

## LETTERS TO THE EDITOR.

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## The Problem of Man's Ancestry.

HAVING read Prof. Wood-Jones's booklet, "The Problem of Man's Ancestry," reviewed in NATURE of June 27, p. 322, it seems to me that Prof. Wood-Jones's assertion that man, instead of being the descendant of the apes, may be looked on as their ancestor contrasts with what the author himself writes on the premaxillary bone (p. 36). If man, who is the forerunner, has lost the individuality of the premaxillary element (which is not present in the human embryo, according to the author), how is it that it is found in the apes? It cannot be a new acquisition, because the premaxillary bone is already found in primitive mammals. Therefore apes have this primitive characteristic instead of man.

As to the judgment of the late Hermann Klaatsch that "man and his ancestors were never quadrupeds as the dog or the elephant or the horse," I think it was superfluous already when written by Klaatsch, as no one then accepted such a view; it is, therefore, not worth Prof. Wood-Jones's while to repeat it, especially as this judgment does not at all say that man was not an ape.

Klaatsch only said that the anthropoids were attempts which had failed, and that man was the successful attempt (this, too, is a fairly banal idea); but he never denied the affinity between man and the Simiidae. On the contrary, his last scientific opinion was an exaggeration of such an affinity, the so-called "pan-anthropoid theory," already criticised by Prof. Arthur Keith and myself.

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Napoli, July 14.

I THINK that Prof. Giuffrida-Ruggeri has somewhat misunderstood my meaning, for naturally I have never asserted that the premaxillary element is not present in the human embryo. All I have ever ventured to state is that "it has ceased to exist as a separate entity on the human face," and that this state of affairs is brought about remarkably early in the embryo. This I have alluded to as "a human specific character," a specialisation from that primitive mammalian condition which is still retained in all the rest of the Simiidae, and I see nothing illogical in assuming that the mammal which possesses this specialisation is yet more akin to the primitive mammalian condition than are those animals which, lacking this particular character, exhibit a host of other features which we know to be departures from the primitive mammalian plan. It is upon a summation of characters that we must judge of the animal's zoological position, and my point is that, when such a complete survey is made, the balance of primitive mammalian features is found in the body of man, and not in the body of the monkey. I need scarcely say that I have never "denied the affinity between man and the Simiidae," but I have insisted upon a proper recognition of the differences between the anatomical structure of man and the Simiidae.

Two classes of criticism have been levelled against my very humble pamphlet. The one, typified by the review in NATURE names it and condemns it as

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"a new hypothesis as to man's origin"; the other, on the lines of Prof. Giuffrida-Ruggeri's last paragraph, assumes that it has all been so long generally accepted that it is "not worth while to repeat it." Since these two types of criticism tend to neutralise each other, I have hitherto refrained from discussion; but if Prof. Giuffrida-Ruggeri imagines that no one believed, even when Klaatsch wrote, that man's ancestors were pronogrades, he should read the review in *Man* (No. 71, 1916), written by a well-known comparative anatomist, who "is, and has long been, convinced of the pronograde ancestry of man."

F. WOOD-JONES.

## LICE AND DISEASE.

TYPHUS fever and the relapsing fever of North Africa are now both known to be transmitted from man to man by *Pediculus humanus*, and for this reason have been in past centuries perhaps the two most characteristic epidemic diseases of overcrowding and poverty, and during wars have attacked beleaguered cities in particular. A third disease, known in the British Army as trench fever, has recently been definitely proved to be conveyed by lice. In Germany this same disease is called *Febris volhynica* or *Febris quintana*. The especial association of the disease with life in the trenches was early noticed, and helped to bring lice under suspicion.

The German Army first recognised the disease in Volhynia, a region of South-West Russia; but it is said to have been previously known to Polish doctors. Cases of the disease were not noted in Mesopotamia, Egypt, or the Mediterranean area until the close of 1916. The disease is thought to have been introduced into Greece by chronic cases which arrived at Salonika from France in the winter of 1916-17.

The first published clinical account of the disease was by Major J. H. P. Graham in September, 1915, and since then much has been written on the subject. The first published attempt to investigate the pathology of trench fever was that by McNee, Renshaw, and Brunt. Two varieties of the disease were described by these authors, who showed, by a series of observations on volunteers, that it could be transmitted from man to man by taking blood from a patient during, or immediately after, an attack of the fever, and injecting it into a healthy man. The red-blood corpuscles especially were suspected of harbouring a causative micro-organism, but microscopical examination did not result in the discovery of a parasite. The virus was not conveyed by filtered serum or plasma.

Trench fever is still responsible for a very large share of the sickness in the Expeditionary Forces in France. Our knowledge of this disease has been summed up in a paper read before the Society of Tropical Medicine and Hygiene on May 17 last by Major Byam, who has had exceptional opportunities of studying cases at the New End Military Hospital, Hampstead.

Three chief obstacles to the investigation of the