

LETTERS TO THE EDITOR

From: Dr. G. T. NURSE, P.O. Box 334, Lilongwe, Malawi.

AFRICA—INDONESIA

Dear Sir,

Dr. Jeffreys has replied copiously and with great learning to several points which I was not trying to make. The Arab slave trade was not in question. What was, was that slaves were taken in sufficient quantity from Africa to Indonesia and beyond, to account for a modern population of four million alleged Negroes. Dr. Jeffreys quotes numerous authorities to prove that the Arabs traded in slaves; not one to prove that they ever transported them to Indonesia. In contrast to this, he does establish the presence of Negro slaves in China. One would consequently expect to find a larger Negro population today in China than in Indonesia; if there is one at all, it has been kept remarkably well hidden. That Nicuhoff saw "Negroes" in Batavia in 1660 does not mean that they were not Melanesians.

Coincidence is, in the case of the non-barking dog, no "haphazard, *ad hoc*, unscientific" explanation of its presence in Melanesia. There are more than two breeds of barkless dog; and, when an animal shows a wide distribution, coincidence is a more rationally acceptable explanation for the occurrence of an unremarkable trait in two places than is any far-fetched hypothesis of specifically associated human diffusion.

In this connection it would be as well to consider the difficulties of navigation in the Indian Ocean. It is a good deal easier to transport people and artefacts from the east than to it. The comparatively quick route to the Persian Gulf was quite suitable for a trade in slaves; the long haul to Indonesia with a bulky, dangerous and perishable cargo could hardly have been economically justifiable unless enormous prices were paid for black slaves. As in China, a few Negroes may have been sold to the wealthy and prized as rarities, but it is hardly likely that there would have been enough to account for four million black people in present-day Melanesia. Can Dr. Jeffreys explain how it has come about that most of them are found at some distance from the places where their ancestors would, according to him, have been held in servitude? Any massive escape or manumission would surely be a matter recorded in history. The better attested importation of Negroes into Europe, the prolonged large trade in the Middle East, have neither of them left so considerable an ethnic aftermath. To regard the slave-trade as responsible for the Melanesians is absurd.

Dr. Jeffreys advances a statement made by Howells in 1948 in refutation of one made by the same authority eleven years *later*. This may have been a slip on his part, or it may be his kind of scientific method. This is suggested by his curious use of statistical material. Mean values are of little significance unless some indication is given of the sizes of the samples from which they are derived; but Dr. Jeffreys quotes a large number of apparent means without stating sample sizes or even range, standard deviation or standard error. His choice of measurements is also outdated: the cranial capacity varies so widely in individuals that it is of little value for comparative purposes (Carothers, R.: *The African Mind in Health and Disease*, 1954); the cephalic index is subject to secular change in settled populations (Bielicki, T.: "Some Possibilities for Estimating Interpopulation Relationship on the Basis of Continuous Traits", *Current Anthropology*, iii, pp. 3-8, 1962, and Udjus, L.: *Anthropometrical Changes in Norwegian Men in the Twentieth Century*, 1964); and facial and nasal indices can have different meanings depending on whether measurements are taken to nasion or sellion, which is not stipulated for the figures he gives, and was indeed the subject of controversy for long after de Quatrefages' day. It is inconceivable that anyone can still take seriously the arbitrary mish-mash of characters jumbled together in Taylor's "ethnograph". Five characters, of which at least two are subject to environmental modification, could never constitute an adequate basis for racial comparisons. Dr. Jeffreys is, I suppose, not familiar with the work of Rao (Rao, C.: *The Utilization of Multiple Measurements in Problems of Biological Classification*, *ç.R. Statist. Soc.* x, 159, 1948), but it is surprising that an anthropologist who has worked in Nigeria should apparently not be acquainted with the elegant statistical analyses in Talbot and Mulhall (Talbot, P. A. and Mulhall, H.: *The Physical Anthropology of Soutner Nigeria*, 1962).

It also seems doubtful whether Dr. Jeffreys understands the serological figures he quotes. Without knowing the sizes of the samples it is difficult to say whether the serological differences between the peoples are meaningless or not; certainly any competent statistician can see that if the Zulu people and the Melanesian one are each over 80 there is a significant difference between them at the 5% level and possibly even lower. The figures might have been of more interest if Dr. Jeffreys had quoted values for other populations as well. Hiernaux (Hiernaux, J.: "*Groupes sanguins et anthropologie*", *Revue de l'Institut de Sociologie*, iii, pp. 465-487, 1960) has demonstrated that, taking only the ABO groups into consideration, the serologically closest peoples to the Melanesians are various Europeans. Comparisons based on the ABO groups call for rather more sophisticated treatment than Dr. Jeffreys seems to realize.

To deal briefly with two of his other points: If Dr. Jeffreys was referring to *lobola* in his review, it is a pity that he did not use that term rather than *bride-price*, which has a rather wider meaning. With regard to the Egyptian/Arab solecism—I suggest that Dr. Jeffreys re-read the relevant part of his review and try to interpret it not, perhaps, as he intended it, but as it would appear to an attentive stranger. The

unquestioning respect which Dr. Jeffreys accords to professorial opinion is truly touching; may I remind him that the infallibility claimed for certain *ex cathedra* pronouncements generally refers to a non-professorial Chair?

Note.—This letter has been abbreviated and this correspondence is now closed.—EDITOR.

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From: A. M. JONES, 52, Warwick Road, St. Albans, Herts., England.

How very interesting that you, too, should have patronised Cairns and Morrison. There was another firm near them, Will Day Ltd., who dubbed some of my aluminium masters onto acetates.

Meanwhile since writing that short article I have remembered that I *should* have started it by recalling that, as a result, I think, of my sending my first essay on African music to Professor Kirby in 1930, he sent me up one of his dictaphones and asked if I could do some recordings for him. As this instrument could only record a single voice (singing down its flexible speaking-tube) and as the typical African song has, of course, the cantor and chorus form, I made an enormous horn out of two 6 ft. x 3 ft. sheets of galvanised iron (intended for out-courses in building), riveted together, and slung from a rooftruss in our college classroom. It measured, I suppose, some 6 ft. long and 4 ft. in diameter at the large end. *The small end I attached to the dictaphone flexible tube. I could thus record a chorus of boys, though you will correctly guess that they were too far away from the dictaphone diaphragm to produce anything but a faint sound. Anyhow, that's how I started, and it was a great stride forward when I came on leave and met Professor Tucker as I describe in my article.*

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