-1909. J'ai su qu'il a eu à S. Thomé des troubles mentaux importants, avec idées de persécution et impulsions homicides ; il y a reçu des injections de atoxyl et était encore vivant le 20-III-1910.

No. LXXXIII. — Nègre ; examen du sang fait au Principe donnant des trypanosomes, ponction lombaire négative ; traité par l'atoxyl et injections intraveineuses de bichlorure de mercure. Arrivé à Lisbonne le 8-X-1908. Examen du sang et ponction lombaire faits par moi négatifs ; pas de symptômes suspects. Parti pour S. Thomé le 7-III-1909. Pas de rechute à S. Thomé ; les dernières nouvelles que j'ai reçues à son égard datent de 20-III-1910.

No. LXXXIV. — Nègre ; trypanosomes trouvés dans le sang au Principe mais pas dans le liquide céphalo-rachidien ; traité par l'atoxyl. Arrivé à Lisbonne le 8-X-1908 ; examen du sang et ponction lombaire faits par moi négatifs ; il n'a pas eu à Lisbonne de symptômes morbides et n'a reçu aucun traitement. Parti pour S. Thomé le 7-III-1909. À S. Thomé il s'est toujours bien porté jusqu'au 20-III-1910, date des dernières nouvelles que j'ai reçues à son égard.

No. LXXXV. — Nègre, diagnostiqué comme un cas de trypanosomiasé par l'examen du sang fait au Principe, où il fut traité par l'atoxyl. Arrivé à Lisbonne le 21-X-1908 ; les examens que j'ai fait du sang et du liquide céphalo-rachidien ont été négatifs ; il n'a pas eu de symptômes suspects et n'a reçu aucun traitement. Parti pour S. Thomé le 7-III-1909. Il a eu à S. Thomé une rechute avec trypanosomes dans le sang. Encore vivant le 20-III-1910.

Comme on peut bien le déduire j'avais l'intention en envoyant ces malades pour S. Thomé de les maintenir dans une région indemne de glossines, soumis au contrôle de l'observation médicale, de façon à vérifier si longtemps que possible le résultat du traitement. J'ai reçu des nouvelles à son égard seulement pendant un an. Je ferai de mon possible pour savoir lesquels d'entre eux seront encore vivants.

No. LXXXVIII. — Blanc ; a été infecté dans la région des Dembos et pendant une expédition militaire

en novembre de 1908. Arrivé à Lisbonne le 19-X-1909; examen du sang et du suc ganglionnaire positifs le 21-X-1909. Traitement par l'atoxyl commencé le 8-XI-1909 et terminé le 28-XI-1910. Les ultérieurs examens du sang retiré le 9-III, 23-VII, 25-VIII, 22-XI-1910, 30-II, 30-V, et 5-XII-1911, toujours par ponction veineuse, inoculé à des rats et soumis à la centrifugation fractionnée pour l'exécution des préparations, ont été constamment négatifs. Ponction lombaire faite le 30-V-1911 sans trypanosomes et sans altération de la formule leucocytaire. Après la terminaison du traitement il s'est maintenu sans trypanosomes trouvables et sans symptômes suspects. Je l'ai revu le 12-I-1914; il était bien portant avec très bon aspect; l'examen du sang obtenu par ponction veineuse ayant donné de même un résultat négatif. Par conséquent ce malade se maintient comme guéri presque quatre ans après la terminaison du traitement et six après la date probable de l'inoculation par les piqûres des glossines qui abondent dans les régions des Dembos où se réalisa l'expédition militaire dont il a fait partie.

Les malades No. CL, CIII, CIII, nègres et CV, négresse furent tous des cas avec trypanosomes dans le sang, traités à Lisbonne par le salvarsan pendant l'année 1911, comme je l'ai dit dans ma communication au Congrès International de Médecine réuni à Londres le mois d'août 1913; sont sortis de l'Hôpital colonial de Lisbonne les trois premiers le 22-XI-1911 et la dernière le 22-I-1912, pour retourner à son pays natal, l'île de St. Iago du Cap Vert. J'ai revu le No. CIII, qui est revenu à Lisbonne, où il a séjourné depuis le 20-I-1913 jusqu'au 13-III-1913; il se maintenait sans symptômes; un examen du sang fait par ponction veineuse avec inoculation consécutive à deux rats et centrifugation fractionnée d'une partie pour l'exécution des préparations a donné des résultats négatifs et une ponction lombaire a aussi été négative. Il n'a pas présenté de récidive un an et demi après la terminaison du traitement; la dernière injection de salvarsan fut faite le 2-X-1911.

No. CVIII.—Blanc; maladie acquise au Congo portugais et diagnostiquée, suivant qu'il m'a dit, par le Dr. Mercier Gamble de la Baptist Missionary Society en 20-I-1912 par ponction ganglionnaire. Arrivé à Lisbonne le mois de mars de 1912; ponction ganglionnaire positive le 19-III-1912. Pas de symptômes nerveux; ganglions cervicaux antérieurs et sus-culculaires droits assez augmentés de volume, visibles, comme de grosses amandes; éruption cutanée dierète, constituée par des plaques érythémateuses dans le tronc. Traitement par l'atoxyl commencé le 20-III-1912 et après cette date suivi régulièrement avec plusieurs périodes de repos. Les ganglions ont beaucoup diminué de volume et la maladie se porte actuellement comme une personne en parfaite santé. Divers examens du sang obtenus par ponction veineuse, dont une partie a été inoculé à des rats et une autre soumise à la centrifugation fractionnée pour l'exécution des préparations, ont été toujours négatifs. Il persiste comme guéri il y a déjà deux ans.

En me rapportant seulement aux cas que j'ai pu suivre jusqu'à présent, je peux dire que de mes malades traités par l'atoxyl seul et qui n'avaient pas de trypanosomes dans le liquide céphalo-rachidien, il y a le No. XLIV, dont la guérison se maintient il y a déjà sept ans, le No. LXXXVIII, il y a quatre ans et le No. CVIII, il y a deux ans. C'est un petit nombre bien sûr, mais, comme je l'ai déjà dit, presque tous mes malades m'arrivent ayant déjà le liquide céphalo-rachidien envahi par les trypanosomes, et malheureusement dans ces circonstances je n'ai obtenu aucun cas définitivement favorable par les médicaments que j'ai employés. Cependant le premier cas n'ayant pas eu de récidive durant les sept années qui se sont suivies à la terminaison du traitement, démontre bien que la guérison est possible, quand la médication est commencée avant l'invasion sous-arachnoïdienne.

Les vraies entraves dans la thérapeutique de la trypanosomiase gambiense chez l'homme sont la toxicorésistance des trypanosomes, c'est-à-dire son habitude aux divers médicaments, et l'imperméabilité des méninges qui ne laissent point passer les remèdes ou seulement dans des quantités minimes et après un usage prolongé. Contre le premier obstacle on

lutte par l'emploi de la médication mixte et comme succédanés les plus importants des arsénicaux on doit citer les dérivés de l'antimoine, dont l'émétique a rendu possibles des guérisons, me suffisant de citer ici celle du Dr. Kérandel, infecté au Congo, qui n'avait pas les trypanosomes sous les méninges. Contre la deuxième difficulté il fallait tenter la lutte en injectant les médicaments sous l'arachnoïde après ponction lombaire. C'est ce que j'ai fait le premier, employant le lysol, diverses matières colorantes et l'atoxyl, sans pouvoir obtenir des résultats satisfaisants. Dernièrement, comme je l'ai dit au Congrès de Londres, j'ai employé le néo-salvarsan en injections sous-arachnoïdiennes; ces injections n'ont pas été exemptes d'inconvénients, cependant la mort n'en fut pas la suite immédiate contrairement à ce qu'ont dit F. Heckenroth et M. Blanchard dans sa communication présentée à la Société de Pathologie Exotique de Paris, dans la séance du 14 janvier 1914. Un de mes malades a vécu trois mois après le traitement sous-méningé et l'autre quatre mois et demi; chez ce dernier une ponction lombaire, faite à peu près quatre mois après l'injection sous-arachnoïdienne du néo-salvarsan, révélait encore la présence des flagellés, par conséquent je n'avais pas obtenu la stérilisation définitive du liquide céphalo-rachidien.

Du reste dans les cas traités par Heckenroth et Blanchard, nonobstant les ponctions lombaires faites après le traitement sous-arachnoïdien avoir donné des résultats négatifs, l'état des malades n'en profita pas, ce que les auteurs attribuent à l'irréversible altération des centres nerveux. Pour moi je pense que le résultat négatif d'une ponction lombaire n'est pas suffisant pour affirmer que les trypanosomes n'aient pas persisté en minime quantité dans le liquide céphalo-rachidien et ne puissent pas repulluler plus tard. Ce qu'est indiqué c'est d'essayer de faire le traitement sous-arachnoïdien le plus tôt possible après l'invasion sous-méningée, par conséquent de procéder tant que possible systématiquement à l'examen du liquide céphalo-rachidien des cas de trypanosomiase humaine, même quand les malades ne présentent pas encore des symptômes nerveux. Dans la paralysie générale le Dr. Bériol de Lyon a fait la ponction cérébrale suivie de l'injection de médicaments, avec l'intention d'une lutte si locale que possible contre les tréponèmes dans la trypanosomiase humaine, arrivée à la phase nerveuse, une telle voie d'application des médicaments est, en thèse, indiquée; je doute cependant qu'on la puisse mettre en pratique chez les indigènes à la phase de la maladie pendant laquelle cette façon d'appliquer les remèdes pourra encore être présumée utile; je crois que sans être soumis à l'anesthésie générale et sachant ce qu'on leur veut faire, ils ne le permettront pas.

Je fais maintenant des essais de thérapeutique sous-arachnoïdienne avec le gallyl d'accord avec Mr. le Prof. Mouneyrat.

Tout cela pour ce qui regarde l'action curative des médicaments déjà utilisés contre la maladie du sommeil. Relativement à son action prophylactique il n'y a pas de doute que tous les médicaments détruisant rapidement le trypanosome gambiense dans le sang, pourront contribuer d'une façon très importante pour éviter la dissémination de la maladie du sommeil dans les régions où elle est due à ce flagellé.

Chez mes malades l'évolution de la maladie a été toujours supérieure à un an, même dans les cas observés à une période dans laquelle je n'avais pas encore employé l'atoxyl comme agent thérapeutique.

Je n'ai jamais observé des opacités de la cornée comme conséquence de la trypanosomiase chez l'homme. Quelques-uns de mes malades ont eu des névrites optiques après l'usage de l'atoxyl, comme je l'ai communiqué à la première Conférence sur la maladie du sommeil, réunie à Londres le mois de juin de 1907. Par les études faites à notre École il me semble que la maladie du sommeil elle-même altère quelquefois les nerfs optiques rendant plus facile l'action nocive ultérieure de l'atoxyl. Cet inconvénient ne s'est plus produit dès que je n'ai pas atteint les hautes doses que j'employai premièrement.

Les autres insectes hématophages, comme les moustiques, les stomoxys, les tabanides, etc., et même la transmission directe d'homme à femme par les rapports sexuels, pourront jouer dans la propagation

de la maladie du sommeil un rôle très minime purement accidentel, à eux seuls ils ne pourront certainement pas implanter la maladie endémiquement dans une région; nous en avons la preuve dans nos deux îles de S. Thomé et Principe, lesquelles ont les mêmes conditions climatiques et où l'on rencontre des moustiques, des stomoxys, des tabanidés, etc., mais avec cette exception curieuse qu'il y a des glossines (la palpalis) seulement au Principe et qu'il n'y a pas de glossines à S. Thomé. La maladie s'est implantée au Principe parce que dans cette île elle a trouvé l'agent transmetteur, la glossine palpalis, et ne s'est pas implantée à S. Thomé où cette mouche n'existe pas, nonobstant les cas de trypanosomiase humaine

importés de Angola parmi les noirs (serviçaes) recrutés pour le service agricole des roças. Quand j'ai été à S. Thomé, l'année 1904, j'ai fait l'examen du sang de quelques-uns de ces serviçaes et j'ai trouvé parmi eux un attaqué de maladie du sommeil.

Pour ce qui regarde la trypanosomiase humaine, vu que dans les cas non guérissables les trypanosomes persistent dans le liquide céphalo-rachidien nonobstant les traitements faits, ça suffit pour expliquer la marche de la maladie sans recourir à l'hypothèse d'un état non encore connu de l'évolution naturelle de ces flagellés, ce que, bien sûr, ne veut pas dire que cette phase n'existe pas.

PROFESSEUR F. MESNIL, INSTITUT PASTEUR, PARIS.

En Afrique tropicale, il existe trois groupes de trypanosomes pathogènes:—

- (1°) le groupe *brucei-pecaudi-gambiense-rhodesiense*;
- (2°) le groupe *dimorphon-congolense-pecorum-nanum*,
(?) *simiae*.
- (3°) le groupe *cazalbani* (*vivax* des auteurs anglais)-
uniforme-caprae.

Nous considérerons seulement le premier groupe, spécialement visé dans le questionnaire du Sleeping Sickness Committee. Tout ce qui sera dit pour les trypanosomiasés animales de ce groupe s'applique d'ailleurs à celles des deux autres groupes.

A.—Tout le monde admet l'individualité de *Tr. gambiense*. La question en discussion est celle de *brucei-rhodesiense*. J'ai montré (*Soc. Path. exotique*, décembre 1913) que, sous le nom de *brucei*, on comprend des trypanosomes qui ne sont probablement pas de la même espèce: par exemple, les agents des nagana du Zouloulund et de l'Ouganda.

La méthode biométrique qui, à mon avis, a été un progrès dans l'étude morphologique des trypanosomes, a amené les savants anglais des commissions de la Rhodesia et du Nyassaland à des conclusions, à mon avis, au moins prématurées en ce qui concerne les relations de *rhodesiense* et de *brucei*. Les recherches récentes de Taute et de Fischer ont établi qu'on ne peut pas conclure, comme l'ont fait les savants anglais en question, à l'identité de deux virus parce qu'ils ont mêmes courbes de Galton et même action pathogène sur divers mammifères. Il n'est pas prouvé que le virus trouvé en Rhodesia et au Nyassaland sur le gibier sauvage et les animaux domestiques, bien qu'identique morphologiquement à *Tr. rhodesiense*, appartienne à cette espèce; et les expériences de Taute laissent supposer qu'il y a une différence spécifique. Il est possible, néanmoins, qu'il existe, dans ces régions, chez les animaux à l'état naturel, à la fois un virus du type nagana et le *Tr. rhodesiense*. Enfin, je ne repousse pas à priori la captivante hypothèse de Sir David Bruce et de ses collaborateurs qu'il y a unité spécifique, le virus nagana n'attaquant l'homme qu'accidentellement, pour ainsi dire. En tout cas, ce problème est de première importance en ce qui concerne la question du réservoir de virus pour le *Tr. rhodesiense*, et l'extension de cette trypanosomiase humaine en Afrique orientale et australe.

B.—*Tr. rhodesiense* et *Tr. gambiense*, dans l'état actuel de nos connaissances, diffèrent morphologiquement en ce que la première espèce seule présente des formes avec noyau postérieur. Bien que, malgré des recherches personnelles étendues, je ne sois pas arrivé à trouver des *Tr. gambiense* avec noyau postérieur, je ne suis nullement convaincu qu'il y ait là un véritable caractère différentiel. Je crois à la distinction de ces deux trypanosomes humains en raison des réactions biologiques d'immunité; mais j'estime que ces deux trypanosomes constituent des espèces voisines, plus voisines entre elles que chacune d'elles ne l'est des autres espèces du groupe *brucei*. Peut-être *Tr. rhodesiense* est-il un *gambiense* modifié par suite de son adaptation à *Glossina morsitans*. On sait que *Tr. rhodesiense* peut évoluer aussi chez *Gl. palpalis*, et que *Tr. gambiense* peut évoluer chez *Gl. morsitans*. Il serait intéressant de rechercher si chacun de ces deux trypanosomes est modifié dans sa morphologie et sa virulence par cette évolution chez un hôte anormal.

C.—Pour les autres espèces du groupe *brucei*, on n'est pas encore fixé sur leur nombre. Elles sont probablement multiples: *brucei* type, *pecaudi*, *togolense*, *ugandae*. Peut-être l'évolution chez les tsé-tsés permettra-t-elle de les classer. Ainsi, *pecaudi* a une évolution qui commence à l'intestin et finit dans la trompe, tandis que *gambiense*, *rhodesiense* et probablement les virus nagana de l'Afrique orientale, ont une évolution qui commence aussi dans l'intestin, mais finit dans les glandes salivaires.

Toutes les espèces de ce groupe sont très polymorphes dans la nature; et je crois qu'il est difficile, parfois même impossible, de les distinguer. La confusion probable commise par les savants anglais entre le *rhodesiense* et le nagana en est une preuve. Je crois que ce polymorphisme s'atténue sensiblement par passage continu chez les animaux de laboratoire; les formes courtes et trapues disparaissent de préférence (peut-être *Tr. pecaudi* fait-il exception). Cette constatation s'accorde bien avec la conception de Miss Robertson qui regarde les formes trapues comme surtout propres à évoluer chez les tsé-tsés.

D.—Dans le questionnaire du Sleeping Sickness Committee, figure (28) la question de la variation des espèces de trypanosomes pathogènes. C'est là un problème des plus intéressants tant au point philosophique qu'au point de vue pratique, problème que j'ai personnellement en vue depuis plusieurs années. Jusqu'ici, je n'ai pas observé que les trypanosomes en question dépassent en variation ce qu'on regarde comme limite d'espèces.

E.—On peut dire que la maladie du sommeil ne déborde pas les régions à tsé-tsés. Il n'y a aucun doute que les glossines constituent les éléments de conservation de cette maladie. Koch et Kudicke ont insisté sur l'existence de cas autochtones de maladie du sommeil sur une rive du lac Victoria où manquent les tsé-tsés. Les membres de la mission française au Congo ont insisté sur ce qu'ils ont appelé les épidémies de familles et de cases, difficilement explicables par la seule considération de la tsé-tsé. Ils ont montré d'ailleurs que l'intensité de la maladie ne varie nullement avec l'abondance des glossines. L'exemple de Carnot, sur la Haute-Sangha, où la maladie du sommeil sévissait il y a trois ans avec une morbidité de plus de 25 pour cent, et où les glossines étaient rarissimes, est typique.

Pour les savants de la mission française, des insectes, particulièrement des insectes à mœurs domestiques, seraient à incriminer pour leur rôle de vecteurs mécaniques, et ils ont surtout accusé les moustiques des genres *Mansonia* et *Stegomyia*. Des expériences récentes viennent à l'appui de cette manière de voir.

Les insectes en question ne constituent pas de véritables hôtes. Seuls, les tsé-tsés d'une part, l'homme (et quelques mammifères) de l'autre méritent ce nom. En l'état actuel de nos connaissances, il n'y a pas de raisons de supposer l'existence de quelque autre stade des trypanosomes dans la nature. Toutes les expériences tentées infirment la supposition que l'infection trypanosomique soit héréditaire chez la tsé-tsé.

F.—Pour le *Tr. gambiense*, j'estime que l'homme est, de beaucoup, le principal réservoir de virus. Au Congo, il paraît bien être le seul. Peut-être, n'en est-il pas de même en Afrique occidentale (bassins du Sénégal, du Niger, et affluents); mais la preuve d'autres réservoirs manque encore; il sera bon de les rechercher en suivant les suggestions de mon collègue Bouffard,

En Ouganda, l'expérience des rives abandonnées du lac Victoria et des îles a mis en évidence le rôle des antilopidés, mais la disparition graduelle de l'infectiosité des mouches montre que ce rôle est peu important.

Pour *Tr. rhodesiense*, en vertu des remarques du paragraphe A (v. supra), la question du réservoir de virus reste entière. Il est à désirer que des expériences, analogues à celles déjà citées de Taute, soient entreprises en Rhodesia ou au Nyassaland.

Pour les trypanosomiasés animales, le rôle du gibier comme réservoir de virus me paraît incontestable. Les expériences de Bruce de 1894-96 l'avaient déjà prouvé; les constatations récentes, faites en Afrique australe, le confirment surabondamment. Mais il est probable que les races d'animaux domestiques (bovidés, caprins . . .) assez résistantes, jouent également un rôle comme réservoir de virus.

Il est depuis longtemps reconnu, en Afrique australe, que là où il n'y a plus de gibier, il n'y a plus de tsé-tsés, et, partant, plus de trypanosomiasés animaux. C'est là une conséquence de la civilisation et de l'exploitation méthodique d'une contrée.

Bien que je sois convaincu d'avance du résultat, pour ce qui concerne les trypanosomiasés animaux, je conseille néanmoins d'organiser une expérience de destruction du gibier et je demande qu'elle soit faite en Rhodesia ou au Nyassaland, car elle tranchera en même temps la question du réservoir de virus pour le *Tr. rhodesiense*. Je m'associe pleinement aux desiderata formulés par Sir David Bruce et ses collaborateurs et par Yorke.

EXTRACT FROM REPORT presented by
Professeur MESNIL and Monsieur ROUBAUD to the
15TH INTERNATIONAL CONGRESS OF HYGIENE
AND DEMOGRAPHY at Washington,
September 1912.

Sur la Prophylaxie des Trypanosomiasés.

Plusieurs rapporteurs devant traiter spécialement la question de la prophylaxie de la maladie du sommeil, nous serons brefs sur cette trypanosomiasé et nous envisagerons sa prophylaxie surtout comme un cas particulier de la prophylaxie des trypanosomiasés à glossines.

Maladie du sommeil—Suppression du réservoir de virus.—Un côté du problème de la lutte contre la trypanosomiasé humaine doit être néanmoins envisagé à part, c'est celui qui consiste à empêcher l'homme infecté de servir de réservoir où les insectes vont puiser le virus.

A cet égard, une première mesure consiste à isoler autant que possible les malades du sommeil. Il existe des régions, encore indemnes, où, néanmoins, toutes les conditions, favorables à l'éclosion de la maladie, paraissent réunies (ex. : certaines régions de l'Afrique occidentale où les *Glossina palpalis* abondent). Des mesures doivent être prises pour que des individus atteints de trypanosomiasé ne puissent pas infecter ces régions. Des ententes, par exemple comme celle existant entre les gouvernements de l'Afrique occidentale française et de l'Afrique équatoriale française, seront des plus utiles. Dans les régions déjà contaminées, les hommes malades devront être recherchés avec soin et isolés dans des villages, dits de ségrégation, disposés dans des conditions telles que les malades s'y trouvent bien et demandent à y rester. Ces villages seront naturellement des lieux de traitement, et on n'oubliera pas qu'une bonne nourriture est un adjuvant très important dans l'action des médicaments.

Mais, à notre avis, la mesure prophylactique la plus efficace consiste dans la stérilisation, pour un temps plus ou moins long, du sang circulant des trypanosomés. Les Drs Aubert et Heckenroth, de l'Institut Pasteur de Brazzaville (Congo français), qui ont pratiqué sur une large échelle cette prophylaxie chimique, et qui pensent qu'elle "permettra d'obtenir rapidement de décisifs avantages," sont arrivés aux conclusions suivantes :—

"Parmi les médicaments susceptibles de jouer un rôle important dans la prophylaxie chimique de la trypanosomiasé humaine, il faut citer l'atoxyl et l'arsénophénylglycine. Ce dernier, utilisé à doses convenables de 4 cgr. à 5 cgr. par kilogramme, en une, ou mieux deux injections

intraveineuses à six jours d'intervalle, permet d'obtenir des durées de stérilisation supérieures à 12 mois dans la majorité des cas. Des malades, suivis régulièrement pendant plus d'une année, de 15 à 25 mois, n'ont pas présenté de rechute et sont dans un excellent état de santé. Chez certains sujets, à la première période de la maladie, ce traitement prophylactique ainsi compris semble devoir conduire à la guérison définitive.

"La technique des injections intraveineuses ne doit pas être considérée comme un obstacle à l'utilisation de ce médicament dans les campagnes de prophylaxie.

"L'atoxyl ne possède pas un pouvoir de stérilisation d'une aussi loange durée que l'arsénophénylglycine, toutes choses égales d'ailleurs. Pour obtenir une durée de stérilisation suffisante et donner une efficacité pratique à l'intervention thérapeutique, il faut arriver à trois ou quatre injections successives, ce qui entraîne, dans les campagnes de prophylaxie, quelques difficultés matérielles avec lesquelles il faut compter, mais qui ne sont pas insurmontables.

"Les résultats obtenus dans la vallée de la Haute-Sangha prouvent qu'avec l'atoxyl seul on peut restreindre considérablement la morbidité par trypanosomiasé.

"Dans les régions à maladie du sommeil, il est indispensable que des tournées médicales périodiques soient effectuées par un personnel uniquement chargé de ce service. Ces missions auront pour but : 1°, de repérer les endroits contaminés ; 2°, d'apprécier exactement le nombre des individus atteints, par l'examen systématique de tous les indigènes ; 3°, de faire le traitement prophylactique de tous les malades comme il est indiqué ci-dessus. Le traitement curatif ne sera fait que dans les postes où un médecin se trouve en permanence."

Aubert et Heckenroth ajoutent que les indigènes du Congo reconnaissent déjà les bons effets de ces mesures et viennent en masse se faire traiter.

À côté de l'homme, le gros gibier joue un certain rôle dans la conservation du virus; exemple, les rives désertées du lac Victoria restées infectantes. Toute mesure dirigée contre le gros gibier sera donc utile. Mais, sauf peut-être pour ce qui concerne la trypanosomiasé humaine de Rhodesia et du Nyassaland, il ne faut pas se faire trop d'illusions sur l'efficacité de la destruction du gibier.

L'instruction des indigènes devra se faire peu à peu, mais on ne peut compter sur des résultats immédiats.

L'administration devra étudier le remplacement des industries prédisposantes (pêche; cueillette du charbon de bois; cultures au bord des eaux) dans les zones contaminées, par d'autres qui mettront les indigènes plus à l'abri de l'infection possible. L'extension de la vente du poisson sec, des sécheries sahariennes, pourrait aider en Afrique occidentale à éloigner des rivières des populations ne vivant que de pêche.

D'une façon générale, l'administration devra exercer son action sur les villages dans un sens utile à la prophylaxie. L'étude des moyens propres à diminuer les chances de contagion dans les différentes régions devra être faite soigneusement par elle, d'après les conseils techniques de personnes informées, spécialement affectées à ce service.

Lutte contre les glossines.—En dehors de la maladie du sommeil, un certain nombre de trypanosomiasés animales de l'Afrique intertropicale sont conservés à l'état endémique ou enzootique par les glossines ou tsé-tsés. Au point de vue des conditions de vie, une distinction doit être faite entre les glossines, nettement hygrophiles, qui ne se rencontrent jamais loin des rives ombragées des cours d'eau (la *Gl. palpalis* est le type de ces tsé-tsés) et les glossines xérophiles, telles que la *Gl. morsitans*—qui vit dans la brousse et même la savane et s'éloigne des rivières à la poursuite de sa proie. Des espèces, comme *Gl. longipalpis*—présentent des conditions de vie intermédiaires.

Les moyens à employer pour supprimer les glossines, ou mettre l'homme et les animaux à l'abri de leurs piqûres, diffèrent donc suivant l'espèce qu'il s'agit d'atteindre.

Contre la *Glossina palpalis*, l'agent principal de propagation de la maladie du sommeil, et de plusieurs trypanosomiasés animales, comme la souma, il faut déboiser, ou au moins éclaircir les galeries forestières dans les endroits fréquentés par les hommes (points d'eau, gués, ponts, etc.), ainsi qu'aux points de passage des caravanes et des troupeaux. Il faut abattre les arbres le plus complètement possible, couper les palmiers nains touffus qui, malgré leur faible hauteur, forment un couvert impénétrable où s'abritent les glossines; détruire les lianes; en un mot, faire de la forêt une clairière.

Pour la protection des villages, il faut comprendre un déboisement absolu sur une étendue variable autour des villages ou des lieux fréquentés, avec déboisement partiel (éclaircissement) sur une zone périphérique, la plus étendue possible; l'incendie des herbes ou de la brousse n'est pas utile. Si une protection efficace ne peut être réalisée, le déplacement des villages dans des zones sans tsé-tsés est indiqué.

Contre la *Glossina longipalpis*, qui propage, entre autres, la baléri, il faut abattre les broussailles vertes et les petits arbres touffus qui bordent les derrières des cordons forestiers des cours d'eau.

Contre la *Glossina morsitans*, qui paraît apte à propager la plupart des trypanosomiasés des zones où elle habite, et, en Rhodesia, la trypanosomiasé humaine, il faut élaguer la forêt claire, abattre les buissons épineux (joubiers, mimosées) où la mouche gîte souvent. L'incendie de la brousse, à la saison favorable, peut être utile.

Les mesures d'action contre cette glossine, dont les gîtes sont mal définis, sont surtout difficiles. Sa présence dans la brousse claire ou la forêt plus ou moins couverte est surtout fonction de celle du gibier. Elle le suit et peut se trouver abondante un jour, là où on ne la rencontre plus peu après. La destruction

du gibier est, dans ce cas, une mesure fondamentale. Il n'y aura d'élevage intensif possible dans les zones à tsé-tsés qu'avec la disparition du gibier. La question des réserves de chasse et de la protection du gros gibier, actuellement à l'étude, est absolument contraire au but prophylactique qu'il y a lieu d'atteindre, et par suite à l'intérêt économique des colonies.

Cette destruction du gibier doit être préconisée partout où il y a des trypanosomiasés animales transmises par les tsé-tsés, quelle que soit l'espèce de glossine en cause.

Dans les vastes territoires de l'Afrique tropicale, les régions d'élevage se trouvent surtout à l'intérieur, loin des côtes. Le moyen le plus économique d'amener les animaux (en particulier les bovidés) dans les colonies côtières ou dans les ports d'embarquement, est de les faire voyager en troupeaux. Pour que ces troupeaux ne se contaminent pas en route, comme c'est généralement le cas à l'heure actuelle, des routes sanitaires doivent être créées. Le tracé de ces routes doit être étudié avec soin par des personnes compétentes; les points de traversée des cours d'eau doivent donner lieu à une attention spéciale et, s'il y a lieu, être débarrassés des glossines en employant les procédés décrits précédemment.

À côté de ces mesures générales, des mesures individuelles peuvent être prises. L'homme protégera autant que possible, par des gants ou des voiles, les parties découvertes de son corps. Des pièges à tsé-tsés seront mis en œuvre. On cherchera à écarter ces insectes par des fumées. On évitera, si possible, de voyager le jour. Mais toutes ces mesures ne seront jamais que de faibles palliatifs. Ce n'est qu'en recourant aux mesures générales sur lesquelles nous avons insisté qu'on arrivera peu à peu, à force de persévérance, à faire disparaître les trypanosomiasés à tsé-tsés.

MONSIEUR E. ROUBAUD, CHEF DE LABORATOIRE À L'INSTITUT PASTEUR, PARIS.

Depuis bientôt huit ans, j'ai eu l'occasion d'étudier la maladie du sommeil, les trypanosomiasés animales et les glossines au Congo français, et dans toutes les possessions françaises de l'Afrique Occidentale. Les résultats de mes recherches, qui ont principalement porté sur les conditions de transmission des trypanosomes et sur leur mode d'évolution chez l'insecte propagateur, ont été publiés, soit en détail dans le rapport de la Mission française d'Etude de la Maladie du Sommeil*, soit en notes préliminaires diverses prises également aux "Comptes rendus de l'Académie des Sciences" et au "Bulletin de la Société de Pathologie Exotique de France." En priant MM. les membres du Comité de bien vouloir se reporter à ces publications, je me bornerai dans cette note à souligner quelques-unes des questions auxquelles j'attache une importance spéciale parmi toutes celles qui figurent dans la liste publiée.

1°. La spécificité de transmission du *Tr. gambiense* par la *Gl. palpalis* me paraît principalement liée au mode de vie particulier de cette mouche, qui, beaucoup plus que les autres espèces de glossines, tend à vivre au voisinage de l'homme. Dans les régions les plus infectées de maladie du sommeil que j'ai visitées au Congo, le *Gl. palpalis* ne trouve à se nourrir que de sang humain, indépendamment de celui des animaux ou des reptiles; le gros gibier y est le plus souvent extrêmement rare. La mouche s'adapte à l'homme et vit au voisinage des villages. D'un autre côté il me paraît certain que si, en principe, toute espèce de mouche tsé-tsé peut se montrer capable de subvenir à l'évolution et à la transmission de toutes les espèces de trypanosomes sévissant dans les régions à tsé-tsés, en fait, dans la nature, il n'en est pas ainsi. Il existe, suivant les régions, des races particulières de glossines, races qui sont fonction du climat et des conditions géographiques et qui se montrent plus ou moins aptes à transmettre les différents virus. Une même espèce

de glossines pourra, dans une localité déterminée, se montrer très sensible à l'infection pour un virus donné, alors que dans une autre région elle s'y montrera réfractaire ou beaucoup moins sensible. Pour cette raison je ne considère pas la notion de l'espèce de la glossine comme liée d'une façon fondamentale à la transmission d'un virus donné. C'est surtout le genre de vie des glossines dont il faut tenir compte pour apprécier leurs possibilités plus ou moins grandes de transmission naturelle. A cet égard il y aura lieu de rechercher si, dans les régions de l'Afrique Orientale où existe le *Tr. rhodesiense* transmis par *Gl. morsitans*, cette mouche ne vit pas au voisinage de l'homme d'une façon plus immédiate qu'elle ne le fait, d'ordinaire, dans les régions inhabitées qu'elle fréquente habituellement.

2°. La distinction spécifique exacte des virus divers qui peuvent être rencontrés chez les animaux considérés comme réservoirs de virus pour les trypanosomes humains est d'une importance considérable. A cet égard j'estime que l'un des critères de différenciation les plus nets que l'on puisse posséder actuellement, consiste en l'étude de leur mode d'évolution chez les glossines. L'évolution dans les glandes salivaires proprement dites n'a jusqu'à présent été observée que pour les deux trypanosomes parasites de l'homme, le *Tr. gambiense* et le *Tr. rhodesiense*. Toutes les fois qu'un virus dimorphe, rencontré chez un animal, est reconnu accomplir son évolution chez les glossines en dehors des glandes salivaires, ce virus n'a certainement rien de commun, dans l'état actuel de nos connaissances, avec le virus humain. Le *Tr. peccaudi* de l'Afrique Occidentale, virus morphologiquement voisin du *Tr. gambiense* et aussi du *Tr. brucei*, accomplit son évolution dernière dans la trompe des glossines comme nous l'avons montré avec le Dr. Bouet, et non dans les glandes salivaires, ce qui le distingue radicalement des virus humains. Il y aura lieu de rechercher d'une façon systématique le mode d'évolution chez la mouche des deux agents du nagana (*Tr. brucei*), celui du Zouloulou et celui de l'Ouganda, dont notre collègue

* G. Martin, Lebeuf et Roubaud. La Maladie du Sommeil au Congo français. Paris: Masson, 1909.

M. Mesnil vient de montrer récemment la non-identité. Ces virus accomplissent-ils leur évolution dernière dans la trompe des mouches, ou dans les glandes salivaires ? Nous croyons fermement que la solution de cette question apportera des éclaircissements utiles à la question si importante soulevée par le général Sir D. Bruce et qui a trait à l'identité des *Tr. brucei* et *Tr. rhodesiense*, et nous engageons vivement les chercheurs à porter leur attention de ce côté.

3°. Si les glossines sont bien les agents fondamentaux de la conservation des virus dans la nature, j'estime que dans certains cas d'autres insectes sont aptes à jouer le rôle d'auxiliaires, pour la transmission mécanique à forme épidémique des affections à trypanosomes. Les stomoxes, particulièrement, peuvent intervenir comme notre collègue Bouffard l'a montré, dans la transmission du virus de la souma (*Tr. casaboui* ou *vieux*). Les moustiques quand ils sont très abondants aussi, dans les lieux habités par l'homme, doivent également être comptés parmi les vecteurs auxiliaires des trypanosomiasés humaines. Ils agissent en diffusant la maladie dans une famille, par exemple, et sont vraisemblablement les principaux agents actifs des épidémies familiales dont nous avons observé de très nombreux exemples en Afrique équatoriale et occidentale. Les expériences effectuées à l'Institut Pasteur de Brazzaville, celles plus récentes que nous avons réalisées à Dakar avec le Dr. Lafont démontrent le bien fondé de cette manière de voir. Dans la nature, le rôle vecteur des moustiques est limité par la faible abondance habituelle des trypanosomes dans le sang; quelquefois cependant, comme nous l'avons observé avec MM. G. Martin et Leboeuf, et avec le Dr. G. Bonet, il est loin d'en être ainsi; les conditions favorables se trouvent alors réalisées pour permettre aux moustiques de diffuser la maladie dans l'entourage immédiat des malades.

4°. Je considère que des cas de guérison naturelle pour l'homme atteint de trypanosomie humaine (*Tr. gambiense*) sont possibles. Dans des régions du Congo particulièrement infectées, j'ai eu l'occasion de voir dans les villages des individus âgés, des vieillards, ayant toujours vécu sur les lieux mêmes où sévit la maladie qui décime la population depuis de longues années. Quelques-uns m'ont certifié avoir guéri de la maladie du sommeil. Le fait est rendu probant par les observations récentes des Docteurs Ringenbach et Heckenroth, qui ont retrouvé en état de santé satisfaisant plusieurs indigènes reconnus trypanosomés en 1907 par la Mission française au Congo, et qui n'avaient subi aucune sorte de traitement. Je suis porté à croire qu'il existe dans la nature des cas peut-être assez nombreux d'immunité acquise, parmi les populations qui vivent depuis fort longtemps dans les zones à trypanosomie humaine.

5°. La question du rôle joué par les animaux sauvages dans la conservation naturelle des virus, aussi bien ceux de l'homme que ceux des animaux domestiques, est certainement d'importance capitale. La belle série de recherches effectuées à ce sujet dans l'Ouganda par Sir David Bruce et ses collaborateurs me paraît entraîner la conviction, en ce qui concerne le rôle de réservoir de virus pour le *Tr. gambiense*, joué par les antilopidés. Les expériences de M. Duke en particulier échappent à la critique. Toutefois il me semble que si ce rôle, en ce qui concerne la trypanosomie humaine à *Tr. gambiense*, transmise par *Gl. palpalis*, est possible, en fait il se limite notablement et n'offre pas un caractère d'absolue généralité. Dans beaucoup de régions du Congo français dévastées par la maladie du sommeil, le gibier fait défaut ou se montre extrêmement rare. Dans ces régions le principal réservoir de virus me paraît se limiter à l'homme lui-même.

Il n'en est peut-être plus ainsi en ce qui concerne la trypanosomie humaine à *Trypanosoma rhodesiense*, transmise par *Gl. morsitans*. Ce que je connais des habitudes de cette glossine me paraît légitimer les conceptions de MM. Kinghorn et Yorke touchant le rôle joué par les animaux sauvages dans la conservation de ce virus. *Gl. morsitans* étant par excellence une mouche de gros gibier, il est infiniment probable que c'est au contact de ce dernier qu'elle s'infecte. Si les chiffres d'infection naturelle des animaux sauvages au

Tr. rhodesiense donnés par les auteurs ci-dessus sont sans doute au-dessus de la vérité (la distinction absolue de leur virus sauvage n'ayant pas toujours été faite avec des virus du type *Tr. pecaudi*), je considère cependant leurs conclusions comme justifiées. Il me paraît de toute importance que des expériences soient instituées à bref délai dans le but d'apprécier les effets de l'éloignement ou de la destruction du gros gibier. Ces expériences devront être faites, autant que possible, dans des territoires circonscrits, où la faune sauvage se trouve en quelque sorte isolée, sans communication facile avec des territoires giboyeux limitrophes. La question qui offre un intérêt tout à fait spécial pour le *Tr. rhodesiense* mérite également d'être étudiée pour les autres trypanosomiasés. Il est hors de doute que les faunes sauvages sont, dans les pays à tsé-tsé, le principal élément de conservation des virus, surtout dans les territoires envahis par la *Gl. morsitans* et les espèces voisines. La conservation du gros gibier est incompatible avec la pénétration et la mise en valeur économique des colonies africaines.

L'éloignement du gros gibier constitue d'ailleurs un moyen prophylactique à deux faces: en même temps qu'il supprimera la source la plus constante d'infection pour les mouches, il entraînera aussi fatalement une diminution considérable dans l'abondance des glossines. Pour certaines espèces même, comme la *Gl. morsitans*, nous ne connaissons pas d'autre moyen de destruction pratique. A ce double point de vue cette méthode s'impose comme mesure d'action fondamentale contre les trypanosomiasés et nous ne pouvons que souhaiter voir réaliser prochainement dans les pays intéressés des expériences décisives qui susciteront la mise en œuvre de mesures plus étendues. La Rhodésie nous paraît être une des régions où de telles expériences pourraient être tentées avec le plus d'intérêt, en raison de la double portée prophylactique pour l'homme et les animaux domestiques qu'elles y présenteraient. En Afrique Occidentale française, nous serions également désireux de voir le Gouvernement français procéder, au Sénégal par exemple où ce serait facile, à des expériences de même ordre. La région dite de la Petite Côte, aux confins de Nianing, est infectée de maladie du sommeil et de trypanosomiasés animales diverses. Il y serait facile, étant donné le caractère très circonscrit (une trentaine de kilomètres carrés) de la zone envahie par le gros gibier réservoir du virus, d'y procéder à une expérience démonstrative qui présenterait une portée pratique immédiate. On trouverait également dans la région dite du Niom-Bato, située entre l'embouchure de la Gambie anglaise et l'estuaire du Saloum, un territoire limité, infesté de *Gl. morsitans* et de trypanosomiasés animales diverses, sur lequel on pourrait avec fruit observer les résultats pratiques de la destruction du gibier.

De telles expériences demandent plusieurs années avant de pouvoir donner les résultats espérés. Elles devront être poursuivies avec patience et méthode. Je n'insisterai pas davantage sur les autres questions intéressantes: la prophylaxie des trypanosomiasés et le mode de destruction des glossines. Mes conceptions ont été indiquées dans les travaux divers que j'ai indiqués plus haut et dans ceux qui sont mentionnés ci-dessous.* Je prie MM. les membres du Comité de bien vouloir s'y reporter.

* "Expériences diverses de transmission des Trypanosomes par les Glossines." G. Bouet et E. Roubaud. (Extraits du Bulletin de la Société de Pathologie Exotique. Tome III, No. 9; Tome IV, No. 8; Tome V, No. 3.)

"Expériences de transmission des trypanosomiasés animales de l'Afrique occidentale française, par les stomoxes." G. Bouet et E. Roubaud. (Extrait du Bulletin de la Société de Pathologie Exotique. Tome V, No. 7.)

"Sur la prophylaxie des trypanosomiasés." F. Mesnil et E. Roubaud. (Extrait des Comptes-rendus du XV^e Congrès International d'Hygiène et de Démographie, Washington, Septembre 23-28, 1912.)

"Expériences diverses de transmission des Trypanosomes par les Glossines." G. Bouet et E. Roubaud. (Extrait des Annales de l'Institut Pasteur. Tome XXIV, Août 1910.)

"Entomologie: Nouvelle contribution à l'étude biologique des Glossines. Quelques données sur la biologie des *Gl. morsitans* et *tachinoides* du Soudan nigérien." Note de M. E. Roubaud.

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The Relation between Sleeping Sickness, Big Game, and Domestic Animals.

(Translation.)

1. The first to point to the importance of wild game for the spread of trypanosome diseases was Bruce. A short time after the discovery of the trypanosome named after him he found, in Zululand in the nineties, that wild game may be infected with similar parasites without showing symptoms of disease, and he then expressed the opinion that this phenomenon would perhaps prove of importance in epidemiology.

2. Curiously enough these findings of Bruce received comparatively little attention and were soon almost forgotten.

3. The discovery that sleeping sickness was a trypanosome infection turned attention again to the animals from which the *Glossina* obtain their nourishment. In this respect Robert Koch pointed to the crocodile.

4. Later Kleine found that it was possible to transmit *T. gambiense* to cattle, sheep, and goats if one put infected *Glossina palpalis* to feed on them. In his opinion this experimentally-produced occurrence played hardly any part in natural conditions.

5. Since then much active work has been done on this question. In the course of recent investigations, especially by Englishmen, the opinion has gained favour that great significance is to be attached to mammals, and especially wild game, as reservoirs of the trypanosomes pathogenic to man. This question was specially important to the British authorities, since, on the Victoria Lake, they had done little against the disease-bearing *Glossina* by the clearing of vegetation, but had sought to gain the same object by moving the infected population into fly-free regions and thus by a sort of sterilisation of the flies. If one now assumes that the game which remained behind in the evacuated districts and there increased, serves in a large degree as a reservoir for *T. gambiense*, there is no doubt that if the old fly areas are later resettled sleeping sickness must again occur amongst man.

6. In fact, Bruce, and later, Duke found, in investigations of the islands in the northern part of the Victoria Lake, which had been completely cleared of the infected population for some years, that where antelope lived in large numbers the flies might be still infected with trypanosomes. On the other hand, Duke showed that on those islands where no antelope, but only hippo, reptiles, and birds lived, the flies were, after some years, free from trypanosomes. The species of antelope concerned (*Tragelaphus spekei*) undoubtedly forms, therefore, on the islands of the Victoria Nyanza a trypanosome reservoir.

7. How far this applies to *T. gambiense* in particular is certainly difficult to decide, and one must be very cautious with one's conclusions.

8. Let us put shortly together what has been established recently by scientific observations and experiments on this question.

9. First of all, as concerns *T. gambiense*, it is possible, if not always, at all events in a number of cases:—

- (1) To transmit this parasite both by direct blood inoculation and by the bite of infected flies to wild game and domestic animals.
- (2) These trypanosomes thus experimentally transmitted may, under certain conditions, maintain themselves for a longish time in a body of antelopes and domestic animals. The wild game remains apparently quite unharmed by the infection; the same holds good, as a rule, for domestic animals.
- (3) The parasite can, in a number of cases, be again further transmitted by means of *Glossina* from antelope and goats which had been infected with *T. gambiense*; in an experiment described by Duke this was possible even 22 months after the (original) infection; in others reported by Kleine and Fischer the interval was considerably shorter. In any case the infectivity diminishes with the length of time which has elapsed since the appearance of the trypanosomes in the blood.

10. The conditions are quite similar with *T. rhodesiense*, only that the considerably greater virulence of this parasite is apparent in all species of animals. The wild game appears in general, in experimental inoculations with *T. rhodesiense*, to show likewise no symptoms of illness. All domestic animals, however, as cattle, sheep, goats, donkeys, dogs, sicken promptly after inoculation with this parasite, and as a rule quickly succumb. They are, therefore, certainly not parasite carriers for long periods.

11. Scientifically considered, the possibility that the wild game and, in a restricted sense, the domestic animals also, can serve as a reservoir for the parasite of sleeping sickness is by no means to be denied.

12. However, in these experimental observations differences in degrees are apparent. The German investigators established the possibility of mammals being for some time carriers of trypanosomes pathogenic to man; they found, however, at the same time that this occurrence, especially as regards the duration of the infectivity, is comparatively restricted. On the other hand, some English investigators, especially Bruce, believed this occurrence to be not only possible, but almost the rule; as a consequence of their investigations they assumed that the wild game served as the reservoir of choice for the parasite of sleeping sickness, from which the trypanosomes could be transmitted after a long time and with great ease by means of *Glossina*.

13. Thus the position stood when, in consequence of the occurrence of human trypanosomiasis in Rhodesia and Nyasaland, fresh investigations were undertaken in these regions. Kinghorn and Yorke, and Bruce also, believed that they had found there a clear and incontrovertible proof that the game, not only in experiment but also in nature, played quite an extraordinary rôle as carrier of the trypanosomiasis of man. It has been formerly objected to the alleged finds of *T. gambiense* in naturally infected game and domestic animals that it was not really a question of this parasite, but of one only morphologically resembling it, and not a trypanosome pathogenic to man. However, the position in the case of the parasite of sleeping sickness in Rhodesia and Nyasaland (*T. rhodesiense*) was apparently much simpler. To this parasite had been ascribed, as is well known, morphological peculiarities by which it was to be at once distinguished from all other trypanosomes; in fact, till a short time ago, displacement of the nucleus towards the hinder end of the parasite, the so-called posterior nuclear forms, which curiously enough had never been observed before, was regarded as absolutely characteristic of *T. rhodesiense*.

14. Kinghorn and Yorke in their field of work now showed that among the wild game the very considerable figure of 16 per cent. was infected with trypanosomes, which, when inoculated into experimental animals showed typical posterior nuclear forms and behaved in their virulence towards animals exactly like *T. rhodesiense*. It was, according to their view, proved that in that district 16 per cent. of the game were carriers of human sleeping sickness. This finding quickly became known and gained so much the more significance in the outer world in that it corresponded with the view already expressed by Bruce, and thus it is to be explained that about this time, not only in scientific circles, but also among the European population threatened by sleeping sickness, annihilation of all wild game was continually pressed forward in the interest of sleeping sickness prevention. German authorities concerned with the suppression of sleeping sickness were sceptical of the conclusions of Kinghorn and Yorke, and also on the English side there were individual objectors, especially Bevan.

15. Since in a tour of investigation I had the opportunity of working under similar conditions to those of Kinghorn and Yorke, I decided, having regard to the great practical importance of the question, to repeat the experiments of these two workers in

Portuguese Nyasa. The results obtained were published some time ago in the *Arbeiten aus der Kaiserliche Gesundheitsamte*. I will therefore go into them only shortly.

16. In Portuguese Nyasaland, according to my observations, 16.2 per cent. of the wild game was found infected with trypanosomes which, in their appearance and their pathogenicity for animals behaved absolutely like *T. rhodesiense*; examination of the few domestic animals of the natives gave very similar results. Up to this point the results therefore completely correspond with those of Kinghorn and Yorke. Now it must be noted that in the field of work of the English investigators extremely few, relatively, sleeping sickness cases were found amongst the population, and in my district no cases at all could be detected amongst men. It follows that 16 per cent. of the game and a scarcely smaller percentage of the domestic animals are infected with the parasite of sleeping sickness in regions in which man remains healthy. This is a surprising discrepancy, because in the large fly areas men cannot escape the bite of infected *Glossina* any more than the wild game and the domestic animals can. Hence the conclusion, certainly not at that time accepted by English investigators, that the *rhodesiense*-like trypanosomes found in wild game, in spite of the far reaching correspondence in other respects, were not identical with the parasite of human sleeping sickness.

17. In order to obtain indisputable experimental proof for this conclusion, the pathogenicity of the trypanosomes in wild game and in domestic animals was tested in several experiments on man and it was found, as was to be expected, that this trypanosome could not be conveyed to man either by the bite of *Glossina* or by direct blood inoculation. The trypanosome was, as a matter of fact, *T. brucei* and not *T. rhodesiense*.

18. Consequently it was shown that trypanosomes in naturally infected wild game and domestic animals can be regarded with certainty as *T. rhodesiense* only if they prove to be pathogenic for man. Secondly, it was established that wild game and the domestic animals do not take part in the spread of sleeping sickness, at least, not to the extent assumed by Kinghorn and Yorke, and that therefore indisputable scientific grounds were lacking to support immediate eradication of all wild game.

19. So for the time being and until the obtaining of scientific proofs, the proposal to shoot off the African wild game in Rhodesia and Nyasaland would have come to nothing if the whole question had not been re-opened recently by an alteration of the standpoint of Bruce. Bruce, who at first strictly separated *T. rhodesiense* from *T. brucei*, to-day considers it very probable that these two parasites are identical and that, therefore, sleeping sickness in Rhodesia, Nyasaland, and in the south of German East Africa, is nothing else than an infection of man with the animal trypanosome of nagana, *T. brucei*, which is spread over the whole of Africa. Since one can frequently find this mammalian trypanosome there in antelopes, according to the evidence, Bruce is of opinion, in spite of all counter-evidence, that the wild game plays quite an enormous part as reservoir of human trypanosomiasis.

20. For the fact that *T. brucei* in spite of the frequency with which it is to be found in mammals and tsetse-flies, attacks such an infinitesimally small number of men, Bruce is not able to adduce any reasons that will hold ground.

21. Sleeping sickness in Rhodesia and Nyasaland, as many years' experience has taught, attacks strong people and weaklings, children and adults; moreover, no support is to be found in other directions for the idea that the one or the other is more or less protected against the disease. Whoever is once attacked by it succumbs, and indeed pretty quickly. Intermediate stages, e.g., persons who are infected with trypanosomiasis without showing any noteworthy phenomena of illness, are never found.

22. It is very important moreover that the same *T. brucei* which is alleged in Rhodesia and Nyasaland to cause sleeping sickness, in Zululand, where it has been long known, has never made a single man ill. According to a personal communication which was made to me in Theiler's Institute, this has been

confirmed by recent investigations also. Examples of the kind can be brought from many other parts of Africa.

23. Bruce, therefore, in his opinion that sleeping sickness in Rhodesia and Nyasaland is nothing but an animal trypanosomiasis caused by *T. brucei*, which attacks on occasion specially predisposed man also, stands almost alone. Already by Laveran and others counter-evidence has been brought, but with the authority that justly attaches to Bruce his recently maintained views must of course be earnestly weighed.

24. Bruce himself has formerly always taken the standpoint that in order to work in a practical manner, in distinguishing the trypanosomes we must do as we are compelled to do in the case of the bacteria where morphology does not suffice, i.e., make use of the biology of the parasites, however much it may violate the canons of the strict systematist. And when such a practical test is used it is found that *T. brucei* can be exactly distinguished from *T. rhodesiense* in that it is pathogenic only for the various mammals, whereas *T. rhodesiense* is pathogenic for man as well as for these.

25. Recently another further small complication has occurred, since, according to Stephens and Blacklock, *T. brucei* must on account of its dimorphic type be called something else. Fortunately this is only a question of an alteration of name.

26. How now does the rôle of wild game and domestic animals in the spread of sleeping sickness shape in nature? Of course there is no possible doubt that mammals and even cold-blooded animals supply blood for the tsetse-flies and Koch, as is well known, has drawn attention to the crocodile as supplying food for *G. palpalis*. As concerns especially this assumption of Koch, there are indeed places on and in the Victoria Nyanza where this occurs and where the flies are accustomed to the blood of cold-blooded animals. Were one, however, to try the experiment, so advisable on other grounds, of destruction of the crocodiles one would not starve the tsetse-flies but only attain the result, that the flies would maintain themselves so much the more on man and warm-blooded animals. For observations made later in East Africa have shown that *G. palpalis*, once it has got the taste for mammalian blood, prefers this to any other. In a precisely similar manner, after the shooting off of all antelopes and the cessation of stock-keeping in populated regions, man would become the chief blood supplier for the flies. This is the case not only for *G. palpalis* but also for *morsitans*, which is not a fly peculiar to stock as has often been erroneously believed, but likewise vigorously attacks man. We cannot therefore cause the tsetse-flies to die out by shooting off the wild game.

27. Much more important is the fact that the wild game in nature not only supplies blood, but also, speaking quite generally, is a trypanosome carrier. To what extent this is especially the case with the trypanosomes pathogenic to man and whether it plays a really considerable part in the spread of sleeping sickness is, as stated, still very questionable.

28. As regards the position of things in German East Africa under natural conditions in *palpalis* regions, I should like to bring forward some practical observations which were there made in connection with the combat with sleeping sickness; they concern chiefly the domestic animals, likewise incriminated as sleeping sickness reservoirs.

29. The population on the Mori river, near Kirugu in the Victoria Nyanza district, was severely attacked by sleeping sickness in the year 1908. The blood of the numerous stock animals, both large and small, which regularly drank at the customary watering places and were there constantly bitten by *Glossina* in just the same way as man, was however free from trypanosomes. That the domestic animals there played a noteworthy rôle as reservoirs of human sleeping sickness, in spite of the favourable conditions existing, is therefore excluded.

30. The second observation concerned Tanganyika. In the year 1909 information was received from its north shore that in the sleeping sickness and *palpalis* regions the stock also was frequently infected, and indeed with trypanosomes which perhaps corresponded

with the trypanosomes of man. By experiments carried out by Kleine, Fischer, and Fehlandt it was shown that in addition to other peculiarities the suspected parasites could not be transmitted at all to monkeys and that therefore they could be distinguished with absolute certainty from *T. gambiense*.

31. In the past year Kleine and Eckard have again carried out investigations on Tanganyika in this direction. They found in a severely infected district, where 25-80 per cent. of the human population suffered from sleeping sickness, only 3.5 per cent. of the domestic animals infected with a trypanosome which after careful investigation they regarded as *T. gambiense*. They established in the same region that, amongst 23 bush-buck, one hippo, and four wild pigs, no single animal was infected with sleeping sickness trypanosomes.

32. Therefore, on Tanganyika also only a slight significance can be attributed to domestic animals, and to wild game probably even less, as carriers of the sleeping sickness parasite, compared with the high percentage of infection of man.

33. Assuming one places oneself on the standpoint recently taken by many people that a common war of extermination against wild game should be as rapidly as possible begun from all sides in the interest of sleeping sickness prevention, the question arises whether it could really be done so easily at all. I am of opinion that a real extermination of all wild game for instance, in Portuguese Nyasaland, where my last observations were made, would be hardly at all feasible. In the vast wild game and *morsitans* regions great difficulties of all kinds would be quite unavoidable; in addition a common plan of campaign would be necessary not only on the borders of districts but also on those of the different colonies and, as regards the last point, it is, as a rule, a question of good will.

34. On the other hand, individual cases can be thought of in which a destruction of game could be carried out and might perhaps attain the desired object. I have in mind *self-contained* wild game forest, and *palpalis* regions which are not too extensive, where the wild game is numerous and has actually been demonstrated to be a trypanosome carrier. If one shifted the population for some years out of such a *palpalis* region, infected with sleeping sickness, with the object of later on resettling it, the advantages of an experiment of making a clean sweep and removing from the district all the material for infection by the extermination of the wild game would be undeniable.

35. To turn now to the significance attaching to the wild game in nature as carriers of various kinds of trypanosomes of domestic stock—*T. brucei*, *pecorum*, *vinax*, &c.—this is greater or less, according to the frequency of the game, the character of the region, the laying out of the settlements, and other external conditions. In any case there is no doubt that the percentage of antelope so infected in individual regions which are specially suitable may be somewhat high. And therefore it was obvious that the complete extermination of game would be furthered from various sides in the interest of stock, even when such a measure would be unnecessary for the protection of man. On these grounds this question must here be at least shortly touched on, and I would like to remark that, according to my view, a radical procedure against the wild game cannot be recommended, quite apart from the already discussed doubts as to the practicability of such general measures.

36. For where rational stock raising is carried on, there must be large expanses of pasture, and where such expanses arise, the tsetse give way and with them also the trypanosomes. An example of this is furnished by the Tabora district. Round about Tabora is a large district of cultivation and pasture land with a flourishing stock-raising industry, which is only seldom disturbed by the introduction of nagana, and it is not till one comes to the outer edge of the circle that one finds a vast forest and tsetse zone. The systematic clearing of large expanses, which is to be carefully distinguished from the customary attacks on the forest by the negro, carried out sometimes here, sometimes there, and then again neglected,

entails therefore of itself sanitation in respect of the flies. Simultaneously, as a rule, the wild game also draws back, in most regions of South Africa, moreover rather more than the inhabitants like. We can, therefore, free the stock establishments from disease best by as speedily as possible advancing the cultivation of our colonies; the results of all other measures are very doubtful.

37. If one surveys the experimental results hitherto obtained and the scientific considerations on the rôle of the wild game as trypanosome reservoirs, one asks oneself how it happens that a complete plan for the thorough extermination of a whole animal world has so quickly arisen. One must then consider the special conditions with which sleeping sickness prevention has to reckon in the *morsitans* regions of Nyasaland and Northern Rhodesia. Drugs are, according to the experience of the English physicians, as good as useless. Everyone familiar with the subject knows that, as a rule, one can only proceed against *morsitans* by the slow method of cultivation of land. The removal of a population from infected into fly-free districts is certainly possible if it is a question of individual villages, but is quite excluded when it is a question of big districts. Nevertheless, people wish to bring help as quickly as possible, and it can therefore be understood that they have fastened on the object which seemed at least in some degree assailable, viz., the game as the supposed reservoir of sleeping sickness.

38. However, even the most zealous advocates of the necessity of game extermination no longer press the matter quite so far. From England a large experiment, to be carried out in nature, has now been proposed, to obtain clear proofs of the dangerous part played by the antelopes. The proposed experiment is estimated to cost over 1,000,000 marks; however, I am informed that the British authorities will probably provide the necessary means. The plan is shortly as follows: A well-populated sleeping sickness district in Nyasaland, very rich in game, and in which there are many tsetse-flies, is to be enclosed by a high wire fence to an extent of about 10 English miles, that is to say 16 kilometers in the square, for a period of several years. In the experimental region an exact census of the population and of the domestic animals will now be taken, and the number of men and cattle infected with trypanosomes ascertained. At the same time the percentage of tsetse-flies infected with trypanosomes will be found out by various experiments.

39. Then the wild game will be shot off completely and the opportunity taken to ascertain the number of antelope, buffalo, wild pig, &c., infected with trypanosomes.

40. Exact figures of the population and the domestic animals having been obtained in the interval, one will after a series of years be in a position to obtain a precise picture of the result of the extermination of the game. Further, one will find out whether, and if so, how many of the domestic animals are infected with trypanosomes. One will, moreover, see if the tsetse-flies have disappeared and, if they are still there, whether they are still infected with trypanosomes.

41. I have marched four times right through the district proposed for this gigantic experiment, between Domira Bay and Kasu Hill, and consider it very suitable. The Governor of Nyasaland told me that from the standpoint of the authorities also the experiment was practicable. Since sources of error may easily creep in, the work must, of course, be done with all possible precautions. In any case, one is filled with admiration at the large scale and the spirit of enterprise with which this experiment is planned.

42. Bruce, Kinghorn, and Yorke, also hold this experiment to be in any case necessary; indeed, the stimulus to it comes from them. This would certainly not be the case if the enormous dangerousness of the game, on which stress was laid by the authors named as a result of their early experiments, were already uncontestedly proved. On the other hand, it has been shown that Kinghorn and Yorke in this direction have drawn decidedly too far-reaching conclusions.

43. This is the present position of affairs, and to this Fleming also, the Medical Director of Southern

Rhodesia, has recently agreed, at the same time expressing himself on all these questions with the greatest reserve.

44. The correct standpoint for our colonies must be to abstain from a scheme of game extermination made on a large scale. In any case, as already stated, especially suitable isolated cases might occur in which we should have to consider the experiment of freeing completely from disease the region concerned, by the

shooting off of the antelopes in a not too large, self-contained, *palpalis* region, rich in game, from which the population had been removed. No instance, however, in German East Africa is known to me at present where such a measure would be necessary or even recommendable, on the ground alone that the sleeping sickness districts in that country are in general not very rich in game.

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ON THE RELATION OF WILD GAME TO DISEASES OF MAN AND THE DOMESTIC ANIMALS, by Surgeon-General Sir DAVID BRUCE, A.M.S., Director, Scientific Commission of the Royal Society, Nyasaland, 1911-13.

Definition of terms used in this summary of evidence.

Trypanosoma brucei (9, 10, 11, 35, 39, 40, 45, 34).*—The parasite found in the wild game and tsetse flies of Nyasaland and many other parts of Africa. It gives rise in man to the "Nyasaland sleeping sickness" and in other animals to the disease known as nagana.

T. gambiense (56, 17, 18, 14).—This parasite has been found in the blood of cattle and of antelope on the shores of Victoria Nyanza. It gives rise to the sleeping sickness of West Africa and Uganda—the "Congo sleeping sickness."

Sleeping Sickness.—There are two varieties of this disease in Africa. They will be distinguished in this paper by calling the one the "Congo sleeping sickness," or "*T. gambiense* disease," and the other the "Nyasaland sleeping sickness," or "*T. brucei* disease." It would probably lead to less ambiguity if all infectious diseases were called by the name of the parasite causing them. For example malarial fever is an ambiguous term, whereas the names "*Plasmodium malaris* fever," and "*Plasmodium vivax* fever" show exactly what kind of malarial fever is being dealt with.

Tsetse Fly.—"The Fly." The principal carrier of the trypanosome diseases in Africa. The "Congo sleeping sickness" or "*T. gambiense* disease" is carried by *Glossina palpalis*, the "Nyasaland sleeping sickness" or "*T. brucei* disease" by *Glossina morsitans*. It is possible in a laboratory experiment to infect animals with *Trypanosoma gambiense* by means of *Glossina morsitans*, and with *Trypanosoma brucei* by means of *Glossina palpalis*, but this has never been proved to occur in nature (77).

"Fly Country."—Any belt or tract of country where tsetse flies abound.

Anatomy of Tsetse Flies:—

Proboscis.—The piercing and sucking apparatus of the tsetse fly (90, 89).

Labial Cavity.—This is the tube in the proboscis made by the bringing together of the two parts, the labium and the labrum. The labial cavity is the tube through which the blood passes to the stomach, when the fly is feeding.

Hypopharynx.—The terminal duct of the salivary glands. It runs in a groove in the floor of the labial

* NOTE.—The references in the text refer to the list of literature to be found at the end of the Summary.

cavity into which it opens near the tip of the proboscis. It conveys the secretion of the salivary glands.

Salivary Glands.—Large glands which lie principally in the abdomen of the tsetse fly. They secrete a fluid which when injected into an animal causes irritation and a flow of blood to the bitten point. This enables the fly to obtain a plentiful supply of food.

Alimentary System (80).—The digestive tube of the fly, comprising the labial cavity, the proventriculus, the sucking stomach, the fore, mid, and hind gut, and the rectum.

"Proclaimed Area" (38).—The area in Nyasaland where most of the cases of "Nyasaland sleeping sickness" or "*T. brucei* disease" have occurred. It is also known as the "sleeping sickness area."

Infected and Infective Flies.—An infected fly is one in which development of the trypanosomes has taken place, but unless the development has reached a certain stage the fly is not infective. An infective fly is one which is capable of conveying infection. An infective fly must be infected, an infected fly need not be infective.

Blood forms of Trypanosomes (26).—The trypanosomes in the blood of an infected animal are very different in size and shape from the same trypanosomes undergoing development in the tsetse fly. When the development is complete, however, the trypanosomes revert to the blood form, and it is only when the "fly" contains these "blood forms" that it is infective.

HISTORICAL.

In 1894 in Zululand (9, 10, 11) it was discovered for the first time that the "tsetse fly disease" or "nagana" was caused by a blood parasite belonging to the protozoa, a flagellate called by the generic name trypanosoma. The particular species of trypanosome encountered in Zululand in 1894 was one which was giving rise at that time to a serious epidemic among the native cattle. This species, which was afterwards called *Trypanosoma brucei* by Plimmer and Bradford (83), is particularly deadly to horses, mules, donkeys, and dogs, but not so much so to oxen, sheep, or goats. In Zululand at that time it was thought that a single species of trypanosome was being dealt with, but now with further knowledge it is clear that there was also present another species, *Trypanosoma pecorum*, a much smaller parasite and one particularly fatal to cattle, sheep, and goats.

It was proved at that time, and for the first time, that these parasites were carried from sick to healthy animals by the local tsetse fly (*Glossina morsitans*).

As no healthy horse or dog could be exposed even for a few hours in the Zululand "fly country," and as there were no horses or other domestic animals in this fly country, it was evident that the "fly" must procure the virus somewhere, and this led to the examination of the blood of the wild animals living in this district. In the blood of these animals, buffalo, koodoo, &c., the same trypanosome was found, and thus was established the fact that the wild game act as a reservoir of the virus of the trypanosome diseases of man and the domestic animals.

This work was done in Zululand between the years 1894 and 1897, and the following table represents the result of the examination of some of the wild animals found in the neighbouring fly belt.

Table 1.—Examination of the Blood of Wild Game in Zululand for Trypanosomes.

Animal.	Total Number examined.	Number found Infected.
Buffalo - - -	8	1
Koodoo - - -	8	4
Wildebeeste - - -	13	3
Waterbuck - - -	2	0
Impala - - -	2	0
Zebra - - -	2	0
Bushbuck - - -	1	1
Steinbuck - - -	2	1
Warthog - - -	2	0
Wild pig - - -	1	0

Animal.	Total Number examined.	Number found Infected.
Hyena - - -	1	1
Reedbuck - - -	1	1
Leopard - - -	2	0
Duiker - - -	1	0
Roi Reibok - - -	2	0
Mongoose - - -	1	0
Jackal - - -	1	0
Total - - -	49	12

Percentage of infected animals—24 per cent.

Since 1897 some further observations (21, 32, 53, 52) have been made regarding the presence of these parasites in the blood of wild game on the West Coast of Africa, in Uganda, and notably in North Eastern Rhodesia, by Kinghorn and Yorke (71, 73, 74).

In 1911 the Colonial Office sent out a Commission under the direction of the Royal Society to Nyasaland to investigate this question more fully than had been done in Zululand.

The Commission reached Kasu Camp, in Central Angoniland, on the 12th January 1912, and proceeded to examine the wild tsetse flies and the wild animals of that district.

This camp is situated some 30 miles to the west of Domira Bay, on Lake Nyasa (lat. 13° 40' S., long. 34° 12' E.). Its height above sea level is about 3,500 feet.

The country lying along the lake and extending some 20 miles inland is flat and covered with low scrub. It is full of tsetse flies and wild game, and it was here that the investigations of the Commission were carried out.

FIRST TERM OF REFERENCE TO COMMITTEE.

To report upon the present knowledge available on the questions of the parts played by wild animals and tsetse flies in Africa in the maintenance and spread of trypanosome infections in man and stock.

1. TRYPANOSOMES CAUSING DISEASE IN MAN AND DOMESTIC ANIMALS IN AFRICA.

Let me first describe briefly the various species of pathogenic trypanosomes found in Africa.

These trypanosomes may be divided into three groups—

A. Group. *T. brucei* group:—

1. *T. brucei*.
2. *T. gambiense*.
3. *T. evansi*.
4. *T. equiperdum*.

B. Group. *T. pecorum* group:—

1. *T. pecorum*.
2. *T. simia*.

C. Group. *T. vivax* group.

1. *T. vivax*.
2. *T. capre*.
3. *T. uniforme*.

These constitute all the known disease-producing trypanosomes of Africa. With the exception of *T. evansi* and *T. equiperdum* they are all carried from the sick to the healthy by tsetse flies. It may be convenient at this point to give a short description of each of them.

Each group is distinguished by certain characters. For example, in Group A. the *T. brucei* group, the trypanosomes are all more or less polymorphic, varying in shape and size from short and stumpy forms to long and slender. *T. brucei* and *T. gambiense* also develop in the same way in the tsetse fly. At first the development takes place in the intestinal tract, afterwards the parasites pass forward through the labial cavity and invade the salivary glands by way of the hypopharynx.

In Group B., the *T. pecorum* group, the trypanosomes are monomorphic and develop in the intestinal tract, labial cavity, and hypopharynx of the fly, but do not invade the salivary glands.

In Group C, the *T. vivax* group, the trypanosomes are very active, monomorphic, and develop in the labial cavity and hypopharynx of the fly but not in the intestinal tract or salivary glands.

Group A.

1. *Trypanosoma brucei* (8, 83) (Plimmer and Bradford, 1899).

Synonyms: *T. rhodesiense*; *T. togolense*; *T. pecaudi*; *T. ugandae* (87).—This was the first species of pathogenic trypanosome discovered in Africa. It gives rise to a fatal disease in man, horses, mules, donkeys, oxen, dogs, and many other animals. It is the cause of the so-called "Nyasaland sleeping sickness" in man. In Zululand it is called nagana by the natives, and is now widely known by this name.

Distribution.—It is widely distributed in Africa, extending from the Sudan to Zululand.

The districts in Africa where cases of "Nyasaland sleeping sickness," or "*T. brucei* disease," have been found are Portuguese East Africa to the east of Lake Nyasa, Central Angoniland to the west of the lake, the Luangwa Valley in N.E. Rhodesia, and "also in the districts south of Fort Jameson and to the west of Serenje" (Kingham and Yorke). "In N.W. Rhodesia, between Broken Hill and the Anglo-Belgian boundary" (Kingham and Yorke). Lately a number of cases have been discovered south of the Zambesi, in the Sebungwe district (61). In all probability cases will be found to occur wherever *Glossina morsitans* and wild game abound.

Morphology (33).—*T. brucei* is very various in shape, some forms being short and stumpy with little or no free flagellum, and others long and slender with a well-developed free flagellum.

Susceptibility of Animals (45).—Animals of many different species are attacked by this trypanosome, as will be seen from the following table:—

Table 2.—The Average Duration of Life, in Days, of various Animals infected with *Trypanosoma brucei*, Nyasaland. The letter R stands for refractory.

	Average Duration, in Days.	No. of Animals employed.
Man - - - -	90	?
Ox - - - -	134	1
Goat and sheep - - -	42	29
Baboon - - - -	R	1
Monkey - - - -	26	20
Dog - - - -	34	25
Rabbit - - - -	28	7
Guinea pig - - - -	67	15
White rat - - - -	30	21

Reservoir.—This parasite is found in the blood of wild animals throughout a large part of Africa. Its presence in the wild game of Nyasaland will be discussed more fully later on.

Carrier.—The carrier of *Trypanosoma brucei* in Nyasaland is the common tsetse fly, *Glossina morsitans*. The development of *T. brucei* in *G. morsitans* takes place at first in the intestinal tract of the fly. After some 20 or 30 days the trypanosomes pass forward through the labial cavity of the proboscis, and from there, by way of the hypopharynx, into the salivary glands, where they multiply to an extraordinary extent.

The trypanosomes undergoing development in the intestinal tract are not infective. It is only when the trypanosomes reach the salivary glands that they assume the "blood form" and become infective.

2. *Trypanosoma gambiense* (Dutton, 1902).

This is the parasite which gives rise to the "Congo sleeping sickness."

Distribution.—It has a wide distribution in Africa between 16° N. lat. and 14° S. lat.

Morphology (14, 81, 16).—It is very similar to *T. brucei* in shape.

It is doubtful if it is possible in every case to distinguish *T. brucei* from *T. gambiense* by morphology alone.

Susceptibility of Animals (69).—This disease is, as a rule, fatal to man. It is usually chronic and may last for years. The natives, who are often neglected by their friends, frequently die in Uganda in as short a time as five or six months. In the French Congo it has been stated that as many as 17 per cent. are still alive after six years. Among Europeans a few recoveries take place under treatment. *T. gambiense* differs in a marked way from *T. brucei* in its behaviour to animals other than man. *T. brucei* infects most animals—horses, cattle, monkeys, dogs, and rats—readily, and kills them off quickly. On the other hand, it is a matter of difficulty to infect animals with *T. gambiense*. It would almost appear as if *T. gambiense* had become adapted to live in human blood and not in the blood of the lower animals.

Reservoir (55, 31, 32).—The native himself is probably the most common reservoir of this virus, but wild monkeys, antelope, and domestic cattle have also been found infected, and the antelope (53, 52, 54) are undoubtedly keeping up the infectivity of the "Fly" at the present time on the shores of Victoria Nyanza. The natives were removed from these unhealthy areas some years ago, but the tsetse flies (*Glossina palpalis*) (22) still remain infective.

Carrier (57).—The "Congo sleeping sickness" is carried from the sick to the healthy by means of the tsetse fly *Glossina palpalis*. The development of *T. gambiense* in *Glossina palpalis* is similar to that of *T. brucei* in *Glossina morsitans*.

3. *Trypanosoma evansi* (Steel, 1885).

This is a trypanosome disease of North Africa and India, where it is known as surra. It principally attacks camels and horses. It has probably spread along the camel routes from Africa into India.

Distribution.—This disease is known in the Sudan and neighbouring parts of North Africa.

Morphology (15).—Belongs to the *T. brucei* group.

Reservoir.—It is probably spread from one infected animal to another. It is, however, possible that the native Indian buffalo may act as reservoir.

Carrier.—It is thought at present that it is carried mechanically from sick animals to healthy by means of the large biting flies, the tabanids or horse flies. It is believed that no special development of the parasite takes place in these flies, but that they merely carry the virus from one animal to another in the same way that a surgeon carries vaccine from one child to another.

4. *Trypanosoma equiperdum* (Doflein, 1901).

A trypanosome disease of horses in North Africa, Europe, India, and North America. It is not conveyed by a fly but by the act of coitus between stallion and mare. Geldings never suffer.

Group B.

1. *Trypanosoma pecorum* (Bruce, 1910).

Synonyms.—*T. dimorphon*, *T. congolense*, *T. confusum*, *T. nanum*.

This parasite gives rise to a fatal disease among cattle, goats, sheep, and dogs, and is probably the most important trypanosome disease in Africa among the bovines. It is probably the same disease or very similar to that caused by *T. dimorphon*, *T. congolense*, *T. confusum*, and *T. nanum*.

Distribution.—It is widely distributed in Africa, occurring in the Congo territories, Uganda, Northern Rhodesia, Nyasaland, Zanzibar (19), Zululand, &c.

Morphology (43).—It is one of the smaller types and does not possess a free flagellum.

Animals susceptible (43).—The following table gives the average duration of this disease in various animals:—

Table 3.—The Average Duration of Life, in Days, of various Animals infected by *T. pecorum*.

	Donkey.	Cattle.	Goat.	Pig.	Monkey.	Dog.	Guinea Pig.	White Rat.
Average duration in days	87½	121½	55	21	129	48	41	33
Number of animals employed	1	4	59	1	11	57	5	10

Reservoir.—Common in the blood of wild game in many parts of Africa.

Carrier.—The tsetse fly *Glossina morsitans*, in Nyasaland, and probably *Glossina palpalis* in Uganda, but there is some evidence available that when this disease has once been introduced into a herd by the tsetse fly it may spread from one animal to another by means of other biting flies, probably tabanidae and hematopota. The development in *Glossina morsitans*, as already mentioned, takes place in the intestinal tract, the labial cavity, and the hypopharynx.

2. *Trypanosoma simiae* (Bruce, 1912).

Gives rise to a rapidly fatal disease in the monkey and the domestic pig. Frequently a healthy pig will die in four days if bitten by a tsetse fly infected by this trypanosome.

Distribution.—Only found up to the present in North-Eastern Rhodesia and Nyasaland.

Morphology (36, 46).—Closely resembles *T. pecorum* except that it is larger.

Animals susceptible.—An interesting feature in this trypanosome is the virulence it displays towards monkeys and the domestic pig, killing these animals in an incredibly short period of time, whereas it is harmless to oxen, antelope, dogs, and the smaller experimental animals, such as the rabbit, guinea-pig, and rat. Curiously enough this trypanosome also infects goats and sheep, although cattle and antelope escape.

Reservoir.—The warthog seems to be the only reservoir of this parasite among wild animals. It will probably be also found in the bush pig, but the Commission failed to obtain any of these animals for examination.

Carrier (47).—*Glossina morsitans*. The development of this parasite in *Glossina morsitans* is identical with *T. pecorum*.

Group C.

1. *Trypanosoma vivax* (Ziemann, 1905).

Horses, oxen, and goats seem to be the animals chiefly affected by this disease.

Distribution.—Wide distribution in Africa, Gambia, Senegal, Dahomey, N. Nigeria, Cameroons, Uganda, Tanganyika, and the Sudan. The Commission did not find it in Nyasaland, where it seems to be replaced by *T. caprae*.

Morphology.—The chief characteristic of this species is its extreme rapidity of motion when alive.

Susceptibility of Animals.—One peculiarity of this trypanosome is that it is harmless to monkeys, dogs, rabbits, guinea-pigs, and rats.

Reservoir.—The wild game.

Carrier (23, 27).—*Glossina palpalis* in Uganda. The development takes place only in the labial cavity and the hypopharynx.

2. *Trypanosoma caprae* (Kleine (48, 37).

Similar to *T. vivax* in its action on animals.

Distribution.—It has only been reported from German East Africa and Nyasaland.

Morphology.—Very similar to *T. vivax* but stouter in build.

Susceptibility of Animals.—Goats and sheep are attacked.

Reservoir.—The wild game.

Carrier.—*Glossina morsitans*.

3. *Trypanosoma uniforme*.

Similar in many ways to *T. vivax*.

Distribution.—Only found up to the present in Uganda.

Morphology.—Similar to *T. vivax* but smaller.

Susceptibility of Animals.—Cattle, goats, and sheep are susceptible to this species of trypanosome, but not monkeys, dogs, or smaller experimental animals.

Reservoir.—Found in antelope in Uganda.

Carrier.—*Glossina palpalis*.

2. THE TSETSE FLIES OF AFRICA (1, 2).

General Characters.—These insects are dull coloured, ordinary looking flies about half an inch in length. The strong proboscis stands out horizontally in front. The wings are long and close over each other like the blades of a pair of scissors when the fly is at rest. The dorsal aspect of the abdomen is marked by five more or less distinct transverse bands.

Habitat.—Some species are only found on the wooded banks of lakes or rivers, where the water is clear and free from reeds. Others, again, are found scattered over the country among the thorn scrub, and often at a distance from water, to which indeed they seem to have an antipathy.

Reproduction.—This is important. Tsetse flies do not lay a number of eggs like the majority of the diptera, but they produce a single larva, which immediately after birth creeps into the nearest cranny among loose sand or earth or dead leaves and there turns into a black and hard puparium. Five or six weeks afterwards the adult fly emerges.

Classification.—The entomologist is constantly changing the names of these insects and dividing and subdividing them into new species. In 1894 there was only one species, *Glossina morsitans*, now; in 1913, there are at least 17 species described.

For the purpose of this Committee it will only be necessary to describe briefly three species of tsetse flies:—

- | | | | |
|-----------|---|---|-------------------------------|
| 1st Group | - | - | <i>Glossina morsitans</i> . |
| 2nd Group | - | - | <i>Glossina palpalis</i> . |
| 3rd Group | - | - | <i>Glossina brevipalpis</i> . |

1. *Glossina morsitans*.

This is a lighter coloured fly than *Glossina palpalis* and the markings on the dorsum of the abdomen are more distinct.

Distribution.—This fly is more widely distributed than any other tsetse fly.

Habitat.—This species lives in the dry thorny scrub which covers large areas of tropical Africa. It is not, as a rule, found along the banks of rivers or lakes. It requires shade, however, and is never or rarely found on open grass plains where it would be liable to be scorched by the sun. It is much more numerous towards the end of the rainy season and diminishes in numbers towards the end of the dry season when all the grass is burnt off. It occurs in some places in great numbers and attacks with great voracity, resembling a swarm of angry bees. It is more attracted by a moving object than a stationary one, and it is when riding a motor-bicycle through "fly country" that the greatest number attacking at one time has been seen.

2. *Glossina palpalis*.

This is a darker coloured fly than *Glossina morsitans*, and the transverse markings in the dorsum of the abdomen are less marked.

Distribution.—This tsetse fly is widely distributed in West Africa from the mouth of the Senegal River (about 16° N.) to Angola.

Habitat.—This species lives close to the margins of rivers and lakes. On account of this it was a comparatively easy matter for the Uganda Government to stop the epidemic of "Congo sleeping-sickness," which had decimated the islands and shores of the lake for some years. This was done by removing the natives from the islands and lake-shore to healthy sites inland.

3. *Glossina brevipes*.

This tsetse fly is much larger than *Glossina morsitans* and *Glossina palpalis*. It is brownish in colour, and the transverse dorsal markings on the abdomen are faint and indistinct.

Distribution.—This is the common large tsetse fly of British, German, and Portuguese East Africa, Nyasaland, and N.E. Rhodesia.

Habitat.—This fly is found in Nyasaland along the banks of the numerous streams which run into the lake. Little is known as to its disease-producing activity, but it is probable that it cannot be ignored, since the Royal Society's Commission have found specimens of this fly infected with more than one trypanosome disease.

3. TRYPANOSOMES FOUND IN THE BLOOD OF THE WILD ANIMALS WHICH LIVE IN THE SLEEPING SICKNESS AREA, NYASALAND (28).

The Method of examining the Blood of Wild Game.

When an animal was shot in this fly country by a member of the Commission, a small quantity of the blood was taken in a bottle containing citrate of soda solution to prevent coagulation. This blood was used for inoculation purposes, and a thick and thin film of the blood spread on glass slides for microscopical examination. The blood was then sent to a point on one of the main paths, where a motor-cyclist was waiting to carry it to the camp. When the blood arrived it was at once injected into a goat, a monkey, and a dog. Up to the end of 1912, 180 specimens of the wild animals living in the fly area adjoining the Commission's camp at Kasu were examined for disease producing trypanosomes.

The part of the country where the animals were procured is situated in the "proclaimed area" or "sleeping sickness area" of Nyasaland, which extends from the Chirua River (lat. 13° 20' S., long. 34° E.) in the north to the Lintipe River (lat. 13° 50' S., long. 34° 30' E.) in the south. It is bounded on the east by the lake and on the west by the foot hills. The area is about 50 miles from north to south, and 25 miles from east to west. This is the part of the country in which most of the cases of the human trypanosome disease of Nyasaland, up to the present, have been found. It will be seen, then, that these animals were procured from the very heart of the sleeping sickness area.

It is by no means easy to arrive at anything like a correct enumeration of the number of big game living in such an area as that under consideration.

Mr. Ockenden, Senior Resident Magistrate, is of opinion that there are 600 elephants and 30,000 eland in his district, Central Angoniland, of which the "sleeping sickness area" forms about a fifth part. But as these animals are constantly on the move, migrating from one part of the district to another, it can be understood that a large number of wild game may pass through the sleeping sickness area in the course of the year.

Among the 180 animals, 57 were found to harbour pathogenic trypanosomes—31·7 per cent.

This, however, is probably much below the actual percentage. A wild animal is only examined once, and that often under difficult conditions. If it were possible to examine the same animal several times, it is probable that all would be found to be infected. The trypanosomes come and go in the blood; if absent to-day they may have been present a fortnight ago, or will be present a fortnight hence. The big game live

among swarms of infected flies, and are constantly liable to reinfection.

Table 4.—Species of Trypanosomes found in the Blood of Wild Animals living in the Sleeping Sickness Area, Nyasaland.

Date.	Experiment Number.	Animal.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
1912.						
Jan. 22	41	Eland -	-	1	-	-
" 22	41	" -	-	1	-	-
May 19	616	Waterbuck -	-	-	-	1
" 26	591	Reedbuck -	-	-	-	1
June 2	613	Buffalo -	-	1	-	-
" 23	743	Oribi -	-	-	-	-
" 30	783	Reedbuck -	1	-	-	1
" 1	779	Hartebeeste -	1	-	-	-
" 5	828	Reedbuck -	-	-	-	1
" 6	826	Warthog -	-	1	-	-
" 8	863	Oribi -	1	-	-	-
" 10	860	" -	-	-	-	1
July 20	912	Reedbuck -	-	1	-	-
" 22	933	Warthog -	-	1	-	-
" 23	955	Hyena -	-	1	-	-
" 23	956	" -	-	1	-	-
" 23	957	Hartebeeste -	1	-	-	-
" 23	958	" -	1	-	-	-
" 25	988	Reedbuck -	-	-	-	1
" 27	1,000	Hartebeeste -	1	-	-	-
" 29	1,007	Duiker -	1	-	-	-
" 30	1,013	Eland -	-	1	-	-
Aug. 2	1,027	Duiker -	-	-	-	-
" 4	1,044	Eland -	-	1	-	-
" 11	1,058	Koodoo -	-	1	-	-
" 11	1,061	Waterbuck -	-	1	-	1
" 11	1,064	Warthog -	1	-	-	-
" 18	1,078	Bushbuck -	-	1	-	-
" 18	1,081	" -	-	1	-	-
" 18	1,084	" -	-	1	-	-
" 18	1,087	" -	-	1	-	1
" 19	1,096	Oribi -	-	1	-	-
" 21	1,139	Warthog -	-	-	1	-
" 21	1,142	Hartebeeste -	1	-	-	-
" 22	1,150	Reedbuck -	-	-	-	1
" 22	1,153	" -	-	-	-	1
" 22	1,156	" -	-	-	-	1
" 22	1,162	" -	-	-	-	1
" 24	1,180	Waterbuck -	1	-	-	1
" 24	1,186	Warthog -	-	-	1	-
" 24	1,189	" -	-	1	-	-
" 28	1,202	Eland -	-	1	-	-
" 28	1,203	Bushbuck -	-	1	-	-
" 28	1,210	Waterbuck -	1	-	-	1
" 30	1,216	Bushbuck -	-	1	-	-
Sept. 7	1,261	" -	-	1	-	-
" 7	1,264	Waterbuck -	1	-	-	-
" 11	1,304	Buffalo -	-	1	-	-
" 12	1,308	Warthog -	-	-	1	-
" 13	1,339	Waterbuck -	-	-	-	1
" 13	1,347	Reedbuck -	1	-	-	-
" 14	1,363	" -	-	-	-	1
" 17	1,380	Koodoo -	-	1	-	-
" 18	1,388	Waterbuck -	-	-	-	1
" 20	1,406	" -	-	-	-	1
" 20	1,410	" -	-	-	-	1
" 23	1,415	Reedbuck -	1	-	-	-
" 25	1,453	Hartebeeste -	-	1	-	-
Oct. 6	1,471	Eland -	-	1	-	-

Table 5.—Species of Trypanosomes found in the Blood of Wild Animals in the Sleeping Sickness Area, Nyasaland, and the Number of Times each was found.

Number of Animals.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
180	14	26	3	20

In every 100 wild animals living in the sleeping sickness area, Nyasaland, taken at random, the following numbers may be expected to be found infected with these species of trypanosomes.

Table 6.—Percentage of Animals infected by the different Species of Trypanosomes.

T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
7.8	14.4	1.7	11.1

Table 7.—The Species of Animals dealt with, the Total Number examined, the Number found Infected, and the Species of Trypanosomes by which they were Infected.

Animal.	Total examined.	No. found infected.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
Eland	10	6	—	6	—	1
Sable	5	—	—	—	—	—
Waterbuck	13	9	3	1	—	8
Koodoo	3	2	—	2	—	—
Bushbuck	10	7	—	7	—	1
Hartebeeste	35	6	3	1	—	—
Reedbuck	19	12	3	1	—	9
Oribi	26	4	1	1	—	1
Duiker	7	2	1	—	—	—
Buffalo	9	2	—	2	—	—
Lion	1	—	—	—	—	—
Hyena	3	2	—	2	—	—
Elephant	2	—	—	—	—	—
Warthog	33	7	1	3	3	—
Wild cat	3	—	—	—	—	—
Porcupine	1	—	—	—	—	—
Total	180	59	14	26	3	20

The next table gives the percentages of the different trypanosomes occurring in the wild animals. The numbers are too small to be taken literally, but it is interesting to learn that in this fly district the waterbuck, hartebeeste, reedbuck, and duiker are dangerous neighbours to man; the eland, koodoo, bushbuck, and buffalo to cattle, goats and sheep; and that the warthog is the only animal which harbours *T. simiae*, the lightning destroyer of the domestic pig.

Table 8.—Percentages of different Species of Trypanosomes harboured by Wild Animals in the Fly Area.

Animal.	No. examined.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
		Per cent.	Per cent.	Per cent.	Per cent.
Eland	10	—	60	—	10
Sable	5	—	—	—	—
Waterbuck	13	23	8	—	61
Koodoo	3	—	66	—	—
Bushbuck	10	—	70	—	10
Hartebeeste	35	14	3	—	—
Reedbuck	19	16	5	—	47
Oribi	26	4	4	—	4
Duiker	7	14	—	—	—
Buffalo	9	—	22	—	—
Lion	1	—	—	—	—
Hyena	3	—	66	—	—
Elephant	2	—	—	—	—
Warthog	33	3	9	9	—
Wild cat	3	—	—	—	—
Porcupine	1	—	—	—	—

Conclusions.

1. 31.7 per cent. of the wild game in the fly country below Kasu Hill harbour pathogenic trypanosomes.

2. The species of trypanosomes found are *T. brucei* 7.8 per cent., *T. pecorum* 14.4 per cent., *T. simiae* 1.7 per cent. and *T. capre* 11.1 per cent.

3. It is self-evident that these wild animals should not be allowed to live in "fly country," where they constitute a standing danger to the native inhabitants and the domestic animals. It would be as reasonable to allow mad dogs to live and be protected by law in our English towns and villages. Not only should all game laws restricting their destruction in "fly country" be removed, but active measures should be taken for their early and complete blotting out.

4. It must be strictly borne in mind that this only refers to wild animals living in fly areas. No pathogenic trypanosomes have, up to the present, been found by the Commission in the blood of animals living in fly-free areas.

4. INFECTIVITY OF THE TSETSE FLY (*Glossina morsitans*) IN NYASALAND (42).

Methods employed.

The method employed in studying the infectivity of the flies was simple. Native boys were employed in catching the flies, which were brought up to the camp at Kasu in small cages by a native cyclist. Each cage of flies was fed on three healthy animals—the first day on a monkey, the second on a dog, and the third on a goat. To ensure, as far as possible, that each animal was fed on by every fly, the flies were fed nine times—three times on each animal.

The number of flies brought up on each day would probably average about 60, and as each animal was fed on by three cagefuls, then each monkey, dog, and goat ran the gauntlet of some 180 flies. It is therefore impossible to arrive at any very precise knowledge of the proportion of infective flies in each cage.

Infectivity of the Flies.

As will be seen from Table 9, every experiment, with the exception of one, was positive, and on two occasions a goat was infected by all the four species of pathogenic trypanosomes occurring in this neighbourhood.

The Commission have shown that one* in three of the wild game found in this district is infected with trypanosomes, and recommended that the animals should be destroyed. If this were done and a year allowed to elapse, the proportion of infective flies then found would be an index of the usefulness or futility of such operations.

The Commission is of opinion that the wild game is the principal factor in the spread of trypanosome disease, and that, for practical purposes, the smaller mammals, birds, and reptiles need not be taken into account.

The following table gives, in the first column, the date the first cageful of flies was fed on the monkey, the second column the number of flies fed; the signs *plus* and *minus* show the result of feeding the flies on the monkey, dog, and goat.

The four species of trypanosomes carried by the "fly" in this district are *T. brucei*, *T. pecorum*, *T. simiae*, and *T. capre*. The first and second of these attack all three experimental animals, the third the monkey and goat, being harmless to the dog, whereas the fourth only produces disease in the goat.

Where no *plus* or *minus* sign occurs it means that an animal was not available. For example, the experiment beginning on January 20th shows that the monkey was infected by *T. simiae*, the goat by *T. brucei*, and that no dog was available. The experiment on February 21st shows that neither the dog nor goat became infected by the bites of 170 flies, and that no monkey was available.

Table 9.—The Result of Feeding 10,081 Tsetse Flies (*G. morsitans*), caught in the "Proclaimed Area," Nyasaland, on Monkeys, Dogs, and Goats.

1912.	Number of Flies fed.	Monkey.				Dog.				Goat.			
		T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.	T. Brucei.	T. Pecorum.	T. Simiae.	T. Capre.
Jan. 20	296	-	-	+	-					+	-	-	-
" 24	370	-	-	+	-					-	+	-	+
" 29	280	-	-	+	-					-	+	+	-
Feb. 2	295	-	-	+	-					-	+	+	+
" 8	220	-	-	+	-					-	+	+	+
" 13	200	-	+	+	-					-	-	+	+
" 16	195					+	-	-	-	+	-	-	-
" 21	170					-	-	-	-	-	-	-	-
" 26	170					-	-	-	-	-	-	+	-
Mar. 2	140					-	-	-	-	-	-	-	+
" 9	165					-	+	-	-	-	-	-	+
" 14	100					-	-	-	-	-	-	-	+
" 17	160					-	+	-	-	-	+	-	-
" 22	205					-	+	-	-	+	+	+	+
April 3	135					-	+	-	-	-	+	-	-
" 10	275	+	-	+	-	+	+	-	-	-	-	-	+
" 15	330	-	-	+	-	-	+	-	-	-	+	+	+
" 18	200	-	-	+	-	-	+	-	-	-	+	+	+
" 18	180	-	-	+	-	-	+	-	-	-	-	+	-
" 23	230	-	-	+	-	-	+	-	-	-	-	-	+
" 23	140	-	+	-	-	-	+	-	-	-	+	-	+
" 26	100	-	-	+	-	-	+	-	-	+	+	+	-
" 27	260	-	-	+	-	-	+	-	-	-	+	-	+
May 3	155	+	+	+	-	-	+	-	-	-	+	-	+
" 3	96	-	-	-	-	-	+	-	-	-	+	-	-
" 8	330	+	-	+	-	+	-	-	-	-	+	-	+
" 9	120	-	-	+	-	-	+	-	-	-	+	-	+
" 13	50	-	-	-	-	+	-	-	-	-	-	+	+
" 14	250	-	-	+	-	-	+	-	-	+	+	+	+
" 17	190					+	+	-	-	-	-	+	+
" 24	113					-	+	-	-	-	+	+	+
" 29	120	-	-	-	-	-	+	-	-	-	+	-	-
" 29	230	-	-	-	-	-	+	-	-	-	+	-	-
" 29	320					+	+	-	-	-	+	-	+
" 29	240	-	+	-	-	-	+	-	-	-	+	+	+
" 29	100					-	+	-	-	-	+	+	+
" 31	175	+	-	+	-	+	+	-	-	-	+	+	-
June 2	300					-	+	-	-	-	-	+	+
" 6	210	-	+	-	-	-	+	-	-	-	+	-	+
" 7	230	+	-	-	-	+	+	-	-	+	+	-	-
" 11	160	-	-	+	-	-	+	-	-	-	+	-	+
" 18	135	-	-	-	-	-	+	-	-	-	+	-	-
" 25	90	+	-	-	-	+	-	-	-	-	+	-	+
July 3	95					-	+	-	-	-	+	-	-
Sept. 25	70	-	+	-	-					-	-	-	-
" 27	25	+	-	-	-					-	-	-	-
Oct. 29	87	+	-	-	-	+	-	-	-	+	+	-	+
Nov. 5	145					-	-	-	-	-	-	-	+
" 11	150	-	-	+	-	-	+	-	-	-	+	-	+
" 18	157	-	-	+	-	-	+	-	-	-	-	+	+
" 21	95	-	-	-	-	+	-	-	-	+	-	-	-
" 25	180	-	-	+	-	+	-	-	-	+	+	-	-
Dec. 3	180		+	+	-	-	+	-	-	-	+	-	+
" 6	198	+	+	+	-	+	-	-	-	+	-	-	+
" 11	156	-	-	+	-	+	-	-	-	+	+	-	-
" 16	113	-	+	+	-	-	+	-	-	-	+	-	+
Total	10,081	9	9	26	-	14	34	-	-	11	35	17	35

Table 10.—The Number of Times a Monkey, Dog, and Goat became infected with *Trypanosoma brucei*, *T. pecorum*, *T. simiae*, and *T. capre* in a Series of 56 Experiments, averaging 180 Tsetse Flies each.

T. Brucei.			T. Pecorum.			T. Simiae.			T. Capre.		
Monkey.	Dog.	Goat.	Monkey.	Dog.	Goat.	Monkey.	Dog.	Goat.	Monkey.	Dog.	Goat.
9	14	11	9	34	35	26	0	17	0	0	35

This shows that the monkey is less susceptible to *T. brucei* and *T. pecorum* than the dog, whereas it is remarkably susceptible to *T. simia*.

The dog is not susceptible to *T. simia*, and neither the monkey nor dog to *T. capra*.

Table 11.—The Proportion per 1,000 Tsetse Flies, caught in the "Sleeping Sickness" Area of Nyasaland, found to be Infective with Pathogenic Trypanosomes.

<i>T. brucei</i> .	<i>T. pecorum</i> .	<i>T. simia</i> .	<i>T. capra</i> .
2.0	4.6	3.4	3.5

This is only allowing one infective fly to each series of flies fed on the experimental animals, and is therefore the irreducible minimum. The average number of flies fed on each animal was 180, and it might well be that there were present in the same batch several flies infective with the same species of trypanosome. Ten thousand flies gave rise to 135 infections, and taking it for granted that no fly was infective with more than one species of trypanosome, then 13.5 per 1,000 flies are infective with one or other of the disease-producing trypanosomes of this district.

Table 12.—Number of Times the Trypanosomes under consideration were found in 56 Experiments.

<i>T. brucei</i> .	<i>T. pecorum</i> .	<i>T. simia</i> .	<i>T. capra</i> .
20	46	34	35
35.7 per cent.	82.1 per cent.	60.7 per cent.	62.5 per cent.

This means that, in experiments carried out in the manner described, *T. brucei* may be expected to turn up once in every three series, *T. pecorum* eight times in ten, and *T. simia* and *T. capra* six times in ten.

Months and Seasons.

On examining Table 9 it will be seen that these infective flies occur all the year round, and are just as numerous during one season as another. It will also be seen that no experiments on the infectivity of the flies were carried out during July and August. This was due to the fact that all the energy of the Commission was devoted during these two months to the study of the wild game.

Conclusions.

1. The tsetse flies (*Glossina morsitans*) caught in the "fly country" near Kasu Camp in Nyasaland are infected with four species of disease-producing trypanosomes—*T. brucei*, *T. pecorum*, *T. simia*, and *T. capra*.
2. The proportion of infective flies is 13.5 per 1,000.
3. The proportion of flies infective with *T. brucei*, the cause of the human trypanosome disease of Nyasaland, is 2 per 1,000.
4. The flies are found infective all the year round.
5. To prevent the infection of tsetse flies it is proposed that the experiment should be tried of destroying all the wild game in the "Proclaimed Area" of Nyasaland.

5. ON THE IDENTITY OR NON-IDENTITY OF *Trypanosoma brucei* AND *Trypanosoma rhodesiense*.

This is a most important question; and, with our present knowledge, one impossible to answer with any certainty. If *Trypanosoma brucei* and *Trypanosoma rhodesiense* are identical, then we must expect to find cases of "Nyasaland sleeping sickness" (*Trypanosoma brucei* disease) cropping up in all parts of Africa

where *Glossina morsitans* and *Trypanosoma brucei* occur. If, on the other hand, they are separate species, then cases of this disease would only be expected to occur where *Trypanosoma rhodesiense* is found. In the past, *Trypanosoma brucei* has always been regarded as harmless to man; and certainly no harm seemed to come to those who lived or travelled in the "fly country."

It must be explained at the outset that up to about the year 1908 it was believed that *Trypanosoma gambiense* was the only trypanosome capable of setting up disease in man.

It must also be clearly kept in mind that, up to that time, no case of trypanosome disease in man had ever been found in an exclusively *Glossina morsitans* or *nagana* area. All cases of sleeping sickness up to then had been found in districts where *Glossina palpalis* was present. Since the year 1908, however, cases have been discovered in Northern and Southern Rhodesia; in Nyasaland, and in Portuguese East Africa, where there are no *Glossina palpalis*, but where only *Glossina morsitans* is found.

At first it was supposed that this was a spread of "Congo sleeping sickness" or "*Trypanosoma gambiense* disease" from Lake Tanganyika along the trade route through the Luangwa Valley into Nyasaland. And, in truth, this explanation seems very natural. But, in the Liverpool School of Tropical Medicine, it was discovered that the trypanosomes (88) causing the "Nyasaland sleeping sickness" differed in some ways from "*Trypanosoma gambiense*"; and it was therefore called "*Trypanosoma rhodesiense*" and the disease declared to be a new disease.

In 1912, the Scientific Commission of the Royal Society, in further studying this question, also came to the conclusion that "Nyasaland sleeping sickness" and "Congo sleeping sickness" were separate diseases. In other words, that *Trypanosoma gambiense* and *Trypanosoma rhodesiense* were different species.

The Commission was soon struck by the fact that, although the fly country of Nyasaland extends almost uninterruptedly into the Zululand fly country, no *Trypanosoma brucei*—the common trypanosome of wild game in Africa—could be found. Everywhere, in the wild tsetse flies and in the wild game, it was always *Trypanosoma rhodesiense*. It then began to dawn on the mind of the Commission that perhaps *Trypanosoma rhodesiense* was in reality the old *Trypanosoma brucei* masquerading under a new name. At once a strain of *Trypanosoma brucei* from Zululand was procured for purposes of comparison; and the object of this part of the summary of evidence is to give the arguments in favour of the theory held by the Commission that *Trypanosoma brucei* and *Trypanosoma rhodesiense* are one and the same species.

In trying to separate species of trypanosomes from one another, the following points may be considered:—

- (1) Are they alike in size and shape?—Morphology.
- (2) Have they the same habits and actions when alive?
- (3) Do the two trypanosomes infect the same animals and kill them in the same time?
- (4) Have they the same carrier? and does their growth or development in the carrier follow the same course?
- (5) Can both be grown outside the body on artificial media? and do they show the same characteristics when thus cultivated?
- (6) Cross-inoculation experiments:—If an animal has recovered from an attack of disease set up by one of the two trypanosomes, is it immune or non-immune to infection by the other?
- (7) Experiments on the effect of various sera.
- (8) Geographical distribution.

It is unfortunate that the paper on various strains of *Trypanosoma brucei* obtained from Zululand, which the Royal Society's Commission in Nyasaland is at present preparing, has not yet been published. It is therefore impossible, at this distance, without notes, to do more than state what I believe to be the facts.

1. Allow me then to run through these several points in order to decide, as far as possible, whether

the two trypanosomes under consideration belong to one or more species.

In regard to the first point—and a most important point—namely, the morphology—I can produce coloured plates and black-and-white drawings of both *Trypanosoma brucei*, Zululand, and *Trypanosoma rhodesiense*, Nyasaland. On comparing these drawings, it is quite evident that in size and shape these two trypanosomes are identical.

2. In my opinion, they are also alike in habits and actions when alive.

3. They have the same action on cattle, goats, sheep, monkeys, dogs, rabbits, guinea pigs and white rats. *Trypanosoma rhodesiense*, Nyasaland, is also fatal to man; but as it is impracticable to obtain man for experiment, it is impossible to say whether *Trypanosoma brucei*, Zululand, would or would not infect him. In my opinion, if the experiment was repeated sufficiently often, it would succeed in infecting man. On the other hand, it will be urged that Dr. Taute (91) in Portuguese East Africa injected blood from an animal infected with what he considered to be *Trypanosoma brucei*, under his skin, and also allowed tsetse flies, infected with this trypanosome, to feed on him. Both experiments were negative. It is a matter for some scientific regret that these experiments were not successful—though we can ill spare our bold and somewhat rash colleague—as then the question would have been answered. As it is, this negative experiment proves nothing. It may be that only one man in a thousand would become infected in this way.

There is also the argument that men have been exposed to *Trypanosoma brucei* for many years, and no case of *Trypanosoma brucei* disease has occurred. To this it may be answered that it is only within recent years that microscopical examination of the blood of such cases has been made.

Many cases of death are reported to have occurred among hunters and explorers, and these were put down to "malaria"; but as there was no blood examination, it is quite possible that some or all of these cases might have been due to infection by *Trypanosoma brucei*. It is doubtful if man ever dies of uncomplicated malaria.

Take the case of kala-azar: this widespread disease, which is exceedingly common in India and, indeed, in most parts of the world, remained undiscovered until a few years ago. This was about the year 1900. Up to that time all these thousands of cases had been returned as "chronic malaria." The reason why these cases of kala-azar were not properly diagnosed was probably also due to the fact that up to 1900 few microscopic examinations of the organs of this disease were made. The discovery of these minute parasites also often depends on the introduction of some new mode of staining.

Or take the case of Malta fever: up to the year 1886, when its cause was discovered, all cases of this well-marked and widely-distributed fever were returned as "remittent fever," or "enteric fever," although its symptoms and post-mortem features bear no resemblance to either of these diseases.

Typhus fever and enteric fever are other cases in point.

Now, just as in the case of kala-azar, Malta fever, and enteric fever, so, in my opinion, cases of *Trypanosoma brucei* disease may have, from time to time, occurred in Zululand, Nyasaland, and other parts of Africa where wild game and *G. morsitans* are found, without attracting attention.

It is to be regretted that this important point must still remain in the domain of speculation and not rest on the firmer foundation of fact and experiment; but, I repeat, nothing can be done at present to settle this question satisfactorily, since man may not be used as an experimental animal.

The Committee will find it difficult to arrive at a certain decision in regard to the identity of *Trypanosoma brucei* and *Trypanosoma rhodesiense*. Many apparently cogent arguments can be advanced against the theory.

One of the strongest witnesses on the opposing side is Dr. Murray, of the Mvera Mission, Angoniland.

Dr. Murray informs me that, in his opinion, no cases of sleeping sickness could have occurred in the

proclaimed area, Nyasaland, before the year 1908. He was at that time medical missionary at the neighbouring mission station, Mvera. He states that he saw hundreds of cases at his dispensary before 1908; and he believes that if there had been cases of sleeping sickness before that time he must have noticed the fact.

He also is of opinion that the cases which have occurred at Hartley and around Salisbury were, without doubt, infected through natives from Nyasaland or from the Luangwa who had gone south in search of work, having easily evaded the regulations of the sleeping sickness area against their leaving.

But, on the other hand, it may be argued that the Nyasaland native suffering from sleeping sickness is more apt to remain in his own hut than ask for medical advice in a mission dispensary. If a native has something tangibly wrong with him, a broken arm, a wound, an ulcer, he is much more likely to seek advice than when he is suffering from some fever. And then again this disease makes such rapid progress that in a short time the native is unable to travel.

There are comparatively few cases of Nyasaland sleeping sickness in the proclaimed area. Since 1908 only some 120 cases have been found, and that after much searching. As there must be some 15,000 or 20,000 natives in this district, it seems quite probable that Dr. Murray never had a case in his dispensary. Even after it was known that this disease existed in his district, I am not aware that he found any cases.

4. *Trypanosoma brucei* and *Trypanosoma rhodesiense* have the same carrier, *Glossina morsitans*; and their development in the fly proceeds in the same way.

5. In regard to their culture outside the body, they behave in exactly the same way.

6. No satisfactory evidence against these two trypanosomes being the same species has been brought out by cross-inoculation experiments.

7. Human serum acts in the same way on *Trypanosoma rhodesiense* as it does on *Trypanosoma brucei*. In this, these trypanosomes differ from *Trypanosoma gambiense*. Many experiments have also been made in regard to the action of various sera on *Trypanosoma brucei* and *Trypanosoma rhodesiense*. Up to the present these experiments have failed to bring to light any fact which would assist in establishing the non-identity of these two trypanosomes.

8. In regard to the geographical distribution of "Nyasaland sleeping sickness," if Dr. Murray's theory is correct, that all these outlying outbreaks of trypanosome disease in man are due to infection from Lake Tanganyika, then the geographical distribution of the disease will not help in establishing the identity or non-identity of these two trypanosomes.

Dr. Murray's theory is that "Congo sleeping sickness," or *Trypanosoma gambiense*, has spread into Nyasaland from Lake Tanganyika. If it can be proved that *Trypanosoma gambiense* and *Trypanosoma brucei* vel *rhodesiense* are separate species, then his theory falls to the ground.

For my own part, I think that the geographical distribution of "Nyasaland sleeping sickness" or "*Trypanosoma brucei* disease" is the only way—experiment on man being impracticable—of showing that *Trypanosoma brucei* and *Trypanosoma rhodesiense* are one and the same.

If, in the future, cases of this disease are found in widely separated places, where *Glossina morsitans* and wild game abound, then it would be a strong argument that this form of sleeping sickness is caused by *Trypanosoma brucei*. Have we sufficient facts at present to enable us to form an opinion? Hardly, I am afraid; but they are multiplying. Take, for example, the discovery of cases in the Sebungwe District in Southern Rhodesia. Even Dr. Murray will hardly be able to say that this is due to infection from Nyasaland. Then the cases in German and Portuguese East Africa are difficult of explanation from his point of view. Further, the cases just found in the Marimba District north of the proclaimed area, and at Malembo on the southwest arm of the lake, south of the proclaimed areas.

The disease has been found in the Luangwa Valley, in North East Rhodesia, in Nyasaland, on the Rovuma, in Portuguese East Africa, in North West Rhodesia, and

in Southern Rhodesia. In all these widely separated places *Glossina morsitans* is the tsetse fly present. In my opinion, then, evidence is accumulating, from the geographical distribution point of view, that this new kind of sleeping sickness is really caused by *Trypanosoma brucei* and that the name *Trypanosoma rhodesiense* must be dropped.

Conclusions.—In the opinion of the Commission, *Trypanosoma brucei* and *Trypanosoma rhodesiense* are identical.

2. That the disease "Nyasaland sleeping sickness" has not been introduced into Nyasaland from Lake Tanganyika, but that it is an endemic disease of the country.

6. ON THE IDENTITY OR NON-IDENTITY OF *Trypanosoma gambiense* AND *Trypanosoma brucei* vel *rhodesiense*.

In the last section of this summary of evidence it was sought to establish that *T. brucei* and *T. rhodesiense* are one and the same species of trypanosome.

In this section it will be attempted to show that *T. gambiense* and *T. brucei* vel *rhodesiense* are separate species and give rise to distinct diseases (88).

This would be considered by most authorities as a work of supererogation, as most are convinced that these two trypanosomes constitute separate species.

But the more one works at these parasites the more difficulties spring up in regard to their classification.

1. **Morphology** (14, 16).—*T. gambiense* and *T. brucei* resemble each other closely in size and shape. There are the same short and stumpy and long and slender forms in both. It may be that there are more post-nuclear forms in *T. brucei*, but this is a very changeable quantity. In my opinion, it is impossible in every case to separate *T. gambiense* and *T. brucei* by morphology alone.

2. They are also alike in their habits and movements when alive.

3. In their action upon animals there is a distinct difference between the two trypanosomes (93, 18, 65). Whereas it is easy to infect horses, cattle, goats, sheep, monkeys, dogs, rabbits, guinea-pigs, and white rats with *T. brucei*, it is a matter of difficulty with *T. gambiense*. The disease in these animals is much more chronic with *T. gambiense* than with *T. brucei*.

When man is attacked by *T. brucei* (84, 85), it is a matter of months, whereas with *T. gambiense* (65) it is a matter of years.

It would almost appear that *T. gambiense* had become adapted to man and had lost much of its power of infecting the lower animals.

4. **The Carrier.**—These trypanosomes have different carriers, *T. gambiense* being carried by *Glossina palpalis* and *T. brucei* by *Glossina morsitans*.

It is an easy matter to get *T. gambiense* to develop (20, 26) in *Glossina palpalis*, but a difficult matter, and only after many failures, to succeed with *Glossina morsitans* (77).

5. **Cultural Characters.**—Nothing is gained by a study of these two trypanosomes in artificial cultures.

6. **Cross-inoculation Experiments.**—Animals recovered from *T. brucei* infection are still susceptible to *T. gambiense* and vice versa.

7. Probably the best means of separating *T. gambiense* from *T. brucei* is by the human serum test. This serum acts much more powerfully on *T. brucei* than on *T. gambiense*, and by suitable means this difference can be shown.

Conclusion.—In the opinion of the Commission, *T. gambiense* and *T. brucei* are separate and distinct species of trypanosome and give rise to a different disease.

7. COMPARISON OF THE TWO DISEASES, "CONGO SLEEPING SICKNESS" OR "*Trypanosoma* *gambiense* DISEASE" AND "NYASALAND SLEEPING SICKNESS" OR "*Trypanosoma brucei* DISEASE."

A.—"Congo Sleeping Sickness" or "*Trypanosoma gambiense* Disease."

This disease has been known for more than 100 years on the West Coast of Africa, and on account of certain peculiarities it was looked on, not as a disease

of much practical importance, but as a pathological curiosity. The disease had long been endemic on the West Coast and had, as it were, assumed a condition of equilibrium. It was therefore not taken very seriously as a disease. But it was a subject of much speculation as to its causation, because it was noted that although it behaved like an infectious disease on the West Coast, it lost this power of infecting others as soon as the sick native was removed out of the sleeping sickness zone. For example, natives of West Africa introduced into other countries as slaves often died of sleeping sickness, and this sometimes after an interval of several years after leaving their homes. They lived, as was natural, in the closest contact with their fellow slaves, inhabiting the same hut, working side by side in the same field, but, strange to say, the disease never spread.

Various theories were held as to the causation of this mysterious disease; for example, that it was a food intoxication, or that it was caused by various kinds of bacteria, or that *Filaria perstans*—the embryo of a nematode found in the blood—was its cause, and so on. All these speculations were found to be erroneous when the discovery was made that the true causal agent was *Trypanosoma gambiense*.

Symptoms of the Congo Sleeping Sickness or "Trypanosoma gambiense Disease" in Man.—When the *Trypanosoma gambiense* gains entrance to the human organism it begins to multiply, and appears in the blood. How long it is before it appears in the general circulation is, in the case of man, as a rule, difficult to ascertain, but in the monkey it usually appears about seven days after inoculation. The course of the disease is so slow and insidious that months, and even years, may elapse before any marked signs manifest themselves. As an example of this, in Uganda, in March 1903, we had five natives in Entebbe under constant supervision. In January 1905, after a period of nearly two years, Captain Greig informed the writer that two of these men died of pneumonia in April and May 1904 respectively. Of the others, one appeared to be undoubtedly in an early stage of sleeping sickness; he gradually developed the characteristic signs of the malady, and trypanosomes were now always found in his cerebro-spinal fluid. The remaining two present some of the features of the disease, but are still able to do their work, and have not yet shown trypanosomes in the cerebro-spinal fluid. Again, in June 1903, 80 apparently healthy natives from the sleeping sickness area were examined and trypanosomes found in the blood of 23. Captain Greig reported after two years that four have died of undoubted sleeping sickness, two died from pneumonia, and five are now in an early stage of the disease. No information could be gained concerning six, and the remainder do not show any symptoms of sleeping sickness. Dr. Wiggins also relates the case of an "askari" or native policeman, stationed at Fort Ternan, in British East Africa, for two and a half years before he developed the disease.

All these facts go to show that the first stage of this disease, when the trypanosomes are found in the blood but not in the cerebro-spinal fluid, may be of a very variable duration. If one may be bold enough to put this into definite figures, it may be said that this so-called stage of trypanosoma fever, this first stage of human trypanosome disease, may last from three months to three years or more. During this time the native is going about in his ordinary way. He says he feels perfectly fit and strong. But there is one outward mark which proclaims the disease, and that is the presence of enlarged lymphatic glands. This is a disputed point, but in my opinion this enlargement of the lymphatic glands must be looked on as a constant and early feature of the disease. It is not that enlargement of the inguinal or femoral glands should be taken as a sign of human trypanosomiasis; there must be a polyadenitis or general enlargement, and, as a matter of routine, the post-cervical glands should be examined first. Every case of sleeping sickness examined by us in Uganda showed this glandular enlargement, and the trypanosomes are readily demonstrated on examining the gland juice. Later, it was found that the early cases of trypanosome disease, the so-called trypanosoma or Gambia fever, also in every

case presented enlargement of the lymphatic glands, and in them active trypanosomes could readily be found. The natives themselves are alive to the fact that, when the glands in the neck enlarge, they will, sooner or later, pass into the stage of sleeping sickness, and their custom is to anticipate the event by eating up their live stock, goats, chickens, &c. This enlargement of the cervical glands was used in Uganda to gauge the incidence of the disease in a sleeping sickness area. The result was that in about three-fourths of the population of the islands of Sesse and Kome this symptom was present. Whether there is any other symptom which can be depended upon to point to the first stage of this disease is a question which, in my opinion, must be answered in the negative. Some writers state that there is fever from time to time of an irregular type, but if the charts of the men we kept under observation in Uganda be examined it will be seen that they kept absolutely normal for several months. The first stage of this disease may be dismissed, then, by saying that the blood and lymphatics contain the trypanosomes, and that there is a general enlargement of the lymphatic glands of the body.

Second Stage—Sleeping Sickness.—Naturally, in a disease so insidious as this it is impossible to say with absolute accuracy when the first stage merges into the second. But a time comes when a slight change in the man's demeanour becomes evident; when he is less inclined to exert himself; he lies about more during the day, and at last his intimates see that he has the first symptoms of the disease. When these are well advanced the expression of the face is sad, heavy, dull-eyed, and apathetic. The body, however, is well nourished, and this is the rule, even up to the time of death, if the patients are well nursed and fed. Complaints of headache or indefinite pains in other parts of the body are often made. The pulse is rapid, shallow, and weak, and the heart sounds faint and distant. The breathing usually presents nothing abnormal. The lymphatic glands are generally enlarged, and vary from the size of a pea to that of a bean. There is nothing abnormal about the skin; it may be at times harsh and rough, but usually is smooth and sleek, and any eruption is quite the exception. The gait is weak, uncertain, and shuffling. There is little strength in the hand-grip and the hands when held out are tremulous. Tremors of the tongue are, as a rule, well marked. The voice is weak, indistinct, and monotonous. At this time the temperature is usually irregular, often normal in the morning and rising to 101° or 102° F. in the evenings. During this time the patients in hospital are usually up a great part of the day, sitting in the open air, and to the casual observer show little or no sign of disease. The symptoms gradually increase until after weeks or months the patient is unable to walk, speak, or feed himself. He is then altogether confined to his bed, lying in an absolutely lethargic condition all day long. It is at this time that the sick are often neglected by their friends and become much emaciated. The urine and faeces are passed involuntarily. During the last two or three weeks the temperature sinks six or seven degrees below the normal, and, the condition gradually deepening, the patient dies in a state of coma.

Diagnosis or Method of recognising Congo Sleeping Sickness.—In Uganda we usually depended on finding the trypanosomes in the blood, glands, or cerebro-spinal fluid of the patients.

It must not be supposed that a casual examination of the blood is all that is required to demonstrate this body. An ordinary blood preparation taken from the ear or finger would in all probability not show this parasite once in fifty times. As a rule the trypanosomes are so scanty in the peripheral blood that, in order to demonstrate them in every case, a quantity of blood must be examined. In Uganda we examined many cases of sleeping sickness and found the trypanosomes in the peripheral blood of all but one. At that time we had five workers and five microscopes. Ten cubic centimetres of blood were drawn off from a vein of the arm; this was run into test tubes containing a small quantity of citrate of soda solution in order to prevent coagulation; the blood was then centrifugalised for a

quarter of an hour, to throw out the red blood corpuscles; the clear liquid was pipetted off and again centrifugalised for a quarter of an hour. Specimens were taken from the surface of the red blood corpuscles and examined by the five workers. As soon as the moderately clear fluid had been subjected to the fifteen minutes centrifuging, it was poured off, leaving a small quantity of sediment behind. This sediment was again made into five specimens and examined microscopically. The clear supernatant fluid was for the third time centrifugalised and the resulting small quantity of sediment again examined. As a rule it was in the sediment obtained from the third process that we found the trypanosomes most frequently. It will be seen, then, that five microscopes were employed and frequently fifteen preparations would be examined before a parasite was discovered.

In the specimens made from the first two preparations there are too many red blood corpuscles to give a good view; it is only in the third, or perhaps not till the fourth, that the red blood corpuscles have been so completely removed as to leave a clear fluid only containing a few white blood corpuscles; in this fluid the active living trypanosome is readily picked out under a low power.

From the above description it will be seen that "Congo sleeping sickness" is a slow, chronic, insidious disease and may last for one year to four, five, or more. In this it is like another disease due to an animal parasite—syphilis—which may take 20 or 30 years to kill.

The trypanosomes are exceedingly scanty (92) in the blood, and are only found with the greatest difficulty. The affected native seldom or never suffers from oedema or swelling of the face or limbs.

Finally, no case of interstitial keratitis or opacity of the cornea in Congo sleeping sickness has been recorded.

B.—"Nyasaland Sleeping Sickness" or "Trypanosoma brucei Disease."

The first case of "Nyasaland sleeping sickness" was found in Nyasaland in October 1908. An examination of the natives on a large scale was at once begun, and some 60,000 natives were said to have been examined. No further cases were, however, found. In December 1908 Capt. Hardy, R.A.M.C., who had been one of the medical officers employed in this investigation, was found to have the disease. In 1910 a European named Roux (86), belonging to the Dutch Missionary Society, came into the district for a few days' shooting and contracted the disease. Since then some 128 cases have been reported in the "proclaimed area" and neighbouring districts.

At first this disease was supposed to be "Congo sleeping sickness" imported from Lake Tanganyika, and it is possible that one or two of the first cases were. But when the Commission arrived, and it was seen that the trypanosome causing the "Nyasaland sleeping sickness" was somewhat different from that causing "Congo sleeping sickness" (*T. gambiense*), and that the symptoms and course of the diseases were different, then it was recognised that we were dealing with the same disease as that occurring in North East Rhodesia and described by Stephens and Fantham under the name of *T. rhodesiense*.

This was in the case (88) of a man named Armstrong, who contracted the disease in North East Rhodesia in September 1909, and was sent sick to England.

After further work the Commission came to the conclusion that *T. rhodesiense* was in reality *T. brucei*, hence the name "*T. brucei* disease" given to it.

Symptoms of the Nyasaland Sleeping Sickness or Trypanosoma brucei Disease.—As the Nyasaland Commission has had no practical experience of the symptoms of this disease, it is necessary to take the description furnished by Drs. Sanderson (84) and Shiroore (85), the medical officers placed in charge of the sleeping sickness area.

Shircore divides the disease into three stages as follows. His description is given mostly in his own words:—

"Incubation Period and Course of the Disease."

"The incubation period, or the time which elapses after the native is infected by the bite of the fly and the appearance of the trypanosomes in his blood, is from 5 to 10 days, after which symptoms, usually severe, immediately supervene. In two cases after this initial period a remission of the severe symptoms lasting about a week to 15 days took place. Patients as a rule grow progressively worse. There are, roughly, three stages in the majority of cases, with symptoms much as these.

"1. Headache, pains in the chest and limbs, or abdominal pain and diarrhoea. There is no loss of intelligence or indication of any interference with mental functions. Delirium is, however, sometimes associated with the intensity of the initial pyrexia, which is remittent. There is an increase of pulse and respiration rates well above the normal, and the patient appears nervous. Trypanosomes few in number."

"2. With these symptoms a change in character takes place. Cerebration is impaired; asthenia is marked and increasing; oedema of feet and other oedemas; muscular wasting; tremors. The temperature is swinging and intermittent. Pulse and respiration rates continue high. The patient can still walk or sit about. Trypanosomes increased in numbers."

"3. Patient is now unable to sit up; he may still crawl. Marked mental change; seems oblivious of surroundings; mumbles, or is unable to talk; drowsy or comatose. Temperature either still intermittent for a few days or absolutely sub-normal for the twenty-four hours. Pulse and respiration rates sub-normal and diminishing in frequency. Trypanosomes numerous. Death."

"The duration of these stages is approximately three weeks to a month for the first, up to four or even five months for the second, a few days to four weeks for the third. An average duration for the whole course is about 4½ months. Six months has been found to be exceptional. In the young the course is very rapid.

"The trypanosomes, from their first appearance in the blood shortly after infection, are always present, and markedly so during the last month. Six per field, or even eight or more, are quite usual till death."

"In the average cases the points to observe in diagnosis are the expression and puffiness of the face; mental condition; tremors; emaciation; enlargement of epitrochlear glands. The glands in the posterior triangles vary in size or may be absent. Increased frequency of pulse and respiration rates. The temperature depends on the time of day when examination takes place, but if markedly sub-normal in the morning indicates a fairly advanced stage, and two or three months' duration; as also any decided change in the mental condition. Speech thick, sometimes the voice is high-pitched, at other times hoarse. Skin harsh, dry, and scabby. Oedemas usually of feet. Gait shuffling, short-stepped; the feet just clear the ground as if the patient were pushing something along with them. Interstitial keratitis or opacity of the cornea occurs in some 17 per cent. of cases. The above, combined with a history of headache, pain in the chest or legs, sometimes abdominal pain and diarrhoea or vomiting, as features marking the onset are almost pathognomonic. The headache, as once described, was a left-sided hemiania with pains shooting backwards to the occiput and forwards to the left eyeball."

"To differentiate this from the acute form of the Uganda disease (Congo sleeping sickness)

"one may say that there is nearly always a very definite and usually severe onset in our cases, which is far from insidious. The diagnostic value of the glands in the posterior cervical triangles is practically nil as regards palpation. A large percentage of cases show interstitial keratitis, and, further, in 27 cases examined (fresh cover slip preparations) the trypanosomes have always been found in the peripheral blood whenever sought for. They are invariably few in the early stages, one or two in the whole preparation; later they are numerous, several to a field. It has never once been necessary to examine a second fresh preparation of a known case in order to find the organisms. This cannot be said for the Uganda fever."

"This disease is invariably fatal in a few months. The first well-defined diminution in the frequency of the pulse-rate indicates that the case is on its downward course, and that vital resistance has been overcome. The state of the pulse is the main feature in prognosis."

From Dr. Shircore's description of Nyasaland sleeping sickness it will be readily seen that it differs greatly from Congo sleeping sickness. The following comparison in parallel columns will assist in comprehending this:—

Symptoms.	Congo Sleeping Sickness.	Nyasaland Sleeping Sickness.
Duration	One to five or more years.	4½ months, average.
Trypanosomes in blood.	Extremely difficult to find.	Always present; often numerous.
Oedemas, or swellings	Absent or very rare.	Invariably present.
*Opacity of cornea	Never noted	Occurs in some 17 per cent.
Prognosis	May recover	Always fatal.

* NOTE.—A common symptom in animals suffering from Nagana, or "*Trypanosoma brucei* disease."

Conclusion.—The disease "Nyasaland sleeping sickness" belongs to the same family of diseases, the sleeping sicknesses, but is specifically different from "Congo sleeping sickness."

SECOND TERM OF REFERENCE TO COMMITTEE.

Whether it is necessary and feasible to carry out an experiment of game destruction in a localised area in order to gain further knowledge of 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.

8. IS IT NECESSARY AND FEASIBLE TO CARRY OUT A FENCING SCHEME?

From what has been written in the previous part of this summary of evidence, it must be admitted by every member of the Committee that one fact stands out clearly and unmistakably, without danger of contradiction, and that is, that the wild game in the fly area are a poisonous and dangerous crowd. There may be a difference of opinion as to whether they carry *Trypanosoma rhodesiense* as well as *Trypanosoma brucei*, but if the contention is right that *Trypanosoma brucei* and *Trypanosoma rhodesiense* are one and the same, then the wild game in the fly country are a grave danger to human life.

As to their deadly effect on the domestic animals, they carry in their armoury, not a single weapon, but a whole arsenal, so that if one kind of weapon fails to kill, another will succeed. Even the domestic pig, an animal immune to most diseases, does not escape.

One would think, therefore, that the time had come to attempt to advance our knowledge on this important subject by carrying out an experiment of game destruction in a localised area. That it is urgent and necessary there can be little difference of opinion.

As to whether it is feasible, that is a matter of opinion, and it is scarcely a question which the Commission can be expected to answer. It is a matter to be decided more by the practical Administrator and Director of Public Works than by the Scientific Commission.

9. WHAT MIGHT BE LEARNT FROM AN EXPERIMENT OF THIS KIND.

1. If 100 or 200 square miles of fly country were fenced in, and the big game driven out or shot out, it might be found that the tsetse fly disappeared with the game, and that the locality became healthy for man and cattle.

2. It might, on the other hand, happen that the flies did not disappear, but remained in their old haunts, and that they ceased after a while to be infected with disease-producing parasites. This would mean that they had some mode or means of subsistence other than the infected wild game: small mammals, birds, lizards, &c. In this case the district would also become healthy.

The percentage of infected flies in the proclaimed area has been worked out for 1912 and 1913. This would form a standard for comparison.

3. Again, it might fall out that the fly neither disappeared nor became non-infective. This would probably be due to the fact that the tsetse fly, when hungry, ranged some miles from its home, flew outside the fenced-in area, found big game, had its feed, and returned to its old quarters. As this fly is strong on the wing, and can possibly fly a mile in a minute—it certainly overtakes with ease a motor cycle going at 30 miles an hour—this would be no great feat.

In this case the district would remain unhealthy, and dogs and goats exposed to the "fly" would become infected.

In my opinion, it is probable that the first of these, namely, the disappearance of the "fly," and the consequent healthiness of the district, would be brought about.

4. In the case of the third event happening—the flies not disappearing and not becoming non-infective—then the further experiment of clearing away the scrub might be tried. This would certainly result in the wished-for success. The cutting down of the scrub would be an easy matter. The thorn trees of which it is composed are scattered, average only about 6 inches in diameter, and when cut down and allowed to dry for a few months during the dry season would readily burn and disappear on the first burning of the grass.

If this cleared area lay for, say, 5 miles on each side of an important road, such as the Domira Bay road from Lake Nyasa to Fort Jameson, it would probably render the road safe for man and beast. When the railway reaches the southern end of the lake this road should regain its old importance.

10. DIFFERENT SCHEMES WHICH MAY BE PUT FORWARD.

1. It might be proposed to take the whole proclaimed area and try to kill off most of the game in it. If this was placed in the hands of a few professional boer hunters much would be done in a short time. The hunters would receive a certain reward for each head and skin. In the north of Natal these same hunters rapidly cleared the country of its teeming herds of game. They received something like 3s. 6d. for each skin. Accompanied by a number of natives, the hunters shot down the game by the score, the natives rapidly stripped the pelt, and the body was left to be eaten by the vultures.

Lately in Southern Rhodesia, a herd of 120 elephants was doing damage, and it was decided to exterminate them. A few boer hunters were employed, offered some reward, and in a week every elephant in the herd was dead.

But the objection to this scheme is that as long as there was plenty of game the hunters would kill, but as soon as it grew scarce their receipts would dwindle to little or nothing, and then they would go. In a

short time, as the area is unfenced, the game would creep in from neighbouring districts and the *status quo ante* would be re-established.

2. An area might be chosen in the middle of the "fly country" which was noted for the abundance of tsetse flies and wild game, and 100 square miles fenced in. This would be a definite square with sides 10 miles long. It would probably be necessary to choose an area free from villages, or to remove the natives out of the area.

The most fertile 100 square miles along the lake shore should be chosen, in order that if the experiment was a success the land could be utilised to the best advantage. It must be remembered that in a short time the railway will have reached the south end of the lake, and all the fertile land on the west shore of the lake will be open to settlers.

3. The Commission's scheme, as proposed to the Nyasaland Government, was to enclose a strip of country on each side of the main road to Domira Bay. This strip would be some 20 miles long and 10 miles broad. The lake end would not require to be fenced. If this main road was made safe for transport it would be a great boon to the districts lying to the west of the lake. A cattle trade would spring up when the railway reached Fort Johnston, and it would open up large tracts of fertile country to settlers. At present it is impossible to take an animal along the 20 miles of this road, so infested is it with the fly, without losing the ox, goat, or dog. On June 9th, when I last came through, I was severely bitten, although every precaution against this was taken.

11. PRACTICAL CONSIDERATIONS REGARDING A FENCING SCHEME.

At the beginning of 1912 I proposed to the Nyasaland Government that a rough experiment should be undertaken. The area chosen was that through which the Domira Bay road runs. A wooden fence was to be run from the foot hills to the lake shore at a distance of three or five miles north and south of the road. There would be no necessity to fence the lake shore end. The area enclosed would be about 200 square miles. The fence would be made of logs from that kind of tree which, when placed in the ground, sprouts, so that in a short time the area would be surrounded by a living hedge. This would protect the standards from white ants. As many of the living trees as stood in the line of the fence would be left standing. Barbed wire would be run between these living stumps, or, if wire was not procurable, then strong bars of wood formed from the branches of the trees felled in the process of clearing the space for the fence. These bars would be nailed to the living standards, or, if nails were not procurable, by wooden pegs, as used by the native carpenters in making bridges. The ground on each side of the fence would be cleared, hoed, and planted with sweet potatoes to a distance of, say, 20 yards on each side of the fence; this to prevent the fence being burnt down when the grass is burnt, and also to prevent the grass and scrub springing up again on the cleared space. Incidentally the potatoes would provide food for the natives employed to keep the fence in repair. To keep elephants from breaking down the fence a narrow ditch might be dug along the outside of the fence and covered with branches and grass, as it is a well-known fact that these animals will not cross over such a concealed ditch, as they suspect a game pit. The natives in the sleeping-sickness area, some 20,000 in number, are not allowed to leave the area, and therefore relief works must be found in order to feed them, and in order that they may earn their hut tax. 2,000 of these people could quite well carry out the scheme during one dry season.

When the fence was built huts would be built round it at a distance of, say, a quarter of a mile. In each hut two natives would be stationed. This would mean that there were 400 native sentries constantly on watch round the fence. Their duty would be to keep the fence in repair; to see that the grass or scrub did not approach so near to the fence as to be a danger from fire; to report if any herds of wild animals had broken through the fence; or on the approach of such wild animals to try to frighten them

away. Firearms might be provided at the rate of one to each hut.

In my opinion, therefore, the time has come for an experiment of this sort, and the sooner it is begun the better. The map of Nyasaland, for about a quarter of its area, is marked red, showing the distribution of *Glossina morsitans*. It is now that an attempt should be made to render the 5,000 square miles of fly country in Nyasaland free of tsetse, and this can only be done by depriving them of their main food supply—the big game living in the fly country.

12. THE LOCALITY, PROBABLE COST, AND OTHER DETAILS OF A FENCING EXPERIMENT.

If I were now asked to choose one out of these various schemes, I think I would give preference to the 100 square mile block surrounded by a wire fence with steel standards, to be situated in the proclaimed district, and taking in part of the Domira Bay road, or at least in that locality. The cost of this, with upkeep for five years—according to Mr. Binnie, the Director of the Public Works Department—would be 37,907l.

The next scheme I would choose—supposing the first was impracticable on account of its cost—would be to enclose the Domira Bay road by a post and rail fence. This would be 20 miles long by 10 miles broad. The cost, at 15l. a mile, would be 750l., clearing the tract 500l., and upkeep 225l. = 1,475l.

THIRD TERM OF REFERENCE TO COMMITTEE.

WHETHER IT IS ADVISABLE TO ATTEMPT THE EXTERMINATION OF WILD ANIMALS, EITHER GENERALLY OR LOCALLY, WITH A VIEW OF CHECKING TRYPANOSOME DISEASES IN MAN AND STOCK.

In an article in *Nature* (12) written in 1905 I wrote "civilisation and big game cannot exist together." As soon as a new country is divided into farms, either for agricultural or stock purposes, the great mass of wild animals must go. Take, for example, the destruction of the fences by stampeding herds of zebra, wildebeeste, or buffalo, not to speak of the probability that there is not enough food to go round. Even in exceptional cases, when the wild animal has been protected from sentimental and picturesque reasons, as in the case of the herd of hippopotami preserved until lately in Natal, a time came when the neighbouring farmers could no longer put up with their destructive habits, and they had to be destroyed. We may say, then, that when a country becomes settled and civilised the big game goes. This has occurred in Cape Colony, the Orange Free State, Transvaal, and Natal, and will occur in Nyasaland when the country is opened up.

But this inevitable disappearance of wild animals before the advance of civilisation is very different from the instant carrying into effect of an international measure for the wholesale destruction of big game all over Africa. Such a measure in the present state of our knowledge would be quite unjustifiable, and would probably fail to a great extent in its object. *Festina lente*. Let local authorities frame regulations from time to time as the exigencies of the place demand. But there ought to be room for the next thousand years in many parts of Africa for game reserves in which all the varieties of big game may live, thereby gladdening the eye and enriching the imaginations of many future generations, and delaying the day when man will have for his sole companions the horse, the cow, the dog, and the domestic hen.

That opinion, expressed in 1905, is very much the opinion I hold now. I wish an experiment to be tried in a restricted area to find out the effect on the tsetse fly of driving out the wild game. Beyond that I do not advise the instant indiscriminate destruction of the wild animals. The fly country in Nyasaland is still to a great extent unsettled and uncivilised. The natives in these wild places may be looked upon almost as part of the wild fauna. The mortality among them from Nyasaland sleeping sickness is insignificant and may be ignored. As in my opinion Nyasaland sleeping

sickness is caused by *Trypanosoma brucei*, then I expect within the next few years, when attention is directed to the question, that cases will be found wherever *Glossina morsitans* and wild game are found.

There is, therefore, no reason why these natives should have strict quarantine regulations imposed upon them which they do not observe. The disease is a short and severe one, and a native suffering from it will not be likely to travel far, if at all.

There does not seem to be any likelihood of Nyasaland sleeping sickness becoming epidemic and killing off large numbers of the population as Congo sleeping sickness has done. The cases in the Luangwa Valley and Nyasaland are not increasing year by year. I would therefore remove all irksome useless quarantine regulations, and let the "proclaimed area" revert to its original ways, the *status quo*.

But, without attempting at present a wholesale extermination of wild animals in Nyasaland, either locally or generally, I would advise a gradual weakening of the game laws.

There is a tendency, at present, in our protectorates to the indiscriminate passing of hard and fast game laws, which are often unwise and lead to undesirable conditions. For example, the hippopotami being protected on Victoria Nyanza led to their increasing to such an extent that at Jinja, near the Ripon Falls, nothing was safe from their depredations. Hippopotamus hunters from the Nile had to be called in to restore a better balance. Another instance has been given of the protection of wild pig in Busoga. The pigs increased to such numbers, and the lions along with them, that, what with the pigs eating up his crops and the lions eating him up, the poor native was almost exterminated.

Take the Angoniland district, in which the sleeping sickness area is situated. The senior magistrate says that there are 600 elephants and 30,000 eland in his district. The district is a populous one. It seems ludicrous that such a number of large useless animals should be allowed to roam at will through this thickly populated country, destroying the native crops and often the native himself.

I would therefore advise the carrying out of game preservation on more intelligent lines and not allow, even in a reserve, the excessive multiplication of wild animals unless the reserve was uninhabited and uninhabitable by man.

In "fly country" I would remove all restrictions and allow the natives to pursue and kill any of the ordinary wild animals without let or hindrance. After all they form, and have always formed, part, and an important part, of their food supply. They might use their own methods, such as game-pits or traps, and I would encourage in them the possession of a certain number of useful guns. At present they have no means of defending themselves. At Kasu we were constantly receiving complaints of damage done by elephants and buffalo, with requests to come and drive the animals away.

The wild game in the fly country are all infected, and the fewer of them about, the better for man and his domestic animals.

Conclusion.—It is advisable to attempt the extermination of wild animals in fly country with a view to checking trypanosome disease in man and stock.

FOURTH TERM OF REFERENCE.

Whether any other measures should be taken in order to obtain means of controlling these Diseases.

14. SEGREGATION OF INFECTED NATIVES.

Every infected native in the fly country is a source of danger to his neighbours. If the trypanosome causing disease in man in Nyasaland is in truth the common *Trypanosoma brucei* of the wild game, which under certain conditions can infect man, then *Trypanosoma brucei* taken by a fly from an infected man would be more likely to infect man than a *T. brucei* taken from a wild animal. It would have become acclimatised to the blood of man just as *T. gambiense* has become. Therefore, it would be well to move infected natives out of the fly country into fly-free,

This could be done without isolating him in a segregation camp. These camps are very unpopular, as no one who goes in ever comes out alive. The sick native might be sent to some neighbouring village out of the "fly," and attended to by his own relatives.

This, however, is not an important matter. The sick are not usually discovered until they are *in articulo mortis*, when removal would be impossible, and the sick are few and far between.

15. CLEARING OF FOREST AROUND VILLAGES.

This, I think, is useful from several points of view. It helps to keep the fly away from the village proper, so that if there is an infected native there, there will be less chance of his being bitten and the tsetse flies becoming infected from man. It lets in sunlight and air and keeps the wild animals at arm's length. For purposes of administration it would be well also to gather the natives together in fairly large villages rather than to allow them to live in single huts scattered through the bush.

16. PROPHYLAXIS AND TREATMENT BY MEANS OF DRUGS.

This with our present knowledge I consider useless.

17. DESTRUCTION OF THE TSETSE FLY.

In this Committee there will doubtless be heard a great deal about studying the bionomics of the tsetse fly, and I would not be surprised if the fencing scheme, changes in game laws, and destruction of infected animals in the fly country will all be shelved in order to give the entomologists another chance of finding out some method of fly destruction.

In my opinion the problem cannot be attacked with any chance of success from the side of the fly alone. The country is huge and mostly desert. The tsetse flies are numerous and deposit their pupae all over the jungle. The results of entomologists up to the present regarding the destruction of the fly have been very meagre. Carpenter has been working in Uganda for some years and his results in this direction are practically nil. Other entomologists of various nationalities have attacked the same problem, but with little success.

I have no objection to work being done on the side of the "fly." All knowledge is good and it ought to advance along every line. But I am strongly of opinion that this particular problem cannot be attacked from the side of the fly alone, but that it can be attacked successfully from the side of the wild game.

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Part III.—Written Statements by Gentlemen who did not subsequently give Oral Evidence.

Mr. A. CONNALL, M.D., Ch.B., D.P.H., D.T.M. and H., West African Medical Staff (Director of the Medical Research Institute, Lagos).

1. The statements made in these notes are based on seven months' experience in the Quittah District of the Gold Coast (May-November 1911), and on one year's work in the Accra Laboratory (May 1912-May 1913).

No cases of human trypanosomiasis were encountered in the Quittah District. A widespread

epidemic of smallpox necessitated extensive travelling and the examination of a very large number of people, so that there was ample opportunity for the detection of cases of sleeping sickness. "Game" is scanty in this district. The blood of numerous cows, sheep, goats, pigs, dogs, and rats was examined and trypanosomes were only found in *Mus rattus* (*T. lewisi*).

Large herds of cattle graze all along the lagoon side from Dance to Awunaga. The animals appeared to be well nourished and free from disease.

Only one tsetse-fly was caught during the seven months (*G. palpalis*, at Dsodje). Various species of *Tabanus*, *Haematopota*, *Hippobosca*, *Stomoxys*, and *Lyperosia* are common. At certain times of the year *Glossina* are found at Attitite and along the banks of the Volta River.

My experience in Accra was also confined to man and domestic animals. No opportunity of examining "game" was obtained, wild animals being very scarce in the vicinity.

Three cases of human trypanosomiasis, verified by the finding of the parasite, were encountered. The patient in each instance had previously resided in the Northern Territories of the Gold Coast, in which region the disease probably originated.

With regard to stock, a considerable percentage of cattle was found to be infected with trypanosomes. The actual figures are not at hand, but they will be found in the Annual Report of the Laboratory at Accra for 1912. Trypanosomiasis in sheep was observed rarely, and no cases were met with amongst pigs and goats. The meat market in Accra is supplied mainly from cattle which come from elsewhere, and graze for a variable period before slaughter in the outskirts of the town. They originally hail, for the most part, from the Addah District, and these are sturdy, of small height, and well nourished. A few animals come from the far north and they are of an entirely different breed, being tall, with a distinct hump between the shoulders, and very emaciated. They (the latter) come down to Accra through numerous "fly-belts." During 1911 the Addah cattle were found to be entirely free from blood-parasites, while trypanosomes were found in practically all the animals of the northern breed (Dr. J. M. O'Brien, Accra Laboratory Report, 1911). During 1912 the Addah breed was discovered to be widely infected. It seems clear that the latter obtained the infection of trypanosomes from the former during 1911.

The town and precincts of Accra may be said to be practically free from tsetse-fly. During the year under review less than a dozen specimens were captured, all *G. palpalis* except one *G. fusca*. Only one of these flies contained ingested blood, and none was found to be infected with trypanosomes on dissection.

The tsetse is first met with about 10 miles north on the railway line, beyond Dome. At Oblogo and Weshiang, on the waterworks line, they are very plentiful, and there is also a fly-belt near Dodowah, about 20 miles from Accra. *Glossina morsitans* preponderates in these districts. The few flies which were captured actually in Accra were most probably carried down on the swiftly-moving trains and motor-lorries. The

cattle graze in the immediate outskirts of the town, so that they may be regarded as being seldom molested by tsetse. The prevalent blood-sucking insects are *Tabanus*, *Stomoxys*, *Lyperosia*, ticks, and mosquitos. Leeches occur in the ponds which are used for watering the cattle. If *Glossina* be the sole insect-carriers of trypanosomiasis of stock, then a very few flies can cause a widespread infection. On the other hand, if mechanical transmission be admitted, there is abundant scope for the process in the number of other biting insects mentioned.

With regard to the trypanosomes found in the cattle, no conclusion as to species was arrived at, but it seems clear that there were several types. One species, obtained from a horse, was successfully inoculated into, and caused rapid death in, the goat, the sheep, the dog, the cat, and the guinea-pig.

It should be added that there was a considerable death-rate, unique in its large extent, amongst the horses in Accra during June, July, and August 1912.

2. The experiment of game destruction in a localised area appears to be both feasible and necessary in order to gain further knowledge. The destruction would, of course, be not wanton, but would be scientifically conducted, investigated, and recorded.

3. The advisability of attempting general or local extermination of wild animals, with a view to checking the trypanosome diseases of man and stock, would depend upon the results obtained from the procedure proposed in paragraph 2. More data are required before such a sweeping preventive measure be adopted.

4. A systematic and regular examination of all lunatics would probably lead to the detection of more cases of human trypanosomiasis.

Probably many of the game-hunters in Africa take blood-smears from the animals they shoot, and steps could be taken to make this practice more general. Most of the Government officials are familiar with the method of making a smear, and most of them are also keen sportsmen, so that a large amount of useful material could be collected by their agency. The response from all classes of officials and others to the appeals of the Entomological Research Committee has proved to be generous, and there would be an equally satisfactory response were blood-films, tsetse-flies, and field-notes asked for by the present Commission.

Where possible, new trade routes, railway tracks, and roads should make a detour to avoid fly-belts. Some of the existing lines of communication might with advantage be diverted past areas where the tsetse abounds.

It would be useful to find out if tsetse will leave a belt when deprived of food, as would occur were wild animals destroyed in a local area, and if they will merely die out or seek another area.

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Term of Reference No. 1.

I append a brief epitome of the present knowledge available, along with references to the papers in which the main facts have been made public:—

1. The question of the parts played by wild animals and tsetse-flies in Africa in the maintenance and spread of trypanosome infections in man and stock was first brought into prominence by the work of the Sleeping Sickness Commission of the Royal Society, Uganda, 1908-10. The report and conclusions of the Commission (consisting of Colonel Sir David Bruce and Captains Hamerton and Bateman) are found in the *Proceedings of the Royal Society*, February 28th, 1911.

It had been previously shown that cattle may act as a reservoir of the virus of sleeping sickness (*Trypanosoma gambiense*).

The very suggestive new discovery that, though man and his domestic animals had been removed from the shore of Victoria Lake for about 2½ years, the tsetse-flies (*Glossina palpalis*) on the lake shore were still capable of infecting susceptible animals with the human trypanosome, led the Commission to investigate the possibility of the antelopes of the vicinity being susceptible to *T. gambiense* through the bites of the

tsetse-fly, and capable of transmitting the virus again to other tsetse-flies, thus establishing a vicious circle indefinitely without the presence of man.

Their experiments were performed under laboratory conditions, and justified the conclusions, which in the main were as follows:—

- Certain common species of antelope can readily be infected with a human strain of the trypanosome of sleeping sickness by the bites of infected *Glossina palpalis*, and can transmit the infection again to clean uninfected tsetse-flies of the same species, which in turn can convey the infection to susceptible animals.
- An appreciable percentage (about 7 per cent.) of *G. palpalis* thus used in experiment will become infected with the virus of sleeping sickness when fed on antelopes bearing this virus in the blood.
- Antelopes infected with *T. gambiense* live in apparently perfect health, and microscopic examination of the blood fails to reveal the presence of the parasite (infection being proved by feeding healthy flies on the antelope's blood, and after an interval feeding the same flies on healthy animals, which after a few days show *T. gambiense* in the blood).

d. Antelopes infected with *T. gambiense* can transmit the infection to healthy flies during at least several months, while themselves apparently healthy.

The inference from these conclusions is that "antelope living in the fly-areas are 'potential' reservoirs of the virus of sleeping sickness."

It remained to be shown whether antelope living in the fly area would be found naturally infected with *T. gambiense*. The Commission left the country before this link in the chain of evidence could be completed.

II. The Luangwa Sleeping Sickness Commission (consisting of Dr. Allan Kinghorn, Dr. Warrington Yorke, and Mr. Llewellyn Lloyd) was appointed early in 1911 by the British South Africa Company to inquire into the subject of trypanosomiasis, and especially to ascertain the transmitting agent in those parts of N.E. Rhodesia where *Glossina palpalis* did not exist.

The Commission proved conclusively that in that region *Glossina morsitans* is the vector of the virus of the form of human trypanosomiasis met with in south Central Africa, and named *T. rhodesiense*.

In regard to the question of the association of wild animals with the human disease, their conclusions (published first in the *Annals of Tropical Medicine and Parasitology*, March 1912, Vol. VI., No. 1 A., and *Ditto*, Vol. VI., No. 2, and complete report in *Ditto*, Vol. VII., No. 2) were that a considerable percentage (16 per cent.) of local game in the Luangwa Valley have been found infected with *T. rhodesiense* in nature, more particularly the various kinds of antelope (waterbuck, hartebeeste, mpala, as well as wart-hog), and that these wild animals are, therefore, acting as reservoirs of this human trypanosome.

They proved that *G. morsitans* caught in nature can transmit this virus to monkeys and other mammals, and, therefore, we must conclude, also to man in that region.

Their experiments showed that about 5 per cent. of the tsetse-flies used in experiment may become permanently affected and capable of transmitting the virus each time they suck blood throughout their life.

The proved facts concerning human trypanosomiasis in N.E. Rhodesia must now be held to be equally true of Nyasaland, where the disease is shown to be the same. The work of this Commission gives thus the earliest positive demonstration that human trypanosomes occur in wild animals under natural conditions.

III. In a paper by Dr. H. L. Duke, read before the Royal Society (March 28th, 1912, *Proceedings, Royal Society*, 1912, B. LXXXV., No. B. 577, pp. 156-169), it was stated that 3½ years after the coast-line of Chagwe, Lake Victoria, Uganda, was officially declared free of population, the tsetse-flies on the lake shore were still capable of infecting monkeys with *T. gambiense*. Experiments were carried out on Damba Island, and the local antelope, the situtunga, was incriminated as acting as a reservoir to *T. gambiense*, and thus maintaining the prolonged infection of *G. palpalis* on that island, although it had been evacuated by human beings since September 1909. On the mainland shore the continued infectivity of the tsetse-fly was similarly explained, including the other species of antelopes already experimentally proved to be a potential reservoir by Sir David Bruce's Commission, viz., waterbuck, bushbuck, and reedbuck.

It was at the same time concluded that the infectivity of the lake-shore tsetse to other species of trypanosomes (*T. vivax* and *T. uniforme*) not pathogenic to man was also due to the antelope reservoir.

Dr. Duke, in later experiments on the lake shore, showed that antelope (reedbuck, waterbuck, and bushbuck) may remain capable of infecting *G. palpalis* with *T. gambiense* for a period of at least 22 months after their original infection. (*Proceedings of the Royal Society*, 1912, July 25th, Series B. LXXXV., No. B. 579, pp. 299-311.)

If the evidence from these two different localities is comparable, it appears that human trypanosomes in the blood of antelopes in a natural state are much rarer in Uganda than in N.E. Rhodesia. Duke examined 21 antelopes on the mainland shore of Lake Victoria

without finding *T. gambiense*, while Kinghorn and Yorke in Rhodesia found *T. rhodesiense* in 7 out of 41 antelopes, or over 16 per cent.

Other wild warm-blooded animals do not appear to be of equal importance; e.g., monkeys and wild rats examined in large numbers in the Luangwa Valley were not found to harbour the *T. rhodesiense*.

T. gambiense has only very rarely been found in a monkey naturally infected (once by the German Commission of Koch, Beck, and Kleine, and once by the Royal Society's Commission under Sir David Bruce).

Turning to the proportion of tsetse-flies found in nature to be infected with human trypanosomes, one may mention the following:—

Kinghorn and Yorke found in freshly caught wild *G. morsitans* in the Luangwa Valley the ratio of flies infected in nature with *T. rhodesiense* to be 15 per cent., as compared with 3.5 per cent. amongst the laboratory-bred flies which were fed on infected animals.

In Uganda, on the shore of Lake Victoria, the ratio of *G. palpalis* found in a state of nature to be infected with *T. gambiense* was calculated to be 0.14 per cent. 3½ years after the natives had been removed. Laboratory-bred flies used in the same locality by Bruce, Hamerton, and Bateman showed a ratio of 7 per cent. infected.

As regards other trypanosomes not pathogenic to man but pathogenic to domestic stock, several species have been found in wild animals in the natural state. Some examples may be given:—

T. brucei.—The virus of nagana, a disease of domestic stock in South Africa, has been found by Sir David Bruce himself in Zululand in the kudu, buffalo, bushbuck, wildebeeste, hyena, reedbuck, and steinbuck. ("Further Report on Tsetse-Fly Disease in Zululand.") It has since been found in other parts of Africa in these and other common antelopes, though apparently it was not found by the Luangwa Valley investigators.

T. vivax, another pathogenic species, has been found in the situtunga, and in bushbuck on the shore of Lake Victoria in Uganda, and in waterbuck and other species in the Luangwa Valley.

T. pecorum was found in several different species of antelope in the Luangwa Valley, in as many as 7.9 per cent. of the game examined, and *T. nanum* was similarly found in bushbuck and waterbuck.

T. dimorphon has been found in West Africa in wild animals.

Of a similar significance is the fact that *G. morsitans* caught wild have been found to be infected by trypanosomes pathogenic to domestic stock.

The French observers in French West Africa found wild tsetse of no less than three species (*G. morsitans*, *G. tachinoides*, and *G. longipalpis*) infected with and capable of transmitting *T. dimorphon*, the parasite of West African horse sickness (Bonet et Roubaud in *Bull. de la Soc. de Path. Exot.* 1912, Mars V. No. 3, pp. 204-211); and another investigator believed that *T. cazalboui*, a virulent parasite of cattle, was similarly spread by *G. palpalis* and *G. tachinoides* (Bouffard, in *loc. cit.* 1912, Juin. V., No. 6, pp. 380-385).

Kinghorn and Yorke in the Luangwa Valley found freshly caught *G. morsitans* to convey *T. pecorum* to clean monkeys by feeding the flies on the latter, just as the same fly was shown in nature to transmit *T. rhodesiense*.

It is probable that *T. vivax* and *T. nanum* are similarly transmitted in nature from wild to domestic animals. (*T. vivax* and *T. cazalboui* are possibly identical.)

G. morsitans has also been proved in Nyasaland to be in nature heavily infected with a more recently discovered trypanosome—*T. simia*, which causes disease in goats.

It may be accepted that big game act as a reservoir for trypanosomes pathogenic to domestic animals to a much wider extent than for those of the human disease alone. In the Luangwa Valley the proportion of game animals infected in the natural state with trypanosomes pathogenic to man and cattle inclusive was estimated at not less than 50 per cent., while at Ngao, on the Congo-Zambesi watershed (where some experiments were carried out), the proportion is stated as 35 per cent.

The above proved facts appear to justify the apprehension that the existence of big game which harbour trypanosomes pathogenic to man and domestic animals is calculated to maintain, and under certain circumstances to lead to serious extensions of, sleeping sickness and trypanosome diseases of stock.

One may mention as perhaps an extenuating factor that, although the proportion of game harbouring pathogenic trypanosomes seems alarming, the proportion of tsetse-flies actually found in nature to be infected and capable of passing on the infection is comparatively small—15 per cent. of *G. morsitans* infected with *T. rhodesiense* in south Central Africa, and 0.14 per cent. (or between 0.03 and 0.34) of *G. palpalis* with *T. gambiense* in Uganda, and the suggestion is made that a large proportion of the flies in nature are immune.

French observers in West Africa did not succeed in proving the transmission of *T. gambiense* through wild *G. palpalis* as was demonstrated in Uganda, and it is thus possible that the proportion of wild flies capable of becoming infected with the human trypanosome is in West Africa much less than in Uganda, &c. ("Trypanosomiasis et Glossines de la Haute Gambie et de la Casamance," Bouet et Roubaud. Bull. de la Soc. de Path. Exot., 1912, Mars. V., No. 3, pp. 204-211.)

Term of Reference, No. 2.

Since it is definitely proved that wild animals afford a reservoir of the sleeping sickness virus, it seems theoretically plain that places where such animals and the requisite species of tsetse-fly exist together will be uninhabitable by man until the vicious circle is broken by the extermination of either the fly or the parasite-bearing animals.

That the fly would disappear on the destruction of game has been assumed by some, but this result is perhaps little more than conjecture.

One has to remember that the natural food of the tsetse-fly appears to be mammalian blood (although under certain circumstances they appear also to prey upon birds and crocodiles, &c.). Given the extermination of wild animals the flies would probably be driven to find means of subsistence in domestic animals and man on the one hand, or in birds and reptiles on the other. (Koch went the length of incriminating the crocodile as an important natural reservoir of trypanosomes.)

It may be taken as proved, however, that either mammalian or avian blood (probably more usually the former) is necessary for the female tsetse-fly for reproduction, and that breeding does not occur when fed on reptilian blood alone.

When, therefore, cases of sleeping sickness or trypanosomiasis of stock already exist, it is conceivable that by destruction of game, a closer intimacy of the fly with human activities occurring, the result might sooner or later be a serious extension of these diseases.

It is possible on the other hand that the absence of wild animals on a large scale might, by depriving the female *Glossina* of blood which is their usual pabulum for reproduction, result in a great diminution in the number of these insects.

Exact experiments of game destruction in localised areas are necessary in order to gain further knowledge.

It appears to me that several experiments are required, each in a strictly localised area where the conditions are mutually different:—

- (1) An experiment carried out on one of the islands of Lake Victoria from which human inhabitants have already been removed, and in which *G. palpalis* and antelopes are in intimate and frequent contact. If tsetse-flies still continued to live and breed, and if their pupae could still be found locally after extermination of the game, evidence would be afforded that they are comparatively independent of the larger wild animals as a means of subsistence. Dissection of the stomach of the flies would reveal whether they were having resort to avian or reptilian or other blood. This might suitably be combined with a coincident effort on the lake-shore or on a selected mainland area where game and *G. palpalis*, though abundant, are more sparsely distributed.

I suggest Uganda for experiment (1) in a *G. palpalis* area, mainly because the important Commissions which have worked in Central Africa have accumulated a vast amount of accurate data, statistical and otherwise, which form an excellent basis for comparative reference in future reports, such as probably exists in no other part of the continent.

- (2) Another experiment might be conducted in West Africa, for the reason that there the history and progress of sleeping sickness has been different from its course in Uganda, the disease has not (at least within the period during which it has been studied) shown the same virulence or epidemic extensions, the proportion of naturally infected tsetse-flies appears to be less, and in various ways the relationship of the different factors—fly, animals, and man—seems to differ from that in Uganda. An experiment in West Africa for comparison with Uganda is, I think, justifiable, and may be expected to yield results suggestive for future prophylactic measures in the different regions.

- (3) Another experiment might usefully be carried out in a suitably chosen area in Nyasaland or N.E. Rhodesia, where *G. morsitans* is the vector both of the human trypanosome and of those of domestic stock.

The relations of *G. morsitans* with certain varieties of wild game have long been supposed to be peculiarly intimate, and in South Africa the reduction in numbers or banishment of the buffalo has been believed to be accompanied by the diminution or absence of this tsetse-fly.

I believe that if a locality could be found in which measures might result in an actual extermination of the game, and not merely their being driven farther afield, an excellent opportunity would be thus afforded of observing both the effect on the numbers of the tsetse-fly and the effect on the incidence of trypanosomiasis in man and domestic animals and comparing it with the conditions existing previously, provided these had been accurately studied in the first place.

Such experiments seem quite feasible, and, provided the actual or approximate extermination of the game over the area in question were assured, the main facts desired could probably be ascertained within two years.

The age to which tsetse-flies live in nature is not known. Kleine states the longest survival of laboratory-bred flies to be 227 days, and there is no evidence that hereditary transmission of trypanosomes from an infected fly to its offspring ever occurs. Thus, from a period of, let us say, twelve months after extirpation of the game, material for the study of the question as it affects the tsetse-fly would be to hand, and, in the event of its continued presence, further bionomic questions would suggest themselves, and, perhaps, find solution. At the same time any fresh cases of human or animal trypanosomiasis occurring after this period would be specially studied and their origin traced, and, if possible, accounted for.

The cost and other details would have to be determined by the local administration.

Term of Reference, No. 3.

The objections to attempting a general extermination of wild animals in Africa with a view to checking trypanosome diseases of man and domestic stock are the practical difficulties and lack of certainty that the measure, though effectively carried out, would secure the desired result.

The practical difficulties are obvious to anyone acquainted with the topography of tropical Africa. Doubtless the larger game are doomed eventually, and active measures specially directed against horned ruminants would hasten their disappearance, but our present aim is to check effectively the ravages of trypanosomiasis in the space of a very few years. If this result is dependent in the main on the complete extermination of antelopes, &c., I fear that the present generation will not live to see the curse lifted. The wild animals of Africa are in no sense dependent on man, and there are vast areas intervening between the populated districts in which they already abound,

or which are suitable for them should they migrate thither for safety.

The first effect of efforts at the local extermination of game around inhabited spots will be to drive the animals farther afield where they may live and multiply for years to come. This local reduction or banishment of game is, however, a hopeful measure in that, if tolerably complete in each neighbourhood, it may be expected to remove or to keep at bay one source of the virus. It is also by no means difficult to accomplish if undertaken seriously with the definite object in view to kill or drive permanently away from the neighbourhood of man.

Would the local destruction of game result in a diminished incidence of trypanosome diseases in the community concerned? Proof is required and therefore some local experiments are desirable.

The tsetse-fly naturally follows its means of subsistence, and though with the game the fly may be driven away, it is possible that it may in numbers be forced into closer intimacy with domestic animals, which might thus in greater proportion become the natural reservoir at present afforded chiefly by wild animals. It is already known that cattle can and do serve as a reservoir of pathogenic trypanosomes in the same way as antelopes. Sheep and goats are also susceptible to most known trypanosomes, suffering but mildly and betraying but few symptoms. It is reasonable, therefore, to expect that cattle—not to mention sheep, goats, horses, dogs, &c.—might in this way become increasingly infected, and man might share the same fate.

I am of opinion, however, that this danger can be more than met by directing increased attention to local destruction of tsetse-flies and their haunts in conjunction with local destruction of game.

In a part of the Munchi Division of Muri Province, Northern Nigeria, in the angle between the Katsina River and the south bank of the Benue River, the Munchi tribe have not exterminated indeed, but greatly reduced the number of, wild animals. Tsetse-flies abound, however, and in certain localities sleeping sickness is endemic; humped cattle (known to be non-immune to trypanosomiasis) cannot be kept, while the more resistant and partially immune dwarf breed of cattle is common, and the horse is such a rarity that some villagers do not know its name! If one may draw any inference from this limited and unauthorised experiment, it might be to the effect that half-measures in game destruction are of little value unless accompanied by fly destruction, and that domestic stock may be capable of affording a vicarious reservoir of almost equal importance: one should rather say of greater and more dangerous importance, owing to its intimate relationship with man.

The destruction or banishment of game from the vicinity of human settlements is a practical and reasonable measure, but with the same limitations as the crusade against mosquitoes and tsetse-flies near dwellings. We may assume that on the whole the fly would tend to follow the remaining game farther afield, and the diligent application of local measures against the persistence or return of tsetse would keep the neighbourhood of human settlements free from the pest. It is otherwise when the effort to exterminate game is in question.

To carry out completely such a labour is not to be hoped for within any reasonable period, and if it could over some particular area be effected, the exact result is with our present knowledge by no means certain.

It is quite possible that the effect would be, in the absence of other supplementary measures at least, to bring the vicious cycle of invertebrate and vertebrate host nearer home, and to establish a reservoir of pathogenic trypanosomes amongst domestic animals.

Another possible result might in some regions be that migrating game would carry their trypanosomes into areas where neither man, nor his domestic stock, nor the local tsetse-flies are at present infected. Such an unrehearsed effect as an epidemic extension of sleeping sickness in an area hitherto free from this curse would be too deplorable to contemplate.

Term of Reference, No. 4.

In regard to other measures, it is first of all necessary that complete and precise knowledge of the circumstances of each locality should be acquired. This has perhaps been done very thoroughly in some parts of Africa and less completely in others. The local medical staff of a Colony or Protectorate is insufficient to carry out the necessary observations in the complete and exact manner necessary, and special workers are required, e.g., to prepare accurate local statistics and to watch carefully the progress of the diseases, to study the life-history and habits of the tsetse fly more fully, to supervise the clearing and other prophylactic measures, &c., &c. In fact, the appointment in each Colony of special medical officers devoted entirely for a period of years to trypanosomiasis investigation and control is urgently called for.

On the whole, the general distribution of human and animal trypanosomiasis and of tsetse-flies has been satisfactorily ascertained. In some cases the further measures will be administrative rather than medical, but incessant supervision by medical officers will be indispensable to the end.

A systematic plan for each district is necessary.

In some countries, or in some circumscribed areas, the major preventive measures required may be the subordinate measures of other localities and other circumstances.

In districts where serious extension of the disease to fresh areas is actually occurring or likely to occur, the major preventive measures must be segregation of the infected, removal of villages from fly-infected neighbourhoods, and control of native movements. Clearing and destruction of tsetse-flies will in these circumstances be the most important of the minor measures, since it is a matter of immediate urgency to check the spread of the disease and reduce it first to a sporadic condition, before larger efforts at complete eradication can be made effective.

The procedure will also be modified according as *G. palpalis* or *G. morsitans* is the carrier of the disease in man. In some *G. palpalis* areas certainly clearing will be the best plan, in some places usefully supplemented by digging of wells so as to avoid dangerous watering-places, while in others, where clearing cannot be made effective, removal of natives may be the more urgent measure, e.g., where the population is sparse and the cost and labour of attacking the fly-haunts is excessive.

In *G. morsitans* areas clearing would seem to be out of the question as a universal measure, and chief reliance will be placed upon removal of population to fly-free areas, with segregation of cases and control of native movements, along with a war against great game.

Previous to the discovery of the part played by mammals as a reservoir of the virus, one seemed justified in adopting prophylactic measures along two main lines, viz., extermination of tsetse-flies and removal of infected and exposed persons from fly areas. The former it is not always possible to carry out completely, while the latter, where practicable, is rarely altogether free from objections. A combination of both is known to have resulted in an enormous reduction of the mortality from sleeping sickness in Uganda in 1908-9, and may in many localities still be regarded as the chief means of controlling the disease.

On the other hand it is obvious that destruction or banishment of the fly is the higher aim in prevention, since it is radical, while removal of villages is palliative, for it is only when the fly has disappeared that all other restrictions can be relaxed. Even with the existence of a reservoir of wild animals, I believe that one must look more and more to destruction of tsetse flies and their breeding-places as the true prophylaxis in areas where *G. palpalis* is the important species.

Where human trypanosomiasis is caused by *T. rhodesiense* carried by *G. morsitans*, clearing as a general measure is probably impracticable. *G. morsitans* has a much more extensive and scattered distribution than *G. palpalis*, whose haunts are comparatively fixed, and can often with some likelihood be predicted where rank vegetation and evergreen foliage exist with perennial water. *G. morsitans* extends freely, and even seems

to live a nomadic life, over large areas of deciduous forest and even the more open savannah forest and bushland, often far from water, and as a rule only a partial diminution in numbers may be expected by clearing, except where circumstances are locally favourable to this measure. The wide occurrence of *G. morsitans* over vast inland areas inhabited by many species of antelope and other big game which are admitted to act as a "reservoir" for pathogenic trypanosomes, while it renders clearing measures impracticable, suggests that the proposed widespread destruction of game as a possible—though unproven—radical measure for combating trypanosomiasis may well be directed, in the meantime at least, in relation to this species in particular.

I am distinctly of opinion that *our energies must be directed chiefly against the invertebrate host of the trypanosome.*

In those countries especially where *G. palpalis* is the carrier of human trypanosomiasis, and they are the majority, I think that more attention should be given to persistent and maintained clearing, and even that this should be enforced. Enforced clearing is less objectionable than compulsory removal and segregation.

I believe that, as a broad principle, in the vicinity of human settlements, around village water supplies, at fords and riversides, along trade routes and through known "tsetse belts," more good is to be effected in a given period—say 10 years—by clearing and deforestation than by any other single measure (remembering, of course, that nowhere will any single measure suffice, a combination of preventive means being always necessary). To clear and maintain clear may be expensive to begin with, but it will probably prove cheapest as well as most effective in the end. It is true that it will generally involve some degree of compulsion on the native population, but it is unnecessary to believe that it represents a constant yearly fight with nature.

By "clearing," however, one may understand two very different forms of procedure.

- (1) The radical clearing, followed and maintained by cultivation of herbaceous crops, or by planting of certain grasses, such as dhub, citronella grass, &c. This is intended to be a permanent occupation of the soil in a manner incompatible with the requirements of the tsetse-fly. It is expensive at first, but of lasting utility and probably economical in the end. It involves felling, uprooting, grubbing up all vegetation of every sort and burning *in situ*. Tsetse-flies generally persist on such areas for a few weeks before finally disappearing (probably because adults already hatched remain for a time around their accustomed spots until they perish or find cover elsewhere, while the pupae in the ground do not have an opportunity to hatch out).

This radical clearing is specially suitable where there is a considerable population to carry it out; it has the further advantage that land possibly fertile is not abandoned, and the difficulties and dissatisfaction coincident with removal of natives are avoided.

- (2) A less radical clearing, consisting of cutting underwood, scrub, shrubs, &c., and burning on the spot when dry. Timber may be left. The soil is not exposed, and uprooting in general is not carried out. Such a cleared area soon reverts to its previous state, but a second cutting and burning involves much less labour, and I am of opinion that when this is carried out for not less than three successive seasons the area so treated ceases to be suitable to the tsetse-fly. This degree of clearing is feasible where tsetse-flies exist, but where sleeping sickness has not yet made its appearance; it should be effective, if performed over a sufficient width, along roads which it is required to render safe. By this method timber is not wasted, the expense is less, but there is no economic return in crops. The width necessary for clearing has to be ascertained locally, and depends to a large extent on the existence and distance of fly areas, and the

direction of connecting water-channels or traffic routes, &c. Generally 100 yards on either side of a road will be necessary, though in some places it will be less; in others a greater width of clearing will be required, e.g., on trade routes where *G. morsitans* is the culpable species.

It is also obvious that if one is justified in the belief that a third or fourth clearing remains permanently effective in banishing tsetse-flies, it becomes practically a radical measure preferable to removal of natives, and can, therefore, be applied in some instances where both flies and cases of sleeping sickness occur.

I think that even in villages situated within or near to belts of tropical jungle, if migration is not feasible, it is better to face the labour of clearing and maintaining it for several successive years. An ultimate width of 600 yards should in these cases be the aim to be gradually attained, part of which should be of the radical type and put under cultivation, while part might be temporarily at least of the lesser degree.

Half measures of clearing or clearings not effectively maintained must never be undertaken, as the last state may prove equally suitable for other species of tsetse fly. There are several species of *Glossina*, and we do not yet fully know to what extent one species may act as substitute for another in conveying the different forms of trypanosomiasis. *G. tachinoides* is a species which is content with less deep shade than *G. palpalis*, though its habitat is somewhat similar and has an even wider distribution. It has been shown to convey trypanosomes of animals, e.g., *T. caudaboui* (or *vieux*) on the Niger, in Dahomey, and in Uganda, and has often been suspected, though never actually proved, to convey sleeping sickness under natural conditions. The substitution of this ubiquitous and probably dangerous species for *G. palpalis* in ill-conducted clearings is to be guarded against.

We now know through the labours of the experts a vast amount about the trypanosome, perhaps as much as is necessary to guide us in prevention, but we are still lacking in precise knowledge of the natural history of the tsetse-fly. Further special work is required in each infested district to discover inimical conditions, natural enemies, and particularly its usual breeding places. On the shore of Victoria Nyanza, Marshall and Fraser found that *G. palpalis* pupates chiefly in sand, that not much shade was required, and many thousands of pupae might be found at once. The same conditions do not necessarily hold elsewhere, and there are many parts of West Africa at least where diligent search has so far failed to discover the breeding grounds, although the flies may be numerous.

The significance of the excessive preponderance of males over females at certain seasons in the experience of several observers has still to be adequately explained and may be of importance.

The extent to which other species of *Glossina*, e.g., *G. tachinoides* and *G. longipalpis* may play a vicarious part in spreading trypanosomiasis in place of the more commonly incriminated *G. palpalis*, and *G. morsitans* still requires demonstration.

I consider that in particular we still require special officers to investigate the bionomics of tsetse-flies, working for at least a complete year in each district. It seems reasonable to hope that, with more accurate knowledge of their life-history, the destruction of tsetse-flies in the pupal stage will become as much a practical measure against sleeping sickness as the destruction of mosquito larvae is against malaria, and may prove indeed the easier task.

A series of detailed regulations drawn up by the Principal Medical Officer or Senior Sanitary Officer in each Colony or Protectorate would serve a useful purpose. It is true that such a set of regulations, if complete, might appear a counsel of perfection, and in any Colony some regulations or others might come short of practicability, but this should not deter each from setting up a local standard, which might gradually be attained. It could hardly fail to demonstrate how widely the needs of each region differ, or how far the measures which can be carried out in one may be impracticable in another. The distribution of the disease and its virulence, the period during which it has been known to the inhabitants, its existence in a

chronic endemic or active epidemic state, the question of apparent increase or decrease of prevalence at the present time, the possibility of a certain degree of immunity in some native populations, the proportion of Europeans to the area under administration, the capacity or otherwise of the native to co-operate or to help himself, the topography of the country and

conditions of fly prevalence, the density of infected populations and of "game reservoirs," the existence of infected frontiers and possible immigration—are a few of the factors, which vary enormously in different parts of British Africa.

25th August 1913.

Mr. W. KIRBY GREEN, Second Grade Resident, Nyasaland.

The Commissions under Dr. Allan Kinghorn in Northern Rhodesia and Surgeon-General Sir David Bruce in Nyasaland are believed to have resulted in proving that game found in districts infested by the tsetse-fly act as a reservoir for the human and stock trypanosomes.

My own observations during the past eleven years while Assistant District Resident in Central Angoniand, Nyasaland, leave little doubt on my mind that within such area game has been instrumental in the spreading of tsetse-fly. When I first visited the lake shore division (now the sleeping sickness area) in 1901 and 1902, there was only a small patch of tsetse (*G. morsitans*) to the south of Rifu, although game was abundant all over the division right up to the foot hills. There were numerous villages along the edge of the lake, including Domira Bay and Rifu, where there were large herds of cattle, whereas the rest of the division up to the foot hills, twenty miles back, was uninhabited.

Buffalo and other large game were numerous on the Lingadzi and Lipimbi rivers which run through the division, and such game was never disturbed.

In 1904 the natives from the hills commenced settling on the fertile lands along the Lipimbi river, and thus disturbed the buffalo and game, which then began to cross over to the wilder country near Rifu where the tsetse were. During the dry season these buffalo returned to their old haunts, where the grazing was much better and where there was always water, and with them came the "fly."

It was shortly after this disturbance of the game that I noticed that the game tracks crossing the main road were becoming much more numerous, and at the same time tsetse were first seen on the road.

The cattle had to be moved from Domira Bay to the hills in order to save them, but those at Rifu, which were nearer to the "fly" centre, had already become infected, and nearly all of them died.

Since 1904 and up to 1910 (when the outbreak of sleeping sickness occurred) there was a continual influx of natives into this area on account of the fertility of the land, and tsetse-fly rapidly spread over it.

It is, of course, possible that the tsetse would in any case have spread over the district in the course of time, but there can be little doubt that the disturbance of the game and its consequent movement very considerably hastened this event.

Game destruction over a localised area could, no doubt, be carried out by fencing in a tract of country, but whether such a costly undertaking would be likely to be justified by results is a scientific question upon which I am not in a position to pass an opinion.

(1) If the experiment were decided on, an area of at least 100 square miles would have to be enclosed within an iron fence of at least 9 feet in height, with wire netting along the lower half to keep out the smaller animals. Wooden fencing would be useless, owing to the ravages of white ants and grass fires. Such a fence would, moreover, have to be carefully guarded.

(2) The experimental area would have to be in a district where there were no elephant, since, as is well known, elephant move about by night, and a breach caused by them might remain undiscovered for several hours, during which time other animals could easily enter the area, and thus nullify the experiment. To make a fence strong enough to resist elephant is impracticable.

(3) The enclosed area should, if possible, be uninhabited.

(4) It should be as near to a main line of communication as possible.

So far as Nyasaland is concerned, it is only in the Chikala or Ruu districts that such areas might be found. In both these districts there are large tracts of country where there are tsetse and game, but where elephant are not met with. They have also the advantage of being near Zomba and the railway, which would facilitate the transport of the heavy materials required for the fencing, &c. Also in the Chikala district use might be made of Lake Shirwa to form one of the boundaries of the enclosure, which would be a great consideration.

A large staff, European and native, under a scientific officer would be required, and the experiment would be most costly. It must be borne in mind that no fence or barrier could be constructed that would prevent the tsetse-fly from entering the prescribed area, but the bearing of this fact may be left to scientific judgment.

The general extermination of wild animals is, I consider, impracticable under the conditions obtaining in Central Africa at present.

Further, there are large tracts of country free of tsetse-fly and abounding in game, which are at such an altitude as to preclude the possibility of tsetse appearing, and it does not seem to me that any useful end would be served by attempting the destruction of big game in these localities.

In areas which have a thick population, and where there are tsetse, it might be advisable to allow the natives as well as Europeans to hunt without restriction, but the present regulations would have to be withdrawn slowly, as any sudden indiscriminate hunting in an infected area might cause harm by scaring the game into neighbouring districts at present free from "fly." The risk might be so presented of the tsetse following the game, and so spreading the trypanosome diseases.

Owing to the fact that tsetse are but seldom, if ever, found in open country, and that bush or forest would seem a necessity for them, one possible method of combating them would be the clearing of bush from the vicinity of villages.

(1) In an infected area the villages could be concentrated on the more fertile tracts and on the banks of the rivers.

(2) The land all round these settlements should be cleared of forest for a distance of at least three miles, and kept clear by cultivation. The fact of concentrating the villages into settlements would ensure the rapid clearing of the bush by the natives when they made their gardens or fields. These gardens are generally of great extent.

(3) In the case of a river, the villages would be settled on both banks, not too far apart, with the cultivated lands behind. In this manner a belt of open country, several miles wide, would be formed right through the fly belt.

(4) The main roads running through fly belts should also have the forest cleared well back for a distance of two miles on each side.

In the course of time patches of forest would become entirely cut off and surrounded by these clearings and cultivated belts, and then it would not be a difficult matter to have these isolated patches of bush cleared of game.

The power of the native in clearing away the bush is very great: whenever there has been a large native population settled for any length of time it will be found that large areas of forest have been cut down, and that the game has either deserted the district or been exterminated.

(5) Caravans and travellers coming from "fly" areas should be stopped, on passing out, to see that they are clear of tsetse before continuing their journey.

I have known men on bicycles finding tsetse still on them several miles after having ridden out of a "fly" belt. (Tsetse settled on the back of a man

can be carried a good distance, even into open country, before leaving him; this is especially the case when motor bicycles are used.)

This is undoubtedly one of the means by which areas infected with tsetse have been increased in size.

28th August 1913.

Mr. CHARLES GREY.

(A.)—AREAS FREE FROM *G. morsitans*.

West of Lualaba River in Southern Katanga, between latitudes 10° and 11° south. (Congo State.)

Bush and country generally appear to be the same on both sides of the river. Altitude, 4,000 to 4,500 ft. on both sides.

There is very thick fly east of the river and none west of it.

Cattle can be kept on west side.

The same buck on both sides, but buck became very scarce 15 miles west of the river, though there are plenty near it.

Probably no buffalo west of the river between latitudes 10° and 11° for some distance. Very few buffalo east of it.

Tsetse follow people across the Lualaba from the east bank, but return again.

A few *palpalis* reported west of the river at one or two places.

Elephants very rare both sides, also rhino.

East of Lualaba River, the high open plateau land north of latitude 10° 30', has large areas free from fly. Altitude, 5,000 to 5,500 feet.

Very little bush. Game abundant.

Cattle have been kept successfully.

1903-4.

Luano Valley, to south-east of Broken Hill in Northern Rhodesia, about latitude 15° south.

Valley about 2,000 feet lower than the rest of the country, which has an altitude of about 4,000 feet.

Fly in Luano so scarce that, though cattle cannot be depended on, donkeys have been successfully used.

Morsitans is very thick on the high country quite near the edge of the valley.

The bush in Luano is about the same thickness as that on the high country, but of a different sort—mopani and baobab. Mopani is rare and baobab absent on the high country.

Geological formation quite different to that of the high country.

Game abundant in Luano, but sable, hartebeeste, reedbuck, and oribi, which are very common on high country, are very rarely seen in Luano. No elephants in Luano, but fairly common on high country right to the edge of the valley. Many buffalo both in valley and out of it.

Rhino pass in and out of the valley at places where fly is found near the edge in fair quantities.

The fly is in very thick swarms on the high country, but the thickest belts are irregularly distributed.

Major A. E. HAMERTON, R.A.M.C., Nyasaland Sleeping Sickness Commission.

Human Trypanosomiasis.

*1. In Uganda from October 1908 to August 1910, and in Nyasaland from August 1911 to December 1913.

2. Research. Member of the Royal Society's Commission.

7. In Nyasaland the disease has not spread to any serious extent. Not epidemic.

8. Yes.

10. When the investigations were commenced about 2½ years ago the natives were timid and suspicious and undoubtedly concealed cases. Now, however, the natives have become accustomed to the examinations. The medical officers have gained their confidence, and I do not think many cases are hidden.

* The figures at the beginning of the paragraphs refer to the questions in Appendix A.

Natives say that the fly is coming into the west end of the valley, and is now more common there than it used to be.

Ten miles west of Luano Valley on the high country there is no tsetse, for some distance at any rate, and cattle will live.

The bush here is the same as elsewhere, but thinner than that in the high country further east, where fly is thick.

Twenty or thirty miles south-west of Luano begins the fly free country, which is probably fairly thinly covered with bush. Cattle are used there on all farms.

1909-13.

No memory of *morsitans* in country where there was no game.

(B.)—SUGGESTIONS FOR EXPERIMENTING UPON THE EFFECT OF REMOVING GAME FROM FLY AREAS.

Choice of Area in Northern Rhodesia.

In the middle of a large patch of fly, where game is abundant, but where there are no elephants or rhino, if possible.

No large river to be enclosed.

Area to be in country covered with timber suitable for heavy fencing (the common high country bush).

Select an area of 40 square miles approximately, square or circular, and surround it with a fence about 6 ft. high. Drive out all buck before finishing fence. Remove natives and stop all travelling through the area. About 25 miles of serviceable fencing could be put up for 200l. or so, and maintained for two years.

It would be necessary to have small camps of natives outside the fence at intervals, to avoid, as far as possible, the entrance of lions, leopards, cats, baboons, and monkeys.

Bush and grass should be cleared back for some distance outside the fence, and the grass for some distance inside it, to avoid fire destroying the fence and to detect animals attempting to enter the enclosure.

Dogs might be brought into the area in fly-proof boxes when a test is required.

Two hundred natives would be required for driving out the game.

I do not see how the small mammals can be dealt with, nor do I know whether their presence would be considered a drawback to the experiment. Burning the grass, however, might be so arranged as to drive out all beasts that have no holes.

30th September 1913.

11. Trypanosomes were sometimes numerous in the blood of a patient, at other times very scanty. Glands and cerebro-spinal fluid not examined in Nyasaland.

12. Yes, periodicity occurs, but I have no records bearing on this point. My clinical experience of human trypanosomiasis in Nyasaland is not extensive.

15. In the laboratory it has been proved that two species of human trypanosome can be transmitted by one species of tsetse-fly, viz., that *T. gambiense* and *T. rhodesiense* can be transmitted by *Glossina morsitans*, but there is no evidence that this occurs in nature. We know, however, that other species of trypanosomes are naturally transmitted by more than one species of tsetse-fly. I therefore consider it possible that any tsetse-fly may, under a favourable combination of circumstances in nature, transmit any human trypanosome.

16. Trypanosomes of the *brucei* group are morphologically distinct, and can readily be distinguished from other trypanosomes on microscopical examination by an experienced observer.

17. I do not believe that trypanosomes of the *brucei* group can always, and under all circumstances, be distinguished from each other, either in man, mammals, or the fly.

18. In rare instances it may be impossible to distinguish *T. gambiense* infections from *T. rhodesiense* infections, except by a series of subinoculation experiments and prolonged observation. Generally, however, *T. gambiense* infections can be distinguished clinically from *T. rhodesiense* infections in man, monkeys, and dogs, but not in goats and cattle. In order to identify either parasite found in cattle and goats it would be necessary to subinoculate into dogs and other laboratory animals. With regard to trypanosomiasis caused by other trypanosomes of the *brucei* group, I have not had sufficient personal experience in experimenting with these parasites to enable me to express an opinion.

19. *T. evansi* in India and Mauritius and *T. pecorum* in certain parts of Africa are found and cause disease in domestic animals in localities where there are no large animals and where no tsetse have been discovered. In Africa there is the possibility that *T. pecorum* was introduced into these localities by cattle or goats that had passed through "fly" or "game country."

20. I consider that at the present time trypanosomes of the *brucei* group and that *T. vivax*, *caprae*, and *uniforme* and *sinix* are in Africa conveyed solely by the bites of infective *Glossina*, and that the *T. pecorum* infection in African cattle situated in localities devoid of large wild animals and tsetse is maintained by some unknown agency.

21. I consider it possible and probable that Tabanids, or some other blood-sucking arthropod may play a part in spreading *T. pecorum* amongst a herd of cattle when once an infected animal has been introduced into a healthy herd. In Uganda and Nyasaland goats, dogs, and cattle moving about the country contract *T. pecorum* infection in the first place from tsetse. They may then carry the germ to healthy herds in "non-fly" areas where the parasite is spread by carriers other than tsetse.

22. At the present time I would say that few men are susceptible to *T. brucei* vel *rhodesiense* infection, and that most men are very susceptible to *T. gambiense* infection.

23. Yes. I think some individuals may become tolerant of the trypanosome.

24. Improbable.

26. Since the removal of the human beings from the fly area in Uganda, they play no part in the spread of sleeping sickness. Infected people remain reservoirs of the virus so long as trypanosomes exist in their blood. In Nyasaland, no doubt human cases of trypanosomiasis infect a few tsetse-flies,—and mankind may be the only host of the variety of *T. brucei* which is virulent to human beings. The fact that they are generally too ill to go about—and soon die—may explain the comparative rarity of human trypanosomiasis in Nyasaland. It is uncertain whether *T. brucei* vel *rhodesiense* obtained direct from the blood of wild animals, or from the game through the agency of the fly, will or will not infect human beings.

27. No.

28. I think that all types and species of trypanosomes are liable to differ and vary according to environment. On the other hand, any one type or species may remain constant for many years in the laboratory, and I believe in nature also.

Trypanosomiasis in Domestic Animals.

29. Yes.

33. In Uganda, cattle with *T. vivax*, *T. pecorum*, *T. uniforme*, *T. brucei*, and *T. gambiense*.

In Nyasaland, cattle with *T. pecorum* and *T. brucei*. Goats with *T. caprae* and *T. pecorum*. Dogs with *T. brucei* and *T. pecorum*.

T. pecorum is very fatal to cattle and dogs.

T. brucei is very fatal to dogs and rats.

The virulence of the other trypanosomes varies; some animals exhibit tolerance to the presence of a trypanosome; for instance, cattle tolerate *T. vivax*.

Trypanosomiasis in Wild Mammals.

34. In Nyasaland, yes, elephant, buffalo, zebra, wild pigs, and many species of antelope and carnivora.

35. 31.7 per cent. of the wild game in the fly country near Kasu harbour pathogenic trypanosomes.

The species of trypanosomes found are *T. brucei* vel *rhodesiense* 7.8 per cent., *T. pecorum* 14.4, *T. sinix* 1.7, *T. caprae* 11.1 per cent.

The percentage of wild animals found infected with pathogenic trypanosomes in other fly districts in Nyasaland (Shire River valley) was about the same as at Kasu.

36. No. They exhibit tolerance.

37. The blood of antelope obtained from districts free from tsetse was never found infected with pathogenic trypanosomes. Over 200 blood films from many species of antelope inhabiting fly free country were examined and several inoculations were done with negative result.

38. Sometimes rare; at other times abundant. They can generally be found by an expert microscopist.

39. Yes. The results of the work of the Royal Society's Commission in Nyasaland were, for all practical purposes, in accord with those of other observers.

40.—(1) In fly districts in Uganda antelope are certainly a danger.

In fly districts in Nyasaland antelope are a danger in localities where human trypanosomiasis exists. I know of districts in Nyasaland where human beings, antelope, and tsetse-fly are found in abundance in the same area, yet no human trypanosomiasis has been discovered after most careful search, but I consider the antelope in such districts a potential reservoir of the variety of *T. brucei* which is virulent to human beings.

(2) In Uganda and in Nyasaland, antelope in or near fly areas are the greatest possible danger to all domestic animals.

Measures for checking Trypanosomiasis.

41.—(1) Removal of all people from the fly area wherever this is possible.

In districts where depopulation is not possible, segregation of infected individuals in localities away from the fly.

Clearing round villages in the fly area.

Clearing along main roads passing through fly country.

Destruction of wild animals in the vicinity of native villages in the fly country. Natives should be encouraged to wear white clothing and to carry fly whisks in the fly country.

(2) Segregation or slaughter of all infected domestic animals.

No restrictions should be placed on the hunting of wild animals in the fly country. The natives should be encouraged to hunt and drive the wild animals off the land they occupy, if in or near fly country.

Clearing along main roads.

These suggestions cannot be carried out in wild, remote parts of the country, but wherever they can be enforced continuously, I am of opinion that trypanosomiasis will be reduced.

With reference to game destruction I would like to emphasise my opinion that the necessity for the destruction of wild animals only applies to the tsetse-fly country and the environs thereof—and that the hunting by natives should be carried out with their own primitive weapons and devices. They should not be supplied with modern rifles and ammunition.

On the high plateaux remote from fly country the fauna (excepting carnivora) should be rigidly protected, wherever the land is not required for cultivation or settlement by Europeans.

42. Clearing around villages. Result:—reduction in the numbers of tsetse infesting the villages.

43.—(1) Not desirable and not feasible

(2) Desirable in all "fly" areas, but feasible in populous districts only.

(3) Desirable and feasible around native villages and along main roads in settled and populous parts of the country.

(4) Not desirable or feasible in Nyasaland.

(5) Desirable and feasible.

(6) Desirable and feasible in and around European stations and settlements.

(7) Not feasible.

44. Yes. I am of opinion—

That the destruction of game in fly areas might be followed by a reduction in the numbers of tsetse-fly.

Whether the numbers of tsetse were reduced or not, I would hope that there would be fewer infective flies when the chief reservoirs whence the flies obtain their trypanosomes had been destroyed or reduced in numbers; and that there would be a corresponding decrease in trypanosomiasis.

If a certain result followed the extermination of game in one area infested with *Glossina morsitans*, I think the same result would follow from the same measures if adopted in any other *Glossina morsitans* area.

I think an experiment in game destruction is feasible.

(1) I would suggest for consideration:—

An area within a radius of 25 miles of Liwonde in the Shire River valley in Nyasaland. The locality for the experiment would include a section of the Zomba—Liwonde—N'chen road, which runs through the fly belt, and a part of the Liwonde—Fort Johnston road, which also passes through this "fly" area. The exact site on which the experiment could best be carried out would have to be selected by the European officials resident in the locality. In suggesting this locality I would point out that—

It is near the centre of administration, and easy of access by officials from Zomba, Liwonde, N'chen, and Fort Johnston.

It is a very populous district. The most frequented main roads pass through the fly area near Liwonde. If the destruction of the game resulted in the disappearance of the "fly," or if the fly became innocuous, then these highways would be safe for man and his domestic animals.

The Royal Society's Commission has investigated trypanosomiasis in this district, and there is some information available regarding the degree of infectivity of the fly, the species of trypanosomes with which they are infected, and as to trypanosomiasis in wild and domestic animals during the summer of 1913.

(2) An area enclosed within 25 miles of fencing.

(3) Hunting parties of natives and Europeans could be organised for the destruction of game within the area.

(4) A fence would be necessary. I think a wooden stockade fence, such as the natives make for their cattle kraals, would answer the purpose. In cutting the wood to make the fence a clearing would be made around the enclosed area.

(5) A senior medical officer who is thoroughly acquainted with experimental work in trypanosomiasis, and one who has had considerable practical experience of such work.

An assistant medical or veterinary officer.

An entomologist to study the bionomics of the fly within and without the enclosed area.

A European official to organise labour and control subordinate staff of Europeans and natives.

(6) Perhaps 40,000*l.*, i.e., 10,000*l.* a year for three or four years.

(7) Around the fence and on the outside of it there should be a clearing at least half a mile wide, to reduce the chance of flies and game from outside breaking into the enclosed area, and on the inside of the fence there should be a clearing 20 yards wide all the way round.

During the time the fence was being made the medical officers would ascertain the infectivity of the fly by dissection and fly-feeding experiments.

On the completion of the fence the game within the enclosure would be got rid of, and the medical officers would continue the fly-feeding experiments and dissections of flies with flies caught within the enclosure.

The entomologist would study the bionomics of the fly as affected by the destruction of game within the fence. He would mark large numbers of flies in the fly area beyond the fence and ascertain if they invaded the enclosure.

During the third and fourth year of the experiment healthy domestic animals—dogs, cattle, and goats—would be introduced into the enclosure and kept under observation for trypanosomiasis.

(8) Three or four years.

(9) Infected wild animals might break through the fence.

Infected flies from beyond the fence might invade the enclosure. If large numbers of flies from outside got into the enclosure, and if antelope or other large animals frequently entered, then the experiment would be quite useless.

(10) If a small isolated fly area stocked with game could be found it would be better to use such a place for the experiment than to enclose an area in the midst of a vast tract of "fly" country, but if there were wild animals near the isolated fly area it would be necessary to build a fence to keep them out.

In a large fly area such as the Shire River valley, it would take nearly ten years to exterminate the game.

I consider the quickest way to get accurate knowledge regarding the bionomics of the fly and the whole natural history of trypanosomiasis would be to carry out an experiment in game destruction in an enclosed fly area.

Treatment of Trypanosomiasis.

45.—(1) No.

(2) No.

(3) No.

Dr. H. HARDY, German Colonial Office.

The Extermination of Wild Animals in order to prevent the Spread of Infectious Diseases in German Colonies.

The only experiment of combating sleeping sickness by exterminating wild animals in German Colonies was made on a small island in Lake Victoria, near Schirati, by killing crocodiles with poisoned meat. This experiment proved to be a failure and was abandoned. The smell of putrefied corpses of the poisoned crocodiles had only attracted larger quantities of crocodiles to that island.

The killing of other wild animals (antelopes, &c.) in order to combat sleeping sickness has not yet been tried in German East Africa, and it is not likely to take place in the future. The scientific medical researches of the German authorities do not promise any particular success with such a measure.

Experiments made by Dr. Faute (published in Volume 45, Part I., by the Imperial Sanitary Office)

give some important evidence on this question. The general opinion of German authorities is that the scientific basis for recommending the killing of wild animals as a preventive for the spread of sleeping sickness is not yet sufficiently founded.

In order to prevent rinderpest from spreading into German East Africa a great number of game have been shot on the British East African boundary, in order to establish a zone free from wild animals. By order of the Government an Askari Company killed a few thousand antelopes of different kinds, but these measures were soon abandoned, and no definite result has been arrived at. A German medical officer, Dr. Weck, has lately experimented on the boundary of German and Portuguese East Africa, near the Rouvuma River, and found it possible to transfer the trypanosoma of human sleeping sickness to game of different kinds (the *à priori* found trypanosoma of wild animals are distinct from human trypanosoma in several respects). It is to be supposed that infection can be

transferred from men to animals. If this is the case, Dr. Weck specially warns against driving away game from one territory, because the infected game can easily spread the disease into other territories not yet infected, and there infect the flies, game, and human beings. If any measures were to be taken against wild animals, only the absolute extinction of all game, including small game, could be of any use—a measure which in those parts of the country does not seem feasible.

Dr. Hoering, medical officer of the German East African Forces, has published an interesting article on big game and its influence on the spreading of sleeping sickness, in No. 18 of the *Deutsches Kolonialblatt*, 1913.

The translation is as follows:—

"In recent years there has been a vigorous and uninterrupted investigation of sleeping sickness. The Germans and English have especially taken a great share in increasing our knowledge of the nature of this disease. Whilst on our side the work of R. Koch was worthily continued by Professor Kleine, in England Sir D. Bruce was in the first rank of English authors; he has been for a long time well known as a successful investigator of tropical diseases. It is particularly pleasing to be able to state that the scientific expeditions of the two nations have always entertained the most pleasant and friendly relations to each other, and have, whenever the opportunity presented itself, sought to further each other's endeavours.

"In 1908 Kleine had ascertained by experiment the sexual development of the *trypanosomata* (to which belong the excitants of sleeping sickness) in the body of the fly which induces that disease (*Glossina palpalis*), and had corrected the previously current view of the mechanical conveyance of the infection. In 1911 his collaborator, Taute, at his suggestion furnished the practically very important evidence of a possible transmission of sleeping sickness by the ordinary tsetse-fly (*Glossina morsitans*). These two results were shortly afterwards checked on the English side (Bruce, Kinghorn, and Yorke) and confirmed on all points. Quite recently the main interest of investigators was directed to a hardly less important question: The point was to decide whether and to what extent African large game and domestic animals were responsible for the spread of sleeping sickness. Kleine and his collaborators, on the strength of their past observations and experiments concerning the rôle which mammals may possibly perform in nature as the carriers of human pathogenic *trypanosomata*, expressed themselves with great reserve, and considered it too early to be able to conclude from hitherto experimental studies as regards practical application in combating sleeping sickness. In contrast to this the English took a much more radical view, and had already drawn conclusions pointing to the entire destruction of game for the purpose of combating sleeping sickness. They based their assumption mainly on the investigations of Kinghorn and Yorke, who were of opinion that in Northern Rhodesia the enormous proportion of 16 per cent. of the game was infected by *Trypanosoma rhodesiense*, which is the exciting cause of sleeping sickness in that region.

"In view of the great divergence of opinions and the practical importance of this matter as affecting our struggle with the disease, Surgeon-Major Dr. Taute, of the German East African troops, determined to submit the question to a renewed experimental test. He carried out this plan, taking advantage of an expedition in Portuguese East Africa, which was supported by Imperial funds. The results arrived at by Taute are published in No. I. of Volume 45 of "*Arbeiten aus dem Kaiserlichen Gesundheitsamte*" (Work of the Imperial Bureau of Hygiene). We will mention the most important practical points.

"To his surprise Taute found in 37 cases of shot game (antelopes, buffaloes, boars), that is to say, 16·2 per cent., a species of *trypanosomata* which, in appearance and in pathogenesis as affecting animals, entirely resemble the *Tryp. rhodesiense*, the excitant of sleeping sickness; his examinations of the few domestic animals of the natives resulted in very similar discoveries. So far, then, the results obtained by Taute perfectly agree with those of Kinghorn and Yorke (*vide supra*). Taute then proceeded as follows in his reasoning: In the district examined by Kinghorn and Yorke (Rhodesia) comparatively very rare cases of sleeping sickness occurred among the population, and in his own district (Portuguese East Africa) no cases at all of this disease could be discovered among the inhabitants. Accordingly, more than 16 per cent. of the game and a scarcely inferior percentage of domestic animals were infected with the excitant of sleeping sickness in a territory where inhabitants were immune. But human beings cannot escape the bites of the infected *Glossina* in the extensive tsetse districts any more than can game or domestic animals; consequently in the territories concerned the human population ought to have been infected in a high proportion with sleeping sickness. Now, this not being the case, Taute drew the conclusion that the *trypanosomata* found in game by Kinghorn and Yorke and by himself were not identical with the germs of human sleeping sickness, notwithstanding their otherwise very close resemblance.

"This had, of course, to be proved experimentally, and it constituted the main difficulty of the investigations. The *Trypanosoma brucei* (the ordinary animal *trypanosoma* prevailing all over Africa), which was the first to be examined as likely to cause confusion, can be distinguished specifically from the *Tryp. rhodesiense*, the excitant of sleeping sickness, solely by the fact that the latter acts pathogenically, not only on animals, but also on man, whereas the *Tryp. brucei* can exclusively induce sickness in animals of the most varying species. Consequently, in order to be able to distinguish these two species of *trypanosomata*, experiments on human beings are necessary.

"In the course of two experimental journeys, Taute allowed 32 or 49 *Glossina* (tsetse-flies), which were infected with *trypanosomata* from antelopes, to suck blood daily for five days from himself, before and after, but also from healthy dogs, apes, and goats (for the purpose of verification). In the course of a third experimental journey, Taute gave himself a subcutaneous injection of two cubic centimetres of the blood (equal to about 82 millions of *trypanosomata*) of a dog which was infected with the *trypanosomata* in question; the same, or a smaller dose, was at the same time administered to seven control animals, applying the injections to the epithelium of the stomach.

"In all three series of experiments the result was that, after an incubation of from three to six days, the control animals sickened of trypanosomiasis, and shortly afterwards perished, whereas the human subject continued in good health, showing no reaction. The inoculated *trypanosomata* were consequently identified as the *Tryp. brucei*, and not the excitants of sleeping sickness.

"It was, therefore, established that *trypanosomata*, when occurring in game or domestic animals naturally infected, can only be with certainty pronounced as excitants of sleeping sickness after having proved their pathogenic effect on man.

"In the second place, it had been proved that game, at all events, and domestic animals do not participate in the spread of sleeping sickness to the extent as was presumed by Kinghorn and Yorke.

"It will be necessary to exercise great caution in drawing comprehensive conclusions. For

certainly it will not be possible to deny that, in certain circumstances, game plays a rôle as a 'reservoir,' and consequently also as a disseminator of the animal *trypanosomata* (especially the *Tryp. brucei*, *nanum*, *vivax*). On the other hand, in connexion with the dissemination of sleeping sickness among human beings, the importance of game and domestic animals has been considerably exaggerated by many authors, and, after the investigations of Taute, it will be necessary in this matter to revert to the standpoint occupied by Kleine and his collaborators as far

back as 1909. At all events, the propaganda in favour of immediately destroying all game for the sake of combating sleeping sickness, as carried on by many, and partly already by the parliaments of the colonies, is not called for. It will be advisable first to await the results of expensive experiments to be carried out *in situ*, which will extend over many years, as planned by the British Government. But even these experiments, if they are to be free from objection, offer great difficulties; it is to be hoped that they will throw light on the matter."

Mr. F. H. HAWKINS, LL.B., Secretary of the London Missionary Society.

I am the foreign secretary of the London Missionary Society for its Central Africa Mission. The Society's field lies between the Lunpula (Congo) and Lake Mweru on the west, and a line running north and south about 35 miles east of Abercorn on the east. The northern boundary of the Society's sphere is the Congo-Belge frontier, the south shore of Lake Tanganyika and the parallel of latitude running through Kasanga. The southern boundary is approximately a line drawn from the Government station at Kawambwa (20 miles south-east of Moereshi) through Mpolokoso to the neighbourhood of Mambwe (one of the eastern out-stations of Kawimbe) on the Stevenson road. The whole of this is situated in the northern part of Northern Rhodesia except a small tract of country in German East Africa extending as far as Kasanga (Bismarckburg) on the south-eastern shore of Lake Tanganyika.

The Society commenced work in Central Africa in the year 1877, but its missionaries have only been at work in the area now occupied since 1887. Its present European staff consists of 10 missionaries.

Part of the territory recognised as the Society's field lies within the sleeping sickness area, especially that adjoining the west shore of Lake Mweru and the southern shore of Lake Tanganyika.

I visited the whole of the Society's Central Africa field in the present year, and was travelling through it during the months of April, May, and June.

The steps taken by the local administration to stamp out sleeping sickness in the area seemed to have met with great success. I was informed that in the Society's field of operations there was only one fresh case during 1912.

Complaints were made to me by the natives, especially in Tanganyika district, that the operation of the Sleeping Sickness Regulations had been a great hardship because they had been moved from their homes on the shores of the lake and had been prevented from fishing in these waters. Fish had been the main article of food for many years amongst the people in the neighbourhood of the lake, and they resented the fact that they were no longer permitted to obtain fish. I was informed that similar restrictions did not exist in the German territory, and that this had led to large numbers of natives escaping from Northern Rhodesia over the boundary in spite of Government regulations to the contrary and of the efforts made to stop the emigration.

I heard complaints with regard to the policy of establishing game reserves. It was pointed out to me

that wherever game abounds carnivora abound, making the district dangerous to natives and to white men. I met with abundant evidence of the danger from lions during my journey. In the neighbourhood of the Society's station at Mbereshi, 20 miles north-east of the Government station at Kawambwa, I was informed that during the previous few months 23 people had been killed by lions. A few weeks before I arrived a lion had entered a hospital hut in the station and killed a leper. Lions had also destroyed donkeys and cattle belonging to the Mission. I visited Chiengi, on the north-west shore of Lake Mweru, and was informed that 53 natives had been killed by lions in this district in a short time. On my journey to Mbereshi I passed through a village where three women had been killed by a lion on the previous night. At Kambole, another station of the Society near the south end of Lake Tanganyika, six miles from the Government's old station at Katwe, lions and leopards were frequent visitors at night.

All our missionaries stated that the nuisance from these carnivora had greatly increased in recent years, and they ascribed the increase to the policy of game reserves and to the regulations preventing the natives from shooting. Complaints were made that it was a hardship on the natives not to be able to shoot animals that were destroying their gardens except by paying a prohibitive licence.

The consensus of opinion of the Society's missionaries at the south end of Lake Tanganyika was to the effect that the tsetse-fly increases and spreads as the game increases.

It was pointed out to me that in cattle disease it was possible to isolate a herd of cattle, but that game could not be isolated, and therefore the disease spreads from herd to herd.

Another matter which has an indirect bearing upon the spread of sleeping sickness is the smuggling which is constantly going on round the south end of Lake Tanganyika in spite of the efforts of the administration to stop it. This smuggling constantly takes the natives through the danger zone of the sleeping sickness area. I was informed that the Swahili smuggle rubber and ivory from the Congo Free State through Rhodesia to Bismarckburg, passing through the fly belts and constantly travelling to and fro between affected areas. I am informed that there are statements about this matter in Mr. Cullen Gouldsbury's book, "The African Year," published in 1912, but I have had no opportunity of consulting the book. Mr. Gouldsbury is a Native Commissioner.

Mr. ALLAN KINGHORN, Medical Officer, Northern Rhodesia.

BIG GAME AS A RESERVOIR OF HUMAN TRYPANOSOMIASIS AND THE QUESTION OF ITS DESTRUCTION IN NORTHERN RHODESIA.

As the result of the past three years' work on sleeping sickness in this country, I have been led to the following conclusions:—

1. Human trypanosomiasis is due to infection by *Tryp. rhodesiense*, Stephens and Fantham.
2. The disease has not been recently introduced, but is of old standing, and has now reached a state of comparative equilibrium.

3. One of the trypanosomes infecting game and *Glossina morsitans* is identical with the human parasite, i.e., it is *Tryp. rhodesiense*.

1. Identity of the Human Trypanosome.

The strain from which *Tryp. rhodesiense* was originally described by Stephens and Fantham was derived from a European who had been infected in the Luangwa Valley. This parasite presented certain peculiar features, both morphological and biological, which sufficed to distinguish it from *Tryp. gambiense*. Briefly these consisted in a marked posterior displace-

ment of the macronucleus, which occasionally proceeded to such a degree that it was actually situated behind the micronucleus, and in a decidedly higher degree of virulence for laboratory animals.

During the continuance of the Luangwa Sleeping Sickness Commission, strains of trypanosomes from 16 cases of the disease were studied, and in each of them the morphological and biological peculiarities of *Tryp. rhodesiense* were observed. These cases originated chiefly in the Luangwa Valley, but a few from other areas, e.g., the vicinity of the Livingstone memorial to the west of Serenje, and the district lying between Ndola and Kansanshi, were also obtained.

The Royal Society's Nyasaland Commission has shown that the infections in that territory are due to the same parasite. In Portuguese East Africa this has been proved by Taute, and in Southern Rhodesia by Bevan. That is to say, in all South-East Central Africa, where *Glossina morsitans* exists in the absence of *Glossina palpalis*, the cause of the human disease is *Trypanosoma rhodesiense*.

2. Age of the Infection.

Attention was first drawn to the Luangwa Valley as a focus of sleeping sickness by the occurrence of several cases of the disease amongst Europeans in the years 1909 and 1910. It was naturally thought that the disease had extended from the foci on the Luapula River, Lake Mweru, or Lake Tanganyika, and also that possibly the presence of *Glossina palpalis* had been overlooked. However, it was proved that this tsetse fly did not exist in the area in question, and eventually it was shown that *Glossina morsitans*, which is particularly abundant, was the vector. The trypanosome differed from *Tryp. gambiense*, and the question arose, from whence had it come?

Native evidence was obtained, and was to the effect that a disease, known in various localities as "Chilolera," "Chivilwa," and "Nyamakazi" had long been recognised, that is, for at least two or three generations. The symptoms of this disease are described as being headache, fever, emaciation, and cedemata, so that it bears a striking resemblance to sleeping sickness. Caution must, of course, be observed in accepting native evidence on such a subject, but as it is everywhere quite definite, and as no history of any epidemic exists, it is probable that the disease known under these names is in reality trypanosomiasis.

Quite apart from the native evidence, however, the history of the disease is not consistent with the theory that it is a new importation. As stated above, the first definite information was obtained in 1909, prior to which date no competent examination of the natives had ever been undertaken. During 1909 and 1910 a large portion of the Luangwa Valley was examined by Dr. Leach, who found a number of cases, though the proportion to the population was small.

In October 1911, I examined 1,332 natives in 20 villages in the Mpika portion of the Luangwa Valley and isolated five cases of the disease, while in June 1913, in the same villages, I palpated 1,155 people without finding a single case.

During the months December-February 1911-12, Dr. Wallace examined 10,209 natives in 154 villages in the Lundazi portion of the Valley, and isolated eight cases of the infection, while during July, September, and October 1913 I palpated 10,576 in the same 154 villages and diagnosed six cases. It may be added that all these examinations were carried out with the help of accurate censuses, so that a very high percentage of the available population was seen.

We have, therefore, the following definite figures for exactly the same localities, many hundreds of square miles in extent:—

Year.	Number examined.	Number of Cases.	Per cent. infected.
1911	11,541	13	0.11
1913	11,731	6	0.05

Thus, after a lapse of from 20-24 months, the disease had retrogressed to such an extent that only half as many cases were found on the second as on the

first occasion. And it must be borne in mind that this change had occurred despite the presence of factors which should have favoured its advance. Laveran and Mesnil state, "Les guerres et la famine ont favorisé, en Afrique, la propagation de la maladie du sommeil; c'est la conséquence ordinaire de ces fléaux sur les maladies épidémiques, et la maladie du sommeil, habituellement endémique, peut prendre la caractéristique épidémique."

Owing to the failure of the rains in the season 1911-12, and to the floods which occurred during the wet season 1912-13, the people were suffering from chronic hunger, which amounted to absolute famine in some instances. I saw several villages which were temporarily deserted through lack of food, and in most of them many of the inhabitants had left for the same reason to visit relatives in more favoured localities.

The fact that the disease has remained stationary, if not actually retrogressed, during the past two or three years is corroborated by other medical officers who have worked in the Luangwa Valley. Thus in a group of villages near Chinundais, some 40 miles north of Fort Jameson, in which in 1910 Dr. Leach found five cases, he was unable in August 1913 to find a single one. I believe I am correct in saying that the Principal Medical Officer, Dr. May, has had a much similar experience in the Petauke portion of the Valley.

The consensus of opinion is, therefore, that the disease is endemic, and shows no tendency whatever to assume epidemic proportions such as might be expected were it of recent introduction. In the experience of the Congo Free State and Uganda, we have evidence of what rapid strides the disease makes in virgin ground. Nothing of the sort has happened in this country, and the conditions obtaining here may with much greater propriety be compared with those found on the West Coast of Africa. The disease has there admittedly been present for at least a century, and exists in an endemic form. In the portion of Ashanti with which I am familiar, the disease is perfectly well recognised by the natives, who, however, have no tradition of an epidemic. They say it was known to their fathers and grandfathers, and that a case crops up here and there, but never many at a time. Much the same thing is said by the natives in Rhodesia.

In 1910, I palpated some 16,654 natives in the Western Province of Ashanti and isolated 97 cases. This gives a percentage of infection of 0.58, though probably this understates the actual percentage slightly. Quite recently (September 1913), I have heard from the Medical Officer who has been in charge of Sleeping Sickness Investigation since I left the country, and he says, "I did the Western Province again examined 39,742, and only found 87 new cases," a percentage of 0.21.

The resemblance between the conditions obtaining in this portion of West Africa and those in the Luangwa Valley in Rhodesia are, therefore, very striking, and point, in my opinion, to the conclusion that the disease in this country is an old one which is now found in an endemic form only.

It has been argued that the comparatively large number of cases which occurred amongst Europeans travelling in the Luangwa Valley during the years 1909 and 1910 is an indication of the disease being a new one. Certainly the diagnosis of four or five cases in quick succession was striking, but it may be observed that Europeans in this country are living in a strange environment, under exceptional climatic conditions, and in most cases in a much rougher manner than that to which they have been accustomed, all factors which would render them more susceptible to any infection. Owing to the imperfect condition of the records it cannot be stated definitely that no cases had occurred amongst the white population prior to 1909. Moreover, it is my opinion that the true epidemiology of the disease can be learnt with much greater certainty by studying its course amongst the indigenous population rather than amongst an alien and transient one.

3. Identity of the "Game" and "Fly" Strains.

Laveran and Mesnil state that the determination of the species of any trypanosome rests upon a study of:—

1. The Morphology.
2. The Pathogenicity.
3. Special methods:—
 - (a) Cross-immunisation.
 - (b) Serum diagnosis.

To these may now be added a fourth method—the manner in which the trypanosome develops in the invertebrate host and the portion of the fly in which it takes up its station at the completion of development.

The third method of identification, in so far as it affects the strains under consideration is at once put out of court by the fact that it has, up to the present, been impossible to immunise any experimental animal against them.

As regards methods (1) and (2), the morphology of the "game" and "fly" strains is exactly that of the "human" strain of *Tryp. rhodesiense*, and the pathogenicity of all three shows a most remarkable similarity. So close is the resemblance of the three in every particular that we could come to no other conclusion than that they were one and the same organism, namely, *Tryp. rhodesiense*.

Further, it was found that on the completion of the development of this trypanosome in *Glossina morsitans*, the salivary glands of the fly were invariably invaded before it became capable of transmitting the virus, and a dissection of naturally-infected tsetse flies which were transmitting the apparently identical parasite, revealed the fact that the salivary glands were infected. We have, therefore, an additional point of resemblance between the "human" and "fly" strains, and this should be emphasised as none of the other trypanosomes which we encountered, namely, *Tryp. vivax*, *nanum*, *peccorum* and *simia* (*ignotum*) invade the salivary glands of the invertebrate host.

The conclusion which we reached that the "game" and "fly" strains are *Tryp. rhodesiense*, has been fully confirmed by the Royal Society's Commission, though they are of the opinion that, while the "human," "game," and "fly" strains are identical, reasons exist for identifying *Tryp. rhodesiense* with *Tryp. brucei*. This view, however, is not accepted by many observers, e.g., Stephens, Laveran, and others.

Taute has also borne testimony to the extremely close resemblance between the human strain and that obtained from game and domestic stock in Portuguese East Africa.—"Es wurden somit unter 37 Stück Wild durch Blutüberimpfung auf Affen und Hunde im ganzen 6 mal Trypanosomen nachgewiesen, die, wie im folgenden gezeigt wird, in ihrem Aussehen und ihrer Pathogenität für Tiere vollkommen dem *Tryp. rhodesiense* gleichen." But while stating this, he believes that he has shown that the two are not identical by twice feeding bred *Glossina morsitans* infected with the game strain on man, and once directly inoculating the same strain. Apparently all these experiments were carried out on himself, and as he is satisfied that the three game strains are identical, the whole series becomes reduced practically to a single experiment. Until some worker has sufficient confidence in the result to confirm Taute's observations, I consider they cannot be accepted as conclusive. If they are accepted, it would mean that in certain localities two strains of trypanosomes exist which are absolutely indistinguishable except by inoculating human beings. So far as I am aware this would be unique.

4. Should the Game be destroyed?

Should Taute's conclusions be regarded as correct, it may be said at once that no possible excuse can be found for destroying the game in so far as it is a reservoir of the human virus.

If, on the other hand, the view accepted by us and the Royal Society's Commission—that the game and fly strains are identical with *Tryp. rhodesiense*—be regarded as the more correct one, the case is altered to a slight degree. The game now becomes the principal reservoir of the virus, in Luangwa Valley to the extent of 16 per cent. Superficially, therefore, it might at once be concluded that the game must be destroyed. But

in view of the known position of sleeping sickness since 1910, can this be justified? The available statistics, compiled by competent observers throughout the whole extent of the Luangwa Valley, show clearly that the disease shows absolutely no tendency to become epidemic, but on the contrary a tendency to retrogress, although all the conditions favourable to its spread, viz., an abundant reservoir, an ubiquitous and plentiful vector, and lowered vitality amongst the natives, have been present. The proportion of cases found during 1913, 8 per 10,000 (15 cases amongst 17,178 natives), cannot possibly be regarded as a serious matter, and the data show that this proportion compares very favourably with that found some two or three years ago. The death rate from several other diseases is much higher than that from sleeping sickness.

Moreover, it should be remembered that the removal of the game alone will not solve the difficulty. The parasite has been found in domestic animals by several independent observers: in the Luangwa Valley by Kinghorn and Yorke, in Nyasaland by Bruce and his co-workers, and in Portuguese East Africa by Taute. Logically, therefore, the domestic stock in all these areas would have to be removed as well. In those localities in which the natives possess herds of these animals, it is probable that such a step would lead to political complications, and in view of the uncertainty which exists as to the ultimate effect on tsetse flies of the destruction of the game and stock, it should not be undertaken without the fullest and most careful consideration.

The smaller vermin are not of such great importance, as, with few exceptions, they are nocturnal in their habits.

On the plateau in Northern Rhodesia the case for the destruction of the game becomes much weaker. Here it is infected with *Tryp. rhodesiense* to the extent of only some 3 per cent.; the examination of the natives, so far as it has been accomplished, has revealed the presence of only some two or three cases of the disease amongst some thousands of inhabitants; only one *Glossina morsitans* in 1,500 was found to be capable of transmitting the parasite in nature, and, finally, the meteorological conditions obtaining during, at least, two-thirds of the year, are such as to preclude the complete development of the trypanosome in the fly.

In my opinion another very valid reason for not exterminating the game at the present time is found in the uncertainty which exists as to the exact relationship obtaining between the game and tsetse flies. There are almost as many views on this question as there are observers, and this may, perhaps, be taken as a criterion of the actual knowledge we possess. So far as I am enabled to judge, after some seven years' experience, I do not think that the destruction of the game would result in the absolute disappearance of the fly, and until it has been conclusively shown that this will be the result I consider that the extermination of the fauna would not be justified. Experiments which would settle the point have been outlined, and, as the question is one which intimately concerns all the African colonies, the expense should not prove an insuperable obstacle.

To sum up, therefore, I consider that the destruction of the game, as a reservoir of human trypanosomiasis, is not warranted at the present time, since—

1. The disease is in a stationary, if not retrogressive, condition, and the incidence, 8 per 1,000, comparatively unimportant.
2. Some doubt exists, in view of Taute's work, as to the identity of the trypanosomes infecting man and game.
3. The destruction of the game involves that of any domestic stock in the affected areas.
4. The question of the relationship existing between game and tsetse flies has not been settled.

I have above only considered the question of the destruction of the game and stock in so far as it acts as a reservoir of the human virus. If we consider them in so far as they act as reservoirs for all the pathogenic trypanosomes their destruction might be justified to a much greater extent. In the Luangwa Valley we found that at least 57.5 per cent., and on the plateau 23.3 per cent., harboured trypanosomes of

this character, and in all probability the actual percentages should be placed much higher. Moreover, a large percentage of the native stock—dogs, goats, sheep, and cattle—were found to be similarly infected, so that the destruction of the game automatically involves that of the stock. In a farming community this is, of course, a very serious matter, though undoubtedly European settlers have acted very unwisely in buying stock indiscriminately from the natives without making any enquiries as to the place of origin of such stock. As a general rule such purchases are allowed to run with the healthier stock, and it is possible that some of the outbreaks of trypanosomiasis amongst domestic animals can be traced directly to this procedure, particularly in cases where tsetse flies have not been found.

Several of the arguments against the destruction of the game and domestic stock as a reservoir of the human disease apply with equal force against its extermination as the reservoir of animal trypanosomiasis. Moreover, North-Eastern Rhodesia is as yet very sparsely settled, and the game can well be left undisturbed for at least a few years until the "game and fly" question has been settled.

5. Do the Natives possess any immunity?

There is one question which naturally arises from the foregoing statements about human trypanosomiasis.—How can the very low incidence of the disease be explained if such a large percentage of the game acts as a reservoir, and the vector is so plentiful? If our results at Nawalia are applicable to the whole valley, 16 per cent. of the game is infected, and 1 fly in 500 is capable of transmitting the virus. The fly and game is abundant over the whole extent of the region, and it is therefore probable that in the course of any one year a native will be bitten by an infected fly, more particularly the men, as they spend much of their time in the bush, travelling, hunting, cutting trees, collecting honey, and various other occupations. Yet only an occasional one contracts the disease.

The conditions in this country are almost exactly comparable with those obtaining in Ashanti. There the tsetse fly (*Glossina palpalis* more particularly) is abundant, and may be caught in the houses in many of the villages; about 1 per cent. of the total population is infected and is not segregated; and the meteorological conditions are extremely favourable to the development of the parasite in the fly. Yet the disease shows no tendency to become epidemic.

Whatever views may be held on the subject, it is an extremely difficult matter to advance cogent arguments

in favour of them, but in spite of the difficulty of proving such an assertion, I am inclined to agree with the suggestion that the natives may possess a certain degree of immunity.

We have shown that when once a tsetse fly (*Glossina morsitans*) becomes capable of transmitting *Tryp. rhodesiense* it retains that power during its life, and is infective to small animals at every meal. In theory, therefore, it should be capable of infecting man similarly, though in view of what I have said above, it seems probable that this is not the actual case. On the assumption that the game strain is not identical with the human one, Taute explains his failure to become infected on the grounds that the game trypanosome does not find the required conditions in the human body to enable it to propagate. What these conditions are he does not say, but in any event it would not explain the conditions of affairs found in parts of West Africa. We have still to account for the endemicity of the disease there. Todd, who has had an extremely large and varied experience of sleeping sickness, has come to the conclusion that in the Gambia relative immunity, at least amongst the older people, does exist.

The objection to the immunity theory would rest in the fact that to acquire such a condition, infection by the virus, either alive or dead, is a necessary antecedent, and that there is no evidence whatever to show that infection by *Tryp. rhodesiense* is ever followed by recovery. Certainly in all diagnosed cases death has been the result, but this does not necessarily imply that every human being who is bitten by an infected fly contracts the disease, and if such an occurrence is possible, as it would appear to be if our conclusions are justified, is it unreasonable to suppose that the injected trypanosomes during their transitory life in the human host, or even after they have been killed off, will not stimulate the body to produce protective substances against the virus? Recent experiments on immunisation would indicate that there may be some support for such a view.

But whatever theory may be advanced to account for the low incidence of the disease, I think there can be no doubt of the essential fact that, so far as it is possible to form any judgment, the disease in this country shows no tendency to advance. So long as this is the case, I consider that the destruction of the game is absolutely unwarrantable, in view of the unsettled state of the "game and fly" question.

Mpika, N. Rhodesia.

2nd April 1914.

Mr. W. A. MURRAY, M.B., Ch.B. (Edin.).

The views I hold on the subjects of the terms of reference, based on my 12 years' experience in British Nyasaland, and three years of sleeping sickness, are briefly these.

1. In my mind there is no doubt as to the culpability of both *G. morsitans* flies and wild game in the transmission of trypanosome infections of man and stock. In 1909 I discovered that our cattle at Mvera Mission Station (2 miles from the present Royal Society Commission Laboratory under Surgeon-General Sir David Bruce at Kasu), as well as those of neighbouring native chiefs, were dying of a trypanosome disease, contracted in the vicinity, whereas they had been quite healthy before. This coincided with the spread of wild game (eland and kudu), from the plains below, accompanied by tsetse-fly (*G. morsitans*), the previous year. Prior to that time no game or fly had been in that neighbourhood since the rinderpest epidemic of 1896.

Other similar cases could be cited. With regard to man, I had the charge of the sleeping sickness camp for native patients for eight months from August 1910, and have not the least doubt that they were infected through *G. morsitans*.

2. I think it very advisable, and also feasible, to carry out such an experiment. The selected area, which must be a fairly large one to be of value, would

need to be fenced in so as to prevent the doomed game from escaping and possibly infecting other areas, and also to prevent fresh game from entering and re-infecting, or merely sustaining, the flies in that area. The value of such an experiment would be to prove—

- Whether *G. morsitans* would continue to live in an area denuded of game.
- Whether, in case they did survive for a year or more, they lost their trypanosome infections.
- Whether the fly was able to live on the blood of birds, small mammals, &c., in the absence of larger wild game.

It would, of course, be necessary for the success of this experiment that no natives should enter the area after the game in it had been killed off. Also a wide stretch of clearing, sufficient to deter tsetse-flies from crossing to and from the selected area, would have to surround that area.

3. I believe the only hope of exterminating trypanosome diseases in Nyasaland and Rhodesia is to exterminate or drive out all wild game within all forests known to shelter, or likely to shelter, *G. morsitans* fly. At the same time natives must be prevented from re-infecting the fly. I believe the scheme quite feasible if the best use were made of the natives in or around those forests, to aid in destroying game.

4. Strict measures should certainly be taken to control the movements of natives from infected to non-infected areas, districts, and countries. I am convinced that the outbreak of sleeping sickness in Southern Rhodesia (about Salisbury) during 1912 was largely, if not entirely, due to the escape of hundreds of natives from the infected area near Kasu to Salisbury and vicinity, owing to lack of sufficient control. I know this is a very difficult matter, but not impossible,

I think, if the Nyasaland, N.E. Rhodesian, and Portuguese East African Governments work together.

To my mind the imminent danger lies in the extension of human trypanosomiasis throughout every tsetse-infected forest in Nyasaland, Rhodesia, and Portuguese East Africa down to Zululand—an enormous area.

The danger to cattle and other domestic animals in the same area is equally great.

Mr. C. O. OCKENDEN, First Grade Resident, Nyasaland.

Report on Game in the Lilongwe District.

Lilongwe is probably the richest district in the Protectorate as regards game of all the common species and its general distribution throughout the district.

Elephant.—Large herds of elephant roam throughout the southern and western portions of Lilongwe and to a lesser degree in the thick forests near the lake shore in the Dowa sub-district. These herds consist almost entirely of cows and their young, the large bulls having been shot out. They are extremely dangerous and destructive, and portions of the district have been rendered uninhabitable owing to their depredations.

Rhinoceros.—Scarce; there are a few in the game reserve and along the Marimba boundary.

Hippopotamus.—Numerous; found in all the streams and along the lake shore—are very destructive to native crops.

Buffalo.—Formerly very numerous, but almost exterminated by rinderpest twenty years ago—now found in small herds of ten or so scattered throughout the district—probably about five hundred in all.

Eland.—The most numerous of all game, generally distributed and very destructive to native crops. Estimated number—probably twenty thousand.

Sable and Roan Antelope.—Very common and well distributed; the herds are, however, small in comparison with eland.

Kudu.—A few.

Lion.—Many, often hunt in packs of five or more.

Wild Dog.—Common.

Zebra, Hartbeeste, and Waterbuck.—Numerous throughout the district.

Puku.—Very numerous along the Bua River, but not met with in other localities.

Reedbuck, Pigs, and smaller game.—Well distributed.

Crocodile and other Reptilia.—Common throughout the district.

Game Reserves.—The only remaining game reserve in the Protectorate is situated on the high plateau in the southern portion of the district along the Anglo-Portuguese boundary. I believe it was originally intended as a sanctuary for elephant; the boundaries, being natural ones, do not prevent game from wandering and I think it is as plentiful outside the reserve as inside. There are at present about twenty thousand natives settled in the game reserve; much of the land being eminently suitable for maize production.

Population.—The population of the district has been estimated at 200,000 of which about 18,000 are within the proclaimed (sleeping sickness) area.

Experiment in Game Destruction.—I personally consider that it would not be feasible to utterly exterminate the game in any given locality of the sleeping sickness area owing to the prohibitive cost. Such an experiment would be negative and useless unless properly carried out on a sufficiently large scale and over a large period of years. Heavy iron fencing would be absolutely essential to withstand bush fires, termites, and the depredations of large game, and even natives. I consider that a minimum of 50,000*l.* (fifty thousand pounds) would be necessary to ensure the success of such an experiment.

Mr. R. PASKE-SMITH, District Commissioner, Uganda.

This evidence refers to the Uganda Protectorate only, and is from notes and reports made while carrying out the duties of officer of sleeping sickness preventive measures. It is therefore confined solely to the trypanosome infections affecting man. As I have not been employed on the above duties for the past 18 months, the evidence will not, I fear, be of any great value.

Terms of Reference to Committee, sections 2 and 3.

2. A definition of the word "game" appears necessary when considering whether it is necessary and feasible to carry out an experiment of game destruction. If under "game" all mammals and non-mammals are referred to, it would, in my opinion, be impossible to carry out effectively any such experiment as the destruction of game in Uganda, owing to the dense vegetation and forest in, and adjacent to, fly areas, where the game would find a safe refuge.

3. If only antelope, buffalo, &c., are included under the head "game," the experiment appears in a more feasible light. The destruction of these animals could be carried out by hunters engaged for the work, or by the native population. The cost of an experiment carried out on these lines would, of course, depend on the amount of game in the area selected for the experiment. A few cents for each head killed would be sufficient inducement to natives, and it would be possible in some districts to organise large hunting parties of the natives.

4. In selecting an area in the Uganda Protectorate suitable for an experiment, the distribution of game in

and adjacent to fly areas, the range of the game, and extent of fly areas require consideration. Antelope are plentiful in all the fly areas in Uganda, with the exception of the north shores and islands of Lake Victoria. Of these areas, the most suitable for such an experiment would possibly be that part of the "Katwe infected area" lying to the north of Lake Edward and the Kazinga channel, and to the west of Lake George, and backed by the foothills of the Ruwenzori Mountains. The country comprises open plains dotted with light scrub, and is easy to traverse and hunt in. Game (cobus cob, reedbuck, waterbuck, buffalo) is plentiful. Various fly—*palpalis*, *morsitans*, *pallipes*, and *fusca*—are found within this area. Dr. Van Someren also found trypanosome infection affecting the stock in and near this area.

5. The area lying along the north shores of Lake Victoria would also be suitable for an experiment, but owing to the scarcity of game the value of any conclusions arrived at would be doubtful. The scarcity of game in this area even now, four years after the population was removed from the shores of the lake, would suggest that game has played little or no part in the maintenance and spread of trypanosome infections of man in these areas, where the ravages of sleeping sickness have been greatest.

6. Under existing conditions the destruction of game as a preventive measure against the maintenance and spread of sleeping sickness in Uganda does not appear necessary. *Glossina palpalis*, I believe, the sole carrier in this Protectorate of trypanosome infection of man. The areas in which this fly is found

are known and defined. These areas are declared infected areas and persons are prohibited from entering or communicating with them. Provided, therefore, that these areas, which generally comprise a two-mile strip of country along the foreshores of the lakes and rivers inhabited by *Glossina palpalis*, are maintained as prohibited areas, there can be no risk of infection to man, there being no carrier of infection outside the limits of these areas to which the game would act as a reservoir.

Terms of Reference, section 4.

7. The extermination of fly, *Glossina palpalis*, can, I submit, be considered as an alternative and possible measure for controlling the disease affecting man. From observations made while in charge of preventive measures, I am of the opinion that many of the infected areas in Uganda could be rendered fly-free by extensive clearing. In addition to any beneficial result in controlling the maintenance and spread of sleeping sickness, the clearing of the foreshores lying along the north of Lake Victoria should be of considerable economic value, as valuable plantation land at present lying within the infected and prohibited areas would be reclaimed and populated.

8. That the extermination of fly by clearing measures is feasible is suggested by the success of the clearing operations which have already been carried out at the ports and ferries on the lakes, and also by the extreme clearing carried out lately on the River Nile between Masindi Port and Foweira, where the banks of the river have been rendered fly-free over a length of about 60 miles, the actual fly area on this stretch of river totalling about 22 miles in length and varying in width from 5 or 6 yards to 200 yards where the forest area is dense.

9. It is true that fly have been caught within the cleared areas at the ports from time to time, but there is, I think, usually an explanation, the fly following natives and others from adjacent fly areas, or being brought in by dhows. The dhows sailing on the lake are, and will be, a source of danger while the areas on the shores of the lake remain infected. They are often driven into dangerously infected fly areas by stress of weather, proceeding afterwards to one of the cleared and fly-free ports. It remains, therefore, to be proved, I submit, that fly will reappear if clearing operations are effectively carried out over extensive areas along the foreshores of the lakes and rivers. The extermination of fly by extensive clearing operations has, I believe, already been carried out in German East Africa with success, and similar methods have proved successful against other insect carriers of disease in Panama and elsewhere.

10. If the extermination of fly can be carried out successfully by the clearing of forest, bush, and scrub from the foreshores of fly areas, the question arises as to how long it would be necessary to maintain the clearings, and whether the vegetation could be allowed to grow up again after a period of time. An experiment on these lines might be carried out on one or more of the isolated islands on Lake Victoria. The foreshores of two islands should be cleared when fly are plentiful and the clearing maintained until the fly

is extinct, and the vegetation then be allowed to grow at different periods on the two islands after fly has disappeared. It could then be seen if fly will reappear. It would be necessary to take strict precaution against unauthorised communication with the islands while the experiments are being carried out.

11. An experiment on a larger scale could be carried out on the mainland in the fly areas lying along the north shores of Lake Victoria between Kampala and Jinja. The coast line here is regular, the fly areas are distributed at intervals along the coast, and a few only comprise dense forest.

12. I regret I can give no figures as to cost of clearing in these areas. A careful, systematic examination of the foreshores and forests in the fly areas would have to be made before even a rough estimate could be prepared. The original cost of the clearings carried out on the River Nile near Masindi Port was estimated at 3*l.* per acre, and the maintenance for the following year at about 1*l.* per acre. After this period the natives were expected to keep up the areas, much of the land cleared being suitable for growing crops on. The areas cleared here were, however, very favourable for such an experiment, the fly areas being few and far apart and covered with light scrub for the most part, there being little heavy timber to be cut.

13. The cost of clearing on the foreshores of the islands on Lake Victoria would be quite double this estimate, owing to the higher rates of wages ruling in these districts and the heavier vegetation and forest lying on the shores. It would be on this account necessary to carry the clearings further back from the shores than was the case on the Nile.

14. A scheme for clearing could possibly be carried out in connection with the supply of fuel to the railway steamers, by which the original cost of clearing would be met, or nearly so. This has already been done on the Entebbe peninsula, where a trading firm was granted the timber rights under conditions that the foreshores of the lake were effectively cleared of all bush, scrub, &c. In some areas where clearing would be necessary the timber cut would be suitable for building and other purposes.

15. No scheme of clearing on an extensive scale could be carried out in a short period, owing to cost and the heavy demands that would have to be made on the labour supply in the Protectorate, which is already scarce. There should, however, be no difficulty in providing a scheme for the gradual clearing of the foreshores of Lake Victoria on the lines suggested.

16. The banks of the River Nile between Lake Albert and Nimuli would also be suitable for such an experiment, and in these districts the cost of maintaining the cleared areas would, I think, be found to be small. I have noticed that on the banks where the Marine Department has cut fuel for the steamers the forest, scrub, &c. has not grown again, leaving a clean grass bank. This is probably due to the long dry season experienced in this part of the Protectorate. Further north, in the Sudan, this is still more noticeable. I was pointed out long stretches of clear grassy banks, and was told that these were at one time covered with thick scrub, which had been cut for fuel.

Professor J. W. W. STEPHENS, M.D., D.P.H., The University, Liverpool.

In attempting to answer the issues raised by questions 16* and 17, it will be necessary to define as precisely as possible the terms we are using. I shall consider only those trypanosomes which have been or may be mistaken for *T. gambiense* or *T. rhodesiense*. They may be classified in the following way:—

(A) *Monomorphic trypanosomes*, i.e., all trypanosomes, whether long or short, which have a free flagellum.

1. *T. brucei*, Plimmer and Bradford, 1899 (or, as it may be called, *Zululand brucei*).

There are two opposite views as to what trypanosome is signified by this name:—

(a) that it is a monomorphic trypanosome.

(b) that it is a dimorphic trypanosome.

(a) The name *T. brucei* was given in 1899 by Plimmer and Bradford to a trypanosome in a dog brought to England in 1896 that had been inoculated in Zululand with trypanosomes derived from native game (what particular species is not known). All figures and descriptions of this trypanosome published in England refer to a monomorphic trypanosome. The trypanosomes in old slides made years ago in England are monomorphic. Stephens and Blacklock further studied this trypanosome carefully and found that it

* See Appendix A.

was *monomorphic*, and hence they conclude that the name *T. brucei* is applicable solely to this *monomorphic* trypanosome derived from Zululand. To indicate this fact the term *Zululand brucei* is convenient in order to avoid confusion.

(b) On the contrary, Bruce and others have figured trypanosomes which are *dimorphic*, from some of the old slides made in Zululand from the blood of the horse, donkey, ox, monkey, dog, but they bring no evidence to show that these slides were made from the same strain of trypanosomes as that used for inoculating the dog sent to England. We should remember in this connection that Bruce had inoculated dogs from a variety of wild game, viz., wildebeeste, kudu, bushbuck, and buffalo. Bruce and others conclude that the trypanosome from Zululand is identical with the trypanosome from the Uganda ox, which is admittedly *dimorphic*.

The most important fact, however, is that the trypanosome named *T. brucei* by Plimmer and Bradford is still kept alive in Liverpool and that it is *monomorphic*. Hence no *dimorphic* trypanosome can be called *T. brucei*, and therefore the *dimorphic* trypanosome from the Uganda ox called *T. brucei* by Bruce should not be so called. Stephens and Blacklock, who also worked with this trypanosome from the Uganda ox, have suggested the name *T. ugandæ* for it, or, if people are loth to use new names, it should at least be qualified as "*T. brucei*, Uganda." We have then two *different* trypanosomes:—

- (a) A *monomorphic* trypanosome *T. brucei*, Plimmer and Bradford (*sens strict*) or *T. brucei*, Zululand.
- (b) A *dimorphic* trypanosome *T. ugandæ*, Stephens and Blacklock or "*T. brucei*, Uganda."

2. *T. evansi*, Steele, 1885. This is a *monomorphic* trypanosome occurring in cattle, horses, &c., in Africa and India. It cannot be distinguished *morphologically* from Zululand *brucei*, but according to Laveran it can be distinguished by biological tests (*vide infra*), so at the present time some authorities consider that *T. evansi* and Zululand *brucei* are *identical*, others that they are not.

3. *T. equiperdum*, Doflein, 1901. The cause of dourine in horses is a *monomorphic* trypanosome. It cannot be distinguished *morphologically* from Zululand *brucei* or *T. evansi*, but it can be distinguished *clinically* from the symptoms of dourine and by the fact that it is transmitted by coitus and not by biting flies (*vide also* under *T. equi*). Neither of these three trypanosomes could, however, be mistaken either for *T. gambiense* or *T. rhodesiense*, so we may eliminate them from further consideration.

(B) *Dimorphic trypanosomes*, i.e., those which not only show long and short forms with a free flagellum, but in addition short or stumpy forms without a free flagellum.

(a) *Without posterior nuclei* (i.e., nuclei near to or even at the posterior end instead of in the middle of the trypanosome).

4. *T. gambiense*, Dutton, 1902, in man.

5. *T. multiforme*, Kinghorn and Yorke, 1912, in bushbuck, Rhodesia.

6. *T. nigeriense*, Macfie, 1913, in man; and others unnamed.

These three trypanosomes are easily distinguished by their morphology from the *monomorphic* trypanosomes of the previous group, and the *dimorphic* trypanosomes of the next group, but whether they represent three or more species or only one is at present unknown.

(b) *With posterior nuclei*.

7. *T. pecaudi*, Laveran, 1907.

8. *T. equi*, Blacklock and Yorke, 1913 (syn. *T. equiperdum pro parte*).

9. *T. ugandæ*, Stephens and Blacklock, 1913.

10. *T. rhodesiense*, Stephens and Fantham, 1910 (syn. *T. brucei vel. rhodesiense*); and others unnamed.

The question of the identity or non-identity of the members of this last group is a difficult one. Broadly speaking, there are two kinds of tests by which we can distinguish trypanosomes: (1) morphological, (2) biological.

(1) Morphological.

If we accept morphological identity as constituting specific identity then all these trypanosomes (7-10) are identical, but certain objections to this view may be mentioned.

1. The trypanosome of the rat (*T. lewisi*) and the trypanosome of the hamster (*T. rabinowitschi*) are morphologically identical, but it is generally admitted that they are specifically distinct.

2. If dourine is really produced by the *dimorphic* trypanosome *T. equi*, then, as *T. rhodesiense* is morphologically identical with it, they are one and the same trypanosome, which is a *reductio ad absurdum*.

3. If all these trypanosomes with posterior nuclei morphologically identical are specifically identical, then it is difficult to understand—

(a) Why sleeping sickness of the *rhodesiense* type does not occur in West Africa, where *dimorphic* trypanosomes with posterior nuclei occur.

(b) Why it does not occur in the French Sudan, where *dimorphic* trypanosomes with posterior nuclei (*T. pecaudi*) occur.

(c) Why it does not occur, so far as is known, in Zululand, where morphologically identical trypanosomes occur.

(2) Biological.

These are open to criticism and are by no means universally accepted, but I consider cannot be entirely set aside, for in no case have the biological tests asserted *identity* where the trypanosomes are distinguishable, e.g.:—

—	Morphologically.	Biologically.	Authority.
<i>T. rhodesiense</i> and <i>T. gambiense</i> .	Distinct	Distinct	Mesnil and Ringenbach.
<i>T. brucei</i> , Zululand, and Uganda <i>brucei</i> .	Distinct	Distinct	Mesnil.

On the other hand, there is discrepancy between the morphological and the biological tests in the case of trypanosomes morphologically *indistinguishable*, e.g.:—

<i>T. lewisi</i> and <i>T. rabinowitschi</i> .	Indistinguishable.	Distinct	Various.
<i>T. brucei</i> , Zululand, and <i>T. evansi</i> , Steele.	Indistinguishable.	Distinct	Laveran.
<i>T. pecaudi</i> and <i>T. ugandæ</i> .	Indistinguishable.	Distinct	Mesnil and Leger.
<i>T. rhodesiense</i> and trypanosome of mule, Rovuma G.E. Africa.	Indistinguishable.	Distinct	Beck.
<i>T. rhodesiense</i> and Uganda <i>brucei</i> .	Indistinguishable.	Developmental in proboscis, developmental in salivary glands of tsetse-fly.	Roubaud and Bouet.

It will be evident from what has preceded that no proof acceptable to various authorities can be brought forward at present as to the identity or non-identity of trypanosomes morphologically indistinguishable. Nevertheless, we know that where *T. gambiense* exists trypanosomes indistinguishable from *T. gambiense* exist in the native game, and in regions where *T. rhodesiense* exists trypanosomes indistinguishable from *T. rhodesiense* also exist in the native game. Further, both these trypanosomes are communicable to animals by inoculation and by means of the bite of tsetse-flies, so that although strict proof is not at present forthcoming, yet we believe there can be little doubt that in each case we are dealing with the same trypanosome in man and in the native game.

Summary.

1. As no monomorphic trypanosomes have so far been found in man, the three trypanosomes, *T. brucei*, Zululand, *T. evansi*, and *T. equiperdum* can be left out of consideration. These constitute the true "brucei" group.

2. The dimorphic trypanosomes can be divided into two distinct groups:—

(a) those without posterior nuclei: *T. gambiense*, *T. multiforme*, *T. nigeriense*. These constitute the "gambiense" group.

(b) those with posterior nuclei: *T. pecaui*, *T. equi*, *T. uganda*, *T. rhodesiense*. These constitute the "pecaui" group.

It is impossible at present to say with certainty whether the members of each group are one and the same species or not.

3. Although the proof is not absolutely complete, it is practically certain that *T. gambiense* and *T. rhodesiense* of man are each identical with the trypanosome found in the native game in the regions where these respective trypanosomes occur.

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McGill University, Montreal.

HUMAN TRYPANOSOMIASIS.

*1-2. In Africa, in England, and in Canada. 1902-1903, Gambia; 1903-1905, Congo; 1905-1907, Runcorn Research Laboratory; 1907-1914, McGill University, Montreal, Canada. Observations, as publications show, were concerned with the disease from clinical, research, and administrative standpoints. So far as my observations are concerned, many of the questions can be best answered by reference to my publications, many of which appeared in the publications of the Liverpool School of Tropical Medicine.

3. (?)

4. Memoir XVIII., Liverpool School of Tropical Medicine (L.S.T.M.).

5-6. Untreated, declared trypanosomiasis is probably always fatal, within, at the most, about four years. The incidence of the disease varies greatly. In some persons it seems as though it must be possible for individuals to possess a relative tolerance to infection. However, these points can only be decided by the observation of a large number of persons in a locality, such as the Gambia, where trypanosomiasis exists in a less acute form than it does in pandemic centres.

7. Memoir XVIII., &c. I know of no reason why trypanosomiasis may not spread and become endemic wherever transmitting agents exist.

8-9. Yes; Memoir XVIII., L.S.T.M.; Annals, L.S.T.M., V. 2.

10. Natives often conceal cases from widely varying motives. Natives are like children; tact and consideration will accomplish anything with them. I can only remember two villages in which it was absolutely impossible for us to make all the examinations, from pricking of fingers to lumbar punctures, that we wished.

11. (See various publications.) Our experience was that trypanosomes may sometimes be very much more numerous in the peripheral blood than is usually taught.

12. There is a slight rise, irregularly concomitant with the rise in temperature; otherwise, I know of no quantitative or qualitative periodicity.

13. Natives often say that infection is greater in the rains—most probably because poor feeding and cold at that time lower individual resistance, so that existing and possibly new infections find immediate expression in symptoms.

14. No.

15. Until the contrary is proven, for efficient prophylaxis the assumption should be that all biting flies are capable of transmitting trypanosomiasis.

16. My opinion of the factors of differentiation upon which the classification of trypanosomes depends, is expressed on page 263, Vol. 1, No. 2, Annals, L.S.T.M. Though somewhat modified by later refinements of the three means of differentiation, my opinion remains much the same as it was.

17-18. I have little personal experience with the appearance of trypanosomes in flies. By comparison, through the various means of differentiation, the human trypanosomes can probably be distinguished with fair accuracy from the trypanosomes which are usually parasitic in animals.

19. I have never seen pathogenic trypanosomes in persons permanently resident in places where there was no large game and no tsetse-flies.

20. It has been proved that it is possible for human trypanosomiasis to be transmitted, mechanically, by the bites of and contact of Diptera other than *Glossina*, and, also, there is a possibility of infection being produced when infected blood touches unbroken skin or mucous membranes. The existence of numerous cases in places where flies are scanty, furnishes room for doubt that transmission by *Glossina* can be the only method by which the disease spreads. Page 213, Vol. 1, No. 2, Annals, L.S.T.M.

21. Our experiments were negative. Page 213, Vol. 1, No. 2, Annals, L.S.T.M.

22-23. There seems to be variation in natives' susceptibility. Proof can only be given through the observation of persons known to be infected over long periods. The cases examined by us in the Gambia, have been kept track of, up to the present, by the Commissioners in the district of the Gambia, and it is very much hoped that they may continue their observations in order that a partial answer to this question may be obtained. Our experience led us, in Liverpool, to believe that men might become relatively tolerant.

24.

25.

26. Their importance probably varies with the local conditions, but the history of the spread of human trypanosomiasis in the Congo Free State, leaves no room for doubt that the introduction of infected persons to districts where trypanosomiasis apparently did not exist, has been the starting point of pandemics of that disease.

27. I have not made the experiment.

28. Experimentation might throw light on this question. My personal leaning is that, while some species are more or less fixed, each of them may vary considerably in character.

29-33. Trypanosomiasis was common in animals. Nothing was known of its origin. There is a great deal of cattle in the Gambia, and a fair amount in some parts of the Congo Free State. If cattle is well cared for, the percentage of animals infected seems to be small, and a certain amount of tolerance seems to have been acquired by them. The mortality, especially among young animals, is great. A systematic examination of the cases was made in the localities visited. See page 233, Vol. 1, No. 2, Annals, L.S.T.M., and Memoir 11, L.S.T.M.

34-5. Usual African fauna. The records of the wild animals examined in Africa by us can be found in Memoir 11, Memoir 21, and Vol. 1, No. 3, of the Annals, L.S.T.M. The fauna of the West Coast and Central Africa is not nearly so luxuriant as it seems to be in the South Central and Middle West Africa.

36. All of the wild animals, infected with trypanosomiasis, which we killed, seemed to be in good health.

37. I have seen no part of Africa from which I would dare to say that tsetse-flies were always absent.

38. As a rule, rare. Sometimes, as in one buck at Kasongo, they may be quite numerous.

39. I believe that my results are not in entire accord with those of other observers.

40. I have every reason to be satisfied that the published papers of Messrs. Kinghorn and Yorke, and of many who preceded them, are the truthful, recorded results of accurate work. I therefore believe that it is possible that wild animals may act as reservoirs, from which domestic animals certainly, and perhaps man, may become infected.

* The figures at the beginning of the paragraphs refer to the questions in Appendix A.

41. Human trypanosomiasis seems to be one of those diseases which cannot be controlled by any one method. I believe, in most places, that it can be successfully combatted by a combination of many methods. They will be the same in principle, whether they are destined to prevent human or animal trypanosomiasis. The precise form which the combination of methods is to take, will depend entirely upon the local conditions existing in the localities to be treated. (See "The Prevention of Human Trypanosomiasis in Africa," Trans. XV. International Congress on Hygiene and Demography, Washington, D.C., 23rd-28th September 1912). The only difference between the methods to be employed will be that the measures employed in the prevention of cattle trypanosomiasis can be much more radical than is possible in the prevention of human trypanosomiasis.

42. As far as I am aware, nothing has been done in the Gambia, and I have no information, beyond that which has been published in reports in medical journals, concerning the results of the preventive measures, which were advised and adopted for the prevention of trypanosomiasis in the Congo Free State.

43.—(1) As a preventive measure, neither desirable nor feasible. As a result of extending colonisation, evidently more or less inevitable.

(2) Both desirable and feasible. Desirable because the game furnishes a reservoir for various forms of piroplasmiasis and trypanosomiasis, at least (and probably also of other diseases). (Whether the Caucasian colonisation of Africa is to come as a result of the development of domestic animals, immune to African diseases, or as a result of combating, by various means, the diseases which make Africa impossible for most European animals, is a large question. At present the weight of opinion seems to be, that the diseases must be combatted.) I believe that it would be possible to find an area in which the game could be killed or sufficiently to ascertain whether its extermination had any effect of lessening the amount of trypanosomiasis. The details of the experiment,—the place in which it should be carried out, the personnel which would find employment in carrying it out, the way in which they would work, the time which would be necessary for its completion, and its cost, are all matters which must be left to the decision of administrators and their advisers. I am (Lancet, Nov. 22, 1913) entirely in favour of the institution of such an experiment. In spite of recent publications, which seem to tend to invalidate some of Kinghorn's and Yorke's conclusions, I think that their recorded results find their logical sequences in such an experiment, and make that experiment necessary to an administration which wishes to derive full benefit from the work of their commission.

(3) The German reports seem to indicate that clearing operations may be exceedingly useful. They should always be used in protecting individual buildings, or collections of buildings. The exact extent to which they can be used must depend upon locally varying economic and topographical conditions.

(4) I have no personal experience.

(5) Desirable for treatment and for the isolation of the source of infection. (However unsatisfactory the records of natives in sleeping sickness camps may be, there is no doubt that several cases of trypanosomiasis in Europeans have been cured.) Experiments show that it is possible, many cases, to persuade the natives to send infected persons to villages for detention and isolation, and that it is possible to make such villages largely self-supporting.

(6) Destruction of infected stock might be possible and advisable in districts where cattle is owned by Europeans and imported stock is being brought in. I think that more must be learned concerning the course of trypanosomiasis in stock before generalised destruction of native's cattle could be advised.

(7) I know of no efficient method of destroying tsetse-flies.

44. My belief in the desirability of attempting to destroy large game in a small area has been stated. It is hoped that the normal incidence of disease, especially of piroplasmiasis and trypanosomiasis, among non-immune and uninfected cattle, existing or imported

into that district, would be greatly lessened, and much less than it would be in similar districts where extermination of large game had not been carried out. If such a result were obtained in a comparatively small area, it would be distinctly worth while to initiate a similar policy over a larger area. I think that the experiment could be best carried out in some locality probably in southern Central Africa, where Europeans have commenced to settle, by removing game laws, and possibly by importing hunters. The game would disappear, simply by hastening a process which is inevitable if permanent resident European colonisation in tropical Africa is successful. I scarcely think that a fence would be necessary if the area selected were sufficiently large. My impression would be that it would be useless to attempt the experiment in an area less than 50 or 100 miles in diameter. The amount of game that would drift in from the periphery to the centre of such an area would be negligible. The staff required for the experiment would vary greatly according to the locality in which the experiment is carried out. The superintendent, a qualified medical man, who had special knowledge of trypanosomiasis, with his assistant and entomologist, might find opportunities, while engaged in the conduct of the experiment, to follow much-needed studies concerning, for example, the bionomics of tsetse-flies and the many questions concerning trypanosomes and their development and transmission, which can only be settled by observations made in the field. The cost of the experiment can only be determined by those who have local knowledge of the locality in which the experiment is to be carried out. A moment's thought of the economic value, and the far-reaching results, of the experiment, should it be successful, should be sufficient to convince one that a considerable expenditure would be justified in carrying it out. The details of organisation and administration of the experiment, must depend upon local conditions. My experience is that natives, for the sake of the meat, would be delighted to assist European hunters by all the means in their power. In the Congo Free State soldiers are often sent out, singly or in pairs, with a limited number of cartridges, say five, to hunt. A strict proof of the way in which every cartridge is expended is demanded of them. Could not native forces be used in some such way in British territory? If the locality in which the experiment were carried out were all accessible, the number of "sportsmen" who would come into a game country, without game laws, would not be negligible. The length of time which would elapse before conclusions could be made from the experiment would again depend upon local conditions. I should imagine that an idea of the final results might be obtained after two or three years, but that from four to five years would be necessary before the experiment could be accepted as conclusive. Yorke and Kinghorn's papers have, to my mind, successfully answered all of the objections which can be suggested against the proposed experiment; it would be part of the work of those engaged in the conduct of the experiment to safeguard, by their observations, against every source of fallacy.

45. I know nothing more concerning the treatment of trypanosomiasis than has appeared in medical journals.

46. Most decidedly, more information is needed.

47. There is hardly any point, connected with the bionomics of tsetse-flies, of which one can feel that knowledge is at present sufficient and exhaustive. Food supply, factors governing distribution, mating habits, natural enemies and diseases, all require investigation.

48. Mosquitoes are successfully combatted through attacking them at a weak point in their life cycle. They require water to reproduce. It does not seem impossible that a complete knowledge of the bionomics of the tsetse-fly might reveal a weak point in their life cycle. At present their only weak point seems to be the slowness with which they reproduce. It might be well worth while for those in charge of a station placed in a district where tsetse-flies (*Glossina palpalis*) exist in considerable numbers to study the effects of the capture and destruction of a considerable number of

flies daily, over a period of at least eighteen months to two years.

49. No.

50.

51.

52.

53.

54. (See above.)

55-56-57. In the Gambia *Glossina morsitans* called Congo fang jolo, a black monkey fly, because the natives say the fly will usually be found near bands of baboons. On at least one occasion it seemed to us that the flies did follow the monkeys. I have only seen one place (Vol. 1, No. 1, Annals, Trop. Med. and Parasitology, L.S.T.M., p. 62), in which tsetse-flies were certainly absent over a considerable period, although I have heard of, and seen, other places where the flies were either extremely scanty or said to be absent. I have sometimes wondered whether a disease

of the flies might not account for their absence at such places. It would be exceedingly interesting to determine the limits of such an area, and to examine the flies caught at its edges.

58.

59. I repeat, several of the questions asked in this list can only be answered by experiments undertaken in the field by competent persons; and from their nature, answers to those questions can only be obtained by constant observations made in the field by competent observers whose work extends over a period of at least two years. The determination to carry out experimental extermination of game over a comparatively small area will necessitate the appointment of a staff of specially trained workers to secure its proper execution. Their necessarily prolonged residence in Africa would afford them an opportunity for investigation, and for finding answers to some of the questions.

Part IV.—Written Statements by Gentlemen who also gave Oral Evidence.

Mr. LL. E. W. BEVAN, M.R.C.V.S., Veterinary Bacteriologist, Southern Rhodesia.

I must explain that I have only had a cursory view of Surgeon-General Sir David Bruce's recommendations, and have had no personal experience of the disease in the country in which his work has been carried out.

As far as Rhodesia is concerned, I am of opinion that the gravity of the situation has been greatly exaggerated. In spite of the statements of certain experts that a very high percentage of game acts as reservoirs of a virus—admittedly an exceptionally virulent one—which can be transmitted with ease by the common tsetse-fly (*G. morsitans*), local conditions obviously being favourable, we find from Dr. May's report that the number of cases detected in human beings has decreased since 1909.

It is at last recognised that the feature upon which the Liverpool experts based their opinions that the casual trypanosome was a distinct species is met with in other and well-known and widely distributed species, and I (as well as Bruce) was able to demonstrate to the workers in the Pretoria Laboratory that it was present in a strain of *T. brucei* obtained in the first place from Zululand, where this trypanosomiasis has in all probability existed for years without, as far as we know, causing any appreciable mortality in human beings.

Therefore, as far as Rhodesia is concerned, I consider that, although it is desirable to take active measures to eliminate this disease, these should not be of a "panic" nature nor so drastic as to draw unfavourable attention to this Colony, which under present conditions is making satisfactory progress; and I am confident that, with the assistance of expert advisers, the medical officers of these Colonies, acting upon a practical rather than a theoretical knowledge of the conditions which prevail, are competent to deal with the situation.

Fencing.—I do not know the nature of the experiment which it is proposed to conduct, but a considerable experience of fencing in Southern Rhodesia leads me to think that any experiment which depends upon fencing to control the movements of game is likely to fail.

Only recently a scheme to fence the southern boundary of Southern Rhodesia to prevent the illicit introduction of cattle from the south had to be abandoned, although many miles of fence had been erected, because it was found that not only the larger

wild animals but also herds of small game could break down with ease the stoutest fence.

Also, in a broken country it is almost impossible to satisfactorily lay the fence over rocks and kopjes and across sluits and waterways.

The cheapest form of fence in any degree reliable will not cost less than 50*l.* a mile.

Destruction of Game.—Here, again, I can only venture to express an opinion on the question as far as it relates to Rhodesia.

Any drastic measure or attempt at wholesale destruction of game is likely to cause adverse criticism at home as well as in the Colony. My experience of organised "shoots" is that they have always proved unqualified failures. It is true that the game may be driven from a district for the time being, but it returns when the hunters have withdrawn.

In Southern Rhodesia the "Game Law Consolidation Ordinance, 1906," provides for the destruction of certain animals under certain circumstances, and until recently (31st March 1913) Government Notices No. 216 of 1911 and No. 387 of 1911 provided for the payment of rewards for the destruction of wild carnivora.

From the observations of several observers in different infected areas it has been found that the animals in which the offending trypanosome has been most commonly met with are water-buck, hartebeeste, reed-buck, and wart-hog, and rarely in a few others.

I would therefore suggest that war should be waged upon these animals in fly areas by placing them in the open class under the Ordinance, and by offering a reward for every pair of horns (or tusks) brought in by hunters (native or European) to certain Government officials authorised to pay them.

If this were carried out, and natives were informed by their Commissioners of this new source of income and urged to avail themselves of it, I am confident that this source of danger would rapidly diminish.

This method, in my opinion, is less likely to give rise to any opposition and disturbance than more drastic measures, which are calculated to defeat their own ends.

I wish it to be understood, however, that this is merely my own personal opinion, which may not coincide with that of the medical departments of Rhodesia, with which I am not conversant, but which should be obtained through official sources.

6th May 1913

Mr. LL. LLOYD, B.Sc., Entomologist to the British South Africa Company in Northern Rhodesia and lately to the Luangwa Sleeping Sickness Commission.

The evidence will refer only to *Glossina morsitans*.

Question 44.—This experiment, or an equivalent one, is necessary to prove or disprove the dependence of the fly or the larger mammals. In the event of the

dependence being proved, the destruction of the game animals over other areas would be justifiable, provided that no other means of prophylaxis were available in the meantime. The experiment is quite feasible, but difficult.

It could be carried out in the Lukasashi Valley of Northern Rhodesia, an area which has been examined and reported suitable. The area would be approximately 20 miles long and from 6 to 10 miles broad. No slaughter would be necessary, as the animals could be driven out. A fence would be necessary, but one of wire would be useless. A stockade, 10 feet in height, would have to be made around the area. Two villages would have to be removed from the area. Observations should be continued for two years, and a very thorough knowledge of the habits of the fly, such as seasonal migration and fluctuation in numbers, should be obtained before the experiment was commenced.

In the event of the fly disappearing, it would not be certain whether it had died out or migrated from the area. The difficulty of driving out all the small antelope, pig, baboons, and carnivora would be great, and there would be a tendency for the larger mammals to return, necessitating constant patrolling and repairing of the fence.

An alternative experiment is suggested. A fly-proof cage, of dimensions 100 yards long by 50 yards broad by 7 feet high, would be constructed and stocked with the type of fauna which would remain if the larger mammals were eradicated. Large numbers of tsetse would be introduced, and observations made as to the increase or decrease of the fly. In the event of the fly being unable to support itself on such a fauna, a number of small antelopes, goats, or sheep would be introduced and the experiment repeated. Such an experiment should show whether the fly is dependent on the larger animals. The two experiments would occupy a period of two years and three months, but in the event of the first being conclusive, 15 months only would be required. The advantages of this experiment over the fencing experiment are:—

The cost would not be prohibitive.

Control would be simpler.

Preliminary observations would not be necessary.

In the event of the disappearance of the fly, definite information as to what had become of it would be obtained.

Questions 49-52.—The breeding places of *G. morsitans* on the Congo-Zambesi watershed are now well known. They appear to have a close association with the game and human paths, fords, &c. (A diagram illustrating this will be produced and statistics given.) The one character common to all the breeding places found is that, above the spot where the pupae lie, there is always some relatively dark place where the mother fly could be concealed during pregnancy. The larvae are not necessarily dropped in mould or other positions where they can burrow, but sometimes on hard bare ground, where they lie exposed. The breeding places are: under fallen trees or branches, in holes in trees, in animal burrows, and in salt licks in ant hills. There appears to be a definite breeding season.

Question 53.—The number of female flies in an indiscriminate catch has been known to be as low as 2 per cent. and as high as 40 per cent. This phenomenon is believed to be related to the mating habits. The male flies accompany a moving object or animal

in the expectancy of the female flies rising to feed, when an opportunity of mating is given. The numbers of the sexes are probably equal in any area.

Question 54.—Experiments in trapping *G. morsitans* by this method at Nawalia proved useless. I know of no other proved method of trapping the flies.

Question 55.—No. There is probably no area in Northern Rhodesia where game is not present.

Proposed question on the relation of *G. morsitans* to small mammals, birds, and reptiles, and the effect of such bloods on the fly.

G. morsitans is willing to feed on any of these. Whether it is able to do so or not depends on the agility of the animal. The majority of small mammals and many birds are nocturnal, and sleep in the hiding places which *G. morsitans* haunts. The fly would probably feed upon them when they are asleep.

The flies are unable to breed on a diet of reptilian blood, and soon die if fed upon it in the laboratory. They are able to live and breed on a diet of avian blood, but this appears to be less digestible than a diet of mammalian blood. The effect of this is that the undigested blood causes pressure on the developing larva, and those produced are, on the average, 4.5 per cent. smaller than those bred on mammalian blood. This does not necessarily obtain in nature, as laboratory conditions are very artificial in this respect.

Question 56.—It may be possible to turn to account the habits of the fly of breeding close to the paths and fords. The experiment would consist in the removal of all potential breeding places within two or three hundred yards of a piece of road. These would be replaced by logs of wood at frequent intervals, and slightly raised, which would be coated on the lower surface with bird lime. When the female flies searched for places in which to hide or deposit pupae, it is thought that they would possibly be caught by this method. The experiment would be commenced on a very small scale, and only extended if likely to be beneficial.

Question 57.—There are several such areas on the Congo-Zambesi watershed, surrounded by similar country in which fly occurs. The absence of fly from these is probably a fortuitous circumstance. If the present favourable conditions to the fly continue, these areas will be probably invaded by it. The fly appears to spread very slowly.

Question 58.—In the Luangwa Valley, in Northern Rhodesia, through the dry season, there is a tendency for the fly to move towards the main streams. In the early part of the dry season, fly is very rarely seen in the immediate neighbourhood of the main river. The large mammals make a similar migration to the rivers as the surrounding country dries up, and the fly probably accompanies them. It is always found, however, in the districts from which the partial migration has occurred.

Proposed question on the parasites of *G. morsitans*.

A nematode worm and a dipterous parasite have been found in the fly in Northern Rhodesia. From marks on some of the pupae found in nature a third parasite (probably hymenopterous) is known to exist.

Mr. GUY A. K. MARSHALL, Director of the Imperial Bureau of Entomology.

In the case of human trypanosomiasis caused by *Trypanosoma rhodesiense*, the view that *Glossina morsitans* is capable of transmitting this organism is generally accepted. The next most important question is the status of the disease. In the "proclaimed area" in Nyasaland the number of cases has averaged approximately 1.25 per thousand annually over a period of four years. This and other considerations indicate that we have to deal with a disease which has been already endemic for some time and is not likely to become worse under present conditions. The hasty adoption of any dubious prophylactic measures is therefore to be deprecated.

The statement has been confidently made in various quarters that the only practical method of coping with this form of trypanosomiasis is by eliminating the larger mammals in the affected areas. This view

has been supported by two lines of argument:—(1) That *G. morsitans* would be unable to exist in the absence of game; (2) that the antelopes are the only significant reservoir of *T. rhodesiense*, and that upon their removal the fly would become innocuous.

With reference to the latter argument, the recent experiments of Dr. Taute indicate that the attempt to discriminate between *T. rhodesiense* and *T. brucei* by morphological characters only may be entirely misleading, and serious doubt is now thrown upon the statistics of infection by *T. rhodesiense* cited by Drs. Kinghorn and Yorke, and also upon the inferences which have been drawn from them. Further, if the disease is really endemic in Nyasaland and Rhodesia, as there is good reason to suppose, the probability that man himself may be a reservoir has to be considered; a point that has been strangely neglected

by local investigators. Indeed, it is by no means impossible that man may yet prove to be the *only* reservoir of this particular strain of trypanosome.

Thus the suggestion that antelopes constitute the most important reservoir of the disease remains for the present a mere hypothesis, which has yet to be established. Until that has been done, this suggestion affords no sufficient justification for the destruction of game, even upon an experimental basis.

The alternative contention in support of game destruction may be called the "starvation" argument. This is generally based upon the fact that in many areas *G. morsitans* is known to have disappeared, or to have been much reduced in numbers after the rinderpest in 1896. But data are available in some of the cases which indicate that the disappearance of the fly cannot be attributed to the absence of food. On the other hand, certain observers have maintained that *G. morsitans* is capable of existing in considerable numbers in areas where larger mammals are very scarce or apparently non-existent. This view finds support in the fact that the Luangwa Commission has recorded that, in a series of *G. morsitans* in the intestines of which blood could be recognised, no less than 15 per cent. contained nucleated cells, and this in a district where game is said to be plentiful.

The suggestion that the starvation hypothesis should be tested by a purely experimental eradication of game appears to be not unreasonable; but it presents certain practical difficulties. In the first place, it will be by no means easy to reproduce the necessary natural conditions, and at the same time to eliminate all reasonable probabilities of error; secondly, the cost will certainly be considerable, while the practical result is dubious. In any case, a policy of

wholesale game destruction can, at best, be only a partial palliative, and is so obviously unsound economically that it should not be contemplated except as a last resource.

Major J. Stevenson Hamilton has given a very circumstantial account as to the existence of an area in which *G. morsitans* was abundant in the absence of game. I would suggest that this area be again carefully examined, for, if his conclusions are confirmed, there would be no need for any slaughter experiment.

The main flaw in the position of those who strenuously advocate game destruction lies in the fact that they start out with the assumption that no other method of controlling the fly is possible. I submit that in a matter of this kind the only opinions that have any real value are those of men who have had actual experience in combating insect pests on a large scale. In its present stage the problem of tsetse control is essentially one for the entomologist to deal with, and there are various lines of research which might yield valuable results. Yet hitherto this aspect of the question has received but the scantiest attention, and adequate control experiments under skilled supervision have never been attempted in the countries under special consideration.

I would therefore urge that, before any final policy be adopted, and even before experimenting with game destruction, a comprehensive entomological investigation should be organised under the control of the most competent and experienced men procurable, in order to elucidate the various points in the life-history of *Glossina* concerning which we are still entirely ignorant, and a knowledge of which is essential for carrying out any rational measures of control.

MR. AYLMER MAY, M.D., Principal Medical Officer, Northern Rhodesia.

In connection with your request for a brief summary of the main points of evidence, I shall be prepared to offer evidence on any aspect of human trypanosomiasis as it occurs in Rhodesia, and suggest for your consideration that the following points are of special importance in this connection.

(1) The present position of the disease.

(a) Transmitted by *Glossina palpalis* (*T. gambiense*).

(b) Transmitted by *Glossina morsitans* (*T. rhodesiense vel brucei*).

(2) The failure of the disease to spread under conditions which, as far as our present knowledge goes, are most favourable, and which may be due to—

(a) Immunity—i.e., that the disease is an old one and the population relatively immune;

(b) That the extent to which game is infected with the *T. rhodesiense* may not be so great as is at present thought, the question being still unsettled as to whether the "fly" "game" and human trypanosomes are identical, though similar morphologically and in their animal reactions.

(3) The relationship of game to human trypanosomiasis.

The experimental determination of the relationship existing between game and fly, and incidentally between game and human trypanosomiasis.

A locality suitable for such experiment.

The cost of such experiment.

The correct interpretation of the results following such experiment.

The interpretation of the results following such an experiment will give rise to considerable difficulty. One or other of the following conditions will be found:—

(a) The total disappearance of the fly from the area cleared of game.

(b) A diminution in the amount of fly present.

(c) Unchanged conditions as regards the presence of fly.

(No importance is attached to the estimation of the effect on the infectivity of the fly, since there is at present no known method of identifying the trypanosome.)

(d) The disappearance of the fly as a result of the removal of game from a selected area cannot be taken

as a guarantee that similar results will follow such procedure carried out elsewhere. (A remarkable instance of the persistence of fly after the gradual extermination of game can be given.)

An experiment has been suggested with the idea of determining the relationship of fly to mammals, birds, and reptiles in an enclosed space under conditions approximating as far as possible those found in nature.

The general extermination of game is considered to be neither feasible nor desirable.

The local removal rather than the local extermination of game is considered to be both feasible and desirable. (This is recommended independently of whether or not game acts as a reservoir for the human trypanosome.)

The general removal of healthy natives from *Glossina morsitans* areas is not at present considered necessary in view of the absence of any indication of the spread of the disease.

Segregation of the sick is considered desirable under certain favourable conditions.

Pending the discovery of a means for the identification of the trypanosome, the destruction of infected stock is considered advisable.

It is considered that more information and extended entomological investigation is certainly required as to the bionomics of the tsetse, and in particular as to the following points:—

(1) The influence of the various sources of food supply on the breeding capabilities of the fly.

(2) A further study of the breeding places in relation to food, water, seasonal variations, &c., with the object of devising a means of attacking the fly with a full knowledge of their breeding habits and places. In this connection it is thought that a much more concentrated effort can be made at the beginning of the insect's life than afterwards.

(3) The reasons for the limitations of the fly to certain areas and their absence from other areas apparently in every respect suitable for their maintenance.

31st January 1914.

Professor E. A. MINCHIN, F.R.S.

I. Is the destruction of the wild game likely to have the desired effect, namely, the elimination of the "reservoir" of the trypanosomes affecting man and domestic animals? Some considerations from which it appears not improbable that the destruction of the game may leave matters no better, or even worse, than they were before.

1. The reservoir is contained in ruminants generally, not in the wild game alone; that is to say, domestic stock, including dogs (see *Ann. Trop. Med. Parasitol.* VII., pp. 235, 236), also harbour, or have been proved capable of harbouring, the human parasite. Consequently extermination of wild game would remove only a part of the reservoir. (Take, as an analogy, the Highlands of Scotland, with deer-forests and domestic cattle. If a human disease broke out there, caused by a microbe capable of living both in human beings and ruminants, and transmitted by means of a common blood-sucking fly, very little good would be effected by destroying the deer, so long as the cattle remained.) It has been argued that human beings and domestic stock do not form an effectively dangerous reservoir, because the trypanosome is so rapidly fatal to them. But even if an animal lives only a week with trypanosomes in its blood, that is quite time enough for it to infect a very large number of flies if they have access to it. (Note further, that the longer the disease has been established in a locality, the more its virulence tends to diminish, and infected animals or human beings tend to live longer.)

2. It is impossible to foresee at present with any certainty what effect the destruction of the big game may have on the distribution of the tsetse-flies. I am of opinion that the destruction of the natural food of the flies in the jungles may cause the flies to migrate towards human habitations, where they will be more effective than at present in spreading infection amongst human beings and domestic stock. To this suggestion it has been objected that human habitations are in cleared areas, where the fly will find no shelter. But in Uganda, at least, native huts are often in the midst of dense bush, and still more often surrounded by banana-plantations in which the fly swarms. (Note that the pupæ of *Glossina palpalis* were first found in nature by Dr. A. G. Bagshawe in the soil in banana-plantations.) It would be possible to order clearance of the jungle in such cases, but it would hardly be possible to order the destruction of the banana-plantations, which furnish the staple food of the natives.

I think, therefore, that the disturbance of the natural conditions of the tsetse-flies would be dangerous to human beings.

Conclusion.—If any destruction of the wild game is to be carried out, it should be done at first experimentally and tentatively in a limited area, in order to obtain accurate knowledge of the effects produced by such measures.

II. On the feasibility of measures for the extermination or reduction of tsetse-flies.

1. Generalities with regard to the life-history of tsetse-flies. The genus *Glossina* is viviparous, that is to say, the tsetse-fly does not, like the common house-fly, lay an egg from which is hatched out a maggot, which in its turn feeds, grows, and becomes, when full-fed and full-grown, a quiescent pupa. In tsetse-flies the egg is retained in the oviduct, which functions as a uterus; the egg hatches there, and the larva is nourished by the secretion of special uterine glands until full-fed. It is then extruded by the fly as a full-grown maggot, which wriggles about for an hour or so, buries itself or seeks shelter, and becomes a pupa, from which the adult fly emerges in due course.

Thus there are practically only two free stages of the tsetse-fly: (1) the adult fly, (2) the full-grown maggot and pupa.

2. Differences between the haunts and habits of *Glossina palpalis* and *G. morsitans*.

(a) *Haunts*.—It is a curious but well-known fact that *Glossina palpalis* always occurs in the vicinity of water, lakes, rivers, pools, &c. There is nothing known with regard to the habits of the fly to explain this peculiarity of its distribution. That mosquitoes, for instance, occur near water, is easily explained by the fact that the larval stages of these insects are aquatic. The viviparous tsetse-fly, on the other hand, is entirely independent of water so far as its reproduction is concerned.

G. morsitans, on the contrary, is not limited in its distribution to the vicinity of water, but occurs far from it where other conditions are suitable.

(b) *Habits*.—The knowledge which has accumulated in recent years shows that there are noteworthy differences between the breeding habits of the two species.

G. palpalis drops its larvæ on the ground and they burrow into earth and pupate there.

G. morsitans, on the other hand, appears never to scatter its larvæ on the ground, but always to deposit them in holes in trees, in sheltered places under tree-trunks, or in holes in the ground (Jack, *Bull. Entom. Research*, II., pp. 357-361; Lloyd, *Ann. Trop. Med. Parasitol.* VII., pp. 289-292.)

For these differences in the habits an explanation must be sought in the natural life-conditions of the two species.

3. *Birds as natural enemies of the tsetse-fly*.—It is, I think, doubtful, if the adult fly suffers much from the attacks of birds. There are probably but few birds which could capture a tsetse-fly on the wing. On the other hand, there can be no doubt that the pupal stage of the tsetse-fly is the most vulnerable part of its life-history, and that the pupæ are preyed upon readily by birds, especially by gallinaceous birds such as the guinea-fowl, francolin, &c., birds which have the habit of scratching up the ground in search of food—a habit easily observed in the common domestic fowl.* Jack (l.c.), whom Lloyd (l.c.) confirms, finds that all the ground under the bushes is scratched up by guinea-fowl and that it is useless to search for the pupæ of *G. morsitans* there; both these observers agree that the propensity of the fly to deposit its pupæ in trees and holes is an instinctive habit for protecting its pupæ against their worst enemies, the guinea-fowl and francolins. If this be a correct explanation of the peculiar breeding-habits of *G. morsitans*, why is *G. palpalis* able to deposit its pupæ in the ground with impunity? As mentioned already, this fly always breeds near water. Can it be that the vicinity of water is avoided by gallinaceous birds for some reason, such as perhaps the presence there of some foes dangerous to them? This is a question for ornithologists to decide; but I venture to put forward the following suggestions as a working hypothesis which should be tested by naturalists in the field:—that *Glossina palpalis*, which deposits its pupæ on the ground, can only breed in localities where its natural enemies, the gallinaceous birds, are absent or scarce, and therefore (?) is confined in its distribution to the vicinity of water, while *G. morsitans*, having acquired the instinct of depositing its pupæ in sheltered places, can breed wherever there is jungle sufficient to shelter it. If this supposition be correct, the difference in the distribution of the two species would receive a simple explanation.

(N.B.—Ogilvie Grant, in "A Hand-Book to the Game-Birds," Lloyd's Natural History, mentions three species of gallinaceous birds which occur especially in the vicinity of water, namely, *Francolinus adspersus*, Western South Africa (Vol. I., p. 124); *Numida coronata*, Eastern South Africa (Vol. II., p. 91); and *Numida cornuta*, Western South Africa (Vol. II., p. 92).

* Compare especially Prof. R. Newstead's Report on the Habits, &c., of the Common House Fly (Liverpool, 1907) as regards the efficacy of fowls in destroying the larvæ and pupæ of the fly.

It will be noted that none of these species occur in the regions infested by *Glossina palpalis*; if introduced there, they might be effectual in keeping down the fly. I am informed that gallinaceous birds in general prefer dry ground to scratch in; possibly these water-frequenting species might be an exception to this rule.)

4. *Reduction of the Flies*.—In any case there is no doubt that the gallinaceous birds are great, perhaps the greatest, enemies of the flies, scratching up their pupae and devouring them readily, and I suggest that the following measures should be adopted with the object of reducing the numbers of the tsetse in the fly-belts:—

- (a) That keeping domestic fowls should be encouraged as much as possible round villages and habitations, especially in the banana-plantations in the localities infested with *G. palpalis*.
- (b) That guinea-fowl and other wild gallinaceous birds should be protected as much as possible in the jungle near human settlements, and the gallinaceous contingent should be reinforced by

the introduction of the Indian jungle fowl and other suitable species.*

- (c) That in *morsitans*-areas all hollows and shelters in tree-trunks near the villages should be systematically stopped, tarred, or in some other way rendered useless for the flies to breed in. (Probably it would only be necessary to do this up to a few feet from the ground.)

Conclusion.—The habits of the tsetse flies and the problem of the possibility of reducing their numbers should be studied carefully on the spot by competent entomologists working in collaboration with ornithologists. In a limited or enclosed area experiments should be carried on to test how far a reduction in the numbers of the tsetse-flies is effected by the presence of gallinaceous birds or other factors, natural or artificial, in the environment.

3rd October 1913.

* For example, pea-fowl, though they probably would not eat fly-pupae, might indirectly, by their habit of destroying snakes, help other gallinaceous birds to maintain their existence. The megapodes or brush-turkeys would probably be extremely efficient as destroyers of pupae, and like the pea-fowl, they can be semi-domesticated.

MR. SHEFFIELD NEAVE, M.R.C.S., M.R.C.P.

The question as to the advisability of the destruction of game on a wholesale scale depends more on the theory that *G. morsitans* conveys disease to man by means of trypanosomes than anything else, as the area occupied by this fly is so much more extensive than that by *G. palpalis*, and I propose to consider this theory to the exclusion of the question involved by *G. palpalis* infection.

A. The evidence of this theory depends chiefly on the work of Kinghorn and Yorke in respect of *T. rhodesiense*. Shortly, these workers found a trypanosome in the blood of big game, said to be conveyed by *morsitans*,

- (1) which, in their opinion, was identical in its morphology with that found in fatal human cases,
- (2) and which, in a number of experiments with animals infected therewith by *morsitans*, showed a very similar pathogenicity.

Hence they concluded that the trypanosome was the same in the two cases, and that big game contains in the Luangwa valley a reservoir of trypanosomes pathogenic to man.

This evidence, although tending towards their conclusions, I maintain, is not scientifically or accurately conclusive.

- (1) As to morphology of trypanosomes: this has been shown over and over again to be a very uncertain guide. R. Koch and others have pointed out that the forms of known pathogenic trypanosomes vary very much in the same species both as to measurement, form, and position of the nucleus, and that they are not pathogenic to one species of animal only is, of course, accepted. So that by obtaining much the same measurements and finding much the same shapes and arrangement of organs in two trypanosomes, one does not get any more than negative evidence in favour of their being identical. Kinghorn and Yorke's main theory as to the posterior nucleus has been shown to be otherwise than proved.
- (2) Pathogenicity for animals being to a great extent similar in two trypanosomes is not a definite proof, and is only, again, negative evidence of the two being identical, though, of course, that evidence should receive proper and adequate weight in estimating its value, such as it is.
- (3) Again, the whole is based on the supposition that the human trypanosome was conveyed to man by *G. morsitans*, and there is no direct evidence of this; in fact Taute's single experiment on himself would, as far as it goes, point to the contrary, as well as the fact that the four white men representing

the first cases on record attributed their infection to a severe bite quite different to that of *G. morsitans*.

It has been contended that it is contrary to morality and against law to experiment on humans, other than oneself, in the laboratory by letting *G. morsitans* feed on them, but an experiment in nature is worth a thousand laboratory experiments, and is not this now going on every day in the Luangwa valley? And what is the result? Although 16 per cent. of the big game is said to be infected and *morsitans* swarms in myriads, yet May shows there are fewer cases to be found than at his first examination.

B. Bruce's theory that *T. rhodesiense* is nothing else than *T. brucei* must carry great weight, seeing the impossibility of identification by morphology only, and the likelihood of cases in South Rhodesia having been misdiagnosed as malaria, and this the more so now that cases of trypanosomiasis have been also there discovered. This theory, if correct, tends to show that this form of human trypanosomiasis has been with us from the commencement of our tropical inquiries at any rate, and is no new thing.

His theory is also much supported by the difficulties with which the identification of a trypanosome is beset, considering the great variability in form, pathogenicity, chemo-resistance, virulence, and in development under different circumstances that this organism displays. In fact the present confusion makes one inclined to think that owing to mutability, varieties are apt to be raised to the level of species, as in the case of the higher animals.

In conclusion, I would urge that, as there is no new and terrible danger which is increasing, and as the new partial discoveries point to no new epidemic, it is not right on half evidence and half-digested facts to take so radical a measure as to condemn the big game (and on the same grounds, if some doubtful trypanosome is found, all and any of the mammals) in a fly country to extinction. I would therefore plead that further facts be collected to prove or disprove the theories of the advocates of such a course before proceeding to the slaughter.

In respect of an experiment of extermination of game in a localised area, I can only heartily recommend it as being feasible, not precluded by excessive expense, and being probably more instructive than any other single experiment.

In my opinion, two localities should be chosen (a) in the Luangwa valley, where the climate is of a sufficiently high temperature to permit of the proper development of the so called *T. rhodesiense* in *G. morsitans*, (b) some other colder climate, possibly in Southern Rhodesia.

No doubt the question of more than one experiment and the extent of the area to be involved depends on that of funds, but of this I have no cognisance.

Of course, a control area immediately adjoining would have to be carefully studied, commencing as soon as possible, unless it were necessary to previously study the area to be enclosed itself, prior to enclosure.

This latter course would, however, not be necessary, in all probability, if the former were undertaken, and would have the drawback of much delay.
2nd November 1913.

Sir ALFRED SHARPE, K.C.M.G., C.B.

I take it that the *immediate* cause of the appointment of this Committee is the result of Dr. Yorke's investigations, the views he has publicly stated, and the proposals he has made for experiments in the way of game extermination in Africa.

It has been shown that *G. morsitans* is capable of infecting man with trypanosomiasis, and that "game" acts as a reservoir. We have not yet, however, sufficient evidence as to what "game" is capable of so acting. My experience is that *G. morsitans* attacks practically every form of life in the bush, not only "big game," but monkeys, birds, snakes, crocodiles, lizards, rats, &c. Dr. Yorke, in the various papers he has read, generally uses the expression, "big game." He says: "Investigation has made it perfectly clear that the main reservoir of the trypanosomes of man and domestic stock is the big game." What does this include? This is the very first and essential matter we want settled. If only the larger wild animals acted as a reservoir, it is perhaps conceivable that by a wholesale organized system of slaughter all these larger animals might, in the course of years, be exterminated. If, however, the smaller animals, such as monkeys, bush-pig, badger, rock-rabbits, &c., are capable of acting as reservoirs, extermination with such means as we can possibly command, even at a vast expenditure, would be an impossibility in such a country as Africa. If, moreover, birds, lizards, snakes, crocodiles, rats, &c., can act as reservoirs, the impossibility is more apparent still. Dr. Yorke speaks of "big game" and "small vermin." Referring to the latter, he said in a recent paper read before the Zoological Society: "They are to a considerable extent nocturnal, and although *G. morsitans* does occasionally bite at night . . . this is exceptional," but we must know definitely whether or not even "vermin" can act as a reservoir before we can form any opinion as to the advisability of game extermination experiments. Doctors Yorke and Kinghorn found that wart-hog, for example, were affected to the extent of 11 per cent. I doubt the feasibility of an attempt to exterminate wart-hog in any district.

Once it is granted that most forms of life act as reservoirs, it becomes obvious to anyone who has personal acquaintance with tropical Africa that extermination is quite impossible. All native stock would have to be killed—cattle, goats, sheep, dogs. Indeed, to make any such experiment complete, all natives would have to be removed. Monkeys, "vermin," every form of life would have to be destroyed or removed.

It has been suggested that "experiments in limited areas" should be made, but fly (in Nyasaland and Northern Rhodesia, at any rate) ranges over huge districts, and to include the whole of any area which at any season of the year might hold fly would mean dealing with a vast area. Yet, such experiment would be incomplete and inconclusive unless it were made quite certain that there was no possibility of incursion into the area dealt with, by fly (or animals) from outside.

It is one thing to say: Let all animal life be exterminated, it is another to carry it out. Take, for example, lions and leopards; they are pests in Africa, everyone destroys them when they get the chance; there can be, I should say, little doubt that they get freely bitten by tsetse-fly, and act as reservoirs; how are we to exterminate them in such a thinly populated country as Africa?

With regard to the connection between game and fly, I gather that from the point of view of Dr. Yorke this question is comparatively unimportant to some extent, as he does not desire to maintain that fly depends on game for existence; he only wishes to separate them (by killing the game), and hopes that in due course fly will become innocuous. As, however, this question will no doubt be considered, I may state

that of late years I have come to the conclusion that the presence of big game is not in any way necessary for the existence of tsetse-fly (of any variety). In Nyasaland and Northern Rhodesia there are striking examples of practically gameless country with plenty of fly, and flyless country with plenty of game. I think that most men with experience of varied districts of Central Africa have arrived at this opinion.

I enclose notes made by me in 1910 on the habits of *G. morsitans* in Nyasaland and adjoining territories.

[Extracted from the *Bulletin of Entomological Research*, Vol. I., October 1910.]

NOTES ON THE HABITS OF *Glossina morsitans* IN NYASALAND AND THE ADJOINING TERRITORIES.
By Sir ALFRED SHARPE, K.C.M.G., C.B., Governor of the Nyasaland Protectorate.

Throughout the Nyasaland Protectorate there are many areas which are infested by *Glossina morsitans*; and this species is also to be found in North-Eastern Rhodesia, and in German and Portuguese territory bordering on Lake Nyasa. I have been frequently through all these territories during the last twenty-two years, and know them well.

From my observations, made very carefully during the last six or eight years, the opinion I have arrived at in every case is that fly-areas have neither diminished nor increased during the time I have known the country. It is impossible to say what are the exact conditions which are suitable in any district for the existence of tsetse-fly. Within a fly-area it does not follow that fly will be found throughout, nor on all days, nor at all seasons. To speak of a "fly-belt" gives a wrong impression. A "fly-area" has more or less definite limits, and within this area fly may be found, sometimes in one part, sometimes in another.* It is possible to go through a fly-area many miles in extent on some occasions without meeting much fly; on other occasions they may be found thickly throughout. The season of the year has a great deal to do with this, but there are other causes which it is impossible, as yet, to define. For example, between the settlement of Zomba and the Mlanje Mountains, a distance of about 40 miles, lies an extensive plain. Horses or cattle can be driven across from Zomba during the months of May or June without meeting any fly at all; later in the year, say October, they would have to pass through about 25 miles of fly. On the advent of the first rains in November there is a noticeable diminution of fly, but they are still found more or less in this particular area until the arrival of the cold weather in April and May.

My own opinion, which I have expressed on various occasions before now, is that the existence of wild game has very little, if anything, to do with the existence of any species of tsetse-fly. At the time when rinderpest visited Nyasaland there was a noticeable decrease in the quantity of big game in some of the districts where tsetse-fly exists, especially as regards buffalo, but I have never noticed that this had any effect on the presence of *Glossina*. In Nyasaland the common kind of tsetse is *Glossina morsitans*. In certain areas *fusca* is found, but no *palpalis*.

Within a fly-area, as already stated, the quantity of fly met with varies at different times of the year, and I am inclined to think that the prevalence of strong winds to some extent drives the insects away.

* The fact that the terms "fly-belt" and "fly-area" are frequently used as though they were interchangeable is apt to cause confusion. It would be well therefore to restrict the term "fly-belt" to country in which *Glossina* is actually existent; whereas the word "fly-area" would have a wider sense, signifying a district which is liable to be infested by tsetse, either wholly or in part. Thus evidence as to the alteration of fly-belts would not necessarily be in conflict with the author's view that the limits of fly-areas are stable.—ED.

I have not noticed that any special soil, bush or herbage seems to attract them. So far as *morsitans* is concerned it is certain that water is no attraction to them; nor have I observed that the presence of fly is in any way associated with sand. In Nyasaland, *Glossina* is never found in open grass country; it is only found in bush, not necessarily dense forest, but country scattered with trees which give shade. *G. morsitans* is seldom found above 3,000 feet in Nyasaland.

While I hesitate to give a decided opinion on the question whether *morsitans* is entirely dependent for its existence upon the blood of wild mammals, a point which is perhaps better answered by scientific men, yet I can say that out of innumerable tsetse which I have caught I have never found one which had any trace of blood in the abdomen, with the exception of flies caught in the act of feeding either upon myself, upon natives, or animals; and the impression which I have formed is that tsetse-fly are no more actually dependent upon the blood of mammals for their existence than are mosquitos. Unless I am right in this opinion, I am at a loss to understand how the enormous numbers of tsetse-fly which are found in some areas can exist, as in many of those areas game is either extremely scarce or almost non-existent. In Nyasaland it is distinctly noticeable that many of the fly-areas are almost destitute of game, whereas, on the other hand, some parts of the country where game is most abundant, such as the valley of the Rukuru River, are entirely free from tsetse; and in this locality, as in others, buffalo are fairly abundant. At the north end of Lake Nyasa, before the advent of rinderpest, there were many thousands of buffalo, but no *morsitans*.

I am acquainted with villages which are situated inside fly-areas, and wherever the natives build their villages in such localities and clear ground for their food-gardens tsetse immediately disappear from the cleared ground. I have often noticed that, when approaching these villages from the bush, fly which are following the carriers, or are actually upon their persons biting them, will gradually disappear after entering the cleared ground, and by the time the village is reached no fly can be seen. On the other hand, I have known cases where villages have been abandoned, and after a time, as the natural bush has grown up, the flies have reappeared in places where the native food-plantations formerly were.

I have known many cases, especially in the country lying between Tanganyika and Mweru, where native

villages situated in fly-areas have possessed cattle, noticeably so in the case of the powerful chief Nsama. In these instances the cattle were always kept either inside the village itself, or else were only allowed to feed immediately outside, and were always herded by boys to prevent their straying into the bush. Goats, however, are found in all villages in Nyasaland, whether in fly-areas or not, and it is certain that the bite of *morsitans* may have little or no effect upon these animals. On the other hand, I have been informed by natives living in fly-areas that if goats are brought by them from other districts where no tsetse-fly exist, they frequently sicken and die. I cannot myself vouch for the truth of this.

Generally, it will be gathered from the foregoing remarks that my opinion is that the presence or absence of big game has little or nothing to do with the existence of fly. I know that this opinion does not coincide with that of Mr. Selous, whose experience is, I think, principally confined to Africa south of the Zambesi.* I can only say, however, that my observations during the last six or eight years have been very carefully made, that the conclusions I have given have been very carefully arrived at, and that they are shared, I think, by many careful observers in Central Africa.

Statements are made from time to time by persons who may even have spent some years in tropical Africa—notably so in Nyasaland—that tsetse-fly is "spreading." As already stated, I do not think that "fly-areas" alter their limits to any appreciable extent. Those who make these statements do so, no doubt, because they honestly believe them to be true, but I know from personal experience that in many cases fly has existed in the supposed new areas for the past twenty years. There are various causes for fly being now more noticed:—More attention than formerly has been called to the subject; cattle are now kept by natives in districts where formerly, owing to wars and raids, there were none; large native settlements have altered their locations (notably at Fort Johnston, at the south end of Lake Nyasa), and as the old clearings grew up fly appeared. Increased transport by ox-waggons has revealed the existence of fly also in places where it was not noticed before.

* Mr. Selous, since his journey to the Soudan, has altered his views. In a letter to *The Field* he stated that in the Soudan tsetse fly did not appear to be dependent on game.

Mr. J. O. SHIRCORE, M.B., B.Ch., M.R.C.P. (Edin.), Medical Officer, East Africa.

Suggestions on the Limitation and Destruction of *Glossina morsitans*.

I.—"PRIMARY FLY CENTRES": THEIR INFLUENCE ON THE DISTRIBUTION OF *G. morsitans*.

Several months' constant travelling in the "proclaimed area" had led me to conclude that there are at least four "primary centres," situated as shown on the map as follows:—

- No. 1. Opposite Rifu, near Patsanjoka Marsh.
- No. 2. N.W., W., S.W. of Kuti Marsh.
- No. 3. At Nyansato, near the Chitawa Marsh.
- No. 4. Along and on each side of the Lingadzi River between long. 38° 8' and 34° 13' E.

The main reasons why these are defined as "primary centres" are: (1) Fly are found here, and numerous, throughout the year. (2) They are present at these situations when there are none or extremely few elsewhere, when the whole country is bare of grass, the trees without foliage, and the ground baked hard. (3) These are the only areas where water is actually above the earth's surface or not any great depth below, e.g., at the Lingadzi River the centre is almost definitely limited in length by the water and moisture in the river bed. At Patsanjoka the water is present throughout the dry season. At Kuti and Chitawa it dries late, but here it is seen that although these marshes are

superficially dry, yet during late October and early November, before any rain, fresh grass already springs up and trees begin to put on green foliage in the low-lying country near these marshes, markedly in contrast to other parts of the district, i.e., the portions between Nsadz—Waya—Matumba to Mtalamanja. This is somewhat raised undulating broken country with fairly rocky surface. (4) It is in these situations that the fly can best feed during the long drought season. There are herds of eland and buffalo at the Lingadzi, waterbuck, hartebeeste, and eland at Nyansato. Various species of large and small game are found at Patsanjoka and Kutidambos, and numbers can be seen at various times resting in the shade during the heat of the day. (5) At these places it is light forest with fairly short grass, here and there open glades, and not far from water. This combination is ideal for both game and fly. The latter feed as the game come to and from water and the light forest enable them to easily see and follow the game. Heavy forest and thick high grass impede the flight of fly, and moreover they are unable to see any distance. They are sometimes found at these situations but not in any numbers.

The fact that the fly are found at the above-mentioned centres all the year round and that they still exist here at the height of the dry season when there are few or none elsewhere, makes it quite reasonable to suppose that they breed at these places—and that it is from these centres that "extensions" along connecting forest are sent out as soon as these

connections become suitable. This is actually observed to take place. During May, June, and early July fly are found few in number, but may be seen constantly along routes where during August and September they absolutely disappear or are present in almost negligible numbers or only at odd times. This applies to almost all the main routes except just at and opposite the four centres indicated.

Late September, October, and November show a distinct but gradual increase, and fly become evident in situations during November, before the rains, as they were early in May. Radiations from these centres begin about this period so far as the physical characters of the country, the large garden clearings, and the seasonal conditions allow. With the onset of the rains the spread increases, as more suitable situations are created. The rough stony country becomes a habitat and other breeding centres—"secondary centres"—are established, and fly increase *pari passu* until the rains are over, the streams dry, the grass withered, and the hot season with its bush-fires once more performs a natural prophylactic measure.

On regarding the subject from this aspect the point I submit and advocate is that the forest connections along which radiations take place be attacked, cut off or otherwise dealt with early during the year about May, so that the actual "primary centres" delimited in any district at the height of the previous dry season are isolated. After this has been performed all the trees of all sizes in these centres indiscriminately cut down and left lying to dry and ultimately, when the whole country is parched and dry, grass-fires being strictly prohibited during this period, by the end of July or August wholesale and extensive burning over these areas, commencing from the side of the "extensions," and progressing towards the main and isolated body of fly, thoroughly carried out by a gang of natives. At Kutu, Nyansato and Patsanjoka (Rifu) this could be done, and with special ease at the last situation. At Lingadsi it would be difficult, but sufficient could be done here to prevent a southern extension which links up with the north arm of the Nyansato during the rains.

If "extensions" depend on primary sources, upon which they undoubtedly do, then it must needs follow that the limitation or lessening of their numbers must lessen the formation of "secondary centres" and their "extensions," and therefore the whole distribution and numbers of fly.

Game plays a minor part in the actual habitat of *G. morsitans*. It is true that at the above-mentioned areas game is plentiful and that blood is necessary to the life of the fly, but there are numerous situations where game are present, in this same prohibited area, where fly are not found and where fly though present during the wet season are absent during the dry.

II.—THE EFFECT OF PROPHYLACTIC FOREST CLEARING ON *G. morsitans*.

With a view to indicate the results of even moderate clearings of some 150 yards around villages in the "proclaimed area" a few observations may well be recorded. Before such measures were adopted it was noticed that clearings made for agricultural purposes by the inhabitants of a certain chain of villages, east of Mtalamanja, lying north and south of the River Lipimbi along its course to Lake Nyasa, showed a decided effect, as compared with other parts of the district, on the distribution of *G. morsitans*, which was further emphasised by the clearing of the areas in the vicinity of these villages. African villages as a rule have more or less quite definite patches of forest, light or otherwise, surrounding them, for various reasons. The paths from such villages leading to main roads and gardens and water supplies and neighbouring villages traverse these patches and it is at these positions that fly are found. Where there are groups of villages arranged somewhat in continuity one does not find much game immediately near or between such habitations, at any rate not during the day, yet fly in fly districts are invariably present in such situations if

the conditions are not unsuitable, i.e., intervening "dambo" areas, sandy soil near the lake, and rough rocky elevated country with little vegetation inland. Such places are unsuitable for much vegetable growth, superficial moisture is absent, and subsoil water is probably at a great depth, forest cannot support itself, and without its continuity, however irregular, fly cannot exist in any numbers. Travelling through such areas fly are not seen, nor are they found feeding on game shot in these "dambos" except when near the fringe of forest harbouring the fly. It is observed that in favourable places some distance from thick fly "centres" one finds small belts or rings of fly congregated round villages and in forest patches on one or other or both sides of the roads between villages in fluctuating numbers dependent on the seasons. So that the villagers are constantly fed on for greater part of the year by these fly both in and every time they leave or return to their villages. It is obvious that villages must be situated, if inland, in such positions. Forest is to the native not only an indication of arable land but is also a protection from heat and weather conditions. The timber and grass is utilised for building purposes and fuel. Water supplies must also be near at hand. As the above, i.e., forest and relative humidity, together with the natives, their sheep, goats, and dogs supply all the wants of the fly, the fly therefore persist in the habit of frequenting these haunts. It is seen that along the river Lipimbi the rough continuity of villages, i.e., 19 over a distance of some 8½ miles, their garden clearings, &c. has performed a natural division of the fly belt at the junction between its southern extension from the Lingadsi area (No. 4) and the northern extension from the Nyansato area (No. 3).

The prophylactic clearings which were adopted made a still further and decided impression, so much so that after the summer grass fires had swept the country scarcely a single fly was noticeable along the road between Mtalamanja's village and Maosas, the latter situated at the lake. Again it was noticed that, before village clearing was performed, fly which followed one during journeys from Domira Bay, they being first met shortly after crossing the "dambo" opposite area No. 3, kept one company till Matumba's village, and through Matumba's group of villages well along the Chunzi road—in fact some invariably came into the Medical Officer's house at Chunzi with the "safari." When subsequently clearing was performed at Matumba's it was found that this area acted as a block to the progress of fly, they dispersed shortly after passing the edge of the clearings, the few that followed were lost while going through the villages, and none were evident on the other side. Other instances could be quoted showing these effects in varying degree according to the nearness of villages to the main "fly centres" and the physical characters of the country, e.g., where villages are protected by slightly elevated rocky sites, others situated near stream beds which are dry during the summer, in which subsoil water is at a depth of 8 to 12 feet, and others by small abruptly rising hills which bar the extensions from fly belts. When these facts are considered, the formulating of systematic prophylactic measures becomes a matter of comparative ease. A thorough knowledge of the country is essential. What is suitable in one place may not be so in another. In some instances special facilities may offer, in others great difficulties. Each area must be dealt with according to its particular requirements. It is clearly indicated that work must be based on these lines.

The clearing of villages, increase of agriculture, the splitting up of fly belts near villages and along main routes by forest destruction and burning, the attacking of "fly centres" as indicated above cannot otherwise than profoundly modify the entire distribution of *G. morsitans* in inhabited areas. These practical methods would ultimately limit fly to areas which need not be entered by natives while carrying on their ordinary means of livelihood, and further, those who contracted trypanosomiasis after entering these "segregated fly areas" would be harmless as regards the spread of the disease.

Mr. J. O. SHIRCORE, M.B., B.Ch., M.R.C.P. (Edin.), Medical Officer, East Africa Protectorate.

I have the honour to submit a suggestion as to a suitable and inexpensive site where an experiment, if desired, of proving the effect of game destruction on *G. morsitans* may, with exceptional facility, be carried out in Nyasaland. The fencing required for such a purpose would be under 20 miles and might, in fact, be reduced to nearly 10 if taken over a straight line.

The line of fence could run from some suitable spot a few miles south of Monkey Bay, e.g., from somewhere between Mesa Village and Nkudzi on the east littoral of the Livingstone Peninsula to a point north or south of, that is, excluding or including, a village called Kasenka, on the west coast of the same, situated in the Fort Johnston district.

There would thus be an area of some 200 square miles cut off from the rest of the district, having the lake as a natural boundary on all other sides.

This area contains large numbers of game and fly. A clearing could be made of a mile on each side of the fence, and no person allowed to enter or leave the cut-off area from the land side. Five policemen picketed along the fence could control this.

There would be no elephant in this area; the other game consists of some hippopotamus, found ashore during the night, a few buffalo, numerous eland, kudu, waterbuck, nswala, bushbuck, reedbuck, oribi, and a few klipspringer, lion, and leopard, some wild dogs, foxes, and conies.

2nd January 1914.

Mr. WARRINGTON YORKE, M.D., Director of the Runcorn Research Laboratory.

In 1911 Dr. Kinghorn and I were sent by the British South Africa Company to North Eastern Rhodesia to investigate and report upon the conditions governing the spread of sleeping sickness in their territories.

The results of our investigations may be summarised as follows:—

1. The human trypanosome (*T. rhodesiense*) is distributed widely throughout south Central Africa.
2. There is no essential difference between the clinical manifestations of the disease in man caused by *T. rhodesiense* and that due to *T. gambiense*, except, possibly, the greater virulence of the former.
3. *T. rhodesiense* is transmitted in Rhodesia by *Glossina morsitans*.
4. Approximately 3.5 per cent. of the flies may become permanently infected and capable of transmitting the virus.
5. The period which elapses between the infecting feed of the flies and the date on which they become infective varies from 11 to 25 days in the Luangwa Valley.
6. Attempts carried out at laboratory temperature on the Congo-Zambesi plateau during the cold season to transmit the human trypanosome by means of *Glossina morsitans* were invariably unsuccessful in spite of the fact that 680 flies were used in these experiments.
7. The developmental cycle of *T. rhodesiense* in *Glossina morsitans* is to a marked degree influenced by the temperature to which the flies are subjected. High temperatures (75–85° F.) favour the development of the parasite, whilst low temperatures (60–70° F.) are unfavourable.
8. The first portion of the developmental cycle can proceed at the lower temperatures, but for its completion the higher temperatures are essential.
9. The parasites may persist in the fly at an incomplete stage of their development for at least 60 days under unfavourable climatic conditions.
10. These observations afford an adequate explanation of the extremely long latent periods of trypanosomes in *Glossina* which have occasionally been observed by various workers.
11. The relative humidity of the atmosphere has apparently no influence on the development of the trypanosome in *Glossina morsitans*.
12. Mechanical transmission does not occur if a period of twenty-four hours has elapsed since the infecting meal.
13. *Glossina morsitans* in nature was found to be infective with the human trypanosome. In the Luangwa Valley, of 3,200 wild tsetse-flies examined, at least 6 were definitely proved to be capable of infecting animals with *T. rhodesiense*, whilst at Ngao, on the Congo-Zambesi watershed, of 5,250 wild *Glossina morsitans*, only 4 were found to be capable of infecting with this trypanosome. The ratio of infective to non-infective flies in the two localities is, therefore, at Nawalia, 1:534, and at Ngao, 1:1,312. The variation is to be accounted for by differences in climatic conditions, notably temperature.

14. The chief reservoir of the human trypanosome is the big game. In the Luangwa Valley 127 head of game were examined and 16 per cent. were found to be infected with the human trypanosome; whilst on the Congo-Zambesi watershed, of 124 head of game 3.3 per cent. were found to be infected with the same trypanosome. Beside the human trypanosome the game were found to be heavily infected with trypanosomes pathogenic to domestic stock. It was estimated that 50 per cent. of the game in the Luangwa Valley and 35 per cent. of those on the Congo-Zambesi watershed were infected with trypanosomes pathogenic to man or domestic stock.
15. The results of examination for the human trypanosome of the blood of a large number of monkeys, wild rats, and mice were invariably negative.

Prophylaxis.—The problem of how to deal with sleeping sickness in those countries (Rhodesia and Nyasaland) where the disease is spread by *Glossina morsitans* is one of extreme difficulty. There are at present only three possible ways of dealing with the situation—

1. To evacuate the "fly" areas.
2. To exterminate *Glossina morsitans*.
3. To destroy the reservoir—the game—from which *Glossina morsitans* derives the infection.

The first of these policies can hardly be recommended in countries where the disease is spread by *Glossina morsitans*, which is ubiquitous in its distribution. It would mean relinquishing enormous stretches of country. Moreover, there is a still more serious objection to such a procedure. Removal of the population from a district would convert it into a game reserve. The game would increase in numbers and spread, and most probably with it the tsetse-fly, until the invasion of surrounding districts demanded further removals.

Regarding the second measure nothing requires to be said beyond that at the present time we have no knowledge of how economically to exterminate *Glossina morsitans* in large tracts of country. At present the only satisfactory means of exterminating the fly in a given area is to clear the bush and undergrowth. It is obvious that in sparsely populated countries like Rhodesia and Nyasaland this is only practicable in the immediate vicinity of villages; and it must be remembered that not only is it necessary to clear the country, but also to keep it cleared.

It is to the third measure—the destruction of the reservoir—that one must turn if anything is to be done. As, in comparison with infected game, human beings form but a trifling fraction of the total reservoir, the mere isolation of sleeping sickness patients is of practically no value, and the question resolves itself into one of destruction of the game reservoir. Whether this would be followed by disappearance of the tsetse-fly is uncertain, but it is clear that any procedure which reduces the number of the wild fauna would result in a proportionate reduction of the reservoir from which the fly derives the infection pathogenic to man and domestic stock, and if the game were completely exterminated the fly would become non-infective. It is on these grounds that I consider that this is the

line along which future efforts might with advantage be directed.

From the evidence which has so far been accumulated it seems highly probable that the systematic extermination of game in the neighbourhood of villages situated in fly belts would be followed by most satisfactory results. The extermination might be accomplished in various ways, such as by the removal of the restrictions relating to the killing of game, by encouraging the natives to kill game in their own way, and by issuing a certain number of firearms to them.

Such a proposal would doubtless be met by considerable opposition from zoologists and others on the ground that the fauna of Africa must not be interfered with until still more evidence has been accumulated that such an interference would be followed by beneficial results. In order to obtain this precise information it is desirable to undertake the experiment of exterminating the game in a localised sleeping sickness area.

A suitable locality for such an experiment would be one in which game and *Glossina morsitans* were

plentiful and where the population was known to be infected with sleeping sickness. It is most desirable for the successful carrying out of the experiment that it should be conducted in an isolated fly area as widely removed as possible from any other fly area. The advantage of this is that the problem of possible migration of *Glossina morsitans* would be eliminated, and as fencing would be unnecessary a great economy would be effected.

A place which conforms to the above requirements is the Sebungwe fly area of Southern Rhodesia, where 12 cases of sleeping sickness (including one European) were discovered lately in a population of about 3,000.

The British South Africa Company have informed me that they have no objection to my suggesting that the experiment be undertaken in this locality.

In conclusion, therefore, I submit that it is most desirable that such an experiment should be undertaken, and suggest, for the reasons given above, that the Sebungwe district of Southern Rhodesia is a most suitable place.

Mr. R. B. WOOSNAM, Game Warden, East Africa Protectorate.

I have met with tsetse-fly and trypanosomiasis in Uganda and the Congo in 1905 and 1906, and in the Ngamiland district of the Bechuanaland Protectorate in 1909, and in the East Africa Protectorate in 1910-13. My experience has been chiefly derived, however, from the East Africa Protectorate and Bechuanaland.

I have not been personally engaged in experimental investigation of human or animal trypanosomiasis, but I am familiar with the methods of investigation, and with the reports and results as published in the Sleeping Sickness and Tropical Diseases Bulletins, and the Tropical Veterinary Bulletin.

Human Trypanosomiasis.

It is, of course, well known to the Committee that the "Sleeping Sickness Problem" is not of great importance at present in the East Africa Protectorate, and, as far as I am aware, human trypanosomiasis is confined to the shores of Lake Victoria, a *palpalis* area, being probably most prevalent in the neighbourhood of the mouths of the Yala and Nzoia rivers, among the Kavirondo tribe. Waterbuck, Thomas cob, situngu, and hippopotamus are found in this area, but are not numerous. In 1909, I believe, I met with several cases of sleeping sickness in the marshes of Ngamiland, south of the Zambesi, and reported the cases to Dr. Bagshawe. This was in a *morsitans* area, of which the boundary was most sharply defined. The vegetation and general conditions were identical in the fly area and outside it, and the same species of game, which was very plentiful, inhabited both areas; but I think buffalo were more numerous in the fly area. This is the most remarkable instance of this phenomenon I have seen.

Trypanosomiasis in Domestic Animals.

Trypanosomiasis is not a serious factor among domestic animals in the East Africa Protectorate at present, but is far more widely distributed than human trypanosomiasis.

Fly areas are numerous, but the parts of the country which are occupied by European stock farmers are not in contact with these fly areas, and those animals which become infected are almost invariably in the possession of travellers, traders, or shooting parties.

In cases where fly areas occur in the native reserves of pastoral tribes, the natives, from long experience, are well aware of these areas, and do not take their cattle into them. In most of the fly areas of the East Africa Protectorate game animals are numerous, but in the neighbourhood of Kilwezi, in which both *G. pallidipes* and *G. brevipalpis* occur, game animals are very scarce, dik-dik being the most numerous. On some parts of the coast, between Mombasa and Lamu, *G. pallidipes* is very numerous in places where game is not plentiful.

If it was known that the extermination of all game animals would result in the disappearance of tsetse-fly or the disappearance of all pathogenic trypanosomes,

I know of no fly areas in the East Africa Protectorate in which the game could be exterminated except—

- (1) By an enormous and prohibitive expenditure which would be out of all proportion to the ultimate benefit.
- (2) By less drastic and costly measures, which would consequently extend over a very long period, perhaps 20 or 30 years, and which would, therefore, be of little practical use.

Measures for checking Trypanosomiasis.

I can see no hope of any practical and permanent benefit being derived except by the clearing of bush and forest around villages and along roads, &c., and by continued active search for a specific trypanocidal drug of practical use and immunizing serum.

Experiments in Game Destruction.

I am entirely in favour of carrying out an experiment in game destruction, because until this has been done the suspicions and outcry of the general public against the game will never be allayed. Besides this, such an experiment will be of great scientific interest. By such an experiment, I should hope to see three points settled:—

- (1) Whether, after the disappearance of the game the tsetse-fly will also disappear.
- (2) If the fly remains, whether it will cease to be infected with pathogenic trypanosomes.
- (3) Or whether the percentage of infected flies will be greatly reduced.

It will be a matter of some difficulty to find an area in which the experiment will lead to any definite conclusion being arrived at within a sufficiently short period to be of practical utility.

I have grave doubts whether such an experiment will lead to definite conclusions. The extermination of game is certainly not a measure which could be practically applied to all the fly areas in Africa. It could only be carried out in a few cases, and then at great expense, and would not be likely to be completely successful in each instance.

I believe that any expenditure upon game extermination, whether as an experiment or as a general measure, will produce far more practical and permanent results if the money is devoted to—

- (1) Clearing operations.
- (2) Therapeutic and immunizing research.

Fencing would almost certainly be necessary for an experiment. The position of the line of the fence is of great importance, because much assistance in clearing an area of game can be given if the fence is placed in the right position, so that game can jump out of the area but cannot jump into it. I can give some information on fencing, as the question of fencing the Southern Game Reserve in the East Africa Protectorate has been under consideration for some time. It is a matter of considerable expense.

Mr. R. B. WOOSNAM, Game Warden, East Africa Protectorate.

Estimated Cost of Wire Fence.

The cost of the strand wire and wire netting for 100 miles (i.e., an area of 25 mile sides) would be 4,680*l.*, according to the figures supplied to me.

To this must be added the cost of transport to Nyasaland, cost of fence poles, which must have holes for the wire bored in them, and also the cost of labour for erecting the fence. These figures can only be obtained from Nyasaland with accuracy, but it is unlikely that they would be less than 1,000*l.* This would bring the cost of the fence out at approximately 56*l.* a mile, making a total of 5,680*l.* for the 100 miles. Of course, if a suitable fly area could be found where only two sides need be fenced, the cost of fencing would be reduced to 2,840*l.*

However, this is not the only expenditure, for I consider that it is absolutely essential that a trench should be dug on both sides of the fence to prevent herds of game charging into the fence. This will need a great deal of labour, and will, consequently, be an expensive item, although it will also assist in clearing the experimental area of game, because it can be arranged in such a way that game can jump out of the enclosed area, but cannot jump into it.

Palisade and Bank.

If it is decided to carry out an experiment in clearing an area of game, I believe it is worth considering whether it is not possible to enclose an area and clear it of game without using wire fencing and netting at all and without killing the game.

With this object I suggest a palisade and bank. This would consist of poles with a sloping bank on the inside. The poles could be obtained from the cleared belt, and also much of the bank could be composed of the debris from the cleared belt.

Such a scheme in England would, of course, be far more costly than a wire fence, but in Nyasaland, where labour is cheap, such a palisade and bank might be erected for much the same cost as a fence and trenches, and it would certainly be more effective. It is, of

course, a matter of the cost of labour, and figures for this can only be accurately obtained in Nyasaland.

I attach a tabulated comparison of the advantages and disadvantages of the two schemes.

It appears to me that neither the fence and trenches nor the palisade and bank methods of enclosing an area form the most serious item of expenditure in the experiment under discussion.

It is the cleared belt around the experimental area which will be the most costly undertaking. It is universally admitted that this belt should be not less than a mile wide, and I doubt whether the work of clearing such a belt could be done at a cost of less than 20*s.* to 25*s.* an acre.

Taking an experimental area of 25 mile sides, of which only one side need have a cleared belt, the cost of 25 miles at 640 acres to the square mile would amount to 16,000*l.*

Unless an isolated fly area can be found for the experiment where no cleared area is necessary the cost of the experiment appears unjustifiable.

In view of the fact that labour is the most expensive item, and that the experiment is largely in the interests of the natives, the chiefs might, perhaps, be induced to supply free labour or at a very low figure. This would enable a palisade and bank to be put up around an isolated fly area at considerably less cost than a wire and wire netting fence.

In conclusion I can only repeat what I said in my original evidence. I believe that an area could be cleared of game, but that it will be a very costly experiment, and prohibitively costly if a mile belt has to be cleared to separate the experimental area from adjoining fly infested bush or forest.

I believe that any measures directed towards removing the reservoirs of any trypanosomes, animal or human, will not be successful, and that the greatest hope of success lies in endeavouring to eradicate the transmitting agents, the tsetse-fly, by finding the permanent fly areas and clearing them of bush.

Comparison of Merits of Fence and Palisade and Bank.

Palisade and Bank.		Fence.	
Advantages.	Disadvantages.	Advantages.	Disadvantages.
1. No expenditure upon purchase or transport of material.	1. Increased expenditure upon labour.	1. A wire fence could be erected in a shorter time than a palisade and bank.	1. Large expenditure upon purchase and transport of material.
2. Game can be removed without being destroyed.	2. In places such as the crossing of a river where the palisade and bank were impossible, a wire fence and trenches might be used with heavy swing gate opening down stream only and hung from above.	2. Less expenditure upon labour.	2. Greater expenditure upon supervision and upkeep as it is more liable to be broken down than a palisade and bank.

Palisade and Bank.		Fence.	
Advantages.	Disadvantages.	Advantages.	Disadvantages.
3. Great assistance in clearing the area of game, because game can escape or be driven out over the palisade but cannot return. If access to water is prevented in the area the game will find its own way out rapidly.	3. It would probably take longer to erect the palisade and bank than the fence unless a large amount of labour was available, but it must be remembered that the construction of the fence would require skilled labour to a large extent, while the palisade and bank only requires intelligent supervision.	3. The wire of the fence will be of some value after the experiment is finished.	3. Game cannot escape out of the area so easily over a fence as in the case of the palisade and bank, and it will consequently take longer to clear the area.
4. The palisade and bank cannot be knocked down by herds of game or rhinoceroses.	—	—	4. If there are herds of game in the district such as zebra, hartebeest, eland, &c., I consider it is essential that the fence should have a trench on both sides as shown in the accompanying diagram. These trenches would be unnecessary where the fence could be made to follow the course of a stream or dry river bed. The fence should then run along the slope of the bank, leaving the stream outside the experimental area.
5. Game are less likely to try and jump back into the area as they cannot see through a palisade as they can through a fence.	—	—	—
6. The palisade can be largely composed of the debris from the cleared belt.	—	—	—
7. Less expenditure on supervision and upkeep.	—	—	—
8. Small antelope and pigs will be excluded with the great expense of wire netting.	—	—	—
9. The trenches along the fence will be an expensive item, but will be unnecessary along the palisade. The cost of making the trenches would go a long way towards paying for the palisade.	—	—	—
10. I doubt whether elephants can be kept out of the area by either a fence or palisade and bank unless the latter is 8 or 9 feet high, and even then they might pull it down in places. However, they can escape out of the area by means of the palisade and bank without doing it much damage, while they would certainly break down the fence seriously when determined to cross it in either direction.	—	—	—

Part V.—Miscellaneous Written Statements.

BRITISH SOUTH AFRICA COMPANY TO
COLONIAL OFFICE.

2, London Wall Buildings, E.C.,

SIR, 10th October 1913.

I AM directed to forward, for the information of the Secretary of State, a copy of a report by Dr. Aylmer May, Principal Medical Officer for Northern Rhodesia, on the relationship between big game and the spread of sleeping sickness. Dr. May has an extensive and specialised knowledge of the disease, and my Directors think that his report may be of service to the Committee which has recently been appointed to investigate this subject. I am also to say that Dr. May and Mr. Lloyd, the entomologist attached to the recent Sleeping Sickness Commission in Northern Rhodesia, will be in London towards the end of the year, and will be prepared to give evidence before the Committee. The Administrator is of opinion that the evidence of these two officers might be of considerable value, in view of the fact that they have made a close study of the subject on the spot.

I am, &c.,
A. T. MILLAR,
Assistant Secretary.

Big Game and Sleeping Sickness
(A. May).

Medical Department, Livingstone,
Northern Rhodesia.

SIR, 27th August 1913.

THE accompanying number of the British Medical Journal (August 2nd, 1913) is forwarded for His Honour's information. It contains an announcement that the Secretary of State for the Colonies has nominated a Committee to report on various aspects of the relationship between big game and the spread of sleeping sickness with a view to determining whether the destruction of the game, either wholesale or partially, is necessary.

A considerable amount of agitation for the destruction of game as a preventative to the spread of this disease now seems to exist in England, and it is probable that both this agitation and the appointment of this Committee is due, partly to the opinions recently expressed on this subject by Sir David Bruce, the Director of the Nyasaland Sleeping Sickness Commission, and partly to some papers and discussions which have also recently taken place, *i.e.*, before the Royal Zoological Society, as the result of a paper read by Dr. Warrington Yorke on "The Relationship of the Big Game of Africa to the spread of Sleeping Sickness," and before the Society of Tropical Medicine in connection with a paper written by the Director of the Tropical Diseases Bureau on "Recent Advances in our Knowledge of Sleeping Sickness."

It would seem that all arguments in favour of the destruction of game are based on the presumption that sleeping sickness is spreading.

There is no authority whatsoever for this presumption.

It would appear from the information available as to the arguments brought forward in favour of this measure that those who advocate it have been very indifferently informed as to the true facts of the case. Our present knowledge of the infecting organism leaves us still in ignorance as to its source, identity, and the time during which it has been in existence in this territory.

Our present knowledge of the bionomics of *Glossina morsitans*, the carrier of this disease, leaves us in absolute ignorance as to its real relationship with, and as to what would be the result of wholesale or even partial destruction of the game; further, it seems well within the bounds of possibility that the resulting condition would not be an improvement on that which now exists.

The only one factor in the argument on which we have accurate knowledge is the one which is ignored altogether by those who advocate this policy.

We have accurate knowledge as to the present position of the disease, we know that it is *not* spreading, that its incidence is markedly less than it has been. That with every known condition favourable for its spread, during a period of at least six years, it has failed to spread. That there is no reason to suppose that the disease will ever assume anything like epidemic proportions, or ever change from the condition of slight endemicity which now exists.

In July 1907, the first case of this disease was discovered. Since then another 92 cases have been found. 87 of these were infected in the present Luangwa closed area, five elsewhere in this territory.

It can be presumed that the known cases number probably about one-third of the actual infections; on this estimate 261 cases have occurred in the Luangwa closed area in four years (since the commencement of a systematic effort for their discovery).

The population of this area is roughly 120,000 people; therefore, in each year 65 people out of 120,000 died of this disease, a death rate per mille of 0.54!

(In the Mpika section of this area, of the deaths reported during the last year two are known to have been due to sleeping sickness, 12 to "accidental causes," viz., lions, crocodiles, snakes, &c.)

In this same area a recent examination of 91 per cent. of the censused population by Dr. Kinghorn resulted in the discovery of two cases of the disease. This identical district examined at periods varying from 18 to 24 months previously had resulted in the discovery of eight cases.

An examination of the northern part of the Petauke district in November and December last year resulted in the finding of nine cases, or three less than a similar examination carried out during the previous year.

The lower third of the Lundazi division which has just been examined resulted in the finding of five cases, a result which is identical with that of an examination carried out there in December 1911.

These figures show that sleeping sickness is not spreading, that it shows no tendency to spread, and that any argument in favour of drastic legislation directed towards the introduction of measures, the results of which are still in doubt, and based on an erroneous presumption (that this disease is spreading, or likely to become a menace), are not justified.

I regret that the Annual Sleeping Sickness Report for the territory containing full details as to the present position of the disease is not yet completed, but pending its completion, I would suggest for His Honour the Administrator's consideration that, owing to the obviously imperfect information at the disposal of those responsible for advising on this subject, the above facts might be made known.

I have, &c.,
A. MAY,
Principal Medical Officer.
The Secretary,
Livingstone.

BRITISH SOUTH AFRICA COMPANY TO COLONIAL
OFFICE.

2, London Wall Buildings,
London, E.C.,

SIR, 12th December 1913.

I AM directed to forward, for the information of the Secretary of State, a copy of a report upon sleeping sickness in Northern Rhodesia by Dr. A. May, the Principal Medical Officer, covering the period from February 1912 to October 1913, and a memorandum dated the 10th November 1913 by the Medical Director of Southern Rhodesia.

I am, &c.,
D. E. BRODIE,
Secretary.

Enclosure 1.

Dr. A. M. FLEMING to the ADMINISTRATOR,
Northern Rhodesia.

SIR,

10th November 1913.

I HAVE the honour to forward for His Honour's perusal, and for favour of return for record in this office, a copy of the sleeping sickness report of Northern Rhodesia for the period from February 1912 to October 1913 by the Principal Medical Officer, Northern Rhodesia.

This report is full of interest, and the observations and deductions have been most carefully made.

In an interesting report such as this is, it is difficult to say what are its special features, but some of the points of most practical interest to this Administration are as follows:—

- (1) Remarks with regard to the scattered nature of the infection. This bears out what I referred to with regard to the occurrence of trypanosomiasis on the Busi River and the probability that even in large areas infested with the tsetse fly, they may be only isolated foci of infection as far as man is concerned.
- (2) Doctor May's remarks regarding the known spread of the disease since the last examination of natives in infected areas, and the fact that the disease appears not to be of recent origin, and its equilibrium has now been established, are of particular interest and importance.
- (3) The relationship of fly to game does not appear to be altogether proved as the result of the observations made in Northern Rhodesia in the last eighteen months. As far as an experimental destruction of game is concerned, I am still of the opinion that experiments can be carried out with greater advantage in the more limited fly areas of Southern Rhodesia than in Northern Rhodesia where tsetse fly covers such large tracts of country and where small and limited fly-belts are practically unknown.
- (4) Doctor Kinghorn's estimate of the death rate amongst the indigenous population of the Mpika districts is certainly rather startling where he gives the figure at 37·8 per thousand, which is abnormally high as compared with other communities, and may possibly have a strong bearing on the mortality rate amongst North-Eastern Rhodesian labourers on mines, as compared with natives from other places.

It would be of the greatest interest if these estimates of the native mortality rate in their own homes could be carried out in other districts and territories, including Southern Rhodesia.

Altogether, this report by Doctor May has placed our knowledge of trypanosomiasis on a sound footing. The work done by himself and staff has been most thorough and the deductions sound and cannot be gainsaid in the light of present knowledge, and I can only express the hope that this report will be accorded the recognition it undoubtedly deserves.

I have, &c.,

A. M. FLEMING,
Medical Director.

LUAPULA MWERU AND TANGANYIKA AREAS.

A very satisfactory position as regards the prevalence of sleeping sickness in these areas now exists, the whole country to the east of the Luapula and Lake Mweru has been depopulated, and it may now be reasonably considered that practically all risk of the extension of the disease is at an end.

One case only of the disease has been discovered during the past year (this was in a native of Shiwiri's Village, which has been removed from the Congo Border, north of Lake Mweru).

There are now twenty-three cases under treatment, namely, ten at Fort Rosebery, six at Kawambwa, and seven at Abercorn, and it seems improbable that this number will be increased, except by the addition of a few who may possibly have escaped detection in the moved villages.

Restrictions on the movements of the natives of these areas need now aim only at the prevention of

their return to foci infection, i.e., *Glossina palpalis* areas, and for this purpose it will be sufficient to include as closed area only those parts of the Luapula, Mweru, and Tanganyika Districts which have been depopulated.

It has, therefore, been recommended that the restrictions formerly in force in these areas should be dispensed with, and the following have been adopted in their place:—

Boundary Lines for the Luapula (Fort Rosebery) District.

The original closed area extending between the Luera, on the south, and the Luango, on the north, has, with the exception of one village, namely, "Chansa-Kafushya," on the Kashya Stream, been depopulated. *Glossina palpalis* exists on the Mansa River to within a few miles of Fort Rosebery Station; it would, therefore, be difficult, without making a separate closed area, for the Mansa to reduce the extent of the original closed area, and it has been recommended that it be allowed to stand as at present.

Mweru District.

Since the only object future restrictions need have is the prevention of the return of the population to dangerous areas, it does not seem necessary to include the large area to the east of the Mweru Marsh, and between there and Lake Tanganyika. Thus a narrow stretch of country running along the east bank of the Luapula and the shores of Lake Mweru has been adopted as closed area.

The Mweru and Tanganyika closed areas are now completely separated, and restrictions have been removed from the Mweru Swamp and surrounding country between Lakes Mweru and Tanganyika.

Tanganyika District.

The boundary of the closed area in this district has been fixed at a distance of ten miles from the shores of the Lake, and a similar distance from the Lovu River as far up stream as the crossing of the Abercorn-Mporokoso Road.

Restrictions as to Movement, &c., in these Closed Areas.

The following have been approved:—

- (1) That no natives be permitted to live within these closed areas.
- (2) That no native be allowed to enter these areas except at the discretion of the District Official or Medical Officer.

Tanganyika District.

Dr. W. H. T. Storrs.

No fresh case of the disease has been found since October 1910.

Segregation Camp.—Since the establishment of this camp the total number of cases admitted has been 34.

Of these five were found west of the Lovu River, from Niamkolo and Kasakalawe on the Lake shore.

There are now six patients in this camp, none of whom have for some time shown any signs of the disease; it is anticipated that these can shortly be discharged with safety and the camp closed.

Glossina palpalis. Distribution.—The Lovu River has been examined by Dr. Storrs, from the crossing of the Mporokoso-Abercorn Road down stream to the Lukwesa crossing. No fly was found up to a point about six miles north of this crossing; thus there has been no extension of fly up stream since the limit was defined by Dr. Leach (1909).

The Lake shore was also examined from Kasakalawe to Kituta, and also some six or seven miles up the Lunzua River.

Fly was found to be abundant at both Kasakalawe and Niamkolo.

No fly was found at Kituta.

Clearings.—The Mwepwe and Kanone Clearings, on the Lovu River, are still maintained.

Mucuru District.

The late Dr. D. C. Master.

One new case of the disease has been found during the last 18 months, that of "Nampweto" (mother of a former patient). This infection was in all probability contracted at Shiwiri's Village, on the northern border, where this woman lived before the removal of the village.

Owing to the complete removal of the population from *Glossina palpalis* areas it is unlikely that more than a very few other cases, which may up to the present have escaped detection, will be found in this district.

Segregation Camp. (Kaukambwa.)—There are at present six cases in this camp, namely:—

Kaneke, 5th January 1913. Glands only.
Casimota, 1st March 1913. Glands only.
Moa, April 1912. Glands only.
Wauni, April 1912. Glands only.
Kabwe (female), 1st February 1913. Glands only.
Nampweto (female), 1st March 1913. Glands and blood.

Treatment.—Atoxyl injections.

Glossina Palpalis. Distribution.—No change has taken place in that previously recorded, with the exception that during July, as reported by Dr. C. E. Storrs, no fly could be found at the mouth of the Luano River.

This is probably a seasonal change only in the distribution of this species; fly was numerous at this place during the preceding February.

Kilwa Island.

A careful palpation of the population of this island did not lead to the discovery of any case of the disease.

Fly is numerous on the Belgian shore in close proximity to the island. The absence of the disease is a strong argument in favour of this fly being uninfected.

These people are now recruited for local labour.

Moved Villages.

Conditions are reported as satisfactory.

Luapula Division.

Dr. E. G. Storrs.

Glossina Palpalis. Distribution.—No extension of fly has occurred on the Luango and Mansa Rivers.

The clearing at the Mansa Chofosi Junction has been maintained.

Removal of Villages.—The villages recently removed from the sleeping sickness area are as follows:—

Chisunka Group:—

Chisensela,
Katanga,
Mutante,
Marapula,

were removed to a site about three miles east of Kasempe on the Luafumu River.

Chemesi Group:—

Musakwa,
Kapoli,
Muloshi,

have been removed on to the Chofosi stream about 1½ miles north of Chemesi.

There is now only one village, Chansa, on the Kashya stream, a tributary of the Lukundushi River, remaining on the sleeping sickness area.

Kapwepwe, Kopolwa, Mutando, and Musakanga have moved into Congo territory.

Segregation Camp. Fort Rosebery.

Fifteen cases have recently been discharged as cured; they are still under supervision. None of these cases had exhibited trypanosomes for at least two years. Eight cases terminated fatally, and one escaped into the Congo.

Of the remaining cases it is hoped that three will be discharged cured within the next few months.

Treatment.—Atoxyl and mercury.

Glossina morsitans transmitted Human Trypanosomiasis.

The Present Position of the Disease in Northern Rhodesia.

Since the date of the last Report (February 1912) 29 cases of the disease have been found, namely:—

European:—

Ndola District 1 case.

Natives:—

Petauke District 9 cases.

Fundu Detention Camp 4 "

Moika District.

Plateau Area 1 case.

Valley Area 2 cases.

Chinsali District 1 case.

Lundazi District 5 cases.

Serenje (Plateau Area) 2 "

Ndola District 2 "

Nkushi District 2 "

Total 29 cases.

The total number of cases found since August 1909, therefore, now amounts to 95; 66 of these have previously been reported.

Petauke District.

1. An examination of the northern part of this district during November and December last year resulted in the finding of nine cases, three less than a similar examination carried out during the previous year.

Mpika District (Valley Area).

2. Examined by Dr. Kinghorn, June 1913; 91 per cent. of the censused population was seen. Two cases of the disease were found.

This identical area, examined at periods varying from 18 to 24 months, previously had resulted in the discovery of eight cases.

Lundazi District.

3. The examination of the southern third of this district was carried out during July and August by Dr. Kinghorn. Five cases were found, a result which is identical with that of an examination carried out there by Dr. A. F. Wallace during December 1911 and January 1912.

Chinunda and Rukusi Areas.

These were examined by Dr. H. Leach during August of this year.

No cases of trypanosomiasis were found.

The ground covered was practically identical with that covered in the earlier part of 1911, when five cases were found.

Fundu Detention Camp.

Some 1,700 natives from the various parts of the Luangwa closed area and Nyasaland have passed through this camp, en route for Southern Rhodesia and elsewhere. Four cases of the disease have been found amongst them.

Fundu and Southern Border Road Area.

Was examined by Dr. R. S. White during September and October 1912. 2,300 people were seen, one case only of the disease being found.

Lukasashi Valley.

Was examined by Drs. F. O. Stohr and A. D. Edington during June and July 1912. No case of the disease was found.

Plateau. West Serenje District.

Lulimala and Lukulu Rivers.

The fly area in the neighbourhood of the Lulimala and Lukulu Rivers to the east of Lake Bangweolo was

examined by Dr. G. W. Ellacombe during October and November 1912.

Twenty-four villages were visited and 3,247 people examined.

One case of trypanosomiasis was found (at Chitono's village).

Dr. Ellacombe draws attention to the fact that of the only two cases of trypanosomiasis previously found in this fly area, one "Nandwe" (Case No. 3, 1910 report), was also a native of Chitono's village. The other "Naglandi" (treated by Dr. Brown at the Chitambo Mission), came from Chimesi's village about 6 miles away, and suggests that there is a focus of infection in this locality.

Fly Distribution. (West Sereje District.)—With occasional interruptions due to the nature of the country (swamp and open plain) fly is numerous throughout this area. The fly-belt extending roughly from the neighbourhood of the Livingstone Memorial on the east to the Bangweolo swamps on the west.

This district was previously examined by the late Dr. D. C. Master in July 1909. No cases were then found.

Mpika District.

Ngoa-Chambezi Fly-belt.

This was examined by Dr. Kinghorn in February of this year.

1,038 people were seen (census population, 1,041).

No case of the disease was found.

Dr. Kinghorn gives the following table of the relative incidence of glandular enlargement in men, women, and children in this area:—

	Glands.				Totals.	Per cent. with Glands.
	X	X -	X - -	- -		
Men - - -	0	2	115	119	236	49.5
Women - - -	0	0	131	219	350	37.4
Children - - -	0	0	253	199	452	55.9
Totals - - -	0	2	499	537	1,038	48.2

Luano Valley District.

Examined by Dr. D. S. MacKnight during September 1913. One case of the disease was found.

Mpika.

The Mpika portion of the Luangwa closed area was examined by Dr. Kinghorn in May and June 1913. All villages in this area were visited and all available natives examined.

The examination of this district was carefully controlled by the census returns, to which Dr. Kinghorn applied the requisite corrections for death, numbers working elsewhere, families who had left the district either temporarily or permanently, &c., and it is estimated that 91 per cent. of the total adult population were palpated. The death-rate for the district is estimated from the figures available at 37.8 per 1,000.

The chief causes of death mentioned were:—

Accidental: (Snakes, lions, crocodiles) -	12
Intestinal - - - - -	30
Chest - - - - -	4
Leprosy - - - - -	2
Old age - - - - -	2
Sleeping sickness (diagnosed) - - -	2
Unknown - - - - -	47

Dr. Kinghorn states:—

"In only one village, Kapitis, was the statement definitely made that two of the three deaths there had been due to 'Chilotera.' In a few instances, the symptoms mentioned—emaciation, oedemata, headache—were suspicious of sleeping sickness, but from the meagre amount of detail available no definite conclusion could be drawn. The large number of deaths due to intestinal disorders was, to a very large extent,

attributed directly to the lack of proper food. The result of last year's drought."

In discussing the discrepancy which exists between the censused population and the members examined, Dr. Kinghorn remarks: "While I regret this discrepancy, I think that the results are sufficiently exhaustive to afford a fair index of the prevalence of 'sleeping sickness' in this portion of the valley at the present time."

The total number palpated was 2,613. Details are given in the following table:—

	X	X -	X - -	- -	Totals.
Men - - -	3	2	266	342	613
Women - - -	1	0	242	755	998
Children - - -	0	1	533	468	1,002
Totals - - -	4	3	1,041	1,565	2,613
Per cent. - - -	0.15	0.11	39.63	59.89	99.98

All those with X and X - glands were punctured and trypanosomes were found in the gland juice of two.

These two positive cases were found in Kapampa's and Msoko's villages, the first of which lies seven miles to the north of the old Nawalia Boma, and the other on the Mwalezi River which forms the Mpika Chinsali boundary.

"So far as it is possible to judge therefore the outlook is distinctly encouraging. The villages in this portion of the sleeping sickness area were visited at periods varying from 18 to 24 months previous to the present occasion, and whereas eight cases were then diagnosed only two have been isolated this year. Strong support is afforded accordingly to the view that the disease is of old standing in the Luangwa Valley and that it has now reached a state of comparative equilibrium; were this not the case, it might have been expected that the disease would have made more pronounced strides in two years, more particularly in view of the general lowering of vitality which must have occurred in the natives during the period of semi-starvation which existed last year.

"The general health of the community now appears to be satisfactory."

Lundazi.

The southern third of the Lundazi sub-district was examined by Dr. Kinghorn during July and August 1913.

Six thousand three hundred and seventy-four natives were seen, and five cases of trypanosomiasis found.

These results are identical for this part of the division with the condition found by Dr. Wallace, who made a similar examination during December 1911 and January 1912 (*vide* last report, page 16). Details of this examination are not yet to hand.

Luano Valley.

This area was examined by Dr. D. S. MacKnight during September of this year.

One thousand six hundred and fifty-three natives were examined, of whom 21 had puncturable glands, and from 77 others fresh blood films were taken and examined.

One case of trypanosomiasis was found. (Matowera, of Mwanna's village, near the Mwapula stream.)

Previous History of the Disease in this Area.—During July and August of this year a series of deaths were reported, the cause for which was obscure.

A blood smear taken by the Native Commissioner (Mr. E. K. Jordan) from one of these cases was found by Dr. Wallace to contain trypanosomes.

An examination was recently made by Dr. D. S. MacKnight of the natives living in this valley. Every village was visited, and all the inhabitants palpated. A few villages on the Luensemwa, to the south of the Luano, were examined, also a few villages on the plateau to the west.

Dr. MacKnight Reports.

Glossina morsitans is by no means numerous in any part of the area, but more plentiful near the Mwapula stream than elsewhere.

The natives state that the fly is always most numerous and most troublesome just before the rains (October and November); they think that during the past year tsetse has been more numerous than usual; as far as can be ascertained, these flies disappear at certain times, and do not make the valley their habitat.

Game is not by any means plentiful except on the Mwapula, near its junction with the Lunsemfwa. Fly at this point was particularly scarce.

Domestic Animals.—In the Mwapula area there were very few dogs, and no goats; elsewhere throughout the valley healthy dogs and goats were found in the villages.

Previous History of the Disease.—The natives state that the disease is not by any means a new one, they have known it since they were children. They have never seen or heard of so many cases as have occurred lately. Only two cases have been noticed by them in the five years preceding the one on record. They recognise the disease by the oedema in the legs, and talk of it as "the disease in which the legs swell." They have no name for the disease. Many years ago a name was given to it; this has, however, been dropped. They do not believe that small children ever get the disease. The sickness may be looked for just when the crops begin to grow, that is, with the first rains. Somnolence is not recognised as a symptom of the disease.

The number of people who have recently died of the swollen leg sickness is said to be about twenty.

Fundu Medical Examination and Detention Camp.—This medical station was established in January 1912, with the object of:—

- (1) Providing a medical examination and detention for all natives passing south to Southern Rhodesia from the Luangwa closed area and Nyasaland, and so preventing the spread of trypanosomiasis in that territory.
- (2) Providing a medical station for the southern part of the Luangwa closed area, from which the population of the surrounding district could be periodically examined.
- (3) To provide medical assistance and hospital treatment for the natives of the surrounding districts.

The station was opened by Dr. Leach, who remained in charge pending his transfer to Fort Jameson, until April 1912. Since that date Dr. R. S. White has been in charge, assisted by Dr. F. O. Stohr, and subsequently by Dr. A. D. Edington.

Each native passing through is detained for a minimum of ten days. Daily observations as to temperature are taken; all are carefully palpated. All enlarged glands are punctured and microscopically examined. A microscopical examination is also made of the blood of all cases showing any rise of temperature.

Since the establishment of this station some 17,000 natives have passed through for work in Southern Rhodesia.

Four cases of trypanosomiasis have been found at this station, three amongst the natives passing through to the south, one from a neighbouring district.

The disease has not been found in any native subsequent to his having passed through this camp.

The duties of the medical officer stationed at Fundu include, in addition to the examination of a natives passing through to the south, the general medical work of the district (including vaccination), and the examination of the district and the investigation of the position of the disease.

During December and January the villages on the Fundu-Nyanje road, and within a ten-mile radius round Nyanje, the Nyanje Petauke, and the Petauke-Kamona roads, were examined by Dr. White. 7,122 people were seen, and one case of trypanosomiasis was found.

The southern part of the valley of the Lukasashi River was examined in November by Dr. Edington. No case of the disease was found.

The Lukasashi Valley from Fundu to the Mlembo plantation was examined by Dr. F. O. Stohr during May and June of last year; 1,385 people were seen; no case of the disease was found.

During August and September the inhabitants of that area known as the Sitchitambo-Kanduzi fly-belt were thoroughly examined by Dr. R. S. White; in 310 instances a microscopic examination was made of either the blood or gland juice, or both, of these people. In no case was the disease found.

No satisfactory explanation can be offered as to the failure of this disease to spread under the seemingly eminently favourable conditions which exist in the greater part of the Luangwa Valley area and elsewhere at similar altitudes, and under which it has been known to exist for at least five years. A large proportion of the game, which is plentiful in these areas, harbours a trypanosome which is identical morphologically, and in all its reactions with the *T. rhodesiense*; the transmitter, *Glossina morsitans*, is plentiful, and climatic conditions are suitable, notwithstanding which, the disease has shown no inclination to spread.

It may, however, be suggested as a possible explanation that—

- (1) the disease is an old one and had, in all probability, existed for a considerable number of years before it was recognised, and is endemic, and that the bulk of the population is immune;
- (2) the source of supply of the infection, namely, the extent to which the game is infected with *T. rhodesiense*, may not be so extensive as is at present thought;
- (3) the tendency of the disease, so far as is at present known, to appear localised, such as in the neighbourhood of the Luangwa, north and south of Hargreaves, and in the proximity of main roads, and, as pointed out by Dr. Ellacombe in the West Serenje district, rather than to be found equally distributed elsewhere where climatic conditions are suitable, and where there are equally favourable conditions as regards the abundance of both transmitter and "reservoir," would suggest that there is still a link wanting in the chain of evidence in favour of the "fly" and "game" trypanosome being identical with that which causes human trypanosomiasis.

The Relationship of Human Trypanosomiasis to Game.

—It was pointed out in the last Sleeping Sickness Report for this territory (February 1912) that, in connection with the prophylaxis of the disease, game would have to be considered from two practically independent points of view.

The first, as offering a means of subsistence to the carrier of the disease, i.e., the *Glossina morsitans*.

The second, as reservoirs for the organism causing the disease.

1. The question as to whether game is essential for the maintenance and propagation of the fly is still in exactly the same position as it then was. We are still in ignorance as to what relationship exists; proof is still wanting that any essential relationship does exist.

2. As reservoirs for the organism causing the disease. It has now been accepted by a considerable number of authorities that the trypanosome first found in the blood of a waterbuck at Nawalia in August 1911 by Dr. Kinghorn is the same organism which causes that variety of human trypanosomiasis met with in *Glossina morsitans* areas in this territory. This organism has since then been found by Dr. Kinghorn and the other members of the Luangwa Sleeping Sickness Commission to be widely distributed amongst the fauna of this country, and it has also been shown that its carrier is the *Glossina morsitans*. The final proof of its identity, i.e., its pathogenicity to human beings is, however, still wanting.

The identity of this trypanosome is questioned by Dr. Taute, of the German East Africa Sleeping Sickness Commission, who has recently performed the bold

experiment of injecting himself and a series of experimental animals as controls with blood from a naturally-infected dog, and of allowing himself to be bitten by flies known to be infected with it.

In the former case, all the seven animals used as controls became infected and died.

In the latter experiments, also, the control animals became infected and died, whilst the author remained healthy, and has since proved by the inoculation of susceptible animals with his own blood that he did not become infected.

There is, therefore, some doubt thrown on the identity of the *T. rhodesiense* and the organism found so widely distributed in game, and hence on the part which the antelope plays as the reservoir of sleeping sickness.

The contention in favour of the non-identity of these trypanosomes is also supported by the conditions found in association with a heavy infection of the game with this trypanosome, and its failure, under suitable conditions, to spread to human beings; these circumstances have been reported elsewhere.

The complete proof of the identity of this trypanosome needs a series of experiments such as that carried on by Dr. Taute. No more than an inference as to identity can be drawn from the facts at present available.

It can, however, be claimed that if the organism be the *T. rhodesiense*, then the possibility of immunity amongst man is an established fact, and one having a most valuable bearing both on the future of the disease and the alleged necessity for drastic preventive measures, such as the wholesale destruction of game.

The Experimental Determination of the Relationship which exists between Game and Fly, and incidentally between Game and Human Trypanosomiasis.

With the object of determining these points, it was proposed that an experiment (*vide* Report for 1912, page 25) should be undertaken which was to include the complete destruction or removal of all game in the supposed limited and definitely restricted fly belt in the neighbourhood of Sitchitambo's and Kanduza's villages on the Southern Border Road, from which it was expected definite conclusions could be drawn as to what effect such destruction had on the life of the fly.

This experiment has not been carried out. It was found on more careful examination (made by Dr. R. S. White in September 1912) that this fly belt, formerly thought to be definitely restricted to the area named was not so restricted, but extended on the west to the banks of the Luangwa river and there became continuous with the fly belt running north and south along this river; and on the east and south extended into Portuguese territory. It was also found that game in this area was extremely scarce and, consequently, its destruction could not be expected to yield the required information. It was therefore decided that this was not a suitable site for the proposed experiment.

The Lukasashi Valley, north of the Mkushi-Petauke Road, was next examined (by Mr. L. Lloyd, October and November 1912) with a similar object in view. It was found to be in every respect eminently suitable. The following is an extract from Mr. Lloyd's reports:—

"From these data it will be seen that tsetse fly exist practically throughout the valley of the Lukasashi in the part examined, including part of the Kaombi stream and the Manda as far as the hills.

"Game is moderately numerous throughout the district. The fauna is that of the Luangwa Valley rather than that of the Plateau.

"The secluded position of the valley and its dry condition during the winter render it a specially suitable place for the proposed experiment."

An experiment of this nature and magnitude requires careful consideration in all its aspects before being attempted, *i.e.*, (1) its cost, (2) the probable result, (3) The possibility of arriving at a correct conclusion as to the interpretation of the result.

Its Cost.—The following rough estimate is submitted; it is based on the assumption that the area experimented on should not be less than approximately 400 square miles, and that this area must be effectively fenced in order to prevent the return of the game, and that the experiment will extend over three years:—

1. <i>Estimated Expenditure.</i> —Observations on the seasonal variations of the fly in the locality selected for at least one year before the commencement of the experiment -	£ 500
2. <i>Fencing.</i> —Original double fence, 100 miles at 50l. per mile -	5,000
Repairs to fencing during the continuation of the experiment, including material, white and native labour for at least two years -	2,000
3. <i>Quarters.</i> —Erection and maintenance of quarters for European staff -	500
For native staff -	50
4. <i>Salary.</i> —Of trained observers (microscopist and entomologist) engaged in observations as to the results -	2,000
5. <i>Transport</i> -	400
6. <i>Removal of Game</i> -	100
7. <i>Incidental Expenses</i> -	300
Estimated total expenditure -	11,350

One or other of the following conditions will be found to result from this experiment:—

- (a) The total absence of fly from the area cleared of game.
- (b) A diminution in the amount of fly present.
- (c) Unchanged conditions as regards the prevalence of fly.

The Interpretation of these Results. (a) and (b).—In the event of either of these results being found to have followed the removal or destruction of game within a given area it will then be necessary to determine—

- (1) Whether the fly has died as the result of the removal of one of its sources of food supply; or
- (2) Whether the fly has migrated in search of food.

Until a method for the determination of these essential points be available an experiment giving these results will be open to grave doubts.

(c) The finding of apparently unchanged conditions after the removal of the game will also not be in any sense conclusive as to the result of the experiment.

The bionomics of this fly are as yet but little known, there are reasons to suppose that more than one condition is required for its suitable habitat. At least two conditions may be regarded as essential, namely, a food supply and a suitable breeding ground. One locality may provide one condition, another adjacent one the other. The locality selected for this experiment will in all probability provide both.

On the completion of the experiment food will be sought for elsewhere, possibly where it exists alone. The area over which the experiment was carried out may still be utilised as a breeding ground. This will necessitate a constant interchange of the insects according as to whether they are in search of one or other condition, and little if any information will be gained from the results of game destruction on the numbers of the flies.

(In carrying out this experiment over a limited and properly fenced area it does not seem necessary that the game should be destroyed, results which will prove to be at least equally instructive could be gained by driving the game from the area under observation.)

A study, therefore, of all the circumstances bearing on the experimental destruction or removal of the game from a selected area owing to the large expenditure involved, our present ignorance of the bionomics of this fly, and therefore our inability to correctly interpret the results following this experiment, does not lead to the opinion that this experiment would be justified by its results.

Our knowledge of the facts bearing on the relationship of sleeping sickness to big game, and our knowledge of the present position of the disease do not

justify the agitation which at present exists for the general destruction of game as a preventative of the spread of this disease.

Until there is definite proof (1) that the disease is spreading, (2) that the game is responsible for the maintenance of the fly, (3) that the game is the only reservoir of the disease, or even the chief one, and (4) that the conditions following the destruction of the game will be an improvement on those which at present exist, it is a reasonable conclusion that the presence of this disease as it now exists does not afford sufficient excuse for killing out the game.

Since the date of the last report on this disease a considerable amount of evidence has been obtained to show that in the 18 months which have since elapsed there has been no spread of this disease or evidence of a tendency to spread. There is, on the other hand, increasing reason to think that the disease is an old one and has arrived at a condition of equilibrium, i.e., that it is endemic, and in many respects approximates to the conditions found on the West Coast and that sporadic cases only will continue to be found.

Its present condition therefore requires no hasty or drastic measures "for extirpating the living reservoirs of sleeping sickness," the grounds for which are purely hypothetical and unreliable. All that is required to meet the present situation is a continuation of the work on the lines recommended in the Sleeping Sickness Report for this territory, February 1912, which were as follows (page 27):—

1. That natives in fly areas should be prohibited from keeping domestic animals, goats, sheep, dogs, &c.

2. That they should be encouraged by means of trapping, pits, drives, and, if possible, in being allowed a certain number of firearms, to clear the vicinity of all villages in fly country of game, and that, except in the vicinity of villages, game should be as far as possible unmolested in order to prevent their movement into areas free of infection.

3. That natives should be encouraged to protect all cultivated lands, by means of cuttings or clearings, from fire, with the object of allowing a more complete clearance to be made by means of grass fires. At present, in many places, the native is reluctant to start a grass fire owing to the danger of losing his crops thereby.

4. That the headman of each village should be encouraged by means of small payments, to rid as far as possible the immediate vicinity of his village of fly by means of mechanical appliances for trapping, &c. The details of this work might be under the supervision of the medical officer of the district.

5. That certain routes, to the exclusion of all others, should be used for all natives and others travelling through these areas, and that these routes should, as far as possible, be rendered safe, both by the removal of infected cases and by the encouragement of game destruction in their vicinity.

Corrections.—A considerable number of errors have crept into discussions and statements which have recently been made in England on the question of the relationship of game to the spread of sleeping sickness. It is advisable that these should be corrected.

(1) There is no reason for supposing, as has been done, that sleeping sickness had not occurred in Rhodesia prior to 1908, when the first case was discovered ("Proceedings of the Zoological Society of London," June 1913, page 321). There is, on the contrary, very good reason for thinking that the disease had existed there for many years before that time.

(2) The unqualified statement has been made that "sleeping sickness has already crossed the Zambesi." (*Ibid.* page 328.) The present state of our knowledge offers no justification whatever for what is implied by this statement; it might equally well be said that sleeping sickness had crossed the Zambesi from the south.

(3) The available evidence as to the spread of the disease does not "strongly suggest that during the past few years sleeping sickness has been on the increase" (*Ibid.* page 335); it, on the other hand, shows that the disease has not spread.

(4) It is difficult to understand how the statement that "The most reliable information is to be obtained from the incidence of the disease in Europeans" can be qualified. To anyone familiar with this country and local conditions, it is very evident that the incidence of the disease in Europeans bears no relationship whatsoever to its incidence in natives. A devastating epidemic amongst natives might be raging without 1 per cent. of the European population coming within many hundreds of miles of the possibility of becoming infected.

(5) The statement that "since the Luangwa valley has been closed, quite a number of Europeans have contracted the disease in North-Eastern Rhodesia" (*Ibid.* 336) is quite incorrect. Only one case of the disease has occurred since that date.

(6) The opinion has been expressed that "the spread of trypanosomiasis south is a thing that at present no adequate attempts have been made to prevent." ("Transactions of the Society of Tropical Medicine and Hygiene.")

That these remarks are incorrect is sufficiently obvious, and that they could not be made except as the result of ignorance is also obvious. Their object can only be conjectured.

Unqualified and incorrect statements such as the above should not be made. Their publicity gives them an unwarrantable significance; their only result is to produce a false impression of what is being done by the governments concerned, and to embarrass those who, with the advantage of accurate knowledge at their disposal, are responsible for, and are carrying out, the requisite measures for checking the spread of the disease.

The occurrence of Gl. morsitans transmitted Human Trypanosomiasis outside the confines of the present closed Area.

It was stated in the Sleeping Sickness Report, February 1912, that there was little reason to hope that transmission would be in any way inhibited by the natural conditions existing throughout the territory, and, consequently, that the disease would not prove to be confined to the Luangwa Valley, and similar altitudes in Nyasaland, and the instance of the discovery that the disease was transmissible in the Chinunda district (altitude, 3,500 feet) was then quoted (*vide* page 29).

It has since then been found by Dr. Kinghorn and his co-workers on the Luangwa Sleeping Sickness Commission that temperature exercises a very marked influence on the developmental cycle of *T. rhodesiense* in *Glossina morsitans*. High temperatures (75°–85° F.) favour the development of the parasites, while low temperatures (60°–70° F.) are unfavourable. Cases of the disease have also been found in which the history proved them to be locally infected in various parts of the territory where these unfavourable conditions exist to a greater degree than in the Luangwa Valley.

The disease has been found by Dr. Ellacombe in the Serenje District to the east of Lake Bangweolo, September 1912; by Dr. Kinghorn, in the Mpika District, May 1913; by Dr. Ward, in the Ndola District, 1912–13; and by Dr. MacKnight, in the Luano Valley, September 1913.

It may, therefore, be accepted that, although certain unfavourable climatic conditions exist to a more marked extent in some districts than in others, transmission is possible wherever there is tsetse-fly.

Luangwa Sleeping Sickness Commission.

In April 1912 this Commission was moved from Lawalia, in the Luangwa Valley, to Ngao (Mpika District), on the Congo-Zambesi watershed, with the object of determining what influence climatic conditions have on the transmission of the parasite by *Glossina morsitans*, and thus determining the possible extent to which the disease could spread.

It was found that—

(1) attempts carried out at laboratory temperatures on the Congo-Zambesi plateau during the cold season to transmit the human trypanosomes by means of *Glossina morsitans* were invariably unsuccessful in spite of the

fact that 680 flies were used in these experiments;

- "(2) the developmental cycle of *T. rhodesiense* in *Glossina morsitans* is, to a marked degree, influenced by the temperature to which the flies are subjected. High low temperatures (60°-70° F.) are unfavourable;
- "(3) the first portion of the developmental cycle can proceed at lower temperatures, but for its completion, the higher temperatures are essential;
- "(4) the relative humidity of the atmosphere has apparently no influence on the development of the trypanosomes in *Glossina morsitans*."

The object which determined the formation of this Commission, namely, the incrimination of *Glossina morsitans* as the carrier of human trypanosomiasis having, therefore, been accomplished, the work of the Commission came to an end, at Ngoa, in August 1912.

Very great credit is due to Dr. Kinghorn and his co-workers, Drs. W. Yorke, A. F. Wallace, and Mr. L. Lloyd, for the rapidity and thoroughness with which this work was accomplished.

Modifications in existing Sleeping Sickness Regulations.

Luapula, Mweru and Tanganyika Areas.

The complete removal of the population from all contact with *Glossina palpalis* has rendered it necessary only to prevent the return of these natives to the Luapula and to the shores of Lake Mweru and Tanganyika. A suggested boundary line embodying this recommendation has already been submitted.

Luangwa Closed Area.

The regulations at present in force in this area were designed to prevent the spread of a disease which was then thought to be confined to this area (1910). More recently it has been found elsewhere, and there is reason to think that its occurrence is possible wherever *Glossina morsitans* is found. Transmission, however, being modified by certain climatic conditions, the Luangwa Valley providing more favourable conditions for its transmission than elsewhere in the territory.

It has been found that, notwithstanding the known presence of infection in the Luangwa Valley for at least six years, and in spite of apparently every necessary condition favourable to its spread being present, there is no evidence of any tendency to spread, and that the tendency of the disease is to assume a sporadic character.

It may, therefore, be expected that since the disease is not spreading in this area, it will not do so under less favourable conditions. For these reasons it is thought that a continuation of the present stringent rules and regulations governing the movements of natives in the Luangwa closed area are no longer necessary, and that the restrictions consequent on these to trade and development may now be relaxed.

A recommendation has, therefore, been submitted for the modification of these regulations.

Entomological.

The following is the general programme of work which has been adopted in connection with the investigations now being carried out:—

- (1) Determination of the relative value of various food supplies with reference to breeding capabilities.
- (2) Determination of what parasites affect the fly or pupæ.
- (3) Further investigations into breeding places and habits and habits, namely:—
Is breeding seasonal?
Are the pupæ carefully placed, or are they deposited at random?

Are breeding haunts at special places in fly belts which are visited periodically for the purpose of depositing pupæ, or are the pupæ deposited throughout the belt?

Is there any special relation in the breeding places to—

- Soil?
- Water?
- Shade?

(4) *General*.—As to food, water, plant juices, invertebrate animals' preferences in food, re small mammals and birds.

(5) *Distribution*.—Seasonal variations as to shade, water, game, native cultivation, European settlement, reasons for avoidance of certain areas where conditions are apparently favourable.

(6) *Enemies*.—What animals or insects prey on the fly and its pupæ?

Are there any special enemies apart from parasites (fungi)?

What parasites attack the fly?

What parasites attack the pupæ?

(7) *Trapping*.—Is there a reasonable probability of reducing the number of fly by any form or forms of trapping.

(8) *Tsetse Flies*.—Are there any such in nature?

The Entomological Staff at present consist of Messrs. L. Lloyd, H. C. Dollman, and R. A. F. Eminson.

Mr. Lloyd, since the completion of the work of the Luangwa Sleeping Sickness Commission, has been stationed at Ngoa, Mpika District, and has for the most part been engaged: (1) On the determination of the relative values of the various food supplies with reference to breeding capabilities; and (2) In determining whether *Glossina morsitans* will feed on small mammals, birds, reptiles, or amphibians.

The results of these experiments will be published in detail when the series is completed; in the meantime, with reference to No. 1 sufficient work has not been done from which to generalise upon. Great difficulty was experienced in getting the flies to breed freely in captivity, and the number of pupæ produced can only be regarded as a fortuitous circumstance as far as the work has gone: however, it would seem that as regards the dimensions of the pupæ produced the mammalian series has the advantage, otherwise it would seem that there is little or no advantage in a mammalian diet.

2. A fairly extensive series of experiments was carried out by Mr. Lloyd to determine whether *Glossina morsitans* will feed on small mammals, birds, reptiles or amphibians.

Twenty-nine experiments were carried out, which included lizards, chameleon, toad, fowl, rat, mouse, burrowing rodent, mongoose, bat, shrew mouse, caterpillars, from the result of which it would seem that these animals do not provide a suitable food supply for the fly.

Messrs. Dollman and Eminson have been stationed at Nwengwa near the Upper Kafue, where they have been inquiring into the seasonal variations, breeding habits, and places, and distribution. Their report thereon will shortly be available.

The details of a concentrated experiment with the object in view of determining the relationship of fly to game are now under consideration.

It is thought that the conditions which would obtain after the removal of the game can be produced on a small scale and suitably enclosed, and that careful and accurate observations of the result as regards the life and breeding capabilities of the fly will throw some light on what might be expected to happen as the result of the removal of the game.

These particulars, should further consideration show the experiment to be practicable, will be submitted for approval later.

It is also proposed, subject to approval, to establish a permanent entomological camp within easy reach of the railway, probably on or near the Mulungushi River, to the east of Broken Hill.

The Secretary,
Department of the Administrator.

Domestic Animals as Reservoirs of Trypanosomes.

It is generally known to those conversant with the subject that the domestic animals which live in close association with man in Africa, namely, cattle, sheep, and goats, and dogs, may harbour trypanosomes which are lethal to other domestic animals or to man himself without themselves showing any evidence of disease. Probably in all these cases the animals in question have lived long in contact with tsetse-fly and have, like the game, acquired a tolerance for the trypanosomes conveyed by them. I am not aware that the recorded observations have ever been collated. I have here briefly given a sample of the evidence that the domestic animals act as reservoirs of these trypanosomes; to collect all the observations would take more time than is at my disposal.

Domestic Animals as reservoirs of T. gambiense.

Dogs.—Greig, who visited a sleeping sickness area near Lake Albert, Uganda, in 1904, found that many dogs were dying of a wasting disease. Two were sent to the laboratory three weeks' journey away. They were thin but had no symptom of disease. They died from worms two months later. A trypanosome not distinguishable from *T. gambiense* was found in them, and a dog inoculated from one of them lived seven months and died with the brain lesions characteristic of sleeping sickness.*

Cattle.—Bruce and the other members of the Sleeping Sickness Commission in Uganda infected cattle by causing *G. palpalis* naturally infected with *T. gambiense* to feed upon them. Afterwards clean flies were fed on these cattle, became infected from their blood, and transmitted the infection to other animals by biting. Moreover, of 17 cattle examined in the sleeping sickness area, one was found to harbour *T. gambiense*; it was apparently healthy.†

Sheep and Goats.—Kleine and Fischer, on Lake Tanganyika, in German East Africa, infected several sheep and goats by causing *G. palpalis* infected with *T. gambiense* to feed on them, and in three instances clean flies became infected from these animals and transmitted the infection to others. The blood of the sheep and goats was infective to fly for 99 days.‡

French observers have succeeded in infecting domestic fowls with *T. gambiense*. There is at present, however, no evidence that these animals become infected in nature.

Domestic Animals as reservoirs of other Trypanosomes lethal to Stock.

Dutton, Todd, and Kinghorn, in 1903-1905, made a study of cattle trypanosomiasis in the Congo State. They write as follows:—

"At Kasongo trypanosomes were seen in 5 per cent. of cattle. The percentage actually infected is certainly much larger; yet this herd is increasing. There were no deaths in it from disease during our stay at Kasongo, and the cattle were in splendid condition.

"Trypanosomiasis is common and often fatal among Gambian horses. But two horses in the Gambia found to be infected with *Trypanosoma dimorphon* in December 1902 were still alive, apparently healthy, and were constantly worked in November 1906

"As will be gathered from the notes on the animals present at various posts in the Congo [details are given elsewhere in the paper], individual cattle, horses, &c. seem to be resistant, since they live for some years in places where the disease is of a very severe type. Cattle are susceptible to inoculation with *Trypanosoma dimorphon*, yet at many places in the Gambia, at Cape St. Mary, for example, large herds of cattle in splendid

condition graze over the same ground as the horses

"From all these facts it is not unreasonable to suppose that, like the game, domesticated animals may thrive in spite of actual trypanosome infection and exposure to constant re-infection."*

Sheep and Goats.—Montgomery and Kinghorn, in 1907-1909, in North-Eastern Rhodesia, found sheep and goats regarded by both Europeans and natives as immune. These animals, three and a half months after the diagnosis of the natural infection, were in as good condition as at first and not suspected of sickness by the owners. They write:—

"Speaking broadly, trypanosomes were generally visible in the peripheral blood, both in naturally and experimentally infected animals, and being apparently in good health, it appears to us that they may act as reservoirs *par excellence* for the virus, and by trade and movement may become dangerous potential disseminators of the disease."†

†Pecaud, in Dahomey, studied the trypanosome infections of the smaller domestic animals. About a dozen infected sheep and goats were kept under observation for five to six months; all appeared to be in perfect health. He thinks these animals must serve as reservoirs, especially as wild animals are rare in the region in which his observations were made.

Goat.—Duke, in Uganda, kept at the Mpumu Laboratory a goat naturally infected with *T. nanum*. Seven months after arrival it was in excellent health, trypanosomes being visible in its blood at rare intervals.§

Cattle.—Pollard, in Northern Nigeria, writes as follows:—

"There is in the Munshi division, and in the northern part of the Province, a small black breed of cattle, which is apparently immune to tsetse. At any rate, these cattle can be kept in the Munshi district where no horse can live and where imported Fuhiani cattle all die. I have not yet obtained any blood-films from these animals. It is interesting to note that the Munshis are great hunters and that they have practically destroyed all the wild game in their district, and yet, in spite of this, the trypanosomiasis of cattle and horses is rampant.||

Foy, as a result of his work in the same Colony, concluded:—

"That a certain breed of cattle found in pagan districts possess a high degree of natural immunity in that they may harbour the trypanosome in the blood and yet keep in good condition and show no signs of the disease, nor do they die from the infection so long as their environments are favourable. These environments are a free life, with ample food, especially plenty of green grass. Confinement, poor feeding, and hard exercise tend to make the disease manifest itself clinically.

"That such domesticated cattle may act as a reservoir of infection, since the blood may prove infective at such times when clinical symptoms manifest themselves, although the trypanosome may not be found on making a microscopical examination."¶

This breed was black and thick-set, probably the same as that mentioned by Pollard.

Pigs.—The members of the Portuguese Sleeping Sickness Commission that visited the Island of

* Report of the Sleeping Sickness Commission, Royal Society, No. 8, p. 64.

† Report of the Sleeping Sickness Commission, Royal Society, No. 11, p. 71.

‡ Zeitschrift für Hygiene und Infektionskrankheiten, 1911, Vol. 70, pp. 1-23.

* Annals of Tropical Medicine and Parasitology, Vol. 1, p. 268.

† Annals of Tropical Medicine and Parasitology, Vol. 3, p. 335.

‡ Bulletin de la Société de Pathologie Exotique, 1901, Vol. 2, p. 127.

§ Proceedings of the Royal Society, B. Vol. 85, pp. 4-9.

¶ Bulletin of Entomological Research, Vol. 3, p. 219.

¶ Journal of Tropical Medicine and Hygiene, 1911, p. 307.

Principe in 1907-1908 found that the tsetse-flies fed chiefly on pigs. In the blood of most of these trypanosomes were found. The pigs were fat and their health was unimpaired.*

Similar observations, of older date, could be collected from the writings of R. Koch.

* Archivos de Hygiene e Pathologica Exoticas, 1909, Vol. 2, p. 336.

It is worth recalling in this connection that domestic cattle are generally recognised in India and Indo-China as forming reservoirs of surra.

There is, at present, no evidence that domestic animals serve as reservoirs for *T. rhodesiense* in Rhodesia and Nyasaland; no domestic animal naturally or artificially infected with this trypanosome has survived long.

ARTHUR G. BAGSHAWE.

March 19th, 1912.

EXTRACT FROM REPORT ON HUMAN TRYPANOSOMIASIS in the WESTERN PROVINCE of ASHANTI.

Prophylaxis.

Dr. Kinghorn said in his report in 1910:—

"The measure I think will be most easily applied and which will effect a great amount of good is the enforcement of clearings around all the villages and the water supplies, the ferries, and the fords. If this is carried out intelligently, it should be possible to protect these places from the flies, and thus in time bring about diminution in the incidence of the disease. The villages are habitually used as resting places by the floating population, amongst whom trypanosomiasis exists to some extent, and they frequently remain for a considerable length of time in one place. As I have pointed out, the difficulty of controlling this class of native is almost insurmountable over the greater part of the country, and every effort must be made to prevent them from acting as disseminators of the disease, as well as those cases which exist amongst the indigenous population. The clearings can be done by the villagers themselves without remuneration, and, in most instances, if supervised, would not entail a great amount of work. It is hopeless, of course, to expect that this work will be carried out without some persuasion. The general supervision of the clearings and the other measures must rest in the hands of the Medical Staff."

"The Commissioner of the Western Province wrote in 1911, when I was Medical Officer of Sunyani: 'The following extracts from my report on my recent tour may be of interest to you, and you may consider it advisable to transmit them to the head of your Department. Sleeping sickness.—I inquired in Gaman and Wenchi into the result of the cases diagnosed last year by Dr. Kinghorn. The result is surprising, and would tend to show that the disease is of a more virulent type than was originally supposed. In Pulliano, three of the diagnosed cases have died in March or April. In Seketia, two; in Korase, two; in Mengi, one, a stranger; and in Wenchi, out of 17 cases, all are dead except two who are now in *extremis*. It is probable that in other Gaman villages and in Berekum there has been the same percentage of mortality, and it is impossible to conceive that with all this mortality there is a diminution in newly infected cases. I still think that Dr. Kinghorn's recommendation for the establishment of an observation station in Gaman or Wenchi is worthy of consideration, and that our joint recommendation for a native staff to travel and see to the making of clearings round villages and the general improvements of towns and sanitation is the practical step which it is incumbent upon the Government to take."

"T. E. FELL,

"Tekiman.

"12th September 1911."

"C.W.P.A."

These clearings around villages recommended by Dr. Kinghorn and the Commissioners I entirely agree with, and last year I made recommendations of a similar nature, more extensive I must admit, but very necessary if they could be possibly carried out. After

going very thoroughly through the province, I must say that if any attempt has been made to carry out these recommendations it has proved a failure; in a few villages it has been done, but has not been maintained, and if clearings once made are not going to be maintained I believe it would be better not to make them at all.

There are two reasons to my mind why these clearings are not made, or when made are not maintained by the natives:—

- (1) The disease causes no alarm to the native mind.
- (2) The natives do not think the Government are serious.

1. A disease occurring sporadically, with a gradual on-set, chronic course, and lingering death is a natural sequence of events, one that to the native mind cannot be prevented; therefore, why should it alarm them? If it were small-pox, or some epidemic which rapidly killed off numbers, they would become alarmed; but, with this disease, no; especially when it so often happens to be the least important members of the community who are picked out by the disease.

2. Year after year the medical officer on sleeping sickness duty goes round this province telling the chiefs and people to make these clearings, and frequently marking out the 100 yards around their villages for them, only to find on his return each year that nothing has been done. The natives find that no notice is officially taken of their disobedience, therefore, why should they treat the matter seriously?

I shall here recommend what I consider should be the minimum to be done this year: 100 yards clearings around all villages and zongos on the main trade roads and around all bush villages where at present there are any cases of trypanosomiasis, and that these clearings, when made, be kept properly maintained; 50 yards clearings around the water supplies and washing places of all these villages. When making the clearings, I do not think it necessary to cut down all the trees, but to thin out the forest, and root out all the bush and undergrowth. If any large trees after the thinning out has been completed give too much shade, they could easily be ring-barked. I suggested last year that these clearings might be used for growing sweet potatoes, and, where the soil was suitable, ground nuts; and that any clearings made beyond the 100 yards might be used for yams or corn. The native objection to this was that the cattle and sheep would eat their crops. I suggested fencing in the farms, but to this they did not seem to be agreeable. I now think that if these clearings were planted with dube grass and looked after for a while, they would make an excellent pasturage for sheep and cattle, and would be a much more suitable place to keep them than at present—in their streets and compounds. I do not say that 100 yards clearings are sufficient to keep tsetse flies out of the villages, but they would at least be a start in the right direction, and could be gradually increased. I look on these clearings as necessary sanitary measures, and I think if any person were to go round the outskirts of as many of these villages as I have done they would agree with me. As regards the 50 yards clearings around water supplies and washing places, what is required here is not the cutting down of trees, but the thorough rooting out of the undergrowth and planting of Bahama grass. These measures should certainly be done by the natives living in the villages and zongos, and, if carried out systematically, would not entail an enormous amount of labour. I may say that, apart

from Sunyani town and zongo, the natives are very rarely asked to do any sanitary improvements. If the work of seeing to these clearings is to be done by the medical officer in Sunyani, then I think it will be necessary to give him some power, especially since this matter of clearing has not been treated seriously for the past three years. This province, with an area of about 6,000 square miles, is too large to keep constantly going round telling the chiefs and people to clear. I look on the matter in the same light as road clearing, and, as the natives were not educated up to clearing their roads without compulsion, neither will they be educated up to clearing around their villages without similar measures.

I would suggest that four native overseers should be appointed who could travel about from village to village and mark out these clearings in the different villages and zongos, and report monthly to the medical officer at Sunyani what progress is being made. I do not think that these overseers should remain in a village till the clearing is made, as if [they did] they would never get round the province. In 1910 I sent an intelligent coast native to a town in this province to have a clearing made; the natives kept putting off the work from day to day till eventually the overseer got disheartened and took to drink, and finally got into trouble. I do not think it is fair, therefore, to keep these overseers in one place any length of time.

The medical officer in Sunyani should be given every opportunity to travel, and for this purpose should be supplied with carriers, who would be at his disposal when he required them. At the present time there appears to be no such supply. In 1911, when I was stationed in Sunyani, I received instructions to proceed round the province with the Chief Commissioner; on applying for carriers, I was informed that they would not be available for five months, so was, therefore, compelled to use the labourers in Sunyani station as carriers. If there is no vote for transport for the medical officer, could not some arrangements be made whereby he might be allowed to use the labourers under his control as carriers?

Segregation.—Segregation of the infected persons in the province would be an excellent prophylactic measure if it could possibly be carried out, but I believe it would be impracticable here. Even if the infected residents of the province were segregated, which could only be done by using strong measures, there would always be the floating population travelling in and out of the province, among whom are many trypanosome carriers, and who, I believe, constitute the greatest danger.

Roads.—The new roads which are at present being made through the forest, though they are a great improvement as regards transport, to my mind are more dangerous as regards trypanosomiasis. Moving objects seem to be a much greater attraction for tsetse flies on these broad sunlit roads than on the dark ones. On each side of these roads where the forest has been cleared back for a few yards there is now a tangled mass of moisture-laden vegetation, shaded by the branching trees, and forming an ideal habitat for tsetse flies. As the sides of these roads cannot be kept cleared I think the best thing is to encourage undergrowth to grow as high as possible, and so shut off the roads from the forest. The first great prophylactic measure which the Government has taken against trypanosomiasis in Ashanti was the prohibition of the importation of north country slaves into the country; north country natives are now coming into the country in much greater numbers, not as slaves, but as free people doing the transport work of the country, and I believe the next radical measure which would prove of greatest use would be the driving of these north country carriers out of the endemic area. The only legitimate way, as far as I can see, that this can be done is by competing with them in their trade, and that can be accomplished by building railways. If railways could be run through the endemic areas where north country natives are employed in large numbers, I think there would quickly be a diminution in the north country population, and

also in the incidence of this disease; but until some such measure as this is undertaken I believe trypanosomiasis in this province will remain, as at present, in a state of equilibrium.

It has been suggested that the north country natives bring the disease down with them into Ashanti. It would serve a useful purpose if it could be definitely settled whether or not trypanosomiasis occurs amongst the natives of the Northern Territories who have never left their district. As a number of Moshis are found infected in Ashanti, similar information from the French authorities would be valuable.

Laboratory.—Useful experiments could be carried out in a well-equipped laboratory by a specially qualified Protozoologist, and if the Government considered such a proposal, plenty of material for working on could be obtained in Coomassie, which would be a most convenient place.

Treatment.—Under this heading I have very little to say. The native method of treatment which is applied to all enlarged glands, no matter from what cause, consists in scarifying the glands and applying some irritating substance, which generally causes the enlarged glands to slough away. I have had an opportunity of examining a few of the people who had submitted to native treatment for trypanosomiasis, and whenever I succeeded in getting gland juice I found trypanosomes. Instead of this treatment doing good, in my opinion, it only destroys one of nature's lines of defence against the invading trypanosome. This treatment causes the enlarged glands to disappear, and so for a time raises the patient's hopes.

As far as atoxyl treatment is concerned, I believe if the people infected could be got to attend the hospital in Sunyani a lot of good could be done. I do not think Sunyani by any means a suitable place to bring a number of infected persons into, but it is the only station in the province in which a medical officer resides, and so the only place where treatment can be given. I took a considerable amount of trouble in trying to get the infected ones to come to Sunyani, and gave each a letter to the medical officer; so far, only ten arrived at the hospital, and of these only two or three attended for a short time. One reason, I think, [which] keeps some away is the fear of running into debt. Sunyani is by no means a cheap place for strangers to live in, and the guardians of the infected ones are unwilling to undertake the trouble and expense of bringing them in and keeping them here for a course of treatment. I would suggest that the infected persons who come to Sunyani and remain for treatment be granted subsistence at the rate of, say, two shillings a week, to be paid to them if they attend hospital regularly. The expense would not be great, as for some time few would take advantage of the offer.

A segregation camp where the infected persons could be kept together would simplify matters considerably as regards treatment, but I am afraid it would be impossible to keep people in the early stages of the disease in it. A prison instead of a camp would be necessary, and unless the Government are prepared to use force the result would be an expensive failure.

Of the twelve people who received atoxyl treatment here in 1910 five (north country natives) are alive and fit at present; four others were fit up to the end of 1911, but have since been lost sight of, and three who were in a late stage of the disease when first discovered have died. I enclose with this report full statistics of all cases of the disease found since these investigations started in this province in 1910. I also enclose two maps, one giving the towns and villages visited, with the number of cases of the disease found in each since 1910, and the other giving the distribution of tsetse flies in the province.

W. M. WADE,
Medical Officer,
Sleeping Sickness Duty.

Sunyani, Western Province, Ashanti,
25th June 1913.

APPENDIX D.

SUGGESTIONS FOR FUTURE WORK.

By Mr. E. E. AUSTEN and Dr. A. G. BAGSHAWE.

In the course of their inquiry the Committee have had the opportunity of noting that some facts relating to its subject are fairly firmly established, while others rest on a less secure basis. Their views in this respect are indicated in the Report. It may safely be said that of the experimental researches few have been on a sufficiently large scale or confirmed sufficiently widely over Africa to afford satisfactory support for a final scheme of preventive measures. It is not proposed to repeat here what may be extracted from the Report, but it seems useful to indicate some lines of enquiry which may help in the elucidation of the general problem. These are given below under the heads Entomological Research and Medical Research.

Entomological Research.

In all work dealing directly with the insect carriers of trypanosomiasis the ultimate aim and object, namely, the *elimination of the fly*, must be kept prominently in view. When seeking to compass the local eradication of these insects, observations of both direct and indirect importance in connection with the bionomics of tsetse flies will almost certainly be made; yet it is none the less important that operations should not be commenced in desultory fashion, but that, so far as possible, each investigator should be provided with a definite scheme of work.

The following are among the subjects to which special attention should be directed:—

(i) *Function of the sensory labyrinth in the second and third joints of the antenna of Glossina.*—It is not yet known whether this is olfactory or auditory. Knowledge of the kinds of stimuli by which the organ is affected would be a useful aid in connection with trapping.

(ii) *Influence of Odours.*—Efforts should be made to determine whether tsetse flies, in choosing a breeding-place, are in any way influenced by odours; or whether, as Mr. Lloyd believes in the case of *G. morsitans*, the existence of a relatively dark spot "where the mother fly can hide during pregnancy" is the dominant factor. The odours by which insects are attracted are not necessarily perceptible to the human nostril, and consequently it does not follow that, because the breeding-places of *G. morsitans* and *G. palpalis* appear to human beings to have no distinctive odour, such odour is not apparent to a pregnant tsetse fly. In India it has recently been found by Mr. F. M. Howlett that *Stomoxys calcitrans* will oviposit freely "on cotton-wool soaked in valerianic acid, one of the acids present in the fermenting vegetable stuff in which the eggs of this species are naturally deposited." Although *Stomoxys calcitrans*, instead of depositing at intervals a single full-grown larva, lays eggs in the ordinary way, the fact that it also is a blood-sucking fly and somewhat closely related to *Glossina* perhaps affords some grounds for hoping that, if it be possible to analyse and separate the components of the humus in tsetse fly breeding-places, some substance or essence of a specially attractive nature may be found. If discovered, this would of course be used in connection with—

(iii) *Artificial Breeding-places.*—Every effort should be made to construct these, and to test their practical efficacy. If bird-lime or other sticky substance were applied to the under surfaces of the dead branches or other timber used in the construction of such breeding-places, the latter might form traps for the adult flies as well as for pupae. It is important to note that, in order that artificial breeding-places may have a reasonable chance of success, all known or probable breeding-places must, so far as possible, first be destroyed or rendered useless.

(iv) *Range of Flight.*—Definite knowledge on this subject is urgently required in connection with *G. morsitans*. Many of the statements on the subject made to the Committee in the course of the inquiry were mere guesses, or based on irrelevant or insufficient data. Important information on this head might, however, be gained in connection with experiments to determine—

(v) *The Precise Effects of Clearing.*—At present we do not know whether clearing actually results in the death of the flies, or merely causes them to migrate. If it were possible, in *G. morsitans* country, to find a series of small isolated patches of tsetse, each separated by a different distance (400 yards, half a mile, three-quarters of a mile, and so on) from the next patch of cover, the actual effect of clearing might be ascertained by marking and liberating a large number of *G. morsitans* in each patch in succession, and then destroying the vegetation and noting the result. If possible, a method of marking which does not in any way impair the activity of the flies should be adopted. The desired end might be attained by collecting large numbers of pupae, and breeding out the flies in cages so arranged that the insects on emerging become permanently marked with coloured powder.

(vi) *Systematic Trapping and Catching continued for a considerable Period of Time (say One Year).*—This should be given a thorough trial. In the Island of Principe a marked reduction in the numbers of *Glossina palpalis* has been effected by catching the flies by means of black cloths smeared with bird-lime and worn by natives. The number thus trapped was 95,000 in the last six months of 1912, or over 500 a day. When tried in German East Africa, however, in the case of *Glossina morsitans*, the method yielded but poor results. It is suggested that the failure may be due to the employment on the cloth of some substance which is actually distasteful to the flies and that further experiments with the same substance that is used in Principe would be of value. It is obvious that catching the adults must, relatively at any rate, produce a far greater effect on an exceptionally slow-breeding fly like *Glossina*, than on an insect such as the house fly, which may produce six hundred eggs at one time. To this extent we are aided at the outset by nature. Correct conclusions as to the value of trapping and catching cannot be deduced from a trial lasting only two or three months, in which but a handful of natives are engaged; the value of lined cloths and of nets in the hands of expert fly-boys should be tested in a selected locality for at least a year, and on a large scale. Some hundreds of natives should be employed in the work, and a corps of, say, a dozen keen and energetic fly-boys should be established in each village in a tsetse area. Payment should be by results, but small rewards would suffice, and the system should therefore prove inexpensive in working.

(vii) *Insect Enemies.*—In the case of a noxious insect, which exists in such numbers as to constitute a plague, it is useless to expect effective aid from endemic enemies. Were such enemies capable of controlling the harmful insect, the latter would never have become a plague, and if results of value are to be produced by this method enemies must be sought for elsewhere. In the United States of America attention has recently been drawn to the life-history and habits of a minute Hymenopterous (Chalcid) parasite known as *Spalangia muscidarum*, which has been bred in large numbers from the pupae of *Stomoxys calcitrans*, and also attacks the pupae of other flies, including *Musca domestica* and *Lyperosia irritans*. Only one species of the genus *Spalangia* (parasitic on a fruit fly) is at present known to occur in Africa, but in North America in addition to *S. muscidarum* several other species are found; these include *S. haematobiae*, which attacks *Lyperosia irritans*, and *S. nigra* and *S. hirta*, both of

which parasitise *Musca domestica*. It would be well worth while to make an effort to obtain a good supply of pupae of all the species of *Spalangia* mentioned, and to endeavour to establish these parasites in a *Glossina morsitans* area, and watch the result. The *Spalangia* pupae could be conveyed in cold storage, in infected fly-pupae. The fact that both *Musca domestica*, *Stomoxys calcitrans*, and more than one species of *Lyperosia* are found plentifully in parts of Tropical Africa infested by *Glossina* would seem to increase the possibility of establishing one or more species of *Spalangia* in these regions. For details of a method of artificially propagating *Spalangia muscidarum*, see H. Pinkus, "The Life-History and Habits of *Spalangia muscidarum*, Richardson; a parasite of the Stable-Fly" (*Psyche* (Boston, U.S.A.), Vol. xx., No. 5, Oct. 1913, pp. 148-158, 1 pl., 1 fig.): see also C. H. Richardson, "Studies on the Habits and Development of a Hymenopterous Parasite, *Spalangia muscidarum* Richardson" (*Journal of Morphology* (Philadelphia, U.S.A.), Vol. xxiv., No. 4, Dec. 20, 1913, pp. 513-549, 4 pl.). For abstracts of both papers, see *The Review of Applied Entomology*, Series B, Vol. II., Part 2, Feb. 1914, pp. 22-24.

(viii) Food. — A large series of *G. morsitans* caught where game is plentiful should be examined to determine the percentages which have fed respectively on mammals, birds, and reptiles. Mr. Lloyd in Rhodesia examined 310 flies and found mammalian corpuscles in the gut of 70, and nucleated red corpuscles, reptilian or avian, in the gut of 12, i.e., in 15 per cent. of those which contained undigested blood. This is a considerable percentage, seeing that these flies are believed by many to depend on game for their subsistence. The examination should be repeated over a larger series, and an effort should be made to distinguish, as Dr. Carpenter did, avian from reptilian blood.

Further, a large series of *G. morsitans* in an area free from game such as that described by Major Stevenson Hamilton (para. 1885) should be examined to determine on what food they subsist, and whether they contain trypanosomes pathogenic to laboratory animals or stock. This might be done in an "experiment-of-game-destruction" area, if the flies remained in it and no favourable opportunity occurred under natural conditions, but a naturally game-free area is preferable, because here the flies have had time to adapt themselves to their environment and possibly have learned to attack animals which ordinarily they disregard.

Medical Research.

(i) Reservoirs. — (a) There are certain species of mammals which have not been examined at all or only insufficiently. Such, besides smaller mammals, are bush-pig, baboon, aardvark, hippopotamus. Attention should be directed to these creatures whenever they are found in fly areas. It is important that a sufficient number of young animals be examined, because old animals may have acquired immunity and rid themselves of the parasites.

(b) From evidence which came before the Committee it is possible that man himself may, under special conditions, be tolerant of *T. rhodesiense* and hence an important reservoir. A large series of apparently healthy young adult natives in an infected area should therefore be subjected to examination, their blood being scrutinised under the microscope, and inoculated into susceptible animals. There are difficulties in carrying out such examinations owing to the prejudices of the natives, but these could be overcome.

(c) Can birds harbour trypanosomes of man or stock, and do they do so in nature? The importance of this investigation depends on the extent to which tsetse feed on birds; this has not yet been satisfactorily ascertained for *G. morsitans* nor even for *G. palpalis*. Dr. Durham in London inoculated a kestrel (*Falco tinnunculus*) with rat's blood swarming with nagana. The blood of this bird was infective to rats 31 days later. He concluded that birds of prey might conceivably be reservoirs of infection.

(d) A fresh test of the infectivity of the fly on the shore and islands of the Victoria Nyanza should be made (the last was in 1911), and more vigorous efforts should be exerted to prevent the clandestine visits of possibly infected natives to these places, for such visits may vitiate any conclusions drawn from the experiment.

(e) There is abundant evidence that native herds in certain parts of Central and West Africa are, like wild animals here and in other parts of Africa, tolerant of the presence of the trypanosomes which are continually injected into them by the local tsetse flies. Whereas domestic animals which are introduced rapidly sicken and die, the animals which have been long subject to infection take no harm. These animals, cattle, sheep, and goats, are easily handled, and their infection can be studied under natural conditions, but little attention has hitherto been paid to them. A study of their serum might lead to knowledge of the cause of their tolerance and enable us, by injections of serum or vaccines or otherwise, to produce a similar condition in introduced cattle, if *immunitas sterilisans* is not attainable.

(ii) Transmission Experiments. — Transmit *T. gambiense* by *G. morsitans* and study carefully the infection produced in laboratory animals, with a view to the detection of a change in virulence or in the morphology of the trypanosomes. This experiment has been suggested by Professor Mesnil. It may be that with a change of invertebrate host there is a change in the biological character of a trypanosome. It is not yet certain that posterior nuclear forms may not be detected in *T. gambiense* infections even when transmitted by *G. palpalis*.

In transmission experiments with tsetse-flies and human trypanosomes it is not found possible to obtain a permanent infection in more than 5 to 10 per cent. of the flies. Miss Robertson, who has especially studied this problem, believes that the failure of the 90 to 95 per cent. to become infected is due to the tendency of the flies to digest their parasites. This problem is worth further study. It is possible that the small incidence of sleeping sickness in parts of Africa where the disease has been long established is due to the low percentage of flies which is capable of becoming infected rather than to immunity in man. It should be noted that the few attempts which have been made to transmit *T. gambiense* by *G. palpalis* in West Africa have failed, and that all our data concerning transmission of this species by *G. palpalis* were obtained in regions where *T. gambiense* has only recently established itself, e.g., Uganda and German East Africa.

(iii) Action of Human Serum. — Test in Africa the effect of healthy human serum on *T. rhodesiense*, both *in vitro* and *in vivo*. The recorded work has been mostly done in European laboratories. It is considered to be an important point of distinction between the two human trypanosomes that one is susceptible (like *T. brucei*), the other much less susceptible, to the action of human serum.

The work in Africa is to some extent contradictory. Dr. Weick, in German East Africa (Rovuma), found that injection of human serum did not prevent the infection of monkeys with *T. rhodesiense*. Dr. Duke, on the other hand, found that when human serum was injected into a monkey infected with *T. gambiense* the trypanosomes disappeared from the circulation for a longer or shorter period. Probably rats or mice should be used in such experiments.

(iv) Diagnosis by Cultivation. — There is doubt whether the method of diagnosing the presence of trypanosomes by keeping suspected blood in contact with artificial media, with a view to obtaining a multiplication of the parasites, has had a sufficient trial. There are difficulties in the field, but the method should have a further trial in places where there are well-equipped laboratories. The advantages of this method are obvious, the main disadvantage being that when more than one species of trypanosome may be present in the blood there is no means of telling which has developed in the culture; that is, the test serves for the diagnosis of trypanosomiasis, but not of the kind of trypanosomiasis. This, however, is of no

moment in the case of man. It may be pointed out that in kala-azar, a disease caused also by a protozoal parasite, the cultivation method has been found of value, and that improvements in media have lately been introduced by the numerous workers who have cultivated malarial parasites.

(v) *Racial Immunity*.—Dr. Wade, who has studied sleeping sickness in Ashanti, where he found two per thousand natives infected, concludes that most of the infected persons are strangers from the north, who are

less resistant to infection than the indigenous natives. He believes that the disease would die out, or at least much diminish, if these north country natives were to cease to visit Ashanti. It would be worth while to study carefully the racial history of natives found infected with *T. rhodesiense* in Rhodesia and Nyasaland. It may be that these are the descendants of natives who had not long been in contact with tsetse and hence had not developed racial immunity, as the mass of the natives may have done.

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- Glossina brevipalpis:
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- somewhat Rare, but commonest in N. Nyasaland,
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- no Evidence of transmission of sleeping sickness by,
Neave 4361
 Found in Nyasaland, *Sharpe* p. 272

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- Carrier of *T. dimorphon*, *Dalsiel* p. 249
 Destruction by clearing along river courses suggested, *Mesnil* p. 226
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- Boundaries vary considerably, *Neave* 4354
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 Attraction by motion uncertain, *Stohr* 5780-81
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 Bites human beings, but does not get much blood, and would not feed on them if game failed, *Stohr* 5783-4
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 Men seldom bitten, *Bruce* 244-8
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- Believed to breed at centres where congregated during dry season, *Shircore* 3602, 3658, 4917-8
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- Finding of, feasible and plan for destruction of fly in, *Lloyd* 5888-9
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 Instance to support theory of breeding in dry season, *Lloyd* 6064-7

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- One spot used by many flies to deposit larvae, *Lloyd* 6069-70
 Positions, closely associated with paths and relatively dark, *Lloyd* p. 268
 typical Bush-loving tsetse, *Mesnil* p. 225
 Carrier of trypanosome in game which is supposed to be identical with *T. rhodesiense* of human beings p. 283
 Clearing of vegetation not essential factor in combating, *Drew* 4476-8
 Connection with presence of game, question of, *Hodges*, 1522, 1886; *Shircore*, 3607; *Selous*, 3003-4; *Garden*, 3990-5; *Drew*, 4454; *Stohr*, 5831-2; *Dalsiel*, p. 250.
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 Description, *Bruce* p. 234
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 by Attacking breeding places possible, but probability difficult to estimate in absence of more observation, *Lloyd* 5941-3
 by Attacking concentrated areas at end of dry season not feasible, *Stohr* 5641
 by Burning of bush suggested, *Pearce*, 4848; *Mesnil*, p. 226.
 Experiment with artificial breeding places covered with bird lime suggested, *Lloyd* p. 268
 by Introduction of enemies, worth trying, but success doubted, *Neave* 4385-87
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