

LETTERS TO THE EDITOR.

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The Relation of Big Game to Sleeping Sickness.

THE article by Sir Harry Johnston in NATURE of December 7 on "The Preservation of the African Fauna and its Relation to Tropical Diseases" gives a most admirable and sympathetic review of the subject; but there is one statement to which I must venture to take exception as not conveying, in my opinion, an accurate impression of the known facts of the case, namely, the following sentence:—"But within the last twelve months or so it has been proved conclusively by the biologists at work in Uganda that the large antelopes of that country are the hosts of dangerous trypanosomes, amongst others of the trypanosome which causes sleeping sickness."

So far as I am aware, this statement is based on the experiments reported by Bruce, Hamerton, and Bateman (Proc. Roy. Soc., B, 83, pp. 311-27), in which it was shown "that antelopes can be readily infected with sleeping sickness by the bites of artificially infected tsetse-flies" (p. 317), and that "the flies (*Glossina palpalis*) when infected by the virus of sleeping sickness obtained from the blood of infected antelopes are capable of transmitting the virus to susceptible animals" (p. 319). These results are based entirely on experiments conducted in the laboratory, and the authors state expressly that "positive evidence" is required "to complete the chain of evidence that antelope living in the fly-areas may act as a reservoir of the virus of sleeping sickness. So far it has only been proved that they are 'potential' hosts" (p. 325; the italics are mine).

The only instances known to me in which *Trypanosoma gambiense*, the trypanosome of sleeping sickness, has been identified as occurring in the blood of wild animals, naturally infected, are two in number, and in each case the animal was a monkey, and the locality Uganda; one such case is reported by Koch, Beck and Kleine (*Arbeiten k. Gesundheitsamte*, xxxi., p. 18); the second is reported by Bruce and his collaborators (Sleeping Sickness Reports, xi., p. 102). If there are other known instances of *T. gambiense* occurring naturally in wild animals, I should be glad to be informed of them; if there are not, however, it seems to me premature to state that antelopes have been proved to be the hosts of the trypanosome of sleeping sickness. If laboratory experiments have shown them to be the potential hosts of *T. gambiense*, the same can be said of many other animals which can be inoculated with this trypanosome in the laboratory. The following list of animals susceptible to *T. gambiense* is taken from Laveran and Mesnil, "Trypanosomes and Trypanosomiasis," p. 382 (translated by Nabarro; Baillière, Tindall and Cox, 1907):—monkey (several species), lemur, dog, jackal, cat, rabbit, guinea-pig, rat, mouse, jerboa, hedgehog, marmot, horse, donkey, cow, goat, and sheep. This list is based chiefly on experiments performed in Europe, using European mammals or exotic animals in captivity, and there is no doubt it could be greatly extended by anyone experimenting systematically in the tropics on tropical animals; but as it stands it is sufficiently extensive, and indicates that a great many species of wild animals, small or large, might be incriminated as potential hosts of *T. gambiense* equally with the antelopes, and that the destruction of the "big game" alone would be likely to produce very little amelioration, if any, in the conditions.

The whole history of sleeping sickness in Uganda indicates that the disease has been imported from the west by human agency (compare Laveran and Mesnil, *op. cit.*, pp. 359-66), and that man is the primary host of the trypanosome, at least in Uganda. If, however, the parasite has now been transmitted from man to other susceptible animals by the tsetse-flies, there is no reason to regard the antelopes or other big game as having monopolised the functions of being "reservoir" hosts of the virus. From the point of view of preventing the

infection from spreading from animals to man, domestic animals would seem to be a much greater danger as a reservoir of the virus than antelopes and creatures the natural instincts of which impel them to keep at a distance from the haunts of human beings.

If, therefore, it is desired to extirpate the potential hosts of *T. gambiense* in regions where sleeping sickness is rife, it would not be sufficient to destroy the big game; it would be necessary to convert the whole country into an uninhabited and lifeless desert. In my humble opinion this method of preventing the spread of sleeping sickness is a futile one, and not likely to yield useful results. I believe that there is only one practicable method of interrupting the transmission of the trypanosome, and that is by measures calculated to destroy or keep down the tsetse-flies. At the present time the most urgent need is more knowledge of the bionomics of the species of *Glossina* and of their natural enemies. Some years ago I made the suggestion in NATURE (November 8, 1906) that fowls, wild or domesticated, would be likely to be efficient in keeping down the flies by scratching up their pupæ and eating them, but, so far as I am aware, no experiments have ever been carried out to put this notion to the test. So long as sleeping sickness cannot be made amenable to treatment, attention must be concentrated on prevention, the central problem of which, in my opinion, is the destruction of the insects concerned in the transmission of the disease.

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The Inheritance of Mental Characters.

THE reply of Dr. C. Walker to Dr. Archdall Reid in your issue of last week seems to me somewhat quibbling, and suggests that he is not intimately acquainted with Prof. Pearson's Huxley lecture. The particular part of this lecture quoted by Dr. Reid, and referred to by Dr. Walker, reads actually as follows, the italics being Prof. Pearson's own:—"... We have found the same degree of resemblance between physical and psychical characters. That sameness surely involves something additional. It involves a like heritage from parents. The degree of resemblance between children and parents for the physical characters in man may be applied to the degree of resemblance between children and parents for psychical characters. We inherit our parents' tempers, our parents' conscientiousness" (not *conscientiousness*, as printed in Dr. Reid's quotation), "shyness, and ability, even as we inherit their stature, fore-arm, and span."

Now surely Dr. Walker cannot justly charge Dr. Reid with misinterpreting Prof. Pearson's statement in this instance, where Dr. Reid apparently infers from the words "a like heritage" that Prof. Pearson meant "inherited in the same way"; and that the words "a like heritage" implicitly connote in this context an actual identity of the modes of transmission and reproduction of a parent's "conscientiousness" with those of the transmission and reproduction of a parent's fore-arm. Nor, it seems, would it be unfair to impute, on this ground, to Prof. Pearson the doctrine that external influences brought to bear on the child, such as experience of the world, training, example, &c., could not have any greater effect on his ultimate "conscientiousness" than any external influences could exert on the ultimate length of his fore-arm.

London, December 10.

H. BRYAN DONKIN.

I THINK Dr. Walker is scarcely clear as to the situation. A personal acquaintance with a writer is not necessary when we judge his published opinions. By "character" biologists mean any trait of a living being—a head, a hair, a characteristic of a hair, a characteristic of that characteristic, and so on. Of course, no character of any sort—neither a head nor a scar, for example—can develop in the individual unless the potentiality to develop it under fit conditions is antecedently present. If, then, we think in terms of germinal potentiality, all characters, for example heads and scars, are equally inheritable. But biologists commonly apply the term "acquired" to actual somatic characters which have developed under the influence of use or injury, the term "inborn" to characters which