

half, mounted it on a little swivel and fixed this to my red-coloured cutting head and let the nut ride on the threads of the metal arm with complete success. I could now make quite fair recordings of a single voice — though I always had a good deal of surface noise through using aluminium discs. But how was I to record typical African songs with cantor and chorus — let alone drumming?

Not willing to accept defeat, I cast around and found the key to the solution. This was a seed-sower made by the well-known English seed firm of Carters — a little conical tin vessel about three inches high and four inches in diameter. I cut off the point of the cone and soldered on a collar which fitted the neck of the cutting-head into which its horn was normally mounted. I then, on the flat top, made a hole in the middle and about six holes in a circle near the edge and soldered similar collars to each. In the centre one I inserted my Fay Tone horn, and in the others I fixed similar sized horns made of cartridge paper (for lightness). This gadget weighed at least some three to four ounces, so, to counterbalance it, I fixed a rod on the back end of the gramophone arm with a sliding weight at the end of it.

At first I still used aluminium discs, but later I graduated to discs with an aluminium base coated with acetate — very superior indeed, but introducing an awful complication, namely how to prevent the swarf from winding round Cairns and Morrison's tracker and bringing the turn-table to a stop when recording. The solution proved simple — I continuously swept it clear with a paint brush.

Imagine now one of my recording sessions. Standing round the gramophone are six African boys each with his mouth as close as possible to one of the cartridge paper horns which he had to follow as they moved inwards during recording. Outside the ring of boys was a stool on which stood my cantor, Robert Kabombo, who had to lean right over the other boys and place his mouth close to the central Fay Tone horn which he, like them, followed as it moved towards the centre of the turn-table. But this is not all: for I had to crouch and squeeze myself inside the ring of boys so that I could not only start and keep an eye on the apparatus, but also work away desperately with my paint brush clearing the swarf. If I missed the swarf for a fraction of a second the record was ruined. Comic? Well, I only wish we had photographs of the process: but it worked.

As a result I sent some of the aluminium master-records to Cairns and Morrison who processed them, making normal pressings. Later, when using acetates I found another firm, Will Day Ltd., also I think in Old Compton Street or near by, who dubbed them and so made a small number of copies available.

I still possess both the aluminium masters and the pressings and dubbings. Very poor quality of course, but still serving as a permanent record of African music of over 30 years ago made on a mission station right out in the African bush.

\* \* \* \* \*

## LETTERS TO THE EDITOR

*From:* COOTJE VAN OVEN, International School, P.O. Box 2651, Dar es Salaam, Tanzania.

I made two more trips in my vacations in Sierra Leone since I wrote to you last — the second one with much better equipment as I had inherited some money in Holland and had thus been able to buy an Akai X-IV. By that time I was gradually getting worried because my full-time school teaching job just never left me time to digest and analyse the material I was collecting. I very much wanted to produce a short illustrated textbook for Sierra Leone schools, with accompanying gramophone record. And as my second contract with the Sierra Leone Grammar School was due to expire in 1967 I began to look out for other work, more in the research line. A research fellowship was advertised in the Institute of African Studies at Sierra Leone's University, but the position was later "frozen" as part of Sierra Leone's economy drive. I received a letter saying that my application would be retained and would be considered again if and when the post would be opened again. Other attempts to find suitable work in Sierra Leone all came to nothing. On completing my contract with the Sierra Leone Grammar School I came to Dar es Salaam to stay with my sister who lives here. From this Tanzanian base I kept searching Africa for work, and finally a solution presented itself in the form of part-time employment. I am now teaching music at the International School in Dar es Salaam, and in my spare time I work on my Sierra Leone material. Although I am a very slow worker I am still hoping that in the course of this school year I may get that school textbook written and the gramophone record made. By that time there may be a research opening in Sierra Leone — or elsewhere, although I should particularly like Sierra Leone because so little has been done about its music so far.

I am still transcribing my field notes into record cards that can be used for study. I am therefore likely to pester you from time to time with classification queries, etc. You may remember that I had the record cards printed after the model of your field cards, and that at the back of each card, under the continued "Remarks", I paste a photograph illustrating the item in question. I have sent a tape containing 55 of the recorded items, together with duplicates of the corresponding 55 record cards, to Professor Merriam of Indiana University, with whom I have had some correspondence, and I hope the material will be of use to him. I had intended to send copies to several other people as well, but I find that copying all the record cards, etc. takes up such a lot of time and I feel that at the moment I should press on with all the unfinished work and with the school textbook. Maybe the material I sent to Professor Merriam can be passed round to other interested people later on, when he has finished with it. The

textbook really seems urgent to me, for if attempts to introduce Sierra Leone's music into Sierra Leone's education are not started now, the music will soon be swamped by outside musical influences. Before I can start writing the book I have to digest the material I collected; before I can digest it I have to make the field notes digestible by channelling them into the proper record cards — and just now I am about 200 record cards behind!

As for a further answer to your 1965 letter, I cannot remember clearly which parts of it I answered in 1966. I do think I mentioned to you that the map of the Westermann and Bryan book did not really solve my language classification problems as most of Sierra Leone's languages were not even mentioned in its list of languages. In some cases the book itself appears to help me where the map does not, but even there I have to be careful as the order of things does not always seem to be the same in map and book. In other cases I have just hidden myself behind question-marks; for instance Temne (spoken quite widely in the northern half of Sierra Leone) has become no more than WB I-?-?.

I also think I told you how pleased I was you had offered me the additional classification 12.58, "rattle with external sounding device, beads on netting, etc." for my shegbureh, but I do not remember answering your points about the kongoma and the koningei. I can see why you call the kongoma a mbira, and I have in fact classified it as such, but I should still like to make two points about it. I always think of a mbira as a melody instrument. A kongoma, being so big and hollow and wooden with rather broad blades (hacksaw blades) instead of slender tongues, produces a drum-like sound when the blades are plucked and is essentially a rhythm instrument. In fact it is not infrequently used instead of a set of drums where these are not available. The second point is that no self-respecting kongoma player will ever play his instrument just by plucking the blades. He will always beat out a different rhythm on the side of the box with his other hand. I have therefore decided to give it the following classification (there definitely is a big sound hole) —  $13.14720 \times 3$  — }  
—12.151— }

In the case of the koningei I have also adopted your suggestion of classifying it as a frame lyre, although seeing musical bows with external resonators here in Tanzania rather strengthened me in my original suspicion that the instrument may have grown out of the bow by a simple change of shape. I first got that suspicion because the Mende people in Sierra Leone appear to use the same name for the simple bow and for this instrument, but it may of course be that koningei is their collective name for string instruments. I can see your point about this one having the shape of a frame lyre and so I have ended up by making it —10.6416 × 7—. I do, of course, realize that in spite of its triangular shape it has nothing to do with what is normally called a triangle — an instrument which is also occasionally played in Sierra Leone.

Two more classification problems, both occurring in the same song. It is a Mohammedan song on the subject of Ramadan but it does not appear to come under either 23.41, 23.42 or 23.43. It is a lively African song with a spirited drum accompaniment. I have therefore ventured to call it 23.44. Would this be alright or have you given that number to another type of song already? The two drums are rectangular frame drums, and I should be tempted to call them 14.93 (particularly as the smaller one seems just on the verge between deep and shallow, 14.91 and 14.92!) but they look as if they are specially made, not just improvised from the sides of a box. What to do?

From: Dr. G. T. NURSE, Fort Mlangeni, Malawi.

Dear Sir,

Without wishing to involve myself in a controversy in which I cannot claim to speak with any authority, I nevertheless feel that it is necessary that someone should draw attention to the rather tenuous basis of some of the arguments used by Dr. M. D. W. Jeffreys in his review of A. M. Jones' *Africa and Indonesia: the Evidence of the Xylophone and Other Musical and Cultural Factors*, in *African Music*, IV, 1, 1966/67.

Dr. Jeffreys accuses Dr. Jones of overlooking "two important statements on the origin of the xylophone." One of these turns out to be the claim of Ellis in 1883 that the xylophone was invented by the Jolloffs (Wolof); the other, a statement by Sir Harry Johnston which implies that the Negro, being "totally savage", must have received all his more highly developed musical instruments from Egypt. It is difficult to understand in what way these assertions are important; the former appears to be simply an opinion in isolation, while the second is so redolent of the nineteenth-century *ex Africa numquam aliquid novi* school of thought that it seems incredible that any present-day Africanist could take it seriously. A little further on, another statement by Johnston, that "Bantu phonology may be matched . . . (to a remarkable degree) among the Papuan and Melanesian languages" is advanced as evidence of "linguistic similarities between Negro African languages and those of Melanesia." Overall phonological similarities are scarcely admissible as the basis of linguistic comparisons; the wide-ranging phonological differences between, for instance, Danish, Norwegian and Swedish, which are from a comparative viewpoint virtually dialects of a single language, should be enough to point the truth of this; and the similarity in range of phonemes between Nyanja and Italian, languages which I am sure Dr. Jeffreys would allow to be unrelated, gives an unexpected conversational advantage over all other expatriates to the

Italians in Malawi. The romantic speculations of writers of the latter part of the last century and the early part of this are no sure grounds for the establishment of a connection between the Bantu and the Oceanic languages; if Dr. Jeffreys can adduce any more recent and scientifically admissible evidence I, and I am sure many others as well, would be most interested to hear it.

It would also be interesting to learn the source of Dr. Jeffreys' figure of "over four million" for the population of "African Negroes" in Oceania. Surely he is not assuming that it is an established fact that Papuans and Melanesians share a common ancestry with the Negroes of Africa? Indeed, he assumes even more than that: he states quite baldly that "the derivation of the Oceanic Negroes from Negro Africa is attested by a long list of distinctive, similar culture traits." Yet when these traits are enumerated they turn out to be very far from conclusive. Ancestor-worship may have a very specific distribution among primitive peoples today, but any student of the classics will be familiar with the extent to which it persisted in the hero-cults of Greece and Rome, and until quite recently it formed a noteworthy part of the Chinese social pattern. Weapons with falcate edges are by no means unknown outside of Africa and Melanesia: one of the oldest myths of Greece tells of the mutilation of Chronos with a sickle. Two strains of barkless dogs can be as much coincidental as phonological resemblances in speech. For what it is worth, an aged Zulu man told me many years ago that the *umncedo* penis-sheath was only adopted by the Zulu when they abandoned circumcision; I have the impression that it has, in fact, never been worn by those tribes which have retained circumcision. Other mutilations, such as those mentioned, seem to proceed from the same original *rationale* as circumcision; the deformation for ritual, probably quasi-sacrificial, reasons, of parts of the body which are not functionally essential. Liana suspension bridges and woven sleeping-bags are more likely to be the products of independent ingenuity and common-sense than of cultural connections; one is inclined to remark in exasperated exaggeration that some anthropologists seem disposed to believe that no-one ever invented anything. And when it comes to such widespread phenomena as the payment of bride-price (has Dr. Jeffreys never read Hardy's *Mayor of Casterbridge*?) or the polishing of stone implements, the devising of face-masks and secret societies (I could give references to Greece again here, but assume that they are familiar to him) and bark-cloth and bows, the parallels become plainly irrelevant.

Much of Dr. Jeffreys' case rests on the supposition that there was an extensive transport of slaves from the East Coast of Africa to Indonesia. He charges Jones with ignoring the "historical evidence" for this, but does not himself produce any direct substantiation of it, being apparently content to depend on secondary sources which, to judge from his quotations from them, refer to the extent of Arab trade without specifically mentioning this particular commodity. "This trade in Negro slaves", he writes, "readily accounts for four million odd Negroes settled throughout Indonesia and beyond and hence for the cultural similarities in these two regions." In refutation of this, and without any first-hand knowledge either of Indonesia or "beyond", I can quote William Howells (*Mankind in the Making*, 1959) to the effect that "there are no traces whatever of Negroes (in contrast to the Negritos) in Indonesia." Howells also points out that the present geographical situation of the Papuans and Melanesians indicates that they arrived in the areas they now inhabit before the Indonesians did, and that their superficial physical resemblance to Negroes is belied by their bone structure, which much more closely approximates to that of Polynesians. Besides, if the Indonesians wanted slaves, what need was there for them to be imported from Africa? There is an abundance of over-populated countries with impoverished and defenceless inhabitants much closer to Indonesia than the coast of Africa, to say nothing of the archipelago itself; in fact, it is possible to claim that the trend was the reverse of that which Dr. Jeffreys suggests, since there are known to have been Indonesian slaves at the Cape in the days of the Dutch East India Company.

Dr. Jones may steer clear of the Arabs; but at least he does not commit the solecism of equating them with the Egyptians. Furthermore, it is difficult to follow the reasoning behind Dr. Jeffreys' reference to the Manganya (presumably Mang'anja is intended) word *ngalawa*, a canoe, and its Makua cognate *ikalawa*, and their probable Arabic derivation. The word is also found in Yao; the Yao, a Muslim people, adjoin both the Mang'anja and the Makua; the provenance of the word is obvious. Yet it is rather extraordinary that a single word should be seriously advanced as evidence of cultural contact; by employing the same method, and one of the same languages, it is possible to claim, rather absurdly, that there must have been at some time Danish influence in Central Africa, since the Mang'anja verb *-tola* is identical in meaning and pronunciation with the Danish *taale*. That there are no Indonesian words in Swahili is scarcely surprising, even if one accepts Jones' hypothesis; Swahili only developed as a language in consequence of the spread of Arab trade along the coast; and this happened at a date rather later than that suggested by Jones for Indonesian penetration. It might be more rewarding to examine the other two large East Coast clusters, Yao and Makua. I wonder whether Dr. Jeffreys has ever noticed how common the Mongoloid epicanthic fold is among the Yao and the tribes with which they have inter-married?

The limitations of my knowledge deter me from taking sides in this controversy, and I am in any case averse from seeking diffusionist explanations at all costs for ostensibly autochthonous phenomena. I feel, however, that when an hypothesis, mistaken though it may be, is subjected to an erroneous refutation, it is only fair that the shortcomings of the refutation should themselves be pointed out.

From: M. D. W. JEFFREYS, Witwatersrand University, Johannesburg, South Africa.

Dear Sir,

Herewith some comments on the main points in Dr. Nurse's letter. Drs. Jones and Jeffreys are in agreement that the presence of the xylophone in Indonesia and in Africa is an example of diffusion. Dr. Jeffreys admits that he does not know where the xylophone originated. Dr. Jones claims that it originated outside of Africa and that it diffused into Africa. Dr. Jones gives his data to support this east to west diffusion of the xylophone. Dr. Jeffreys scrutinised this data and found it unconvincing.

Dr. Jeffreys then marshalled data of which the weight of evidence was against the east-west diffusion theory and in favour of a west-east theory.

Dr. Nurse is opposed to diffusion and wrote: "I am, in any case, averse from seeking diffusionist explanations at all costs for ostensibly autochthonous phenomena."

If the xylophone is ostensibly an autochthonous phenomena in Indonesia it would also, *ipso facto*, be an autochthonous phenomena in Africa and the *raison d'être* of Dr. Jones' book falls away. One recalls a remark made by La Fontaine: "Rien n'est si dangereux qu'un ami ignorant; mieux vaudrait un sage ennemi." (Fables, Bk. VIII, fab. 10).

In the review mention was made that in 1695 de Gennes described a xylophone in Senegambia. Two hundred years later Ellis in the same region describes this instrument and ascribes its invention to the Wollofs, an autochthonous people. Dr. Nurse objects to Ellis' claim but raises no objection to Jones claiming an autochthonous Indonesian invention.

On the matter of the diffusion of culture traits, the late Professor Kroeber who was one of America's leading anthropologists, wrote: "The total part played by diffusion in human culture is almost incredibly great. . . . All cultures are largely hybrid composites of material that once entered them from outside. Naively, human beings do not realize this to any great extent. It is of no advantage to a society, ordinarily, to have its members aware of the foreign source of most of their culture; nor is it a satisfaction to most individuals to be reminded of their debt to aliens. So the facts tend to be forgotten, perhaps more rapidly even than most events in history. In addition, there is a precritical impulse in us to look upon foreign customs as things that flow spontaneously out of foreign peoples: Chinese *naturally* worship their ancestors and defer to their fathers, just as they *naturally* talk in sing-song and *naturally* grow long black hair. This goes with the normal naive assumption on *race* differences and inferiorities. So totems and couvade and bows and arrows and shell money and sympathetic magic were long construed as more or less *automatic* emanations of the *natural* human mind in its mere *childlike* manifestations.

"This older view, or rather assumption, has had to be largely abandoned under analysis. If our own civilization is due mainly to elements introduced into it from without, there is no reason to believe that backward peoples would be more spontaneously creative, especially since they obviously also incline to repeat one another in certain grooves. Above all, more accurate study of the forms of culture traits, of their related variants, and of their territorial distributions has yielded so many instances of proved or highly probable diffusion of things like totems and couvade and shell money that the explanation of spontaneous emanation is rarely invoked any longer." (*Anthropology*. 1948. pp. 412, 413). For further information on diffusion by Anthropologists Goldenweir, A. *Anthropology* (1937, Caps. XXVIII and XIX) and Lowie, R. *An Introduction to Cultural Anthropology* (1946, Cap. XX), can be recommended. Dr. Nurse also wrote: "One is inclined to remark in exasperated exaggeration that some anthropologists seem disposed to believe that no-one ever invented anything." In reply and in confirmation of Kroeber, Dr. Jeffreys quotes from a letter of his that appeared in *The Listener* (5/7/1956). "I refer to the line of mountain ranges starting with the Cantabrian Mountains abutting on the Atlantic and ending with the Carpathians on the Black Sea. North of this line no indigenous European ever invented an alphabet and hence writing, the criteria of civilization, or domesticated dog, sheep, goat, horse, ass, cattle, camel, fowl or wheat, barley, oats, rye, flax, cotton, silk, melon, grape, peach, pear, bean, pea, onion, lettuce, cabbage, or, to use an American verb, "originated" agriculture, architecture, plough, boat, ship, sail, wheel, pulley, screw, compass, masonry, arch, navigation, astronomy, calendar, money, law, kingship, religion, metallurgy, medicine, anatomy, cartography, glass, pottery, weaving, carpentry, arithmetic, mathematics, chemistry, the atomic theory, weights, measures, land surveying, irrigation, paper, explosives." Professor Goodwin so approved of this letter in *The Listener* that he quoted from it *in extenso* in *The South African Archaeological Bulletin* (Sept. 1956, p. 80). Up to 1000 A.D. would Dr. Nurse mention what fundamental inventions or discoveries were made by Europeans north of that line of mountains?

To come nearer home, Professor Dart, in his S.A.B.C. talk of 4th May, 1954, said: "As far as we know neither the Bushmen nor the Hottentots nor even the Bantu discovered of their own accord a single element of their divergent cultures. The Bushmen's bows and arrows, paintings and perforated stones, fire-making and bead-decoration, like the Hottentots flocks and herds, dogs, goats, sheep and cattle, pottery and weaving, mythology and magic, and like the Bantu fishing crafts and dug-out canoes, cattle and poultry, basketry and pottery, assegais and hoes, kraals and crops, huts and furniture, are one and all cultural gifts that they have received from more advanced peoples. The South African races never discovered spontaneously how to make a basket, to fashion a claypot, to build a hut, to shape a canoe, to forge an iron weapon, to domesticate an animal or to cultivate a single vegetable or fruit . . .

All these techniques and crafts . . . were gifts that came to South Africa from afar off."

So long as Dr. Jeffreys' views find approval at professorial level, it is of little moment that one, who writes "I cannot claim to speak with any authority", differs.

Dr. Nurse asks: "It would be interesting to learn the source of Dr. Jeffreys' figure of *over four million* for the population of *African Negroes* in Oceania. Surely he is not assuming that it is an established fact that Papuans and Melanesians share a common ancestry with the Negroes of Africa?"

Dr. Jeffreys makes no assumption. He finds himself in agreement with Professors de Quatrefages, Keane, Sir Grafton Elliot-Smith, Sir Arthur Keith and Raymond Dart, at present a world authority on anatomical matters.

De Quatrefages published *Crania Ethnica* in 1882. Professor Keane in 1896 in his *Ethnology* (pp. 263, 264) quoted, de Quatrefages on the Oceanic Negroes as follows: "Passing from the Negrito to the Negro proper, the most important point is the now established physical identity of the African and Oceanic branches. The evidence bearing on this question has been summed up in a masterly manner by de Quatrefages, from whose comparative craniological tables are taken the subjoined broad results.

PEOPLES	Cranial capacity	Cephalic Index	Facial Index	Nasal Index
	c.c.			
S.W. Sudanese .. .. .	1300	69.78	71.09	54.00
S.E. Sudanese .. .. .	1355	71.66	71.09	54.16
Mandingans .. .. .	1460	72.82	68.18	54.00
Serrers .. .. .	1490	69.79	72.51	54.54
Krumen .. .. .	1445	72.28	69.16	51.92
N.W. New Guinea .. .. .	1305	71.11	71.42	55.10
S.E. New Guinea .. .. .	1385	71.89	69.92	53.56
New Hebrides .. .. .	1485	68.42	69.69	54.16
Loyalty Is. .. .. .	1460	69.84	68.38	51.92
New Caledonia .. .. .	1445	69.66	67.40	52.47
Africans (mean) .. .. .	1424.2	71.23	70.04	54.49
Papuans (mean) .. .. .	1412.5	70.38	68.87	53.03

Such then was the view of scientific opinion in 1882. As will be seen by ensuing extracts this view has since then been amply confirmed.

Elliot Smith, *Human History* (1929, p. 140) wrote: "The vast bulk of the Negro race is to be found in Africa. The other great mass of the Negro race occupies New Guinea, and spreads right out through Melanesia, the Bismarck Archipelago, the Solomon Islands, New Caledonia and Fiji, and even to Easter Island, far out in eastern Polynesia and the American coast. These Melanesian Negroes present the general characteristics of the African Negro, but in a form suggesting more or less admixture with other races. We thus have the race divided into two great groups — African and Oceanic . . . The question naturally arises as to the geographical situation of the original home of the Negro." Africa has been shown to be the Negro's ancestral home.

Keith in his introduction to B. Thomas's book, *Arabia Felix* (1937) wrote: "At each extremity of the Belt in Africa as in Melanesia, we find peoples with black skins, woolly hair, more or less beardless, prognathous and long headed. We cannot suppose these Negroes to have been evolved independently of each other."

Dart in *The Bantu Speaking Tribes of South Africa* (1937, pp. 8, 13) wrote: "At any rate, the cradle of the Negroes, as nearly as we can determine, was in the basin of the Congo watershed . . . The Oriental commerce was responsible for spreading the Negro Race through the East Indies and into Melanesia in the same way as the slave-trade also dispersed the Negro Race through the West Indies and America." In Dart's South African Broadcast on *The Oriental Horizons of Africa* delivered on June 1st, 1954, he stated: "The 400 years of the European slave-trade in the Atlantic removed from Africa at least 12,000,000 black Africans . . . The Moslem Arabian slave trade persisted during that 400 years and the previous 800 years i.e. for three times as long and the Arabs were just as expert slavers as the Europeans . . . from the Indonesian island of Flores to Fiji there is distributed a fundamentally Negro population . . . These Papuans and Melanesians owe their origin to the transport of Negroes eastwards by sea . . ." Dr. Nurse refers "to the rather tenuous bias of some of the arguments used by Dr. Jeffreys." Dr. Jeffreys favours evidence rather than arguments. In support of the claim that African Negroes are the basis of the Melanesians the evidence from blood groups is important.

Professor Dart in "African Serological Patterns and Human Migrations" *South African Archaeological Bulletin* (No. XVIII, 1951) showed, in his chart comparing African and other serological patterns, that wherever there is a Melanesian blood group pattern there is alongside it a corresponding Negro blood group pattern in Africa and he wrote: "Some of the most outstanding features of the chart and diagram are: (1) The relative consistency of serological pattern found in the Bantu as is shown by the aggregation of the Negro (Western, Central and Southern African) groups in a comparative solid block comprising 15 of the 68 occupied squares. (2) The clear separation of the black-skinned Australian aboriginal

groups from the black-skinned Negro block; while the black-skinned Melanesians (and various of the other Indonesian — Oceanian) groups share by contrast the blood patterns exhibited by the Negro block . . .

"The inferences logically drawn from these facts are firstly that sea-traffic and insular isolation have played a preponderant part in the great divergence of serological patterns; and secondly, that where Melanesian (or other insular) and African (or other continental) patterns persistently correspond there exists a strong probability that they are due to direct intercommunication between one another. Such direct intercommunication could naturally have occurred only by sea traffic, of whose existence we have in some instances strong evidence but in other instances can only be inferred by cultural similarities and by genetic correspondence such as these blood groups and other physically inherited features provide . . . As six of the Melanesian groups are in the (6-10%) B column and eight are in the (11-15%) B column and which there is only one in the (16-20%) B column and none in the higher B columns, one can safely deduce that the *Melanization* of the East Indies and the Indian and Pacific Oceans was caused by the conveyance of typical African Negroes into that region during the recent proletarianization of the Indian Ocean during the Christian era . . ."

This serological evidence marshalled by Professor Dart is corroborated by that supplied by Professor Hooton, *Up from the Ape* (1947, pp. 549) and by Professor Ruggles-Gortes, *Human Ancestry* (Cambridge Mass., 1948, p. 338). Their blood grouping tabulations are respectively:

PEOPLE	O.	A.	B.	AB.	P.	q.	vh.
Zulu .. .. .	53·2	25·3	19·2	2·3	·157	·122	·730
Senegalese .. .. .	47·4	25·0	24·1	3·5	·163	·157	·689
New Guinea .. .. .	53·7	26·8	16·3	3·2	·164	·104	·733
Melanesian .. .. .	37·6	44·4	13·2	4·8	·239	·099	·613

  

PEOPLE	O.	A.	B.	AB.
Melanesians, New Guinea .. .. .	53·7	26·8	16·3	3·2
Melanesians, Schouten Islands .. .. .	63·2	16·9	17·6	2·2
Moluccas .. .. .	51·0	27·0	18·7	3·5
Micronesia, Suipan .. .. .	50·0	33·8	14·0	1·7
Micronesia, Yap. W. Carolines .. .. .	57·7	20·3	17·8	4·2

The similarity in the blood grouping of the Senegalese Negro and of the Zulu Negro is explicable on the grounds that they belong to one race — African Negroes. The similarity in the blood grouping of the Zulu Negro and of the New Guinea Negro is likewise explicable. The difficulty is to account for the present geographical distribution of these two similar Negro groups. The Zulu group is part of a southward migration of African Negroes whose left flank was constantly raided by Arabs for slaves which were then transported eastwards across the Indian Ocean. Such transportation readily accounts for the presence of Negroes with the similar blood group patterns in Africa and in New Guinea.

In support of the above conclusion Snyder's third law of serology, enunciated in *Blood Grouping in Relation to Clinical and Legal Medicine* (1929, p. 153) is applicable: "If any people show blood-group frequencies similar to a group of people not related to it, the conclusion may be drawn that the former traces back to the latter somewhere in its ancestry, or else the former has undergone crossing with the latter group or some similar people."

The similarity between the blood groupings of the New Guinea Negro and those of the African provides a clear example of Snyder's third law of serology and as the cradle of the Negro is in Africa the New Guinea negroes derive from African Negroes. As a summary of the evidence for the diffusion of the Negro eastwards from Africa Taylor's (1949, p. 66) statistical analysis presented as an ethnograph to correlate physical features with race is noteworthy. He wrote: "The ethnograph is a six-sided figure, where the east-west axis corresponds to the value of the *cephalic index*. From centre to top is the measure of *hair type*. From centre to bottom is measure of *stature*. From centre to north-west is the measure of the *nasal index*. From centre to north-east indicates *face-breadth* . . . These ethnographs illustrate difficulties which most anthropologists seem to have ignored. How can one explain the close resemblance (in five specific criteria) between such far-distant types as are here graphed? . . . Sudanese and Northern Melanesian . . . only the spreading of racial zones from a common *cradle-land* . . . can possibly explain these biological affinities."

Furthermore, Howells, W. in *Mankind So Far* (1948, p. 246) writing of Melanesia said: "The one racial type which seems proper to the area is Negro . . . of a sort which seems indistinguishable from the African . . . The Negro proper seems to be more characteristic of the main coastal routes, being found on the shores of New Guinea (especially the north coast) and of the islands to the east." As the above is Howells' view the quotation from Howells by Dr. Nurse appears inept unless it is intended to draw attention to the fact that it is equally true to say that there are neither Indonesians nor traces of Indonesians in e.g. west Africa.

Dr. Jeffreys claims that anatomically, serologically and physiologically, sufficient evidence has been

marshalled to show that the Melanesians are transported African Negroes among whom the xylophone is found.

Dr. Nurse asks for evidence of a slave trade in Negro slaves. The literature on it is voluminous. Here-with a few items.

Newman in his book *Matabeleland* (1895, p. 35), describing the early occupation of Rhodesia, wrote: "There are at present neither missionaries nor hunters in Gazaland. A few Arabs sometimes come up the river . . . to barter for slaves, and do a good business in that line with the knowledge and consent of the Portuguese authorities, through whose territory they pass and repass freely." Craster in his book, *Pemba: The Spice Island of Zanzibar* (1913, p. 127) stated that about 1840: "It was estimated that the Seyyid of Muscat raised a revenue of £20,000 a year from the sale of slaves in Zanzibar, 40,000 to 45,000 being sold annually. Laurie, L. and Wittle, J. in their book, *The Oriental Navigator* (1794, p. 90) describing Cape Delgado on the east African coast wrote: "The Moors are the proprietors of the country, and carry on a great trade, as well in blacks, which they send to Kilwa, as in ivory, which they sell to the Arabs."

Hunt, D. R. in his article "An Account of the Bapedi Bantu", *Bantu Studies* (1931, p. 277) reported that Arabs had raided the Bapedi circa 1650. Knox, J. in his *A New Collection of Voyages, Discoveries and Travels* (Vol. II, 1767, p. 469) reports John Nieuhoff's experiences in Batavia in 1660: "From four in the morning till late at night . . . the crowds are so very great through the fruit market which is stocked with all sorts of herb, fruits, etc. by Chinese and Negroes . . ." Hirth, F. and Rockhill, W. W. in their translation of *Chu-fan-chi* a work on the Chinese and Arab trade in the twelfth and thirteenth centuries (1911, p. 149) wrote: "In the West there is an island in the sea on which there are many savages, with bodies as black as lacquer and with frizzed hair. They are enticed by (offers of) food and then caught and carried off for slaves to the Ta-shi (Arab) countries . . . In Chau K'u-fei's work, this island is not located, and after the words 'carried off' occurs the phrase 'thousands and tens of thousands of them are sold as foreign slaves.' In Sachau's, E.L. translation of Alberuni's *India* (Alberuni was in India and wrote circa 1030 A.D.) (1910, Vol. I, p. 197) one reads Alberuni's account of Africa: "On the plains of this continent live the Western Negroes, whence the slaves are brought . . ." Kenyon, K. M. in his appendix V to Miss Caton-Thompson's *Zimbabwe Culture* (1931, p. 262) wrote that the Persians "traded direct to Ceylon and China at least as early as A.D. 651. At the time of this revolution in Canton in 876, there were said to be 120,000 Mohammedans, Jews, Christians and Porsees in the city, many of whom were killed. The Persian trade, no longer supported by a strong empire at home, did not survive this blow, and their place in the Chinese trade was taken by Arabs." Smith, E. W. in *Events in African History* (1942, p. 2) wrote: circa 80 A.D. "The Periplus of the Red Sea tells of . . . Arab enterprise in East Africa as far south as Dar-es-Salaam, and of an active trade in slaves." Duyvendak, J. J. L. in *China's Discovery of Africa* (1949, p. 24) wrote about slaves in China as follows: "We also find *Seng-chih-nu*, slaves from *Sengchih* . . . which undoubtedly is the same as *T's'engch'i*, as a transcription of *Zanggi*, the general Arabic word for Negroes. In the name of the country *K'un-lun T's'eng-chi*, we may therefore recognize the country of the blacks, Zanzibar, pre-fixed by the appellation *K'un-lun*." Then follows an account of Negro slaves kept by the wealthy Chinese.

Arab slave trade in Negro slaves from Africa eastwards to Canton has been shown to exist for at least two thousand years.

The statement by the aged Zulu that the penis-sheath was adopted only after circumcision was abandoned is not confirmed by Nathaniel Isaacs who lived among the Zulu and in 1831 fully described the male dress. The penis-sheath is not mentioned. However, this aged Zulu's remark has no relevancy to the wearing of the penis-sheath elsewhere in Negro Africa or among the Negroes of New Guinea.

The following information shows its antiquity and persistence amongst e.g. the Bantu. In *A Complete System of Geography* (Vol. II, p. 500) published in 1747 is a description of Monomotapa. "The inhabitants go naked from the Waist upwards; but about the Waists they wear a Piece of coloured Cloth, and they put their Privities into a Gourd . . ." S. Kay, *Travels in Caffraria* (1833, p. 111) wrote: "But leaving out of the question a small and indecent covering that hides the part whence the foreskin was cut in circumcision, a state of nudity is that in which the men are most frequently seen." Maclean, C. B. *A Compendium of Kafir Laws and Customs* (1858, p. 126) referring to the circumcised Gaikas, wrote: "If a man wilfully exposes himself without the penis cover, he may be fined by the chief." Meek, C. K. *Northern Nigeria* (Vol. I, 1925, p. 41) reported that "the penis-sheath is worn among the following tribes; Berom, Burum, Ganawuri, Jarawa, Katab, Mada, Marna, Ninzam, Nungu, Pakara, Rukuba, Teria, Zeranda and Zul. Some of these tribes do not practise circumcision (e.g. Ganawuri), but others do, so that we cannot connect the use of the penis-sheath with the non-circumcising peoples."

Dr. Nurse's ensuing remarks attributing "other mutilations" to some "origin rationale" is an expression of his private opinion and may be neglected.

In the matter of the non-barking dog, Dr. Nurse chooses a haphazard, *ad hoc*, unscientific explanation to account for its presence in Indonesia, namely, coincidence. Dr. Jeffreys, on the other hand, prefers to be guided by the rules of diffusion as laid down by Professor Lowie *An Introduction to Cultural Anthropology* (1946, p. 362): ". . . diffusion is convincingly demonstrated when: (a) the resemblances are numerous; (b) the similarities compared are highly distinctive or unusual; (c) the course of diffusion is clear . . ."

So far as (a) is concerned this matter has been demonstrated by the numerous *ad hoc* objections raised by Dr. Nurse.

So far as (c) is concerned the slave trade suffices as an explanation. With regard to (b) Dr. Jeffreys sees the non-barking dog as part of the complex, New Guinea Negroes with African blood grouping, gourd penis-sheaths, possessing the African Negro non-barking or Basenji dog. To this complex may be added numerous other cultural traits common to both groups, such as the cult of the skull. This cult appears in West Africa\* and is reported by Brown in *Melanesians and Polynesians* (1910, p. 396) where chiefs are interred with the head above the ground for the purpose of taking the skull and placing it in some sacred grove." Dr. Nurse asks whether Dr. Jeffreys has noticed how common the epicanthic fold is among the Yao, etc. Dr. Jeffreys in 1917 was among the Yao but was too busy fighting to notice the epicanthic fold.

Professor Carleton S. Coon in *The Living Races of Man* (1966, pp. 66, 117) states: "Arabs and Berbers are themselves Caucasoid . . . The Shluh Berbers of the Atlas mountains have a 12 per cent incidence of epicanthic fold." What, therefore, is the significance of the epicanthic fold among the Yao?

Over the matter of "bride price" Dr. Nurse refers to an irrelevant illustration, namely the sale of a wife, recorded in a piece of fiction. It seems that Dr. Nurse regards "bride price" as the purchase and sale of a woman: it never is. Dr. Jeffreys recommends Dr. Nurse to read "Lobolo is child-price" *African Studies* (Vol. X, December, 1951).

Finally, for it would take too long to refute all the issues raised, Dr. Nurse writes: "Dr. Jones may steer clear of the Arabs: but at least he does not commit the solecism of equating them with the Egyptians." Dr. Jeffreys agrees. The solecism is in the mind of Dr. Nurse.

\* Jeffreys, M. D. W. The Wiya Tribe. *African Studies* Vol 21. 1962.