

## Correspondence.

### SLEEPING SICKNESS.<sup>1</sup>

TO THE EDITOR OF THE "TIMES."

SIR,—Questions of priority in scientific discovery are usually of more interest to the individuals concerned than to the general public; nevertheless, I trust that, on the plea of desirability for historical accuracy in connexion with an important disease and discovery, you will find space for this letter.

Those who are familiar with the history of our knowledge of the connexion of the trypanosome with sleeping sickness are probably aware that the disease and parasite were found in association for the first time by myself. Naturally, I claim the discovery. It would seem, however, that certain persons do not regard my claim as valid, or they maintain that, at the most, it is of quite secondary importance to that which they assign to Colonel David Bruce.

Soon after my return from Uganda in 1903 this was made unpleasantly apparent to me, and I daresay that but for Continental opinion and that of a few friends in England, my name would have quickly vanished from the roll of those who have worked more or less successfully to elucidate the mystery of this terrible African disease.

During four years' absence from Europe I have been subjected to a series of misrepresentations on the part of Sir E. Ray Lankester. On my return to England a few days ago, my attention was called to a book entitled "The Kingdom of Man," written by him, in which these misrepresentations are restated and greatly accentuated. I deem the time has come when, in self-respect, I must speak out. In the chapter devoted to sleeping sickness Professor Lankester makes the following statements:—

(1) That, though it was Castellani who first found the trypanosome in sleeping sickness, he gave no importance to it, and when Colonel Bruce arrived in Uganda, Castellani, still having this idea about the trypanosome, simply mentioned the matter to him in a colloquial way.

(2) That Colonel Bruce immediately saw the importance of the observation, and then, as a matter of noteworthy concession, "generously allowed Castellani to take part in the investigation, which resulted in the immediate discovery of the trypanosome in the cerebro-spinal fluid of twenty cases out of thirty-four.

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(3) "Castellani returned to Europe three weeks after Bruce's experiments were commenced and announced the discovery, which has been in consequence erroneously attributed to him, although mainly due to Bruce."

To show the incorrectness—to use a mild word—of Sir E. Ray Lankester's statements, I will briefly give the exact history of this matter.

Before Colonel Bruce arrived in Uganda I had examined for trypanosomes fifteen cases of sleeping sickness, and found the parasite in the cerebro-spinal fluid of five and in the blood of two. Before the arrival of Colonel Bruce I acquainted Dr. Moffat and Dr. Baker, of the Uganda Medical Service, with the fact that I had observed a trypanosome in the cerebro-spinal fluid of several cases of sleeping sickness, and made them promise not to divulge it either to the members of the second Commission (Colonel Bruce and Dr. Nabarro), when they should arrive, or to anyone else.

Now, if I had considered the presence of trypanosomes so unimportant, why should I have wished these gentlemen to keep my discovery secret? Moreover, before Colonel Bruce arrived I wrote a letter to Professor Dainelli, of Florence, in which, speaking of the etiology of sleeping sickness, I stated that I was beginning to regret that I had committed myself to the streptococcus theory, and that it had now occurred to me that sleeping sickness might be a trypanosome infection, the streptococcus being only a secondary agent. I added that I wished to go home for a few months, and then try to get back to Uganda again and work on this trypanosome theory. This letter is still in Professor Dainelli's hands.

Colonel Bruce arrived in Uganda on March 16th, 1903. Two days later I acquainted him with my discovery of the presence of trypanosomes in several sleeping sickness patients. And how did I acquaint him? According to Sir E. Ray Lankester I simply mentioned the observation without giving it any importance. This is absolutely incorrect. I acquainted Colonel Bruce in this way: I wrote him a note asking him to be so good as to come to my bungalow, as I thought it was my duty, as a member of the first Sleeping Sickness Commission, to acquaint him with some observations I was sure would interest him greatly, seeing that they opened up a new line of investigation. He kindly came. As conditions to my acquainting him with the new line of investigation I asked:—

(1) That I should remain a few weeks longer in Uganda to continue my observations in this line.

(2) That my work till my departure from Uganda should be independent and my own, and that he and Mrs. Bruce should control my findings.

(3) That at the end of my stay in Uganda I should write a note and that this note should bear my name, and my name alone.

If the idea of a possible relation between trypanosomes and sleeping sickness had not occurred to my mind, and if I had considered the trypanosome as unimportant, as Sir E. Ray Lankester would have people believe, why should I have made these conditions?

Colonel Bruce acquiesced to all of them and gave me his word of honour not to break them. On receiving this promise, I then acquainted him with my discovery of trypanosomes in several cases of sleeping sickness. Colonel Bruce was, of course, very much interested, but he said that as the Uganda natives are full of all sorts of parasites, the trypanosome might easily be a harmless parasite like *F. perstans*.

Work was at once started, and the same day I performed a lumbar puncture on the patient Kaperi, and there and then I showed Colonel Bruce the trypanosome in the cerebro-spinal fluid of this boy. All went well for two days, during which time Colonel Bruce learnt from me the technique necessary for finding the trypanosome in the cerebro-spinal fluid. On the third day his manner towards me changed. As I was far from looking well, he advised me, in a somewhat abrupt manner, to go home, and gave me the advice to stick to the streptococcus theory. I answered that I wished to remain a little longer and to make some additional observations as agreed. He finally consented to this, and we got on again on very good terms, so much so that when, on April 5, 1903, I brought him a rough draft of my note, he very kindly corrected the English of it. Moreover, he gave me a letter to the Secretary of the Royal Society, in which he recommended the publication of this note. How, then, I would ask, can Sir E. Ray Lankester offensively hint that I acted dishonestly in publishing my discovery?

In this note I described the special technique to be used, and in a table I gave particulars of the thirty-four cases of sleeping sickness I had examined up to date for the presence of the trypanosome, which I had found in twenty of them. I stated, as a conclusion, that I thought the disease to be probably due to the trypanosome, and that the streptococcus was probably a secondary infection. Colonel Bruce did not take exception to any part of the note; he only advised me not to come to any conclusion as regards the etiological significance of the trypanosome. This advice I did not take. On my voyage home I sent from Port Said, on April 26th, a registered letter to the secretary of the Royal Society, stating that, influenced by my latest investigations, I considered the trypanosome to be the probable cause of the disease.

On my way to London I stopped, on May 3rd, for a day in Bonn, and acquainted more completely Professor Kruse, my old teacher and friend, with my results and conclusions. The same day he mentioned these to Professor Finkler and others, and soon after he made a communication on the subject to the Medical Society of Bonn. (See *Sitzungsberichte der Niederrhein Gesellsch. f. Natur. u. Heilkunde zu Bonn*, May, 1903.)

As regards my original note, it was published in the *Proceedings of the Royal Society* in May, 1903. This is a very different story from that given by Sir E. Ray Lankester; it is the true one, nevertheless.

With all due respect to Sir E. Ray Lankester, the public must not take him seriously when he plays historian of medicine. It is not very accurate for a zoologist, a protozoologist, and a medical historian to state, as he did in a book published in 1903, that Bruce discovered the nagana trypanosome before Evans found the surra trypanosome. Every medical student knows that Evans's reports were published many years before those of Bruce. As regards the investigation of sleeping sickness, he simply ignores everyone else except Bruce; the names of all those who directly or indirectly had some part in the elucidation of the disease, such as Forde, Dutton, Todd, Nabarro, Baker, Low, Manson, Sambon, Brumpt, &c., are absent or hardly mentioned; the part I took is minimised and misrepresented. In his eagerness to champion Bruce, Sir E. Ray Lankester states that Bruce found the trypanosome in twenty patients out of thirty-four. In this way Sir E. Ray Lankester gives Bruce credit for having investigated some of my patients who had been dead months before Bruce arrived in Uganda. Anyone who has the patience to go through Reports I. and II. of the Sleeping Sickness Commission of the Royal Society, can easily see through this piece of unjustifiable special pleading.

If Sir E. Ray Lankester's statements are often inaccurate when he discusses questions of protozoology, they are not much better when he tackles helminthology. It is somewhat strange to hear from a director of the most important Natural History Museum in the world that the adult form of *Filaria sanguinis hominis* (being a zoologist he might more correctly call it *F. bancrofti*) is one-fifteenth of an inch in length (!), and that the development of the embryos of the same filaria in the mosquito is simply a suggestion "no proof of which has ever been made."

If Professor Lankester had taken the trouble to leave his professorial armchair for a moment to consult any of the recent works on parasitology on his shelves, he could have ascertained that the "adult" form of *F. bancrofti* measures from three to four inches in length, and that its development has been thoroughly worked out from the time it enters the mosquito as a microscopic larva a hundredth of an inch in length, till it creeps out of the proboscis of the same insect an object visible to the naked eye.

I could point out many other gross inaccuracies in Sir E. Ray Lankester's "Kingdom of Man," as well as in several of his other works. I merely mention these as they occur in juxtaposition to those he has made with regard to myself. They serve to show that the Professor is not to be regarded as a trustworthy guide, either in medical zoology or in medical history.

I see that Professor Lankester has not repeated in his "Kingdom of

Man" the error about the date of the discovery of *T. evansi*. This capacity for practical repentance, although it is not accompanied by acknowledgment of sin, leads me to hope that, in some future work, he may correct the gross errors I have referred to, and, possibly, that he may yet see fit to judge more favourably of me and my work.

I am, Sir, your obedient servant,  
*Norfolk Hotel, Harrington Road, S.W.* ALDO CASTELLANI.  
June, 1908.

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TO THE EDITOR OF THE "TIMES."

SIR,—As an interested spectator, and to some extent a participator, in the investigation of sleeping sickness in Uganda in 1903, I feel qualified to make a few comments on Dr. Castellani's letter of this morning in justice to Sir David Bruce, against whom this letter is partly directed.

Early in March, 1903, I found a trypanosome in the blood of a patient suffering from fever, but having no symptoms suggestive of sleeping sickness at that time. I showed this to Dr. Castellani, who, having bound me to secrecy, told me he had found a similar parasite in a patient suffering from sleeping sickness in November, 1902, and I have no reason to doubt this statement. He at the same time said that as he had found the parasite in a very few cases only, he still considered his previously discovered streptococcus to be the true cause. My finding trypanosomes in two other cases (published *British Medical Journal*, May 30th, 1903) apparently made him attach a greater importance to the parasite than hitherto, for in the few days before Colonel Bruce arrived he displayed much energy in a further search for trypanosomes in the cerebro-spinal fluid of his patients. Then I learnt to my surprise that he had himself informed Colonel Bruce of his discovery without letting me know, so that as I thought his conduct peculiar, and as I felt then at liberty to do so, I gave Colonel Bruce as much information as I could.

Dr. Castellani had previously expressed his intention of leaving Entebbe before Colonel Bruce's arrival, so that it is difficult to understand his statement that he persuaded Colonel Bruce to allow him to remain on; in fact, I firmly believe that it was Colonel Bruce who persuaded him to stop, and helped him in the investigation of further cases so that he might obtain sufficient data for a full report. Up to the time he left Entebbe he still maintained that his streptococcus theory was the true one, and if it had not been for the timely occurrence

**JRAMC**

## **Sleeping Sickness**

Aldo Castellani

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