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AFRICAN MUSIC

JOURNAL OF THE INTERNATIONAL LIBRARY OF AFRICAN MUSIC

VOLUME 6 | 1982 | NUMBER 2

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The hypothesis that sea-going peoples from the general area of present-day Indonesia contributed to the formation of various aspects of African culture, and in particular to aspects of material culture, continues to be a controversial topic in African studies, and the prominent contribution by ethnomusicologists to the argument makes it of special interest to students of African music. Since it was first formulated by Frobenius at the end of the last century (Frobenius, 1898: a convenient English summary in Frobenius, 1900), new evidence has been adduced by von Hornbostel, (1964, 1971 and 1972). It is not my intention to deal with all the literature on this subject, but rather to focus on musicological and related topics and to scrutinise the validity of the style of argument employed by these authors.

Frobenius's original case (1898 and 1900) is difficult to assess today as it ranges over broader territory than the work of later writers. Frobenius assigned Malayo-nigritic origins to the whole of the 'ancient stratum' of African culture. He based this statement on parallels between the material culture of the Oceanic and the South-East Asian areas and that of Africa, emphasising hunting technologies and musical instruments. He lists as 'Malayonigritic' the following African string instruments; 'Bambuslaute' (the Malagasy idiochord tube-zither, *valiha*), 'West *kinanda*' (the narrow, gourd-resonated trough-zither, typical of West-Central Tanzania), and the '*sambi*' (a pluriarc like those used in the area around the mouth of the Zaire river). He then states that the '*tangola*' (a mouth-resonated stick-modulated musical bow), the 'Mandingo-laute' (the West African *kora*, a notched bridge-harp) and the 'Kru-laute' (the chest-resonated, forked harp of West Africa) are the descendants of the 'Bambuslaute' (the tube-zither). While it is true that tube-zithers are found in the Indonesian islands and generally accepted that they were brought thence to Madagascar, the other two instruments in his first list show no significant parallels with instruments in any other part of the world. It may be that Frobenius's reasons for regarding the Malagasy tube-zither as an import were somewhat confused, since his text actually compares it to a rather different heterochord tube-zither from the Nicobar Islands. There is, moreover, no reason to connect the instruments in his second list with the tube-zither, restricted, in any case to Madagascar.

The major African percussion instruments; slit-gongs, hourglass drums, goblet drums and xylophones are similarly placed in the Malayonigritic stratum. The 'butterfly' slits on cylindrical slit-gongs found among the Luba of Central Zaire (found, incidentally, over a wide area of West/Central Africa) and also on the slit-gongs of 'Neupommern' (evidently an island in Melanesia) are used in evidence, without consideration of the functional requirements that could lead to such morphological parallels. Frobenius also compares the leg-xylophones of New Caledonia

with those found on the African mainland. Further parallels between the artefacts of Oceania and Africa, in particular the carved canoe prows found both in Cameroun and among the Dayak, the animal masks in both areas, and also the carved human figures, are presented without comment (Frobenius, 1898; pp. 325-336).

As one writer has observed in another context, this is to use the conjunction 'therefore' in the sense of 'and'. Taking into account the period in which Frobenius was writing, his casual style of argument is understandable, if not admissible. I have taken some space to expound it because a number of its themes were taken up by later authors, somewhat uncritically; and it may be that the lustre of his name caused later authors to consider that the burden of proof had been lifted from their shoulders.

There are a number of reasons for exercising especial care when putting forward hypotheses of this nature, with implicit historical content. They imply a type of diffusionist thinking which has not been recognised as part of the legitimate subject matter of anthropology for many years. The reasons for this probably relate as much to the history of anthropology as an academic discipline as to the validity of the arguments themselves. Modern social anthroplogy began in the twenties as a reaction to the extreme speculative thought of Elliot Smith (1929) and the German *Kulturkreislehre* school, which had argued for historical connections between cultures on the basis of comparisons between isolated elements within them. Some of the ideas put forward at this time now strike us as bizarre or humorous – for example, that the South American Indians had their origins in one of the lost tribes of Israel, or that the West African pastoral nomads, the Fulani were Malayan in origin. It is important to remember that it was not diffusionism that was disproved as a method. Its more careless practitioners were reprimanded and at the same time social anthropology announced a shift of focus that allowed it to ignore the historical aspects of the cultures it studied.

Recent advances in historical technique, such as ethnoarchaeology and palaeobotany have enabled us to discuss the history of pre-literate peoples with more confidence. These permit us to assess more accurately the evidence drawn from material culture in support of migration hypotheses. The case of Madagascar shows us how data from musical instruments anticipated the conclusions of lexicostatistical and haematological studies. In an important study of Malagasy instruments Sachs (1938) drew up a list of the affinities of each instrument with parallel morphological types in other parts of the world. He then postulated successive migrations to Madagascar, from the Indonesian islands, from the East African coast, and from Arabia, to account for the presence of the various instruments. The Austronesian migration was confirmed nearly thirty years later on the basis of lexicostatistical comparison of the dialects of central Madagascar and the languages of Borneo, in particular, Maanyan (Dyen, 1965). Even Sachs's dating of the original migration as previous to 500 A.D. (based on the absence of gongs and tuned metallophones in Madagascar) turns out to have been remarkably consonant with the lexicostatistical evidence. Similarly, the evidence from morphological characteristics of the population later confirmed Sachs' suppositions as to the African origin of the 'black' element in the population (Chamla, 1958).

This is interesting, but it raises a general problem of method: what standards are we to use when comparing artefacts from different culture areas? How close must

the connections that we observe be in order to postulate migrations not attested in historical records? Sachs' arguments in the case of Madagascar rest almost on formal similarities of construction between instruments; but we have no general theory to tell us what the frequency of similarity should be before a historical connection can be assumed – nor indeed can we be certain what counts as a similarity. In the years intervening since the publication of Sachs' book, it has become clear that a number of instruments he identified as 'Malayan' in fact occur in various parts of Africa; for example, the struck bamboo, the struck wooden plaque, the leg-xylophone, the transverse conch, and the frame-drum made from a broken-off pot-neck. This leaves only four instruments without immediate African parallels; suspended concussion tubes, the tube-zither, the suspended drum beaten with three sticks, and the conical drum with Y-lacing. While to the organologist these instruments may indeed present striking analogies to instruments found in the Indonesian islands, this list might well seem neither very long nor very impressive to a prehistorian.

My point is that while Sachs' work turns out to be remarkably accurate, it is so in spite of its lack of formal criteria. If we only had Sachs' intuitions to work with, I doubt that they would today be regarded as a serious source for the pre-history of Madagascar. Parallels in material culture may be the inspiration for the construction of a hypothesis, but evidence from other disciplines must be brought to bear on any argument with such far-reaching implications.

There is another reason for care in this particular case, which has specifically to do with African ethnography; the links that this hypothesis has with the notion that 'high culture' elements in Africa were imported. In the earlier part of this century Egypt and the Ancient Middle East were favoured, and typically, this led to the attribution of Zimbabwe ruins to errant Phoenicians or Sabaeans, simply because Africans 'couldn't have' built such structures. An early example is Johnston (1913) who declared that 'domesticated animals, all musical instruments superior to the musical bow and the drum, several types of game played with hollowed or divided boards, and a good many Egyptian notions about religion' came to sub-Saharan Africa from Egypt. These same notions are also present in an otherwise very reliable study of the Jukun people of Nigeria (Meek, 1931) which also has a section remarking on the similarities between Central Nigerian and Japanese names. More wide-ranging, but no more reliable is the account by Hutton (1946) drawing on parallels between Central Nigeria and customs distributed across the face of Asia.

It is difficult now to evaluate objectively the assumptions implicit in this sort of reasoning. In the case of Africa, this type of ethnography undoubtedly reflected a desire to reinforce ideologically the position of the colonial rulers, as MacGaffey has clearly documented (MacGaffey, 1970). Beyond that there will be an inevitable ingenuousness exhibited by scholars working with a limited range of materials and often confined by the perspective of a single ethnography. However that may be, the effect was to excite a reaction, vindicating African creativity and demanding rigorous standards of proof before any cultural element could be admitted as an introduction. Ethnomusicological arguments for external influences on Africa were seen as another version of the idea that 'high culture' elements were imported, and were therefore disdained. Ethnomusicologists undoubtedly laid themselves open to this sort of rejection, since they by and large confined themselves to asserting that a connection between Indonesia and Africa existed, without attacking the problem of

the historical circumstances of such a connection.

The purely musicological aspect of the argument first appears in an article by von Hornbostel (1911) in which the comparative tunings of some Burmese and central African xylophones are set out, showing their similarity both in tonal system and absolute pitch. Later, von Hornbostel (1920) developed the fully fledged theory of the 'cycle of overblown fifths', a complex idea that included an attempt to demonstrate a relationship between Oceanian and South American panpipes. This theory is generally held to have been discredited by Bukofzer (1949), but in its time it was considered to be significant for the reconstruction of musical prehistory, and Jaap Kunst made it an important element in his exposition of Javanese music (Kunst, 1973). It is not my purpose to examine the theory here but to consider the use to which it was put in Kunst's (1936b) expanded discussion of the tonal systems of African and South-East Asian xylophones. Kunst asserts that not only is 'the structural relation of the notes of the scales compared always the same, but also their absolute pitch' and goes on to conclude that 'this fact disposes of the hypothesis of a series of accidental coincidences'. We may note in passing that this would not be a series of coincidences, but merely two coincidences, namely that the same tonal system has been adopted in two widely separate parts of the world, and that the absolute pitch of treble xylophones was approximately the same.

Up to this point, the argument might merely be taken to show that similar tonal systems can arise from some more fundamental cause such as the perceived musical properties of overblown tubes. But Kunst shows that he wishes to draw specific historical conclusions by continuing with a list of morphological comparisons between the xylophones in question, in order to demonstrate that the traditions are related. The xylophones under consideration are the large frame-xylophones found both in Africa and South-East Asia. African xylophones normally have gourd resonators, either suspended under the keys or else gummed to a wooden board which passes under the keys. The keys themselves rest on insulating bars, or more rarely, are suspended from spacers running transversely between the keys. Suspension-xylophones are the most common type of South-East Asian xylophone, although instruments with the keys resting on insulating bars are recorded from Indonesia. The most common resonator in South-East Asia is a box-resonator, the only form known from the mainland, although individually resonated keys are found in Indonesia. A significant feature of virtually all African xylophones is the use of a mirliton or kazoo membrane on the resonators of xylophones in order to give the instrument a conspicuous 'buzz'.

Several points emerge from this comparison. First, it is clear that a certain sort of xylophone has been selected for study. This is the standard 'treble' xylophone of Africa and South-East Asia, the type most often collected by museums, as the instrument is complete in itself and is most convenient to transport. This suggests that the coincidence of absolute pitch is a phenomenon of the selection of the instruments for study. It also reveals a crypto-hypothesis in Kunst's argument – that the large frame-xylophone arrived in Africa fully-evolved and has, thus, no connection with the many other types of xylophone found in the continent. This is one of the most disturbing aspects of the argument, as, in Africa at least, all the various stages intermediate between the single struck stick and the large frame-xylophone are actually present. Leg-xylophones, pit-xylophones, and xylophones consisting of

one or two bars laid on a mortar or clay pot as a resonator provide a fairly convincing line of development for the xylophone with individually resonated keys. Curiously enough, it is in Indonesia that the sequence is more deficient – there seem to be no instruments between the leg-xylophone and the c nplex orchestral xylophones. The only exception to this may be the *gloenggangan*, a type of xylophone reported from Bali; a set of slats lying on a wooden frame, resonated with hollowed coconut shells placed beneath the keys. A photograph of this instrument accompanies Kunst (1936a) and Jones (1971) includes a sketch of it, but in neither case are important morphological features apparent – it is not clear how the resonators are fixed to the frame, or how the keys stay in place. It is not even certain that this is not a very recent tradition.

Kunst (1936a) also gives comparative photographs of the Javanese 'Gong Bujung', a single struck key, suspended on cords from posts mounted on a built-up, wooden, box-resonator, and the single-note suspension xylophones of Central Africa. In this latter instrument, found in Malawi, Zambia, and Southern Zaire, a single bar is suspended from two bowed pieces of wood gummed to the top of an open spheroidal gourd. When the bar is struck, the hand is inserted between the bar and the sound-hole of the gourd and moved in order to alter the timbre of the note. The terminology and use of the instrument suggest that it is a portable simplification of xylophones found in the same area and that its construction is designed to help the performer imitate the speech-tones of the vernacular language. Certainly it would be curious if an instrument from Central Java were to make its way to this part of the continent, isolated from the other instruments in this study, and without being reported either from Madagascar or coastal Mozambique.

In a further article, 'The origin of the Kemanak', Kunst (1960) expands some of these arguments, again based on the instruments. The discussion focusses on the *kemanak*, Javanese concussion-clappers. *Kemanak* are made by folding round a metal sheet (normally iron) into (roughly) the shape of a banana. The edges of the sheet are not joined, so a slit runs along the edge of the vessel. Handles are added and a pair of such instruments are struck together as an accompaniment to orchestral music. Kunst says 'Nowadays this same instrument is found in West Central Africa among the Fang or Pangwe (an obsolete term for speakers of Fang languages in Cameroun and Gabon), and again as an accompanying instrument in religious dances. There is the same dual playing technique, the slight warp in the back, a shape comparable to a banana slit open lengthwise. . . . '. Kunst also claims that Schaeffner reports the same instrument from the Kissi people of Guinea. Unfortunately this is due to a series of errors on Kunst's part. The source to which he refers is 'Die Pangwe' (Tessmann, 1913), with a chapter on musical instruments by Erich von Hornbostel. The illustration (Abb. 121) that Kunst claims shows the 'same instrument' as the *kemanak* is clearly labelled 'Schellen' in the caption. 'Schelle' is one of several German words for bell, and is normally used in ethnomusicological contexts to apply to pellet-bells which is indeed what von Hornbostel's illustration shows. The rattling pellet is clearly visible and remains so in the reproduction Kunst made of the original drawing to accompany his own article. These bells are normally strung and tied to the leg for dances. The text contains no mention of the 'dual playing technique', nor of any restriction to religious dances. The Kissi instrument Kunst refers to is the *kende* described in Schaeffner (1954 pp. 22-25), an iron, banana-shaped slit-bell.

Schaeffner, is however, careful to use the singular form 'cloche' throughout his article, because the *kende* is held in one hand and struck with a stick by the other. It thus lacks one of the essential structural features of the *kemanak*, the handles, which enable the Javanese instrument to be sounded by concussion. The *kende* bears only a small, iron, suspension-hook. Schaeffner's review of the contexts of use of the instrument suggest that these are extremely various even within Kissi territory, and that no specific inferences can be drawn from them.

Kunst's statements were repeated uncritically by Kirby (1966) and Jones (1964 & 1971) and Jones further compounds the error by claiming that the Kissi use the *kende* as concussion bells (Jones, 1971 p. 158), though nothing to that effect is to be found in Schaeffner's text, and that a number of other peoples including the Dogon, Ewe, and Bemba also use instruments like the *kemanak*. Schaeffner has a reference to a slit-bell among the Dogon (Schaeffner, 1954, p.22, footnote 17), but this instrument, *ginu*, is in fact a small, spheroidal, iron bell sounded with a percussion ring which bears no resemblance whatsoever to the *kemanak* (Schaeffner, 1960). Jones' other examples are equally suspect. His own photograph of the Ewe *atoke* show that it is a percussion bell, after the manner of the *kende*. Walton (1955) describes a single iron slit-bell excavated at Zimbabwe. We cannot know exactly how it was played, but the presence of suspension-hooks at both ends of the instrument suggest strongly that it was hung up and struck. Jones further reports its use among the Bemba; but Father Corbeil, Curator of the Moto-Moto museum at Mbala, who has been studying Bemba musical instruments for some years, and is the author of a monograph on the subject (Corbeil, n.d.), has never encountered such an instrument. From this we may conclude a banana-shaped percussion bell is restricted to isolated areas of West Africa, and that it has only very general morphological similarities with the Javanese *kemanak*, insufficient evidence on which to base an argument for a historical connection between the two.

Kunst (1960) also illustrates a whistle from the Atoni people of Timor, intending to show its similarity with those found among the Bari in the Sudan. The Timorese instrument has a saddle embouchure set into an egg-shaped upper part, from the base of which projects a cylinder roughly half as long again as the 'egg'. The 'egg' portion is pierced with two fingerholes. The Bari instrument illustrated, along with a less similar instrument from Northern Nigeria, are certainly much the same structurally. The problem is that the variety of whistles in Africa is so great that it is easy to find examples to support almost any case. A study by Laurenty (1974) of Zairean whistles, illustrates nearly a thousand such instruments, classified into more than thirty types. Whistles with saddle-embouchures are found in Burma and Lithuania in forms comparable with those present in various parts of Africa. Moreover, Timor and the Southern Sudan both lie outside the zones of Africa and South-East Asia normally postulated by supporters of Indonesian-Africa connections. Significantly, the only area where such whistles are definitively absent is Madagascar and the East African coast – the area where 'Indonesian' influences are most evident.

Work by Kunst (1936 a,b) on the tunings of the xylophones was considerably revised and expanded by Jones (1964 & 1971). Jones's central argument is that African xylophones and sansas (mbiras) are tuned in most cases to one of three scales; equiheptatonic, equipentatonic, or a scale 'close' to the heptatonic *Pelog* scale of Java. Jones's methods of assessment leave much to be desired. He permits xylophone-builders to use 'gapped' scales (pp. 69-74) and, further, to bisect the interval created by the 'gap'. Builders may also add 'intercalary' notes, either explained as somewhat out-of-tune duplicate notes or as the notes created by bisecting the 'gaps'. While we have a fairly full commentary on the intentions of xylophone-tuners in Indonesia as a result of the researches of Kunst (1973) and Hood (1954), there is no equivalent material for African xylophone-builders. It is illegitimate to term certain notes on an instrument 'intercalary', or to assume that scales are 'gapped' without knowledge of the intentions of the builder, or failing that, without an intensive assessment of the musical forms within a particular culture. In a review of the 1964 edition of Jones's book, Blacking (1966) criticised in detail Jones's explanation of the methods adopted by African xylophone-builders, on the basis of his own fieldwork in South Africa and Zambia. It is to be regretted that Jones did not answer the objections raised in Blacking's review, nor even include it in his bibliography, when he added new material for the 1971 edition.

Hood (1965) also reviewed Jones's book and criticised his method of arriving at tunings by striking averages, a basic methodological error, and shows that this would make nonsense of a description of the scale-system of the various large *gamelan* orchestras on Bali. Hood further pointed out that Jones had relied almost exclusively on Museum instruments, the tuning of which might be considered unreliable for a number of reasons. Jones recognises this problem, but uses it in a curiously selective manner to explain 'bad' notes of the scale. Our confidence in this procedure is likely to be destroyed when Jones used the lack of fine-tuning wax on Javanese xylophones in museums to account for 'bad' notes, when we learn that the Javanese do not use tuning-wax (Hood, 1965, p. 1580). Jones made some reference to Hood's criticisms in the 1971 edition of his book, by including sequences of tunings from xylophones recorded 'in the field' (and thus, presumably, tuned as the players desired), but he did not mention Hood by name, nor did he respond to the serious objection that standard theoretical tunings were calculated by striking averages. A further feature of his 1971 additions to the text is that all mention of the scale 'close' to *pelog* is dropped. I can only assume that xylophones recorded in the field do not bear out his contentions about this scale.

Few musicologists would be inclined to dispute the presence of an equiheptatonic scale both in Africa and in South-East Asia. Morton (1976) has argued forcefully for an equiheptatonic scale in Thailand; Gilbert Rouget and Gerhard Kubik have identified such a scale in various parts of Africa; and I have personally recorded equiheptatonic zithers in the South of Malawi. But this scale does not occur in Indonesia, whence Jones derives most of his other evidence. If the only instruments with the equiheptatonic scale are present on the South-East Asian mainland then this creates something of a problem, because the evidence suggests that xylophones are a recent arrival on the mainland. Morton (1976) notes that they are not depicted at Angkor Wat, despite the wide variety of instruments illustrated on the reliefs. They do not appear in Thai manuscripts until about 1730 A.D., about a thousand years after the latest possible date for Indonesian voyages to Africa. Even if this were not the case, the coincidental presence of a scale is too weak a basis on which to sustain a migration-hypothesis. The equiheptatonic scale also occurs among the Are'are of the Solomon islands (Zemp, 1972); surely we do not have to explain this on the basis of an influx of the ancestors of present-day Thais?

As for the equipentatonic scale, Hood cogently objects that the Javanese scale *slendro* is not an equidistant scale, the conclusion of his study of the modality of *Patet*, published in 1954 (Hood, 1954), supervised by Kunst, who presumably concurred. Jones' figures may show the presence of an equipentatonic scale in Africa, but it does not further his case if no such scale is present across the Indian ocean.

In the case of equiheptatonic xylophones, one might expect African instruments to show morphological similarities to their South-East Asian counterparts if the two are connected. But the xylophones of the Asian mainland, whether Burmese, Cambodian or Thai are all trough, suspension-xylophones. This form of xylophone does not occur at all in Africa. Suspension-xylophones (that is, the keys are suspended on cord loops and thus insulated from the damping effect of being in contact with a frame) are found in a restricted area of South-Central Zaire (Boone, 1936) but these instruments have individually-resonated bars. Trough or box-resonated xylophones have been reported from a few widely dispersed areas; the Ekoi of Southern Nigeria (Talbot, 1926), the Bwaka of Zaire (Boone, 1936), the Cuabo of Northern Mozambique (Kubik, 1963) and among various peoples on the East African Coast (Anderson, 1967). In every case the keys are supported on the edge of the trough by an isolating pad of fibre or rubber, running along its rim. This is the normal method of supporting the keys on box-resonated xylophones in Java today – but Javanese instruments are not equiheptatonic. Nevertheless, the presence of this type of xylophone on the East African coast might be suggestive, if it were not for the fact that it only seems to have supplanted the more traditional instruments, laid on bananalogs, in the late nineteenth century (Anderson 1967). This seems reasonable because the resonators of the East African xylophones are all of the 'built-up-box' type - that is, the resonator is built up from planks which are nailed or tenoned together. This type of construction seems to have been traditionally unknown in Africa (Stuhlmann, 1910) and it is significant that the Nigerian and Zairean instruments both have carved-out boxes. The switch to box-resonated instruments in East Africa presumably occurred when sufficient quantities of waste planking became available to make possible the construction of built-up boxes.

Another chronological problem is provided by the absence of individually resonated xylophone-keys on the reliefs at Borobodur in Java, which date from the eighth and ninth centuries (The first clear illustration of individually resonated keys dates from 1597 (Kunst, 1973). A wide variety of instruments are represented, including box-resonated xylophones – implying, perhaps, that the former type had not yet been introduced. If this is so, then it would be impossible for Indonesians to have introduced it to Africa later than this date.

The linguistic evidence from Madagascar suggests that the source of the people who colonised the island was fairly localised – Malagasy has been related to the language of the Maanyan in Borneo. This would explain the absence of xylophone in Madagascar, as large frame-xylophones seem to be unknown in Borneo (Shelford, 1904), except for those introduced by the Javanese at Banjermasin (Kunst, 1973). It also implies that any group responsible for the introduction of frame-xylophones from Indonesia would have to be quite separate from the migration of peoples from Borneo to Madagascar. This points to another error in Jones's method; the conflation of evidence from a fairly well established migration on the East coast with that from a highly dubious migration to all other parts of the continent. He discusses the distribution of the *sese* or bar-zither, an instrument found in a large part of Eastern and Central Africa, as well as Madagascar. The distribution and possible origins of this instrument have already been considered in some detail (Blench, 1983) but it is sufficient to say that this instrument was almost certainly introduced from Asia and most probably from Indonesia. Its distribution is conspicuously different from that of the xylophone, as it is found principally along the East Coast, whence it diffused to the interior of Kenya and Tanzania, and to Zambia, Zaire and Uganda only at the end of the last century (Blench, 1983 & de Hen, 1960). It is surely indefensible to use this as evidence that the xylophone was introduced from Indonesia; to do so is to blur the distinction between a well-established historical fact and a very questionable hypothesis.

Another piece of evidence adduced by Jones is the 'drum-xylophone' or multiple-slit percussion cylinder, occurring over a large area of West Africa between Senegambia and central Ivory Coast. This instrument, normally wooden, has two or more tuned 'lips' and is described by Schaeffner (1954) and Zemp (1973). Smaller versions in bamboo are used by women and children. A struck slit bamboo is also found in Java, but it seems the purpose of the slit is to make the instrument sound louder, as in the case of the Thai *krong*, a struck bamboo log (Yupho, 1960). Certainly the Javanese struck bamboo is not a scaled-down version of a larger, wooden instrument with tuned 'lips'. If the instrument were imported from Indonesia, it is not at all clear why its distribution in Africa should be so limited, given the extensive range of the frame-xylophone.

Jones has a discussion of the old Javanese *kemanak*, a type of slit-bell; apparently no more than a sheet of iron folded into a cylinder, with a handle projecting from one end of the cylinder, struck, presumably with a separate beater. He then describes the West African *gankogui* conical, clapperless bells with separate beaters, made from two similar iron plaques welded together. These bells are widely used in Africa, they are hardly comparable with the *kemanak* slit-bells. He fails to show that this latter bell is found in Africa, though a conical, clapperless, folded bell of this type is used by Afro-Cuban musicians. A photograph (Picture 20) purporting to show African slit-bells shows bells at a shrine in Iwo, Nigeria; but as the photograph clearly reveals, these are clapper-bells, and, moreover, conical, with an elaborate ornamental handle. He says 'the Afro-Cuban manner of playing is exactly the same as that of the Ewe (cf. Picture 19)'. In fact, Picture 19 is a museum photograph of a pair of *gankogui* welded bells previously described, telling us nothing of the manner of playing, nor affording any relevant comparison with the slit-bells in question.

Leaving musical instruments, Jones passes to *mancala*, a variety of board-game found extensively in Africa and Eurasia. His description of its distribution closely follows that of Béart (1955), who comments on morphological similarities between forms of the game found in West Africa and in South-East Asia. According to Jones, West Africa is the home of the 'classical game', that is, with two rows of six holes, two end-holes, and carved wooden boards, and this is the form most widely found in the general area of present-day Indonesia, and by implication, not elsewhere. This would be remarkable if it were true, but, regrettably, Jones has distorted the evidence to fit his case. His bibliography lists H.J.R. Murray's remarkable study 'A history of board games other than chess' (Murray, 1952), but he has ignored the implications of the discussion of the history of the game contained therein. The version of the

board-game under consideration is classified by Murray as 'Mancala II' and is characterised by its two rows of holes. This version of the game is recorded in Egypt, Sudan, Arabia, Turkey, Iraq, Iran, India, Ceylon, Annam, Thailand, throughout the Indonesian islands as well as in West Africa, and in the West Indies whence it was taken through the agency of the slave-trade. It seems to have been known in Ptolemaic Egypt as well as in Neolithic Sri Lanka (second to fourth centuries A.D.) precisely in the 'classical' form. The similarities between the Indonesian and African forms of the game may then be convincingly explained as a historical connection via the Eurasian land-mass.

A further objection is that the only form of the game recorded in Madagascar is 'Mancala IV' – the most common form of the game in South-Central Africa, and not found elsewhere in the world. If the Indonesian form of the game is not known in an area where Indonesian navigators certainly landed, it would be curious if they were responsible for its establishment in areas where their presence is hypothetical only.

Jones also wishes to connect the West African brass-casting industry with 'old Khmer bronzes'. He sees 'Indonesian' features in the thirteenth century bronzes from Ife in South-Central Nigeria and suggests that the technique of cire-perdue was imported from South-East Asia. Apart from the folly of supposing that individuals of an Indonesian physical type were present in Medieval Nigeria and yet have left no other record of their existence there would seem to be no reason why cire-perdue casting should be so highly localised, if the frame-xylophone is found continent-wide.

In considering the possibility that ancient Indonesian navigators could have rounded the Cape, Jones quotes Villiers (1957), the most competent authority; 'it is my belief that these Eastern vessels could have sailed round the Cape of Good Hope to the west'ard, but I am not so sure of their ability to go back again . . . .' suggesting that the possibility cannot be ruled out. The presence of outriggers in Madagascar and on the East coast of Africa is normally thought to have been introduced from Indonesia. Jones presents some evidence for both outriggers and square-sail rigs (another typically Indonesian rigging) on the West African coast. He is unable to give photographs of either type of craft in use, leaving their existence in precolonial times an open question. Recent reviews of the navigational capabilities of the Austronesians (Golson, 1972) have left us in no doubt that these were considerable and experiments by Bechtol (Golson, 1972, pp. 98-101) have shown that Oceanic canoes should be able to sail successfully to the windward. But to allow that Indonesian navigators could have rounded the Cape is not to show that they did.

In a more recent article Jones (1972) offers evidence perhaps more convincing than any other element in his case. Laurence (1968) has demonstrated that elephantiasis originated in Oceania and that it has probably spread across the Indian Ocean at a relatively recent date, and that its presence in Africa is due to a large migration of individuals carrying the disease. He considers the carriers may have been the prehistoric Indonesian migrations – although if this is the case, then a high incidence of elephantiasis would be expected on Madagascar. Laurence (1968) marks the presence of the two most common carriers of elephantiasis (or Bancroftian filariasis) on Madagascar and I assume the implication is the disease is common there. If that is so, then it is interesting that Jones (1972) fails to mark Madagascar on his sketch map comparing the African distributions of xylophones and elephantiasis. The absence of

frame-xylophones on Madagascar makes nonsense of his claim that the two are found together. Even without this, it seems odd to argue that a disease should spread with a musical instrument, since the ecological constraints which define their distribution are so different. Nevertheless, one point of interest does emerge from this investigation. Several of the clay figurines excavated in central Nigeria and identified as belonging to the Nok culture are clear portrayals of victims of elephantiasis (Willett, 1967, pp. 61-63). This dates them to between 900 B.C. and 200 A.D. This is extremely difficult to explain if we consider that elephantiasis was brought to the East coast and then spread across the continent and if we accept the conventional dating for the Indonesian presence on the East coast.

I do not consider we are presently in a position to estimate the possible significance of prehistoric Austronesian migrations on the course of African history. Undoubtedly there was an important influx from across the Indian Ocean to Madagascar and the East coast of Africa before 500 A.D. and with the immigrants came certain sailing technologies (Hornell, 1928 & 1934) as well as important agricultural staples. Although many of his conclusions are dubious, the emphasis placed by Murdoch (1959) on the impact of the South-East Asian food-crops (banana, cocoyam and water-yam) on African subsistence agriculture was fully justified. Until further information on indigenous cultivars of these crops becomes available, I doubt that we can increase our understanding about the routes whereby they spread through the continent. For musicologists, however, I think the moral is clear. The evidence for connections between South-East Asia and West-Central Africa, whether in types of instrument or scale-system is thoroughly insubstantial. There is little doubt about the Indonesian origins of some aspects of Malagasy culture, but this has no significance as regards the hypothesis of a connection between Indonesia and West Africa. More than this it is an error of method to argue for a connection between cultures if the evidence is based solely on material culture artefacts without taking into account evidence drawn from other disciplines, in particular linguistics and ethnobotany. Historical arguments constructed in this way are likely to bring ethnomusicology into disrepute with other branches of African studies. In an area as academically sensitive as diffusionist-hypotheses in the explanation of cultural origins extra care must be taken, since the growth of a sense of historical identity among peoples of other cultures means that, without thorough documentation of a case, it is unacceptable to attribute their cultural developments to outside influence.

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