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Drawing by G. W. Stow after Rock Engraving at Wildebeestkuil; Rubbing by Miss D. Gross from Rock Engraving in the British Museum

Springe Corral, South Sumatra

The Awuta Stool

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SOME SPECULATIONS CONCERNING A DRUM CHIME IN BUGANDA

By Professor K. P. Wachsmann, Department of Music, University of California, Los Angeles

In the course of two centuries the entenga drum chimes rose from the obscurity of a remote village in Buganda to one of the most eminent positions in the music of the Kabaka’s court. Recently, musical scholars began to speculate on the similarity between the entenga set and the drum chimes of Burma (Kirby, 1961a), and to ask whether that similarity could be used as evidence for musical contact between East Africa and South-East Asia. The entenga are rarely played and seldom seen, and until more recent times they were not easily accessible. This may explain why so little has been published about them hitherto. Thus it may now be opportune to describe the set and examine its place in the oral tradition of Buganda in detail, however little data there may be and in spite of the—deceptively—parochial nature of these data. For this task I have at my disposal vernacular and other literature listed in the bibliography to this article and my own field notes. The regions in Uganda that these speculations concern are Kyaggwe in East Buganda, Busoga, Bugwere and Lango (fig. 1).

The instruments are of the so-called Uganda kettle-drum type (Trowell and Wachsmann, 1953, pp. 369f). The wooden shell, open at both ends, is tubular in shape for at least half of its height, and tapers towards a narrow base for the rest of its length. The term ‘cylindrical’ is indeed apt for this type (Sachs, 1942, p. 32). The drums differ from many other Uganda kettle drums, which have convex sides instead, and this difference may well be found significant when the East African kettle drums are studied afresh.

Membranes of cowhide are placed across the open ends of the shell and a dense lacing of thongs of twisted cowhide runs from skin to skin in a continuous series of narrow V turns. At the turns the thong is threaded through holes in the edge of the skins; the V turns are so close that they conceal the shell almost completely. The lacing supplies tension to the skins but only the larger skin is used in playing, while the other, stretched across the base, is nonsonorous and provides anchorage for the lacing. In a medium-sized drum the lacing cord may be about 300 feet long, completing some 150 V turns.

Lacing and tuning are jobs for an expert. At the first tuning of a new drum and at later major overhauls only the Kabaka’s drum-maker—who in 1945 at the time of my second visit was Mr. Kasumba at Kabowa village—is considered competent. In his absence and for minor adjustments wooden wedges are placed between the lacing and the shell to raise the pitch, a need that is most likely to occur when, after a long spell of inactivity, some of the tension has gone out of the cords. In Buganda the insertion of wedges into the cordage appears to be a makeshift arrangement, but of course it is also possible that it shows an affinity with other cultures that employ these wedges habitually, and not as a temporary device. In entenga, the

FIG. 1. MAP OF UGANDA

Drawn, as also figs. 4, 6 and 8, by A. Apostolides, Ethnic Collections, U.C.L.A.

Description

Comparative studies require as much detail as possible, however tedious the description may be. The most noticeable entenga features can be summarized as follows.

Entenga (fig. 2) requires six musicians to play 15 drums.

* With 12 figures

FIG. 2. THE ENTENGA, PRESUMABLY IN 1913

The drums in the centre background do not belong to the chimes. After Barnes and Kearton, 1935, p. 87
pitch is raised usually by striking the handle of a heavy drum stick against the lacing where it passes through the holes along the rim of the head, with a vigorous, downward, slanting motion. This transfers to the sound-skin some of the tension ‘stored up’ in the lacing. For flattening the pitch, the centre of the head is forced down by the fist applied with steady, firm pressure. During tuning the pitch is tested continuously by tapping a thin entenga drum stick against the edge of the head. In this way the resonance of the shell itself is probably also taken into account.

Drums 1–12 are tuned with special care; this care is required because of the comparatively small intervals between neighbouring drums. The musical aspect of the tuning procedure is the same as that employed in Kiganda harps and xylophones; it has been described in detail elsewhere (Wachsmann, 1950, p. 40; 1957, p. 14). Needless to say, Kiganda harp strings and xylophone keys can be tuned more accurately than drum skins. At my only opportunity of checking the pitch of the drums with a Hornbostel Travel Tonometer, the approximate sequence given in fig. 3 was noted.

**FIG. 3. APPROXIMATE TUNING OF ENTENGA**

The arrangement in which the drums are set out for playing could be seen in fig. 2, a photograph that was taken by Kearton in 1913 (Barnes and Kearton, 1915, p. 87). It is shown in detail in fig. 4, in the form of a diagram sketched on my visit in 1945. Drums 1–12 are tied to a horizontal beam that is itself secured at some distance from the ground to two upright posts. Four players crouch in front of them, with the high-pitched end of the ‘keyboard’ to their left. Drums 13–15 are heavy and tall; they stand on their own, and the musicians in order to reach the heads comfortably remain on their feet, facing the line of drums 1–12. In fig. 2 the smallest drums are nearest to the viewer. This arrangement was also found inside a hut in the Kabaka’s enclosure where the smallest drums were in view close to the entrance.

In fig. 4, the hatched areas show how a player’s reach overlaps that of his neighbour when he is required to play more than just the three drums in front of him. At the most, the range of a player embraces five drums so that he can, by means of octave-displacement (Wachsmann, 1965, p. 80) render any theme that might be demanded of him in a pentatonic pattern.

A characteristic feature of the drums 1–12 is their being coupled in pairs. In fig. 4 the little links of twisted thong are clearly shown; fixed to the drums just below the head, they permanently join two neighbouring instruments. In ordinary kettle drums in Uganda, both ends of a link are secured to one of the drums forming a U-shaped loop that makes a convenient carrying device. The men explained that the coupling makes it easy to fold the drums so that the heads lie cheek by jowl and to wrap them up, and that in any case it prevents the drums from moving about during play.

Another notable feature is the extremely thin and long sticks with which drums 1–12 are played. Because of their thinness, the handles are wrapped in a bulge of cloth to provide a grip. These sticks gently curve toward the tip through about 45°; the drum head, which is slightly tilted towards the players, is struck, or rather whipped, by the convex curve, either in the centre of the head or close to the rim. In the first case, the entenga, if listened to from a distance, sound like an orchestra of bow harps; in the latter case they give the illusion of the ripple and clutter of faraway xylophone music. The illusion is all the greater because the repertoire and the texture of entenga music are those of xylophones and harps. I would like to mention, for what it is worth, that Le Veux (1917, p. 762) gives a word entenga as meaning ‘essaim de termites qui bourdonnent.’

As for drums 13 and 14, ordinary middle-weight beaters are used, whereas drum 15, Nakawombe, is struck by a pair of heavy wooden clubs. Kaggwa (1949, p. 48), however, differs on this point; his text is given in full in the section on ‘The Tradition’ below.

**Terminology**

The terminology applied to drums 1–12 identifies the range of each player’s part rather than the drum group which he plays, and, in consequence, where the reaches of two players overlap, the nomenclature for the drums embraced by their ranges will also overlap. Thus the highest-pitched group may consist of drums 1–5 and be called obutono, lit. the small ones, an expression generally applied to high-pitched sounds; followed (in the order from high to low in which drums 1–12 are arranged for playing) by 3–7, the enjauwuzi, lit. the dividers, a term that may signify no more than the central position of both the drums and the pitches; by 6–10, the enazi, whose function is to start the first theme, from the verbal root -naga for
which Le Veux (1949, p. 702) gives French ‘préluder’ as one meaning; and by 8–12, the *amentengezi*, which may be translated as the ‘tremblers,’ a word derived from the same root as *tengëteza* which may be used, for instance, to describe the effect of an earth tremor on a house (Le Veux, 1917, p. 1002); of course, the sensation of trembling, both orally and visually, is characteristic of objects yielding a low-pitched sound. These 12 drums are looked upon as a unit, an *omuwawans* (Wachsmann, 1953, pp. 397 and 404).

The largest drum, 15, is called Nakawombe; this word, a proper name rather than a technical word, also serves as an alternative reference to the *entenga* set as a whole (Le Veux, 1917, p. 762; Kaggwa, 1934, p. 263). *Entemyo*, 14, gives the fifth, and *ekyatwakati* the octave, above Nakawombe. *Entemyo* is an interesting term: it is borrowed apparently from the topmost rings (sgr. *akatemyo*, pl. *obutemyo*) at the apex of a cone-shaped roof. *Obutemyo* also describes the three shortest strings of the bow harp *enimanga* that run deepest in the concave curvature of the neck of the instrument, a position in which the equation ‘innermost roof rings = top strings’ is particularly graphic. But in fact the drum *entemyo* does not sound the highest note of the trio 13, 14, 15, so that one may be justified in doubting this reasoning. Further research is all the more desirable since yet another term, *enjongo*, is often rather confusedly applied to drums 13 and 14. Probably it should apply only to drum 13.

The word *njongo* is a case in point of the difficulty which one often has in understanding the vernacular nomenclature for drums and ensembles or groups of instruments. It seems to be used commonly as a noun in the singular, as if there were only one drum with this name (Kaggwa, 1934, p. 263; 1949, p. 48). However, it may be the plural of the noun *olnjongo*, a form in that fact is used by musicians. The drum pointed out to me by the name *olnjongo* was housed in the same hut as the *entenga* drum chime, together with the *entamiviu*, battery of drums, and the xylophones, and I was told that this *olnjongo* belonged to *entamiviu*. It was remarkably slim for its height of 105 cm.: the maximum diameter across the head measured a mere 26 cm. It is perhaps relevant that Le Veux (1917, p. 562) lists an adjective *olnjogjongo* which describes, for instance, a man who is as thin as a post. It is also noteworthy that in the drum and dance ensemble *amag boringu*, two of the members, the *namatongo*, can alternatively be called *obogongo* (Nsimb, 1956, p. 234). For others (Kaggwa, 1949, p. 50), *obu* is the plurality prefix for the word that refers to small drums as object in the *aka*-class (small object).

**The Tradition**

Kiganda oral tradition is much occupied with the history of musical instruments and with events that gave rise to the composition of songs. Next to the most important of the regalia drums, Ttamba—whose origin is ascribed to Kimera’s reign at the beginning of the sixteenth century (Haddon, 1957, p. 115)—, most detail seems to have been lavished on the story of the *entenga* chime. Kaggwa’s (1949, pp. 47f) is the most complete version of those available in print. The following paragraphs are a translation from the Luganda of the chapter on the achievements of the Lugave (Manis) clan.

Mukadde’mwango is the chief of the drum called *entenga*, and *entenga* began like this: There were four men, Nagamala, Kijo, Kikunyi and Namugiga, the sons of Kysimbi, in the village Lugala in Kyagwe county. They started to learn to play *entenga* on their own account while they were still at their father’s home. When they were satisfied that they had mastered the art, they served as musicians to Mmuniyi, who belonged to the Kajugwe division of the Bugiko clan, and who was the priest for the spirit cult of Nende at Bokerere. Mmuniyi gave them the village Budu. He suffered from cancer, and they began to play a song. *Mwango: 'Entenga: 'takongwa,' i.e., ‘Mmuniyi suffers from cancer now he will not recover’ and Mmuniyi was much entertained by it. The spirit Nende got to know the musicians very well and they began to play for him as his retainers.

Once they accompanied Nende on a visit to the place where Mutebi (i.e., the omulongo, lit. the ‘twin,’ meaning here the navel cord of Kabaka Kyabaguzi) was kept, and they played for Mutebi. When Kyabaguzi heard the sound of the *entenga* being played at Mutebi’s he became enamoured with it, and taking it away from Nende, he kept it for himself. He gave the village Kalebera to the young musicians. They added other drums to the set later, having produced many offspring. The drums remained, until today, a Kalebera’s set. Thus developed *entenga*.

What makes *entenga* so pleasing is its murmuring sound. Kyabaguzi loved to have *entenga* played for him, and he was fond of the men who played it; he gave them many presents and they grew rich. He also gave them a spear called Kawawa (meaning; ‘as the fly, kawawa, does not leave the cows, so should this spear never leave the drums’; it was to watch over them for him). It is planted always in the midst of the set played in its presence, and it was meant always to be an object of great prestige. He also gave them the tip of an ivory tusk to hammer the drum sticks with when they had got limp and they then became taut again.4

But the drums of *entenga* are not as many as those of Nakaguzo. *Entenga* consists of a set of small drums called *omuwawans*, twelve in all to play the song, and another drum called *enjongo*, which is struck with one stick only, and two others that are called *entamiviu*. *Entenga* has thus fifteen drums altogether.

Several points of this narrative invite discussion, most of all the dramatis personae. As in all Kiganda oral tradition, the narrator makes much of citing the names of the people that figure in the story. As an artistic device to lend reality to the plot this practice is presumably universal; for a Kiganda audience it helps to assign a place to the musical data in the network of clan and dynastic relationships that until recent times was so important for every man.

Eight persons are thus named, one of them, Kyabaguzi, a king. To these should be added Kajugwe and Nende, the two ‘personae’ that were attributed to Mmuniyi, the first patron of the drum chimes. Kajugwe was the title for the head of the Kajugwe division of the Bugiko (Mushroom) clan, a post that Mmuniyi occupied; the other persona, Nende, was an immortal spirit, represented by Mmuniyi. Both, Kajugwe and Nende, had once been names of real persons. In those days, before they [the gods] became spirits [halubale], they were still real people’ (Kaggwa, 1927, p. 19). That they should appear and play such an important role in the genesis of *entenga* calls for comment. Both add a strong local emphasis to the plot, as if a simple statement that the *entenga* came from Kyagwe
The most important actor in the *entenga* drama is of course Kabaka Kyabagyu. The oral tradition has much to tell of his career (Kaggwa, 1927, pp. 59–63). He had the murderer of his royal uncle executed, but the ghost of the murderer haunted him. His reign was full of antagonisms between himself and his people. He conquered Busoga and did not wish to return to Buganda. His love of Busoga was so great that he planned a mystical union between the two countries byshedding a handful of Buganda earth over Busoga, but his own people refused to let him use earth for this purpose. A rebellion of the princes led to Kyabagyu’s death. One of his wives, Nambooze, fought in the ensuing struggle.

She was a performer for the *namatongo* pair of drums, which, as we have seen, is a part of the *amagunjju* ensemble. Kasire (1959, p. 31) describes her as a *nmamitongo*. Nambooze was a good dancer and her beautiful singing is said to have been responsible for her name: *olina emboozu eupoomera, egwe nambooze*, literally, ‘you have conversation that pleases me, you are the queen of conversation’.

Nsimbi (1956, p. 6) muses that Kyabagyu himself could well have spoken. She was the first person ever to have been named thus. She belonged to the Kkobe (yam) clan, the only clan in Buganda that claims to have originated from Bugwe at the extreme north-east corner of Lake Victoria. This tradition, too, directs our attention once more beyond the eastern border of Buganda. Nsimbi (1956, p. 241) tells us that since Kyabagyu’s time, the women of this clanwere exceedingly famous for their beautiful singing and three of them are known on this account at the present time.

That Kyabagyu took a personal interest in music we know from the *entenga* story. This interest is tersely summed up: ‘He stretched (the skins, i.e. created) *entenga* and *entamitu*; it is noteworthy that Entamitu is also called Nkagwe (Lush, 1936, p. 11) and that, like *entenga*, it belonged to Kyagwe.

Kyabagyu also figured in several songs. There is the anecdote that the people made up a song on the flutes which greatly pleased the king, about his baldness. The song, *ow’ekirlataata*, i.e. ‘the bald head,’ is still in the repertoire of several ensembles. When, during the war in Busoga, Kyabagyu’s men grew sick with the slaughter and wished to go home, but Kyabagyu showed no sign of returning, the trumpet ensemble created a song:

If only my brother were round
He would have heeded me.
At Busoga the spears are destroying the people.

If my brother only were with me
He would have allowed me to go.
At Busoga they are cutting leaves.

‘My brother’ refers to the gentle and humane Namugala, Kyabagyu’s brother and predecessor on the throne whom Kyabagyu had forced to abdicate in his favour. When Kyabagyu was murdered, the people sang:

We ourselves warned Kyabagyu
That a king must never in his life cross the Kiira (Nile),
The Kiira even though it is shallow.
No, we warned him.
It should be possible at this stage to draw some conclusions from the rich pattern of traditions that are woven around *entenga* and its supporters. Certainly, first of all, there was a strong external influence at work in the genesis of the chimes, an influence that came from what were Buganda’s eastern borderlands towards the Nile in the eighteenth and nineteenth centuries. Secondly, it appears that the Butiko clan participated in the promotion of *entenga*, partly through the affairs of its Kajugujjwe division in Kyagwwe and partly by bringing its previous experience with the *amaggunju* drums to bear upon *entenga*. The *amaggunju* itself had a moving history: it was employed in the late sixteenth century to cheer the little Mulyondo when he became king while still a child at the death of his father Nyakibinge. This story suggests that the *amaggunju* drums were well established by the time the *entenga* appeared on the scene in the eighteenth century.

These hypotheses do not contradict the explicit mention in the story that the ensemble was a local invention made by ordinary villagers. The claim to invention does not necessarily imply discovery that is spontaneous and independent in every aspect; it is not less valid if a foreign stimulus contributed to the invention initially. The principle of the drum chimes, in this sense, have been borrowed from elsewhere even further east than Kyaggwe. If it was, it would not be the only instance in Kiganda music in which a foreign model became assimilated and yet was spoken of as an indigenous invention—cf. the bowed lute *endingidi* (Wachsmann, 1953, p. 407). Before I speculate on possible external models for the *entenga* from well to the east of the Nile, I would like to bring the *entenga* story up to date.

**Recent Practice**

In discussing the more recent developments it should be kept in mind that the *entenga* is not as vital to the Kabaka’ship as for instance the regalia drums Ttimba and Namanyonyi. Nevertheless, they are, as Kaggwa puts it, ‘Kabaka’s drums’ and they could not be played during the Kabaka’s absence abroad: their place in Kiganda society was exalted indeed. To illustrate this aspect and at the same time report the changes that have come about in the use of *entenga* in the years from 1930 to 1955, I can do no better than quote Kyagambiddwa (1955, p. 114) on these points:

> The *entenga* is the king of Ganda instruments. And no one else throughout the kingdom, but the king alone, could or was ever permitted to possess it. Bishop Joseph Kiwanuka of Masaka is the first man to have secured this royal instrument for public use. He purchased it for the Native Music Department in the Blessed Kizito’s Technical School. The grandeur of the instrument must be experienced.

In former times a man qualified for the loan of the Kabaka’s *entenga* either by enjoying the Kabaka’s special favour or by connections of clan membership that brought him into the orbit of *entenga*. Thus Mr. Mugwanya, one of the regents during Kabaka Daudi Chwa’s minority, had the set lent to him on many occasions because he was also the descendant of Mmunyi, the original owner of *entenga*. This was the position some 30 years ago; Mugwanya died in 1938. In his case membership of the Butiko clan counted for much.

The appearance of *entenga* in September, 1944, in Prime Minister Wamala’s residence just opposite the main gate to the Kabaka’s enclose raised an interesting question. Did the *entenga* play for Wamala because he was much in the Kabaka’s favour at that time? Or was it rather because Wamala felt entitled to have it played for him as a member of the Lugave clan to which Nagamala, one of the original team that played for Mmunyi (Lush, 1936, p. 12) and Mukadde’mwangu, the chief in charge of *entenga*, also belonged? Wamala was known to make much of clan privilege and loyalty (Apter, 1961, p. 214). He was not supposed at that moment to be the favourite, an issue that came to a head a few months later during the riots in January, 1945. Yet there was the *entenga*. Political historians may find its presence with Wamula at that time quite interesting.

The Butiko and Lugave clans are not the only ones that have a special relationship with *entenga*; the members of the Endiga (Sheep) clan also have a hand in it, literally, because they serve as musicians to play the set for the Kabaka (Nsimbi, 1956, p. 295).

**Drum Chimes outside Bugwere**

The recent developments in the *entenga* usage, related in the preceding section, were not the first changes to occur in the history of the chimes. The narrator presented the *entenga* story itself as a dynamic process, and not as a static situation that determined the status of *entenga* once for all. He made his point when he said ‘they added other drums later.’ This remark is helpful in other ways too. It makes it easier to accept, as possibly valid, comparisons with other drum chimes that may be much smaller in range and personnel, and rank lower in society, than *entenga* was and did toward the end of Kyabagu’s reign.

Thus two drum chimes from other Uganda tribes qualify for comparison: one is the *namaddu* of Bugwere (see fig. 5), and the other the set of seven drums on which the Lango play *myel bul*, i.e. the drum songs (fig. 6). The *namaddu* seems to be replacing gradually the Bugwere

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**FIG. 5. NAMADDU DRUM CHIME OF BUGWERE**
xylophone entala, which already incorporates one drum into its 'key board,' and the Lango bul has a counterpart or shadow set in another 'key board' instrument, the percussion reed set bul ryang apena, lit. the drum of reeds. The apena (figs. 7, 8) applies drum names for each pitch and is today used as a practising instrument for boys or young men who wish to learn myel bul. In the apena, too, we find the team work between players that characterizes the bul in fig. 6; each performer employs a pair of beaters, and their reaches overlap at times as in entenga (fig. 9).

![Image of xylophone and drum set](image)

**Fig. 6. The Myel Bul of Lango**

*Above: A plays drums 2 to 6; B plays drum 1 only. Below: one of the alternative arrangements. A single skin drum is added played by C on a hobbyhorse fashion. Note that the large instruments, i.e. the low-pitched sounds, are to the left of the main player.*

The point I wish to make with this comparison is that the three sets, namaddu, myel bul and entenga, have in common an association with xylophone-type instruments. We saw that entenga was tuned and played, and also sounded, like a xylophone; also that it shared with the xylophones and the entamiri drums a special hut in the palace grounds; that myel bul could be substituted for by the apena idiophone, and that namaddu was musically merging with the xylophone entala. It is also noteworthy that, except for the apena, they are set up against a beam or stem in a manner that is reminiscent of log xylophones. The drum sets are all true chimes in the sense that the principal performers manipulate a whole range of pitches—in contrast to those batteries of drums in which each player plays one or two, but rarely more pitches. One might draw the conclusion then that in East Africa the true drum chime belongs to the orbit of the xylophone.

Other elements in entenga music that invite comparison with similar traits in other drums are the thin, curved sticks, and the coupling of the ensemble in pairs of neighbouring pitches.

In Uganda, thin bent sticks, that flay rather than strike the skin, are employed also in one of the six instruments of a seldom seen battery in Busoga that has one player to

![Image of drum set and players](image)

**Fig. 7. Myel Bul Apena of Lango**

*Above: schematic elevation, not to scale. Hoop No. 6 approximately 40 cm. wide. Below: ground plan showing also reach of players A and B. As in myel bul the low-pitched notes are to the left of the players.*
each of its members. Another occurrence is found on the single drum that accompanies the Kisoga harp kimasá, which is practically extinct. But the technique of reviving the tension of the drum sticks by hammering, which Kaggya reports, is unique to entenga.

In Ethiopia the drum orchestra at the Church of Aksum provides further evidence. The only report of these instruments that could be quoted is the photograph in fig. 10, put at my disposal by the photographer, Mr. J. C. Grimes of Oxford. He confirmed that the thin sticks barely visible in the slide were really used like whips on the skins. It is also probable that the drums were set up, and even coupled, as in entenga, but, unlike entenga, they do not seem to be lined up in the deliberate fashion in which the chimes from Uganda are arranged. Furthermore, the drums on the steps of the Church of Aksum are hemispherical kettle drums and in this respect, too, they differ from the other East African chimes.

This scene from Aksum reminds one of the vivid illus-

trations in an Ethiopian MS from before 1730 where similar drums and sticks occur in a series of biblical scenes (Wachsmann, 1961) (fig. 11). What are of especial interest in these pictures are the crozier drum sticks, to use the descriptive term coined I believe by Curt Sachs. Sachs said of the occurrence of this type of sticks that they 'still exist in Tibet, South India, Sumatra and Borneo, while they have disappeared from Europe, Northern India and Persia.' He recognized crozier sticks in reliefs at Bharanhat (third century B.C.) and concluded that 'it probably was

Persia that gave India the hooked stick.' In Israel one can see today Jewish drummers from Kurdistan who use a gently curved thin stick in their native dances.

Sachs connects the crozier sticks with 'twin drums,' that is, instruments played in couples, albeit of a type that has so far not been met in East African chimes. To play drums in pairs is common in Africa, and to couple them permanently by mechanical devices is not limited to the instances

FIG. 9. MYEL BUL APENA OF LANGO
Note overlap of players' reach.

FIG. 10. DRUM ENSEMBLE ON THE STEPS OF A CHURCH AT AKSUM

FIG. 11. CROZIER STICKS AND KETTLE DRUMS
From the Revelation of St. John the Divine, before 1730, MS Brit. Mus. Or. 533

FIG. 12. A DRUM ENSEMBLE FROM NORTHERN INDIA OF THE SEVENTEENTH CENTURY
After Fox-Strangways, 1914, p. 77
mentioned here. In Uganda alone there are several occurrences of drums physically joined (Wachsman 1953, p. 373) and although there is little in the literature on this usage outside Uganda, there is at least one report that the atabal, the kettle drums of the Maurentian Arabs, are joined in this fashion (Salvador-Daniel, 1915, p. 226).

May I conclude with an illustration from India (fig. 12) dating from the seventeenth century, published by Fox-Strangways (Fox-Strangways, 1914, p. 77)? It shows 12 drums set out in six pairs in a straight line, possibly joined physically in couples, and at least two other independent specimens of larger size. Their appearance is very similar to that of entenga, but there are obvious differences. It would indeed be tempting to take up the thread from East Africa and follow it to South-East Asia, where Kirby suggested that it would lead, and end.

Notes
1 This description is based on and amplifies Trowell and Wachsman 1953, p. 370.
3 Lug. koko in traditionally translated 'cancer' (e.g. Le Veux 1917, p. 496).
4 Mr. R. A. Snoxall's translation of this interesting sentence is gratefully acknowledged.
5 In the Kiganda Pantheism Nende is the son of the Lake Spirit, Mukasa (Kaggwa, 1934, p. 222).
6 Mr. Snoxall's translation of this sentence is gratefully acknowledged.
7 The translations of the song texts are those of Kalibah (1934, p. 141).
8 Kyagwge county and Busoga and their relations with Buganda have been mentioned so frequently in this paper that a brief summary of their history should be useful. It is perhaps significant that the first ruler who allegedly appointed a chief to administer Kyagwge was Kabaka Juko (Cox 1930, p. 134)—who reigned during the last quarter of the seventeenth century; the man who was also Nende's client and patron. But the chieftainship is hardly mentioned during the succeeding ten reigns (Cox, 1930, p. 158) and apparently Buganda had little influence over Kyagwge. During the second half of the eighteenth century Kabaka Mawanda is reported to have raided Busoga and to have founded settlements just across the Nile (Gray, 1957, p. 268). With Kyagwge's arrival the picture changes. Stanley's informant in 1875 (?) thought of Kyagwge as the man who invaded Busoga and believed that before this event the Basoga had 'never been reduced to submission by the Waganda.' The dates for Kyagwge's reign are controversial; the earliest is c. 1703-80 (Gray, 1957, p. 227); another estimate would arrive at 1814 as the year of his death (Haddon, 1957, p. 113).
9 Personal communication from M. B. Nsibibi.
10 For another example of curved sticks in association with this type of drum see Amulet MS. British Museum Or. 12859.

A Note on Nayar Marriage. By Dr. Kathleen Gough, Department of Anthropology, University of Oregon.

In his Study of Polynancy, reviewed in this issue of MAN, H.R.H. Prince Peter of Greece and Denmark raises a question of fact concerning Nayar marriage, to which I should like to contribute an answer. Prince Peter notes that some writers have reported fraternal polynadry for the Nayar, whereas, he notes, I have argued that in Central Kerala the fraternal form of polynadry was forbidden (op. cit., pp. 92f., 499). Most of the seeming contradictions in the literary accounts are, I think, because the Nayar had different forms of marriage in different regions and periods. In what follows I shall write in turn of Nayar marriage in North Kerala, Central Kerala and Travancore. Insofar as I know of them from my fieldwork and reading, the facts are as follows.

North Kerala

Nayar living north of the Kora River in North Malabar District (the former kingdoms of Kolattunad, Kottayam and Kadattunad) have lived in avunculocal extended families at least since the beginning of British rule in 1792, and probably for a considerable time previously. This mode of residence was described by Buchanan in 1807 as being traditional and customary (F. Buchanan, A Journey to Madras through Mysore, Canara and Malabar, London, 1807, Vol. II, p. 513). Avunculocal residence seems to me to have been consistent with the fact that Nayar in this area were not full-time warriors but for the most part cultivated their own estates, which were held jointly by the matri-
lineage segment. In this century the extended family has tended to break up with the coming of individually owned cash-crop farms.

North Malabar Nayar were traditionally optionally monogamous or polygynous, but I could find no evidence that they were ever polyandrous. A Nayar girl's tali was tied before puberty, in most areas by a Nambudiri Brahman, in a ceremony which conferred no sexual rights. Shortly before or after puberty the girl received a cloth from a bridegroom, usually of her own subcaste, in a ceremony which marked the beginning of her sambandham or marriage union. Unless the bridegroom was of higher caste, she then went to live in her husband's household until his death—or, in rare cases, until divorce. As early as 1792, a British government commission reported that 'although the custom of the Brahman being often the parents of the Nayrs, seems to have always equally obtained over every part of the country; yet, in the manners of the Nayar women in general, there is a considerable difference between the southern and the northern parts of the province of Malabar, ... inasmuch that although, in the former or Zamorin districts, they indulge for the most part in a free system of polyandry without marriage, yet those in the northern districts are said to be attached and even limited to one male connection at a time; which difference and greater strictness in this country, may perhaps serve to account for the report, furnished by the Chirucal Raja, comprehending a clause that "if a man detect another in the night-time in an apartment with his wife or mistress, he is permitted to kill him, and cut off her hair and repudiate her"' (Report of a Joint Commission from Bengal and Bombay, appointed to inspect into the State and Condition of the Province of Malabar, in the years 1792 and 1793. Reprinted Madras, 1862, pp. 233f.).

My informants confirmed that the cutting-off of an adulterous woman's hair, followed by divorce, had been a recognized procedure within living memory. Far from engaging in the fraternal variety of polyandry, moreover, a Nayar woman is forbidden to marry a man from the same matrilineal property group as that of her former husband, either after divorce or after widowhood. Nayars in North Malabar pay extremely formal deference to the wives of their elder brothers, and were traditionally required to avoid their younger brothers' wives altogether.

Regarding privileged access to a woman as distinct from marriage, I should however mention that pre-marital sex relations between own or classificatory cross cousins, both matrilateral and patrilateral, appear to be rather common, and that several Nayars told me that they were not traditionally frowned upon. Such relations sometimes precede an arranged marriage between cross cousins, bilateral cross-cousin marriage being a preferred form. If, moreover, a married woman has sex relations with a cross cousin who is not her husband (nor, of course, her husband's brother or close classificatory brother), I was told that the husband is more likely to ignore it than if the lover is unrelated to the woman, and that he is unlikely to make it the grounds for a divorce. Such relations are, however, quite distinct from marriage, for they are clandestine and give the cross-cousin lover no customary rights in his mistress and no rights in nor obligations to her children. It should also be noted that the cross-cousin relationship, which is one of informality and joking, completely changes to one of marked respect on both sides when a woman's cross cousins become her husband's brothers. I should perhaps note, finally, that in Kottayam and Chirakkal taluks at all events (where I did fieldwork), privileged relations between cross cousins also had no connexion with the now extinct pre-puberty tali rite, which in this region, as I have noted, was performed by a Nambudiri Brahman and conferred no sexual rights. Whatever may have been the state of affairs in the remote past in North Malabar, I think that we must conclude that there is no trace of polyandry proper, either in the literature or at the present time.

Central Kerala

The Nayar of Central Kerala (South Malabar and Cochin, including the old kingdoms of Calicut, Valluvanad, Palghat, Cochin and as far south as the principalities of Parur and Alanganad) traditionally lived in matrilineage-segment households, had the institution of the visiting husband, and freely permitted plural sambandham unions for both men and women until well into the period of British rule. These arrangements appear to have been consistent with Nayar men's traditional occupation as soldiers, and with the fact that their matrilineally owned estates were cultivated by tenants and serfs. Even after the Nayar armies were disbanded by the British, the avunculocal extended family never became a norm in Central Kerala. Large numbers of Nayars moved into urban wage work or salaried occupations; others remained 'simply sitting' on matrilinearly-owned joint estates which were managed by the eldest man of the matrilineal property group. In these circumstances most Nayars continued to visit the wife in her natal taluk in the early years of marriage, but, when possible, to provide a separate house for the wife and her descendants in middle age. During the nineteenth century polyandry gradually declined, and was quite rare by the end of the century.

It is to this area that most of the well-known European travellers' reports of Nayar polyandry referred. I have described the institutions of Nayar of this region in previous publications (Gough, J. R. Anthrop. Inst., Vol. LXXII, Pt. I, 1952, and Vol. LXXXIX, Pt. I, 1959; also in Matrimonial Kinship, edited by David M. Schneider and K. Gough, Berkeley, 1961, pp. 298-384). Although I have never met a Nayar woman whom I have definitely known to be polyandrous, I heard, from Nayar, of several cases of non-fraternal polyandry in recent times both from Pallivanad and from the Tiruchu taluk of Cochin. In spite of the prevalence of plural unions, especially with members of the affinally linked enangal lineages of a woman's own local community, fraternal polyandry is, however, said to have been even traditionally forbidden among Nayars of this region, and has remained so till the present time. This is the case even though the Tiyyar and other lower castes of this area have practised fraternal polyandry freely until recently. As in North Malabar, a Nayar woman in Central Kerala is prohibited from having sex relations with two men of the same lineage segment from marrying a brother or close classificatory brother of a former husband. Nayar also pointed out to me that when a man goes to perform the 'cloth-giving' ceremony which starts his sambandham with a woman, he is accompanied by male cross cousins and age mates but not by his brothers or other matrilineal kinsfolk. The reason given was that marriage is an occasion of levity and informality, but matrilineal kinship is a serious business, and the two ought not to be mixed. This rule is observed in most families even today, when the ceremony and feast which mark the start of a sambandham have become much more lavish than in former times and the marriage tie monogamous and, usually, of long duration.

Travancore

I have a scantier personal knowledge of Nayar marriage in Travancore. On a visit in 1948 to the village of Cheppad, near Haripad in the taluk of Kollam in Central Travancore, however, I heard from members of a Nayar family with whom I stayed that before British rule the Nayar of this area, who were soldiers, lived in matrilineage segments and had visiting husbands as in Central Kerala. This is confirmed for Travancore generally
by Panikkar (K. M. Panikkar, ‘Some Aspects of Nayar Life,’ J. R. Anthrop. Inst., Vol. XLVIII, 1918). One informant of Cheppad, an educated man of about 35, also told me privately that the traditional form of residence in lineage segments had been accompanied by combined non-fraternal polyandry and polygyny, exactly as in Central Kerala, and that these customs persisted in some households until about eighty years ago.

Possible confirmations of this statement are contained in two early reports of Nayar unions from the old principality called variously Batayma, Matayma, Vennam and Kartigapally, in which Cheppad formerly lay. The Dutch Captain Nieuhoff, writing in 1664 of ‘the kingdom of Batayma with its city called Ktyapery’ (Kartigapally), observed: ‘It is commonly reported in these parts, that the kings of Matayma made a law, by which a man was empowered to kill any woman who refused him a kindness.’ Almost a hundred years earlier, the Portuguese Archbishop Menezes referred to such a law as existing in the same locality: ‘By this law the king of Kartigapally enacted that in his kingdom any Nayar was at liberty to be free with any woman of the caste high or low, and that he could kill a woman, with impunity, if she refuses him a favour’ (quoted in a Memorandum by K. P. Padmanabha Menon, Enclosure B of the Report of the Marumakkathayam Committee of Travancore, Government Publications, Trivandrum, 1908, viii). The supposed law is comparable to that mentioned for Calicut by Barbosa in 1510. Writing of the plural unions of Nayar women, he noted that ‘It is said that the kings made this law, in order that the Nayar should not be covetous and should not abandon the king’s service’ (The Book of Duarte Barbosa, Hakluyt Society 1921, p. 124).

I was told by several people that during the early nineteenth century, most Nayar round Haripad became cultivators rather than soldiers, and that the more prosperous ‘middle-class’ Nayar gradually shifted toward residence in avunculocal extended families, priding themselves on their ability to maintain the wives and children of their male members as well as, when necessary, to uphold their old obligations to the women and children of the matrilineage. Gradually, indeed, it came to be thought shameful for men to ‘creep in’ to the taravad’s of their wives. This statement, too, is confirmed by Panikkar for the Nayar of Central and South Travancore generally. My more confidential informant also explained that as women began to come to live in their husband’s taravad, non-fraternal polyandry was abandoned, but some households began to permit younger brothers to share the sexual favours of a wife brought home by the eldest of them. There had been some cases of this in Cheppad within the memory of my informant, and others, he said, still existed a few miles to the east. With the modern growth of independent earnings and the spread of pan-Indian and Western values, however, most people now looked down on wife-sharing and denied that polyandry had ever existed in any form.

These statements are supported by the Revd. S. Mateer’s report from Travancore that he had known of six Nayar brothers keeping a single wife (quoted by L. K. A. K. Iyer, MAN, 1932, 320). They also cast light on Panikkar’s position regarding Nayar marriage. Panikkar, writing of Travancore Nayar in 1918, opined that they had never had either fraternal or non-fraternal polyandry, but noted that Nayar women in Travancore behaved freely with their husbands’ brothers, for example by rubbing oil on their backs before they bathed—a service normally restricted, in Kerala, to the wife. Panikkar also admitted that men of the enangar or affinally linked taravad of a woman’s neighbourhood had the right to enter her room and to enjoy a familiarity with her akin to marriage, and that in rural areas it was possible that enangar might have sex relations with a woman even after she was married and might be called ‘little father’ by her children. Although he denied polyandry, Panikkar called this ‘a system of supplementary spouses.’ Judging from the above reports, Panikkar seems to me to have been describing a situation in which, in the circles in which he was familiar, Travancore marriage had become monogamous, but in which there were still traces of the fraternal wife-sharing referred to by Mateer and by my own informant; and also, traces of the still earlier state of affairs in which women lived in their own taravad, and, following the pre-puberty tali rite, could be visited by any number of sambandham husbands drawn from the enangar or local neighbourhood group. Incidentally, Panikkar’s reference to the residual sexual rights of enangar—who, as he notes, are also often a woman’s cross-cousins—reminds me so strongly of the behaviour of cross-cousins in North Malabar that I wonder whether there, too, this behaviour might not possibly be a relic of non-fraternal polyandry, now long since died out. In the absence of literary accounts this must remain merely speculative.

Comparing the accounts given above for Travancore and for Central Kerala, we can also see why Aiyanappan vehemently combatted Panikkar’s view and insisted that the Nayar did quite definitely have non-fraternal polyandry as a regular institution but that Nayar adelpbic polyandry was rare (Aiyanappan, MAN, 1932, 99). For Panikkar was born in Travancore, Aiyanappan in Walluvanad, and each wrote chiefly of family customs in the region of his birth.

The sharing of a wife by Nayar brothers in Travancore does not seem to me to have been true polyandry of the type found among the Tiyyar of Central Kerala, who traditionally had patrilineal extended families coupled with dispersed exogamous matrilineal clans. Among Tiyyar, as Prince Peter has noted, at the ceremony which marked the start of the sambandham or marriage union, all the brothers customarily lined up and drank coconut milk with their common bride in the presence of their respective families and guests (op. cit., p. 219). The wife became the legal wife of all of them and all were equally the legal fathers of her children. Again, among Nayar of Central Kerala in the period of non-fraternal polyandry, it was customary for each separate sambandham husband to present his wife with a marriage cloth at the start of his union with her, in the presence of her family. When the woman delivered a child, the husband or husbands who were currently visiting her were required to make gifts to the midwife in recognition of their possible paternity and in order to validate the child’s status as a legitimate member of his mother’s caste. It is primarily because of the male partners’ role in validating the legitimacy of the woman’s children that I regard the traditional Central Kerala Nayar sambandham as marriage rather than as ciscisbeism, and Nayar women’s plural unions as polyandry of the non-fraternal type.

Among Central Travancore Nayar in the period in which brothers shared a wife, however, I do not think that all the brothers participated in the ceremony which initiated the marriage, or that all were considered equally the legal fathers of the woman’s children. Unfortunately I did not enquire deeply into this. My informant told me, however, that after it became customary for men to bring their wives to their own taravad, it often happened that ‘the eldest brother in a Nayar family would go and marry a woman, bring her to his own house, and then share her with his younger brothers.’ This suggests that only the eldest brother performed the cloth-giving ceremony at the woman’s house and was regarded as her legal husband, and that his younger brothers merely had privileged access to his wife. My impression was that the younger brothers might later bring home wives of their own, if their elders were willing to undertake the maintenance of additional women and their children. There were, perhaps, several possible kinds of arrangement, for
Mateer reported to Fawcett, in 1891, 'an instance of six brothers keeping two women, four husbands to one, and two to the other.' Panikkar refers specifically to a woman's familiarity with her husband's brothers who are older than herself. Given the fact that Nayar men are always older than their wives, often by several years, this suggests that some men may have shared access both to their younger brothers' wives and also to those of their older brothers' wives who were younger than themselves. Incidentally, I myself noticed that in the family which I visited in Cheppad, the younger brothers, both married and unmarried, had very informal, jocular relationship with their elder brother's wife, and also that the elder brother spoke freely to his younger brother's wife and made no effort to avoid her. All this contrasted markedly with the extreme respect toward the brothers' wives which I was later to encounter in North Malabar. At the same time, the relations between the brothers in Travancore were respectful and asymmetrical, with the younger brothers declining to sit or smoke in their elders' presence. They were, however, not as distant and avoidant as in North Malabar, nor, for that matter, in Cochin.

My interpretation of Travancore wife-sharing as something less than true polyandry is supported by a statement in the Report of the Travancore Marumakkathayam Commission of 1908. After commenting that the President of the Malabar Marriage Commission of 1894 had 'found polyandry to linger in some parts of Malabar District,' the author states: 'If polyandry means the marriage of one female by more than one male, we may say positively that it does not exist anywhere in Travancore.' He continues, however, 'There may be stray cases in which a brother's wife is treated as the wife of her brothers-in-law' (p. 21). Travancore fraternal wife-sharing was probably, therefore, a form of sporadic ciscisbeism, although it appears to have grown out of an earlier institution of true, non-fraternal, polyandry.

I must admit, finally, that the data which I have presented invalidate an explanation that I offered earlier of the prohibition of fraternal polyandry among Central Kerala Nayar. I wrote in 1952 that 'the marriage of a woman to two brothers ... would, by making them co-husbands, ... gravely disrupt the asymmetrical relation between two men of the same lineage, which is stereotyped to the point of avoidance on the basis of rank by age.' (Gough, 'A Comparison of Incest Prohibitions and Rules of Exogamy in Three Matrilinical Groups of the Malabar Coast,' *International Archives of Ethnography*, Vol. XLVI, No. 1). This was the 'explanation' actually given to me by Central Kerala Nayar. Obviously, however, it does not apply to Travancore Nayar who, in developing avunculocal extended families, also permitted brothers to share a wife—apparently without destroying the respectful relationship between them. I am unable to say why, in the nineteenth century, Travancore Nayar began to permit a form of wife-sharing which was prohibited both by Central Kerala Nayar (who had polyandry) and by North Malabar Nayar (who had avunculocal extended families). My guess is that wife-sharing by brothers depended on two conditions: one, the immediately prior existence of some other kind of wife-sharing, and two, the change to avunculocality. Central Kerala Nayar never made the latter change. North Malabar Nayar had, perhaps, already given up polyandry before they changed to avunculocality, or else had never had it at all. But too little is known of traditional institutions in any of these areas for such constructions to be more than speculations.

**Notes**

1 In 1959 I called the Central Kerala Nayar combination of non-fraternal polyandry with polygyny 'group marriage.' I did so because my understanding is that all men of the neighbourhood group or enangy might have access to all of the women who were not their matrilineal kin, were not older than themselves, and did not already stand to them as spouse's matrilinealkinswomen or as matrilineal kinsmen's spouses. Further, I thought that the tali rite in this area freed, or indeed obligated, the woman to enter into all of these plural unions with the men of her neighbourhood. Professor Dumont has queried this interpretation, suggesting that instead we regard the tali rite as a woman's primary marriage and the sambandham unions as secondary ones (L'Homme, Vol. I, Part 1, 1960). I still think that the tali rite in this region made the woman the potential mate of all of her enangy—and indeed, of her subcaste and of all higher subcastes as well—and that therefore Nayar marriage could legitimately be regarded as a kind of group marriage, at least within the enangy group of equal and affinally related taravads. Nevertheless, I must not lose sight of the fact that each sambandham was separately initiated, at least in theory, by a ceremonial cloth-giving by the husband, and that each husband had to make gifts to his wife at the festivals and to help pay her delivery expenses as long as he continued to visit her. This means that at any given time the number of actual as distinct from potential husbands a woman had was limited—indeed, we are told in the literature that it was usually four or five, although it might be as many as twelve. I think, therefore, that Dumont's suggestion of primary and secondary marriages to describe the tali rite and the sambandham is probably the most appropriate way to view this system and that it does help very much to place Nayar unions in a more general Indian context. Dumont's other points, and also those of Yalman (J. R. *Anthrop. Inst.*, Vol. XCIII, Pt. I, 1963, pp. 25–38), require a more extended treatment elsewhere.

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**A Preliminary Survey of the Beads of Ingombe Ilede, Northern Rhodesia. By A. P. du Toit, University of the North, Sovenga, South Africa. With three figures**

**Introduction**

Ingombe Ilede, a village site situated in the Lower River Region of the Gwembe Valley, Northern Rhodesia, has recently been described in a preliminary report by Fagan (1963a, p. 13) and proves to be an important link in the study of the Iron Age cultures of Southern Africa. It was investigated as part of a large-scale research project into the Iron Age cultures of this area carried out by the Rhodes-Livingstone Museum, and supported by the Nuffield Foundation. The beads recovered from the site were sent to me for analysis.

Historical records are rather vague about the movements of early traders and their trade routes through this part of Africa but scientific investigation lately enables us to form a more complete picture of the past. Although the Ingombe Ilede and other Iron Age settlements that are now investigated were not founded at exactly the same time, it becomes clear that a lot of them figured in a complex where extensive trading was carried on during the early centuries of our Christian Era. Ingombe was first occupied in the middle of the seventh century A.D. and was almost contemporary with such sites as Isamu Pati, Kalundu, Zimbabwe and Mapungubwe. An attempt will therefore be made to correlate the material of the Ingombe with these sites.

The variety and abundance of the Ingombe beads is such that in my opinion they can be used as a yardstick by which similar material from other sites can be judged. With the wide range at our disposal it is possible within certain limits to classify them according to periods of distribution and to establish a valuable chronological scale that can be used when considering other Northern Rhodesian Iron Age sites. Beads recovered from the ten graves numbered 1 to 10 at Ingombe Ilede during the 1960 excavations can be classified into a range of types that were introduced during the occupational period. The typology of the
beads was ascertained through microscopic investigation and four
distinct series can be differentiated according to glass texture,
method of manufacture and form.
Before going into detail it must be pointed out that some of
the beads are encrusted while others are heavily corroded, an
occurrence which sometimes can be rather misleading.
In this collection we have only a few examples of encrusted beads,
mostly from Burial II/25. Through contact with certain types of
soil and dampness a thin crust of chalky substance has formed on
their outer surfaces without causing any pronounced damage to
the glass. It can easily be removed with the aid of dilute acid. It
has been found that beads of barely a hundred years have en-
crusted to such an extent that they convey an impression of old
age whilst in fact they hardly show any signs of corrosion. In
the case of the corroded material, however, a reaction has set in while
they were embedded in soils of a particular composition. This
only applies to the older bead types that were covered up for a
considerable time. Although it is assumed that corrosion does not
necessarily imply old age, it can be said that badly fused glass
through its porosity is more susceptible to weathering than
thoroughly fired glass. This can be clearly seen in cases where
both old and more modern beads have been subjected to the
same climatic conditions.
It is generally accepted that ancient bead factories were con-
fronted with many difficulties in the process of glass-melting.
With unsuitable furnaces and inadequate equipment it was
impossible to create sufficient heat for manufacturing purposes.
In the making of glass beads the raw materials used were never
obtained in pure form and impurities such as aluminium, silver,
nickel, etc., were usually present where the material was extracted
from the earth. Under low temperatures some of these elements,
which have a high melting point, did not fuse at all and caused
the glass substance to become a brittle mass. In fact the heat was
even insufficient to melt the glass itself completely so that a
porous material resulted. An improvement in firing and the
choice of raw material, which can partly be detected through
microscopic observation, gradually took place after the eighth
century A.D.

Microscopic Analysis
It is noticeable that Indian red beads were found in large
quantities and were present in all the graves except in Burial 10.
It is likely, therefore, that Indian reds of various ages from quite
different series are present in this collection. A survey of all the
Indian red beads in the collection revealed the following:

(a) Short irregular cylinders, very much corroded and fragile,
were dissected and found to contain grains of deep red glass mingled
with a brittle semi-molten mass of opaque material. It is evident
that these beads were fired at a very low temperature, as they
contain impurities that are not present to the same extent in glass
of later manufacture. They were, in some cases, probably made
from remelted glass. In view of these criteria all beads belonging
to this class are labelled 'Series 3,' and considered older than all
other types. Other beads in Series 1 are turquoise, green, yellow,
and large green wound forms.

(b) Indian red beads identical to the preceding class in form and
size can be distinguished by microscopic observations. They are
less corroded than their prototypes in Series 1. The glass from which
they were produced contains less impurities but is fairly brittle and,
although fired at a higher temperature, the matrix is badly fused.
These beads are termed 'Series 2' and consist not only of Indian
reds, but also turquoise, small green, yellow and large green wound
beads.

(c) All examples of the next series show a different texture and
composition. The glass was subjected to a still higher temperature
but not enough to obtain the same result as can be seen in later
types. Flaws can be detected where layers in the glass are not com-
pletely fused, and on dissection the beads show an uneven cross-
section. This is Series 3, containing, in addition to red specimens,
turquoise, small green, yellow and large green wound beads.

(d) This is the last series that can be readily identified under a
microscope. The glass was melted under optimum conditions with
sufficient heat and purer constituents. The sectional surfaces of
broken beads are always even and the glass has almost the same
texture found in nineteenth-century glass. These are classed as
Series 4, and include Indian red, turquoise, small green, yellow and
large green wound beads. As far as I could ascertain, all Indian red
beads of Series 1 and 2 and are cylindrical in form with sharp edges.
They vary in diameter from 6 mm. to 2-7 mm. The later series show
a tendency towards smaller forms reaching a diameter of 1 mm. in Series 4, an indication of better craftsmanship and tech-
nical skill. The edges are smooth and round, a change in technique
when compared with the beads of Series 1 and 2.

The seed beads (?) cannot be readily classified and, on account
of their state of preservation, are provisionally placed under
Series 3 and 4. The cornelian present the same difficulty and may
be ancestral beads. Because they are so few in number they are
probably not later than series 1 or 2. The shell beads are not all
of the same class but cannot be identified according to age. They
might have been in existence throughout the whole period of
occupation.

The beads from the central burials can be classified on this
basis as in Table I.

<table>
<thead>
<tr>
<th>TABLE I. BEADS FROM CENTRAL BURIALS</th>
<th>COLOUR</th>
<th>DIAMETER</th>
<th>LOCALITY</th>
<th>SERIES</th>
</tr>
</thead>
<tbody>
<tr>
<td>BURIAL 1</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(a) Indian red</td>
<td>Turquoise</td>
<td>3·4 to 1</td>
<td>Waist</td>
<td>2 3 4</td>
</tr>
<tr>
<td>(b) Indian red</td>
<td>Yellow</td>
<td>3·4</td>
<td>Waist</td>
<td>1</td>
</tr>
<tr>
<td>(c) Turquoise</td>
<td>Green</td>
<td>(corroded)</td>
<td>3·2</td>
<td>Neck</td>
</tr>
<tr>
<td>(d) Camelian</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(e) Turquoise</td>
<td>8·3</td>
<td>(irregular discs)</td>
<td>4·5 to 2·6</td>
<td>Waist</td>
</tr>
<tr>
<td>(f) Royal blue</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

BURIAL 2

(a) Royal blue                       | Turquoise | 4·5 to 2·6 | Neck, left of pelvis, legs, general | 4  |
(b) Turquoise                        | (irregular discs) | 7 | Neck, general | 3 4  |
(c) Indian red                       | 3·1 to 1 | Waist, general | 4  |
(d) Indian red                       | 3·4 to 2 | Waist, general | 1 2 3  |
(e) Indian red                       | 4·3 to 3 | Neck, left of pelvis, legs | 3  |
(f) Yellow                           | 6 to 3   | Neck, general | 3 4  |
(g) Shell                            | 4·6      |          |          | Uncertain |

BURIAL 3

(a) Indian red                       | 3 to 2·2 | Pelvis, neck | 3 4  |
(b) Seed beads                       | Turquoise | 4·5 to 3 | Pelvis, neck | 1 3 4  |
(c) Turquoise                       | 4·5 to 3 | Pelvis, neck | 4  |
(d) Green                           | 6 to 2·2 | Pelvis, waist, neck | 2 3 4  |
(e) Indian red                       | 6 to 2·2 | Pelvis, waist, neck | 4  |
(f) Yellow                           | 3·4      | Neck, pelvis | 1 2  |
(g) Gold beads                       | 3 to 2·2 | Pelvis, neck | 3 4  |
(h) Royal blue                       | Turquoise | (irregular discs) | Neck, mixed | 3 4  |

12
Some of the burials found at the southern border of the site yielded small strings of beads which are classified in Table II. These beads belong to the same periods as and are of similar size, colour and manufacture to those described in Table I.

The beads recovered from the occupation levels tell quite a different story and provide us with a problem which makes dating very difficult. They can be classified as in Table III.

These beads appear to be younger than those from the burials and to be of quite a different type, except for the specimens labelled CC. B3. 1 and CC. B3. 4, which show a slight similarity in colour and form. It seems that there is an anomaly between the two series.

Table II. Beads from southern burials

<table>
<thead>
<tr>
<th>Colour</th>
<th>Locality</th>
<th>Series</th>
</tr>
</thead>
<tbody>
<tr>
<td>BURIAL II/19</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yellow (corroded)</td>
<td>Neck</td>
<td>1 2</td>
</tr>
<tr>
<td>Yellow</td>
<td>Neck</td>
<td>4</td>
</tr>
<tr>
<td>BURIAL II/23</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yellow (corroded)</td>
<td>Waist</td>
<td>1</td>
</tr>
<tr>
<td>Yellow</td>
<td>Waist</td>
<td>3</td>
</tr>
<tr>
<td>BURIAL II/25</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Green</td>
<td>Waist</td>
<td>2</td>
</tr>
<tr>
<td>Green</td>
<td>Waist</td>
<td>3</td>
</tr>
<tr>
<td>Dark green (opaque)</td>
<td>Waist</td>
<td>1 4</td>
</tr>
<tr>
<td>Dark green (opaque)</td>
<td>Waist</td>
<td>1 4</td>
</tr>
<tr>
<td>Yellow (corroded)</td>
<td>Waist</td>
<td>1</td>
</tr>
<tr>
<td>Yellow</td>
<td>Waist</td>
<td>4</td>
</tr>
<tr>
<td>Yellow</td>
<td>Neck</td>
<td>2</td>
</tr>
<tr>
<td>Indian red</td>
<td>neck</td>
<td>3</td>
</tr>
<tr>
<td>Indian red</td>
<td>Neck</td>
<td>3</td>
</tr>
</tbody>
</table>

Table III. Beads from the occupation levels

<table>
<thead>
<tr>
<th>Colour</th>
<th>Reference</th>
<th>Series</th>
</tr>
</thead>
<tbody>
<tr>
<td>LEVEL 1</td>
<td>CC.B3. 1</td>
<td>4</td>
</tr>
<tr>
<td>Blue</td>
<td>CC.B3. 2</td>
<td>3</td>
</tr>
<tr>
<td>LEVEL 2</td>
<td>CC.B3. 3</td>
<td>4</td>
</tr>
<tr>
<td>Red</td>
<td>BC.A3. 3</td>
<td>3</td>
</tr>
<tr>
<td>LEVEL 3</td>
<td>CC.B3. 4</td>
<td>3</td>
</tr>
<tr>
<td>Green</td>
<td>BC.A3. 6</td>
<td>3</td>
</tr>
<tr>
<td>LEVEL 4</td>
<td>CC.B3. 6</td>
<td>3</td>
</tr>
<tr>
<td>Yellow</td>
<td>CC.B3. 6</td>
<td>3</td>
</tr>
<tr>
<td>Blue</td>
<td>CC.B3. 6</td>
<td>3</td>
</tr>
<tr>
<td>LEVEL 5</td>
<td>CC.B3. 6</td>
<td>3</td>
</tr>
<tr>
<td>Nil</td>
<td>CC.B3. 6</td>
<td>3</td>
</tr>
</tbody>
</table>

Note: The levels of the different trenches have been correlated according to surveyed measurements (Fagan, personal communication).

Conclusion

Before evaluating the beads of Ingombe Ilede we may mention the radiocarbon dates from the site to see how far the information detailed above can be correlated with them. The following data are available (Fagan, 1963b):

<table>
<thead>
<tr>
<th>Ref. No.</th>
<th>Inches below surface</th>
<th>Date A.D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>SR—22</td>
<td>30</td>
<td>850±200</td>
</tr>
<tr>
<td>C—720</td>
<td>30</td>
<td>930±100</td>
</tr>
<tr>
<td>SR—23</td>
<td>36</td>
<td>800±100</td>
</tr>
<tr>
<td>R—908</td>
<td>90</td>
<td>680±40</td>
</tr>
</tbody>
</table>

As the first bead (PC. B3. 6) occurred in the 72-inch level we must assume an a quo of A.D. ± 750. The settlement was abandoned, according to Fagan, during the ninth century.

Microscopic analysis shows that the beads of Ingombe Ilede belonging to Series 4, viz. Indian red, turquoise (small barrel-shaped and irregular discs), large green (wound and drawn), small yellow and green, royal blue and dark purple, correspond in texture to the multiple wound beads of Burial 10 (a). This bead type is similar in technique of manufacture, perforation, size and
form to those named 'Trade-Wind Beads' by van der Sleen (van der Sleen, 1938, p. 210). In view of the similarities mentioned and the type of glass used (fig. 1), I must admit that they show all the qualities usually attributed to glass of the period of A.D. 1400 to 1660. It is even harder to assign a greater age to minute Indian red beads (4c., 1 mm. diameter) of burials 1(a) and 6 which belong to the same series. This relative dating is substantiated by the larger variety in colour and form of beads which were fashionable at the end of the occupation of the Ingombe Ilede.

The fixing of an a quo presents less difficulty because a more reliable date can be established for the older beads found in Southern Africa with which a comparison can be made. The green wound beads of Burial 10(e) (see fig. 3) belonging to our Series 1 are the oldest in this collection and can be associated with the heavily corroded Indian red, small green, yellow (fig. 2) and turquoise, and carnelians which most probably formed the main range of imports during the earlier period. If we compare the glass types of the earlier and later beads of Ingombe Ilede there is little doubt that the process of manufacture had undergone a radical change and improvement. This can only have been attained over a period of many centuries. In my opinion an a quo for the beads in question can be considered as early as the first couple of centuries of the Christian Era or even earlier. On historical grounds, therefore, we can assume that all the series in question are of Arabian or Indian origin.

The data given in Table I show that the more common forms were Indian red, yellow, small green and turquoise beads; later, they were joined by the royal blue form. These types were worn around the neck, elbows, waist and legs. The larger beads, such as the wound green form, were only used as necklaces, whilst the turquoise discs and carnelian beads were worn at the neck and waist as well.

The beads associated with the burials show that definite bead fashions prevailed, since the range of colours and types varies but a little. Older types appear to have been replaced by identical forms.

During the periods of Series 3 and 4 the only innovations in fashion that were brought into circulation were Indian red beads of minute size (Burials 1(a) and 6(a)), royal blues (Burials 2(a), 3(h) and 9(a)), turquoise irregular cylinders (Burial 1(c)) and turquoise discs (Burials 1(c), 2(b), 3(j) and 8(h)). The beads in this collection that, to my mind, were treasured as ancestral and

probably considered as having supernatural qualities are the wound green beads found with Burial 10(e). In fact, this burial is of significance because it is the only one where these particular beads were found. They were found only around the neck where the chances of breakage or loss are the least and where lucky charms and amulets are usually worn. It can be said that the older types of beads were almost invariably found around necks and not on other parts of the body. Preference was also given to the wound beads recovered from Burials 4(a) and 10(a, b).

Green wound beads of identical manufacture belonging to Series 2 found their way to Isamu Pati on the Batoka plateau of Northern Rhodesia and south to Zimbabwe (Caton-Thompson, 1931, frontispiece, 50) and are dated from the tenth to the eleventh centuries. If the presumption that they all belong to the same period is correct then the wound beads of Burial 10(e), classed as Series 1, are older than the tenth century A.D.

As only a few beads were found in the occupation levels of the site as opposed to the graves, it is clear that these elements of culture were not nearly so common in everyday use as they
were on the persons of the individuals buried in the centre of the Ingombe. It must be observed, however, that the bead at the 72-inch level (PC.B3, 6) and the few found immediately above it appear to be made of similar glass to that used during the period A.D. 1400 to 1660 in India. With the exception of two doubtful specimens they are of quite a different type from those associated with the rich burials.

From my preliminary investigation of the beads of Ingombe Ilede I am led to deduce that the trade goods in the form of beads which reached this site can be classified as belonging to four periods, viz. Series 1, 2, 3 and 4. All of them appear to be of Indian origin and in my opinion were probably brought to the Ingombe Ilede between the ninth and, at the outside, the seventeenth centuries. The terminus ad quem established here is clearly contradictory to the date fixed by carbon tests. If our C14 dates are correct the only solution to the problem, in my opinion, is the possibility that the beads of Series 3 and 4 might have come not from India but from another source where the technique of bead-manufacture attained an exceptionally high standard as early as the tenth century. It may be added that the manufacture of glass started in Venice about A.D. 420 (Angus-Butterworth, 1948) and was developed into a fine art before the twelfth century. Unfortunately I have as yet no reference specimens from these factories to which a comparison can be made. I believe that if an early trade in Venetian beads could be proved in the future it would entirely change our present theories on the bead chronology of Southern Africa.

Notes

1. The term ‘series’ in this report has a different meaning from that used by Schofield in connexion with beads.

2. The radiocarbon dates for Zimbabwe, phases 2 and 3, are:
   - Phase 2, A.D. 1085 ± 150 (M—914); Phase 3 (late), A.D. 1430 ± 150 (M—915).

3. There is very little difference in the typology of beads between the fifteenth and seventeenth centuries. The specimens of Series 4 can therefore fall between A.D. 1400 and 1660, or the youngest beads may possibly belong to a Series 5.

References


Caton-Thompson, G., Zimbabwe Culture, Oxford, 1931.


—— (1963b), ‘Northern Rhodesia Iron Age Radiocarbon Data,’ Circular 708A/IV/115.


The Presence of the ‘Withdrawn’ High God in North Ibo

Religious Belief and Worship. By Dr. Austin J. Shelton, University of Nigeria, Nsukka, Eastern Nigeria. With five figures

In a recent issue of MAN I presented a paper titled ‘On Recent Interpretations of Deus Otiosus: The “Withdrawn” God in West African Psychology’ in which I argued that the ‘lesser gods’ or spirits loom large psychologically and receive a greater amount of sacrificial offerings in general, although the offerings are made through the lesser spirits created by the High God. In the present paper I intend to begin the exposition of the worship of the High God among the Ibo peoples of Nsukka Division, Eastern Nigeria, primarily in the villages of Aror-Unu, Ohebe-Oba, Owerrisi-Ezeoba, Umugogo, and Umunne-Gwa, which are all more or less the same distance (2–8 miles) in different directions from the town of Nsukka itself. In further studies I will present in detail the worship of the High God through the differing forms of alusi (spirit) worship in these and other Ibo villages.

The Pertinent Social Structure

In the villages from which the following material has been collected, certain social structures differ from those farther south in Iboland, and should be explained briefly. Unlike the position of many Ibo communities, in these areas the village is usually the major social unit, and clans fall within villages, rather than vice versa. For example, Umunne-Gwa village of the Owele comprises the two clans of Umuniyoke and Umuramu; and the village of Ohebe-Oba has five clans: Umu Araoje, Umu Igbru, Umu Ekele, Umu edene and Umu Ngwuku. Marriage until recently has followed a close pattern of clan exogamy, although in these days it is becoming more and more a matter of village exogamy, and the older integrity of many of the villages is tending to break up. Also pertinent to the subject of this paper is the fact that although there is rule by elders, and the onyi (elders’ man) is the Keeper of the Arua (ancestral ‘bundle’), in several of the villages there are actual chiefs (Eze), the system of chieftaincy resulting from Igala influence from the north. Resulting also from Igala influence are the alusi shrines, priests, who are called Attama, signifying those who can ‘make medicine’ or contact the alusi.

The Theological Background

The theological and ontological significance of the High God, to Whom I will henceforth refer as Chukwu or Eze Chitoke, in northern Ibo religion was expressed with utter simplicity and clarity despite the complexities of the subject itself in the market area of Aror-Unu in November, 1963, and with less completeness in Abimo ‘Ajuona approximately a year earlier. In both cases, amidst a gathering of men who became interested in the interrogations about the relationships between Chukwu and the alusi and mankind, the notes which I took can best be summarized by the following diagram, which indicates the source and the destination of things which exist.3

![Diagram]

CHUKWU (EZE CHITOKE)

Alusi (esp. Ane 3)

Ndichie (Ancestors)

Newborn Child

Growth to Adulthood

Ndichie (elder-ship)

Death

Ane alusi and burial in a, ‘earth’

Chukwu (Who will reincarnate the chi in someone else)

Although it might appear repetitious, it is nevertheless necessary to record pertinent selected materials from my personal
field notes which constitute further evidence of the northern Ibo awareness of the functions and presence of Chukwu as distinct from the notion among some persons that Chukwu is relatively 'withdrawn.' The following verbatim recordings of field materials are drastically reduced in number, and are intended to serve only as a synopsized bit of the evidence.

1. Question: Who made all things?
   Response: 'Chukwu.'
2. Q.: Who made alusi (spiritual forces)? (At Ohebe-Oba, the exact question was: Who made Amaif-Oba (the major alusi there)?)
   R.: 'Chukwu.'
3. Q.: Who is Chineke? (Note: Chineke, farther south in Iboland, is a name of Chukwu as the Creative Manifestation.)
   R.: 'The invisible living soul (chi), the same as Chukwu.'
4. Q.: What is a man's chi?
   R.: 'Chukwu and chi mean the same thing. Chi also means the direct person the man is related to, the ndichie (spiritualized ancestor) who came back in him.'
5. Q.: What are other names for Chukwu?
   R.: 'Nna' = 'Father of all things.'
   'Eze Chukuoke and Eze Chitoke' = 'Lord of Creation.'
   'Eze Anyanwu' = 'Lord who owns the sun.'
   'Chi-Okuke' = 'Spirit, Maker of all things.'
6. Q.: How do men come into the world?
   R.: 'We come from Chukwu.' (At several villages, a spirited discussion, resulting in the following elaboration: 'Men come from Chukwu, because we get the chi from Chukwu, and we cannot be people without chi. But Ani and the ndichie also help to bring men into the world.')
7. Q.: What happens to a man's chi when he dies?
   R.: 'Chukwu takes it back, because it is Chukwu, and it belongs to Chukwu.'

The important point about the foregoing excerpts from field interrogations is that each man possesses chi (God Within), which is created by and given him by Chukwu, and which upon death is reclaimed by Chukwu, the High God is always in one sense immediately present to the northern Ibo villagers: 

**Direct Worship of the High God (Eze Chitoke)**

In northern Iboland, particularly in Nsukka religion, a great amount of direct worship of the High God as Eze Chitoke is practised. This usually takes four slightly differing forms: (1) daily worship, either preceding the daily *Ania* ceremony by the onyisi for the entire village or (2) in the village individual's shrine to Eze Chitoke in the family compound; (3) annual worship, sometimes a festival, and at least a day agreed upon by the entire village as the festival day of Eze Chitoke; (4) irregular sacrifices and consequent worship by the individual in his personal altar to Eze Chitoke in his own compound, or by the onyisi for a suppliant at the village communal altar of Eze Chitoke.

1. Daily worship preceding the daily *Ania* ceremony by the village onyisi.
   The *Ania* Ceremony is conducted by the onyisi of the clan, for he is the 'Keeper of the *Ania*,' or the set of staffs and symbols through which the clan maintains close relationships between the ancestors (spiritualized as ndichie, and thus closer to Chukwu, the High God) and the living. Briefly preceding the *Ania* Ceremony the onyisi usually offers a short prayer to Eze Chitoke:

   **Eze Chitoke, bea wehu oje.**
   Eze Chitoke, come have kola.
   **Mmuo, nine di ebea bea wehu oje.**
   All spirits here come have kola.
   **Wetebe umu aha.**
   Bring forth 'children'—i.e. villagers.

   **Wetebe nwani.**
   Bring forth women—i.e. wives.
   **Wetebe ego na ife ona.**
   Bring forth money and good things.

   That the *Ania* Ceremony is directly related to worship of the High God is clear in the foregoing prayer. At Umunne-Gwa immediately after an *Ania* Ceremony (which is more elaborate than the short prayers which I have quoted), the onyisi, Ugwu Ono, and his brother, Eze Nwa Ono, who is an elder, explained the relationships as follows:

   Q.: Why make an offering to Eze Chitoke?
   A.: We call Chukwu to come and take kola because he made the things on the earth.

   Q.: Why do you do this with the *Ania*?
   A.: Chukwu made the people, and the morning *Ania* Ceremony—offering of the *Ania*—praises Him.

2. Worship of the High God by the Family Head in the individual's compound.

   The individual's personal altar to the High God can take several forms in the area of north Nsukka, but the more common are the small, cone-shaped earthen altar (see fig. 1) such as those at

**Fig. 1. Earthen altars of Eze Chitoke**

(Above) In compound of Nwue Nwa Asoguwa of Umue Asoguwa clan in Umasi Village. (Below) In front of Ogwu house at Ibagwa-Ani. Photographs: A. J. Shelton

Umasi Village, Ibagwa-Ani, and elsewhere; a 'life tree,' called either Ogwu or Alagha (see fig. 3) such as those at Umunne-Gwa Village; a very small 'house'-type structure, usually unwalled, with a thatched roof, such as those at Alor-Uno (see fig. 2). The prayers and sacrifices vary from village to village; in some villages there is regular daily worship of the High God, and in other villages worship tends to occur either on an annual festival day dedicated to the High God or when the individual faces a par-
Ketebe unnu, ketebe ije, isi gi dibo.
Bring forth 'children,' bring forth goods, to please you.
Zogidemmu ndi ikenye.
Protect the people the ones you have.
Biko ogide anyi.
Please protect us.
Ekwene ije obuna ka ona anyi.
Do not let things unexpected not to happen (to) us.

4. Irregular Sacrifices and Prayers.

Irregular sacrifices and worship of Eze Chitoke are common in most of the villages about Nsukka, and are conducted by the individual household at his home compound altar to Eze Chitoke or Chukwu'okute, or by the onyisi (acting as the human intermediary or priest for the individual supplicant) at the village or clan altar, which is normally situated in the village square. The more common reasons for such occasional and irregular worship and sacrifice are sickness or suffering because of various fears, insecurity and personal setbacks, although not usually economic. As an example, I might cite the case of Umunne-Gwa Village and the communal altar to the High God, called in this instance Anyanwu Eze Chitoke (Lord, Creator of the Sun).

This altar is in the village square, and is set upon a conical earthen mound four feet in height. The altar itself (see fig. 4) is the wooden carving placed atop the earthen mound, and its main face is directed toward the east, where the sun rises. All offerings and prayers are directed to the altar (actually to Eze Chitoke through this sacred object) from the east side.

When an individual of the village has a truly serious problem—usually illness—he will consult an Onye na ogha afa ('One who can cast afa,' i.e. a diviner or fortune-teller), who will cast the afa (see fig. 5) and from them read or determine whether the supplicant needs to sacrifice to Anyanwu Eze Chitoke or to an alusi, or even whether the problem will disappear by itself. Should the answer be that he must sacrifice to Anyanwu Eze Chitoke, the supplicant will then visit the onyisi and explain the situation. The onyisi later accompanies the supplicant to the village square and makes the sacrifice—which is less important economically than spiritually, consisting usually of kola and palm oil—during which the onyisi invokes Eze Chitoke to intervene and to bring the supplicant out of
his miseries. The prayers themselves are the same as those made by individuals at household altars and during the annual festival of Eze Chitoke, although a few words might be changed in the middle section of the prayer to apply specifically to the individual suppliant’s problem.

FIG. 4. ANYANWÛ EZE CHITOKE
In village square of Umumme-Gwa. Scale: 1

FIG. 5. DIVINING IMPLEMENTS (AFA)
At Owerri-Ezeoba

Conclusion

In this short paper I have attempted to demonstrate that the ’High God’ is present in the lives of North Nsukka Ibo villagers insofar as the villagers consistently maintain that Chukwu creates and recalls the individual, the individual must possess chi to be a person and to be alive (and chi is Chukwu within the person), and that the villagers consistently practise direct worship of and sacrifice to Chukwu. Such worship takes the several forms of daily prayers preceding the Ara ceremony, daily prayers at a household altar, annual festival worship, and irregular although frequent supplication and sacrifice to the High God. In a series of subsequent papers I will present in detail the worship of the ahu and of the Ara, indicating their relationship to the worship of and belief in the presence of the High God in north Ibo religion.

Notes

1 MAN, 1964, 55.

What is a Gong? By Jeremy Montagu. With three figures

The names of many musical instruments are very loosely used, both in common parlance and in serious writing, and that which has suffered the most is the Gong; its name has been applied to many Idiophones of widely differing types and materials. It seems desirable to establish some basic criteria, to define some types of gong and to reclassify some of those instruments which have usurped its name.

Balfour (in Notes and Queries on Anthropology) introduces a 'Gong Series in which the instrument consists of a shaped piece of sonorous material (wood, metal, etc.) which is struck with a special striker.' This is not a definition of a gong in any real sense, and is not intended to be one, since the term is used to cover all struck idiophones except concussion instruments and clapper bells. The use of a specific instrument's name to cover a group of instruments, not all of which are properly described by that name, can only lead to confusion. Further, it seems illogical that if the clapper of a bell is removed and the bell struck with a stick, or with the same clapper externally, it ceases to be a bell and becomes a gong. Sachs (1940) states: 'a gong is made of bronze in the form of a flat or bulging surface in circular shape with the rim bent down; it is hit in the center by a stick, the rim being dead (unlike a bell, in which the center is dead).' He defines the bell thus: 'A bell is a vessel, the edge or sound bow of which produces the strongest vibrations while the top is mute.' Sachs’s definition of the gong is much more precise than Balfour’s, but, while both instruments are described by their acoustical behaviour, the definition of the gong is much more circumscribed than that of the bell. For the gong material, the shape and the mode of playing are all laid down, but the bell is only defined as a vessel. One reason for this is that many shapes of bell are known, many materials are used and the instrument can be played by striking with an internal or external clapper or with a separate beater. As regards shape, Sachs’s limitation of the gong to circular is too strict; as regards material, Simbringer points out that other metals than bronze are found and that therefore this criterion of Sachs’s cannot be allowed; even to restrict gongs to metal may be too limited (see below under Type D). On the other hand, Simbringer introduces a new criterion by specifying the area from which gongs come, India, China, Japan, South-East Asia and Indonesia. However, Kunst’s theory that the gong originated, or at least was known, in Ancient Greece cannot be dismissed. Another instrument which the Chinese owe to the ‘barbarians in the West,’ the Kuan-Tze, is provably descended from the Greek Aulos so that the gong would not be the only instrument which derived from that source. The following evidence, not cited by Kunst, supports his theory.
St. Paul, in I Corinthians, 13.1, refers to χαλκὸς ἰχθὺς ἤ κύμβαλον ἀλαλάζων, in the Authorized Version 'sounding brass or tinkling cymbal' but in the New English Bible more accurately translated 'sounding gong or clanging cymbal.' Clearly χαλκὸς ἰχθὺς is not a cymbal since it is being contrasted with κύμβαλον, nor is it a bell (κώδων). There are further references in Plutarch's description of the Parthians, in Theocritus, in Tacitus and in many others. Unfortunately no detailed description of the χαλκὸς ἰχθὺς has been found yet. All that we know is that it was brazen and loud-sounding and that the Greeks had an instrument which looked like a gong. A Roman gong in the Devizes Museum is illustrated in fig. 1. The detailed circumstances of its discovery are unrecorded; all that is known is that it was found during ironmining operations at Westbury, Wilts, between 1877 and 1882 and it is dated by association to the first or second century A.D. The shape is closely similar to that of some modern Chinese instruments. I will describe it in detail and cite the classical authorities more fully in the Wiltshire Archæological Magazine. It is clear that gongs existed outside Simbriger's area and thus the use of a geographical limitation is invalid.

The prime criterion in any definition should be acoustical. The material should not be restricted further than in the definition of an idiophone. The clearest definition of the gong is that in the Sachs-Hornbostel Classification: '111.241 Gongs. The vibration is strongest near the vertex.' This Classification is a numerical system in which each figure stands for a criterion, the first the most general and the last the most specific. This Sachs-Hornbostel number means that the gong is a directly struck percussion vessel made of a sonorous material in which the vibrations are strongest near the vertex or centre. If this definition is applied to all the instruments known as gongs, any which do not fit it can be examined to see whether they might better be described as something else.

Fig. 2 illustrates only those instruments which this paper accepts as gongs; this is done to avoid confusion.

The first example, A, is that which is, thanks to J. Arthur Rank,
is used with the rim resting on cords. If a bell is supported in this way the sound is completely deadened, producing nothing but a clank.

The Gong Kemodong is sometimes used as a substitute for the Gong Ageng (and smaller versions for the gongs of the Bonang). Although the sound is similar to that of the real gongs, these substitutes cannot be accepted as gongs. They are plates with a boss and not vessels; they vibrate most strongly at the edges and not at the centre. They belong with the Sachs-Hornbostel 111.22, percussion plaques.

Another instrument often called a gong is the hollowed-out piece of wood also known as a slit drum. Sachs rightly describes the use of the term gong for these instruments as “an intolerable abuse.” They are listed in the Sachs-Hornbostel Classification under 111.231, Percussion Tubes, along with Tubular Bells and the Tubaphone. This is acoustically unsatisfactory because these other tubes are open only at the ends, whereas the essence of the slit drum is that it is closed at the ends and open along one side and that it is struck on the lip, at which point it vibrates most strongly. One solution, which seems an unnecessary complication, would be to create a new class of Percussion Troughs. Since these instruments are all vessels in which the vibrations are weakest at the vertex, it would be better to classify them as the bells (111.242) which, both acoustically and morphologically, they are.

One type of wooden drum is exceptional in that it acts as a gong and not as a bell; this is the Fijian instrument illustrated at D in fig. 2. This is struck on the upper surface above the rectangular hollow cut in the lower surface of the wood and not on the lip. A photograph showing it in use is given in fig. 3. The instrument vibrates most strongly on the top and not at the rim and it should therefore be classed as a gong, alone of all the wooden instruments that have been called by that name.

Another mistaken usage of the word ‘gong’ is for the temple instrument of China and Japan which rests with its vertex on a cushion; the best-known form is a deep cup. The instrument is edge-vibrating and it is properly classified as 111.242.11, Resting Bell. The shape of these instruments can vary: there is an unusually contracted example in the Pitt Rivers Museum in Oxford which is the same shape as B (i). This gives little sound when struck in the centre but, struck on the edge while it rests on its vertex, the tone is loud and clear. In appearance there is no difference between this and a true gong, but the distinction is obvious as soon as the instrument is sounded; if it vibrates at the centre it is a gong, if at the rim it is a bell.

I have already discussed the African iron bells, both single and double, in MAN (1964, 141) and shown them to be bells and not gongs. I have since observed examples fitted with clappers, which makes the proof even stronger.

A much more difficult case is that of the bronze drums used by the Chinese, the Karen and Shan, and the Alor. Sachs-Hornbostel specifically include these among the gongs, but Heger is careful always to refer to them as Bronze-Pauken or Metall-Trommel. Sachs (1940) suggests the derivation from a drum of skin and wood which is apparent to the eye and yet cites the traditional origin of the instruments under the Han dynasty when they were made as substitutes for ordinary drums. These are instruments of prestige as well as of value (in areas other than China they are used as currency) and some of them have the top covered with bronze models of a village scene as well as the traditional four frogs. I have not encountered one of these ornate versions, but it does not seem likely that they would be used as musical instruments or would give much sound if they were. The Chinese instruments are cast in sectioned moulds; the Alor, which are brass rather than bronze, seem to be cast in sections and soldered together; the Karen are cast by the cire perdue method, but have the seams of the Chinese carefully reproduced on the sides. An illustration of the Karen type in use can be found in Sachs (1927). The top is struck with a large beater and the side wall with a lighter stick; this is a method of playing that is frequently used with drums in many parts of the world. All three types behave in the same way acoustically. The top vibrates centrally, the body hardly at all. Because of the vibratory pattern the top should be considered as a separate acoustical entity from the body, which acts like the anchor of the diaphragm of a telephone or acoustic gramophone and forces the top to vibrate in this way. The creation, however, of a new main class of Diaphragmophones is not advocated and because Membrane must mean skin these instruments cannot be classed as Membranophones. Since they are not gongs, it is suggested that they be known as Metal Drums or Bronze Drums (Bronze-Pauken is misleading since Pauken is the German for Timpani, an entirely different specific instrument). If the Sachs-Hornbostel numerical system is to be used, a new number and description: “111.243”, a metal diaphragm fixed at the edge including the so-called metal drums” might be adopted.

The oil drums of the West Indian Steel Bands, shown at F, behave differently from the above. The top of a disused oil drum is divided into small areas and each area is beaten out so that it gives a different note. Each area vibrates centrally and separately so that in effect each oil drum is a gong chime on one body; there is no diaphragmatic effect from the top as an entity. Although the oil drum as a whole is not a gong it should be considered as coming under 111.241.2, Sets of gongs (gong chimes), possibly with a suffix to show that the ‘notes’ are combined on to one body instead of arranged separately in the more normal framework.

The rock gong is found in most parts of the world and usually consists of a natural boulder which gives a more or less musical sound when struck. The acoustical functions of boulders have not, as far as I know, been investigated. Clearly they are not all vessels, although such a shape may turn up fortuitously or by design. The Sachs-Hornbostel System classifies struck idiophones by their shape: sticks, plaques, tubes and vessels. If a classification within the System is seriously required, a new series will have to be initiated on the lines of 111.25, Percussion boulders. The term Lithophone is no solution since this also covers the stone instruments of the xylophone pattern and the Chinese L-shaped jade plaques.
Some usages (e.g., 'gong' for the xylophones and other instruments of that shape) are so obviously wrong that they have not been discussed. The definition of the gong in the Sachs-Hornbostel Classification System as 'a directly struck percussion vessel in which the vibrations are strongest near the vertex or centre' has been applied to all the instruments usually termed gongs. Only two instruments, the bronze drums and the rock gongs, fit neither this description nor any other in the System; the rest have been classed as bells or gongs according to their acoustical behaviour. It is hoped that bells will no longer be called gongs, that the end of the 'intolerable abuse' of describing slit drums as gongs is in sight and that the term 'gong' will be reserved for those instruments which behave as gongs acoustically.

Acknowledgment

I would like to express my thanks to the Curators and staffs of the Archeologe and Ethnographie (Cambridge), British, Devizes, Horniman and Pitt Rivers (Oxford) Museums who have permitted me to waste their time, read their books, search their reserve collections and hit and photograph their gongs.

Notes

1 Instruments made of sonorous materials which sound without the need of stretched skins or strings.
2 'Als nicht nur Bronze, sondern auch Messing-, Eisen-, Zink-, Gongs existieren,' op. cit., p. xii.
3 'Von Vorderindien bis zum westlichen Neu-Guinea und von Korea und Japan bis zu dem südlichen Bogen der indonesischen Inselwelt,' loc. cit.
4 Music in Java, p. 143, or article 'Gong' in Die Musik in Geschichte und Gegenwart, or Ethnos, 1947, pp. 79ff. and p. 147.
5 I am informed that the ideograms for this instrument and for the Japanese equivalent, the Hiehirkue, mean from the West.
6 Baines, pp. 192 and 202.
7 I am indebted to Peter Khorocho for drawing my attention to these references.
8 Unmittelbar geschlagenen Idiophone.
9 Aufschlagfläche.

References

—, Die Musikinstrumente Birmas und Assams, Munich, 1917.
—, The History of Musical Instruments, New York, 1940.

SHORTER NOTES

Finger Prints in a Greek Sample. By Dr. D. F. Roberts, V. Luttrelle and C. Pasternak Slater, Medical Research Council Population Genetics Research Unit and Somerville College, Oxford. With three tables

During the course of investigations on ABO blood groups in the Greek island of Tinos in the Aegean, reported elsewhere (Roberts, Luttrelle and Pasternak Slater, 1965), the opportunity was taken to collect finger prints from a small sample of the population. While the sample includes only 95 unrelated individuals, the results of the

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<th>Table I. Ridge-Count by Digits</th>
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<tr>
<td>1</td>
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<tr>
<td>Left hand</td>
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</table>

print analysis are briefly summarized here since there is a general paucity of quantitative data so far available in the literature.

The total ridge-count in males averages 137.51, standard deviation 14.8, and in females 126.65, standard deviation 44.5. Ridge-counts by fingers are given in Table I. The total ridge-counts are generally similar to the few samples reported from Western Europe, and the counts by individual fingers similar to those reported by Holt (1961) on a large British sample, though there is a suspicion of slightly but not significantly lower counts on the lateral digits, particularly in males. Of interest however is the distribution of S² (Table II), measuring (as the sum of squares of deviations) the variation between ridge-counts of individual fingers in a given subject (Holt, 1958). There is a significant tendency for a greater proportion of subjects in Tinos to have lower values of S² (below 200) than in Holt's British sample, and this is particularly due to the female subjects, of whom 25 have S² values below 100 and only 12 have 200 and over. Both these suggestions are of interest in the light of Bonnevie's postulated factors in the inheritance of quantitative values.

The percentages of arches, loops and whorls in the total sample are respectively 60.0, 61.89 and 32.11 per cent.; in males alone 77.55, 56.73 and 36.7 per cent., and in females alone 46.71, 28.71, and 29.3 per cent. This qualitative assessment in the combined sexes suggests affinity with populations to the west rather than with those of the Middle East and Asia Minor (Table III) and the existence of an east-west cline in the frequencies of whorls and loops, though combination of results for both sexes for purposes of comparison with the other data reduces its strictness.
A Corrective Comment on Wikmunkan Marriage. By Dr. Rodney Needham, Institute of Social Anthropology, University of Oxford

Mr. Michael Walter, of Pembroke College, Oxford, has been so kind as to point out to me an expository slip in my 'Note on Wikmunkan Marriage' (MAN, 1963, 46), and I have therefore to acknowledge this and make a correction. A re-examination of the point in question brings in addition a new analytical profit, which further invites the publication of the present comment.

Thomson (1955, p. 40) reports that the woman whom a Wikmunkan man may marry is determined by persons in the second ascending generation, who select 'one or more of the "sisters" of his father's mother (ngatjawaiyo) whose daughters will henceforth be potential mothers-in-law (pinya kentj) to the man involved. In discussing the determination of the pinya kentj, I propose in my note that even if the woman who is initially in question, as the ngatjawaiyo, is indeed the father's own mother, the conclusion still does not follow that marriage with a first cousin is precluded, as Thomson maintains. What I then write is: 'Let us suppose that this woman's daughter, i.e. the pinya kentj, is married to Ego's mother's brother (kala), and on this premise I base part of my argument, but this hypothetical situation is mistaken in two respects.

1. As it stands, it entails (and the genealogical figure by which it is illustrated can be read as showing this) that Ego marries a woman who is not only FMZDD and also MBD, which is the intention of the argument of the note at this point, but one who is simultaneously FZD too.

Now this conflicts with the rules of marriage among both the Archer River Wikmunkan and those of the Kendall-Holroyd grouping. Among the former the father's sister's daughter cannot be married, but this is what would be literally effected by the supposed circumstance; and among the latter, although marriage with the father's sister's daughter is in fact permitted, a woman who is at once mother's brother's daughter and father's sister's daughter may not be married, yet this again is precisely what my note inadvertently but nonetheless erroneously postulates.

2. It is not the father's mother herself whose daughter becomes pinya kentj, but a 'sister' of this woman, a fact which is clearly established earlier in the note.

It is this second matter which constitutes the real slip at issue, and which permits the incorrect inference in the first. Both can be set right, however, by amending the words 'Let us suppose that this woman's daughter' (p. 44) to '...this woman's classificatory sister's daughter,' which was indeed the intended burden of the argument. (The woman's uterine sister would be equally eligible so long as she was not married to the same man.) In this case, the daughter of the pinya kentj is still FMZDD/MBD but not FZD, and she is thus marriageable in either Wikmunkan grouping.

Now the real interest of this conclusion is not merely that a rectification has been made, but that this consequence must be the point of the requirement that the mother of the pinya kentj shall be a 'sister' of the ngatjawaiyo in question; for if the woman designated were the father's own mother her daughter's daughter, rather than being singled out as a potential wife, would not be marriageable at all.

This systematic connexion between the usage discussed and the rules of marriage, variant though these are in the two cultural groupings of the Wikmunkan, tends again to confirm the original analysis (1962) and the argument of the subsequent note (1963). I am therefore particularly grateful that Mr. Walter should have discerned my omission in the latter, for the conclusion to the present comment is a further definite gain in the progress of our understanding of Wikmunkan society, a system which is of quite singular interest and offers unusual theoretical instruction.

Note

See 'The Wikmunkan Mother's Brother' (Needham, 1963; cf. Homans and Schneider, 1962), page 146, note t, for certain other corrections and additions to the original analysis.

References


Three radiocarbon dates of interest to West African archaeologists have recently been determined by Isotopes Incorporated of Westwood, New Jersey, as follows:

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<th>Determination No.</th>
<th>Sample</th>
<th>Age (in years before 1950)</th>
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<tr>
<td>(1)</td>
<td>Sample B Taruka</td>
<td>2,330 ± 120</td>
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<tr>
<td>(2)</td>
<td>Sample C Rop D4</td>
<td>1,975 ± 120</td>
</tr>
<tr>
<td>(3)</td>
<td>Sample A Nok</td>
<td>1,065 ± 120</td>
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</table>

Although each of these will be republished when the material to which they relate is described in detail, the dates themselves are of sufficient interest for immediate release with minimal documentation.

(1) The sample from Taruka, the first non-alluvial occupation site of the Nok Culture to be excavated (14 exploratory trenches
were dug in January, 1961), is of very considerable importance since a date of 280 B.C. ± 120 years for a horizon containing Nok figurine material in situ is a satisfactory corroboration of the original tentative dating estimated on geomorphological evidence, before the radiocarbon method had been developed (see Bernard Fagg, 'An Outline of the Stone Age of the Plateau Minesfield,' Proceedings of the III International West African Conference, Ibadan, 1949, published by the Nigerian Museum, Lagos, 1956).

The only other radiocarbon date which can be accepted for the Nok Culture without considerable reservation is Y 474, which provided acceptable evidence that figurines were being made at Nok before A.D. 206. A duration of nearly five centuries for the Nok Culture will therefore be seen to be proved at last, and this in itself increases the probability that it survived much longer.

(2) The sample from the Rop rock shelter on the Jos Plateau was excavated in 1944 (Proc. Prehist. Soc., N.S., Vol. X (1944), p. 68) and fortunately stored in conditions which minimize the danger of contamination. It consists of fragments of human long bones contemporary with the microlithic industry (Guinea Neolithic). A date of 25 B.C. ± 120 years is perhaps quite consistent with the presence of the long established Iron Age Nok Culture only 30 miles away to the south-west and 2,000 feet lower at the foot of the escarpment. There is no positive evidence that the Nok Culture ever penetrated the high plateau.

(3) The sample taken from Nok is a drizzling from the heartwood of a stout and well carved pounding stick (measuring about four feet long and three inches thick), which was cut in half by a mining labourer's spade. It was found waterlogged and partially carbonized close to a heavy axe handle in a similar condition and some tin beads. Since it was found casually in the course of mining, the exact horizon is uncertain and there is no clear evidence that it relates to the Nok Culture. Its main interest lies in the dating of the oldest wooden artifacts yet found in Nigeria, and possibly also the cast tin beads, to A.D. 875 ± 120 years.

Summer Seminars for the Study of Prehistoric Rock Art.

Communicated by Dr. E. Anati

The Seminars of the Canonica Valley are open to scholars and students in anthropology, archaeology and art history from all parts of the world. They may stay from eight to 12 weeks between July and September at a weekly cost of 29,000 lire (about U.S. $46.50) for full board, tuition and laboratory fees. The programme includes field work in the Valley, laboratory work and research exercises. Applications (with a curriculum and a presentation letter from a university professor in one of the said subjects) must be received before 15 April, and should be addressed to: Seminario e Centro Camuno di Studi Preistorici, Capo di Ponte, Valcamonica (Brescia), Italy.

Examination of the Ife Bronze Heads. By Harold Barker, Research Laboratory, British Museum. With a table.

During 1947-48, 17 metal heads from Ife, Nigeria, were cleaned in the British Museum Research Laboratory. At this same time, the opportunity was taken of removing samples for analysis, which was later carried out by Dr. A. A. Moss (now Deputy Keeper, Department of Mineralogy, British Museum (Natural History)) and Mr. H. Barker. The results are given below, together with the analysis of the head in the British Museum collection (No. 18) (Table I).

It should be noted that none of the alloys is a tin bronze and that, with the exception of No. 4, the analytical results do not add up to 100 per cent, indicating that some other element of the metal has occurred. Nos. 14, 15, 16, 18, and 17 can be described as copper, whereas the remainder, with the exception of No. 3, are best described as heavily leaded brasses. The copper/zinc ratios of this group, however, are substantially less than that of modern brass (70/30), but are quite close to that of modern gilding metal (85/15).

which has a similar zinc content to orichalcum. They would have had a golden appearance originally. No. 7 contains some gold although it is not possible to say whether its presence is purely fortuitous or arises from a deliberate, perhaps ritualistic, addition of gold to the crucible. The compositions of Nos. 16 and 18 (the British Museum specimen) are closely similar and have been reported previously (A. A. Moss, 'Further Light on the Olukum Head of life,' Man, 1949, 159).

Spectrographic Examination

This showed qualitative similarities between all the various alloys, for, in addition to the elements recorded in the chemical analysis, all contained minor to trace amounts of silver, arsenic, antimony and bismuth. In addition, traces of nickel were recorded in all but Nos. 3, 6 and 17.

Metallurgical Examination

Fragments of four of the heads (Nos. 4, 6, 7 and 10) were examined metallurgically. The microstructure of these specimens indicated that the cast metal solidified very slowly. This effect would arise if the heads had been cast, according to current West African practice, by pouring the molten metal into a mould which had been made red hot and then insulated by sinking into a pit in the ground.

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<th>Sample No.</th>
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* Including gold 2-4 per cent. n.d. = not detected.

Hon. Editor's Note

1 Mr. William Fagg, Deputy Keeper, Department of Ethnography, British Museum, writes as follows: 'These results were originally intended for publication together with a very detailed technical description of the "bronzes" which was compiled in the Department of Ethnography at the same time by the late Mr. H. J. Brahmoltz, C.B.E., Mr. Bernard Fagg (then on leave as Government Archaeologist, Nigeria) and myself, and with an exhaustive set of photographs taken in the Museum's Photographic Studio; but this project was not proceeded with.

* A suggestion made in the Research Laboratory at the time (not mentioned in the above report, for lack of positive evidence) was that the composition of the heads might well be due to purposeful alloying, but to the smelting of the metal from a mixed ore, such as orichalcum. (Such ores apparently occur at certain disused mines in the southern Sahara, from which supplies of metal used to be sent by caravan to the Nigerian area in the middle ages.) Such lack of interest in the precise composition of the metal used by the bronze-founders is entirely consonant with what we know of the development of metal technology in West Africa, where the Iron Age seems to have supervened directly upon the Neolithic (e.g. at Nok). Iron would at once have been used for tools and weapons.
while the non-ferrous metals, arriving later, were used only for ritual and decorative purposes, for which composition, hardness, etc., had no critical importance. Ease of casting was no doubt the dominant consideration, as it still is wherever brass-casting is practised in West Africa, and for this purpose the large quantities of lead and zinc in the Ife bronzes would have definite advantages—though the most perfect of all the castings, no doubt to the surprise of European metallurgists, is the "Olabufon" mask, No. 17, which is almost pure copper.

'The "sample numbers" given in the first column are also quoted under the pictures of these heads in Leon Underwood's Bronzes of West Africa, London, 1949. Close study of the heads from the stylistic point of view strongly suggests that they are by several different hands. Nos. 1 and 8 are so similar as to leave little doubt that they are from the same hand (as also, perhaps, may be the complete standing figure discovered in 1957—see MAN, 1958, Plate A, and 1959, Plate Ka); the same applies to Nos. 3 and 6. Nos. 16 and 18, on the other hand, seem to embody quite different individual conceptions of the human head, and could hardly be by the same hand; No. 16 is the "Olokun" head, which Mr. Underwood and I have, we think, demonstrated to be a modern replica (MAN, 1949, 1).—Ed.

CORRESPONDENCE


Sir,—To an outsider and mainly an observer on the sidelines like myself, the amount of energy spent and dialectical heat generated over the past two decades by the structuralists—to give them a generic label although their species are many—in an endeavour to elucidate the question of unilateral cross-cousin marriage and its derivative problems, is astonishing. It is with no small measure of mixed feelings that I am taking the occasion of the publication of Professor Frank B. Livingstone’s letter to make some comment on this general theme.

The main problem, as I see it, is why, where unilateral cross-cousin marriage occurs, it is almost invariably matrilineal and extremely rarely patrilineal that is reported—ignoring here the refinement as to whether it is preferential or prescriptive.

Using Australian data, where there is a wide discrepancy in age between man and wife (wives)—the man on the average being older than the wife (wives)—I have shown that the MB children are on the average considerably younger than FZ children. This average difference in age between the two types of cross-cousins in the society that I dealt with was of the order of 15 years. This average difference in age provides a fundamental distinction between the two types of cross-cousins which can explain most of the phenomena discussed by the structuralists.

Now this fundamental distinction, as I have chosen to call it, results quite simply from the usual average difference in age between man and wife found in society. It is so simple that it is astonishing, in view of the interest taken in the subject of unilateral cross-cousin marriage of recent years that the connexion between the average age difference between spouses and that between the two types of cross-cousins was not shown on purely logical grounds without the use of empirical data.

The structuralists dealing with this question of unilateral cross-cousin marriage start from the false premise that there is no intrinsic difference between the cross-cousins whereas there is this fundamental distinction referred to—extreme in some societies, less pronounced in others.

If the MBD is younger than a man, then in a society where a man usually marries a woman younger than himself, the MBD is by age, at least, a potential wife for him. *Vice versa* in such a society, a FZD is usually older than a man and is therefore by age not a potential wife. But even in the extreme Australian society dealt with there were isolated cases where FZ children were indeed younger than MB children, but these were exceptional. These exceptional cases did not alter the general conclusion that a man in such a society by the age structure, would tend to marry a matrilineal cross-cousin and only rarely a patrilineal cross-cousin. In other words, where cross-cousin marriage is the rule and there is a difference in age between a man and wife—the man being the older —, matrilineal marriage for the man is a natural, shall I say, automatic demographic result. It is not that a man has a ‘preference’ for a matrilineal cross-cousin except in so far as her age relative to the man’s, generally ‘prescribes’ her as a potential spouse.

My problem at the time, however, went considerably further than this. It was to resolve the contradiction between the fact that matrilineal marriage for the man in Australian society inevitably predominates, yet the majority of Australian systems of kinship classification (e.g. the Aranda and Kariera) apparently allow sister-exchange marriage. An account here of how this particular contradiction was resolved would however take too long. The demographic age factor in unilateral cross-cousin marriage has been almost entirely neglected although one or two writers, 4 who like myself are on the periphery of discussion, have indicated its implications. If the structuralists were to invoke the age factor it would render much of their arguments amongst themselves redundant, and moreover would bring the discussion out of the *cult de sac* into which, to me, it appears to have landed itself.

It may be suggested that one cannot argue a general thesis on the basis of the extreme case of the Australian Aborigines. But the difference between Australian Aboriginal society and our own is not one of kind but is quantitative in respect to the relative age of man and wife and that in all human societies a man is usually older than his wife and as a consequence the patrilineal cross-cousin is usually older than the matrilineal and therefore, where cross-cousin marriage occurs it *will* be with a man's matrilineal cross-cousin. No doubt, if the rare cases of cross-cousin marriage in our own society were examined it would be found that matrilineal marriage for the man also predominates. It is the fact that it is almost, if not completely, universal for a man to be on the average older than his wife that preferential patrilineal cross-cousin marriage for the man is never or only in exceptionally rare circumstances found.

In the last paragraph the assumption has been made that in virtually all human societies the average age of the husband is greater than the average age of the wife. I may be open to correction on this point. However, a society can be conceived of, in which the average age of the wife is greater than the average age of the husband (in parentheses it is worth noting that marriages of old women with young men occur even in Australia, but they are in the minority) and we consequently have the reverse situation to that which obtains in Australia and the matrilineal cross-cousin on the average will be older than the patrilineal cross-cousin. In this form of society where we assume that cross-cousin marriage exists, the man is more likely to marry a matrilineal cross-cousin, as this woman is older than he is. Here again—as in the case of Australia—if the *status quo ante* is to be maintained and the former age difference between spouses is to be upheld a man will marry a matrilineal cross-cousin.

One could also conceive of a situation where the *status quo ante* is changing, for instance in a contact situation, and that instead of marrying as in the previous generation, e.g., a woman younger than himself (as in Australia) a man marries a woman older than himself. In this case the man’s choice would fall on the patrilineal cross-cousin and we could possibly observe preferential patrilineal cross-cousin marriage. But the changes would need to be rapid and by the nature of things this situation is likely to be extremely rare.

FREDERICK ROSE
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Notes
1 MAN, 1964, 59.
2 Classification of Kin, Age Structure and Marriage amongst the Groot Eylandt Aborigines, Berlin, 1960, pp. 121-7; and *Die uni-
He suggests that we consider the evidence from 'our own' society. I am prepared to bet that if he tries this out the results will not confirm his theory. It needs only a little investigation to see that, generally speaking, the age gap between English spouses is substantially less than the average age gap between surviving siblings. (The investigation would certainly be worth making none the less. I should suspect a significant class factor here: the greater the property, the greater the age gap between spouses.) Dr. Rose will quickly see the significance of this point.

Dr. Rose's careful work upon age differences between husband and wife among Australian Aborigines deserves respect. It does not illuminate the discussion of 'unilateral cross-cousin marriage.'

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Vedd and Sinhalese Kinship. Cf. MAN, 1964, 139, 189

Sw.—The purpose of my letter on the above subject was to point out the dangers of attempting to correct alleged mistakes in an ethnographical account of the Vedda published over 50 years ago, on the basis of recent fieldwork among Kandyans Sinhalese. Only three points in Dr. Leach's reply (MAN, 1964, 186) need comment from me: (1) Although the three authors in question might have drawn attention to the wide variety of Sinhalese kinship terms, Dr. Leach himself categorically declared in his contribution to the Seligman Festschrift that they were 'superficial differences' which could be ignored. My contention was that the variants could be sociologically important. This is consonant with Radcliffe-Brown's position that kinship terms provide clues to interpersonal behaviour between kinsmen, and this is accepted by many professional writers besides myself. It is a gross exaggeration to say that I maintain that kinship organization is deducible from nomenclature. In Sinhalese Social Organization, based primarily on historical sources, data other than terminology, including proverbs, have been used as clues to kinship behaviour. I suggested that contemporary fieldworkers should differentiate micro-social kinship structures further, rather than dismiss kinship variants as 'superficial.' (2) Although the argument in Dr. Leach's Festschrift article was ostensibly on Vedda 'matrilineal clans,' there were obliter data relating to Sinhalese kinship generally which raised certain methodological issues. (3) I am quite prepared to accept the certification of Dr. Leach, comfortably ensconced now in Cambridge, that Dr. Yalman's account of a Kandyan village was based on '15 months of very intimate study.' But I happen to know that he was assisted by a low-country interpreter. I have no wish to propose an anthropological inspectorate to test the truth of these claims to intimacy. It is rather damaging however that such a long period of 'intimate' led Yalman to conclude that the politest expression for 'wife' is hamine. Comfortably ensconced though I may be in University quarters in Peradeniya, I am certain that this term is not used in Kandyan villages.

RALPH PIERIS
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University of Ceylon, Peradeniya

The 'Paddle' Sign of Camonica Valley

Sw.—Since the publication of Emanuel Anati's Camonica Valley the mystery of the so-called paddle sign must have exercised the minds of many of your readers. I write to suggest an explanation of this sign which I think meets all the requirements of its associations, viz. that it represents a shield. Dr. Anati himself (p. 208), after consideration of all contexts in which the sign appears, states that 'its role must have been to afford protection' and that in one carving it occurs 'instead of a shield.' Why 'instead of'?

This interpretation meets all cases if it be assumed that the sign
sometimes represents a shield in actuality and sometimes symbolically as protection, an idea later perpetuated in the zegis of Jupiter.

It remains only to account for its shape and this is not difficult writing from South Africa. Before the plentiful use of metals the shields would be made of hide, in this case more or less rectangular but probably curved in the horizontal direction (as held). But such shields need stiffening and this is best done with a stout stick vertically at the back of the shield as with the Nguni (e.g. Zulu) shield where the stick is long enough to project two or three feet from the edge of the hide. The shield is usually held with the projection of the stick downwards to parry a swipe at the holder's legs. This is how the 'paddle' is almost always depicted. The small cross piece, knob or ring often shown at the end would, by adding weight, aid the force of the parry and the ring would be useful to hang the shield on the wall.

That other types of shield are also shown in no way invalidates my argument. The shield as a symbol no doubt retained its original form when superseded by other shapes of shield as defensive weapons.

Johannesburg

A. R. WILLCOX

REVIEWs

STUDIES IN KINSHIP AND MARRIAGE DEDICATED TO BRENDA Z. SELIGMAN ON HER 80TH BIRTHDAY

15


Mrs. Seligman is to be congratulated on her eightieth birthday, not only for her significant contributions to anthropology, but also for her magnificent and very human capacity to make everyone comfortable and hence allow him to be more than ever himself. These essays were obviously written both with Mrs. Seligman in mind, on topics that concern her, and out of the going work of their authors, Evans-Pritchard, Firth, Ford, Fortes, Leach, Godfrey Lienhardt, Raglan and Schapera. Seldom can it be said in honour of any scholar that such a collection of authorities could, out of their current researches, write essays of which she is in one sense or another the spiritual grandparent. And seldom can it be said of a Festschrift that the prestation, with its accompanying gift of self, is so apparent as it is from each essay in this volume.

Evans-Pritchard has provided Zande texts and comments on the general topic of relations between husband and wife. Lienhardt has approached a congruent topic, and documented it from oral literature, on 'Dinka Representations of Relations between the Sexes.' Both of these essays have used primary data in a masterly way to show ethnic evaluations in their respective societies, of the bonds of marriage. Schapera, using what must by now be the best tribal census ever recorded by an ethnographer, has given us proportions and quantities about marriage of kin among the Tswana tribes.

Firth, Ford and Fortes have all written about descent groups. Their essays all refine topics that their authors have considered before. Ford has never given so good a statement of Yakó descent groups, with an excellent correlation of ideal and actual; Fortes has never before stated so succinctly the differences between double descent and the 'submerged lineage' (which the late Franz Baermann Steiner always referred to, not irreverently, as Untergetaute Stammlinie); Firth, writing about 'Bilateral Descent Groups' has brought his analysis of the Maori hapu to new clarity (though I must admit to being one of those who think that 'bilatral' groups are what Lowie called a 'lumber room' that is best cleaned up by tactics of divide and conquer, and that the category 'bilatral' will disappear).

Leach has also written about descent groups, turning his insights and techniques onto the Seligman's Veddha data, and proving again the superb quality of their data, but, to my satisfaction at least, that what they (with the theory available to them at the time) analysed as matrilineal are not primarily kinship groups, but are rather types of caste in a caste system. Lord Raglan, beginning with the monumental work of Maine, has written on the association of primogeniture and ultimogeniture with different sorts of social structures.

Every editor who undertakes a Festschrift for teacher or friend would do well to examine this one, which Evans-Pritchard in his Foreword hastens to tell us is not a Festschrift in the ordinary sense but is rather 'an intimate tribute to a great lady from a few personal friends.' Such a viewpoint gives us a clear picture of the vitality of the work of the great lady, and an equally clear picture of her friends. It can be counted completely successful.

Paul Bohannan

Hon. Editor's Note

As this issue is about to go to press, the sad news reaches the Hon. Editor of Mrs. Seligman's death on 2 January. Tributes to her memory will appear in an early issue, but he feels constrained to give some expression at once to the great sorrow which all British anthropologists, and great numbers from overseas, must surely feel that she is no longer among us to inspire us. The void that she has left cannot be filled, and will remain to remind us all—if that were necessary—of all that we owe to her.—En.

LE POUVOIR ET LE SACRÉ. Edited by Luc de Heusch. Annales du Centre d'étude des Religions, Institut de Sociologie. Brussels (Univ. Libree, Bruxelles), 1962. Pp. 256. This volume of essays by members of the Institut de Sociologie of the Université Libre de Bruxelles who are interested in the phenomenological approach to the history of religion concentrates primarily on the sacral kingship in a wide field of inquiry in a number of very different cultures and civilizations. After a general survey of the magical content of kingship by Professor Luc de Heusch, reviewed and discussed in greater detail in the subsequent essays, Professor Pierrene examines the origin and genesis of the monarchy in ancient Egypt conveniently summarized in three cylinders covering the several phases of the Dynastic Period in Upper and Lower Egypt during which it persisted as a duality controlled under Heliopolitan influence and becoming an absolute monarchy in the New Kingdom. The function of the Pharaoh in the maintenance of the cosmic order in terms of Maat, of which the reigning sovereign was the source, is next discussed by Philippe Derchain in relation to the various attempts to define his rule. The unification of Sumer and Akkad by Hammurabi in Mesopotamia and the place and function of the king in the Easuqal at Babylon at the New Year Festival are very briefly considered by Andre Finet along the familiar lines.

Creating the Greco-Roman world Emile Janssen gives an interesting and critical analysis of the Mycenaean conception of royalty, and of the divine right of the Homeric Olympian heroes and kings symbolized by the sceptre. Professor Jean G. Preaux carries the discussion to Rome, paying attention to the recent philological work of Dumézil in this field in relation to the duality of sovereignty, the myth of Cyclops and the foundation of Rome by Romulus, together with the tradition of the Gentre Royal in Tusculum, and its last king Tarquin. In a more debatable region, nothing daunted by the repeated denials of a sacral kingship or any approximation to a priest-king in Aryan India, and the confused state of the evidence, Professor and Mme. Rocher endeavour, not without some success, to establish the divine character and function of the pre-Vedic kings with the aid of carefully chosen quotations from the Manusmriti, several gods having been sometimes combined in the human form of the king. The essentially administrative character of the ruler and his subordination to the Brahman caste are explained as the result of Brahmanic influences and the caste system, the masses continuing to maintain the divine status of the kingship.

Professor de Heusch has an easier task in demonstrating the position of the monarchy in the feudal cultures in the region of the Great Lakes in Africa (Rwanda-Urundi, Uganda), together with the incestuous royal marriages, the exercise of magic, the significance
of primogeniture in the social structure and the vexed question of the ritual death of the king as a regenerative rite. Here the royal magician, the priest-king and the king-god coexist and plays a role of very considerable importance sociologically as a consolidating dynamic ensuring dynastic ancestral continuity and the assimilation of the community with the divine order.

In the two concluding essays the sanctity of royal power in the "Leo Arminius" drama of the German poet Andreas Gryphius (1616–64) is discussed by Professor H. Pardis, and finally the attempts of Nietzsche, Heidegger, Sartre and their contemporaries since the First World War to desacralize the sacred by the declaration "Dieu est mort," are considered by Professor L. Flaim; both of which, however, lie outside the scope of the present review. For the anthropologist if these stimulating studies can hardly be said to have broken very much new ground, they have shown the importance and significance of the widespread institution of the sacral kingship and its theme from a variety of standpoints and diverse disciplines.

E. O. JAMES


This book is a series of vivid portrayals of ten sociological studies. The topics examined, after a critique of Durkheim's work on suicide and anomie from which sociological science is said to date, are Thomas and Znaniecki's Polish peasant studies, Chicago ecological urban sociology, Roethlisberger and Dickson's industrial sociology, subcultures like Whyte's street corner society, social science and the soldier, Kinsey's sex reports, the authoritarian personality, small group studies, and new communities. A final fifty pages ruminate on a number of lesser lessons to be drawn from these studies. The chief adverse one is that social workers, in promoting a one-way social adjustment to an approved middle class image of a somewhat specialized kind, show too little awareness of the many possible forms of social organization and their related ideologies.

If this very useful survey-summary handbook is to be used as a textbook for introducing first year students to scientific sociological theory, as well it might be, two corrections of emphasis would be helpful. The scientific sociology discussed by Professor Madge is, very largely, a sociocentric sociology of America. This is unfortunate for students preferring the sort of comparative generalization to which much social anthropology aspires. Secondly, while it may be true that the lessons of post-Suicide sociology mainly concern matters of method, technique, sources and sampling, the "unremittingly empirical" aspect of this can be over-estimated. Most of the studies discussed, as Professor Madge observes, are concerned with the alleviation of current social problems (in America), and certainly the selection of topics by the authors in the first place was not empirical. An addendum could usefully investigate some non-empirical aspects of apparently primarily empirical studies. This would correct the impression that much of this book gives, namely, that all that is necessary for "truth" to be discovered is for it ... to be sieved painstakingly from empirical materials.

RAYMOND APHTORPE


This book comprises eleven essays, ten of which have been published previously in periodicals or symposia and are well known to students of Islam. Three of them have hitherto been available only in English and now appear in English for the first time. There has been some revision for this edition; in particular the annotation has been expanded. The new essay is entitled "An Analysis of Islamic Civilization and Cultural Anthropology." This is, indeed, the theme of the whole book, but by "cultural anthropology" we are not to understand the study of material culture, but "human culture analysis by means of a 'Kulturkreis,'" and this "holds the key position in the organization of sciences ... as the actual hub of such scientific endeavour as serves our overriding existential urge to understand ourselves through an understanding of our cultural place and to understand that place through an understanding of all the cultural answers to the problem of living which have thus far been devised by man." The book is not easy to read but its shrewd judgments are based on very wide reading, by no means restricted to Islamic studies. There are many useful quotations from Muslim works, some of them difficult of access, from which relevant material has been extracted with patient care. Unlike some commentaries, especially those who hope to find in Islam an ally against Communism, the author does not misunderstand the violence with which Muslims sometimes react to criticism of Islamic dogma. He maintains that "the removal of Koran and Prophet from scientific analysis must not be confused with responsiveness to theological issues as such ... When the position of Islamic theology on a matter like predestination is discussed, positions are taken with a view to evidence (sic) the compatibility of the Muslim outlook with modernization and progress; whether irritation is engendered is not for theologians, but sensitivity to the suspected implication that the Islamic ideology constitutes an impediment to national advancement." One small mistake may be mentioned since it is understandable and often made. Lord Cromer was not British High Commissioner in Egypt (p. 193); he was Agent and Consul-General.

C. F. BECKINGHAM


It is a sobering reflection that students of sociology should be thought to require a glossary of no less than 650 terms. The implication presumably is that the sociological significance of these terms cannot be ascertained from standard dictionaries, and the fact that such a glossary should be needed (or be thought to be needed) shows the extent to which the social sciences are nowadays evolving their own separate languages. No doubt where unfamiliar concepts are being employed new terms are needed or old ones must be redefined if confusion is to be avoided, but the exercise is one to be pursued with great restraint.

Mr. Reading's compilation may be useful to students and others who are interested in keeping tabs on this proliferation, but I doubt if it will be of much help to anthropologists. To start with, he defines social anthropology as "the sociology of primitive peoples," a definition which few social anthropologists would accept nowadays. Some of his definitions in this field are just wrong. Commensalism is not a 'relationship between persons or groups with similar goals'; a lineage is not just any 'kinship group with traced descent'; age sets are not the same as age grades; 'classificatory' terms (in kinship) do not include the word 'uncle'; and sorcery is not just 'protective magic.' Others are so inadequate as to be seriously misleading. Explanation is not only 'the reference back of individual facts to general laws'; ritual (pae Leach, in whom this definition is attributed) is not just 'ceremonial' but symbolizes the status structure'; and a symbol is not just 'a thing that stands by convention for something else'—traffic lights are not symbols, at least not for most people.

Of course it would be unfair to expect much detail in a glossary, and Mr. Reading's 'verbal formulae' may be, as he claims, of use to sociology students who need a guide and aide-mémoire to concepts which they have already studied. But anthropologists will derive more benefit from reading anthropology.

JOHN BEATTIE


At last we have a basic text book of Primitive Technology. Although primarily aimed at those with archeological interests it is of only slightly less value for anthropologists and ethnographers. It is a remarkably accurate piece of work illustrated with the author's own clear and informative drawings, and accompanied by an exceptionally detailed index. It is printed in an attractive format at a rather high price.

In the first part of this book Mr. Hodges reviews the properties
of considerable interest, and in all almost cases, it would help a bewildered newcomer to the subject to remember the appropriate terms (see the treatment of the terms ‘boole’ (p. 121) and ‘reed’ (p. 136)).

This book is really concerned with technological principles and the clear definition of terms so that the anthropologist should not be surprised to find very few specific ethnographical examples quoted; there are almost no specific ethnographical examples either. Perhaps one day we will be fortunate enough to get another book from this author dealing with the theoretical implications of his studies, for he is surely not satisfied with such statements as: the beginning of copper-smelting may have been accidental during a camp fire but was discovered only once (p. 68).

The second part of this book briefly surveys the principal methods used to study (but not to date) artifacts. In most cases the corresponding chapters of both parts dovetail well together but one wonders whether it was really necessary to separate them at all. Were the sections in Part II to follow immediately after Part I chapters dealing with the construction of artifacts, this might make for easier reading. In this way certain inconsistencies would also have become apparent and could easily have been remedied: the investigation of mental objects to determine whether they had been made by the lost-wax method does not mention the presence, or plugging, of holes left by chaplets (pp. 73, 220); nowhere is it clearly stated what to look for when examining various ivories (p. 154); it is not clear how to recognize the effects of different dyeing methods (p. 161). In addition various sentences dealing with the examination of materials would not then be out of place in the first part of the book (e.g. pp. 35, 171).

None of these minor criticisms should obscure the fact that this is an excellent work. It is surely bound to be a success with students whether they be studying anthropology or archaeology. In addition it should become essential reading for the social anthropologist who, having published several works on social organization, sits down to write an article on the material culture of his particular tribe. Too frequently in the past these articles have been written without sufficient understanding of the processes observed and have misused accepted terminology. With Mr. Hodges’s book at their side these shortcomings are no longer necessary.

PETER J. UCKO

AFRICA


The transition from ‘textual material’ to ‘literature’ is more than a transition from one state of mind to another. Deprived of their scholarly apparatus, ethnographers’ translations can be disappointingly thin. Folk-tales in too great numbers turn out to be—folk-tales. An honoured and sheltered scientific position is relinquished for the more uncertain world of literary evaluation. As far as the modern West is concerned, it is an irony that ordinary educated appreciation of the literatures to which those of Africa are most quickly assimilable is at one of its lowest ebb. Saxo Grammaticus, the Mabonigong, Talesin, Niebelungs, the collections of the Grims and the very mention of the Catle-raid of Cooley and the ‘rich’ Irish material, all stir a dusty layer covering the world of folk-dance, Yeats and even Madame Blavatsky. The early western literatures are at this moment yielding great depths of unsuspected linguistic and historical information, and have lived on in the last decades as textual material of high value. It is no coincidence that the Chadwicks should be mentioned in these volumes, for Mrs.

Chadwick and the late Professor led the transformation of a rather misty and mystical appreciation of this material into something like a surprisingly reliable stratum of information on the early societies involved. There can be no doubt that the texts themselves have also gained greatly as ‘literature’ as a result, but this literature has now become ‘difficult,’ and is rarely the drawing-room reading it was in the ‘nineties.

Here, then, comes African literature from precisely the opposite direction, hoping to leave the footnoted world and to appear in the educated household in its own right. It will be no surprise if general reviewers show some signs of furtiveness. Some will be vaguely ready to indulge in a little ‘African twilight,’ but will not like some of the obvious signs of this being yet another ‘difficult’ literature. It will be no surprise also if compilers, all at this stage academics, do not always show a sure touch in marrying the scholarly and the aesthetic. But none of this matters. Africa has been publicized for some years as the land of a quintessential ‘pop’ culture—high-life songs, high-life literatures, lightweight philosophies, all things (some excellent in themselves) which need a real stiffening of the not-facile, and rather complicated, older African artistry. Dr. Whiteley with his two volumes had both the easiest, and the hardest task. On the one hand, much of the material was already published form, and of a type not too unfamiliar in the West, if only from previous anthologies; on the other he was largely dependent for the oral literature on the translations of others, and was not able, as were compilers of the other two volumes, to include originals where these were not in English. Dr. Whiteley’s own word-for-word translation of an Irawy text takes us close to what
we are missing. Contrast: "When they arrived, thing which they saw was not there" with the orthodox "When they arrived they didn’t see anything." The reappearance in the myth of Lake Munkamba of the Welsh Pyrenean, and Cameroonien theme of the drowned city, reminds us once again that the universalities of content in stories of this kind will always raise them to a level better called meta-literary than literary.

In Dr. Whiteley’s second volume (written prose) familiar names appear: Achebe, Tutuola, Ekwenzi, Camara Laye and the like. Passages also appear from Gustave Vassa (Equiano), Reindorf and Casely Hayford which should correct any illusions that African writing in English begins in the last fifteen years. Equiano’s of his capture as a slave, in his native Ibo country, comes through his eighteenth century ghost-writer with a tragedy as fresh as yesterday.

With the heroic recitations of Ankole Dr. Morris takes us straight into another sphere, which really could be Celtic, as Professor Hatto says. Particularly striking is a recitation on the Coronation of Gasyonga II by Patrick Kirindi with its tremendous list of those who took part. It has extraordinary parallels with the great list in Cullin ne à Ohun with its mixture of native and imperial realism, and names ripe for transmutation into myth.

The cyclostyled was done by Manaase; Murumba’s letter was taken by Kabwitwana;
The blind came with Nyere;
The widows brought Saliama;…
Among the smiths came Shariff.
Among the mechanics we regretted the absence of Nanyi;
Gupta Singh came with Virani;
Among the miners came Lefournour…
Bawtree arrived with Pain…
The counties put forth Katangi;
The representative of the princes was Mugoha;
Among the clerks we relied on Tbangana;
Among the interpreters came Mungoomya…

Dr. Morris identifies for posterity nearly a hundred of these names, all of real people—as real as Uren Rheged.

Perhaps by the full standards of world literature, the Somali poetry presented by Drs. Andrejeiwski and I. M. Lewis, comes highest in these volumes. In particular the poems of the ‘Mad Mullah’, Hammed ‘Abdille Hasan, survive what must have been a considerable hazard of translation with great power. The Death of Richard Corfield’: words put in the mouth of the British leader of camel constabulary killed by the dervishes in 1913, transcends its own bloodthirstiness:

Say: ‘As I looked fearfully from side to side my heart was plucked from its sheath.' This, the editors soberly inform us, ‘seems to be exaggerated,’ but Corfield is offered here a tragic apotheosis much nobler than the bullet in the head he in fact received. ‘Great shouts acclaimed the departing of my soul.'

EDWIN ARDENER


This book is an example of a system of analysis which can be disastrous, unless handled honestly by an expert such as Professor Lévi-Strauss. Its theoretical theme does not happen to be very original: it is concerned with the interdependence of opposites, male and female, nature and culture, sacred and profane; and therefore it might not seem to deserve a lengthy review. On the other hand, attention must be drawn to the inaccuracy of the ethnographical data on which the analysis depends.

In the first place, the essays are not about the South-Eastern Bantu but about the Venda, and occasionally the Tsonga, of the Northern Transvaal. The author is hardly justified in assuming that situations in a society which, as she admits on p. 42, is typical apply to a whole group of different societies; and she has a confusing habit of using concurrently the indigenous terms of these different societies, even within a single paragraph (e.g. on p. 28).

The author repeatedly claims to be exceptionally well acquainted with the Venda, and stresses that she has been initiated into many secrets. Yet she confuses the common words nanga (reed-flute: horns) and nangga (doctor) on pp. 56 and 98, and regularly translates mudzimu (ancestor-spirit) as ‘God’ (e.g. on pp. 17 and 33): mudzimu was indeed coined by the missionaries for the Christian God, but it is not the word that the Venda use for their own God, then Deity. She translates shango (country belonging to the other) as ‘universe,’ and curiously describes (pp. 66-67) how she asked ‘très crainvivement’ to photograph a musical instrument which she maintains is ‘peu connu,’ and which she incorrectly calls deza. She is referring to the well-known mbila deza madeza ‘hand-piano’ (deza is the name given to the calabash resonator), which is used for ritual purposes by the Lembu clan, but is also played regularly at beer parties. She describes the donba initiation dance as a symbol of the coiling of the python, with its ‘développement en spirale’ (p. 18, etc.): she must have seen that it is in fact a straightforward circle dance which moves, like all Venda dances, anti-clockwise. She translates dzivhuha (pool in a river) as ‘whirlpool,’ so that she can compare it to the python’s coils; and she makes much of stories of babies coming from these ‘whirlpools’ (pp. 18, 27, 91, etc.): the Venda relate pools to ‘la création primordiale’ about as often as Europeans relate storks to the first chapter of Genesis. Some of these ethnographical errors cannot be corrected, however, as they are necessary for her analyses of symbolism.

The first essay (‘Les origines: Le Python. Réalités mythiques et réalité sociale.’) is an elaboration of Malinowski’s contention that myths are charts for social activity, and that daily experience in turn reinforces the myths. Data from the Venda and the Fulani are compared to show that certain rites are the x factor which bridges the gap between mythical and social realities. Thus an ingenious tripartite division of categories is constructed; but it is a methodological fiction, since a rite is surely a social reality in itself. The author lays great stress on the python as a fertility symbol and the donba as an obligatory, pre-marital fertility rite: how, then, would she explain the fact that many legally married women, who already have children, attend donba, but any unmarried initiate who becomes fertile during the ‘fertility rites’ is condemned? Donba is concerned with legalized motherhood and membership of women’s associations. The milayo (laws), to which the author attaches great symbolic importance, are regarded primarily as certificates of group identity: most initiates, and even some instructors, have little idea of their meaning, and in any case the interpretations, and even the milayo themselves, may differ from one area to another.

The second essay is an attempt to relate Venda concepts of life to their arrangements of kinship, political and territorial categories. It presents an over-rigid picture of the physical stages of life that the Venda recognize, and exaggerates the interrelationship of physical and social development (p. 26): puberty does not make a boy a ‘man’ any more than the menarche makes a woman an ‘old lady’; it is marriage that makes a ‘man’, just as it is the birth of grandchildren that makes an ‘old lady.’ The author appears to confuse the Venda and Tsonga puberty rituals: a Venda girl attends khomba (or vhusha) not in her home but in the home of the headman of the district in which she was born, and she only becomes a graduate (mudabe) after the third stage, and not during the second stage (p. 36). Besides, by no means all girls attend the third initiation ceremony in the ideal order khomba, tshikanda and donba. Another tripartite scheme is created by the omission of the office of petty headmen between those of headman and head of household in the political hierarchy.

The conclusion of the third essay, that the Venda are not patriarchal and that both lines of descent are important, is correct, and was clear from the existing literature on the Venda. But the methodology of explaining the kinship terminology in terms of preferential matrilateral cross-cousin marriage is as unsound as some of the ethnographical information. Besides, the preferential marriage is not as common as the author suggests, and in any case patrilateral cross-cousin marriage is also practised.

The fourth essay attempts to reconcile social and natural ‘seasons,’ and trace the underlying duality (dry-wet, hot-cold, etc.) in various aspects of the life of the South-Eastern Bantu. The seasons discussed

The Kandyan Sinhalese, the Tiyar and related artisan castes of Kerala, the Toda and the Tibetans practise forms of fraternal polyandry having much in common. In all the societies the husbands are either normally or invariably consanguine brothers, the number of brothers being limited to two in Kandy, but unlimited in the other groups. Among the Toda, clan brothers may share a wife in the absence of consanguines, and wives are often kidnaped from one individual or from one fraternal group by another. In Tibet, most polyandry is fraternal but an heiress may take a number of unrelated husbands, and in Central Tibet father and son occasionally share a common wife.

Prince Peter lived among these and other groups of the Indian sub-continent in 1938-39, and again for at least six years between 1949 and 1957. The Chinese Communist takeover prevented him from entering Tibet; so he studied the Tibetans of Ladak, Lahul and Rupchut, west of the then Tibetan boundary, and recorded what he could discover of kinship in Central Tibet from interviews with Tibetans in Kalimpong.

The bulk of this long but rewarding work is devoted to a systematic explication of how polyandry functions in the context of household relations in the four societies, and a summary of literary evidence on polyandry from other parts of the world. As a source book it is extremely useful and contains important new information; as a travelogue it is delightfully intimate and often humorous. It is a pity that so long a work, bristling with native terms, should have no index or glossary and no exact references to literary sources, but only a generalized bibliography.

Prince Peter’s account of how polyandry functions is much more satisfactory than are his answers to three other questions, namely: As a form of plural marriage, does polyandry exist? What is its distribution? And, if it exists, what are the reasons for its existence?

The first answer depends on one’s definition of marriage. Prince Peter’s is circular: marriage, he says, is the relationship between husband and wife, involving customary rights and obligations, and in all societies distinguished from other, less favoured forms of union between the sexes. The definition rests on the fact that most

or all societies do advocate one form of union between the sexes as socially preferable to other forms. It is unsatisfactory, however, in precisely those borderline cases of plural unions in which the author is interested. As a result his judgment of whether a particular society is genuinely polyandrous seems too often to depend on fiat or on whether earlier writers spoke of marriage or of lovers.

More specific criteria are mentioned early in the book, namely ‘the procreation of legitimate children’ and ‘public recognition of the union (p. 23), but they are not consistently employed. Both, for example, existed in the traditional Nayar santhamban, yet Prince Peter judges this to have been a form of civiciseism. While his account of the details of marriages in these less-known societies is most illuminating, I am afraid that I still prefer the definition of marriage which I put forward in 1959 (Gough, J. R. Anthrop. Inst., Vol. LXXXIX, Pt. 1, pp. 23-34). For variant forms of marriage which may be engaged in serially or simultaneously by an individual man or woman, Dumont’s distinction between primary and secondary unions could usefully be applied to Prince Peter’s data (Dumont, El’Homme, Vol. I, No. 1, 1960).

The question of why or under what circumstances fraternal polyandry occurs, is also not satisfactorily answered here, although the issues are clarified. It is important, for example, to know that fraternal polyandry is not invariably associated with an imbalance in the ratio of men to women, nor with female infanticide. The author justifiably rejects the notion that polyandry is a necessary stage in the evolution of marriage, or that it is satisfactorily explained in terms of such general concepts as unity of the sibling group. He points out further that true fraternal polyandry does not seem to occur among hunters and gatherers but only where there is significant property in cultivable land or in herds. Finally, in the text of his description the author stresses that all four societies emphasize their desire to limit population in a region or social class possessing limited resources, and that all except the Toda further stress their desire to maintain their standard of living by keeping their family property intact. These motives do not by themselves adequately explain why some peoples have fraternal polyandry while others do not. Nevertheless, I am inclined to think that they are germane in most instances, and that fraternal polyandry is one of a rather small number of ways of limiting both general population size and

MAN

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John Blacking

ASIA
individual family size, in societies having limited but valuable amounts of heritable property and little or no opportunity for expansion. In general I would favour an approach which sees the fraternally endowed family as one form of the stem family or famille-souche (of which, for example, the Nambudiri Brahman family in Kerala would be another).

Prince Peter's conclusion is that polyandry first arises in difficult natural environments requiring great interdependence between men as herders or cultivators. This unusual interdependence enhances the unity of the sibling group and leads to a repression of aggressive impulses between men and to latent male homosexuality. Fraternal polyandry (which also satisfies incestuous desires) results unless there already exist strong opposing norms in the culture. Once institutionalized, the practice persists through cultural inertia unless it is disrupted by external influences. Opposition from outside the society does not, however, always destroy polyandry: a people under attack may retain it, at least temporarily, through a deliberate "nationalistic" attempt to keep their culture distinctive from those of invading populations—as the Toda apparently have done.

These conclusions do not satisfy, for a variety of reasons. One is that all societies live in environments sufficiently 'difficult' to require male interdependence; how do we judge when there is 'very great interdependence'? A second point is that Prince Peter sees polyandry as issuing directly out of a particular set of male personality syndromes, although indirectly out of environmental and economic conditions. Following Yalman's criticism of a comparable argument of my own regarding pre-puberty marriage rites (Yalman, J. R. Anthop. Inst., Vol. XLIII, Pt. 1, pp. 25-58), I would now hold that such psychological 'explanations' of social institutions are invalid. Male homosexual stirvings may and indeed probably often do accompany and find satisfaction in polyandry, but the existence of polyandry as a social institution is to be explained, if at all, in terms of other social institutions or of ecology. Aside from this, a further criticism is of course that homosexual impulses and repressed aggression, being either universal or at least very widespread, are poor indicators of so restricted an institution as polyandry. The theory as a whole, both of the origins and the reasons for retention of polyandry, is so general as to lack predictive value and to be incapable of verification.

These theoretical weaknesses do not detract from the book's ethnographical value and its very substantial contribution to a neglected area of kinship research. Nor do the strictures on psychological theories of institutional origins preclude keen appreciation of Prince Peter's next major work, on the personality characteristics of the peoples he studied. KATHLEEN GOUGH


The Chinese original of this book, based on lectures delivered by Professor Ch'iü at the National Yunnan University during the war, has since its publication in 1947 won so good an opinion that it is not surprising that it has been decided to make the work available to a wider public. This translation is by the author himself, and incorporates various improvements and modifications.

The law with which Professor Ch'iü is dealing consists essentially of the statutes of the various imperial dynasties, illustrated by their application as recorded in the collections of decided cases. These latter are so voluminous, and the author is so thoroughly acquainted with them and indeed with all the relevant literature that he has amassed a body of information on social life in the old China the interest of which will reward even the most general reader for not being deterred by the severely sinological apparatus in which it is enclosed. Yet it is hard to agree with Professor Hulsewé who says in his Foreword that the author 'pays the greatest attention to customary law.' Like nearly all Chinese scholars, Professor Ch'iü seems to entertain little doubt of the practical efficacy of the dynamic codes and the other institutional books, and his sources by their nature show the statutes are enforced, though even of his examples it makes clear that, in the domain of what we should call private law, conduct in open breach of statutory provisions was often not regarded as socially reprehensible until extraneous circumstances arose which took the parties before the courts. Such for instance was the case with the illegal marriage in 1798 of a man with his sister-in-law, which was accepted throughout the neighbourhood and was even acknowledged by a local constable (pp. 98 ff.). Only when years later the woman was murdered by a son from her husband's first wife, and the court had to classify the degree of relationship, was the validity of the marriage called into question.

To take another example: Professor Ch'iü says (p. 104), 'A wife owned no property. Usually, she received a limited sum of money for household expenses and this she used as she saw fit but she had no power to dispose over the family property.' This certainly was the ideal. Yet the Japanese in their investigation of actual conditions among the Chinese of Formosa discovered that savings from household expenses, which over the years might amount to a considerable sum, were deemed to be a wife's personal property. And we have a deed from the middle of the last century in which a vendor declares he is selling a piece of land in order to repay, from the purchase money, the principal and interest of a loan made to him by his concubine.

These points do not in any way lessen the value of Professor Ch'iü's work which after all is not designed to be a comprehensive treatise on the private law. As an exposition of the social ideals and policy of the traditional Chinese state the book is altogether admirable.

HENRY MCALEAVY


This account of life in a North Indian village, first appeared in 1930. The present edition contains three additional chapters by Charlotte Wiser, written after she and her husband returned to Karimpur in 1960.

The Wisers, missionaries whose first stay in Karimpur lasted five years, were pioneers in the field of Indian village studies; Gerald Wiser's monograph (The Hindu Jajmani System) which resulted from the research done at Karimpur was the first systematic analysis of the jajmani system. The Wisers wrote Behind Mud Walls to include those of their personal experiences which they felt had no place in the sociological study, but which they nevertheless thought instructive and revealing. As such the book is directed to a wider audience than that of the professional anthropologist or sociologist. The Wisers' account is sympathetic, but unsentimental; the method of presentation is informal and anecdotal. The reader is presented with a series of vignettes of selected facets of social life which, while they do not produce a totally comprehensive picture, contain many valuable and penetrating insights.

When the Wisers returned to Karimpur the most impressive changes which they noted were those directly related to the technological and other innovations for which the Community Development was primarily responsible. In her postscript Mrs. Wiser not only points out the impressive achievements of the Indian government's programme, but also indicates some of the difficulties involved in implementing its policies, and shows how divergent interests and factionalism often inhibit co-operation among fellow villagers for the common welfare.

Social relationships have proven relatively stable. The family remains the primary focus for the individual's interests and loyalties; kinship ties are still critical, and the caste hierarchy fundamental. But the jajmani system is breaking down. One infers that this is in large part due to the withdrawal of patronage on the part of many of the jajmans, who now prefer to obtain in the town many of the goods and services formerly provided by their kaminis. However, there have also been important changes with regard to land ownership, but the significance of these in relation to the jajmani system is not made clear. There has been continuity in community leadership; most of the new leaders, although younger men, are close relatives of former leaders. However, it is emphasized that the attitudes of the younger generation are more receptive to innovation and change than were those of their forefathers.

CHARLES McDOUGAL

This book on the Gallongs is the sixth in the series of monographs on the tribes of the North East Frontier Agency of India, and it is pleasant to record that the administration of N.F.E.A. has been successful in maintaining the initial impetus of publishing this series of basic ethnographical accounts intended to cover all the major ethnic groups of the Indo-Tibetan and Indo-Burmese borders. The inspiration of Verrier Elwin is discernible in this no less than in the other volumes, but within the overall pattern of the series, authors are free to vary the emphasis laid on individual aspects of tribal culture.

Mr. Srivastava's interests seem to lie mainly in the sphere of social structure and organization. The most interesting chapter is the one on marriage, for this contains the first account of polyandry as prevailing among any of the tribes of N.F.E.A. Before the author studied the Gallongs even the occurrence of polyandry in this tribe was virtually unknown and this is borne out by the fact that Prince Peter's great compendium on Indian and Tibetan polyandry contains no reference to its prevalence among the N.F.E.A. tribes. Srivastava is probably right in assuming that there is no direct connexion between Gallong and Tibetan polyandry. Unlike Tibetans and such Bhotias as the Sherpas of Nepal the Gallongs do not emphasize the desire to reinforce the unity of brothers as the principal incentive to the conclusion of polyandrous marriages. Indeed, whereas I have observed that among Sherpas it is just the wealthiest families which favour polyandry as the marriage form most likely to perpetuate their high economic status, among the Gallongs younger brothers contend themselves with sharing their elder brothers' wife as long as they are too poor to afford the heavy bride-prices for wives of their own. There is also the difference that among the Gallongs the wives of younger brothers are added to the household and shared by the eldest brothers, while in the case of Bhotias the marriage of a younger brother to a wife of his own usually involves his separation from his elder brother and the establishment of a separate household. Gallongs conclude also truly polygynous marriages, and the combination of polyandry and polygyny seems to result occasionally in a family-type which might be described as group-marriage.

In a general ethnographic description such as that demanded by the character of the series, there was obviously no space to explore the problems of this marriage-type in depth, and it is to be hoped that Mr. Srivastava will take another opportunity to publish his data on marriage and kinship in full. The rest of the book contains competent descriptions of the Gallongs' material culture, agricultural methods and ritual practices, all of which conform basically to the patterns characteristic of most of the tribes of the Siang and Subansiri areas. There are some excellent photographs and a great number of line drawings. At Rs. 6.50 (less than 10s. od.) the book is extremely good value.

C. von FÜRER-HAIMENDORF


I recommend this book as the best introduction to modern society and politics in Malaya and Singapore. It is clearly set out and well written and of its 21 chapters, three deal with the period after 1945, so that its coverage of the last 20 years is fairly comprehensive.

The author was both an administrator and an economist in Malaya (for some 10 years) and his experience and balanced views in both these fields serve the reader well. The four chapters on economic factors (The Management of the Economy; The Growth of the Economy; Rural Development; The Wage Earner) give a clear and helpful picture. The fact that Mr. Guellick lived and worked in the country he writes about carries a constantly recurring bonus in perceptive insights and minor pieces of information—each of which is a distillation of intangible factors and atmospheres experienced and assimilated during residence in the country but too rarely expressed in its literature.

The book is free from any sort of colonial nostalgia, and usefully traces the earlier political and constitutional developments which made the road to Merdeka a smooth one, as well as some of those leading to the creation of Malaysia. The thumb-nail sketch of Tunku Abdul Rahman (pp. 121-2), to my mind, exactly hits off his magic mixture of endearing geniality and instinctive understanding of basic issues. The conclusions on the critical issues facing Malaya—for instance on defence, the new Federation and rural development are realistic and encouraging without ever being cynical. Taken in conjunction with the slightly austere, if authoritative, style they make this book an earnest and impartial contribution to our knowledge of Malaya. There are good statistical appendices, another on the intricate subject of citizenship and nationality and a critical if short bibliography; also a map and index. The format and print are agreeable and the whole reasonably priced.

J. C. BOTTOMS
It is impossible to understand the world in which Zinancantecos live—how the deities in the celestial order are connected with the social order, how individual Zinancantecos interact with one another and with their natural habitat, or even how an individual becomes ill and then either recovers or dies—without a precise understanding of their two ‘soul’ concepts: ch'ulel and chamul. I use the term ‘soul’ advisedly and in quotes to indicate at the outset that the familiar European concepts of ‘souls’ and ‘spirits’ are inadequate for precise ethnographical description of these concepts. In a very general way, of course, these Zinancanteco ‘souls’ signify that the people are ‘animistic’ in the Tyolian sense of the term; they also signify local manifestations of widespread Middle American beliefs about tonalism and nagualism that have been discussed by Foster (1944) and Correa (1966), and more particularly for the Highlands of Chiapas by Holland (1961) and Villa Rojas (1963). But these statements tell us too little about the complex and subtle meanings implied in these crucial Zinancanteco concepts. Furthermore, there is still a vast amount of ethnographical confusion in the usage of these two Middle American terms. Both tonal and nagual are Nahua terms, and neither term is used when the Zinancantecos are speaking Tzotzil among themselves. And, as Correa (1966) points out, the term nagual has come to mean two things in a confusing way in the literature—the ‘guardian’ or ‘familiar spirit’ and a person who transforms himself into an animal to harm another person. Finally, neither tonal nor nagual as concepts define precisely enough the Zinancanteco belief in a ch'ulel, or ‘inner soul.’

In this article I propose to describe these Zinancanteco ‘souls’ utilizing Tzotzil concepts, and to discuss briefly their significance for social structure, for social control and for man’s relation to the natural world.

Zinancantecos are a Tzotzil-speaking municipio in the Highland Maya area of Chiapas, Mexico, where I have been engaged in field work each season since 1957. The municipio, with an elevation of 6,000 to 8,000 feet, and a 1960 population of 7,600 Indians, is located just to the west of San Cristobal Las Casas. The settlement pattern is typically Maya (Vogt, 1961) with ceremonial centre and outlying hamlets. About 800 Zinancantecos live in the densely settled valley in which the ceremonial centre is located; the other 6,800 live in the 11 outlying hamlets. The ceremonial centre contains the Catholic churches, the cabildo, and a plaza where markets are held during important fiestas. A series of sacred mountains and waterholes which figure importantly in the religious life are located in and around this ceremonial centre (Vogt, 1964a). The most important feature of the social structure in the centre is a religious hierarchy with 55 cargo positions in four levels. These cargos are filled on an annual basis with the cargo-holders moving with their families into the ceremonial centre to live during their terms of office, then returning to their hamlets to farm corn during the rest periods between cargos (Cancian, 1964). The social structure of the hamlets is based upon patrilocal extended families, localized lineages and waterhole groups composed of two or more of the localized patrilinages (Vogt, 1964b).

The Zinancanteco ch'ulel is an inner, personal ‘soul’ located in the heart of each person; it is also found in the blood which is known to be connected with the heart. The ch'ulel is placed in the body of an unborn embryo by the totilme'lilelik, the ancestral deities who live in the sacred mountains around Zinancantan centre.

This Zinancanteco ch'ulel has some very special attributes. It is composed of 13 parts, and a person may lose one or more parts and require a special curing ceremony to recover them. But the ch'ulel, while temporarily divisible into parts in the various kinds of ‘soul-loss’ that can occur, is believed to be eternal and indestructible. At the point of death the ch'ulel leaves the body. It is associated with the grave for a period of time corresponding to the length of the deceased person’s life on earth, and then rejoins the ‘pool’ or ‘supply’ of ch'ulel-lelik (-etik is a suffix indicating the plural in Tzotzil) that are kept by the ancestral deities. It is later utilized for another person. But while the person is alive, the ch'ulel as a unit can leave the body during sleep and go visiting with ch'ulel-lelik of other Zinancantecos or the deities. It can also ‘drop out’ of the body temporarily in periods of intense excitement, as in sexual intercourse. The ch'ulel tends especially to leave the body of a small child, as it is not yet used to this new receptacle. One of the most consistent behaviour patterns for a mother with a small child is for the mother to sweep the ground (on which she has been sitting for a time) with her shawl as she leaves, thereby making certain to gather up all the parts of the ch'ulel of her infant. Likewise, parents are expected to treat a small child with utmost care and affection, lest its ch'ulel become frightened and leave. One of the major purposes of the baptismal ceremony is to ‘fix’ the ch'ulel more firmly in the child’s body.

However, even the baptism does not prevent shiel (‘soul-loss’) from occurring through fright later in life (see Gillin, 1948). There are many immediate causes for shiel: falling down suddenly, seeing a pukuh (‘evil spirit’) on a dark night, etc. The Zinancantecos still relate vividly how a large number of people experienced shiel when the first aircraft swept low over Zinancantan about 20 years ago; apparently the hiloletik (‘curers’) were busy for weeks gathering up pieces of ch'ulel-lelik following this event! But at a more profound level of causation shiel is ordinarily believed to be due (a) to the totilme'lilelik who punish bad behaviour by causing a person to fall down, or

* The colon (:) appearing in Tzotzil words has the value of a ‘glottal stop’; the upper comma (,) signifies that the immediately preceding consonant is ‘glottalized’.
more dramatically by sending a lightning bolt to knock out one or more parts of the ch’ulel; or (b) to an evil person who performs witchcraft ritual in a cave to ‘sell’ one or more parts of a victim’s ch’ulel to yahval balanil (‘earthowner’). Yahval balanil then proceeds to use the victim as a servant.

Without all 13 parts of the ch’ulel, a person cannot be healthy; he feels or possesses chamel (‘sickness’). A h:ilol must be summoned for diagnosis and curing. The diagnostic procedure involves pik ch’ichi (‘touch blood’) or ‘pulsing’ of the patient. When the h:ilol feels the pulse, it is believed that the blood ‘talks’ and provides messages which he can understand and interpret and thereby determine whether or not shi:el has occurred. If so, the h:ilol performs a ceremony called lok’esel ta balanil (‘to extract from the earth’) to recover the lost parts of the ch’ulel and place them back into the body of the patient.

The phenomenon of the ch’ulel is by no means restricted to the domain of human beings. Virtually everything that is important and valuable to Zinacantecos also possesses a ch’ulel: domesticated plants, such as maize, beans, and squash; salt, which possesses a very strong ch’ulel; houses and the fires at the hearths inside the houses; the wooden crosses erected on sacred mountains, inside caves, and beside waterholes; the saints whose ‘homes’ are inside the Catholic temples; musical instruments used in their ceremonies; all of the various deities in the Zinacanteco pantheon, etc. The ethnographer in Zinacantan soon learns that the most important interaction going on in the universe is not between persons, nor between persons and material objects, as we think of these relationships, but rather between ch’uleletik inside these persons and material objects.

The second type of ‘soul’ is the chanul which is a kind of ‘animal spirit companion,’ or ‘spiritual alter ego,’ so to speak. Rising to over 9,000 feet to the east of the ceremonial centre is a majestic volcano called bankikal mukt’a vits (‘older brother’ or ‘senior large mountain’). A series of supernatural corrals containing 7,600 chanuletik, one for each person in Zinacantan, are located inside the mountain. One corral contains jaguars, a second one coyotes, a third one ocelots, and fourth one smaller animals like opossums. These chanuletik are watered, fed and cared for by the totilme:iletik, under the general supervision of the supernatural mukt’a alkalte (‘great alcalde’) who is the celestial counterpart of the highest ranking member of the religious hierarchy in Zinacantan. This house is located inside the mountain and his household cross is the shrine that Zinacantecos visit for ceremonies on top of the volcano.

The connexion between the Zinacantecos and their chanuletik is made in the belief system by the concept that each person and his chanul share the same ch’ulel. This means that when the totilme:iletik install a ch’ulel in the embryo of a Zinacanteco, they simultaneously install the same ch’ulel in the embryo of an animal. Similarly, the moment a Zinacanteco baby is born, a jaguar, coyote, ocelot or other animal is born. Throughout life, whatever happens of note to the Zinacanteco happens to his chanul and vice versa. If, for example, a chanul is let out of the corral and left to wander alone in the forest, he may be injured or shot and then his Zinacanteco ‘companion’ feels the same injury. It follows that if the chanul is let out of the corral and is thereby not being cared for properly by the ancestral deities, a Zinacanteco is in mortal danger. Such a state is also diagnosed by the ‘pulsing’ procedure of a h:ilol who must proceed with dispatch to perform the proper ceremony asking the pardon of the totilme:iletik and persuading them to round up the lost chanul of the patient and place it back in its supernatural corral.

It is ordinarily sometime during childhood when a Zinacanteco discovers what kind of chanul he has, and he usually receives this knowledge in a dream when his ch’ulel is off visiting and ‘sees’ his chanul when he visits the ancestral deities inside bankikal mukt’a vits. Like the transmission of their names, the Zinacantecos believe that the ‘souls’ are also transmitted in the patriline. In fact, the names and the ‘souls’ move down in the patrilineage as a package. They do not pass from father to children, since it will be remembered that a ch’ulel stays with the grave of a dead person for the period corresponding to the length of a person’s life on earth. Hence, if a young child dies, the totilme:iletik can use the ch’ulel and its associated chanul quite soon again. But if an old person dies, it will be many years before the name and ‘souls’ can be used again. But when they are used, they are placed with a new member of the same patrilineage. And this transmission of the name and ‘souls’ makes the new member of the patrilineage a k’esholil (‘replacement’ or ‘substitute’) in the system for the departed ancestor (see Montagu and Hunt, 1962, for comparative data on other Chiapas communities).

This concept of k’esholil is replicated in many other parts of the social and ritual system. For example, when a black chicken is sacrificed during a curing ceremony, it is a k’esholil or substitute for the patient to prevent the totilme:iletik from taking away the patient’s ch’ulel. When an incoming cargo-holder in the religious hierarchy takes over his duties from the outgoing cargo-holder, he is the k’esholil or ‘replacement’ in this ceremonial system.

There is a clear relationship between the beliefs in these two ‘soul’ concepts and social control in Zinacantan. For anything that stirs up the wrath of the totilme:iletik against a particular Zinacanteco can lead quickly and directly to punishment (a) by causing the person to lose one or more parts of his ch’ulel by some form of shi:el, or (b), in more serious cases, by having his chanul turned outside its corral to wander alone and uncare for in the woods. The types of deviant behaviour that can lead to these soul troubles include, significantly enough, the breaking of the important moral codes or the flouting of the central values of Zinacantan. For example, an individual who fights with or mistreats his kinsmen; a man who fails to accept community service as a cargo-holder in the religious hierarchy, especially if he is known to possess the necessary resources to expend on these ritual duties; a man who fails to care for his cornfield properly or a woman who mishandles the corn after it is brought to her house; a person who fails to bathe regularly and change into clean clothes; or a man who fails to make contributions when the officials
arrive to collect the 'taxes' for fiestas—are all prime candidates for chamel caused by 'soul' troubles.

These 'soul' troubles immediately involve curing ceremonies to appease the wrath of the ancestral deities, and the ceremonies in turn have important social and economic effects. In the prayers to the ancestral deities asking for pardon, the patient freely expresses his guilt, and is quite obviously relieved psychologically after the ceremony is over. The ritual patterns require the cooperative presence, without interpersonal friction, of the kinsmen with whom he may have been quarrelling. The ceremonies involve purificatory bathing and putting on freshly washed and incensed clothes. They also stress the respect, the sharing and the proper etiquette that should always be displayed between individuals in Zinacantan society.

The economic effects are also important. If the patient is a wealthy man and has failed, for example, to accept or to perform a cargo duty properly, the costs of the ceremony in chickens, rum, incense and food have the immediate result of relieving him of his surplus. So if he does not expend his surplus as he should in community service, it is, in effect, 'confiscated.' For by the time he finishes a series of curing ceremonies, he might as well have done his cargo duty as he should have in the first place.

Further, Zinacantan, like other close corporate communities described by Wolf (1955), has patterns of institutionalized envy. This envy is especially triggered off by the possession of what other Zinacantecos define as excessive wealth. So, even if the ancestral deities do not punish him, his fellow Zinacantecos will, and they will do so very effectively by beginning to gossip about how the wealthy man must have acquired his riches in an unethical way—by selling ch'uleletik to the 'earth-owner.' When gossip reaches a sufficient level of seriousness, the wealthy man finds himself in a bind and begins to worry about his own actions and his own health. The result is again a series of expensive curing ceremonies that relieve him of his surplus and reestablish a more equal distribution of the wealth.

Finally, there is a fundamental way in which the concept of the chanul relates Zinacantecos in an intimate way to the world of nature. In Tzotzil a clear distinction is made between ta naze tik, the areas that are used and controlled by human beings such as the houses, the patios, the waterholes, and the fields, and tes tik, the wild, wooded slopes of the Chiapas mountains which are not under human control (Acheson, 1962). All of the chonceletik are chonettik ta tes tik (literally 'animals of the forest'). It is significant that all of the areas designated as ta naze tik are enclosed ritually by ceremonial circuits or processions that are performed for the localized lineages, for groups of lineages living around waterholes, and for the cornfields located at some distance from their dwellings. Beyond these areas that are enclosed by ceremonial circuits are the wooded, mountainous areas where the chonettik ta tes tik roam and whence come various birds of evil omen to warn the Zinacantecos of impending sickness and death brought from the tes tik. The ceremonial circuits make annual reaffirmations of this basic dichotomy in the Zinacanteco world. The concept of the chanul fosters a connexion between these dichotomous parts of their universe. It creates an intimate relationship between the lives of men and the lives of wild animals that inhabit the wooded slopes of the mountains. And in these same high mountains dwell the ancestral gods who survey the affairs of their Zinacanteco descendants and exercise maximal control over their behaviour by either care or neglect of the 7,600 chanuleletik upon whom living Zinacantecos absolutely depend for continuing survival.

Note
My field research in Chiapas is supported by a ten-year grant (MH-21000) from the National Institutes of Health, and is sponsored by the Laboratory of Social Relations and Peabody Museum at Harvard University. I am grateful to my colleagues and students—especially Nicholas Acheson, Merida Blanco, Nicholas Bunin, Susan Carey, Nancy Chodorow, B. N. Colby, Lorc Colby, Frank Cancian, Francesca Cancian, George Collier, Jane Fishburne Collier, John Early, Matthew Edel, Robert Laughlin, Mimi Laughlin, Victoria Reifler, Daniel Silver, Jack Stauder, Susan Tax Freeman, James Warfield, Allen Young, Manuel Zahala Cubillos and Michelle Zimmerman—who have all done field work in Zinacantan and have contributed importantly to my understanding of the culture. For additional data on Zinacantan in Spanish see Evon Z. Vogt (editor), Ensayos Sobre Zinacantan, Colección de Antropología Social, Instituto Nacional Indigenista, Mexico, 1965; and in English, my forthcoming monograph entitled Zinacantan: A Maya Community in the Highlands of Chiapas.

References
Some Glass Beads from the Malay Peninsula. By Alastair Lamb, Senior Fellow in History, Australian National University, Canberra. With four figures and two tables.

Tom Harrisson's paper in a recent issue of MAN is of the greatest interest in adding to our all too scanty stock of chemical analyses of South-East Asian glass beads. The figures which he has published, like those which I published in the Federation Museum Journal in 1961 and which were also based on the work of the Eastern Mining and Metals Co. assay laboratory at Dungun, Trengganu, Malaysia, do seem to require, however, some careful interpretation before they can be exploited to throw light on the history of the Asian bead trade. It is hoped that the following observations, based on my study of glass and beads from the Malay Peninsula, will help in such an interpretation.

The first point to establish is the significance of the figures which the Eastern Mining and Metals Co. assay laboratory has provided. In the case of my bead analyses, and also, so J. N. McHugh has informed me, in the case of those of McHugh and Harrisson, very small samples were submitted to the laboratory which, in most cases, confined itself to the study of a single bead in each category. How representative is such an analysis of the total bead population, as it were, from any one site? Intrigued by this question, I submitted for analysis on separate occasions a number of visually identical beads from the same site. The variation in composition between individual beads, always assuming, the reliability of the analysis, is interesting. Take, for example, the standard red opaque bead so common in South-East Asia, the 'mutisalah,' sometimes erroneously described as a terra-cotta bead. In Table I are analyses of such beads from Pengkalan Bujang and Kuala Selinsing, A1 and A2 being from Pengkalan Bujang and B1 and B2 from Kuala Selinsing. Each pair was carefully selected so as to be alike as possible in shape, size and colour.

<table>
<thead>
<tr>
<th></th>
<th>A1</th>
<th>B1</th>
<th>B2</th>
</tr>
</thead>
<tbody>
<tr>
<td>SiO₂</td>
<td>60.5</td>
<td>69.5</td>
<td>67.0</td>
</tr>
<tr>
<td>Al₂O₃</td>
<td>16.2</td>
<td>15.1</td>
<td>15.7</td>
</tr>
<tr>
<td>CaO</td>
<td>3.1</td>
<td>2.4</td>
<td>4.7</td>
</tr>
<tr>
<td>MgO</td>
<td>0.05</td>
<td>0.13</td>
<td>trace</td>
</tr>
<tr>
<td>Cu</td>
<td>0.44</td>
<td>0.50</td>
<td>1.3</td>
</tr>
<tr>
<td>Na₂O</td>
<td>12.7</td>
<td>9.4</td>
<td>2.0</td>
</tr>
<tr>
<td>KO</td>
<td>2.6</td>
<td>1.5</td>
<td>6.0</td>
</tr>
<tr>
<td>Mn</td>
<td>0.006</td>
<td>—</td>
<td>trace</td>
</tr>
<tr>
<td>Fe</td>
<td>0.05</td>
<td>0.25</td>
<td>2.7</td>
</tr>
<tr>
<td>Pb</td>
<td>—</td>
<td>0.50</td>
<td>1.2</td>
</tr>
</tbody>
</table>

It will be seen from the table that all four samples are of basically the same kind of glass, in detail there is considerable variation between them. There is a significant difference in silica content between A1 and A2, in alumina content and in potash content between B1 and B2. In fact, B2 resembles A1 rather more closely than it does B1, yet there is a considerable distance both in time and space between Pengkalan Bujang and Kuala Selinsing. I had a number of pairs of this kind analysed, and the result suggested that, as far as the Kuala Selinsing and Pengkalan Bujang sites were concerned, there were variations between pairs as great as there were variations between sites, so that chemical analysis could not be used, by itself, as a criterion for distinguishing individual Kuala Selinsing beads from individual Pengkalan Bujang beads. Perhaps, had it been possible to produce figures for the average of several hundred samples from each site, then a significant statistical difference might have emerged: it did not, however, emerge on the samples actually submitted for analysis. Variation in the chemical composition of otherwise similar glasses is, in fact, only to be expected. As Turner has shown, the weathering of glass results in changes in its composition, the increase, for instance, of its silica content; and we may suppose that even in the confines of a single site there does not apply an absolutely constant weathering process.

The one fact to come out beyond doubt from these analyses was that the Kuala Selinsing and Pengkalan Bujang samples, with a very few possible exceptions, fell into the glass category which might be described as Middle Eastern or Roman-Hellenistic-Byzantine, a glass with a very low lead content and an absence of barium. On this ground it has been suggested that these beads had a generally Middle Eastern or Mediterranean origin. Can this be substantiated? My research at Pengkalan Bujang in Kedah, Malaysia, followed by work at Kota Tinggi in Johore, Malaysia, at Kuala Selinsing in Perak, Malaysia and at Ko Khau Khoi (or Kakao) near Takuapa in South Thailand has produced strong evidence to suggest that many of the beads from these sites were made locally from a raw material which contained a high proportion of melted-down Middle Eastern glass scrap. While excavating at Pengkalan Bujang in 1961, I came across a number of partially finished or spoiled beads. I also discovered a large number of glass fragments which have now been shown to be glass-house waste. Most significant, in this respect, were fragments of punty-cap, which can only arise out of the process of glass-manufacture. How did these punty-caps, of a glass of clear Middle Eastern type, get to Malaysia? There were not, it seems more than probable, made in Malaysia. It seems likely that they came there as part of a cargo of glass scrap intended as a raw material for local bead-manufacture. If this conclusion is correct, then it should cause no surprise that so many South-East Asian glass beads have an essentially Middle Eastern composition. Moreover, this line of reasoning would go far to explain variability in bead composition, reflecting no more than variations in the batches of imported glass scrap, which may have originated in Iran, Iraq, Arabia or Egypt. Once I had identified punty-caps at Pengkalan Bujang, I had no difficulty in finding fragments of punty-cap at Kuala Selinsing, at Ko Khau Kho in South Thailand and at Kota Tinggi in Johore, indicating a wide spread of the scrap glass trade. Ko Khau Kho and Kota Tinggi are about 800 miles apart at the opposite ends of the Malay Peninsula, and between them there must be a time span of about 600 years.

If scrap glass from the west was being imported into Kota Tinggi, a site where, on ceramic and other evidence, we can suppose occupation from the fifteenth century onwards well into modern times, then it is possible that scrap glass continued to be
used in South-East Asia as a raw material for bead-manufacture in European times. If so, then we may perhaps have an explanation for the lead content of Tom Harrison’s beads from the Kelabit uplands of Sarawak other than the supposition of a Chinese origin. The Kelabit beads published by Harrison have compositions very similar to some of the European flint or ‘crystal’ glasses where a high lead content has brought about good refractive properties. We may, perhaps imagine broken decanters of officials of the Dutch and British East India Companies finding their way into the bead trade just as did Middle Eastern glass scrap at an earlier period.

Research at sites like Kota Tinggi in Johore makes it clear that by the opening of the European era in South-East Asia the bead trade had assumed considerable complexity. At Kota Tinggi can be collected the old range of South-East Asian trade beads, including the red opaque ‘mutisalah’, alongside ‘Roman’ beads and beads made by blowing and moulding techniques of the late Medival and early modern European glass houses. Some of the glass beads from Kota Tinggi are clever imitations of semi-precious stone material like carnelian, agate and jade, others imitate amber and yet others imitate, not very successfully, the old ‘mutisalah’. At Kota Tinggi I even acquired two glass beads made to resemble human molar teeth. One of the ‘Roman’ beads, a chevron, which I obtained from Kota Tinggi resembles very closely a ‘Roman’ bead which van der Sleen illustrates in a recent paper in MAN as a product of the Amsterdam bead factory in the seventeenth century. In a bead situation like that at Kota Tinggi it is unlikely that typological or chemical classification will be simple.

For the pre-European period, if we accept the implications of the punty-caps from Pengkalan Bujang and elsewhere, it may well turn out that detailed chemical analysis will not prove to be particularly informative beyond indicating that the bead falls into the general ‘Middle Eastern’ category, for which it may perhaps prove possible to assign some chronological limits. Probably other criteria will have to be considered on a statistical basis. For example, given a sufficiently large sample, the percentage of beads of different colours might have some significance as indicating an aspect of local demand which might be expected to vary from time to time and place to place. In pre-European times, as Tom Harrison rightly observes, monochrome beads form such a large percentage of any assemblage as to make it possible to ignore the polychromes in any statistical analysis. I have been able to work out the colour percentages of monochromes from Pengkalan Bujang and Kuala Selinsing on the basis of quite large samples which have been collected, especially in the case of Pengkalan Bujang, under controlled conditions. The Pengkalan Bujang sample, all excavated in 1961 from a single cutting through an extensive midden deposit, was 4,525 glass beads (in which category has been included a small number of beads of earthenware). The Kuala Selinsing sample, collected by me and by J. N. McHugh in 1962–63 from the surface of the shell mound and surrounding mangrove, was 848 glass beads. Both sites yielded small quantities of stone beads, agates, rock crystals, carnelians and so on; but these have been omitted from the sample. In both sites monochromes were in the vast majority (the Kuala Selinsing sample, for example, includes only a single polychrome bead). The percentages of the various colours in these two samples are shown in Table II.

<table>
<thead>
<tr>
<th></th>
<th>Pengkalan Bujang (Sample of 4,525 beads)</th>
<th>Kuala Selinsing (Sample of 848 beads)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Red opaque (‘mutisalah’)</td>
<td>31.5%</td>
<td>29%</td>
</tr>
<tr>
<td>Blue, light and dark, opaque and translucent</td>
<td>30%</td>
<td>nil</td>
</tr>
<tr>
<td>Turquoise opaque</td>
<td>1%</td>
<td>nil</td>
</tr>
<tr>
<td>Black</td>
<td>17.5%</td>
<td>0.5%</td>
</tr>
<tr>
<td>Yellow opaque</td>
<td>13%</td>
<td>13%</td>
</tr>
<tr>
<td>Orange opaque, some with black cores</td>
<td>2.5%</td>
<td>14%</td>
</tr>
<tr>
<td>Green opaque</td>
<td>2%</td>
<td>11%</td>
</tr>
<tr>
<td>Earthenware</td>
<td>0.5%</td>
<td>nil</td>
</tr>
<tr>
<td>Misc., including polychromes</td>
<td>1%</td>
<td>0.5%</td>
</tr>
</tbody>
</table>

These are interesting figures, for they show clearly three major differences between the Pengkalan Bujang and Kuala Selinsing assemblages. Black, practically unknown at Kuala Selinsing, is the third most common category at Pengkalan Bujang. Orange, quite rare at Pengkalan Bujang, is the third most common category at Kuala Selinsing. Green, even rarer than orange at Pengkalan Bujang, is fairly common at Kuala Selinsing. Were it possible to prepare tables of this kind, based on good samples, for a greater number of South-East Asian sites, the significance of these differences might perhaps become clearer. It is to be hoped that Tom Harrison will publish some such
figures for his very impressive collections of glass beads from Sarawak.

Notes

1 Harrison, 1964.
3 These ‘mutisah’ beads were discussed at great length in Rouffaer, 1890. See also van der Hoop, p. 170; van Heerden, 1958, pp. 40f. ‘Mutisah’ are still highly valued in Timor, and van der Hoop describes the recent excavation of such beads from ancient burials in Sumatra for sale to the inhabitants of Timor. These beads, though in fact made of glass, resemble in appearance a dark red terracotta, with something of the colour and texture of some Roman ceramics.
4 Turner, 1954 (Part II).
5 I am much indebted to Dr. Donald Harden of the London and Guildhall Museums for information about pumy-caps. The put, or pontil, is the metal rod to which a glass vessel is transferred from the blowpipe so as to enable work to be carried out on the rim. The glass piece is attached to the putty by a blob of glass, which is broken off when the putty is reused. A pumy-cap fragment looks something like a bit of teapot spout incompletely pierced. Inside it shows the space occupied by the putty, and at its end it shows fracture marks where the glass piece has been broken off.
6 Some of the glass fragments from Pengkalan Bujaq originated, so R. Pinder-Wilson of the British Museum suggests, from Iran or Iraq. Some fragments which I collected from near Takuwa in South Thailand Ray Smith has identified as being of Egyptian origin. It seems likely, in view of what is known of early trade in the Indian Ocean, that glass also came from ports along the South Arabian coast where the sites of old glass houses are known.
7 The lead-barium criterion for Chinese glass applies, in general, to glass of the Han Dynasty period. Detailed information on more recent Chinese glasses is defective. See Beck and Seligman, 1938; Turner, 1956 (Part IV).
8 For information on flint glasses and ‘crystal,’ see Morey, 1954.
9 No full account of the beads from Kota Tinggi and Johore Lama, Johore, Malaya, has yet been published, though something can be learned from Gardner, 1937. Gardner’s description of the beads which he collected at these sites is based on the report made by W. Beck. I recently came across this report in the archives of the National Museum, Kuala Lumpur, Malaya, and have edited it for publication in a forthcoming number of the Journal of the Malay Branch of the Royal Asiatic Society, along with a description of a collection of beads made at these sites in 1962-63.
10 Van der Sleen, 1963.
11 J. N. McHugh visited Kuala Selising in 1962 and early 1961. During 1961, accompanied by B. A. V. Peacock and Lord Medway, I visited the Kuala Selising sites on three occasions. On his departure from Malaya, J. N. McHugh left me with the beads that he had collected at Kuala Selising along with the laboratory reports on them.
12 These figures are taken to the nearest half.

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Rock Gongs and Associated Rock Paintings on Lolui Island, Lake Victoria, Uganda: A Preliminary Note.

By George Jackson, J. Stephen Gardtan and Merrick Posansky, Makerere University College, Uganda. With three figures.

Rock paintings have been reported from Lolui Island by Posansky (1961), a rock gong and paintings from other parts of Uganda by Lawrence (1953, 1955 and 1958) and by Wachsmann (1957) and Lanning (1958 and 1959). Previously no rock gongs had been discovered on Lolui Island.

Lolui is the southernmost island of the Butembe-Bunya archipelago and lies a little over 20 miles from both the Uganda and Kenya coastlines, to the north and east respectively. In comparison with many of the other previously or presently inhabited islands like Buvuma, Rusinga and Sigului, Lolui Island is relatively isolated as the waters around the island are up to 25 fathoms deep, open and subject to sudden storms. Its last permanent inhabitants were evacuated in 1958 at the time of the sleeping sickness (trypanosomiasis) epidemic.

Geologically it consists of weathered and exfoliated pre-Cambrian granites which give rise to large numbers of remnant tors where rock overhangs and clusters of perched boulders abound. The island is a little under five miles across along its east-west axis; the area is of the order of between 11 and 12 square miles.

The vegetation of the island is coastal fringe rain forest, which in several rocky areas extends back into the hinterland as relic forest. This has probably never been completely cleared. The major inland area is a derived grassland dominated by only four species of grass. Towards the island margins this grassland is studded with vegetation clumps consisting typically of a small number of monkey-dispersed species, namely Saba florida (Benth) Bullock, Harungana madagascariensis Lam. and Vitex fisheiri Guerke.

During an ecological expedition to the island in May, 1964, extensive surveys covering the vegetation and fauna, including the very large population of Vervet monkeys (Cercopithecus aethiops L.), which have been studied in detail by Gardtan since 1953, led two of us to cover the island thoroughly and in the course of the surveys to find two sets of rock gongs among the tors. A further well defined set of gongs was found at the end of the expedition when the archeologist joined the team and there are other less well defined gongs elsewhere on the island.

The finest of the gongs is situated near the centre of the island near to the start of the south promontory. This consists of a large rounded boulder, concave inside, which rests on a large flat rock, the roof is supported at one side by a large, flatish rock which in turn rests on the floor; the three rocks form a kind of roofed and open-ended cave with a flat, polished floor. The smaller of the flat rocks can be termed a support in the narrower
sense of the word and is also the largest of the gongs, the striking facets, 21 in number, being for the most part well defined depressions in the upright faces of this rock. The other gongs are grouped together at the north end of the cave and consist of six rocks with both depressions and abraded edges as striking points. In all there can be recognized 39 well used striking facets and edges. Faulting or weathering has removed a further concave face from the roof rock at the entrance to the cave; on this protected site certain paintings in red ochre are found. These are not so striking as those described from the previous site on the island. They consist of three well defined crosses, one large one with equal components of approximately 15 inches, and two small ones with one component significantly longer than the other. Associated with these crosses are numerous ochre dots and smears, some of which may be hand prints.

The second set of gongs is located amongst the prominent rocks which separate the north-west promontory from the rest of the island. Here the gongs are a series of split rocks which are more or less vertical and played along the upper edges. In some cases small pieces of rock may have been wedged into position by artificial means. They are less accessible than the first set and have no closely associated floor.

In common with the first and third sets they are roofed over with a large perched boulder which no doubt acts as a sounding board. On a concave facet in the northern face of the roof rock, a series of schematic rock paintings executed in red ochre was found. This set was extremely elaborate but had also been subjected to some weathering, so that freehand copies were made, but direct tracing was found to be impracticable. The motifs include crossed dumbbells, a series of concentric circles and other motifs described from the previous group of Lolui paintings.

The floor of the cave is polished as if well used and the largest subsidiary gong has a well defined patina or incrustation on one face which suggests that it may have been played with bare hands rather than with stone hammers. The incrustation is in such a position, on top of the gong, as to be unlikely to have been left by the feet of people climbing over it. It rings when struck by the palms of the hands.

The third set of gongs is located in a group of rocks which form part of the same ridge in which the last set was described, but 200 metres or so to the south-west. This is the least elaborate of those described. Here there is a typical horizontal wedged rock with an abraded edge and a subsidiary gong. It was perhaps of interest to note that associated with these gongs were two carefully adzed wooden boards (30 and 42 inches long) which were split at one end into two tongues and which appeared to have been abraded by being struck, possibly with other wooden instruments. These boards when jammed under a rock and played with sticks gave a musical tone. No paintings were associated with this gong. Several small hammerstones made from various volcanic rocks not found on the island, as well as quartzite hammerstones
found in the crevices of the rocks at this and the other sites, must have been used as beaters.

The gongs currently described from Lolui compare very favourably with gongs already described from other parts of Africa and the large number of striking facets associated with the first group must bring it into the category of one of the finest on this continent. As such it will be studied later by an expert musicologist and described in detail.

All the currently described sites are located in situations providing a clear view over a considerable part of the approaches to the island.

Lolui Island has a long history of occupation, judging from the vegetation and from the signs of human activity. Though no stone-age assemblages have been found, a single piece of imported obsidian with a scraper edge is evidence of a possible Late Stone Age occupation. Dimple-based ware, of which a large collection was made during the Mau-Mau emergency and is now in the Coryndon Museum, cord-rouletted ware, Entebbe (Posansky, 1961), and fairly recent Jaluo wares are all common on the island. The soils show signs of past and recent erosion, but also, in contrast, attempts at soil-conservation by terracing. Similar terracing has been picked out on several other islands like Buvuma from a series of aerial photographs taken by Dr. W. Deschler and have been observed on the ground by members of the Geological Survey, indicating a former, more intensive arable agriculture than was practised on the mainland. There are numerous grinding holes for millet all over the exposed rocks, and many more are often hidden under re-established vegetation. There are stones, set upright along their narrow edge, laid in lines throughout the grasslands which may have demarcated former land holdings and at least three cairns made up of broken querns. One of these is of unusual interest in being ovoid to rectangular in shape, some eight feet high and up to 30 feet long. Garlan has also found an engraved rock, ovate in shape and approximately five feet along the long axis, and three feet along the short axis, near the forest fringe; the pecked designs of this resemble the rock paintings. He has also found a rock-cut mueso board of four lines of eight holes on top of one high boulder.

There are fishermen on the island who were questioned about the gongs. They include Basoga, Baganda and Kenya coast inhabitants but none could give any clear evidence of knowing about the gongs or how they were played. They were regarded as things which former old people knew. A close survey of the island will no doubt reveal other sites, since on the point of the north-west promontory a group of rocks did have what might have been a little-used solitary gong. Also there are many striking groups of rock formations, both hidden in the forests and on the south promontory which have not yet been visited.

Acknowledgments

We are indebted to the Director of the Fisheries Research Laboratory, Jinja, Mr. Peter Jackson, who provided the use of a launch, to the Medical Research Council and the Wenner-Gren Foundation, who provided Garlan and Posansky respectively with grants for transport and to Mr. Gilbert and to Mr. and Mrs. M. Mann of the Fisheries Research Laboratory for supervising the launch.

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A Professional Tattooer in Beirut. By Professor John Carswell, Art Department, The American University of Beirut. With five figures

The practice of tattooing in the Near East is an ancient art: it has been noted in Egypt as far back as the Xth Dynasty; by Xenophon amongst a tribe living near the Black Sea, and by Herodotus amongst the Thracians in classical times; and even in the Old Testament. Further east, a frozen corpse excavated from an intact Scythian burial in the High Altai was found to be covered with tattooed designs. In the Near East today, tattooing survives amongst the nomadic and semi-domesticated tribes of Arabia, Syria and Iraq, the peasant population of Egypt, including the Copts, and, particularly, amongst the Coptic, Armenian and other orthodox pilgrims to Jerusalem, for whom it has never lost the popularity which it attained in medieval times as a gesture of faith and souvenir of pilgrimage successfully accomplished. However, outside of these tribal and religious groups it is rare, and the fact that a professional tattooer still practises in Beirut in a basically urban and very mixed environment is unusual enough to merit attention.

The Beirut tattooer, Mohammed Khalil Ghoshtian Diab, works in the brothel quarter of Saifi, squatting on the pavement: 31 years old, Palestinian in origin and born in Jaffa, he has lived in Beirut since 1948. He is the only professional tattooer in Beirut and his clients come from as far away as Tripoli in northern Lebanon and Zahleh in the Bekaa valley.

It might be assumed that he learned his craft from other tattooors in his native town, Jaffa; as a flourishing port, it would have been a likely place to find them. On the contrary, he had never heard of tattooing until the Second World War when he enlisted in the British Army at the age of 13 by falsifying his age, and he learned the technique from an Australian. Nor was he aware of the professional tattooors in Jerusalem, for Christian pilgrims.

Fig. 1. The Tattooer at Work

Photographs: J. Carswell
The word he uses for 'tattooer' is daghgh shāmi and for 'tattoo' either dāgh or shām.

His clients are mainly the young men who patronize the brothels. He says he has work whenever he sits down; this is true, for as soon as he starts tattooing a small, interested crowd of spectators and potential clients immediately materializes (fig. 1). He estimated that in a full working day he would tattoo between 35 and 50 people. His prices range from 1 to 5 Lebanese Pounds (35-155) according to the complexity of the design and the client; once a foreigner gave him 50 Lebanese Pounds for two initials. In spite of the flourishing street trade, he maintains that he tattoos more women than men. Some of them are the local prostitutes, but the majority are women who send for him so that they can be tattooed in their own homes.

His technique is the traditional one. Once the design is chosen it is traced onto the skin, previously shaven smooth, and pricked in with a set of three needles bound closely together in a wooden holder. The needles are constantly dipped in a dish of ink. The ink so suffuses the design that it is surprising that he can make out what he is doing. Three is an unusually small number of needles for such a tattooing instrument, but he says that it is better for following a tight curve in the design. He uses two sorts of ink, red and black; the red is diluted powdered mercurochrome and the black a mixture of soot, soap and a little water, with alcohol added as an antiseptic. When the tattooing is finished it is washed; oil is sometimes rubbed in to soothe the flesh and make the design stand out more strongly. The discomfort lasts an hour or so and he denied that his tattooing was ever painful. He works fast and lightly and none of his clients seemed to grimace unduly.

His method of tracing the designs is simple. Clients choose a tattoo from his 'catalogue,' a collection of 30 or 60 designs drawn in pencil on individual slips of cloth (fig. 2). The chosen design is then outlined on the reverse side with an indelible pencil and traced on the skin with a point. If necessary the faint purplish outline is retouched with a pencil. The designs are his own creation, very crude without the charm of being primitive. Sometimes he works from photographs, optimistically maintaining that he can produce an exact likeness. Most of his subjects are predictable considering his pitch; he caters for both Moslems and Christians, with appropriate designs for each. This is a list of his subjects:

- Sword and heart
- Cedar of Lebanon
- Red Indian
- Crucifixion
- Forked sword
- Star and crescent
- Lion and sword
- Skull and crossbones
- Sailing ship
- Boxer

 FIG. 2. A SELECTION OF THE TATTOOER'S PATTERNS, DRAWN ON SCRAPS OF CLOTH
Bird and heart
Syrian eagle
Mosque
Weight-lifter
Soldier
Lovers kissing
Crossed swords and crown
Venus de Milo
Dancing girl
Hand holding lighted torch
Lion and sword
Cross and heart pierced with arrow and dagger

Strong man
Mermaid
Horse's head and horseshoe
Pyramids
Swords, crossed, and palm tree
Swords, crossed, 'Ilhāb Mohammad between
St. George and Dragon
Birch and branch
Women's heads
Helmeted warrior with cloak

The frequency of sword designs is an indication of their popularity with Moslem clients. The sword with a forked blade (fig. 3) is called saeef zil fiqwar or saeef Imam al Ali, and is tattooed as a sign of allegiance to the Shi'ah sect. A seventeenth-century Turkish miniature in the British Museum (fig. 4) shows a Persian Dervish tattooed—or possibly cicatrizated—with a similar forked sword, on his forearm. The tattooer gave the following explanation of the motif:

‘Imam al Ali was the cousin of Mohammed and the only chieftain of the Moslems; he was appointed by God as a conqueror, not to be vanquished by anyone. This sword, saeef zil fiqwar, and his horse, al maymoon, were sent to him from heaven on a palm tree. The sword had never been worked by a blacksmith, nor had the horse ever been conceived of a mare. A single gesture of the sword by Ali towards his enemies would immediately split them in two—hence the forked blade. The sword is still in Yemen, the property of the King of Yemen, and people still go on pilgrimage to see it. After the death of Mohammed there was a conflict over the succession; his closest friend had been Abu Bakr as Sadiq, but his nearest relative Imam al Ali. Thus the fanatical followers of Imam al Ali, the Shi'ah sect, tattoo the forked sword to show their love of him. The followers of Abu Bakr, the Sunnis, tattoo another design of two curved crossed swords with the words 'Ilhāb Mohammad between the blades' (fig. 5).

The mermaid design was copied from a picture belonging to a Greek sailor who requested it. The tattooer referred to it as 'aroos al Bahr, the bride of the sea, and maintained that sailors still catch mermaids in the sea every two or three years. The motives of his clients in being tattooed can be classified as religious, nationalistic, prophylactic, curative, magical, or simply decorative. Religious designs are chosen as a mark of faith both by Christians and Moslems and are invested with considerable importance in a country like Lebanon, which is intensely concerned with religious differences. National allegiance is expressed in designs such as the Syrian eagle, the Cedars of Lebanon, etc., and his repertoire contains a few portraits of prominent political figures, such as Gemal Abdul Nasser.

Prophylactic tattooing is an occasional part of the tattooer's trade. Usually concerned with strengthening weak limbs, a workman might have a band tattooed round his wrist to strengthen it; the tattooer maintained that it would act as a sort of shock-absorber, to guard the nerves against fatigue. He emphasized that the design was of secondary importance and it was the actual pricking that was effective. He used an analogy of a tank, armoured against shells. When questioned about the practice of tattooing birds on each side of the head as a guard against headaches he said that he had heard of it but it was only used by noorie (gypsies) in Egypt.
the fly has settled, that is, the right one. He told of one case of an old lady who had been bedridden for years who was completely cured after she had been tattooed by him. He thinks that the actual pain of the tattooing absorbs the shock of the complaint. After locating the area where the 'bad' blood has accumulated, which is the cause of the malady, tattooing extracts the stagnant, sick blood, the circulation is re-established and the pain vanishes. He again stressed that the design was unimportant and he dismissed tattooing for magical reasons as rubbish. He constantly referred to himself jokingly as hakem, or doctor.

Fig. 5. Tattoo Design: Sunni Crossed Swords, with 'Ilah Mohammad' Between

Whilst I was talking to the tattooer a young man, complaining of an ineradicable itch, was tattooed on the shoulder blade. The tattooer manipulated the area until he decided he had found the right nerve and then tattooed him there. The design in this instance was a long cross with a triangular base; the man was a Christian. First drawing the design lightly, he then drove the needle deep into the flesh, far deeper than for the ordinary decorative designs. He said it was necessary to draw a lot of blood, and on this occasion the client was in great pain and had to clench his teeth in order to stop himself from crying out aloud.

If the patient does not specify any particular design the tattooer draws anything that comes into his head, appropriate to the client's age. Tattooing commissioned by lady clients might be considered as mildly 'magical'; it is usually for furthering a love affair. The tattooer was pleasantly sceptical about its results.

Simple decorative tattooing for young men and visiting sailors is to demonstrate the manliness and virility of the client, and accounts for the weight-lifting and strong-man designs. There is also much tattooing for sentimental reasons, of the names of mothers and sweethearts. However, the tattooer did remark that there is no quicker way to begin to hate someone than having their name tattooed on you. He explained that the name of the person becoming mixed with the blood of the person tattooed, a family relationship was established, and any suggestion of physical love between the two persons would thus have incestuous implications. Thus the result would invariably be hatred, the reverse of the intention.

Notes

1. L. Keimer, *Remarques sur le tatouage dans l'Egypte ancienne*, Cairo (Institut Français d'Archéologie Orientale), 1948, Plates VI–IX.
4. Levitica, 19, v, 28; 21, v, 3; Deuteronomy, 14, v, 1; Revelation, 19, v, 16.

Traditional Designs in Some Modern Farm Tools. By J. Geraint Jenkins, *Welsh Folk Museum, St. Faans* 33

It may be argued that the student of material culture should limit his study to that of 'pre-industrial' technology and that he should not be concerned with 'the overlay of sameness borne over a large part of Europe by the great tide of an industrialism which ignored both national and community boundaries'. It is not always realized, however, that the products of the machine age are often based on designs that have remained unchanged for centuries, and that in some fields of modern production, standardization has not progressed as far as one would have expected. A number of current, general trade catalogues show a vast array of tools and implements that can be bought at present. Many of those could well fit into a museum's representation of eighteenth-century craft workshops.

A number of specialized tool catalogues, too, show patterns of agricultural and craft tools that have hardly changed at all in centuries. Tools for currying leather, for cask-making and lead-moulding, for example, are similar in shape and design to those used by Roman craftsmen, while the medieval countryman would be quite familiar with some small agricultural tools manufactured by large-scale manufacturers at the present time.

It is clear that modern mass production has failed to standardize the types of tool and implement used by craftsmen and land workers at the present time, and any manufacturer concerned with supplying their needs must pay due attention both to the ingrained traditions of localities and to the conservatism of many trades. Attention must be paid, too, to the specific needs of regions from the point of view of topography, soil and vegetation. In the nineteenth century Ransome, the well-known East Anglian manufacturer of farm implements, realized the importance of these factors, and in producing ploughs his firm produced a great variety of mouldboard and share, based on the multiplicity of regional design. In the sixties and seventies, Ransomes employed a team of horses and a skilled ploughman to take part in ploughing matches throughout the country. In this way they learned of local needs by practical experience.
When village craftsmen were concerned with tools and implements for their own localities, they took into full consideration such features as soil and vegetation as well as the traditions of their localities. Thus a wheelwright in the Vale of Berkeley made wagons that were well adapted to the natural conditions of the Vale. He provided his blue-painted wagons with greatly dished double-straked wheels, as befitted a clay land district; he provided them with tall, nearly upright fore and tail ladders to carry the heavy hay crops of this dairying region and since the roads and lanes of the district are generally straight, the locking capacity of the wagon was not a first consideration. The yellow-painted Cotswold wagons on the other hand were entirely different. Here, the wheelwrights built their wagons as light as possible, as befitted a region with many abrupt changes of slopes. Lock was extremely important and small nearly horizontal fore and tail ladders were fitted. These were designed to carry corn sheaves rather than hay and the wide vehicle was designed for a rolling countryside where sheep-raising and cereal-growing formed the basis of the economy.

But local designs, adapted to the needs of the various localities of Britain, could not only be seen in ploughs, wagons, harrows and other large implements but they were also displayed in the variety of hand tools made by country craftsmen. Estyn Evans in describing the Irish spade for example states: 'The precise requirements of each group of townlands forming a neighbourhood unit resulted from the balance of many physical human and economic factors such as conditions of soil and slope, methods of digging, types of crop, length of arm and leg cemented by usage and sanctioned by tradition. The 'spade gauge book' of a County Tyrone factory, which has lately closed, lists some 230 different patterns... and it served only Ulster and the west.'

A tool that varied greatly from district to district was the billhook; a tool described by Richard Jeffreys as 'the national weapon of the English labourer.' In the past, billhooks, like other farm tools, were made by village blacksmiths for the farming population of their own immediate districts, and, since each tool was designed for dealing with a specific type of vegetation and local conditions, many hundreds of different types must have existed. Not only did the shape and size of blade vary tremendously, but the distribution of weight in each tool also varied, as did the shape of the handle. On the Suffolk coast, for example, billhooks made by craftsmen in such places as Orford and Woodbridge were designed primarily for cutting edges and banks mainly below the level of the worker. Consequently, the blades were heavier at the front and each tool was equipped with a fairly long, rounded handle. To deal with rough sedge and rushes as well as woody roots, the Suffolk billhook had a slightly convex blade with a short straight blade at the end for dealing with those roots. The billhooks of Breconshire, on the other hand, were designed primarily for laying hedges of straight hazel, willow and thorn, and consequently the pistol-handled tool required, had an almost straight blade. Leicester billhooks were wide and consequently their blades were made much thinner to cut down the weight. Each tool had two blades; the one sharply curved almost to a sickle shape to cut the lush vegetation of its district, whilst the other at the back was straight and was used to cut the thorn tufts occasionally found on the grassy banks of the Midlands.

The shape of the handle and the method of hafting also varied considerably from district to district. In some places such as West Carmarthenshire, where briar and thorn predominate, long, turned handles which kept the worker's hand well clear of the prickers were preferred, while in many other districts, such as Oxfordshire, the billhooks were equipped with caulked or pistol-shaped handles. While most varieties were fitted to handles by tongs and metal ferrules, some billhooks were socketed and

the rounded projection at the back of each blade was nailed to the handle. This was particularly important in billhooks such as the Suffolk, where the type of vegetation demanded a long cutting edge that could be continued as close to the handle as possible.

In addition to the regional styles of general-purpose billhook, there were also those designed for a specific trade. For example, the wattle-hurdle-maker required two types of billhook. First, he required a short sharply curving hurdles bill, designed specifically for splitting hazel rods, perhaps no more than two inches in diameter. Hurdling bills again varied from district to district, and those used by Dorset craftsmen differed in detail from those used by Hampshire and Berkshire wattle-hurdle-makers. In addition wattle-hurdle-makers required another tool: a specially adapted bill with a concave blade ending in a cutting edge some three inches wide. To trim a finished hurdle, a short cudgel was thrust under the end to be trimmed and with a sharp blow of the billhook the projecting rod was cleanly cut. Another specialized hook, which again varied from district to district, was the sparrowhook. This, with a concave cutting edge no more than seven inches long, was especially designed for cleaning and trimming thatching spars.

In the nineteenth century, particularly during the last quarter, village craftsmen who supplied a distinctly local market with all its requirements were disappearing very rapidly. Blacksmiths, in particular, ceased to be the essential craftsmen that they had been from time immemorial, for their work was being taken over by large-scale manufacturers in Yorkshire and the Midlands. Representatives of these manufacturers travelled widely and supplied ironmongers in market towns and villages with their products. But in small farm tools they did not produce standardized products but still clung to traditional local patterns and still maintained the old local names. For example, Isaac Nash and Sons, Wollaston Mills, Stourbridge, were producing the following spades in the eighteen-nineties:


Each spade could be obtained in a variety of different sizes with one of eight different pattern of handle. There were Crutch, Clasped crutch, Oval crutch, Eye, Riveted eye, Double-riveted eye, Malleable eye and Long knob.

Even today, spade design has not been standardized and there is an amazing variety of tools, many of them of a pattern that has not changed in hundreds of years. The 1962 catalogue of Messrs. Edward Elwell, Wednesbury, for example, illustrates the following spades that may still be purchased:

- Garden spades (18 varieties), Guernsey spades (2 varieties), Gloucester spade, Nurseryman's spade, Somerset Marsh spade, Bristol spade, Transplanting spade (2 varieties), Mote spade, Irish spade (2 varieties), Cheshire spade, Yorkshire spade, Norfolk spade, Jersey spade, Lincolnshire spades, Lug spade, Lady's spade (4 varieties), Fen spade, Border spade (2 varieties), Open socket spades (2 varieties), Bristol spade.

But of all small farm tools the greatest variety still exists in billhook design. Messrs. Edward Elwell for example illustrate 34 in their 1962 catalogue. They are:

A. Tanged Single-Brace Billhooks: Newton (2 varieties), Stafford, Pontypool, Rutland, Norfolk, Knighton, Rodding,

Within the last few years there has been a certain amount of standardization with some well-known patterns with a limited demand going out of production. Elwell's catalogue of 1946, for example, shows the following patterns in production in addition to those produced at present:


Going back even further to 1899, one firm produced as many as 91 different patterns in sizes from six to 12 inches. They were as follows:


These examples from catalogues did not represent the whole range of billhooks manufactured in the late nineteenth and early twentieth centuries. One catalogue dating from the early nineteen-hundreds quoted by Sayce7 says 'To show every pattern of Bill and Broom hook [i.e. double-edged hooks] we manufacture would make this section of the catalogue very confusing and we therefore show only those patterns that are most generally used in their various districts. We do, however, still make any pattern hook and if the pattern which customers have in the habit of buying is not shown here, we shall be pleased to supply them.'

While, on the one hand, manufacturers have been adding to their range of patterns throughout the present century, on the other hand some patterns have disappeared and some measure of standardization has been attempted. For example, Nash's 1890 catalogue does not show the Aberaeron billhook amongst its range of products, for at that period the well-known smithy in the Cardiganshire seaside town was flourishing and its socketed billhook, with an oddly shaped projection at the back, was in constant demand among the farmers of West Wales. When the smithy stopped making billhooks in the nineteen-twenties the pattern was taken over by Brades, Nash & Tyrion and appears as the 'Aberaeron Billhook' in their recent catalogues. On the other hand, patterns such as the wide and clumsy Machynlleth billhook have gone out of production, its place being taken by the well-known Newtown pattern, the design for which can be traced back to a type made by village blacksmiths throughout North Wales.

Any measure of standardization is therefore a very slow process.

Mr. F. H. Middleton, Sales Director of Messrs. Edward Elwell Ltd., Wednesbury, writes in a letter of 6 March, 1964: 'We continue to manufacture the different types as the traditional demand is still very strong. If we tried to sell a Kent billhook in your part of the world, we doubt if we should succeed. On the other hand, there are one or two patterns such as the Newtown and Stafford billhooks that sell in many parts of the country. It might be an excellent idea to attempt standardization, and undoubtedly we shall have to do this in due time, but quite frankly there is an inherent danger in trying to force something upon people that differs from what they have normally purchased. Whilst we have a strong hold on this market, this should never be taken for granted as any refusal to supply might bring competition back again.' It may be added that ironmongers in Wales and the West of England have said that they would find it impossible to sell any other than the local pattern, and it may be suggested that variety has been preserved not only by the stability of local needs but by the conservatism of the countryman. The rural worker still insists on the familiar tool of his neighbourhood and large-scale manufacturers will not, perhaps dare not, introduce a standardized tool. Custom and tradition have dictated that a particular type of tool is best for a particular region and persistence in billhook design provides an excellent example of the persistence of ancient tradition in a most unexpected place.

Notes
5. Ibid., pp. 182-7.

Sacred and Profane: Some Thoughts on the Folk-Urban Continuum of this Dichotomy. By C. Renate Barber, D.Phil., Institute of Social Anthropology, Oxford

When Émile Durkheim in his Forms of the Religious Life developed the dichotomy of sacred and profane this added a new dimension to the understanding of religious and supernatural phenomena which was elaborated in Hertz's study of the right-hand—left-hand juxtaposition and had echoes in the study of burial rights which Hertz demonstrated to be a transition from this life to the next.

Whilst working in the predominantly pre-literate society of some sophistication (Yoruba of the Egbe group north of Abeokuta, Western Nigeria), where at least three types of religious thought were recognized (Christianity, Islam and Paganism), it occurred to me that the sacred-profane concept only arose as an intellectual opposition where participation was defective. In other words, in a situation of total involvement with, and in, the religious life of a society, the sacred and profane aspects merge and demarcations from one to the other are much less clear-cut than they would be to the non-participating, non-involved outsider.

To illustrate this we need but look at our own religious past. In Victorian England, God was not confined to Church where He was to be addressed on Sundays only or perhaps even to be encountered merely at the rites de passage of birth, marriage and death, with possibly baptism and confirmation in the more convinced cases. The Victorian father was head of the family unit worshipping together. He read out from the Bible and led
prayers, the same person in this family thus fulfilling the role of secular and spiritual leader. The weekly menu as a matter of course adhered to the principal of eating fish on Fridays, without, we may be sure, either the housewife or cook being always very aware of the religious significance of this custom. One does in fact wonder whether very closeness to the 'sacred' does no automatically blur the sharp distinctions from the profane: if a girl wears a golden cross round her neck she must no doubt view it as ornament as much as symbol of her faith; and does her vexation with this same object should it break or be lost not show her very matter-of-factness in dealing with it? This must diminish the 'awe'—to use Marett's term—with which a symbolic object is usually approached if it is much less a part of one's daily circumstances.

All this applies very much more in the 'universalistic' society where the normative values of the group make it impossible for the individual to distance himself. When participation is universal and permeates all aspects of daily life the total involvement of such a society and of the individuals in this group will tend to diminish the polarity of sacred and profane.

It is this variability of involvement that is responsible here for the fuzziness of the borders between sacred and profane. It is a truism that the insider is not necessarily aware of the issues at stake in, say, a political controversy. Such involvement may have a physiological dimension, as for instance, in one's personal assessment of health. When one has been ill (especially chronically ill), the experience of 'good health' recedes and statements as to one's relative well-being is in relation to illness rather than health experience. That is, the assessment is not an objective one, it is totally coloured by the involvement in sickness and the comparative objectivity of approach is lost without realization that this has happened.

Thus where the Supernatural is part of everyone's life, where the shrine occupies the centre of the compound, or in some cases, the middle of the living space in the house, where women have idols in their rooms, the remoteness of the gods must necessarily be less than where divinity is but a conceptual entity. Ancestorworship is eminently a participating religion both in the sense that it commits the group as a whole by uniting the roles of secular leader and religious head in the person of the lineage elder and by being a continuum of the living and the dead (the elder also of course is nearer to the departed than are young people). Because of this continued association one finds often that changing status from being alive to being dead is a gradual dissociation and may involve burial and disinterment after a time, the intellectual reasoning being that at first the spirit is unwilling to part from old companions and remains in or near the village until eventually identification with the next world is completed.

Thus the transition from contemporary to ancestor is seen as a gradual one, as a discontinuation and slow disengagement. As involvement in the world of the living decreases so the sacredness of the deceased increases until great reverence is ultimately paid to one common eponymous ancestor, whose interest in his descendants is historical rather than actual. In cases of illness where causation is seen as an expression of dissatisfaction by the ancestral spirits for neglect of sacrifices, such obligations usually only occur towards, or are felt to be called for by, ancestral spirits whose lifetime was within living memory of the threatened groups. But even when the departed spirit has finally been identified with the next world there remains as an integral part of ancestor-worship the continuum of living and dead and terms of commitment to and involvement with the 'religious' aspects of life which pervades all everyday, profane activities.

The all-inclusiveness of ancestor-worship presupposes a tightly knit community of universalistic values, where conformity rather than individualism is at a premium. Thus the separating-out of individuals, compartmentalization and ego orientation that are concomitant with effective division of labour cannot be accommodated within its framework. Only the concept of a High God is compatible with a face-to-face relationship between man and the divinity. It is possible to worship a High God outside a community, divorced from the ancestral lands and shrines. Religious practices no longer depend on community action. Infringements of religious laws no longer affect a whole group of people but have repercussions on the individual only, who thus does not any more have to yield to the pressure of a community who feel themselves threatened by his very lack of devotion.

In this way religion is taken out of the sphere of common concerted action and relegated to a private world where each man 'makes his own peace with his maker.' This change over from public to private concern also emphasizes religious polarity. Whereas the all-pervasiveness of communal religious life tended to blur the distinction between sacred and profane (because religious acts were so much part of everyone's life and so much performed in the public eye), a relationship with a High God that is realized in private and at set times and places is much more likely to distinguish between the profanity of temporal events and the sacredness of religious action and places. If one postulates that as religious familiarity decreases there is an increase in the sacred-profane dichotomy, it follows that this polarity is negatively correlated with participation and involvement in the religious life, i.e. the more religion is a reality to be lived with and by, the less likely it is to be clearly differentiated into compartments of relative sanctity.

The correlative conclusion that follows from the realization that universalistic total-involvement patterns make for the pervasiveness of religious elements in all aspects of life is that developments towards modern society will bring in their wake a change over from universalistic religions to individualistic ones of a personal relationship to a High God.

It is perhaps for this reason that, in general, the progressive elements in African countries are recruited from among Christians and, as it is already becoming increasingly evident that old patterns of extended-family recognition must necessarily recede as it becomes economically and spatially impossible for one man to cater for more than a nuclear family, so ancestor-worship as the religious bond of the group also is waning at least in city environments. It remains to be seen whether it will be replaced by Christianity and/or Islam which fit in with the needs of individualistic organization, which can be demarcated from everyday life and will thus give rise to a clear distinction between sacred and profane.

References

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A Diviner's Apprenticeship and Work among the Bayaka.

By Professor Hugo Huber, Institut Froideville, Posieux, Switzerland. With a figure

The idea of the interaction of forces plays an important part in the sentiments and ritual practices of the Bayaka of the South-west Congo. These forces are believed to be attached to specific statuettes and 'medicines' which, in Ki-kongo are called *mi-kisi* (sing. *nkisi*). A number of them are
regarded as protective as well as destructive. Functionally speaking, they help to explain the ills and misfortunes of man and, at the same time, are supposed to avert them or to heal them. The ritual specialist called in to detect and identify these forces and to reveal the dangerous effects of their activities as well as the protective measures to be taken, is called nganga ngombu. 3 

Vocation

A nganga ngombu usually has his personal story of how he came to be a diviner. The following, told by my informant, is said to be typical: One day, long ago, he had bought a dog. Returning to his home, he suddenly became aware that his dog had disappeared. A diviner who was consulted in this matter revealed that one of his ancestors once had stolen a dog, and that his own dog was missing because he had acquired it, not as his ancestor did, by theft, but by purchase. Meanwhile, nothing else happened until one day he fell ill. He was lying under a tree, when all of a sudden, he caught sight of two civets. He was afraid and called to his people for help. They killed the animals. During the following night his late uncle appeared in his dream, revealing the story of the ancestor’s theft and explaining that he himself was that ancestor who had stolen a dog from the chief of Bakantu—who had given him hospitality—and that by doing so he had drawn that destructive power of ngombu into his clan. As a result, one of the family members was taken ill. It was disclosed that ngombu had come upon him, and that this was a sign of his vocation to becoming its devotee, and that his illness could be healed in this way only. The patient thus recovered and became the first nganga ngombu of the clan. This was revealed to my informant by his late uncle, who also told him that he himself had sent the two civets to provide the medicine bag for him, since he was chosen to become the new nganga ngombu in the clan. Moreover, his uncle placed two snail shells into his hands which, awakening in the morning, he really felt touching his fingers. When, on the same day, his wife was washing her feverish body, suddenly some strange power came upon him and made him jump to the roof of the house. The people shouted: ‘A ngombu, a ngombu has taken possession of him!’ A nganga ngombu had to be called before the patient was able to come down again. That was how his vocation to renew the ngombu medicine in the clan and to be its new diviner was confirmed.

Training

The apprenticeship that followed took one year. A separate hut fenced with palm branches was built for him and a ngombu tree was planted in front of it and tied to its roof by means of a fibre. During the first three months an experienced nganga from the neighbourhood stayed with him. It was his task to teach the novice (mwana) how to prepare the ngombu medicines and to initiate him in the other functions of a diviner. The training was mainly practical.

To learn the composition of the ngombu medicines, the mwana was taken by his teacher to the bush where he had to search for the peculiar ingredients. These comprise the following:

1. madingu: a medicine composed of the head of the honey bird and of a parrot called biolo, and also of a tail feather from the latter and of some herbs; all of these things are kept in a small bag.
2. ngombu: this is the name of a species of grasshopper whose head is worn by the diviner on his neck. From the fact that the spiritual force which is supposed to take possession of a diviner is named by the same term ngombu, this appears to be one of the essential parts of the whole equipment. It also explains why,

Fig. 1. Diviner’s drums (mukoko) and ceremonial axe (kangala)

specific way of beating the drum in order to announce his coming. The axe, often covered with a decorative pattern of cowrie shells, has an ornamental wooden handle. He carries it on his neck as outward sign of his office. He may also use it in pointing out a culprit.

6. finally, the mwana has to learn a new language called Ki-ngombu. The roots of the words are partly the same as in
Ki-yaka while some of them are different. This is illustrated in the following samples:

<table>
<thead>
<tr>
<th>English</th>
<th>Ki-ngombu:</th>
<th>Ki-yaka:</th>
</tr>
</thead>
<tbody>
<tr>
<td>father</td>
<td>kipufula</td>
<td>tata</td>
</tr>
<tr>
<td>mother</td>
<td>malambu</td>
<td>mama</td>
</tr>
<tr>
<td>child, disciple</td>
<td>kindende</td>
<td>mwanika</td>
</tr>
<tr>
<td>sister</td>
<td>khetu</td>
<td>panji</td>
</tr>
<tr>
<td>younger brother</td>
<td>kimbene</td>
<td>leke</td>
</tr>
<tr>
<td>older brother</td>
<td>samba</td>
<td>yaya/mbula</td>
</tr>
<tr>
<td>maternal uncle</td>
<td>kingwila</td>
<td>ngwasi/lemba</td>
</tr>
<tr>
<td>fire</td>
<td>mulemo</td>
<td>mbwila</td>
</tr>
<tr>
<td>water</td>
<td>madibu</td>
<td>mambu</td>
</tr>
<tr>
<td>house</td>
<td>zambisina</td>
<td>nzo</td>
</tr>
<tr>
<td>lighting</td>
<td>nzubussu</td>
<td>nzasi</td>
</tr>
<tr>
<td>dog</td>
<td>mamuka</td>
<td>mbwana</td>
</tr>
<tr>
<td>to sing</td>
<td>kuyimbishi</td>
<td>kuyimba</td>
</tr>
<tr>
<td>to dance</td>
<td>kukini</td>
<td>kukina</td>
</tr>
</tbody>
</table>

The numerals 1–5 are the same as in Ki-kongo while 6, 7, 8... are expressed as 5+1, 5+2, 5+3...

The end of the year’s training was marked by a solemn promotion of the new devotee. Three expert diviners came to take part in it, examining the candidate. In front of the assembled crowd he had to prove that he was able to do the high jumps required of a diviner in trance and to snatch a fowl from the hand of one of the experts, to boil it in a pot, fetch it with his bare hand and eat it. But, above all, he had to show his ability in drumming, singing and ritual dancing.

The act of calling the successful candidate out of his seclusion and of showing him publicly to the people was the climax of the whole celebration. The following is the appropriate song for this occasion:

We have come to the end! We have come to the end!
Come out, young nganga!
Come out at last, that we may see you!
Come, that we may look at you!

Through this graduation ceremony the muwuwa became a nganga authorized to practise on his own what he had learnt during the past year of his apprenticeship.

Functions and Divining Methods

The diviner is expected to detect and reveal the forces and machinations that have caused a peculiar sickness or death and to indicate to the people a means of curing the illness. How does he proceed?

Let us first take a death. The relatives of the deceased take a leaf from a mutundula plant (Anomum alboviolaceum), put it on the grave overnight and carry it to the nganga ngombu on the following day. The diviner places the leaf underneath his bed, where he also keeps his mudinga and his dog-skull medicines. Before he goes to bed in the evening, he takes a small portion of all the ingredients in the skull. These provisions are supposed to induce the deceased to appear in the diviner’s dream and to reveal the cause of his death. If, in a particular case of witchcraft or sorcery, the name of the culprit is not shown to him during the night, he will try to recognize him on the following morning in a magical mirror or else to identify him by questioning him cunningly.

In a case of sickness, some object, for example a coin, is usually brought into contact with the patient’s body and is afterwards taken to the diviner. Sometimes the latter is said to see the cause of the illness in his dream even before the party reaches his house. There may appear to him certain images that give him a clue. If, for instance, he recognizes himself in a dream, violently throwing about his arms and legs if resisting a torrent, he will know that the sickness was caused by the power of nmwulo. If he sees either a tree or himself struck by lightning, this rather points to nkoshi. If, again, it is himself who appears, running about and having on his shoulders trees that he has pulled out, mambapo is supposed to be responsible for the illness, etc. These images with their specific agitations and gesticulations actually refer to the trance movements peculiar to the mi-kisi just mentioned and may be seen particularly during the initiation of the new devotee.

When once the nkisi responsible for the sickness has been identified, the method to be adopted for treatment is clear: upon the diviner’s directions, the patient’s clan will have to introduce that particular nkisi and to assign its devotee. The diviner himself is not a therapist. It is the task of the appointed specialist, be it the nganga nmwulo or the nganga nkoshi or the nganga mambapo, etc., to apply the medicine attached to his peculiar nkisi and thus to heal the patient.

In some way, the nganga ngombu thus stands above the specialists just mentioned. His professional outlook is wider. On the one hand he himself is subject to the manifestations of the various occult forces, and he must be so, even in a special degree, so as to be able to trace them and to identify them; in his professional work he himself is also possessed and directed by one such specific power, i.e. the ngombu medicine. On the other hand, he in some way dominates the field of conflicting forces by revealing them and showing how their destructive power may be neutralized and turned into a healing and protective force. The diviner is thus regarded as a benefactor of man. At the same time he becomes an inspirer and innovator in the magico-religious field, detecting new causes of misfortune and sickness and revealing new rituals for counteracting and healing them.

Dreams appear to be important in his divining procedure. Yet, some magico-mystical influence from outside seems necessary to produce the images. Two things are regarded as indispensable: contact with the sick or deceased person (represented by the object that has been made to touch the patient’s body or the grave of the deceased) on the one hand, and some kind of communication with the potential originators of the sickness on the other hand (hence the diviner takes a small portion of each nkisi medicine before going to sleep). Thus the effect and the potential causes would seem to meet in his dream and, as a result, the actual cause is revealed. In this the diviner himself rather plays the passive role of a medium, but of a professional medium who has been ritually initiated.

Notes

1 This paper is based on information received in 1955 from a diviner who lives near Kimbuk. Though some of the details may only apply to that particular specialist, the basic ideas and the divining procedure may be taken as typical. For the transmission of some additional information I am indebted to the Revd. J. Malfiet of the Catholic Mission of Kimbuk.
The Eagle Dance of At Sabe. By Margaret King, Mus. Bac. (Adel.).

With three figures

This paper describes and illustrates the Eagle Dance of At Sabe, observed while I was engaged on ethnological and musicological research in Portuguese Timor during 1960-61. The dance is a mimed performance of the hunting and defence of nesting eagles; on a subliminal level it also portrays the defence of their home by the mountain dwellers of Timor against infiltration and domination by attackers of 'foreign' origin who are 'alien,' and emanate from the coastal flats of Timor and adjacent mainland areas.

The Suco of At Sabe is situated in the Ramelau Range of western Portuguese Timor (fig. 1). The entire area is extremely rugged and communities are scattered and isolated, but at certain times of festival members of outlying povoações gather together at this administrative centre and perform the dance to be described.

The Eagle Dance tells the story of nesting eagles and their hunters in a slow pattern of rhythmic mime expressed by very subtle manipulations of the muscles and torso, and sharp violent movements of head and arms. Percussion is provided by a shuffling ovoid of women beating drums and gongs. No significance of musical pitch is apparent, rhythm is the basic essential.

Four men and two women perform the dance; the positions of dancers and musicians at the commencement are shown in fig. 2 (A and B). The heads of the Hunter (a) and Male Eagle (b) are turned over the left shoulder, while those of the Female Eagles (c and d) stand at right angles to the male dancers, facing inward toward each other, within the rectangle formed by the sextet.

The positional stance of the women at fig. 2 (c) demonstrates a 'turning-out' of the legs from the hip socket. Their arms are raised and extended as shown at fig. 2A (c and d); the long scarf which falls from the nape of the neck (not shown) is wound round each wrist before falling in an incurving manner to frame the pelvis (fig. 2C). While adopting this stance the heads of the Female Eagles turn abruptly from side to side at two-bar intervals, returning to the frontal position at bars 1 and 6 (fig. 3). It will be observed that these are the points of lowest rhythmic impetus.

This movement is constant by the female dancers throughout sections 1, 2, 3, 5 and 6 of the basic floor pattern shown at fig. 2 (G).

**FIG. 1. SKETCH MAP OF EASTERN TIMOR**

**FIG. 2. POSITIONS IN THE EAGLE DANCE**
At 64 of this pattern the feet are moved sharply apart and the knees bent—the 'turned-out' position still being maintained (fig. 2d). The step is identical with the second position plié of classical ballet.

At this time the elbows are raised to shoulder height, the wrists describing a circle before returning to the basic stance shown at fig. 2(c).

Throughout the fourth pattern the heads of the dancers make a sharp darting action (see inset, fig. 2d), in eloquent mime of the stabbing attacks made by nesting birds when they are threatened. This forceful movement takes place throughout bars 2, 3, 4 and 5 of the rhythm (fig. 3) and always in the direction of the Hunters. During bars 1 and 6 (fig. 3) associated with 64 (fig. 2) the heads of the female dancers take a frontal position which is associated with the lowest rhythmic impetus but, in this instance, it is allied with a general inclining of the upper torso toward the 'protection' of the Male Eagles.

![Fig. 3. Rhythm for Dance](image)

**FIG. 3. RHYTHM FOR DANCE**

*Analyzed and notated by Margaret King*

Floor patterns 4 and 5 (fig. 2c) each take two complete six-bar measures of rhythm (fig. 3), while patterns 1, 2, 3, 6 and 7 take only one six-bar group for each floor pattern described in fig. 2(g). Thus the performance of one complete dance pattern takes place to the accompaniment of 48 bars of rhythm; the positions at fig. 2 (66) show an exchange of position by all six dancers, with the significant difference that dancers a, b, c and d (as shown in fig. 2a) now have their heads turned in the opposite direction from that shown on fig. 2(61).

These basic patterns are repeated many times, the dance lasting for nearly an hour. Throughout the performance no significant loss of precision of motion and rhythm by either dancers or percussionists is evident.

In contrast to the restricted gyrations of the female dancers, the Male Eagles show great subtlety of muscular movement, particularly during patterns 1 and 6 (fig. 2c), when the hovering motion shown at fig. 2(e) is employed. This is also used in fig. 2(g), when it is allied to the swooping encirclement of the Female Eagles as a gesture of 'protection' while the Hunters are also proceeding to an exchange of places, the total change of positions by all six dancers being completed by the movement of the Female Eagles as shown at fig. 2(66).

The dancers portraying the Hunters are not required to show the degree of muscle control necessary for the role of the Male Eagles. Their ceremonial sword, which becomes virtually an extended arm, is chiefly used in g2, d4 and e5 (fig. 2), when the striking movements shown at fig. 2e are utilized. g3 (fig. 2) consists of a slow retreating step, the legs being raised to a high degree to express an exaggerated sense of locomotion.

The costumes of the dancers add emphasis to the mime; the females are dressed in lau and overblouses of tawny shades, the long scarfs flecked in deeper colours of the same basic hue. The Male Eagles wear tai of similar colour but brighter and more strongly patterned than that of their female counterparts, while the rectangular cloths held in each hand are chrome yellow. The cotton cloth worn by these four dancers is of Timorese spinning, dyeing and weaving.

By comparison with this sober form of dress worn by the Eagles, the two Hunters shine with extra splendour. Their waist length tai are of silk, brilliantly patterned, and their bare upper torsos are hung with heavy circles of gold engraved in geometric designs and known as orhs de ora. Wide bands of gold encircle wrists and biceps, and gold inlays decorate both swords and scabbards. Headpieces of the same metal, shaped like the horns of buffalo, are also heavily chased in geometric design. Their ankles are circled by thick bands of goat hair, indicative of fleetness of foot; these are the prerogative of first-class hunters.

The female percussionists are dressed in lau made of cotton, but these are very brightly patterned and coloured—in sharp contrast to the female dancers.

Their drums (tipalu) are 33 cm. in length, the tympanum having a diameter of 15 cm. This instrument tapers to an open base of 11-5 cm. and is slung over the left shoulder by a narrow suspension of plaited bamboo or sial. The gongs are metal, circular in shape and from 20 cm to 25 cm in diameter. These are usually of Chinese origin and obtained by trade.

As will be noticed from fig. 3 the drums are divided into two sections of rhythm. Usually the ovoid formation is split into groups of fours, comprising 1st drums, gongs, 2nd drums, but this is not always so and, on the second occasion upon which I observed this dance being performed, the Gong players were distributed indiscriminately among the players of drums. In this instance it was noticeable that the older more experienced women were 1st drummers.

The method of playing the tipalu is with stiffened fingers and heel of palm of both hands; the subtlety of nuance which is so much a feature of gamelan and Indian drumming is notably absent.

The rhythm employed throughout the Eagle Dance is a simple triple with canonic entry of 2nd drums at bar 3 (fig. 3) for one bar only. The accent on the third beat of each bar is also a notable feature throughout, the only other beat to receive accent being that of the second beat of the third bar (fig. 3) when the 2nd drums give emphasis at the point of imitative entry by accenting the first of the two semi-quavers.

As mentioned earlier in this paper, when the ovoid of musicians is formed of an indiscriminate mixing of drums and gongs, the older women always take the first rhythmic line. This would seem to indicate that the 1st drummers, although beating a regular simple rhythm, are regarded as the primary percussionists and have the responsibility of keeping the precision of rhythm within the group.

The Eagle Dance is one of many performances observed during field work in Portuguese Timor, for the Timorese find a rich cultural outlet in their dance, mime and music. The performances were as varied as the groups inhabiting the island, and it is my intention to prepare further papers from the material collected in 1960-61 so that comparisons of style may become possible, for there are very marked differences between the inhabitants of western Portuguese Timor and those further east.

I should like to record my indebtedness to Governor Themudo Barata and the members of his Administration for the great assistance given to me during field work in Timor. My thanks are also due to Hedley C. Brideson, Principal Librarian, and the Libraries Board of South Australia for their generous help and interest in my work. The journals, tape recordings, and photographs connected with this field work are now in the Sheard Collection of the Public Library of South Australia.

**Notes**

1 In the mountain regions of western Portuguese Timor the dwellers of the coastal areas are called malu (foreigner).
2 Suco, a complete unit of povoadores variable in number from three to ten.
3 Povoado, a village with its own chief who is a dependant of the Chief of the Suco although not necessarily a blood relative.
4 Lan, a length of hand-woven cloth patterned in horizontal designs, rendered tubular by a single seam. The surplus width of material pleated and tucked into upper opening, the bulk being concealed under the left armpit.
5 Tai, a length of hand-woven cloth, wound round the waist. Sometimes the surplus length is carried up over the left shoulder as added protection.
Cross-Cousin Marriage. Cf. MAN, 1964, 59, 140

Sir,—Mr. William Wilder has now (MAN, 1964, 140) jumped into the cross-cousin marriage controversy wielding as aggressive a cudgel as any of the previous combatants, and soundly beats Livingston for some slapdash argument in a previous issue (MAN, 1964, 59). There is one point, however, on which he himself deserves at least a tapping. He says, following Needham, that 'wife-giver' and 'wife-taker' must designate groups, and, in a patrilateral prescriptive model, wife-givers and wife-takers must change in every generation, reversing their positions. The Siane system of kin terms does not show this reversal down the generations, but instead follows the pattern of equal lines whereby MB, MBS and MBSS all receive the same term. Hence the Siane do not have this form of cross-cousin marriage. This appears to be true, but why does Wilder say that the category terms for wife-giver and wife-taker must designate groups, and what kind of groups does he mean to refer to here? In the diagram which he produces for the Siane terms he does not indicate the level of group extension for the terms that he lists. Is mono extended to all the men of the MB clan or only to a section within it? Could it be, for the purposes of certain marriages, seen as exchanges between groups, that one sub-clan would be treated as a mono of a sub-clan, another as a soa sub-clan, within the same clan? I am not arguing that this is the case for the Siane—in fact, Salisbury’s own diagrams refer to terminology applying to the clan level—but has Wilder realized that his assertions about wife-givers and wife-takers partly depend on this kind of question?

It seems to me that the monolithic assumption of cross-cousin marriage with exchange relations between corporate descent groups has been a mistake. It has led people to think that the only possible form of ‘prescriptive marriage’ must involve groups of this kind, and that the only form of ‘exchange marriage’ must involve ‘sisters’ or ‘cross-cousins.’ In fact, men may regularly be marrying women of the cross-cousin category without any block relations of wife-giver and wife-taker resulting between any two clans, say, in the society. And, on the other hand, there can be exchange marriages between clans without any notion of marrying one’s cross-cousin being involved. It is worth suggesting that this latter is the case for the Siane, among whom, as Wilder notes, ‘the bond of reciprocity in women and goods is expressed by numerical accounts and not in a kinship idiom.’ It is this general theme of exchange and reciprocity which lies at the back of Lévi-Strauss’s book on ‘elementary kinship structures,’ and Wilder is well aware of the point, but at the same time he chooses to spend most of his energy in simply proving that the Siane do not have a particular kind of cross-cousin marriage. I should not get into a discussion of kinship systems into whether they are prescriptive forms of cousin marriage or not tends to mean that the general theme is forgotten, and a good many cases showing some form of reciprocity in the exchange of women may also be escaping notice as well. I am led to wonder if this is so by a few data on ‘exchange marriage’ in a part of the Hagen region in the Western Highlands District of Australian New Guinea. The data refer to some marriages contracted by members of a single small tribe with other descent groups around them. Informants readily indicate cases where a woman of one clan was sent in marriage to a clan of another tribe and a woman from his clan ‘came back’ to a clan which is paired with the first giving clan. The ‘paired name’ concept is very readily applied, and its use in this context is only one out of a number—other uses apply to origin myths of tribe pairs, or to group pairs closely linked in ceremonial exchange. Bridewealth is paid, in pig, shell and money, but a good deal of it is returned to the groom’s kin or is exchanged directly for equivalent items at the time of the marriage. The ‘exchange-marriages’ have nothing to do with the adjustment of flows of goods between groups—these are regulated by the formal exchanges of shell and pig on other occasions and by all the other private arrangements between partners. A kind of count of the number of women who have gone to a group and the number who have come back is kept but the calculus is very flexible, and different orders of groups may be invoked as the exchanging units to make the ‘addition’ different. Nevertheless, the concept of exchange marriage is there, despite the fact that in this society marriage with the cross-cousins on either side (designated by a single term, pelpam) is forbidden.

In conclusion, then, the possibility of marital exchanges between groups without notions of kinship categories being involved should perhaps be given more recognition than it has so far; and, conversely, category systems of cross-cousin marriage should perhaps be examined yet again to establish more closely what are the groups involved in the exchanges. This could give the lie to Wilder’s repetition of Needham’s position, that in a patrilateral model ‘wife-givers and wife-takers must change in every generation.’ If y of section 1 in group A marries a woman of category FZD from section 1 of group B, this does not mean that in the next generation y of Bt must go back to A1 to get his wife—he may instead, depending on how the category terminology works, go to A2 or A3, thus avoiding the immediate ‘role-reversal’ problems on which Needham lays stress. Girls of the right category for him may in fact be scattered through a number of groups. These are by no means new points. But, for his neglect of them, Wilder perhaps deserves a light cudgelling in his turn, along with the rest of us who enter the arena.

ANDREW STRATHERN

Mount Hagen,
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The Content of Kinship. Cf. MAN, 1964, 130, 217

Sir,—I think that Professor David Schneider’s attack (MAN, 1964, 217) on my critique (MAN, 1964, 130) of Gellner’s Nature and Society in Social Anthropology is based on a misunderstanding of what I said or tried to say (for which I am no doubt to blame), and also on some confusion as to what the argument is about.

To deal with the second point first, the question is not at all about the ontological status of kinship, ‘just what kinship is,’ as Schneider puts it. We all know perfectly well what kinship is as a biological given. The point at issue is quite a different one; it is how those social relationships which in every culture are included under the rubric of ‘kinship’ may most usefully be thought about and investigated by social anthropologists. The argument is concerned with relevance rather than with ‘truth.’ And my point was simply that a social anthropologist who is analysing a kin relationship, say that between father and son, to take Schneider’s example, is not concerned with its biological character (though of course he does not deny that it has or may have such a character), but with its social and cultural content; with what, in a particular culture, fathers and sons do and think in regard to one another. These are social and cultural questions, not biological ones, and that is why a model based on the categories of biological connexion, such as Gellner originally suggested, cannot be helpful in answering them.

Nothing that Schneider says effectively criticizes this position. Instead he takes me to task at some length for maintaining the quite absurd proposition that kinship has ‘no content,’ and the hardly less preposterous one (logically inconsistent with the first) that ‘the father-son relationship, as a kinship relationship, has no content other than economic.’ Throughout my article, and indeed in the very passages that Schneider quotes, what I was trying to do was to say as plainly as I could that kinship relations can have all sorts of content; that is why social anthropologists spend so much time studying them. ‘The whole point about kinship relations,’ I wrote, ‘is that they must be something else, for example political, jural, economic or ritual.’ To have made myself absolutely clear, I should perhaps have added ‘or a combination of some or all of these and many other socio-cultural qualities,’ or some such phrase. But I would have hoped that it was plain enough from what I did say, and from my whole paper, that what I was asserting was that it is

This collection of 19 essays, plus a summary by the late Professor Zeuner, covers such a variety of topics, always with reference to cattle even if not to man, that I will rearrange and discuss them under three main headings and six sub-headings. One suspects that some of the participants little understood what some of the others were saying, and possibly little cared. Certainly some of the papers, as published, ignore data presented at the same conference and printed here in the same collection.

The symposium was held in 1960 but the papers were not published until 1963. The delay is regrettable, since in 1961 a major meeting on problems of animal domestication was held in Kiel, Germany, and the papers presented there were published promptly (1961 and 1962) as two volumes of the Zeitschrift für Tierzüchtung und Zuchtungbiologie. The participants at the conference in Kiel, except as they coincided (Zeuner, Jewell, Howard) with the group presently being considered, could not profit from the Symposium of 1960 on Cattle and Man, and in the present volume there is only one reference to the numerous published papers resulting from the Kiel conference. In general there seems to have been little chance for authors to rewrite their papers during these three years between presentation and publication.

As mentioned, the published papers might well be rearranged into three main groups, as follows:

I. Anthropological emphasis, wherein man is important and cattle are subordinate;
1. F. E. Zeuner, 'The History of the Domestication of Cattle.' Zeuner deftly packs into a brief discussion much of the material he subsequently published in his History of Domesticated Animals. If all of the subsequent authors dealing with historical sequences and using technical names had followed his careful and scholarly lead the whole tone of the publication would have been improved.

2. Human social organization and/or prehistory, and cattle:
(a) Derrick J. Stenning, 'Africa: The Social Background.' The African pastoralist, prior to the disruption of his society by rinderpest (which destroyed his herds) and European police power (which interfered with his freedom of action), had a society strongly oriented toward the life of his cattle. There was not, however, any worship of the animals; cattle were not gods, but instead were regarded as a different type of sentient beings which had to be manipulated in ways that were intricate and subtle. These traditions are in conflict with today's needs of people in changing from a nomadic to agricultural or industrial life.

(b) J. D. Evans, 'Cretan Cattle Cults and Sports.' This is an interesting and welcome analysis of the evolution of the highly skilled acrobatic bull-leaping and somersaulting of ancient Crete. The sport had its origin in the rude peasant capers with bulls, still to be found in some Spanish and Italian towns, but (as with the Spanish bullfights and the American rodeo) evolved into a sport for professionals, with audience observation.

(c) C. von Fürrer-Haimendorf, 'The Social Background of Cattle-domestication in India.' The hill tribes of the north and north-east of India are considered in relation to domestication of the yak, but the discussion is made of what would seem to be the more important matter of the zebu and water buffalo.

(d) Raymond Allchin, 'On Cattle and Economy in Neolithic South India.' In the Deccan, domestication of the zebu, osteologically indistinguishable from those of modern cattle of the region, is associated with remains of early pottery-making cultures. Pottery figurines of the humped cattle clearly portray the type. Evidence of domestication in situ is lacking, and the animals are assumed to have been introduced already domesticated.

II. A mixture of social anthropology and bovine physiology:
1. E. C. Amoroso and P. A. Jewell, 'The Exploitation of the Milk-excretion Reflex by Primitive Peoples.' This is a fascinating account of the physiology of the milk reflex and the exploitation by primitive peoples of this complex but interesting neuro-endocrinological mechanism. It is a pity that so few other papers combined zoological and ethnological data as thoroughly and skilfully.

III. Zoological emphasis, wherein cattle are mainly considered:
1. Biochemical, with or without genetic considerations; man, in this series of papers, is generally ignored or mentioned only in passing as having been responsible for having controlled the breeding of cattle or for historic movement of them; only bovine proteins are considered in detail.

(a) A. E. Mourant, 'The Genetic Study of Human and Bovine Populations.' The human blood groups, primarily the ABO system, are discussed as an example of a well-known situation which can be expected to be similar in other mammalian species. The biochemical and genetic situation responsible for human erythrocytic sickling, and the correlated selection for the heterozygote with infection by falciparum malaria is quoted as an example of conditions to be expected in cattle.

(b) J. G. Hall, 'Cattle Blood Groups.' The B group of cattle is emphasized as being of great complexity and thus possibly the best analytical tool for genetic comparisons of populations of modern cattle. However, owing to increased use of a small number of stud bulls, often by way of artificial insemination, the effective size of the breeding population is being markedly reduced, and the gene frequencies are being correspondingly modified, so that they may not now reflect ancestral patterns.

(c) A. D. Bangham, 'Cattle Hemoglobin.' Two hemoglobins (Hb) A and B are present in cattle; each gene, when present, is phenotypically expressed. A high frequency of B is found in zebu in India, in various cattle thought to have some zebu ancestry, and in Jersey cattle. In adjacent sentences, the author attributes this situation in Jerseys to generic drift and to direct relationship via cattle imports of zebu stock from North Africa; the two explanations would seem to be mutually exclusive, and the necessity of hemoglobin B as a genetic marker for zebu ancestry is not supported by the data of Singer and Lehmann. AA cattle are said to be resistant to trypanosomiasis, which situation might well explain the low incidence of HB in some African breeds of zebu ancestry.

(d) R. J. Cabannes and Ch. Serain, 'Hemoglobins of Some Species of Animals of Algeria.' Data on a variety of animals, wild and domestic, are presented somewhat haphazardly, with little contribution to the announced subject of the symposium.

(e) R. Schaffenburg, 'Milk Protein Polymorphisms.' In addition to casein, two milk proteins are synthesized in the mammary glands of cattle. Named α-lactalbumin and β-lactoglobulin, each can be further divided into two sub-types, each in turn controlled by a single gene. Study of the gene frequencies of these sub-types in different populations may be useful in tracing migrations and ancestry, in spite of the author's pessimism (seemingly unwarranted) with regard to this possibility for β-lactoglobulin.

(f) Ronald Singer and H. Lehmann, 'The Haemoglobins of Africander Cattle.' This breed, originally the cattle of the Hottentots, has been tentatively traced to prehistoric zebu imports into East Africa by way of Hadramaut. However, the extremely low incidence of the gene for HB B indicates either that this gene is not necessarily a marker for zebu cattle, or that the Africander cattle are not of zebu stock. The authors suggest the possibility that African cattle could have been domesticated from a native African bovine. The article is a nice balance of what is known and unknown, pointing to problems for future investigation.

(g) A. L. Ogden, 'Cattle β-Globulins.' Transferrin, the iron-binding serum glycoprotein, is represented in cattle by five similar molecules, genetically controlled by a homologous pair of genes with five multiple alleles; since each gene has full expressivity, 15 phenotypes are possible. Not all are found in all populations, however; since two of the alleles are found only in zebus and zebus-derived breeds, this character would seem to offer a better means of tracing zebu ancestry than do the other blood and/or milk proteins considered in this book. No data in support of this intriguing possibility are presented, however.

2. Paleontological and Archaeological Emphasis (these papers are useful to the specialist, but the details probably are not of interest to others):

(a) Magnus Degerbol, 'Prehistoric Cattle in Denmark and Adjacent Areas.' This summary, useful to both prehistorian and osteologist, dates the first appearance of domesticated cattle in successive Danish sites, and graphs the changing size of domestic cattle from their earliest appearance to the present time. Measurements are presented of certain key structural and bone, whereby wild cattle can be distinguished from the domestic ones.

(b) Peter Jewell, 'Cattle from British Archaeological Sites.' The history of British cattle is traced from the wild ancestor into the Medieval period, with the data for the standard diagnostic osteological characters presented in a series of graphs. The data and conclusions supplement and strengthen those of Degerbol.

(c) Margaret M. Howard, 'The Metrical Determinations of the Metapodial and Skull of Cattle.' Using the skeletons of modern cattle as a standard, the author undertakes an analysis of sexual dimorphism (including the influence of castration) in cattle in general, and then analyses British paleontological and archaeological materials to determine sex ratios (including castrates) in wild and early domestic British cattle.

3. Emphasis on breeds of cattle, and their history (the two authors in this group would seem to be more familiar with the practical aspects of stock-breeding than are any others in this
symposium, and a healthy bucolic aspect emerges of cattle as living animals in pasture and paddock:

(a) C. Wheaton-Smith, ‘Cattle Breeds—A Study in Progresive Hybridization.’ Some of the economic forces which have shaped British cattle breeds since the Medieval period are here outlined, summarizing several volumes of detailed bi-historical data. Cattle are extremely plastic genetically, a fact well known before genetics was understood, and 20 years of intensive breeding can change a breed to fit new conditions. Such factors in the past have been the rigours of early cattle drives, the distribution of fat necessary for whole-ox roasting, the change in marketing pattern with the spread of railways, the reduced use of candles, and increased use of the kitchen range. The author suggests universal fluoridation as a means of improving beef flavour by improving human teeth; the good flavour which comes with age is now being sacrificed to the untasteful tenderness accompanying bovine youth.

(b) J. Boston, ‘Cattle Breeds in Europe and Africa.’ The first part of this paper, on Pleistocene cattle and the prehistoric period of domestication, lacks critical evaluation of the available evidence. In the remainder of the discussion, on the history of breeds in the broad sense, the author is on more familiar ground, but the conclusions often seem to have little accompanying evidence. The use of multiple taxa, including some invalid synonyms, gives the paper an archaic flavour reminiscent of those of half a century ago, or more.

Professor Zeuner’s closing summary was essentially an outline of the separate papers, including a few not printed with the others, but he did make the excellent point concerning the confusing taxonomy that a domestic breed is biologically different from a natural race or subspecies. He sensibly proposed therefore that the names given to domestic animals be considered as merely descriptive, without official standing under the International Rules of Zoological Nomenclature. I am in full agreement. CHARLES A. REED


The papers here published were originally prepared for a symposium on ‘Anthropology and the Conditions of Individual and Social Freedom,’ held at the Wenner-Gren Foundation’s schloss at Burg Wartenstein in 1961, and organized by the editor of the present volume. The original motivation, he writes, ‘was to bring together a group of anthropologists and humanistic scholars with allied interests in order to examine and explore the concept of freedom and its bearing on anthropological thought.

Perhaps fortunately, most of the contributors have been less concerned with analysing the concept of freedom itself than with investigating its significance and the conditions of its achievement in particular cultures and contexts. Like Dr. Johnson, the editor and his contributors for the most part accept the freedom of the will as a datum of experience—the very essence of will is self-determination and self-expression and hence the will is either free or it is not at all—and so we are spared lengthy and tedious discussion of what freedom can mean, whether it can be reconciled with a deterministic view of the universe, and so on. The contributors recognize that freedom is relative; always it is exercised under conditions, and its negative and positive aspects (‘freedom from’ and ‘freedom to’) are inseparable. So regarded, human culture is plainly an indispensable condition of freedom, and some of the most interesting contributions to this volume, at least from the anthropologists’ point of view, boil down (very usefully) to discussions of the paramount values of particular cultures, and the means whereby and the contexts in which these may be realized.

Most of the contributors are anthropologists, but philosophy, history and human biology are also represented. Naturally the papers vary considerably in quality, and like most such symposia it is a bit thin on connective tissue. But the book contains some really interesting and original work, and the anthropological reader who ploughs his way through all of it will be rewarded. For those who lack either time or inclination to do this, the following brief and partial account of the separate contributions may be useful.

Dr. Drenly opens with a balanced discussion of the aspects of human freedom and concludes unexceptionally that it should be taken seriously as an effective historical cause. Next comes an essay on the biological and medical conditions of human freedom, by T. D. Stewart, which seems a little peripheral to the main theme, though no doubt man would not be man, and therefore capable of freedom, were it not for his upright posture, his large brain and so on. Third is a competent paper by Audrey Richards on the implications of communications and transport development for the freedom, in particular, of African peoples whom she knows, to move about and participate more fully in social, political and economic affairs. The point that the slow communications which ‘the pedestrian state’ implies tend to local autonomy is interestingly developed. This paper tells us something about the actual conditions of freedom in the context, especially, of Benin and Ganda society.

The next essay, by Dorothy Lee, discusses freedom and social constraint in more general terms, starting from the position that there can be no freedom without rules, i.e., constraints, and concluding (as I understand her; I found some parts of this article a little obscure) that different cultures offer different ideas of freedom. In the paper next following Edmund Leach, stimulating and spirited as always, starts with the empirical fact of ‘unfreedom’ (the lack, in a particular status, of particular rights available to other members of the society), rather than with a contentless, ‘absolute’ freedom. He criticizes acutely the ‘double-think’ functionalism which asserts that even where all social institutions are not in perfect harmony the very disharmonies contribute something to functional unity, and concludes that man can have either freedom or equality but not both (since a man is free in the measure that he is privileged), implying that the sooner liberal-minded anthropologists accept the reality of this dilemma the better.

Next, Dorothy Emmet does a useful job of terminological tidying-up (the importance of the distinction between ‘freedom from’ and ‘freedom for’ is again emphasized), and goes on to submit the Popperian development of the notion of ‘open’ and ‘closed’ societies to a much needed scrutiny. She points out that a society may be open in some respects and closed in others, so that a simple dichotomy of societies in there terms is oversimplified and misleading. There follows a paper by Otto von Meering on ‘The Experience of Individual Freedom’ which relates freedom to value orientations and to possible incompatibilities between these, but which I am afraid that I found (no doubt through my own fault) largely unintelligible. In Chapter 8 Robert F. Spencer considers the extent to which the religious beliefs of five different cultures (eskimo, Sirono, Mohave, Maori and Dahomey) are conducive to individual human expression. As he acknowledges, he can do no more than provide ‘brief vignettes’ of the religions that he considers, and his conclusion is that some of the systems which he describes leave more room for individual initiative than others. Christof von Feuer-Haimendorf, in the next chapter, adopts a somewhat similar approach, but he defines clearly the kinds of freedoms that he is concerned with, and the societies that he considers (the tribal Chenchcu and Naga, traditional Hindu society, and the Tibetan-type Buddhists of Nepal) are ones which he knows intimately and in depth. The result is an original, readable and highly instructive essay in comparative ethnography.

In the next chapter (Kathleen Gough on ‘Indian Nationalism and Ethnic Freedom’) we turn to consider freedom as a political goal. Dr. Gough provides a detailed economic and political history of Kerala, and shows how the introduction of universalistic ideas by the British, combined with economic factors, led to revolt by the deprived classes, to whole freedom from social and relief from oppression rather than any desire for personal wealth or power.

Next comes a paper on ‘The Idea of Freedom in African National Movements,’ by Thomas Hodgkin: again the stress is on freedom from (colonial rule, etc.), and he rightly remarks that Africans have had more to free themselves from—slavery, colour prejudice, and so on—than most people. But African freedom has a positive content too, and the author argues that this owes much to the
positivist, critical temper of Western thought, made available to Africa during the 'colonial interlude.'

In Chapter 12 S. N. Eisenstadt reflects, not always in the most lucid of prose, on the problems and conditions of ethnic freedom, especially in the context of the Middle East and of the mass immigration to Israel. He stresses such factors as the degree of solidarity and the flexibility of classes. Next Otto Brendel interestingly discusses Freedom and Art—or rather Artists: art, he suggests, 'progresses by repeated acts of self-liberation.' Not all students of primitive art will agree with him that 'rigidity' is a formal characteristic of all such art, but his conclusion, that art, 'like every other human action, is the answer which an individual gives to a situation' is unexceptionable, if not particularly illuminating. The last essay in the volume, by Herbert J. Muller, on 'Freedom and Justice in History,' is one of the best. For him, freedom consists in a state 'in which a person may decide for himself what is right and good,' and he convincingly shows how the concept of this as an explicit social value is a relatively recent development in human history, most men in most ages having lived by 'miracle, myth and authority' rather than by freedom.

Although the foregoing summary inevitably does less than justice to some of the contributions, it may give some idea of the symposium's scope and variety. Its publication shows that the venture was a worthwhile one, and the editor's courage in tackling, on so wide a front, so fundamental a human problem is to be commended. But it would have been even more important than it is if it had included, as well as an introductory chapter setting out some of the problems, a final one, summing up and assessing the contributions which the various studies, and anthropological generally, can make and are making to the understanding and solution of these problems. It may fairly be claimed, and this book provides plenty of evidence to support the claim, that the work of social science generally and of social anthropologists in particular can contribute importantly to man's understanding of his own condition. But the reader of this symposium will have to work this out for himself.

JOHN BEATTIE


Douglas Fraser has attempted to scan the world of primitive art in one small volume consisting of 56 pages of introductory essay material and 277 pages of developmental exposition including 138 illustrative plates. Many of the plates are excellently reproduced in colour.

The introductory essay sets forth not only the author's condensation of the world of primitive art which he divides into three areas, to wit: Africa, Asia-Oceania and America, but also his thesis regarding the general character and scope of primitive art. It is here that he invites, at the very beginning, many challenges. Witness his contention:

'One of the first things we must realize is that this art is not intended to serve aesthetic ends. The artist merely responds to the needs of his society; he produces precisely the number of objects—no more and no less—that his patrons require. Moreover, all the works he makes have an ulterior function: either they assist in ritual or they perform a social role. Any time or energy left over to the artist will be spent in other activities—not in the further production of art. Thus the Western idea of art for art's sake would seem little to the primitive artist. He does not dream that the sole purpose of an object could be to give himself and others that joy in artistic performance we call aesthetic pleasure.'

One wonders how he could run the risk in the realm of primitive art—a field still too unexplored to permit of dangerous generalizations—of declaring, ex cathedra, that 'this art is not intended to serve aesthetic ends,' or that the artist 'produces precisely the number of objects—no more and no less—that his patrons require,' or finally that 'all the works he makes have an ulterior function.' Scientific thinking does not permit such extravagant generalities especially when there is abundant evidence contradicting all three assertions.

He might have consulted, for example, Justine M. Cordwell's contribution to Continuity and Change in African Cultures.

Distinctions such as Fraser introduces should be based on a more acceptable universal approach to the philosophy of aesthetics. This thesis, to the effect that the primitive mind could not produce a 'true art,' primarily because the primitive mind could not comprehend it, was, of course, untenable and smacks of the proverbial error of Western aesthetic thinking.

As a matter of fact this absence of valid judgments establishes a critical cleavage throughout the entire volume and disallows the very title—Primitive Art. It appears strange that the author should subscribe to the idea that the world of primitive art was essentially inferior because primitive man was 'essentially conservative,' and every 'facet of his religious and social life encouraged stability and permanence.' These were qualities that have marked, for instance, the world's greatest art and all of the seven arts. If such presumed isolating qualities were perchance at the heart of the primitive art contribution, the fact that they did not change, as Fraser opines, should have little to do with a critical evaluation of the end product. The quasi-relationship simply is not clear.

A second challenge is invited by Fraser when he insists that practically all primitive art was evolved from a previous 'high culture' art. So, he contends, European influences (in this case Portuguese) account for the appearance of masquerade and child images in Africa although masquerade and child images antedated the coming of the Portuguese even as African metal-casting antedated their coming. Incidentally, masquerade and child images are as old as mankind and have appeared in some of the most pagan cultures.

The author avows that naturalism may be a rather recent and somewhat alien achievement of the African artist. But, fortunately, he refutes himself with his own excellent naturalistic rock paintings that belong to the late Stone Age—hardly recent. Moreover, he overlooks the fact that the Bantu and the Bushmen often startled Europeans with their sagacity, their 'philosophy' even when they could hardly count beyond their ten fingers. What, may I ask, is this vaunted 'high culture art' of which Fraser speaks? Is it 'pop' art?

The crowning error, however, arises from the unique discovery of the author's idiom in African art as 'a heart-shaped face!' He has chosen a few well coloured objects in support of his contention. The truth is found, however, in the fact that the heart-shaped face actually occurs in African art. In nearly fifty major tribes in Africa, I could find hardly more than eight where the 'heart-shaped' face appears with any regularity. Even if it did appear, it could hardly be considered 'the idiom' in a land where the idiom depends so largely on the weight of variety in shapes and forms.

Similarly, I must object to Fraser's irresponsible desire to trace the beginnings of the art forms of Asia-Oceania always back to a European source. I am tempted to ask if it were not better to assume the evolutionary-cellular origin of 'all things bright and beautiful' and conclude along with that well-known child's poem, that all those things were related because they were God's handiwork? This is a reductio ad absurdum—permissible, I trust.

The illustrations are carefully reproduced, though they seem painfully pre-judged in support of the author's contentions. Nevertheless, Fraser moves more acceptably in his Asia-Oceania Division of eight distinct layers of Pacific art where his close relationships are much clearer. Even his Senitian mother and child images are here accepted, correctly, as indigenous products.

Let us accept, too, his ascription of the three major influences in early American art as: Northern Asia, Northern Mexico and to a more limited extent, Europe. Here at last, he is on essentially firm ground. His over-emphasis of the sources of these art influences beclouds the creative individuality of the primitive Asiatic and American artist even as it did in African art. Though we may have to accept the 'lowest' position for the art of most American Indians we must not by any twist of the mind discount the significance of Inca nor of Aztec art. For here, certainly, the world concedes many indigenous qualities and therefore some originality.

The author might have formed more significant hypotheses had he introduced more instances of parallelism in early American Indian Art. For, as he suspects, but does not prove, some Pre-
Columbian art must have developed in isolation in spite of seeming similarities.

The more one observes Fraser's work, the more one wishes to know how he escaped an obviously inferred conclusion: given primitive men in almost identical environmental circumstances and with comparatively equal anthropological factors, one may well expect striking cultural similarities although there may have been absolutely no known contact with 'higher cultures.'

At least, for this unstated deduction, if for little else, one has to thank Mr. Fraser for his study of primitive art.

J. NEWTON HILL


Mr. Carrington is an author of popular books on geology and biology who here turns his hand to anthropology. The first half of his book is concerned with the early stages in human evolution and with prehistory. This section is carefully composed and checked, and is generally reliable, although it omits some of the most recent lines of research, and makes a few questionable assumptions. It is not clear, for example, how it is known that doliths were used (p. 66), while it is very doubtful if any of the earliest writings were intended for posterity (p. 192).

But in Parts 4 and 5 of the book, dealing respectively with 'Civilization' and 'Man's Place in Nature,' the author becomes much more dogmatic and much less reliable. It would clearly be unfair to criticize such a cursory review of human history for errors of omission or emphasis; but it is punctuated throughout by sweeping opinions on whole cultures and systems of belief, which are manifestly based on an extremely superficial knowledge. The comparison of Christianity and Islam on p. 271, for example, shows that the author has not understood the real nature of either. Towards the end of the book, the author's philosophy becomes more explicit. Goodness and Beauty, he claims, are characteristics of whatever aids and favours the course of evolution, while Truth is a relative matter. This appears to be the new orthodoxy of the fashionable cult of evolution; but Mr. Carrington's somewhat arrogant statement of the case is unlikely to win many converts. The book is illustrated by three charts, 33 line drawings, and 94 plates, including two terrifying pictures of Belsen, which are loosely linked with the text. There is a full index and 13 pages of carefully documented references.

W. C. BRICE


The need has long been urged for a modern introduction to social anthropology which would do justice to the subject's comprehensive aims and achievements and convince the general reader, or new student, of its importance as a challenging intellectual discipline. Where others have faltered, or hung back, Professor Paul Bohannan—eminently qualified by long research and teaching experience—has now attempted this difficult task. His book begins, most appropriately, with a statement of credo. Social anthropology, Professor Bohannan sees as that technique of social analysis which above all others explores social behaviour most profoundly; which, shattering ethnocentric assumptions, leaps cultural barriers to penetrate to the essential core in social relationships; and which leads ultimately to enhanced self-knowledge, understanding and personal security. From these bold convictions the study unfolds.

The first of the six parts into which the book is divided outlines the scope of social anthropology in relation to both the biological and social sciences, and introduces the reader to such key concepts as society, culture, personality, and roles; and the association of roles in clusters which the author unattractively calls 'syndromes.' With this technical preparation, which includes sections on language and art, we are launched into the second part of the book entitled 'The Biological Network.' This is concerned with kinship and the family, with the household as a social unit, and with various types of social groups based in varying degrees on kinship. From kinship we pass in Part III to 'The Network of Agreements' where contract and associations are discussed as a prelude to social stratification and inequality; and less predictably, to race as a biological and sociological problem. Part IV, styled 'Space and Things,' and the largest part of the book, deals with ecology, economy, politics and law. This leaves that other staple of anthropological discourse—religion, magic and science—to be covered under the heading 'The Image of the Unknown' in Part V. Finally, the concluding part of the volume, and the shortest, deals with Social Change.

On the whole, this six-fold division achieves a remarkably comprehensive exposé of the subject and little of importance seems to have escaped notice. Not surprisingly, however, within such a vast field, the treatment of topics is rather uneven. As one would expect, the sections on economics and law seem particularly well done, although there is no 'peace' in Professor Bohannan's feud, and in general the sections on prehistory and civilization are not sufficiently emphasized. The section on contract is interesting, but falls rather disappointingly. The treatment of language and its relation to values, thought and culture, much influenced by Whorf, is imaginative and stimulating. On religion, useful critical reviews are given of the approaches of Tylor, Frazer, Lévy-Bruhl, Durkheim and Weber: but the discussion of non-Western witchcraft, based mainly on the Navaho and Zande, does not do full justice to the critical points of Evans-Pritchard's brilliant analysis. At the same time, one misses any reference, highly appropriate in the context, to Horton's recent work. Nevertheless, taken as a whole, and notwithstanding these strictures and a few other minor criticisms which might be made, there is much to appreciate here.

On many technical anthropological topics, however, Dr. Bohannan is sometimes less successful. While one welcomes, in the sections on kinship and marriage, the absence of the traditional over-emphatic distinction between patriline and matriline, it seems confusing at the present time to make a terminological distinction between patriline and matriline, as 'modes of jural descent,' and agnostic and uterine descent as 'biological' (p. 135). Smaller points in this and cognate fields are the Tokiopia are surely doubtfully classified (p. 138) as a segmentary lineage society; and that the account of Cyrenaican Bedouin chieftship (p. 183) in which there is reference to 'the prophet' (sic) does not accord very well with Evans-Pritchard's description on which it is presumably based. On marriage, the discussion of the nature of the jural rights involved would have been improved had reference been made to Leach's careful analysis; and an adequate treatment of marriage payments is strangely lacking. Finally, while the concluding sections of the book on social change include a welcome affirmation of the value of a dynamic functional approach, there is too much emphasis on the outmoded notion of 'detribalization' and no reference at all to the important work of Gluckman, Firth, Mitchell and Southall—to cite only a few of the most relevant recent sources. One further general technical criticism which must be made is that, nowhere, in his frequent references to society in the abstract does Professor Bohannan inform the reader what precisely constitutes 'a society,' as a unit of social analysis. This, of course, is a difficult problem but the general reader must expect some general indications on the subject.

On a quite different plane, since this book is intended as an introductory textbook, some attention must also be given to its style and mode of presentation. Unfortunately, although Professor Bohannan has made a brave attempt to root his argument strongly in Western popular culture—with a style described in the blurb as 'lively'—his sentences are heavily charged with jargon and tend occasionally to be turgid and difficult to follow. The following (p. 103) is not unrepresentative: 'Whereas extended families are created by the addition of syndromes to one another, the aggregations of families are created by the concurrent repetition of the same roles by the same person.' And on history (p. 160): 'History is, among other things, a noncyclical linear arrangement of events, each of which can be seen as an alternative occurrence to some other occurrence, which limits or at least redirects all subsequent events.' This style of writing is surely unnecessary, and contrasts oddly with Professor Bohannan's commitment to the humanities and his attractive view of social anthropology as a kind of translation between cultures. At
the same time, it must be admitted also that in many places the book is unduly repetitious, and could with advantage have been considerably shortened. Lastly, there are a quite remarkable number of typographical and spelling errors. All this naturally detracts from the value of what might otherwise have been a most welcome addition to the slender ranks of introductory textbooks in socialanthropology.  

I. M. LEWIS


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The author is a graduate in anthropology from the London School of Economics who later worked under Popper and is now a lecturer in philosophy. His book is really several books at once: (a) a critical review of the explanations of cargo cults given by social anthropologists; (b) a discussion of the metaphysical assumptions and methodological failings of social anthropologists; and (c) a collection of obiter dicta and juvenilia. A lively style fully engages the attention, but the writing is marred by slipshod punctuation, and hasty proof-reading. The influence of Popper's own style is visible (the Popper of the contra Historicism—not that of the Logic), but without the master's sure control. The tone and content of the resulting mixture (all too like some 'I was a teenage epistemologist') raise obstacles to the reception of this book as an entirely serious work of methodology.

'Debates about method are mostly meta-meta-linguistic . . . This book . . . may be called a third order study . . . Thus the metamethodological problems are to be tackled . . .' (p. xviii).

This is the manner of (b), but in comes (c):

'Before we know where we are there is a mob of Malinowski followers charging around tearing the place to pieces and rebuilding. The barricades set up by the establishment were hasty and scanty and easily outflown. Soon the voices of the swelling mass of students could be heard demanding to know what they could do to join the cause. "Accept the principle that all men are equal and deserve an unpreserved science" they were told. "Father we are willing to believe, but show us how we may come to do so," they beseeched Malinowski. Not at all disconcerted at being the new chief-priest, Malinowski heeded their plea' (p. 43).

Meta-methodology, certainly. The author would presumably defend passages of this type by his statement that 'the scientist perhaps should be drunk the night before and stone cold sober the morning after' (p. 6).

We hear a good deal of 'Frazier: the dead father' whom Malinowski slew in this primal revolution with which, for Jarvie, anthropologists are all still methodologically involved. These Freudiana are not to be taken very seriously (least of all as even the symbolic history of what really happened). There was indeed a 'revolution' in method of sorts; I am not convinced that it was quite up to the level of examples cited by Popper himself (e.g. 'the introduction of mathematical methods into economics,' Historicism, p. 57), but this is a matter of opinion. It seems clear however that such change as occurred did not come out of methodological debate with Frazier. It requires a mental adjustment to realize that the 'revolution' of Jarvie's title refers to the world of a half-century ago, with Florence Nightingale hardly cold in her grave. The author is projecting a fantasy on the past which is, in one respect at least, more applicable to the present. There is now indeed a swelling mass of students, and some of the students are thirsting for gladiatorial combat.

The author would doubtless ask that his style, his errors and his presentation should be ignored and that his essential ideas be considered on their merits. This is a proper request, but it will be hard for him to receive this kind of justice, because their merits are bound up precisely with their extremely high level of pretension. It is a great pity that social anthropologists who might have benefited from a short well argued essay on 'falsification not verification' and other central Popperian tenets, will rightly reject this volume, and some may even be encouraged in complacency. Jarvie has done his cause grave disservice, and although his book will stir currents, by its almost certain success in capturing a 'New Society' kind of public, a more informed readership will at best merely be reminded of how to say the central arguments better. It is all too common for writers to believe (to cite an engineer) 'because they started many of the required concepts in juxtaposition, that they must really have reached the general conclusion.' While our colleagues are frequently guilty of this (see any controversy pursued in this journal), they are right in thinking that there is little credit to be awarded for advice on the presentation of their ideas, in a form worse than they could do it themselves.

This having been said, and bearing in mind the difficulty of summarizing what is a rather extended train of thought, the following points appear to be central to the author's argument: The Malinowski–Radcliffe-Brownian method of structural-functionalism was in fundamental error. Its apparatus of concepts in juxtaposition were many, were due to the correct elicitation of the 'situational logic.' But functionalism 'goes wrong when it tries to smuggle in, under the guise of producing a causal explanation, the unintended consequences of an event as a cause of that event.' Further, functionalism cannot explain social change induced from outside. 'Because cargo-cults are externally induced change no structural-functional explanations of them (as opposed to their effects or receptions) can be given' (author's italics). Much of this argument is sound, if not startlingly novel. It is disappointing that the discussion of 'social change' should be so unsophisticated. The concept as used by some social anthropologists, even today, is little more than old 'white contact' writ small, and Dr. Jarvie's distinction between 'internally' and 'externally' induced change here merely compounds their errors.

The explanation which Jarvie himself gives of the cults is, if I have understood it correctly, that they were attempts by the cultists to answer questions pertaining to the modern situation of the Melanesians in the light of the beliefs of the societies concerned. On the rapid decline of the cults:

'Assume, as I do, the following theses. (1) The initial enthusiasm was purely intellectual—a craving for a solution to a problem. (2)Intellectually, the acid test of the solution to the problem was whether the cult fulfilled its promises. (3)Intellectually, admitting that the cult failed should amount to the admission that the result of the test was negative, to the admission that the cult's solution to the problem was refuted' (p. 167).

This is intellectualism of neo-Tylorian type and not necessarily the worse for that. But we can only say (at best) that people in groups may act if they had an intellectual problem to solve. We may find, for example, that no single member of a community was actually aware of a certain intellectual problem that we could perceive, but that by their actions they had jointly solved it. In this case, we are asked to say that a community is acting as if it had failed to solve (had even refuted a solution to) a problem, which we can perceive. On the facts, we could list a number of problems which the cults did 'as if' solve, such as: 'How can we ensure that the colonial government will be shaken in the absence of mass political awareness' (Worsley might think it quite likely that at least one cultist actually saw this as a problem). Intellectualist interpretations simply provide a mode of labelling the 'situational logic.' Sometimes the fit is worth trying. But since only individuals have intellects, this mode could only claim a special status if the relationships between the ratiocinations of individuals and the 'as if' solving actions of groups were clearly understood. We do not seem so very far from the old 'steam' functionalism of individual bodily needs.

The passages in which the author makes his enthusiasm for social anthropology clear are among some of the most un-self-critical in the book.

'Ve my criticisms of them are far from attempts to discredit. On the one hand I think I can show the way to deal with problems over which they are at the moment stuck. On the other hand I firmly believe that they can improve and advance their study a great deal by adopting a new tradition of presentation and methodology, one more closely resembling that in natural science' (p. 168).
'The deeper one goes, the more enlightening it gets. This success must be trumpeted from the rooftops: here is a social science with genuine results. Social anthropology is interesting and exciting: and that which is interesting and exciting in it is already the embryo science of man waiting to be hailed and boosted, not guiltily concealed like a secret vice' (p. 176).

The author implies several times that 'earnest young students' who are 'interested in people' are offered something 'cold and dehumanized.' What people of any age find cold and dehumanized depends on their enthusiasm. Students often state 'an interest in people,' but this interest, like any interest, requires some mental stamina to pursue in depth. The author also insists vaguely at the 'ritual' of fieldwork, but has nothing to say that is not being better said in a dozen field-work seminars.

The author's fatal levity weakens the impact of comments which have insight—see the remarks on Leach and the new rethinking on p. 172. On p. 188 the temptation to arabesques leads him to discuss Malinowskian functionalism in one paragraph with the terms 'empiricism,' 'apriorist metaphysical,' 'positivist,' 'metaphysical holism,' 'the machian idea,' finishing: 'Moreover, he explained integration causally on evolutionist lines!' (exclamation in the original). Taxonomies of thought are arid enough without their being used as mere rhetorical devices. Despite the author's interest in millennial movements, one is more and more led to the conclusion that Popper's own powerful advice has been ignored: 'The more fruitful debates on method are always inspired by certain practical problems which face the research worker; and nearly all debates on method which are not so inspired are characterized by that atmosphere of futile subtlety which has brought methodology into disrepute with the practical research worker' (Historicism, p. 57). This is not far from Ryle's 'theorizing is one practice amongst others and is itself intelligently or stupidly conducted' (Mind, p. 15).

For we are no further forward after this book. Social anthropology stands remarkably unshaken, with its many faults. This is inevitable, because the author's chosen view of the subject is extraordinarily narrow. No one is nowadays standing very near Dr. Jarvis' squib (see p. viii) but Dr. Jarvis. Social anthropology was more experimental, even in the 'fifties, than the mainstream literature would suggest (and not all of that is in the bibliography), while the author tells us that the publication of Burridge's Mambu in 1960 virtually superseded his criticisms of cargo-cult explanations. (Even then he gives us an unnecessary rehabilitation of its author.) Dr. Jarvis' criticisms are just not well informed enough, nor skillfully enough deployed, to help with any of the real problems that are being daily tackled. Cries of 'Back to Frazer' (p. 175) are not, as he imagines, controversial or shocking, but simply meaningless in 1964. New developments will come out of the present, and through an increasing flexibility and competence in all departments of the discipline—incorporating the metmethodological.

EDWIN ARDEN

AFRICA


The second edition of Sociologie actuelle de l'Afrique noire has been brought up to date as far as 1962. The original theme was a comparative study of the colonial experience of two different African peoples, the Fang and the Kongo. Now it includes their emancipation and the volume opens with the note that both tribes have produced leaders of sovereign states, since Presidents Yoleu and Kasavubu are Kongo and President Mba is Fang.

The Fang are found in the Cameroons, in Spanish Guinea, and in the Gabon. The Kongo are in the former French and Belgian Congo and in Angola. Each tribe has therefore had three different overlords and the comparison of their experience in different colonial situations would be itself be well worth a volume for each tribe. Unfortunately the data for such a comparison are not to be had without a great deal of difficult research. The author has collected his material from wherever it is to be conveniently found. Most of the time the Fang in question are those in Gabon (only one seventh of the total Fang population). The Kongo in question are mostly those near Brazzaville, when it is a question of modern conditions, and those on the other bank of the river, when it is a question of tribal organization. No account is taken of this unsystematic selection of sources.

The comparison focuses on four main points, demography, religion, land and history. On all four the Kongo are held to have made a better adaptation to colonial reality than the Fang. The Kongo are dynamic, positive, receptive to change; the Fang are negative, withdrawn, static. The general impression is given that the reasons for these differences lie in the culture of the people themselves. The Fang are able to be dynamic partly because they have a dynamic population, and this latter is accounted for by the general dynamism of Kongo culture. Indeed, a very uncritical and loose theoretical framework holds the book together.

Let us start with the demographic question. It seems that the Fang have a declining or stationary population while the Kongo are increasing. The fertility of the Fang fell so low in the 1930's that it created a grave demographic crisis. In other countries a population explosion constitutes a demographic crisis, and the basis for supposing that in an undeveloped country a declining birth rate is a less healthy adaptation than an increasing one needs to be discussed. It is a pity that the demographic discussion of the Kongo was limited to those surrounding Brazzaville, and that for these no account was taken of migration across the river. One would expect the excellent public health services offered by the Belgians to make a big difference in the total comparison and that emigration from the other side might partly account for the relative proportion of young people on the Brazzaville side, or emigration from Brazzaville for work in Leopoldville might account for the relatively greater proportion of women among the Brazzaville Kongo. Without such supporting information the statistics in themselves are not very significant.

The second point of comparison is religious. Both tribes have adopted a new cult. From the very interesting descriptions of Kongo Kimbanguiism and Fang Bwiti it is difficult to see why the first should be labelled dynamic and innovating, while the other is closed and reactionary. Admittedly Kimbanguiism is not exclusively tribal in membership but this presumably reflects the metropolitan conditions of life in Leopoldville and Brazzaville, whereas the Fang are relatively isolated. Both cults have adopted elements from Christianity, and it is only by selection of details that the author can use them to build up his picture of two different responses to the colonial impact, one positive, one negative.

The third point is that the pride of the Kongo in the glory of their former empire buoys them up so that they can compete successfully in the modern market economy. This is even more dubious than the religious comparison. It is certainly true that the thought of their past greatness inspires the Kongo with a sense of national calling. But the Fang also have a past to be proud of—as conquerors and invaders. So have many other African peoples. Why did their history have this positive effect on the Fang, instead of withdrawing them from the present struggle into dreams of greatness, as it seems to have done, to Lunda, Bushong, Ngoni and other heirs of decayed empires?

The comparison on land tenure is most unsatisfactory of all. The argument that the Fang system is advantageous since it places a man securely in his appropriate social and religious cadres. His account of land-holding among the Fang suggests much the same and one is baffled as to what significant difference enables the Kongo to engage more successfully in the market economy. The Fang are patrilineal and the Kongo matrilineal in succession. Matriliny with virilocality as practised by the Kongo usually allows considerable play for individuals to make new attachments and new allegiances. One would suspect any Kongo advantage in modern conditions to

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come from this freedom for the individual rather than from a tidy pinning into the social structure. But here again, and above all, it is absurd to talk about relative success in the market economy without any serious comparison of market conditions. The Kongo are splendidly placed for exploiting the markets of two capital ports. The Fang are obviously in an economic backwater. They have nothing to sell, so access to markets for either goods or labour and the general level of economic activity in their region is much lower. This is the necessary and only basis for the comparison of the two small districts in the former French Congo, but it has not been laid.

MARY DOUGLAS


Oral literature is today a much neglected field of Cultural Anthropology. The volume of current publishing in oral literature is indeed slight, as compared with that in kinship or political structure or primitive economics. A few anthropologists still publish annotated folklore texts or engage in tracing story components through time and space; otherwise the field has been left largely to a variety of non-anthropological scholars who adduce intriguing, but scientifically quite unconvincing interpretations such as, for example, that of Jungian archetypal imagery. With painfully few exceptions—the recent works of Melvile Jacobs on northwestern North American Indian folklore spring to mind as important ones—anthropologists have not endeavoured to treat literary forms and usages of primitive societies in terms of scientifically demonstrable, significant relations of components to the dynamic functioning of their respective sociocultural systems. (Discovery of the reasons why, in terms of the history of Anthropology, the scientific study of folklore is so under-developed, might be an interesting task for someone to essay.)

The paperback, inexpensive reprint of Theodora Kroeber's The Inland Whale is most welcome. Although she has not made it her purpose to present a scientific treatise on folklore, she has presented and commented upon a selected set of California Indian stories in a manner which surely suggests to the perceptive reader the rich potentialities which such materials contain for our deeper understanding of the workings of sociocultural systems, and of the lives of people within them. Her book will also serve—and this may be its major value—to introduce the lay reader to oral literatures in a way which will enable him readily to recognize their literary qualities and to try to view them in a cross-cultural perspective of comparative literature in the wider sense, rather than exclusively from the culture-bound framework of received Western standards.

CHARLES S. BRANT


This book is the second posthumous publication of Bloomfield's linguistic work among speakers of languages of the Algonkian language family of North America. C. F. Hockett, who has made himself responsible for the preparation of Bloomfield's Algonkian manuscripts left unpublished at his death in 1949, has already published his Eastern Ojibwa (Ann Arbor, 1958), and a Menomini-English lexicon and smaller Cree-English and Fox-English word lists are promised.

This review may be the place to say so much for the discussion of technicalities as to draw attention to the significance of works such as this in our scholarly literature. Bloomfield's Menomini Language represents the results of work with informants in the field and in Chicago and elsewhere from 1920 until 1940, and during some of the work Menomini was the only language spoken with them. Its publication is very welcome. Almost all linguists become well acquainted with Bloomfield's Language during their first years of study (and all should do so); he is best known for the rigour and austere self-discipline he there practises and inculcates, an aspect of Bloomfield's work reinforced by two of his best known theoretical papers, 'A set of postulates for the science of language' and 'Linguistic aspects of science' (Language, 2, 1926, pp. 153-64, and International encyclopedia of unified science 1:4, Chicago, 1939). In the present book we have a straightforward and, as far as can be judged, a near-complete description of an American-Indian language (spoken near Wisconsin). And how good it is! No place is spent on discussion or exposition of theory, but the principles set down so well in Language are everywhere applied, and the result is a description of a language, at once easily read and understood by anyone with a minimum of education in modern linguistics, and conforming to all the demands of strict formal analysis.

The description follows what have come to be designated 'classical Bloomfieldian' lines: first the phonemics, followed by the inflectional and derivational morphology (word structure being made up of roots with the potentiality of prefixation by one of three prefixes of personal anaphora, and suffixation by members of quite a considerable array of suffixes of various grammatical functions.) The last two chapters are on syntax, to be especially commended to any one who seeks an example of the concise formal presentation of sentence structure.

Linguists should be grateful to Hockett for his editing of Bloomfield's Menomini Language on several counts. It is an addition to our literature on the important Algonkian language family, and the reviewer would rate it among the best examples we have of a description for the reader who wants to learn something of a typical American-Indian language structure. It also goes a long way in presenting another side of Bloomfield's academic personality as a worker in the field of languages as well as in that of linguistic theory. It may be recalled that Bloomfield brought to linguistics a thorough training in Indo-European, with expert command of several of its languages, living and dead, as well as first hand knowledge of languages in the Malayo-Polynesian and Algonkian families. Finally, this book would serve well as an example of what can be done to extract the structure of a spoken language from informants and to set it out descriptively, free alike from the unsystematic inclusions of so much earlier work and from the distastefully near-terminological terminology of some of Bloomfield's successors.

R. H. ROBINS

What they say in New England and other American Folklore. Collected by Clifton Johnson. Edited with an introduction by Carl Withers. New York, London (Columbia U.P.), 1953. Pp. xix, 289, illus. Price £2.50. This is a reprint of a fifty-year-old 'classic.' The introduction is so competent and comprehensive that only one question is left to the reviewer, intent on learning more about the omissions, changes and explanations of that great American collector, writer and illustrator: did any of his manuscripts survive? To us the less well known information is of greater consequence than the innumerable records with which folklorists well beyond the borders of Massachusetts are all too familiar. Here are some examples: 'People used to believe of anything that was very superstitious ... Many marvellous cures were effected (by means of blue rays), but the agitation and the interest in the matter passed away ... quickly ... (The farmers believed) at one time, in plaster as a fertilizer (and) vast quantities were sowed on the land. It was affirmed that its power was such that if you used it in a field next to that of a neighbour who did not, it would draw his manure right over the fence. Today plaster is a thing of the past ... Some said that President Lincoln met his death because he had gone to see a play at a theatre ... (and that he) saw his double before he was assassinated, ... he told his friends, he knew from that he would not live his term out.'

There is no short-cut to this book, because children's lore and stories about snakes, ghosts and witches, etc., appear in various parts. An index would, however, have been too laborious a task. The bibliography of books written, edited or illustrated by Clifton Johnson (from 1890-1938) covers almost six pages.

ELLEN ETTLINGER

One of the more fascinating oddities of American anthropology is the branch known as 'salvage archeology.' Whenever rood or building work uncovers a site or threatens a known but unexplored site, teams of highly skilled workers rush to the spot and dig a concentrated and rapid 'dig.' Ray's book might well be analogous to work under the heading of 'salvage ethnography.' His book is the result of a team effort initiated by Leslie Spier in 1938, which recorded the impressions and memories of their culture from the remaining Modoc Indians of the Klamath reservation in Oregon. The limits of this kind of investigation are obvious, but any information about the traditional culture of such a tribe is better than none and the material gathered together in this book is in many respects very full and comprehensive. The things which stick best in the memories of informants—such as aspects of material culture and details of ceremonies—are well recorded. It is in the area of social organization that the biggest gaps occur. Without actually seeing it in operation one could not do a proper analysis of, say, the political organization. What we get is the bare bones of the customary processes, and it is a tantalizing glimpse. The Modoc 'Leader' has so much in common with the Melanesian 'Big Man' that we would like to know more about him. Wealth, oratorical ability, and size of household were his distinguishing features, but there must have been plenty of room in such a system for conflict over leadership. There are several puzzling features under 'Marriages' which could really only be solved by direct observation. Why was the levirate so rigidly enforced? Initial residence at marriage was patrilocal, the wife living with her husband's family until the birth of a child, then neolocal but 'near the home of the groom's parents.' This too is puzzling, and presents difficulties when combined with polygyny. Second and subsequent marriages could obviously not have gone through this cycle. The actual composition of households would have been interesting also. A large number of traditional life-histories or even questions like 'who was in your household when you were a child—got married etc.? I might have helped. However, within the limitations Ray manoeuvres well enough and has produced a clear and crisply written account.

If it is frustrating to find a thoroughly interesting culture which one cannot study because it has passed except from the memories of old people, it is doubly frustrating to find an even more interesting one which still exists but which is impossible to study directly. White is better off in his study of Sia than Ray was in his Modoc investigations despite the fact that the Sia are still flourishing. White had to work solely with informants, and has at least the advantage that they are describing an on-going culture, but the problems are similar. Since 1929 White has wrestled with the problem of Keresan reticence and resistance to anthropologists and has succeeded in producing remarkably comprehensive monographs on the Keres Pueblos. The Pueblo of Sia is the culmination of this life-time's devotion to what must many times have seemed a thankless task. The difficulties of 'salvage anthroplogy' are evident here too. Detail on ceremonial is excellent. As informants have had to learn their ceremonial behaviour and paraphernalia consciously they can reproduce it quite well. They can also reproduce the formal politico-religious structure. But, of course, nothing short of direct observation can give the details of social life on which a social anthropologist depends for his analysis. With the Modoc of course this is impossible, but with the Sia, had he been so inclined, White might well have gathered even second-hand data which might have been useful. If informants would tell him of the most closely guarded ceremonial secrets of the tribe, they would not have given details of household composition, of the composition of working groups and so on? He chides Mrs. Stevenson for not providing data on households, but his own section on the subject simply lists the composition of four such groups. Still, he was probably wise to stick to those details which could best be elicited from informants. On this score, it seems strange that he never learned the Keres language. He complains that informants 'find it virtually impossible to help the investigator with the etymology of words,' which seems a little hard on the Sia. In the examples he gives (p. 13), ho:tan (chief) and ho:tanista (the official residence of the cacique), I suspect the -tsa on the second word is simply a verbal suffix and the word means something like 'it is the cacique's,' i.e. the house where he lives. Also nauai, his other example, does not mean the head of a society, but is a term of respect used for various people in authority. It is frequently recorded in the Keres Pueblos as the 'term' for mother's brother in some guise or other. (White has nauwi for Sia.) But perhaps White shares the common American suspicion that the learning of one's subjects' language is merely expertise for its own sake.

Another fact worthy of investigation but which White did not stumble upon until after his field work was completed is the low rate of marriage of the Sia. This seems to be a constant feature. As many as half the adult men are unmarried at one time and 'illegitimate' births are common. This would seem to reflect, as White notes, a dependence on household before family (and, of course, underlines the need for more information on households), and may be a continuation of matrilineal ideology in which marriage has a low priority. In this matter Sia contrasts with the other Keres Pueblos quite markedly. This lack of stress on marriage may well be also related to the astonishingly haphazard Kiva organization. This is stated by one informant who is resident north or south of a line drawn between the two plazas. If a man moves residence, say on marriage, he changes Kiva. Other informants say that a man keeps his affiliation wherever he lives but the War Captain can readjust membership. Certainly in Cochiti and Santo Domingo a man takes his father's Kiva and a woman her husband's. A man can change but this is a rare and grave step. With marriage at such a low premium in Sia the Kiva organization can scarcely depend on it for recruitment. The low marriage rate reflects also the non-Christian nature of the Sia on which White insists.

On the subject of Kivas White notes that various dissensions caused a breakdown in the dual organization and that one Kiva was burned down. For a while Sia functioned with only one dancing group. He wonders whether the dual division was still operative during this period. I can add to this the fact that after the destroyed Kiva was rebuilt in 1957 the Sia had two active dance groups which I witnessed in 1958 and 1959. This suggests that membership probably did lie dormant but was known, but does not settle the problem of the 'north-south' theories of membership.

There is such questions as these that make the lack of proper information on Sia frustrating and raise our sympathy with White's cloak and dagger ethnography. But, knowing his views on the nature of culture and reading his scorn for such studies as 'The Relationship between Economic Organization and Kinship Structure,' I am led to wonder what difference it would have made if White had had unhampered access to Sia. I suspect that had this been the case, he would have produced a book only minimally distinguishable from the present one. Even so, the excellent detail on the Katsina cult alone makes this a valuable book, and a worthy culmination of White's work on Keres culture.

J. R. FOX


Not less than six or seven 'interpretations' of Brazil, in French, Italian and English have been published in the last twenty years. Wagley adds to this list with his 'Introduction' to the knowledge and understanding of a complex nation, diverse even though socially and culturally integrated in a unique way. Like some of the authors of previous 'interpretations,' Wagley intends both to reveal and to interpret that nation to his readers. In formulating a thesis about the society and culture of Brazil, most earlier writers did endeavour to explain how some fundamental 'contrasts' and 'dualities' have functioned historically without splitting the Brazilian people as a unified political entity. However, some of them are motivated by such a perplexity as not to be able to convincingly substantiate the final unity and integration of that people.

An Introduction to Brazil moves to a certain extent in the opposite
direction as the author tends to demonstrate the basic unity—indiversity of the same historical and political entity. The book begins by proving the central hypothesis that 'Brazil is an immense nation of many contrasts and inner differences, and yet it has achieved a remarkably homogeneous national culture' (p. 1). This is done by the examination of a series of variables such as ecology, institutions, values and social inter-action which have operated diachronically for more than four centuries up to the present day. Starting from a characteristically social-anthropological analysis the author felicitously succeeds in Chapter 1 in concluding that 'It was the Portuguese heritage and the common experience in the New World that gave unity to the Brazilian "mosaic"' (p. 24).

In the other chapters the description and analyses are expanded. Chapter 2 deals with ecology; Chapter 3 with stratification, mobility, status criteria and particularly with the growth of the middle class during recent years, a phenomenon which is connected to economic development, expansion of education, increase of State bureaucracy and professional diversification. Chapter 6 covers religion especially in its popular expressions and interpretations, besides State organization and structure. In Chapter 7 the author, besides discussing current educational trends and achievements, analyses familialism and kin-grouping in large 'parentelas' as values and structures that explain much of Brazilian life; familialism would be a peculiar manifestation of privatism and individualism. Finally Chapter 8 is devoted to a kind of internal appraisal of the assets and weaknesses of the national culture and society: the author speaks as if 'I were a Brazilian,' in a confident yet realistic mood about prospects and expectations that at present confront Brazil as one of the most dynamic societies of our times. The last section consists of a valuable list of reading material.

THALES DE AZEVEDO


The release of this handsome volume makes available to anthropologists the first instalment of important information on the Tacana tribe in Northwestern Bolivia, gathered in the years 1952–54 by Dr. Karin Hissink and Albert Hahn under the auspices of the Frobenius Institute. Research work among the Tacana centred mainly on the settlements of Ixmamas, Tumuapasa and San José de Uchupiamonas in the province of Lurinale between the Río Madre de Dios and the Río Beni. In reviewing the collected material the authors maintain that, notwithstanding the infiltration of foreign elements after the Conquest, it may be said that on the whole the old Tacana traditions have survived to this day. Nevertheless, as a result of missionary activities, Christian elements have become a part of Tacana folklore. Themes involved are the flood and the creation of the earth, and legends of God, Christ and the Virgin Mary. These have been accepted but with evident Tacana elements. Besides, there are stories of undoubt Tacana origin into which Christian elements have been introduced.

Living in their tropical forests, hunting and collecting have for centuries been the main occupations of the Tacana. Quite naturally, legends concerning the principal animals in the bush and the indispensable wild plants have played an important part in the life of the Tacana. Animals and plants are thought of as being in the custody of anthropomorphic spirits, such as Deavovaci, Chibute and Enidui. Deavovaci and Chibute are being regarded as the Culture heroes of the Tacana and are said to be the creators of the world. The group of legends around Deavovaci and Chibute is one of the oldest, dating from a period of most intensive hunting. Hence the importance of these spirits.

Far less old is the group of legends connected with the Edutzi and the cultivation of plants. The Edutzi are considered to be the assistants of Cauaquala, the Supreme God-Creator, and are thought of as appearing in different shapes, such as that of man or bird. In many legends the Edutzi are figuring as the personification of natural phenomena, for instance the lightning or the thunderstorm.

The abundant Tacana material to hand might induce Dr. Karin Hissink, who was the scientific leader of the expedition, to start an extensive comparative study, which might ultimately result in establishing important connections between the traditions of the Tacana and those of peoples in surrounding countries. All students of South American culture-history will surely benefit from reading this volume on the Tacana. There is a very short summary in Spanish (3 pages) and an equally short one in English.

The full-page drawings by that well-known artist Albert Hahn are said to have been intentionally chosen to give the reader a clear impression of the distinctive features of those men and women who are in some way connected with the activities of the expedition.

Fieldwork among the Tacana was made possible by grants from the German Government, the Deutsche Forschungsgemeinschaft and the Wenner-Gren Foundation.

J. VICTOR JANSEN


What’s social structure? Any reader of this volume expecting to find an analysis of the major aspects of the social systems of Southeast Asia will be disappointed. For the symposium deals with the most traditional part of traditional societies, the kinship system. The nine essays which go to make it up were delivered as papers at the Ninth Pacific Science Congress in Bangkok in 1957, while the introduction was written to go out at seminars given with Eggn, Fortes and others at the Center for Advanced Study in the Behavioral Sciences in Stanford during 1959.

These nine essays, although written by an international all-star cast, are very variable in quality. Some of them display a sketchiness which is not wholly to be explained by the confines of allotted space. In many cases this thinness is due to disproportionate attention to kinship terminologies, which are sometimes identified with ‘kinship systems’ (e.g. Condominas, fig. 1, a figure which is hardly conducive to understanding, indeed positively misleading in its attempt to fit the complexities of terminology into a simplistic unilineal pattern).

The societies covered in this collection vary widely and include the ‘matrilineal’ Monkg Gar of Vietnam and the ‘patrilineal’ Miao of China. But virtually no mention is made of the ‘prescriptive marriage systems,’ so often in the forefront of recent discussions. Indeed the bulk of the essays deal with societies where unilineal descent groups are absent and it is the kinship systems of these societies that the editor, G. P. Murdock, examines in his introductory essay. His explicit purpose is to do what has already been done for unilinear descent groups, that is, to clarify the organizational principles, typology and terminology for other groupings of kinsfolk.

His analysis, based not only on the bilateral (or cognatic) systems of South-East Asia but upon the whole range of societies in his files produces three basic sub-types of cognatic social organization, the Bilateral (or ‘Eskimo’), the Quasi-unilinear (or Carib) and the Ambraline (or Polynesian). Each of these is characterized by a particular distribution of ‘structural features,’ i.e. rules of (post-marital) residence, the presence of various social groupings (‘small domestic units,’ ‘extended families,’ kindreds and matrilineage), cousin marriage and kinship terminology.

Among bilateral societies of the Eskimo type, which range from Bushmen hunters to Lapp herders to American beef-packers, the editor finds the ‘the degree of identity in diagnostic characteristics... extraordinary.’ The nuclear family is isolated in kinship terminological ramifications (Goodenough’s non-unilineal descent groups) and ‘extended families’ (in Murdock’s definition necessarily linked to lineages or ramages) are absent. His conclusion is that ‘if a society for any reason maximizes the small domestic unit and minimizes
lineal descent groups it tends automatically to arrive at a single uniform configuration of marriage rules, kin alignments and kinship nomenclature.'

Murdock draws his example of his second type of cognatic system, the 'quasi-unilineal,' from Leach’s essay on the Sinhalese of the northern Dry Zone. 'Small domestic units' become absorbed in larger 'extended families' and residence is dominantly patrilocal.

Murdock sees it as 'more cognatic than unilinear' and as commonly transitional to a 'patrilineal system of the Dakota type' (or to a similar matrilineal system), although he acknowledges that under certain circumstances such systems can achieve 'a relatively stable equilibrium.'

The third type of cognatic system, the Polynesian, is marked by an emphasis on ramaages or ambilineal kin groups, such as the Maori hapu.

Murdock's typology will certainly be productive of further discussion and research. The suggested link between Eskimo terminology, first-cousin marriage and 'small domestic units' is an interesting one. But the exact meaning given to 'small domestic units' is crucial here and yet is not, in my view, made sufficiently precise. To make the point in another way, the domestic organization of the Iban or the Bushman differs in many major respects from a household in Cambridge, England, or Pittsburgh, U.S.A. For one thing, from the standpoint of domestic economy, the latter is a boundary-maintaining unit; whereas the Bushmen 'household' is closely linked to a wider group of kin, the band.

The Bushmen remind us how variable even the simplest and most 'bilateral' of peoples can be with regard to kin terms, marriage regulations, etc. Rather than be presented with a ready-made typology, this reader would have found the latter unit in a matrix analysis of the features chosen as significant (and it is to my mind a great tribute to G. P. Murdock to suggest that perhaps only he could carry out such a programme at the present time). Such an analysis might or might not produce a number of 'types' of 'social structure,' depending upon the resulting clusters of characteristics. But tactically it would surely have been better to deal in more specific variables, including not only the kind of kin group but also features such as inheritance, technology, etc., upon which some of the particular studies place great importance.

Murdock regards in types not simply as morphological isolates but as distinct islands in the flux of culture, and he strongly disputes a limited number of the distinctive structural features tend to be treated as 'transitional.' In rather the same way the 'patrilineal principle' is seen as a specific crystallization which is not 'fully elaborated' unless residence, inheritance and lineage recruitment coincide. But broken down into more specific variables, even the presence of patrilineal unilineal descent groups (let alone virilocal residence) is in no way inconsistent with 'bilateral' (or diverging) inheritance. Indeed it is through the coincidence of these two that much of Asia and the Middle East contrasts so strongly with most of the societies of non-Islamic Africa, where distant males exclude close females from inheritance, so that one finds few if any cases of Maine’s 'appointed daughter'. These systems are not transitional in any meaningful sense, but given the system of property inheritance in the Mediterranean world, the jump to a cognatic system was clearly much smaller than it would be in the case of say the Nuer or the Tallensi. I believe this point to be of great importance for the comparative study of social systems in Asia and Africa, but it does not emerge from the present analysis because of the particular choice of structural features and because of the nature of the attempt to set up a typology of 'social structures.'

To return to our opening question, Murdock (as in his earlier book, Social Structure) defines major types of social organization largely by kinship criteria; he even names them after kinship terminologies, or to be more specific, after the ways in which kinsfolk of own or parent’s generation are classified. (Somewhat surprisingly, Leach too remarks of the Sinhalese that 'social structure is likewise a matter of kinship,' though he dismisses the terminology 'as not of any great significance' for understanding the social structure). In my view this restriction of interest, combined with the vagueness of certain of the criteria used (when is a group a group?), limit the utility of Murdock’s important beginning.

Space prevents me from considering the interesting essays by Eggan, Frake, Leach and Freeman. But before the discussion of 'choice' goes too far, I would remark that (i) in my own experience of fieldwork in societies with and without significant unilinear descent groups, members of 'unilinear' societies are not less 'free' (an ethnocentric assumption); in any case they always have the choice of not conforming; (ii) 'alternatives' is often a more appropriate concept than 'choice,' e.g. when membership of a group is based on parent’s residence; (iii) that the problems of choice, social structure, statistical versus normative rules, have been given a work-out by many philosophers, theologians and by other social scientists, some of whom must be turning in their graves at the sound of anthropologists stepping blindly on the same old treadmill, without (apparently) bothering to read what they have written.

Insistence upon choice is no salvation from the reification of 'structural principles' practised by 'certain extreme British structuralists'; one still has to make a positive effort to analyse the 'choices' that are made in relation to other aspects of social behaviour, and here it is clearly very difficult to escape from the frequency appeal to these disembodied entities not only by Murdock himself ('organizational principles', 'patrilineal principles') but also in two of the most competent essays in the book (Eggan, p. 48; Leach, pp. 117, 118). Like Murdock, I feel it is probably time we dispensed with principles—in favour of more precise analytic tools; let us purge ourselves of the fallacies of misplaced concreteness and the reification of unmeasurable forces. But on the same grounds we could well do without 'social structure,' at least when it turns out to mean 'kinship.'

JACK GOODY

OCEANIA


A master of the grand hypothesis, Thor Heyerdahl is equally courageous in putting his theories to empirical test. Kon-Tiki demonstrated that prehistoric Polynesians on balsa wood rafts could have reached Polynesia. The archæological expedition of 1956-6 was organized to discover whether in fact they did. The main effort was concentrated on Easter Island where the large and well-equipped team spent five months. The present volume describes the archæology done there and interprets its results in the light of historical and ethnological records of the island. Other reviewers have paid attention to these latter aspects of the work; here I shall concentrate on the archæology.

Easter Island prehistory is divided into three periods, Early, Middle and Late, on the basis of a sequence of architectural change affecting ceremonial platforms (ahu) established by Mulloy through detailed work on two ahu at Vinapu, but apparently fairly general throughout the island.

Briefly the architectural phases comprise: Early Period, precisely fitted masonry, especially in the seaward wall; Middle Period, the erection of the well-known statues on new or reconstructed platforms with less attention to masonry finish; Late Period, the overthrow of statues and the transformation of ahu into the mounting of stones on their inland parts.

The phases have been dated by radiocarbon (pp. 210-13, 393-6): Early Period before A.D. 400 to about 1100; Middle Period about 1100 to about 1680; Late Period about 1680 up to the missionary era (1868). Only some of the relevant dates derive from the internal evidence of ahu reconstruction. Smith (p. 212) gives general note of caution about all the radiocarbon dates reported, since no duplicate samples were obtained for any single findspot. This caution is particularly appropriate in the case of the earliest reported date, A.D. 386 ± 100, for Poike's ditch, derived from charcoal underneath the accompanying bank (p. 391).
Because the sequence is expressed in terms of architecture and masonry and to some extent statuary, it is extremely difficult to attribute other items of the archaeological record, particularly the secular and artifactual, to their appropriate place in it. Besides European items, the presence, at least in some numbers, of the tanged obsidian spearhead (mutata) puts a site into the Late Period: mutata were found in quantity in Late Period contexts at the two Vinapu ahu and two only in Middle Period contexts. For the rest the excavators rely on radiocarbon dates for attribution to period, as with the crematorium at Vinapu ahu No. 1 and its fishhooks (A.D. 1228±200) or the thick-walled stone house site E 2 and its artifacts (A.D. 1326±100), or on sheer guess work, as with the interpretation of the hare moa or chicken house as initially a Middle Period structure (p. 35).

Unfortunately the excavation of habitation sites did not produce artifacts enough, or of comparable types, from a sufficiently representative cross section of Easter Island prehistory, for artifactual sequences to be possible. For the reader wishing to make the most out of what was found, the presentation of the data on artifacts is not of the best. For some sites they are not illustrated at all (e.g. Maungu Auhepa and site E 3), for others only by photographs (e.g. Puapau Cave). At the same time the verbal descriptions by individual workers are difficult to relate and beyond the occasional cross reference there is no real attempt to assemble and analyse the data. This is promising for the adzes in a subsequent volume of the records. We may presume that Heyerdahl's review (pp. 481-9) in the chapter on Surface Artifacts is meant to do much the same for the rest.

The interpretation of the cultural record described for Easter Island is the work of Heyerdahl and Ferdon. For them (pp. 254-5, 334-8, 481-9, 493-526, 533-5) it displays evidence, in religious and domestic architecture, art motifs and artifacts, statuary and ritual, for two distinct cultural contributions, the one Polynesian, the other—and earlier—South American.

No serious consideration of this thesis can be attempted here. We may note, however, that no single clear statement of how the two cultures are related in the sequence of Easter Island prehistory is offered (cf. pp. 497, 533). Indeed at times contradictory conclusions are reached. Thus on p. 497 Heyerdahl proposes a ‘clear break’ between the Early and Middle Periods, possibly the result of a ‘new cultural impulse . . . brought about by the arrival of a second population group.’ On p. 507, however, he is at pains to produce evidence for the gestation in the Early Period of the uniform Middle Period statue type. This confusion is inevitable where it is not whole cultures that are the subject of comparison, but single traits (cf. Ferdon’s list pp. 354-5). The situation is not improved by a lack of rigour in the analysis: for example, one gets the impression that Heyerdahl accepts as Early Period statues not only those found incorporated in Middle Period masonry, but indeed any type of aberrant statue at all (p. 507). No real analysis is offered to substantiate the claim that the Middle Period statues ‘clearly have an inspiration in earlier statues’ on Easter Island. The further claim (pp. 507-10) that these latter in their turn derive from South American prototypes turns out to be based on two statues, one, the unique kneeling statue found at Rano Raraku outside archeological context (p. 360), the other at Vinapu in Late Period circumstances (p. 133-5). Sometimes relevant literature is overlooked: e.g. in respect of the claim that Middle Period statues have no stylistic counterparts outside Easter Island, the discovery of a pumice head from a coastal site in the South Island of New Zealand needs serious consideration.

The book, however, presents a mass of new factual data for specialist consideration, unaffected by the inevitable or avoidable deficiencies of the present interpretation made of them. Amongst these the botanical considerations reviewed on pp. 520-5 remain still the soundest evidence for a South American contribution to Easter Island culture. Of related interest are the pollen borings made during the expedition, the preliminary results of which suggest that woody plants were formerly more common on the island than at present (p. 519). The implication is that the explanation in terms of local lack of wood is often offered for traits of Easter Island culture aberrant from the Polynesian norm (e.g. masonry houses, pp. 334-5, 515) cannot be fully sustained.

J. GOLSON

**Notes**


The original edition of *Polynesian Navigation* was published as a supplement to *Vol. 71, JFS, 1962*. The present volume is a revised edition with changes in footnotes and supporting material.

The purpose of the symposium was to provide a detailed analysis of the Sharp thesis of accidental settlement of Polynesia from a number of points of view. Papers were solicited from G. S. Parsonson and G. M. Denning, historians, two professional mariners, Capts. H. H. Heyen and B. Hilder and Dr. C. Bechtol, a yachtsman.

Parsonson’s paper begins with an interesting history of European scepticism of native navigational ability. This is followed by an excellent textual criticism of Sharp’s writings. Succeeding sections deal with motivational factors influencing voyaging, canoes and navigation techniques. An illuminating series of European accidental voyages in the Pacific is discussed.

Heyen and Hilder’s papers demonstrate that experienced navigators can take opposite sides on the issue of primitive navigation. Heyen points out Sharp’s weaknesses in maritime matters. He presents an interesting qualitative evaluation of the probabilities of success in a series of possible voyages, and concludes that purposeful voyaging over long distances was possible and occurred. Hilder, however, minimizes aboriginal abilities and magnifies navigational hazards. He maintains that navigation without precise instruments and a coordinate system was impossible; therefore, the Polynesians could not navigate.

Bechtol’s short paper deals with model canoe hull experiments, which he believes confirm the ability of the Polynesians to sail into the wind. While the aim of the experiments is laudable, they were apparently not conducted in any systematic fashion, and results are therefore questionable. Further, no reference is made to the sizable body of theoretical and empirical literature available on such craft.

Dening discusses Polynesian geographical knowledge, based on a study of historical sources. A number of minor points of interpretation are questionable, however. This paper supports Parsonson’s discussion in many respects. He concludes that deliberate Polynesian voyaging was possible over long distances, but occurred more frequently in the more restricted areas of Eastern and Western Polynesia.

An appendix contains descriptions of 215 voyages in the Pacific. The usefulness of this listing has been reduced by grouping the voyages into categories of ‘accidental’ and ‘purposeful’ which appear to be largely unwarranted and even misleading for the specific theory under consideration as the definition of ‘accidental voyage’ does not appear to be the same as Sharp’s definition.

The volume contains a number of typographical errors, some of which are of a serious nature.

In general, each contributor has added something new and interesting to the literature on Polynesian navigation. The general area of agreement between the contributors is significant. Many of Sharp’s claims are refuted. Nevertheless, a number of fundamental problems remain unrecognized, and there are indications of a confusion of basic terms and concepts. A more objective approach following Frankel’s method of analysis is definitely needed.

R. C. SUGGS


This volume comprises the twelve papers presented at a symposium held during the Tenth Pacific Science Congress at Honolulu, Hawaii, in 1961. The editor, Jacques Barrau, states in the Introduction that the purpose of the symposium was an attempt to
see to what extent the peopling of the area can be reconstructed through a study of its ethnobotany. In this connexion he calls attention to the increasing interest in ethnobotany as a valuable source of information to both anthropologists and botanists. The symposium has made two important contributions: first, a restatement of the problems involved with clarity and without rancour; and secondly, the presentation of new information regarding the ethnobotany of the Pacific. Each paper is a self-contained unit with its bibliography and appropriate illustrations and charts, and each is a real contribution to its subject.

The subject of trans-Pacific migrations between the Old and New World is discussed by George F. Carter in 'Movement of People and Ideas across the Pacific' and by Thor Heyerdahl in 'Prehistoric Voyages of the Ancient Mariner.' A symposium on 'The Natural Environment and its Influence on the Development of the Polynesian Society' was the result of a meeting held in 1952 in New Zealand. The symposium was organized by the New Zealand Geographical Society and the New Zealand Institute of Anthropology. The symposium was attended by a number of scholars from the United States, Australia, New Zealand, and the United Kingdom. The symposium was a success and the papers were well received. The symposium was held in Christchurch, New Zealand, on 29-30 May 1952. The papers were published in the New Zealand Geographical Journal as Special Issue No. 1, 1952.

The collection of songs is systematically classified, e.g., as lullabies, laments, and songs of love. Each song is provided with an introductory history and explanatory notes to lines in both Maori and English, with notes of tribal origin and author where this is known. Volumes one and two of the series provide a wealth of Maori lore, and in addition the songs give an insight into the sensitive and poetic feeling of the Maori people. It is noted in the foreword by Dr. Biggs that Te Hurumi is preparing a translation and revision of volume three and this too will be welcomed by all students of Maori lore, especially those concerned with the Maori language. T. BARROW


The lengthy history of contact between Australia aborigines and Whites yields few examples of aboriginal ‘adjustment’ movements. In this monograph, Berndt introduces us to a movement among the Wulamba peoples of eastern Arnheim Land. This movement, which is known as the ‘Wulamba movement’, is characterized by the displacement of the Wulamba peoples from their traditional territories to other regions in the Northern Territory. The movement is marked by changes in the social and economic structure of the Wulamba communities, including the adoption of new forms of land tenure, the introduction of new technologies, and the development of new social institutions. Berndt’s monograph is an important contribution to the study of aboriginal history and culture in Australia.

The Pan-Wulamba nature of the movement is thus more than a typical example of aboriginal adaptation to European contact; it is a case of deliberate and significant cultural change. Berndt’s study of the Wulamba movement is an important contribution to the study of aboriginal history and culture in Australia, and it is a valuable resource for scholars interested in the history of European contact with aboriginal peoples.
ANTHROPOLOGY, PSYCHIATRY AND THE DOCTRINE OF CULTURAL RELATIVISM*

By Derek Freeman, Ph.D., Institute of Advanced Studies, Australian National University, Canberra

Let me begin by giving a succinct outline of the doctrine of cultural relativism. It has been stated in its most explicit form by the American anthropologist Melville Herskovits in his book *Man and his Works* (1948). The principle on which cultural relativism rests has been stated by Herskovits in these words: ‘Judgments are based on experience, and experience is interpreted by each individual in terms of his own enculturation.’

With this principle, in itself, there can be little disagreement, for it is certainly true that the judgments which men form are, ordinarily, much influenced by the values of the culture in which they have grown up. Thus, Sapir and Whorf have shown the extent to which human languages, as symbol systems, colour and distort the thinking and experience of those who use them.

This state of affairs we may then admit. But it is also true, and most importantly, that men may transcend the limitations of cultural evaluations by means of scientific enquiry and discovery—for science is a system of enquiry and knowledge that transcends the separate cultures of mankind.

It is not, however, this unexceptionable principle of cultural conditioning, but the inferences drawn from it, that make up the doctrine of cultural relativism. These are the inferences that whatever occurs in any particular culture is ‘normal,’ and can be understood and evaluated only in terms of the culture of which it is a part.

These inferences are plain in the reply made by Herskovits to the psychiatrist Dorsainvil, who had described the state of possession hysteria that characterizes Haitian voodoo as ‘a religious psycho-neurosis.’ ‘In terms of the patterns of Haitian religion,’ wrote Herskovits, ‘possession is not abnormal but normal, it is set in a cultural mold as are all other phases of conventional living.’

Thus, the cultural relativist conceives of a culture as a reality *sui generis*, as something given and self-explanatory. Culture is looked upon as a closed system, which can be assessed only in its own terms, and which, therefore, cannot be analysed, interpreted and evaluated in other and more objective ways. This doctrine, as Sidney has noted, has proved most congenial to many contemporary anthropologists.

I wish to advance the view that cultural relativism is an unscientific doctrine. It is my opinion, indeed, that not until this doctrine has been transcended will it be possible for a true science of man to come into being.

To assert, as does Herskovits, that behaviour is normal because it is set in a cultural mold, is to say no more than that it is shared and accepted by the members of the culture concerned, but dereistic thinking and behaviour are not one wit the less dereistic because they happen to be shared and accepted. Indeed, the fact that a delusional belief is shared commonly results in an intensification of affect for the individual participant rather than in any diminution.

A basic question, then, is this: Can a culture, either in whole or in part, be diagnosed as abnormal or maladapted, in a psychical sense?

This, it will be noted, is essentially the same question put by Freud in his essay, *Civilization and its Discontents*. ‘If,’ asks Freud, ‘the development of civilization has such a far-reaching similarity to the development of the individual and if it employs the same methods, may we not be justified in reaching the diagnosis that, under the influence of cultural urges, some civilizations, or some epochs of civilization—possibly the whole of mankind—have become “neurotic”? And he goes on to express the hope that one day someone will venture to embark (despite the difficulties involved) upon a pathology of cultural communities.’

In the quotation which I have just cited Freud remarks on the similarity between cultural and individual phenomena. This, as Freud himself stresses, is an analogy, but it is, nonetheless, an analogy with some substance, for the same primary process thinking that characterizes the fantasies, dreams and symbolic behaviour of individuals also characterizes the myths and rituals of cultures, and this for the good reason that these cultural or shared forms of symbolic behaviour are all, finally, endopsychic in origin.

One of the assertions of cultural relativists is that cultural phenomena, however bizarre, by the very fact of their existence, *prove* ‘that they perform their essential tasks. Otherwise, the societies wherein they function would not survive.’ This argument, though specious, is of little weight, for, in a positivistic way, it confuses mere survival with essential or adequate functioning.

Konrad Lorenz has remarked that ‘in the evolutionary process you find everything that is just good enough for survival’; and this, I would suggest, is a basically important realization. Neuroses and psychoses, certainly, are adaptations of a kind: ways in which an emotionally and mentally disturbed individual manages to go on surviving—thus the so-called positive value of the neurosis. In the same way, it is clearly possible for pathological cultural adaptations to achieve survival, and yet remain pathological.

The cultural relativist, we have seen, looks on culture as a thing in itself, he conceptualizes it as a closed, self-explanatory system. He accepts culture as something given, and does not enquire into its origins. This, it may be noted, is close to the view of the uneducated who accept the culture into which they happen to have been born, rather as a teapot accepts hot water.

The evidence of research in paleo-anthropology, a
branch of anthropology which in recent years has made great progress, enables us to view human cultures in what is, I believe, a more scientific way.

There is now decisive scientific evidence that man is an animal, and has evolved from a primate stock. This means that at an early stage in this evolution there were proto-hominoids without any form of culture. It follows that man’s nature, as a phylogenetic entity, was evolved prior to the emergence of culture. Thus, the scientific way to look at culture is as a biological adaptation by way of which evolving human animals have come to terms with their natural impulses and their environment (including their social environment).

The picture which the anthropological evidence compels us to form is of an evolving proto-hominoid, vastly ignorant of the nature of its own organism, of its origins, and of the processes of the world about it; which yet, by the use of a symbol system, its own invention, and an inquisitiveness born of long millennia of primate existence, embarked on uniquely human course of seeking, by the use of its tacit powers of comprehending experience, the meaning of its existence and the nature of the world about it.

Now primitive cultures, as they have been observed, always contain a number of behaviors which are highly adaptive and rational, but they commonly also contain forms of ritual behavior of a very different character, and indeed, these are frequently of a kind which, removed from their socio-cultural setting, are plainly psycho-pathological.

How then did such pathological adaptations come into being? My hypothesis is that they are the outcome of primary process thinking, which is universally characteristic of the human animal, being manifested in his dreams and fantasies, his myths and rituals, and which is the product of his impulsive or instinctive nature.

This, of course, is no new hypothesis, for many psycho-analysts and psychiatrists have drawn attention to primary process thinking in primitive cultures, and to the close resemblance which it bears to schizophrenic thought; as, for example, Röheim in his book Magic and Schizophrenia. Again, Bettelheim has recorded how groups of children at the Sonia Shankman Orthogenic School of the University of Chicago, ‘all of whom could be classified as schizoid if not schizophrenic,’ spontaneously devised rituals which closely resemble those found in primitive societies.

In primitive societies, then,—as indeed in all others—we encounter shared fantasies (which we call myths) and modes of symbolic acting-out (which we call rituals) all of which spring from primary process thinking, and which are thus projections of the human animal’s impulsive, phylogenetically given nature. Loren Eiseley, who has aptly described man as ‘a dream animal,’ puts the matter dramatically by saying: ‘ancestral man entered his own head, and he has been adapting ever since to what he finds there.’

This of course means that culture, in its symbolic and ritual aspects, is not a closed but a projective system; and, therefore, as Schilder and others have noted, cultural phenomena have to be interpreted or deciphered like a

neurotic symptom or like a dream. The implications of this for the science of anthropology are of a radical and revolutionary kind.

Let us now glance at some instances of the kind of cultural phenomena of which I have been speaking. I have selected three examples from the many available in the ethnographical record.

(1) The first is the deception of anus-plugging as it existed among the Chaga, a Bantu people of the Kenya highlands near Kilimanjaro. Chaga men justify their claim to ascendance over women by acquiring power over a bodily function that women cannot control; they maintain that during initiation the anus is stopped up permanently and that men thereafter retain their feces. To be ‘stopped up’ is identical with acquiring the rights of an adult male, and, the stopping-up of the anus is the central rite of initiation. Thus the novices are told: ‘Don’t emit wind in the presence of women and uninitiated youth. If you do the tribal elders will slaughter your cows. Neither must you be surprised by women when you defecate. Always carry a stick with you, dig your feces in, and scratch about here and there pretending that you are digging for some charm... If you dare to tell anybody of the secret of men, then your age group, the tribal elders and the chief will without mercy deprive you of all you own.’

Until recently the fiction was carried through to its final conclusion with a performance in which the plug was removed. A group of men would gather at the home of an old man and slaughter a goat, tying bleeding pieces of meat around his thighs so that blood ran down and covered his legs. Then they would remove and hide the bloody meat and call his wife, entrusting the old man to her care and explaining to her that her husband had had his plug removed for his son’s sake and that the bleeding started when the stitches were taken out.

Here in brief, we have the acting-out of a fantasy that springs from envy of women and the impulsive desire to dominate them.

(2) My second example is from the culture of the Aztec of ancient Mexico. The Aztec shared the delusional belief that the sun was an anthropomorphic god, Tonatiuh, and that his vitality must continually be renewed by human sacrifice. Thus, the breasts of sacrificial victims were cut open, and their hearts removed, and held aloft in presentation to the sun god, to keep him satiated with blood, and so strong and virile.

Here, we have a delusional projection of human properties onto a purely physical object, with highly pathological consequences.

(3) My third example comes from the culture of the Willigiman-Wallalua, a contemporary people of western New Guinea, recently studied by an expedition from Harvard University. The eschatological beliefs of these people demand the ritual amputation of the fingers of young girls to placate the ghosts of the dead. So, when an eight-year-old boy was killed in a raid by a neighbouring tribe, three girls were selected by their elders to donate two fingers each as funeral gifts, and each had two joints of each digit ritually amputated with a stone axe.
Here, as I would interpret these barbaric rites, we have a shared delusional belief system, in which living humans project on to the non-existent ghosts of the dead their own aggressive and revengeful impulses, and then, out of fear, are impelled to placate that which threatens them by the sacrificial offering of the fingers of their own children.

Each of these three examples of cultural behaviour, if removed from its socio-cultural setting, is seen to be symbolic behaviour of a neurotic if not psychotic kind. I feel sure that such would be the diagnosis of a psychiatrist if he encountered such behaviours in the course of his ordinary practice.

There remains the question of whether such behaviours can be properly diagnosed as neurotic or psychotic in their original cultural setting.

This, I think, would be the view of some psychiatrists. Karl Menninger, for example, in *Man Against Himself*, expresses the opinion that: 'Psychiatrists would regard the extreme martyr and ascetic as individuals completely gone over to the acceptance of an unrealistically exaggerated interpretation of life, a kind of socially accepted psychosis.' And in *The Human Mind*, in discussing mass paranoia, he writes of the 'psychosis' of the German nation.

In considering this question we ought, in my judgment, to be cautious of the dangers of thinking by analogy. Devereux, rightly in my opinion, has stressed the need to distinguish between cultural beliefs (even though they may be delusory in character) and delusions proper, as they occur in individual psychotics.

The distinction, I would suggest, is that a cultural belief (as the etymology of the word indicates) is received from someone else, commonly someone in a position of dominance, by way of such psychological processes as identification: whereas a delusion proper is idiosyncratic, predominantly or wholly endopsychic, and born of morbidity.

It remains true, however, that the unconscious and primary mental processes of all the members of a culture are, in basic ways, identical with those to be found in neurotics and psychotics. It is this state of affairs that accounts for the fact that paranoid ideas and delusions are so often contagious. Again, it would seem likely that delusory cultural beliefs and mass delusions can, in very many cases, be traced to the delusions of a neurotic or psychotic individual in the group.

There is here a field in the exploration and understanding of which psychiatrists and anthropologists might fruitfully collaborate.

I have, during the course of my remarks, several times used the term pathological to describe certain forms of cultural behaviour. The final question which I would like to advance for discussion is this: How are pathological forms of cultural and social behaviour to be recognized and defined?

Clearly, if the science of anthropology is to transcend the doctrine of cultural relativism it must evolve ways in which human behaviour—of whatever culture—can be scientifically and normatively evaluated, this being an essential step towards the discovery (to use the words of Bidney) of 'universal principles of cultural dynamics and concrete rational norms capable of universal realization.'

In attempting this task, the values and discoveries of the science of psychiatry are, in my view, of the greatest relevance, and I believe that the best and most scientific criteria presently available for the evaluation of behaviour are those currently being evolved in research on states of positive mental health. I would like to mention two important books dealing with the results of this research: Marie Jahoda's *Current Concepts of Positive Mental Health* and Abraham Maslow's *Toward a Psychology of Being*.

I shall not here attempt to survey this significant field of research, but I would like to take as an example one of the criteria that appears in most definitions of mental health: superior reality perception.

Money-Kyrle, a prominent Kleinian analyst, has recently indicated that he believes it possible to call a neurotic person not only relatively inefficient but absolutely inefficient, simply because he does not perceive the real world as accurately or as efficiently as does the healthy person. 'A man's beliefs,' he writes, 'do not have a relative degree of truth measured by their approximation to the prevalent beliefs of his own or some other culture arbitrarily chosen as a frame of reference. They have an absolute degree of truth measured by their approximation to the facts which alone can prove or disprove them.'

In other words, the neurotic is not only emotionally sick—he is cognitively wrong.

Here we have an empirical and scientific principle which is very different from that advanced by Herskovits, and which enables those who employ it to transcend the doctrine of cultural relativism.

The Correlation of the 'Romano-Celtic' Temples. By G. R. H. Wright, *The Damascus Museum*. With three figures

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The Romano-Celtic temples have frequently attracted comment because of the unexpected type of plan—a square cella enclosed in another square surround to give a peripheral ambulatory (fig. 1). The last word on them seems to have been said by R. E. M. Wheeler (as he then was) in 1928: 'The Romano-Celtic Temple may therefore be regarded not as the representative in any significant degree of a pre-Roman architectural tradition, but as the result of the adoption of essentially Roman elements by native architects shortly after the final conquest of Gaul.'

This is pontifical, but is it cogent? Certainly it seems to have been largely accepted, or at any event not closely questioned. True, a recent French work, as might be expected, emphasized the significance of the Gallic element in the design, but the understanding of the architecture is left to Wheeler's article, which is quoted. Thus H. Hubert, in an encyclopedic survey of 'La Civilisation Celtique,' says: 'Elle [Gaul] avait déjà sans doute des temples et nombreuses. En tout cas, la civilisation romaine la couvrit de bâtiments religieux. Mais ce que prouve l'originalité du temple gaulois, c'est que parmi ces temples de l'époque romaine, il y en a d'un type si particulier qu'il feut recouvrir pour en rendre compte à l'hypothèse d'un héritage gallois.'

That is, from one aspect or another, it is the isolated local nature of the type which is emphasized. The present note, written from a patently eastern background, seeks to show that this view is
First let us define the elements of the subject matter—the plan and sitting of the 'Romano-Celtic' temple. This may be done most conveniently by abstracting from Wheeler's article. He says: 'The structure consists essentially of a square, or nearly square, cella surrounded on all four sides by a portico or verandah. In some instances the floor of the cella rose at the back to a low platform. This platform presumably carried the altar or statues. More often, however, a square basin in the centre of the floor indicates the position of the cult object. It is likely enough that in many cases an altar may have stood outside the temple... The site chosen for these temples was frequently a hill top... In certain cases pools or springs are associated with the temple. The sacred temenos is often surrounded by a precinct wall. Within the temenos buildings other than those of the main temple or temples were frequently included. In some cases these may have been smaller shrines and in other cases bath buildings or residences connected with the cult.' Additionally an important factor is the orientation, and again this is unexpected. It is, in the main, diagonal, i.e. the angles of the structure are oriented on the cardinal points of the compass.

Having defined the phenomenon, Wheeler goes on to draw his inferences from the archaeological facts. It is only fair to present these at the outset. The chronological occurrence of the type seems to be from the first to the fourth centuries of the Christian era, i.e. the sites were in use throughout the full Roman period. As for the geographical distribution, the type does not occur south of the district adjoining Lyons and Clermont-Ferrand, nor east of Lake Constance with the exception of the stray specimen far away at St. Paul, south-west of Vienna.

Analysed in more detail the archaeological facts yield to Wheeler the following information:

1. The type is absent from the Mediterranean lands, and from the first and most intensively Romanized province of Gaul; nor was it known to Vitruvius.
2. It is detached from the more official spheres of military life by its non-occurrence in the military frontier zones.
3. Although it is found occasionally in highly Romanized urban sites, it occurs more frequently in country districts, or on open hill tops bordered by woodland and close to springs.
4. The cults are native in character.

Proceeding from this Wheeler draws his conclusions as follows:

These four considerations point to the origin of the type in a native environment somewhere within the Celtic regions north of Provence and west of the Rhine. On the other hand there is not a particle of evidence for the assumption of a pre-Roman origin for the type. Are these conclusions justified? Indubitably from the evidence adduced there is the valid conclusion that the type does not derive from a classical (Graco-Roman) exemplar. Beyond this the arguments are ex silentio. What durable architectural remains

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**Fig. 1. Two Romano-Celtic Temples in Southern England**

Temple at Harlow after Wheeler, temple at Worth after Antiq. J., Vol. VIII, 1928, p. 77. N.B. Similar orientation and scale to the Eastern examples

**Fig. 2. Eastern Examples of the Square Plan with Surrounding Ambulatory**


**Fig. 3. Temple of Jupiter at Altrachtal Near Trier**

Reconstruction after Miylius

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**Fig. 4.** Achaemenian, Nabataean, Parthian, Sassanian.
might one expect from pre-Roman times? The geographical distribution quoted is precisely the area of the most intensive archaeological activity in lands occupied by the ancient Celts. What may new interest in excavation in Danubian lands reveal? Wheeler's localization in time and place of the origin of the type is not convincingly established by his facts.

His positive arguments to account for the origin of the plan one is hesitant to recapitulate! It is simply not an architectural solution. Briefly, he sees the peripheral ambulatory, which he rightly regards as 'the distinctive feature of the... series,' as a version of the classical peristyle assimilated through the example of a centralized, classical plan, i.e. the round temples. That is, the fundamental feature of an integrated plan is explained adventurously. This is indeed erant and vitriates the substantive conclusion that the plan is 'the result of the adoption of essentially Roman elements by native architects.'

So much for the analysis of what Sir Mortimer Wheeler says. We now turn to the more significant matter of what he does not say. His mention of analogous material other than classic is limited to the following disparaging sentence: 'Attempts which have been made to find Tuscan or even Egyptian prototypes are so fantastic as to render discussion of them unnecessary.' Where, or by whom these 'fantastic' attempts were made, I do not know and it is not my concern to discuss them; but I can only express concern at the omission of a body of analogous material which, at the time of Sir Mortimer's article, was reasonably well known.

The work of de Vogüé and Butler brought to light a group of temples in the Hauran (Southern Syria), for the basic description of which the remarks abstracted from Wheeler's article could stand almost unchanged. These temples, dating to the commencement of the Christian era, were in the Nabataean realm dedicated to Nabataean gods and, although other Nabataean monuments have since been discovered on this plan, it was immediately seen that the plan was alien to its general context. When the work of M. Dieulafoy at Susa revealed a monument of identical plan, the Ayadana (i.e. 450 B.C.), there could be little doubt that this plan had made its way to Syria from Persia.

The plan was also represented at Hatra in Parthian times and subsequent investigation by Herzfeld and Godard showed its persistence in Persia through Parthian to Sassanian times when, with a new arcuated structure, it became the standard Fire Temple which still has modern representatives among the adherents to that cult. Finally, most recently, the work of M. Schlumberger at Surkh Khotal in Afghanistan has brought to light another notable example of this type dating from the first half of the second century A.D.

What are the defining features of this group of temples which stands over against the classical megaron and disputes with it in marginal periods and regions? Essentially the square cela and the surrounding corridor which isolates the sacred place from the profane world. This square is not an addition, an adoption, an adaptation, as Wheeler's account of the Celtic temples suggests. It is a basic element of the functional plan, conditioned by the ritual requirements.

Now the nature and development of Persian religious practices is treated in a vexed subject, complicated by the interweaving of Zoroastrianism with the old religion. However, recent work has brought a measure of understanding to the subject. Fortunately the details do not concern us. It suffices to remark that, throughout, the ritual tendency on the sacred fire was carried out only by priests in a state of utmost purity—the ranks of the faithful were kept rigorously at a greater or less distance. This, to the Greeks with their democratic ideas, meant that the Persians had no temples, as Herodotus says in a famous passage; and if we define temple essentially as 'a place of public worship,' they were essentially right. Or again, as they might have said, the cella (naos) of this 'temple' was an adyton, more a krypta. When we reflect that the 'places of the fire' were sanctuaries of adoration where only the most pure of the priests entered once a day for an hour, barefooted, without cloaks, their mouths and nostrils covered to avoid possible soiling of the sacred element, we can well see the virtue of this surrounding ambulatory. Is it not possible that original Celtic religious ideas, with the overwhelming emphasis on the sanctity of the Druids, might have been of the same order?

Such at any rate was the type of the Achemenid Ayadana, mentioned by Darius in his famous inscription at Behistun and thence, perhaps misleadingly, translated 'temple.' M. Dieulafoy's labour and insight at Susa reveal to us the details of the plan and show us that it stood at the head of our present subject matter. 'C'est un beau monument dont la composition, d'une franchise admirable, accueille nettement l'endroit essentiel, le saint de saints, qu'une double enceinte protège contre toute profanation, l'asteur proprement dit. "Lieu du feu" au centre duquel brûlait le feu divin et éternel.'

Other elements of the type follow logically. Basically, as is well known from Herodotus and Strabo, the Persians adored the natural elements, fire, earth, air (winds), water, and sought out open high places to perform their adoration. Thus the Persian structures, although occasionally associated with urban requirements, were always at home in the wilderness, high places with adjacent springs; courts would have been built up if necessary. This is a feature of the Nabataean sanctuaries, and is not of the 'Romano-Celtic' temples. 'The site chosen for these temples was frequently a hilltop... In certain cases pools or springs are associated with the temple.'

Again the close restriction of entry to the Persian sanctuary with its principal, central cela, demands a court where the less sacerdotal of the community may assemble. This in turn is provided with ceremonial altars. Finally the siting of the sanctuary in a withdrawn locality imports a peribolos enclosing both domestic and service structures. Such are the Nabataean sanctuaries in the Hauran, and again Wheeler's delineation of the Celtic type, quoted above, runs parallel: '... a square basis in the centre of the [cella] floor... an altar... outside the temple... The sacred temenos... surrounded by a precinct wall. Within the temenos buildings other than those of the main temple or temples... smaller shrines... bath buildings or residences connected with the cult.'

Finally we may note the orientation of these eastern buildings. As with the Celtic group, where a significant choice is available, this is almost invariably diagonal. In the eastern tradition this is a matter of some import—it denotes sacred architecture. Surely the congruence of these characteristics is sufficiently striking to warrant mention in a discussion of the origin of the type of the 'Romano-Celtic' temples.

It is within neither my province nor my capacity to settle the fundamental 'Kunstgeschichte' of this form and account convincingly for its wide dispersal. It is difficult enough to keep sight of its origin and development in the heartland of Persia. On structural considerations the form would seem proper to a primitive domical tradition. This gives square rooms, squinched, or corbelled, with the surrounding corridor roofed at lower height, taking the thrust exerted by the dome. If there is anything in Strzygowski's Altai-Iran theories of domes and their origins, this suggests Persian memories of the primitive architecture of their traditional home. Be this as it may, the form bursts into overwhelming prominence in the imperial architecture at Persepolis. Here the new insistence on square units is unmistakable. The hareem of the palace consists of an amazing multiplication of these square units with surrounding corridors. In this connexion,
of course, there exists a cognate functional necessity for isolating the sacred (haran) thalamos. However, this first, sensational appearance of the type is structurally completely divorced from any possible arcuated origins. In an unwooded land, it is a perverse "megalytic" architecture, which was the expression of the resources of a world empire and vanished with it. This is perhaps its only explanation, for domes and vaults in pre-Roman antiquity seem always to have been held a mean and humble form of architecture. When the plan became prolific in Sassanian times, domed and vaulted in the traditional medium, it was capable of being expressed on almost any scale, from the Great Hall of Firuzabad, to a small kiosque, and in the Moslem period inspired the great sanctuaries of the Shi-ite Imams in Iraq and Iran.

Such is a brief account of this vital type of plan in Iranian lands. Others more learned, with specialized information at their disposal, may work out the details of the correlation of the 'Romano-Celtic' temples. As it is, in the face of this material I doubt that it can be longer maintained that the 'Romano-Celtic' temples 'originated in some single spot [in Gaul] ... presumably from a single, unknown [Gallic] architect ... shortly after the conquest of Gaul.'

Notes
1 That is available to me.
2 Wheeler, p. 317.
3 Plommer, p. 321: 'Those strange Romano-Celtic temples, on plan two concentric squares.' This cursory and uncritical reference by one habitually given to incisive analysis, is significant.
4 Hubert, p. 294.
5 The elevation is not certain since the temples are 'always too ill preserved for us to restore the roof or even its inner supports' (Plommer, p. 322).
6 The description of the ambulatory as a portico or a verandah is slightly tendentious, since in many examples the enclosing wall may either be entirely built up or form a high balustrade bearing dwarf columns.
7 Wheeler, passim.
8 Wheeler, p. 370.
9 Ibidem.
10 See (for convenience) Butler, passim.
12 Herzfeld, op. cit.
13 Godard, op. cit.
14 Schlumberger, op. cit.
15 Herodotus I, 131.
16 Godard, p. 13.
17 Hubert, p. 294.
18 Strabo, XV, III, 13.
19 Butler, Section A, part 6, illus. 324, opposite p. 372: 'Ancient Seecia was not a town or a village ... but an ancient "High place" or sacred precinct ...' loc. cit., p. 367.
20 Wheeler, quoted above.
21 Ibidem.
22 Such seems to be the traditional Mesopotamian practice.
23 Compared with Pasargadae (Frankfort, p. 218).
24 Frankfort, fig. 110.
25 Godard, p. 8: 'Fantaisie Royale.'
26 The arcuated structure may mislead the non-technical reader into seeing a fundamental change in plan, since it breaks the line of the cella wall, but this is solely a matter of construction, not of design. Likewise the four internal columns of the more pretentious members of the earlier group are simply a necessary structural device to support an extensive flat timber roof. The roofing of the Celtic examples is uncertain (Plommer, p. 321) but is generally restored in the form of a pyramidal, hipped roof.
27 Herzfeld, p. 93: '... the cupola over the square room as tomb chamber, the cenotaph at the place of the fire altar, a vaulted passage around for the tawaf.'

Some Zande Animal Tales from the Gore Collection.

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In the late Mrs. E. C. Gore of the C.M.S. published Pa Rika Anya na Aisan, a collection of stories about animals and stories illustrating proverbial sayings. A second edition appeared in 1954. Both editions are now out of print. There is no English translation in print nor, so far as I have been able to ascertain, in manuscript; so the tales are accessible to only a very few people other than the Azande themselves; and they are of sufficient interest to be recorded in English. Since nobody else is likely to undertake the task I have last year translated them myself, using the 1954 edition.

I will not now discuss the difficulties of translating from the Zande text without hearing it spoken. I have only to say that having made a translation of these stories and others published by Mrs. Gore I arranged for Mr. Angelo Beda, a Zande student at the University of Khartoum, whose services Professor I. G. Cunnison obtained for me, to make an independent translation as a check on mine. There were no serious discrepancies between the two versions and I have used whichever rendering seemed most appropriate in any particular place. The numbers after each title are those of the Zande texts in Mrs. Gore's collection.

How hatred began between chicken and kite (13)

Chicken happened to lie in the courtyard, spreading her wings and with her legs tucked under them. Kite came and saw her and asked her, 'Daughter of my maternal uncle, why do you lie in the courtyard?' Chicken replied, 'I tore off my leg and gave it to my children to go and hunt animals with it, so I had myself to lie here in the courtyard. Whenever they go with my leg to hunt they kill many animals.'

Kite went home and beat his drum, and all his children and all his subjects gathered. He tore off his leg and gave it to his children to go and hunt with it. When they set off he lay in the courtyard in pain from the wound near to death. He sent a message to his children for them to return. When they came back he said to his children, 'It was Chicken who deceived me, saying she had torn off her leg and had given it to her children to go and hunt animals with it, so that they might kill many animals; and that is why I am about to die. Whenever you see a chicken do not spare it, and whenever one catches her chicks seize them all.'

Chicken summoned all her relatives and said to them, 'do not drink water without looking up. If you do Kite will catch you.' After this the kites started to catch all chickens because of that
pledge their father imposed on them, since Chicken had deceived him so that he tore off his leg and was sore wounded and died from the wound.

That is why people say it is bad to lie, that a lie is a very bad thing. People also say that deceit is bad, that it is a very bad thing. That is how hatred began between chickens and kites.

**HOW GREY DUKER DECEIVES DOG (14)**

When people go hunting duikers they collect their dogs and tie palm-nut bells to their necks, and they take nets with them also. When they have finished erecting the nets a man goes with a dog into the bush to send it after the duikers. When a duiker hears the movement of the dog by the ringing of the bell it pierces a leaf of a large leaved tree with its hoof and conceals itself behind it and comes towards the dog, and the dog will not be able to see it properly. As the dog moves one way the duiker turns the other way. The duiker thus constantly turns the big leaf in the direction of the dog until the dog becomes tired without properly seeing the duiker. Then the duiker runs off and dashes out in a different direction where there are no nets.

The duiker has two tricks. The other is that if people encircle a duiker with nets in a wood it starts to dance its witchdoctor’s dance. As it dances its witchdoctor’s dance it sings thus,

'very much has happened to me duiker’s wife,
I just dodge towards net
and dash towards dog.'

After finishing the witchdoctor’s dance it runs with all speed and, avoiding the nets, it dashes in the direction of the dog and runs off safe.

**DISAGREEMENT BETWEEN WATERBUCK AND BUSHBUCK (15)**

A man trod round a patch of bush for netting game4 and left it and went home. Waterbuck went for a walk together with Bushbuck. When they had walked for a long time they reached the hunting patch of that man. Bushbuck said to waterbuck, 'I am going to lie down at the edge of the patch myself so that if I hear the sound of the coming of the person who trod round the patch I can run away quickly.' Waterbuck said thus to Bushbuck, 'no, I am myself going to the middle of the patch to sleep there. If they attack me there I shall come running and dash out by the way I entered.'

Bushbuck separated from Waterbuck and lay near the edge of the patch and slept. Waterbuck went into the middle of the patch and lay down and slept there. The owner of the patch came, and when he searched the ground he saw the spoor of Waterbuck and Bushbuck where they had entered his patch of bush. He went round the patch without discovering other spoor which showed that they had left the patch. He hastened home and called on the people to come to him with nets. When many people had brought their nets they went and erected them next Waterbuck and Bushbuck.

They finished erecting their nets and began to station boys round the patch to scare out the animals they had put up their nets for. When the boys had just begun their cries Bushbuck was already on his feet and dashed away as fast as he could. When Bushbuck had run away they said that there were two animals, the other one was still in the bush. They started to stalk Waterbuck and they stalked him till he started off. He came running, and when he tried to dash out of the way he had entered he fell into the net, and men speared Waterbuck and killed him. Bushbuck stood far off and said to Waterbuck, 'you see; I have been giving you advice in vain, you just counted me a liar, and you went into the middle of the patch. Now that you are dead, what about it?'

**DISPUTE BETWEEN ELEPHANT AND SNAIL (16)**

Elephant disputed with Snail.5 Elephant said he could not run with Snail, he would leave him behind. Snail said, 'Elephant go home and spend two days there.' Then Snail summoned his friends to come. When his friends arrived he told them that he could not run but if Elephant started to run and asked for Snail let one of Snail’s relatives say, 'I am here.' Elephant would run and see yet another Snail, and that one would say, 'I am here.' Elephant ran until he was exhausted and halted. Then yet another Snail appeared and said to Elephant that since he said we could not compete in running, 'now I have defeated him.'

**HOW HATRED BEGAN BETWEEN MONKEY AND DOG (17)**

Dog went for a walk in the groundnuts cultivation. When Monkey5 saw Dog taking a walk he asked, 'Dog, my friend, why do you always cry at home?' Dog replied, 'Who told you that I cry? It is the oil that I eat at home, and usually when I have not yet finished one lot they call me to eat another.' Monkey asked Dog, 'My friend, what can a person do to eat that sort of oil you eat?' Dog said to Monkey, 'Come in one evening and lie down by the side of the homestead and when I come to you lie flat on my back and hold on to me tightly.'

When it was evening Monkey came to near the homestead. Dog went to Monkey and said, 'My friend, just lie as flat as you can on my back.' Monkey lay prone on Dog’s back. After that, Dog went home in the dim light of evening. When Dog reached home he rushed quickly and dug in a woman’s small pot to lick it. The woman took hold of a pestle and hit Monkey with it very hard on the back of Dog. Monkey stretched himself from the pain of the blow and began to squeak, for Monkey was in unbearable pain. Dog said to Monkey, 'My friend, do not cry. They did not beat you but the pestle just fell on you by accident. They have not yet started to prepare a meal. You will surely eat oil today. When they begin cooking I shall eat oil, that for which you hear my voice at home.'

After that Dog went again in haste and put his muzzle into the woman’s oil in a pot and gobbled it. The woman again took up a pestle and hit Monkey with it on his neck. Monkey leapt up and jumped down from the back of dog and took to his heels, running to the bush, because it hurt him much more than the first blow. Monkey ran into a big thorny bush and stayed there in a tangle of creepers.

In the morning Dog went again to walk in the groundnuts cultivation. Monkey said to Dog, 'als! What a bad person you are. You took me to your home yesterday and people beat me till I nearly died from the blows. I did not know that you are such a trickster. You just steal the women’s things and they beat you for it, and you say you eat oil!' Thus there began to be hatred between monkeys and dogs, and monkeys for their part are in tangles of creepers and dogs are in homesteads here.

**HOW MOCKING-BIRD TOOK THE BLESSING OF BULBUL (18)**

Once upon a long long time ago the father of Mocking-bird was different from the father of Bulbul. Then when the father of Bulbul was so old that he was about to die he wanted Bulbul to be near him. The father of Bulbul became sick, very seriously ill indeed. When he was at the point of death he sent a messenger to Bulbul, that he should come. While Bulbul was coming, Mockingbird aroused so that he and Bulbul could go together to visit the sick father of Bulbul. When they arrived before the father of Bulbul he urged Bulbul strongly, saying, 'Very early in the morning Bulbul come quickly.' For his part Mocking-bird just sat silent, listening to what the father of Bulbul was saying.

Bulbul and Mocking-bird departed. Right early, before sunrise, Mocking-bird awoke and came first to the father of Bulbul. Bulbul thought to himself that he would wait for the sun to rise before visiting his father. When Mocking-bird arrived first, the father saw just him and did not see Bulbul. He waited for Bulbul for a long time but he did not come soon. So the father of Bulbul gave his blessing and his good advice to Mocking-bird instead of, as he intended, to Bulbul. Immediately after that the father of Bulbul became worse and was about to die, and he could only weakly stammer, saying, ‘kpara, kpara, kpara.’4 When Bulbul arrived he saw his father just lisping and he thought that his father was giving him his blessing. That is why the bulbul sings badly, because it took its lisping from the father, while the mocking-bird took the
good blessing which was intended for the bulbil by its father. That is why the mocking-bird sings so well.

WHY FIRELY SEPARATED FROM STAR (19)

Firefly used to be together on earth with Star. Then Firefly happened to kill his animal, a cat, and he did not give some of the meat to Star. That is why Star said he would not be neighbour to Firefly, and he went to live in the sky. That is why it is a popular saying among people that Firefly separated neighbourhood from Star on account of the guts of a cat.

SOLOMON FISH, LEADER OF FISH (20)
The Solomon fish is the leader of all fish. But it is not big, it is very tiny. Its habit is to lead all fishes, and it leads the way clearly for the fish over dry land. When the water (of the streams) dries up for the fish the Solomon fish leads the fish over dry land. When it leads the way for the fish over dry ground it spits on the ground ahead of them on the way it leads the big fish, and the spittle makes the way slimy, and the big fish follow after it, and it goes ahead of them all. Its name is saraba.

HOW GREY DUiker GOT THE BETTER OF LEOPARD (21)

Leopard went after Duiker to catch him and eat him. Leopard went and found Duiker in a riverside wood. Duiker said to Leopard, 'You cannot catch me because I am very powerful.' When Leopard approached Duiker nearer and nearer Duiker ran under a fallen tree and lay down leaving a hollow space under it. When Duiker ran under the tree he stood with his legs astride staring at Leopard. He said to Leopard, 'Approach further here since you can catch me. Just look at this huge tree which I have raised by my strength.' When Leopard tried to get to Duiker, Duiker said, 'If you come any nearer I will let this tree fall on you.' Leopard went round and round the tree till daylight and then he left Duiker and went away.

WHY WARTHOG LIVES IN A LAIR AND GREY DUiker LIVES IN THE OPEN (22)

Now, in the past the bush was everywhere burnt, even the riverside woods, and everywhere the land was clear, as though it had been swept. Duiker and Warthog were both taking walks and they met in the burnt bush. Heavy rain was coming from the east. When it began to fall Duiker ran and stood under a termite mound and a tree, taking shelter from the rain. Warthog was also running and when he reached where Duiker had taken shelter and looked around he saw a burrow in the mound near Duiker. He entered the burrow. As it was still raining heavily Duiker was getting wet, so Duiker entered after Warthog inside the mound. It rained heavily and then it began to stop little by little. Warthog said to Duiker, 'Go and have a look outside.' Duiker went and looked and said to Warthog, 'There is something in the east, lumpy like the head of a big animal in a mound.' Warthog understood the innuendo Duiker had spoken. Duiker entered after Warthog into the burrow. While it was still raining Warthog came out to have a look for himself. Duiker asked Warthog from the inside, 'Has it stopped?' Warthog answered in double-talk, 'It is drizzling.' After that Warthog stayed outside and then rushed into the burrow after Duiker and tried to catch Duiker, but Duiker dashed out with all speed and ran away from Warthog in the burrow. That is why warthogs live in lairs in mounds while duikers do not live in lairs in mounds but live outside.

WHY RED DUiker LEFT GREY DUiker (23)

Red Duiker went to pillage chickens. When he was on his way to pillage chickens he met Grey Duiker. Red Duiker at once caught Grey Duiker by his belt and said, 'You there! Go and carry the chickens.' Grey Duiker said, 'What! What are you saying Red Duiker? Don't you bear in mind that I am the senior of all the animals? I shall never speak to you again. You just see me as small as I am in body and you say that you are my equal. Is it because I am small that you do not respect me? Don't you know that the female grasshopper is bigger in body than the male?' Red Duiker replied, 'Let us fight together first and the one who knocks the other down will be the senior.' They began to fight, Red Duiker and Grey Duiker. As they went on fighting Grey Duiker knocked Red Duiker down, falling on him. Then he said to Red Duiker, 'You see, I am telling you and you contradicted.' That is why Red Duiker left Grey Duiker and went away. Grey Duiker said, 'The animals are trying to treat me so just because I am tiny.' That is what happened between Grey Duiker and Red Duiker.

CROCODILE AND FRANCOLIN (24)

Crocodile seized a man, and when he had seized him he took him to his den, for he had prepared the den and had made indeed a half-outlet. After that Crocodile left the man there and went after Big Crab to summon him. While he was after Crab Francolin came along the river bank and began to scratch the ground for termites to eat them. He scratched for termites until he reached Crocodile's hole. As he scratched for termites above Crocodile's hole he made an opening into it. The man saw Crocodile's hole opening as the light came in. He crawled out by the opening scratched by Francolin and escaped. After that, Crocodile arrived with Crab. Crab said to Crocodile, 'My friend, where is meat for me to eat?' For Crab was a friend of Crocodile. Whenever Crocodile caught a person he would go and call Crab. They entered the den and looked for the man in vain; he was not there. Crab lost his temper with Crocodile and they fought. Crab bit open Crocodile's belly and he died. The man who was saved by Francolin said that their clan were francolins and they must never again eat a francolin.

A FISH WHICH FORETELLS THE SEASONS (25)

There is a certain fish among the fish in the water which counts the seasons for the fish so that they know when the dry season is approaching. It does this by breaking a stick and sticking it upright in the middle of the river. What it then does is to keep on observing it until he sees the stick appear above the water, when it knows that the dry season has come. Then it tells the big fish like giant mudfish and mud-fish to go to their hiding place under water and shut the door after them so that nobody sees them. Every year this fish does not forget to do this. It always keeps count of the seasons.

THE EGGS OF EAGLE AND THE AXE-People (26)

There was a bird, Eagle, which laid her eggs in the hollow of a tree. The eggs were very many, and when she went in search of food she appointed two creatures to watch her eggs so that people would not come to collect them. Those creatures were Chameleon and Gymnastic Lizard. They kept watch over the eggs for a long time until one day they saw men coming with a close-wove basket and an axe. They arrived under the tree in which Eagle's eggs and they began to chop it down so that it would fall with the eggs and they would collect them to take them home. The watchers of the eggs, Chameleon and Lizard, saw them and Lizard said to Chameleon, 'Climb up above and call Eagle.' These men, as they chopped into the tree, sang, 'Dig axe-excavator, this tree will fall today.' They continued in this way. Chameleon climbed up and called for Eagle, but she did not hear. Chameleon just said, 'Haha,' but his voice did not carry far. Lizard said to Chameleon, 'Come down and let me go to call her.' Lizard climbed up and said, 'Eagle come here in haste, People are cutting down your eggs, come here with all speed.' Eagle heard it and came and when she was near the men scattered. Eagle went away again leaving the creatures to keep watch. The men came again while Eagle was absent and began to cut down the same tree, saying, 'Dig axe-excavator, this tree will fall today.' Chameleon called Eagle in vain, for she did not hear, but when Lizard called she heard him. Eagle came and when she was near the men were chopping into the tree and it was about to fall. When they saw Eagle coming they scattered. Eagle could not find them. Eagle went away again after her food and left Chameleon and
Lizard beside her eggs, and while she was absent the axe-men came again to the tree. They chopped it only for a short time and it fell with the eggs in it. The axe-men collected the eggs and went off with them. Chameleon and Lizard called Eagle in vain, for she did not come. When she returned, the men had already gone away. She looked in the nest for the eggs in vain, and she was so angry that she flew away (without settling).

THE FRIENDSHIP OF OWL AND FRANCOLIN (27)

In the past Owl used to go to dances during the night because he was afraid of the birds lest they should mock at him during the daytime, for he was very ugly, having protruding eyes. Owl was worried about this, because all the birds hated him for his ugliness. So he made friends with Francolin. Owl said to Francolin, 'We shall be going with you to dances at night, and when it is near daybreak you must sing as you always do so that I shall know and begin to go home right early in the morning.' So Francolin and Owl would go off together to a dance to dance it. When Owl used to attend dances at night women embraced him much. The women spoke well of Owl among themselves, saying what a fine youth Owl was, what a handsome young man. Owl used to embrace the women also. When it got near morning Francolin would sing, saying, 'Francolin it is morning, Francolin it is morning.' When Owl heard the singing of Francolin he would leave for home before daylight. When morning came women would say, 'Where is the very handsome young man who was here at night?' This friendship between Owl and Francolin lasted a long time. One day they went to a certain dance to dance it. When they arrived there they began to dance a lot. Women bumped into Owl very much, as they had been used to do before. Things stood thus when Owl began to speak spitfully about Francolin to the women, saying to them, 'Look at that ugly fellow Francolin. His legs are red, to have such red legs is not good.' Francolin remained silent and heard it, what Owl spoke in whispers to the women out of spite for Francolin. Francolin just said to himself, 'All right, you are talking spitfully against me. I will make a mockery of you today.'

It was getting towards morning and the people were dancing with vigour and women kept bumping into Owl. Though day was about to break Francolin did not sing at all, as he was wont to do. He remained completely silent. Owl could see clearly his body and he became so worried that he went to Francolin and asked, 'My friend, is it not about to be morning?' Francolin just deceased Owl, saying, 'It is not anywhere near morning.' Owl went to continue with the dance, and after dancing for only a very short time it became very clear daylight. Owl hastened again to Francolin and asked him about the time, saying, 'My friend, is it not already daylight?' Francolin went on deceiving Owl, telling him, 'It is moonlight which you see, morning is still far off.' But no sooner had Francolin finished speaking than the sun came forth. When the birds took a look at Owl and saw him with it they were stricken speechless to see his ugliness. Owl's eyes bulged with terrible ugliness. Suddenly the small bird Sirikoro flew and pecked Owl with his beak and Owl's feathers were blown away in the wind by Sirikoro. All the birds left the dance and fell upon Owl from all directions and pecked him for his ugliness. When Francolin became aware that the birds were going to mock Owl in that way he escaped, and Owl did not know where he had gone to. All the birds, the snowy ones, beat Owl yet harder than before, and they were flying up mocking at Owl. Owl fled away from them with all speed with his wings outspread into a wood. The birds followed him in flocks. He disappeared into the wood and hid among the creepers to save himself from the birds. This is why it is a common saying among people, 'Don't become a friend to me with the friendship of Owl.'

This is because Owl made friends with Francolin and yet spoke spitfully against Francolin to the women, saying that only he was a handsome man. Owl and Francolin broke off their friendship because of Owl's malice. That is why Francolin left Owl unwarned about the morning. So the birds saw his ugly eyes and beat him for his ugliness. The owl does not appear openly like the other birds nowadays. It always conceals itself in a tangle of overgrowth from the birds, for once they see it they begin to mock at it. So it is a popular saying, 'The friend of Owl delayed him till morning.'

WHY LEOPARD CURSED DRAGONFLY (28)

Leopard and Dragonfly were friends, hunting and killing many animals together. Leopard begat ten children. Leopard and Dragonfly went one day to hunt again. There was a man related to Dragonfly who took the children of Leopard to the river. He collected the shells of nettlebeans and washed the eyes of Leopard's children with them. While he was doing this he sang, saying,

'This world, I have spoilt it,
this world, I have spoilt it.'

The eyes of Leopard's children were spoilt. Their bodies became spotted and their teeth projected out. Leopard and Dragonfly returned from hunting, and when Leopard saw his children they had all been spoilt. So Leopard broke up living with Dragonfly and he cursed Dragonfly, saying, 'Dragonfly, you will be small (in stature), you will have no granary of your own, you will not settle properly but you will always be flying around.' Then Leopard killed Dragonfly with all his relatives. Dragonfly cursed Leopard, saying, 'Leopard, you will go to live in streamside woods. Everybody in the world will hate you and if a person sees you he will kill you.' This is the reason why Leopard stopped living with Dragonfly.

ABOUT HOW A CHILD'S HAND GOT STUCK IN A HOLE AND MOLECULED SQUIRREL (29)

There was a woman and her younger sister. One day she was cooking porridge to go with meat to take it to her husband's home. She sent her sister into the hut to go and bring her the broth-stirring stick so that she might stir the broth with it. But the sister refused, saying that she did not know where it was. She said, 'Where is it? I am not going to bring it because I am the younger. The elder should go and fetch it.' The woman said to her younger sister, 'Why, since it is I who want you? She went into the hut and saw the stick on the shelf there. When she put forth her hand to take it there it got stuck there and when people came and tried to pull they could not pull her away. They had to raise her up (and place her on the shelf), and they repaired the hut above her. People continued living near her for a long time until their huts fell into disrepair and they moved to live elsewhere, leaving the girl by herself, her mother bringing her food daily. When the girl's mother came and approached her daughter she stood and sang her song, saying,

'Deringo my child, Deringo my child,
if Ture calls you my child,
do not answer my child,
when it is I who call you my child
answer me my child.'

The child answered her mother, saying,

'Oh mother, o mother, o mother,
I am staying in a deserted homestead,
seeing Azande with my eyes only.'

Her mother came daily with food for her. There was an animal which wanted to eat the girl, and when her mother brought her food it came and hid quite still to hear the song she sang to her daughter. When she departed the animal came and sang exactly like the girl's mother so that the girl thought it was her mother singing. The animal appeared, caught her and ate her.

Next morning the mother came bringing food to her daughter and she looked in vain for her daughter and saw only blood where she had been. She went and told them at home and they wailed for her. When the mourning was finished they asked, 'What can be done to kill the animal which killed this child?' They prepared a big feast to which they invited all people and animals, telling them to come and dance, so that they might discover which animal had killed the child. Many people came together with the animals. The
relatives of the girl said that the animals should beat the drum first. The animals went to the drum and the first animal, Squirrel, came and took the drum and beat it and said in its beats,

‘Kpaka,’ when I see mine in a deserted homestead
I snatch it kpaka.

The relatives of the girl said, ‘That’s it. He is the one who killed her.’ They knew it from the sound of the drum which he beat, saying,

‘Kpaka, when I see mine in a deserted homestead
I snatch it kpaka.’

They gathered themselves and came and seized that animal and killed him.

HOW CHIMPANZEE SEWED HIS THIGH WITH A NEEDLE (30)

Very early in the morning the chimpanzees make a noise only when they are in large numbers, but if one is alone in a tree it will not make a noise in the early morning but remains still until daylight and then it climbs down.

So one morning a man went very early on to a rock to mend his barkcloth of the bagadi kind. He took his prepared thread and his strong needle and went on to the rock. When he arrived on the rock he sat on his wooden stool and held his needle, inserting the thread through its eye, and began to sew his barkcloth with it. There was a single chimpanzee in a tree, which he did not see, neither did he know about it. He sat sewing his barkcloth with the needle and putting the thread through it, raising his hand and as he was doing so it spotted the chimpanzee. When he had completed sewing and it was finished he went to his home, forgetting the needle with a long thread attached to it on the ground on the rock. As he was on his way home the chimpanzee climbed down and came to the place of the man. It saw the needle lying with the thread attached to it. It thought that the man had been sewing his thigh, so it took the needle and pierced it right through its thigh and it wrapped the thread round its lower shank. The chimpanzee sewed its thigh to its lower leg. But the man remembered his needle and came back to look for it. When he arrived at the place and looked around he saw the chimpanzee in the place where he had been sitting. When the chimpanzee noticed the man it tried to run away from him but could by no means do so. The man speared the chimpanzee and it cried out, and he went on spearing it till he killed it, and then he carried the chimpanzee’s corpse on his shoulder to his home.

The habit of chimpanzees is thus: if somebody sees them playing by throwing a ground-fruit stalk at each other, as though fighting, and he waits for them to pass on ahead and then places his spear in the place where they were playing, when the chimpanzees come back to play again there and the man just hides quietly, leaving the spear there, on seeing the spear a chimpanzee will think it is the stalk of their play. It will take it and spear another with it to death. Then the owner of the spear can just wait for them to go away first and then come to get the dead chimpanzee and take it home.

ABOUT A WOMAN WHO CHANGED INTO A HOUSE RAT (31)

There was once a man who set up his snare. A guineafoal which was going about got caught in his snare. He came and untied the guineafoal (from the noose) and took it home and gave it to his wife to cook for him. When she had cooked it he ate it and was repelled. Being repelled, he tied up the mouth of the pot (with leaves and cord) in which there was what was left over. When it was still and they went to sleep his wife turned into a rat so as to go and eat his guineafoal. He took his arrow and he built up the fire and it sparkled, and he went again to sleep. After this she began to eat his guineafoal again in the form of a rat. He took his arrow and shot her in the belly in the form of a rat and she woke from sleep with the arrow in her belly. He said to her, ‘So it was you who were eating my guineafoal.’

ABOUT A MAN WHO TURNED INTO A WATERBUCK (32)

There was a man whose clan were the Abakpaku. There was another man who had a cultivation of elusine, and the elusine was a fine crop in his cultivation. When it was dark the Bakpaku man would turn into a waterbuck and go and eat the elusine of his friend. The owner of the elusine went to keep watch for the animals which ate his elusine. He went and arrived there and saw this waterbuck lying down with its legs upwards. The man hit one leg, thinking it was the stump of a tree, whereas it was a leg of the waterbuck. So this man went home. The waterbuck troubled him for a long time. Whenever he went to his elusine cultivation in the morning he would find the spoor of the waterbuck where it had eaten the elusine. One day he went and saw the waterbuck. As he moved, the waterbuck heard him and raised itself up. He walked until he reached it and raising his arm with his spear he speared it. The waterbuck ran away with all speed. The man left it and went home.

In the morning he went to follow the spoor and he followed it to the homestead of that man. Then the spoor turned into the footprints of a person. He appeared in the homestead and asked the man’s wife, ‘Where is your husband?’ She replied, ‘My husband has gone away. He is not here, and I have not seen him.’ The man continued to stand there, and then he heard a movement in a hut, and there was a groan from the spear wound. The man asked, ‘And who is groaning inside the hut?’ She replied, ‘It is my husband groaning from an illness.’ The man said, ‘I am going to see him.’ He entered the hut, and when he looked at the Bakpaku man’s belly he saw the wound made by his spear. He said, ‘So it is you who have been eating my elusine in the form of a waterbuck!’ When he had left for home the one who was sick from a wound died.

THUNDER AND CRICETUS (33)

There was a man who begat a beautiful daughter, and the daughter wanted to get away from her father. She went and consulted the atari oracles and they told her to go and sit near a ngbehe (thorny) tree so that if it thundered she could ascend by its cord. When heavy rain was coming she went and sat near an ngbehe tree. When it thundered she took hold of the cord of Thunder and ascended by it. She reached the home of Thunder but did not find him there; only his wife was there, who gave her a stool to sit on. Thunder came home and the mistress of the home said to him, ‘Look at that beautiful woman I have married for you.’ Thunder thundered terribly and she was split to pieces, after which she put herself together again and sat again on the same stool. Thunder did this to her three times, and she was still whole. So he said, ‘Truly this is my wife.’ So he built her a hut near his wife’s hut, and she bore a child by Thunder.

Her father consulted the oracles about her, asking what he could do to see his child who had gone up with Thunder. The oracles told him to bring the little animal Cricetus and make him dig many holes, long ones and short ones, all over the place; then he would certainly find his daughter. When all this had been done, the father of the girl told Cricetus to sit by the ngbehe tree so that when Thunder thundered Cricetus could take hold of his cord and ascend by the cord of Thunder. Cricetus sat (by the tree). When Thunder thundered he went up with Thunder’s cord and appeared in the sky, appeared in the very hut of the woman through a burrow he made into her hut. Cricetus told her the message from her father and when it was ended she closed the burrow through which Cricetus had come and hid it from Thunder. She cooked porridge with oil and gave it to Cricetus. She cooked fish for Thunder. Now, she concealed Cricetus from Thunder, and Cricetus sent mosquitoes to bite Thunder until he left the hut on account of the mosquitoes, for they bit him greatly, and went to his other wife. Cricetus sent the mosquitoes so that Thunder might leave the hut and he could escape with the girl. When he left she took her first child, called Banzibi, and handed him down to Cricetus in the burrow. Thunder heard the movement of this child and asked her, ‘What is moving like that?’ She replied, ‘The mosquitoes which you saw when they were biting us and you went to where you could have peace, they are the ones we are beating over us.’ She handed another of her children down again and the
movement was heard with the sound of its metal anklet. When Thunder again asked her lost her temper with Thunder, saying, ‘If you ask again I will not answer you. Why do you ask us while we are being bitten by mosquitoes?’ She handed down her children first and then she got ready to get down herself after them to escape home. She got down and, hearing her movement, Thunder put a question in vain, and the door remained shut for a long time. He went to look and there was no one there. He was surprised and said ‘Oh! My lovely children!’ Then he saw the burrow made by Cricetus and he said, ‘Oh, but see what the woman has done to me!’ He thundered terribly and followed the burrow to its end. The small animal which burrowed the hole sent other insects, scorpions and pinching soldier ants, into the opening of the burrow which it dug, and afterwards also snakes and caterpillars to deal with Thunder to make him turn back from pursuing them. Thunder thundered and sent fire which killed all those creatures, and began following each hole to the end and then returned to his place.

Thunder followed those burrows, but the one by which Cricetus went with Thunder’s wife and her children was a very long one and curved greatly. Cricetus stopped again with those creatures and sent them after Thunder, telling them to worry him till he died. Thunder thundered with fire and killed all the creatures, and he followed the hole to the end and then came back to his place. Thunder followed the holes one after another. Cricetus sent yet other poisonous insects. Thunder thundered at the very hole which the woman followed with her children and he followed it to just the point of getting to the end of it. Cricetus and the woman reached her father’s home. The woman’s father thanked Cricetus much, saying, ‘So! So! I did not think I would see my child again. I am seeing her because of you, Cricetus.’ While the woman’s father was talking thus, Thunder appeared and said, ‘Where is my wife? It is here I have come for her.’ The father of the woman said to Thunder, ‘You did not marry her. Where is your bridewealth you gave for her?’ When he heard this Thunder went away, departed at once for his home.

FAVOUR FOLLOWS FAVOUR (III, I)

There was a woman who had a daughter. She adorned her body with many ornaments and put a mass of metal beads round her neck. This daughter went to play with her comrades by the river. When her comrades went home she stood by a pool and admired her shadow with the ornaments in this water. The mass of metal beads became loose and fell into the pool, which was so deep that a person could not descend into it. She struck her breast, saying, ‘What shall I do about my mother on account of her metal beads?’ She went home and told her mother, ‘Your metal beads have fallen into a pool.’ Her mother replied, ‘You will not spend the night in my home here today until you fetch my beads out of the pool.’ She ran away and spent the night in the bush, and in the morning she set out for the pool again. She went to the home of an old woman and found her sitting by the entrance to her (temporary) shelter, sweeping her resting place. The old woman said to her, ‘Child, where are you going to?’ The girl replied, ‘My mother’s metal beads fell from me yesterday into the water on the other side of the river and my mother said I should keep on going there.’ The old woman said, ‘Child, help me sweep my courtyard.’ The girl looked at the sun and was silent. She took the broom from her and swept the place properly, carried water for her, and prepared her bed. The old woman said, ‘Yes, my child your affair is all right. Go to the water and the one that will say, “River of bumps, river of bumps,” do not fall into it, just pass it by. But the one that will say, “River of metal beads, river of metal beads,” fall into it, you will find your mother’s metal beads and all the ornaments.’

The girl went and reached the river of bumps. When it said ‘River of bumps, river of bumps,’ she passed by; and she reached the river of metal beads, and it said, ‘River of metal beads, river of metal beads.’ She fell into it, and as she came out of the water the metal beads and beautiful ornaments covered her neck. She brought them to her mother, and they were rich in beautiful ornaments.

After this, there was another girl whose mother’s hide (for carrying babies) fell into the same pool, and her mother chased her away in the same manner. She followed the same path and reached the home of the old woman. The old woman said, ‘Child, help me my daughter.’ The girl looked at the sun and replied, ‘You see that the sun is just about to set and you say I should help you? I am going for my mother’s hide.’ The old woman told her to stop for a moment and she said to her, ‘Go, and if you see a river which says, “River of metal beads, river of metal beads” do not fall into it, but when you see a river which says, “River of bumps, river of bumps” fall into it and you will take back your mother’s hide.’ She went and saw the river of metal beads, and when it said, ‘River of metal beads, river of metal beads,’ she passed on until she reached the river of bumps and when it said, ‘River of bumps, river of bumps,’ she fell into it, and when she came out her body was covered with bumps.

FAVOUR FOLLOWS FAVOUR.
THE OBSTINATE ONE WAS CUT INSIDE AN ELEPHANT

There was a boy with his grandmother. When she wanted to go away she would collect (and leave with him) enough food for the days she would spend away so that he would not wander in her absence. The boy would simply wait till she had gone, when he would take his bag and go to the river for certain (black rounded) fruits. These fruits were above a large piece of water. He climbed up and ate the fruits, throwing their seeds into the bag, because if a seed happened to fall from a person into this big piece of water it would rise and trap him. He ate them till a seed fell from his hand into the water. This big piece of water was hot as though it was heated by fire. It suddenly rose and trapped the boy above. He began to wail, singing this song:

'Death, death stand afar,
when I wanted to pluck the fruit,
the fruit fell from my hand into the river.

The grandmother of the boy heard this lament and she came running with all speed. When she arrived she found the water rising and about to reach the boy. So she rubbed (magical) ash on the trunk of this tree up which the boy had climbed and the water receded. The boy climbed down and took him home, admonishing him, 'Don't do it again, otherwise you will die.' He replied that he would not do it again. She thought that he was telling the truth.

She collected and left him all kinds of food and put them beside him for him to eat, and she went again for her salt. When she was far off he took his bag and went for his fruit to the river. He climbed up and began to pluck the fruits into his bag and eating some of them. Suddenly one just fell from his hand into the water again and it began to rise. The boy began again to cry. His grandmother heard his lament far off and she came running and saw the water getting very close to him. She rubbed her (magical) ash on the tree and the water receded to its normal level. The boy climbed down and she came to beside him and said, 'Oh my child! Oh! What is the matter with you? You are going just to die from the water!' She admonished him and took him home.

She wanted again to go for her salt. She collected even more food than before, sweet food, and, leaving him with it, she went again far away for her salt. When she had been away a long time the boy took his bag again and went after the same fruits to the river. He climbed up and plucked the fruits into his bag, eating some of them. The grandmother had gone this time really far. This third time was going to be his death finally. He ate the fruits till one fell from his hand into the river, which began to rise. It rose very much and was getting near the boy above. The boy began to lament but he lamented in vain for his grandmother did not hear his lament because she had gone very far away. The hot water approached and reached the boy and he got badly scorched by it till he died.

Misfortune follows badness. The obstinate one was mourned by his father's drum-heirloom. The obstinate one was cut in the belly of an elephant.'

A MALE SNAKE IS KNOWN WHEN IT IS BURNT

There was a man called Gumba who begat two very beautiful daughters and many people came to ask to marry them. He made it a condition to each of them that the person who would pluck the beard of a certain man called Zipi and bring it to him would be given a daughter in marriage. A certain man came to ask for the hands of the daughters, and their father said, 'First go and pluck the beard of the man called Zipi and bring it to me, then I will give them to you.' He accepted and said that it was he who would marry her.

He went to the home of Zipi and found only his wife at home without finding Zipi there. This man Zipi had a beard as long as from here to the river Mungu and as from here to Mopol. He asked Zipi's wife, 'What can a person do to pluck the beard of Zipi?'

He begged her, 'Oh! Show me what a person can do to pluck his beard. It is because my wife is there whose father told me that unless I pluck the beard of Zipi I shall not marry her. She is really very beautiful.' She told him, 'Go and measure yourself with his bed inside the hut and lie under it. When he comes to sleep saying "Oorp Zipi," if he says it loud, just lie still, and if he says it quietly and slowly, saying "Oorp Zipi," it means that he is asleep. Then you can cut off his beard. She wrapped his beard and broke off some for him and she scraped a reddish on to it. She took the food to the man in the hut of Zipi, and he ate and was replete.

Zipi came back from his walk and as he sat on his bed he asked, 'What is smelling like that? There must be a person here.' As the woman was concealing the man from him she said, 'Are you joking? Where is a person? It is only this pipe smelling like a person.' She brought food before Zipi. He ate and was replete and he went to sleep. In sleep he said the first 'Oorp Zipi' very loudly. The man lay still as the woman had told him to do. When Zipi was deeply asleep he said quietly 'Oorp Zipi.' The man then knew that he was really asleep. He darted out from under the bed and cut off Zipi's beard and ran off with it with all speed.

Zipi awoke from sleep and crossed his teeth in front of the man to bar his way that he might catch him, for Zipi's teeth were very long in truth. The man came and dashed out between them. He crossed them again. The man came and blew his (magical) whistle and dashed out between them. Nobody else but this man had ever been to the home of Zipi. The man rolled up Zipi's beard on to a stick like just a roll of entrails. He gave it to the father of the women who, on seeing it, was so pleased that he offered his daughter to the man, as he had said he would do. He said to this man, 'Go and collect your twine and take her to your home.' He went and collected her and she went with this man to his home.

A courageous person earns the reward of his courage. A male snake is known when it is burnt.

THE THING WHICH HAS A SMALL BEGINNING WILL INCREASE GREATLY IN THE FUTURE

A man went to visit his sister at her home. He told her, 'I have come for you to give me some sesame seed.' The sister said, 'My brother, I have no sesame seed.' After that she went to roast sesame. Her daughter gathered up some of it, some raw, and was eating them as she came towards her maternal uncle outside. As the man looked at the mouth of his sister's daughter he saw sesame on her lips. He put forth his hand and took these sesame seeds from her lips. After that he said to his sister, 'I am going home.' The sister said, 'My brother, what I told you is true. I have no sesame seeds.'

He left for home, and when he reached home he threw these seeds on to the ground. They germinated and flourished and grew high, grew high and fruited. When the crop had ripened he reap it and kept the seed in a small basket. At the next season for planting sesame he sowed it and it yielded a good crop and he reaped it and filled two baskets with it. The sesame accumulated for the man. After this the sister lacked it. When he met his sister again he said to her, 'My sister, the sesame which you refused to give to me, here is some which I took on the lips of your daughter.' His sister begged him for some sesame seeds.

There is nothing that can be concealed. It must appear. There is nothing that can be done in secret, it will come to light. The same container which you use for distribution, they will distribute to you with the same. What a man sows, the same will he reap. All big things start in a small way.

THEY FOUND TERMITES INTO THE POTS OF OTHERS

There were two men, by name Small Pot and Small Cup-Gourd. They were great friends. They used to cook in Small Pot and drink water out of Small Gourd. They ate together, drank water together, and slept together.

They happened to go for a walk and they came to a river and when they reached it Small Pot said, 'My friend, I shall not cross. If I try to do so water will go inside me and I shall sink and drown.' Small Gourd said, 'Come.' Small Pot came and Small Gourd took
him across and he survived the water. They continued their walk until they came across fire on dry land between streams. Small Gourd said, 'Oh my younger brother, I am going to die because fire is coming.' Small Pot said, 'You will not die.' When the fire came near to them Small Pot said to Small Gourd, 'Lie on the ground upside-down.' Small Gourd lay on the ground upside-down and Small Pot covered him by lying upside-down on top of him. The fire came and passed without burning Small Gourd because he was covered by Small Pot. When it passed they came together again and went on their way together, for they were great friends.

They feed the courtyard ridge (with rubbish) and it in return feeds its owner. Termites are pounded into the pot where others were.

**BEE WAS HEIR TO BIG FLY (11, 6)**

Big Fly begat his children. He lived till one day he became sick. He sent after his firstborn son, saying that he was seriously ill. When his son heard it he said that they should tell his father that he was dancing. When his sister's son Bee heard it he went there and said, 'My father, I have come.' Big Fly said, 'My child, I am going to die and my inheritance is here.' As he was going to die let him take over the inheritance and scatter it all over the world.

Then Fly came and told his father that he had returned. He came when his father was dying. The father said to his son, 'Since I was dying in your absence, here is the wound; be sitting on the wounds of people. You shall eat decomposed things and sit on grass.' That is why people say, 'Bee was heir to Big Fly.'

**A RIDDLE ABOUT THE FACE (11, 7)**

There are four men in the same homestead, having the same courtyard, but they never visit each other. Who are these people? They are the two eyes and the two ears. They never visit each other. Although they are handsome they never go round to see each other.

**A PROVERB ABOUT AN INSECT CALLED SPIDER (11, 8)**

Although the river is in flood a spider can put on its white barkcloth to cross without getting soaked. That is why when boys collect together one will ask the others to answer this, 'There is a man who, although the river is in flood, and he puts on his barkcloth, will never get soaked?'

However difficult a thing may be, a capable person may try it and succeed.

**A RIDDLE ABOUT THE AMBOMU AND RED GROUND FRUIT (11, 9)**

The Ambomu said in double-talk, 'the land rainbow.' This means that the red ground fruit resembles the rainbow, for the fruit is very red (like the rainbow). A knowledgeable person will understand at once and say, 'It is the red ground fruit.'

**A RIDDLE ABOUT THE CREATURE CALLED TORTOISE (11, 10)**

It is a habit of the tortoise that when it is twelve o'clock noon it cannot see the sun at all because of its shell which covers all its head and renders it unable to look up properly. The tortoise sees the sun only from early morning up to about eight o'clock, and when it ascends higher it will disappear from its sight. It will only see sunlight (not the sun itself) until three o'clock. It sees the sun from about three o'clock till four o'clock and then till evening the tortoise sees the sun again. That is why people say in a riddle, 'There is a man who cannot see the sun at midday. He sees the sun only in the morning and in the evening.' A knowledgeable person will say, 'It is the tortoise.'

**Notes**

1. An area of high grass is selected and a path trodden round it so that it can be seen by their spoor if animals have entered the area and have not left it. Nets are then erected at one side and men and dogs enter from the other side to scare the game into them. Children line the unnetted sides of the area to scare the game by shouting if they try to break out there.
2. A very large snail called duru in Zande.
3. A blue-grey monkey which makes a squeaky sound, called mbatu in Zande.

4. These words would seem to indicate the croaking of the bird.
5. I think that solomon fish is the right identification for the Zande saraha.
6. This would appear to be the sense of ri nigbi here. It could mean 'over the world.'
7. The word used, tsukpukupu, is that used to refer to the warthog's warts.
8. The word mimo means not only 'with a drizzle' but also in metaphor for the movement of the grey duiker in a drizzle—short quick steps.
9. The word is huru, hole or burrow. It appears to mean here another entrance to the den concealed under the surface of the water.
10. I was told that the word sosoro is a word here used meaning 'dawn' in the language of the partly assimilated Pambwa people, but I think that it more probably renders the cry of this sort of francolin.
11. A tiny bird, the first to sing in the morning, at about dawn.
12. I believe this to be the correct identification for the Zande kpare.
13. The title is about how a child's hand got stuck in a hole, but there is nothing about a hole in the story. This is perhaps a confusion with other stories in which a child's hand is caught in a hole.
14. Kpakpa is the sound made by striking the drum on the wood at the edge of the membrane.
15. Members of this clan have the waterbuck for totem: it is believed that their body-soul changes into this animal at death.
16. Giri, cord. Thunder is regarded by Zande as a sort of creature. There is also a belief that it has a cord by which it descends, as indeed, is the sense of the story.
17. Wau, the capital of the Bahr el Ghazal Province of the Sudan. Its use here is to suggest a very great distance.
18. Nzara appears to have no meaning here.
19. Satu appears to be the same as atari, the usual oracle in Zande folktales. It behaves here in the same manner as the atari. Both take the plural pronoun.
20. The Zande word is akatu, bumps or notches. The word is used for the warts of the warthog.
21. A well-known Zande proverb. A boy was told not to enter the carcase of an elephant when men were cutting it up with their spears. He disobeyed and got cut in consequence.
22. A big river in the Congo.
23. A big river in the Sudan.
24. This appears to be the meaning. As written ri ngua could mean literally 'eats the tree' or 'top of the tree.'
25. The Zande is fonzoro gugu, which I have translated literally. I cannot say what is its meaning here.
26. One of the best-known Zande proverbs. When a male snake is burnt its male organ protrudes, or so Azande say. Otherwise one cannot tell male from female.
27. In some places the singular is used—one daughter—and in other places the plural—both daughters.
28. A river and a place (called after a prince) in the Sudan. Both are intended to suggest a great distance.
29. This may be an interpolation from the Bible. It must be borne in mind that these texts were written by a Christian convert and were used for teaching purposes in a mission school. The phrase does not seem to be Zande, but there is no certain means of knowing.
30. One of the most used of Zande proverbs. The nearest English equivalent would be 'one good turn deserves another.'
31. This proverb also has the sense of 'one good turn deserves another.'
32. This is a rough rendering of the sentence.
33. The word in the text is sanza. Masuku is usually used for 'riddle.'
34. The Ambomu are the 'true' or 'original' Azande who have imposed their culture on foreign, now mainly assimilated, peoples. They pride themselves consequently on their greater understanding of metaphorical language.
35. Ordinary Azande would not at this time have reckoned time in figures. This is missionary influence.
36. The word here is mambhi, an introduced word.
Excavations on a Stone Arrangement in Tasmania. By Rhys Jones, Department of Anthropology, University of Sydney. With a figure

During an archeological expedition to Tasmania, in the summer of 1963–1964, we investigated a row of stones on a midden at the Bay of Fires on the north-east coast. Excavations revealed a second arrangement stratified one foot beneath the top one, and it is postulated that both are of aboriginal origin.

Our attention was drawn to the site by Mr. W. E. Tucker of St. Helens, and it can be found on the coast, about two miles south of Anson's River. As one walks south of the outlet of Anson's Bay on the foreshore, after having passed an area of shifting dunes, one comes across a large storm beach of pebbles, about 12 feet above normal high tide. There is a long narrow grass-stabilized midden piled up behind and slightly above this pebble bank, and the ground immediately inland is swampy and has a marshy vegetation cover. Apart from a farm at Anson's Bay, the hinterland is practically unoccupied, and the bush is dominant until one reaches the environs of St. Helens about 15 miles to the south.

**FIG. 1. PLAN OF EXCAVATION AT BAY OF FIRES, 1964**

On the surface of the shelly soil can be seen a row of 138 stones arranged so as to follow the crest of the midden. The stones are flat, about two feet long and one foot broad, and they are set into the ground with their longest side at right angles to the general direction of the alignment, so as to form a feature which resembles a garden path in appearance. The alignment is 270 feet long, discontinuous in places, and follows a $175^\circ$ to $355^\circ$ direction. It is impossible to attach any significance to this particular bearing as the coast also follows a north-south line at this point. Near the northern end of the feature, and on the surface of the pebble bank, can be seen a few piles of stones, about one or two feet high. Although they could be of aboriginal origin, the possibility of their construction by surveyors or hunters cannot be ruled out.

A small-scale excavation was carried out in order to ascertain the relationship of the stones to the underlying midden. The trench (A) revealed a depth of 12 inches of undisturbed midden material, with charcoal, stone flakes and some bone fragments, resting on clean dune sand. The flaked assemblage was made of crystalline quartz, and although the artifacts were plentiful, their use to the typologist is limited. The flat stones of the alignment were about three inches thick and were sunk into the midden with their top surfaces flush with the surface of the ground. Directly beneath the top feature, and separated from it by six inches of shell midden were found other stones. The excavations were extended at right angles to the first trench, so that an area 12 feet by three feet was uncovered. In this cutting an irregular row of 12 rounded sub-angular stones could be seen, aligned approximately in a $145^\circ$ to $325^\circ$ direction (see fig. 1). All of these stones had the bases set a few inches deep into the sand underlying the midden, and two of them had been extensively flaked prior to being placed in situ. The midden itself had accumulated around this structure, for no traces could be seen of any holes dug through the deposit to let the stones into the sand. There was a depth of 12 inches of shell deposited in the time between the building of the two arrangements. About six inches of midden had been laid down after the total disappearance of the bottom feature before the top one was constructed. In the case of the lower feature, it is impossible to say as yet whether it was intended to be a straight line of stones as is the case in the top one, or whether the stones exposed are part of some more complicated arrangement. Further work is required, to strip off the midden, so as to expose the full extent of lower structure, before any meaningful descriptions can be made.

**Discussion**

Stone arrangements are quite common on the mainland of Australia (for bibliographies see Towe, 1939, and Greenway, 1963), but this is the first published account of one in Tasmania. Bonwick (1879) in a discussion of the religion of the Tasmanian aborigines, and in particular writing of Druidical rites on the island, says (p. 192), 'circles have been recognized in the interior of Van Diemen's Land, piles of stones have been noticed, evidently of human design . . .'. This statement has been noted by Thorpe (1924) and McCarthy (1940). Bonwick, however, gives no reference nor any geographical location to support his statement, and Roth (1899, p. 57) dismisses it, saying that 'no aboriginal stone or other circles have yet been discovered.'

We were fortunate in the case of the lower feature to be able to give a stratigraphical demonstration of its aboriginal origin, and it is reasonable to infer that the top one was similarly built. Stones were readily available from the pebble bank, but large flat ones, such as were used in the top feature are quite difficult to find, and its construction would involve more than a casual expenditure of effort. It serves no apparent economic purpose and its situation on an exposed dune in an area undeveloped and practically unoccupied, makes the possibility of European origin extremely unlikely.

In attempting to reconstruct the sequence of events, one can say that first, the bottom feature was built on the surface of a dune, and the stones were probably pushed down a few inches to give them a little stability. A period of time then elapsed, when up to 12 inches of midden were deposited, after which the
top alignment was built. During this time, there had been occupation at the site, and the large number of flakes and cores indicates that normal industrial activities were carried out. Radcliffe-Brown (1926) mentions stone arrangements on the Australian mainland, marking ceremonial sites such as totemic centres, and initiation grounds. He points out that such ceremonial centres are usually geographically fixed. Whatever use the Bay of Fires arrangement may have had, it is interesting to note that the traditions connected with its construction continued for an archaeologically appreciable length of time, a circumstance of interest to archaeologists and anthropologists alike.

Note
The expedition was carried out under the auspices of the Australian Institute of Aboriginal Studies, and the Department of Anthropology at the University of Sydney. A full report of the expedition is in press; see Jones, Rhys (1965).

The other members of the expedition were F. J. Allen, I. C. Glover and R. A. Wild, of the University of Sydney. I should like to thank Mrs. D. F. McCarthy and Mr. David Moore for their help with the literature, and Miss D. K. Billings for reading the manuscript.

References

OBITUARY

Renato Biasutti: 1878–1965

Dr. Renato Biasutti, for many years Professor of Anthropogeography in the University of Florence, Italy, and Honorary Fellow of our Institute since 1947, has died at Florence, a few days before reaching his eighty-seventh birthday. He was born on 22 March, 1878. He held the distinction of membership of the historic Academy of the Lincei.

He wrote many articles and books on the general subject of the varieties of physical characteristics of mankind and was less concerned to develop a classification of races, a necessarily abstract affair, than to describe and discuss distribution of varieties of skin colour and texture, hair and so on in the various regions of the world. One of his valuable studies is: Studi sulla distribuzione dei caratteri e dei tipi antropologici, Florence, 1912 (Memorie geografiche, No. 18), p. 234. Another is I tipi umani, Bologna, 1925.

SHORTER NOTES


Apart from the few notable anthropologists who have produced films of a professional standard (e.g. Rouche, Gardiner, Marshall, Haimendorf, etc.) the majority of anthropological films have been made by people who tend to fit into one of two categories. They are either professional anthropologists whose films are of a mediocre if not actually bad technical quality, or they are professional film-makers whose films are disappointing with regard to approach and content. The basic problem is, therefore, that two equally specialized fields are involved—the research thesis of the anthropologist and its technical interpretation and presentation by the film-maker. The combination of a research worker and a professional film-maker is nothing new, but in anthropological films there are special problems which arise from their working together.

1. Planning the Film. (a) Personnel. If the anthropological film-maker and the anthropological research worker are to achieve the maximum of success by collaborating harmoniously, they must recognize that each has an equal contribution to make. If the film is based on research carried out by a team, the advice and opinions of all the research workers must be taken into consideration if a balanced film is to be produced—even if this involves showing a social situation in a number of different lights. In the shooting, however, it is advisable that liaison should only be with one of the research workers in order to avoid confusion.

(b) Audience. The degree of detail in the treatment, and the level of knowledge of social anthropology and its terminology which can be assumed in the audience will determine the length of the film and the intellectual level at which it is to be pitched.

(c) Function. The essential question is: what message is to be conveyed, to whom, and how? The answer may be that film is not the best medium to use, and it is here that the opinion of the professional film-maker is valuable. On the one hand film can be used to record an event or a process for later analysis; on the other, to present scientific information to an audience. The distinction is obviously one of convenience and the two functions merge into one another.

From the recording point of view, the Encyclopaedia Cinematographica has a vast number of silent ethnographical films which have mostly been saved from old scraps of amateur material. The films are very short and are intended for visual comparison and analysis.

An apparently much neglected side of fieldwork training for undergraduates is the use of recording techniques.

Considerable use could be made of films in teaching anthropology since film lessens the need for verbal description, and with a suitable script allows visual description and verbal analysis to occur simultaneously. Furthermore, it largely cuts out the need for ethnographical and geographical description as a background for lectures.

There would also seem to be a use for films as fieldwork exercises which can present the student with actual fieldwork situations and leave him to make his own analysis under supervision: all too often
graduate students are thrown into research with insufficient or no practical experience in fieldwork techniques.

With a different commentary anthropological films designed for teaching can be adapted for a general television audience. As the world contracts there is an increasing need for international and interracial understanding, and films at this level can do much to break down the barriers of social and geographical distance.

Furthermore, anthropological films could play an invaluable role in industrial training. It is sometimes frightening how little Europeans working overseas know of the indigenous peoples amongst whom they are living and working.

(d) Finance. At present there is no direct kind of government sponsorship available in this country for this kind of project, and apart from the more spectacular travelscapes there has been no television market in Great Britain for this kind of film until the advent of B.B.C.2. Foundations on the whole tend to view films as being supplementary to pure research and, therefore, outside their scope.

Most teaching documentaries are, in fact, sponsored by commercial companies, but, while these concerns can afford to make films in the pure sciences, as soon as human relationships come into the picture there are immediate political implications; for purely economic reasons a company cannot afford to offend the politicians of the countries in which it has vested interests.

The big companies do make social documentaries, but social change is shown as being merely another name for the smooth flow of progress. Technical problems are permissible, sociological problems are not. Furthermore, many politicians whose ideals and images are placed in the future do not welcome the unit which wishes to film aspects of traditional ways of life, or of an evolving society.

3. The treatment. The treatment should be written by the filmmaker, since if a separate director is brought in at a later stage, he will have to go over all the ground already covered by the writer. He can never expect to get so wide and thorough an understanding of the subject as if he had written the treatment himself.

The role of the treatment is really to allow the filmmaker and the research worker to agree on paper (which is considerably cheaper than film) exactly what the pattern and the content of the film should be. The treatment will need to be re-written several times before the problems of theory and presentation are finally thrashed out.

The content has to be reduced to its barest bones. There is no room for red herrings, and the commentary should be a development of the visual material and not a description of it.

3. Shooting. On the whole a detailed treatment is better suited to the anthropological filmmaker than the precision of a shooting script, for the simple reason that while one has decided in the treatment what one wants to say in general, no one can be entirely sure of what will actually happen in any given situation.

In pure science, the variables in any given situation are essentially controllable. In the social sciences behaviour patterns are far more complex and, for this reason, less accurately predictable. It is, therefore, far more difficult for a social scientist and a filmmaker to combine satisfactorily.

The essential question with sociological film is 'How do you look at things?' It is impossible to show the whole of the subject; one has to be selective. Only in very exceptional cases is the cinéma-verité technique, of running off thousands of feet in the hope that something will happen, justifiable—as a last resort. From a purely financial point of view it is impracticable to shoot thousands of feet just to obtain one tiny significant fragment. Cinéma-verité is really another word for improvisation, and few directors obtain what they want in a single take.

At the other extreme are people like Robbe-Grillet, who were attracted to the film medium because it is so false—there is so much room for trickery, since the director can control both space and time.

The answer would seem to be somewhere in between these two views—that is, film is an artificial means for presenting an analysis of reality.

There is no such thing as film reality. As soon as you introduce a camera into a situation you are necessarily being selective; surely therefore it is better to be selective on the basis of a research analysis wherever possible, rather than taking pot luck as things happen.

In a wedding, for example, several things tend to happen in different places at the same time. It is far easier to shoot the main events which involve a lot of people as they happen, and then to fill in by re-staging some of the simultaneous events afterwards. But dramatization would seem to be only valid when the course of events can be fairly surely predicted, and this usually confines it to fairly simple and uncomplicated situations involving a small number of people.

In certain cases a hidden camera has distinct advantages, particularly for recording in close-up, spontaneous reactions and emotions, although in many cases the lack of mobility can prove to be a disadvantage. If the situation which is being filmed is dramatic in itself, and the participants are used to the presence of a camera, the situation will hold their attention if they are fully absorbed in it, and they can then forget about the camera which can move around freely.

An alternative technique is the interview, but this way the witnesses are likely to say only what they want you to know or what they think you want to know. People must be made to forget the camera. For an interview you need a far more competent actor than you do when you put people face to face to discuss a live issue, when the odds are that they will become so involved with themselves that they will forget that they are being filmed. A silent running camera avoids creating a distraction through motor noise, but even so a visible camera is obviously a foreign element in any situation and quite often, to counteract this distraction, the filmmaker will have to resort to a ruse. For example, he may have to use a dummy camera and shoot with a second less obvious camera with a higher powered lens, or he may have to create a diversion just out of the picture.

4. Editing and finishing processes. By this time the filmmaker, the anthropologist and the cameraman have become too closely involved in the subject to see it objectively. Even so it is the filmmaker who should do the rough cut.

The film was shot in a certain way to produce a certain effect. It may not have been entirely successful, and the visual material might be better presented in a slightly different way. This is the value of an editor who sees the film quite objectively as something new. He is its first audience and its first judge. If the final editing is carried out by a skilled editor under the supervision of the filmmaker it can improve the film enormously.

From this point on the technical process is the same which is common to all documentary films and the problems are not peculiar to anthropological films.

Filming is only a supplementary tool of socio-anthropological research, but in applied social anthropology it has a valuable and as yet virtually undeveloped role to play.

The Insignia of the Igala Chief of Eth, Eastern Nigeria. By Dr. Ray Sieber, Associate Professor, Department of Fine Art, Indiana University, Bloomington. With four figures

In September, 1948, I had the occasion to visit the small village of Eth in the south-eastern extremity of Igala country. Although the Igala are in Northern Nigeria, this village and the surrounding area were incorporated in Eastern Nigeria, possibly as an error in mapping. This political separation was in 1958 a matter of great concern to the local chief, Azike, and his people, for they maintain that their origins were from Eth and their affiliation to the Ata firm and unbroken. Indeed, Azike on one occasion accompanied me to Eth to lay his case once again before the Ata.

Azike was very generous in furnishing information relating to this relatively young chieftainship and to the insignia of rule that he had for the most part inherited.

Abiega (died about 1906). Following Orama there was a lapse of about 20 years before Azike (son of Idoku who was one of Oda’s sons) became Chief in 1926. From these data it would be difficult to put the origins of this chieftainship earlier than the latter years of the nineteenth century, or to assign other than very short tenure to the cousins who were the first seven chiefs.  

The relatively short time space of the list is significant when a group of spearheads and one bell, all symbols of rule and all from Idaho, are examined (fig. 1). All were inherited by Azike and therefore may be assumed to date before 1906. Further, they were said to have been presented by the Ata as insignia of chieftainship as were other attributes discussed below.

Three types of objects are discernible: (a) one iron spearhead with a brass (?) hexagonal form cast onto the socket and an oval cast bell, both decorated in the manner of the Lower Niger Bronze Industry as defined by William Fagg; (b) three iron spearheads with coiled brass (?) wrappings on the sockets; and (c) two undecorated iron spearheads of distinctively differing form. The cast and decorated examples were said to be older than the two undecorated specimens. Although it was not firmly stated, there was some indication that the two cast examples were the oldest. It is tempting to view this group as summarizing or echoing the collapse of the royal casting tradition at Idaho. Unless the two cast examples were old when transferred to Eteh from Idaho, it would seem that brass-casting was practised well into the nineteenth century, to be supplanted by substitute methods (coiling, in this case) before it was abandoned.

The other accoutrements of Chief Azike are of some interest. The stool which he inherited (fig. 2) is of a type discussed by Bernard and William Fagg in MAN, 1960, 155: two discs of wood are joined by nesting bark cylinders, the whole comprising a cylindrical bark box. It relates to examples found at Benin and depicted on Benin plaques. This example did not come from Idaho and apparently was not part of the insignia of chieftainship as conferred by the Ata. It was said to be very old and to have been inherited by Azike from all his predecessors. Certainly it dated at least from the period of Orama (i.e. about 1900).

The spearheads and metal bell were kept in a separate shelter behind the chief’s house. The box-stool was kept in his house and traditionally served as a container for the other insignia of chieftainship. In fact, this stool was kept more or less as a relic, for Azike, shortly after he became chief, had had made a larger and more complex stool (fig. 3) which now served to store his insignia.  

The box-stool is called akpa which perhaps relates to the Edo (Benin) name, ekpokin.  

Fig. 4 is a photograph of Azike seated on his box-stool and wearing his insignia of chieftainship, of which the description follows. (1) Oka, bead bracelets. These appeared to be of mixed, mostly trade beads. Examples were seen worn by other ‘traditional’ chiefs (i.e. Ata-appointed chiefs as distinguished in 1958 from British-appointed magistrates, usually called chiefs). The bracelets apparently were worn at all times and were the most visible attributes of chieftainship. As with the necklace and hat they were presented by the Ata of Idaho. (2) Odugbo, necklace. This was said to be of coral, but appeared to be bauxite. (3) Kebe, hat. This appeared to be a felt hat of European (?) manufacture. The significant aspect of the hat consisted in the fringe of purple feathers of the Ukhun bird (the name means chief of birds).

Notes

1 The material for this paper was collected during a field study conducted under the auspices of a Ford Foundation Foreign Area Fellowship grant. To the Foundation and to the generous advice and assistance of the Nigerian Dept. of Antiquities, Mr. Bernard Fagg (then Director) and to Mr. William Fagg my thanks are due.
If, for example, one were to assign a nominal 20-year reign to each of the chiefs, Adenin, the fifth chief, would have had to be nearly a century younger than his brother, Odu, the first chief!

See for example his Nigerian Images, London (Lund Humphries), 1963, pp. 39f. and Plates 57–73. The spearhead and bell under discussion are, incidentally, akin to brass pipes still cast by the neighbouring Idoma.

Alternatively the Ata may have sent lesser objects to a remote chieftainship of little political importance. Further, it would be of interest to discover the significance or source of the differing types of spearheads included in this group.

This example closely resembles a stool from Ikka, now in the British Museum (see MAN, 1960, 155, fig. 3). Azike’s example was made by Agada of Utoba village, Okpo area.

Cf. MAN, 1960, 155.

A Further Shrine for a Yoruba Hunter. By Frank Willett, Research Fellow, Nuffield College, Oxford. With two figures

In MAN, 1959, 334, I published a short account of a hunter’s shrine at Ifecodo (Western Nigeria) to which Mr. William Fagg added information of two more (MAN, 1959, 335). I have since obtained information about another one at Ibe which differs in some respects from the information which I collected at Ifecodo.

On 20 January, 1962, I was conducting Sir Mortimer Wheeler round the more important sites in Ibe. We had just visited the Ore Grove and were continuing along the Ifewara Road away from Ibe when we noticed the shrine on our left at the top of the slope immediately before the Grove of Obashin. The photographs were taken that day, and my enquiries were made at the beginning of February, when the figure had collapsed, and had been collected for the Ife Museum, where it now is (Registration no. 62/68).

The shrine commemorates Aniwe, who lived at Lemoso Compound, Ilode quarter, Ibe. He held the title of Asipha of Ilode and was considered to be one of the best hunters in Ibe, having killed an elephant. For this reason he had to be commemorated. Besides being a hunter, he was a cocoa farmer, and a Christian. He died on the way home from a private hospital on 7 January, 1962, and he was buried the following day in front of his ‘upstairs’ house.

The ceremony (ibi ipa lagbaja) took place on 12 January, the figure having been carved during the interval by Taye Adegun of 44, Ilode Street, Ibe, a watch-repairer by trade, not a hunter, but as my photographs show, a carver of some skill who carves anything asked of him, notably egungun masks. He was related by marriage to the deceased. The figure was carried in procession by the deceased’s first-born son, together with the basket which his father had used to keep his hunting medicines in. The youngest child of the deceased (whether son or daughter does not matter) carried a chicken. The hunters who took part in the procession called out their greetings to the deceased ‘O digba o’ (‘until then,’ i.e. when we meet again in heaven). When the father answered (at the third call), the figure was put down on the spot with the basket and medicines in front of it, and the chicken was struck against the ground to kill it. It was then cut open, palm oil was poured over it and it was laid on pieces of palm frond on the ground in front of the figure.

A piece of wood igi ita had been taken with them and palm oil was poured over it, and a fire made from it when the figure had been set up.

The lips of the figure were touched with a paste made from the meal which had been eaten during the wake the night before. This had been prepared from dishes the deceased used to like. After this they left without looking back, and none of the participants in the procession was allowed to pass the place for seven days.

The figure, as the photographs show, was carved in a relatively naturalistic style—more naturalistic than is usual among Yoruba carvings, with the exception of the afo (second-burial) figures of Owo. It may be, of course, that the carver was influenced by the naturalistic style of the antiquities displayed in the Ife Museum, or perhaps by photographs.

Fig. 4. Azike on his box-stool

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Only the head and neck are carved. The trunk is left as a rough cylinder, and the arms consisted of a strip of wood attached by nails to the back of the figure. They fell off before the figure was collected for the museum. The figure was clad in a dan-shiki, evidently one of the best gowns of the deceased, not his hunting jacket. The figure is made from onumodo wood and is called ipara ode.

**FIG. 1. HUNTER'S SHRINE AT IFE**
*Photographs: F. Willett, 1952*

when an important hunter is taken seriously ill, a fire of iga ita is prepared for him and the wood from this fire is taken to the shrine for his memorial fire. As Aniwe was in hospital, no such fire was prepared beforehand.

The basket can be seen in the photograph (fig. 1) to be intact. This appears to have been an oversight, as my informant says the basket is normally cut in pieces when it is first put on the ground in front of the figure.

When I was told that the deceased answered the hunters, I thought this might be egungun answering. This does not appear to have been so, however, for although egungun Daramojie belongs to the family of the deceased, he was not taken out on the occasion of the ceremony.

The effect of the ceremony was to prevent the spirit of the deceased from haunting the living. He would be remembered again at Christmas when his friends would call at his house, and he would be commemorated too with all deceased hunters during the annual Olojo festival for Ogun, the patronal divinity of hunters.

My informant was the eldest son of the deceased: Ben Isola Aniwe, of 7 Ajamopo Street, Ilede, Ife. Like his father, he is a Christian (C.M.S.) but also makes sacrifices to Ogun.

**Notes**

3. Mr. John Picton, Curator of the Ife Museum, kindly attempted to make further enquiries from the carver on my behalf, but he found the house empty, and the landlord did not know of the carver.
5. Celtis Swaynii or G. Zemekii. The name is applied indifferently to both species, and both are used for firewood. Cf. J. M. Dalziel, *The Useful Plants of West Tropical Africa*, London, 1937, etc., p. 272.
6. Apart from this, no food was left on the shrine, unlike the Ifetedo example.
7. Photography is now a substantial trade in Ife. In particular, families treasure greatly the photographs of deceased relatives.
9. Presumably the fuller form corresponding to ipade, the name used at Ifetedo.
11. The fact that the deceased was a Christian is clearly relevant here, but Christmas and the principal Muslim festivals are also observed as holidays and appropriate occasions for social events by pagans as well.

**Mixed Cultivation and Distribution of Crops in the Fields as Agricultural Techniques.**

**67**

By Professor Pierre Bettez Gravel, Department of Economics and Sociology, Dalhousie University, Halifax, Nova Scotia. With a table

Eastern Rwanda is a plateau averaging about 5,000 feet above sea level. It has generally been considered as pasture land rather than land fit for agriculture. Nevertheless, subsistence stems very definitely from agricultural activities. It is a rainfall agriculture, but rainfall is very inconsistent in both time and space. Two roughly drawn rainy seasons are recognized: the 'long rains' that should come in January or February and the 'little rains' that should come in September. This results in two harvests, impishi and urungwirigi respectively. The staple crops are beans and plantain banana. The latter is a non-seasonal crop. Sweet potatoes, peas, sorghum, maize, manioc are also grown along with other less important subsidiary crops.
The Rwandese agricultural tools are limited to two very generalized tools: the machete and the hoe. This simple equipment, coupled with the unfavourable ecology, places the people of Eastern Rwanda at a disadvantage in the production of agricultural foodstuffs. The most is made, therefore, of knowledge of the crops and of the soils. This knowledge is evolved into agricultural techniques designed to overcome the problems brought about by the disastrous irregularity of the rains and by poor equipment.

For the purpose of enumerating these agricultural techniques the following rough classification has been made: techniques involving (a) the use of the variable characteristics of the crops grown in mixed cultivation; (b) the advantages of time distribution in the use of fields; (c) the advantages of space distribution of fields; (d) the characteristics of soils.

Whether the Banyaremera are aware of these techniques is a moot question. However, it is a fact that they know and appreciate the different characteristics not only of the various crops, but also of the varieties within the same crop. Examples of these techniques are illustrated below:

(a) The use of the variable characteristics of the crops grown in mixed cultivation. This principle may be applied to the different growth characteristics of varieties within a given crop. For example, there are many varieties of beans. It is usual for all of these varieties to be sown together, so that, whatever the weather, some, if not all, find their own peculiar exigencies and produce a crop.

Or, the same principle may be applied to different species, e.g. beans may be sown with a completely different crop. For example, sorghum is sown in January in a field where an unyanigiti crop of beans has just been harvested. The beans that have fallen on the ground at harvest time are allowed to grow with the sorghum. The beans are harvested in an April-May period, while the sorghum is harvested in May-June. Moreover, the fact that different species make different use of the environment results in greater production from a given amount of tilled land than would be obtained by simply increasing the planting density of one crop.

Other examples of this principle are illustrated by the practice of sowing guhungire (beans and maize in mixed cultivation) and amahunge (beans and peas in mixed cultivation), which has been mentioned previously. Beans can also be sown under growing banana plantations, or in fields of growing manioc.

(b) The advantages of time distribution of fields as an agricultural technique. The signal for planting and sowing among the Banyaremera is the arrival of the first rain. However, the rains are so unreliable and unpredictable that no one can be sure that crops sown at the time of the first rain will, indeed, continue to benefit under the optimum conditions of humidity and sunshine. It is unfortunately true that the first rain—even if it comes at the expected time—may be followed by a month of drought, or by a veritable deluge of water, or by any kind of alternation of sun and rain. Therefore, one may sow a field of, let us say, sorghum broadcast over the prepared dry soil and it is left in the expectancy of the first rain. Then, as soon as the rain comes, the soil is cultivated again. However, this still is no guarantee of success, because of the unpredictable distribution of rain. Therefore, another field of sorghum is sown a week or two later, in the hope that one of the two fields will grow under just the right span of weather it requires.

According to the same principle, the same end may be attained with a different crop, e.g. with beans. For instance, it may happen that beans are sown in March one month or one month and a half after the January-February bean sowings are completed.

(c) The advantages of the spatial distribution of fields as an agricultural technique. This, of course, is the most obvious and the most simple principle. The same crops, for one given season, are often planted in several non-adjacent fields, so that if one field is destroyed by hail, etc., another may be saved. The non-adjacent field may well be the land of a neighbour. The peasants often borrow fields from each other, probably with the idea of spreading the risks further, or to overcome the shortage of land created among their own holdings by rotation failure.

The peasants know enough about their land to know when it has become exhausted, but they do not always seem to know how fast it recuperates, or else they forget when it has been left to fallow. Usually they plant a small patch as a 'test' crop. If it works well one season, they sow the entire field the next season. However, sometimes they prepare and sow a field only to find out that, all conditions being right, the crop still does not grow. In such a case, they may substitute another crop for the one that fails or they may sow another field with the same crop, in which case the new field may be that of a neighbour, as mentioned above.

(d) Characteristics of soil types. Finally, the peasants of Eastern Rwanda have classified soils and use the variable characteristics of each in relation to the above techniques to insure better crops. An informant from Remera, who was generally regarded as one of the best farmers of the Hill, gave me the names of the various types of soil found there. Similar information was obtained from an informant native to Central Rwanda, and also from Monsieur André Bodeux, Chief of the 'Mission du Bassin de la Karuzi,' who had worked on the hydrology of a part of Burundi ecologically similar to the part of Rwanda in which Remera is situated.

Definitions of the soils listed by the Remera informant and compared with the two other sources appearing in Table I are as follows.

1. Inombe (or urubumba) is a heavy soil, dark (black or dark red), with a heavy content of clay. The three sources agree as to this definition. The informant from Remera says that it is best suited for the growing of sweet potatoes and manioc, and, sometimes, a crop of beans that would be harvested in May. It is most often found in the marsh lands.

2. Umusenyi is simply sand. This seems to be definitive in Remera in spite of the fact that the informant from Burundi described it as 'sandy soil,' which, of course, it may well be in Burundi, where no distinction was given for the latter.

3. Urusenyi is sandy soil and must be distinguished from umusenyi, which is sand proper.

4. Ibumba is the dark clay soil found in the swamps proper and used by the potters for their pots and pipes.

5. Ingwa is the fine white clay found usually at the bottom of the depressions on the summits.

<table>
<thead>
<tr>
<th>Table I. the native language terminology for like soils in Remera, Central Rwanda, and Burundi</th>
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<tbody>
<tr>
<td>Remera</td>
</tr>
<tr>
<td>--------</td>
</tr>
<tr>
<td>inombe (urubumba)*</td>
</tr>
<tr>
<td>umusenyi</td>
</tr>
<tr>
<td>urusenyi</td>
</tr>
<tr>
<td>ibumba</td>
</tr>
<tr>
<td>ingwa</td>
</tr>
<tr>
<td>ibitare</td>
</tr>
<tr>
<td>urubuye</td>
</tr>
<tr>
<td>urucekeri</td>
</tr>
<tr>
<td>ikivuvu (ikidudu*)</td>
</tr>
<tr>
<td>igitwa</td>
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<tr>
<td></td>
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<tr>
<td></td>
</tr>
</tbody>
</table>

* Alternative terms
6. Ibbite is not a true soil. It refers to the large stones and boulders that one may find sometimes in the amahanga (pasture-lands area).

7. Urubuye seems to be fine lateritic soil which includes numerous lateritic stones. The Remera informant says that it is good for growing peas, groundnuts, sweet potatoes and potatoes. Both he and the informant from Burundi say that it is most frequently used as pasture land.

8. Urucuseri, according to the informant from Remera, refers to a localized assembly of small stones found isolated in non-stony soils, such as urusenyi, uruhumba (or inoubhe), or igitwa. The informant from Burundi, however, defines it in exactly the same terms that the Remera informant uses to define igitwa (see No. 10, below).

9. Ikivu (ikidudi) seems to refer to a soil that is completely exhausted. It is so fine-grained that it has a texture like flour.

10. Igitwa is fine lateritic soil such as urubuye but without the stones. It is found mostly on impinwa or mugitwa (mu-igitwa), that is, the top part of a hill where people live.

There remain three terms which have no correspondents in the terms listed by the Remera informant: mugiwa (Central Rwanda), which seems to correspond with that description of ikivu (ikidudi), found in both Remera and Burundi (see No. 9, above); urugwagwa (Central Rwanda), which was described as 'whitish clay'; and ikidusi (Burundi), described as being like ikivu (ikidudi) but richer in humus.

Generally speaking, according to Dr. A. van Wambike, these soils situated between 3,248 feet and 3,904 feet above sea level (between 1,600 and 1,800 metres) have a chemical composition as follows:

<table>
<thead>
<tr>
<th>Sample number</th>
<th>C</th>
<th>N</th>
<th>Ca</th>
<th>Mg</th>
<th>K</th>
<th>Na</th>
<th>S</th>
<th>T</th>
<th>V</th>
<th>pH</th>
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<tr>
<td>I</td>
<td>2:21</td>
<td>0:23</td>
<td>2:06</td>
<td>0:12</td>
<td>0:04</td>
<td>5:32</td>
<td>10:12</td>
<td>3:73</td>
<td>2:37</td>
<td>3:50</td>
</tr>
<tr>
<td>II</td>
<td>2:27</td>
<td>0:23</td>
<td>1:02</td>
<td>0:10</td>
<td>0:04</td>
<td>1:83</td>
<td>13:7</td>
<td>1:30</td>
<td>6:16</td>
<td>5:18</td>
</tr>
</tbody>
</table>

Notes

1. For more information on Eastern Rwanda see P. B. Gravel, Remera: A Community in Eastern Rwanda, The Hague (Mouton), to be published.

2. The data from Burundi were collected originally by M. Pahot, who had been part of this 'Mission.' It must be remembered that the language of the Burundi is not quite the same as that of the Bangarwanda and this is related to differences in usage.


Rock Gongs in Tanzania. By Miss M. R. Jellicoe, Makerere University College, Kampala. With two figures

68

In December, 1963, while doing sociological field work in Ikungi Division, Singida District, Central Region, Tanzania (34° 51' long. E., 5° 05' latitude S.), the village people told me of a pile of rocks which they described as 'a house of God' and covered with markings. Later they led me to the site, which proved to be one of the eroded outcrops frequent on the East African Plateau. The particular rock concerned was partly balanced on several smaller pieces which appeared to have broken away from beneath, and in at least three or four different places was marked by lines of hollows about two or three inches wide and perhaps an inch deep. In one place the surrounding rock surface was worn quite smooth.

Photographs taken at the time were later identified by Dr. M. Posnansky of the African Studies Programme of Makerere University College as showing rock gongs. These are the first major ones identified between Zambia and Uganda, and are similar to the Lulac rock gongs (published in MAN, 1965, 31) in that striking areas are on the face as well as on the edge of the rock.

FIG. 1. GENERAL VIEW OF ROCK-GONG SITE, SINGIDA DISTRICT, CENTRAL TANZANIA

A striking area is visible on the lower front face. Photographs: Miss M. R. Jellicoe

FIG. 2. STRIKING AREA ON THE EDGE OF ROCK GONG, SINGIDA DISTRICT

The people state that the markings were there when they arrived in the area, which was probably about 200-250 years ago. The site is probably used for rainmaking or ancestral ceremonies, as are rock-painting sites in the same Region.

The site has been reported to the Conservator of Antiquities, Dar es Salaam, and will shortly be studied in detail.

An Industrial Rite de Passage. By A. J. M. Sykes, Department of Sociology, University of Strathclyde, Glasgow

69

This note describes a rite de passage which is used to mark the transition from apprentice to journeyman status in the printing industry in Britain. The practice appears to be confined to the older, basic, trades of compositor and printing machineman and does not appear to be found among the newer types—stereotyper, lithographer, etc. The following description is
based upon research among printing workers in Glasgow but the practice is also to be found among printing workers in other parts of Scotland and England although it appears to be rapidly dying out. In Glasgow the practice is found mainly in the small and long established printing works and in these it is maintained with varying degrees of intensity. In the large and newer works the practice is not found although many of the workers know of it from their apprenticeship days in other firms.

The following is a general description of this rite de passage as it exists today. The training apprenticeship ends formally at 12 noon on the day on which the apprentice's 'time' is completed. At noon on this day all the other journeymen and apprentices in the department cease work and begin to hammer on their benches or machines while the time-expired apprentice removes his working clothes, puts on his outdoor clothes and leaves the building. This is known as 'hammering out' an apprentice. The apprentice leaves the building but returns almost immediately and asks to see the foreman in charge of his department. He then formally asks the foreman whether he has a vacancy for a journeyman machineman, or compositor, as the case may be. The foreman then formally 'takes on' the ex-apprentice and he is introduced to the men as the new journeyman. In this way the transition from apprentice to journeyman is clearly marked. The apprentice leaves and a new journeyman returns to ask for and receive a job at journeyman status. This is done with the utmost publicity, all work in the department ceases and all attention is turned upon the time-expired apprentice. The 'hammering' announces to all the works that an apprentice has finished his 'time' and thus publicises the fact generally.

In addition to this ceremony the Chapel, that is the unit of trade-union organization at workshop level in the printing industry (Sykes, 1960), organizes a celebration to mark the change of status. This is known as a 'G.I.', an abbreviation for 'General Indulgence' although in some works it is referred to as a 'General Intoxication,' often a very appropriate term. Originally the practice consisted of the new journeyman taking out all the other members of his Chapel for a celebration. At the present time the practice is to wait until several apprentices have completed their 'time' and become journeymen. The Chapel will then vote a General Indulgence, a date will be fixed and the Chapel members will have a 'night out'; this usually consists of a meal followed by an evening's drinking. The new journeymen pay part of the cost but, except in the larger Chapels, part of the cost is also paid from the Chapel funds (Sykes, 1960, p. 54). The payment is authorized by the vote for a General Indulgence. The reason for the change in procedure is that in the past when the practice originated Chapels were small and a new journeyman could afford to entertain the whole Chapel. With the larger Chapels of the present day 'G.I.'s would be expensive and very frequent, hence several new journeymen will share a single 'G.I.'

The origins of this rite de passage are straightforward. The printing trade unions are the oldest in Britain, several being over a hundred years old. However, organization at workshop level through the Chapel is very much older. The earliest Chapel records that we have are dated 1683 and in these the Chapel form of organization is already referred to as having existed 'by custom of time out of mind' (Howe, 1947). In the days before the trade unions were founded there were no formal records of apprenticeship except those kept by individual employers and, obviously, no trade-union cards to prove journeyman status. In consequence it was necessary to impress on the memories of his fellow printers the fact that an apprentice had successfully completed his 'time.' Since the advent of trade unions the need has been less but has, nevertheless, continued into recent years. As recently as 1933 a compositor, Louis Katin, writing in The Worker's Point of View could state: 'When a workman has long passed his apprenticeship period, when he has his Trade Union card in his pocket, and can show that he has given satisfaction to other employers, these proofs are considered adequate for his admittance. But his workmates are not so easily satisfied. Sooner or later the dreaded question casually uttered, arises: "Where were you apprenticed?"' (Katin, p. 137).

However, Katin wrote during the depression at a time when the trade unions could not exercise a strict control and often had to accept as members men who had not served a recognized apprenticeship. Today over 90 per cent. of printers in Britain are trade-union members and the unions firmly control apprenticeship to the industry. In these circumstances the need for such a rite de passage has entirely disappeared. It may continue to linger on in the smaller printing works mainly as an excuse for a social outing but it is fast disappearing and may well be unknown within another generation.

References

Note
1 Known to printers as 'stones.'

Venerated Bees. By Dr. M. J. Field, University of Ghana

In rural England it is (or was) the custom of beekeepers to 'go and tell the bees' whenever a death or other major crisis occurs. If they are not told, they are prone to take offence, desert their hives and take the good luck of the establishment with them. In Atebuch in Ashanti there are two old war drums placed up among the branches of a baobab tree. One of these contains a colony of bees whose honey is never taken and who are kept informed of deaths and disputes. Their caretaker is the Assasuara—the ceremonial land-owner whose forebears were the aboriginal occupants of the land before the Twi-speaking people immigrated into the district. He says that the bees were venerated before this immigration. He still sacrifices a cow to them every year. The Assasuara himself is reticent about his bees, but other people in the town revealed that there are ways in which the bees are used as diviners and their opinion obtained particularly in disputes between the Assasuara and the Chief.

CORRESPONDENCE


With reference to the correspondence of Rose and Leach on the subject of the relevance of demographic data to the study of cross-cousin marriage, might I add these few observations?

1. In terms of the formal structure of asymmetrical prescriptive alliance systems it may be true, as Leach says, that the demographic facts are not relevant. Where the prescription simply reads that a descent group should not give wives to a group from which it has previously taken wives, or that a man must marry a woman from a group which has previously given wives to his descent group, or something such, then the demographic problems about the availability of cousins are probably irrelevant. I should think, however, that this would ultimately depend on the size and genealogical span of the relevant descent groups, their number, etc. Thus, it may be the case that 'demographic variability' would seriously affect the feasibility of following a prescriptive rule in certain types of population with, say, a small number of genealogically shallow descent groups.

2. In societies characterized by preferences for marriage with a first or second cousin then clearly these facts are very relevant. If
age differentials at marriage do indeed affect the relative availability of close cousins, then we know why maternal preferences must predominate regardless of the system of descent or the formal properties of each type of marriage. Here the work of demographers really is important and it is a pity that Rose fogged the issue by attacking structuralists instead of making the positive point which he could have made.

3. In connexion with point 2 I would like to draw attention to two articles which bear on this relationship of demographic facts and marriage preferences.1 Hajnal makes Rose's point in a most sophisticated way and provides an exact model to relate age differentials at marriage and the availability of types of cousin. The other authors show, by means of a computer simulation of population development over 700 years, that if no age barriers were raised to cousin marriages and if the society moved heaven and earth to provide a man with a mother's brother's daughter for a bride, the very best it could achieve would be about 45 per cent. of such marriages. What is more, the feasibility of such a pattern of marriage would be seriously affected by demographic variability.

Thus we know that: (a) only matrilateral cross-cousin marriage is really feasible, and (b) this cannot be universally practised in a society, and for most of the time not even practised on a very large scale, in populations with the demographic characteristics that societies practising cross-cousin marriage usually display.

4. This is primarily with regard to 'preferences.' But it is really the case, as Leach maintains, that such considerations are not relevant to 'prescriptions.' I think that they may be relevant to prescriptions in two ways.

(a) In terms of origins they may tell us why a matrilateral prescription comes to be adopted rather than a patrilateral rule. Needham allows that the Homans and Schneider theory of sentiments may help to account for such origins.2 But the demographic details make it more or less redundant. It would not matter how much a man doted on his father's sister's daughter, he would have a statistically poor chance of marrying her. Thus, only the matrilateral choice would be effectively available regardless of any sentiments on the subject. (It is this theory that Rose should be attacking.) One does not even need to assume that sentiments of the kind that these authors posit were involved. Even if it were not a matter of individual choice but of deliberate 'circulating' of women between corporate descent groups that was involved, the demographic facts would still hold. If these groups were small and composed of closely related persons (genealogically), then it would still be true that a man would be better placed to marry a woman from the group his father had married into, rather than from the group into which his paternal aunt had married. Thus any systematic circulation of spouses between groups would have to take the matrilateral form. Insofar as these groups became larger and internally less closely defined in a genealogical sense, this would become less true. But by then the system would be established.

It seems to me entirely plausible that this was the kind of situation in which such 'elementary' or 'simple' exchanges developed. Individual choices of marriage partners and their governing sentiments are not necessary to an understanding of the situation, although some choices and sentiments are, for example—the desire of groups to enter into marital relations with other groups and to give and take women on some basis other than direct exchange. Such considerations seem to me to be at the heart of Lévi-Strauss's work.3

(b) Even to the understanding of the functioning of 'prescriptive alliance' the demographic facts may well be relevant. What is prescribed after all, so the structuralists are never tired of telling us, is a category of spouses. This point is used to dispose of sentimentalist arguments. But a man does not just marry any old available member of the prescribed category. Within the often very large category of 'permitted wife' a man, or more probably the man's effective descent group, has to pick an actual, i.e., preferred, spouse. Take Australian marriage-class systems, for example. These can be adequately described at a formal level by showing which classes can marry which other classes. But as Radcliffe-Brown, Elkin, Meggitt and Hatt among others have demonstrated,4 a man does not marry any woman of the prescribed class but seeks out a specific genealogically related woman within the class. Similarly, it seems, amongst the contentious Purum, a good many men do in fact marry a mother's brother's daughter.5 She is, after all, the prototypical spouse. If then it is not class or descent group which determines what marriage 'choice' (as opposed to defining the class of permitted mates) but genealogical relationship, then demographic facts may be just as relevant to an understanding of the workings (as opposed to structural form) of some prescriptive systems as they are to preferential.6

To judge this issue, however, we need a good deal of data on actual marriage choices in 'prescriptive' systems. It will probably transpire that this monolithic category needs to be broken down into at least two types: (a) those, like the Kachin, in which genealogical relationship between spouses is not (usually) involved, and (b) those like the Purum in which genealogical relationship may often be operative.

This seems to be an area in which structural, psychological and physical anthropologists (if the demographers will not object to being so classified) could fruitfully pool resources. It is a great pity that failure of communication has led to a situation of sniping from established positions. It may be that the physical anthropologists will make the breakthrough and provide a common framework for discussion by putting the whole question of marriage systems back into an evolutionary framework. I am sure that despite Leach and Needham, Lévi-Strauss would approve.

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Notes


3 Claude Lévi-Strauss, Les structures élémentaires de la parenté, Paris (P.U.F.), 1949, Chapters 1 and 2.


5 See the discussion and bibliographies in the Amer. Anthropol., Vol. LXVI, No. 6, pt. 1, December, 1964.


The Content of Kinship. Cf. MAN, 1955, 75; 1964, 130, 217

Str.,—I have been following the exchange between Dr. Beattie and Dr. Schneider about the nature of the anthropological study of kinship with great interest. It is the following remark of Dr. Schneider, however, that I find particularly confusing, especially in view of the fine distinction which he drew between the conjugal and the jural aspects of marriage, in a note in MAN, 1953, 75:

He has said lately, in defence of the Gellner approach: 'We know what marriage is.' One way or another it turns on the facts of sexual intercourse, undeniably a biological activity' (MAN, 1964, 217). This seems, to me, to be merging the distinction between the conjugal and the jural aspects, which was well made.

I have lately been studying the subject of marriage and I find it far more useful to regard it as a 'rite de passage,' either for both men and women, or only for women, in a given society. It has nothing whatever to do with sexual intercourse as such. Nor is it, as Professor Fortes has stated (in his introduction to Cambridge Papers on Social Anthropology, No. 3, 1962), 'the sanctioned movement from the filial status of son or daughter to the conjugal status of husband or wife,' but rather a sanctioned movement from being
brothers and sisters and children only to being husbands and wives and potential parents as well.

This definition of Professor Fortes overlooks the synchronic (unilateral) conflict between sibling and adult relations that necessitates the incest prohibition. After all, why could a son and a daughter not become a husband and wife as well? In fact, they do. Only the status change takes place in different sibling groups. That is to say that though before their respective first marriages the husband and wife were a son and a daughter, which they continue to be, they were not brother and sister. Though the sex distinction is made, it is purely social, as it is seen in the institution of 'female fathers' in Dahomey and elsewhere, where women play the social roles of husband and father, but engage a male to perform the biological functions. Marriage seems centred rather on the social expectations of being men and women than on biological performance.

Marriage can, therefore, be considered purely as a maturity rite, which involves a third party as a result of the necessity of the sibling-affine distinction on the same generation level. This third party need have no other rights in the partner than in affinem, that is, affinal relations only, as is the case with the Nayar enangar with regard to the tali ceremony which has rightly been described as primary marriage by Dumont. It represents only the social involvement of the allies in the adulthood rites of the group concerned, without giving away any rights in genetricm or in uxorven, that is, rights over the procreative and sexual powers of the women.

This separation of the social role in affinem and those in uxorven and in genetricm is explicable in terms of group interests involved. In a matrilineal society, though the sibling-affine distinction is necessary, the procreative powers of women are the concern of their natal matrilineages. Therefore, once their women have passed to social maturity through marriage, the other functions in uxorven and in genetricm are no longer the concern of the affines and are reapportioned as the matrilineage desires. In societies where the reproductive powers of women are not the concern of their natal groups, the affinal and procreative aspects may be merged in the same institution; but that is hardly a reason for one to merge them analytically.

The institutions of marriage and procreation are two different things. The former is synchronic in character, the latter diachronic, that is, it spans more than just one generation. The degree to which they are merged varies. There is a complete merger of the two institutions in Zulu marriage, where the children are ranked according to the rank order of the marriage; but there is no such distinction among the Baganda, although primary and secondary forms of synchronic marriage exist. A similar situation prevails in many other societies where the children of concubines as well as those of wives are equally legal, although the marital status of the mothers differs.

The special nature of first marriage (whether in time sequence or in rank) is also explicable in terms of a radical status change, which, although it may be reformulated, is seldom reversible, hence the lack of stress on any subsequent reformulations that may occur.

It seems, therefore, that conjugal and procreative rights are not necessarily part and parcel of marriage as such, though their appropriation follows this adulthood rite. Therefore, I feel that the analytical distinction of rights in affinem should be added to the already recognized distinctions of rights in uxorven and in genetricm. Primary marriage may be considered as distinguishable from secondary marriages by the exercise of this right to the exclusion of others. Divorce, too, would mean a negation of this right, while any other form of separation should be described as conjugal separation. Also, the synchronic and diachronic aspects of marriage should be clearly distinguished analytically, and anthropologists should recognize once and for all that they are studying social phenomena, and that their biological referents, if there are any, are only important where they are given social recognition.

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Topless in Crete. Cf. MAN, 1950, 1

73

Sir,—Looking through some back issues of MAN my eye was caught by Sir John Myres' note on 'Minoan Dress' (1950, 1) in which he considered modern parallels.

In connexion with this rare book, Sands' Travels, containing an History of the . . . Turkish Empire . . . also of Greece with the Religion and Customs of the Grecians . . . etc. etc. etc. (seventh edition, London, 1673), is of considerable interest. He travelled in 1610.

He says of the 'Gnossians' (pp. 176f.): 'The Country people do dance with their Bows ready bent on their arms, their Quivers hanging on their backs, and their Swords by their sides, imitating therein their Ancestors . . . called by them Pyrrhichia; and as of old, so used they to sing in their dancings, and reply to one another. The better sort of men are apparellted like the Venetians, and so are the women, who seldom stir abroad except it be to the Church but in the night time. The common people are clothed like the Greeks of Sina, of whom we have spoken; the Women only wearing loose veils on their heads, the breasts and shoulders perpetually naked, and died by the Sun into a loathsom tawnyn' (so much for the beauties of sultan!).

Incidentally he has an interesting passage concerning the labyrinth (p. 176), too long to quote here. He met an English Merchant, who had seen it, and who claimed that a man who had been guiding others in it for over 20 years was himself lost and 'was never more heard of.' He finishes: 'By most this is thought to have been but a Quarry, where they had the stone that built both Gnosus and Gortina . . .'

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REVIEWS

GENERAL

Essays on the Ritual of Social Relations. By Daryll Forde,
Meyer Fortes, Max Gluckman and Victor W. Turner,
edited by Max Gluckman, Manchester (U.P.), 1962.
Pp. vii, 190. Price £1 5s.

The origin of this book was two lectures delivered by Forde and Fortes in Manchester University in 1960, under the auspices of the Simon Fund. Forde discussed Yakó mortuary ritual and Fortes' ritual and office in tribal societies; these were supplemented by an interpretation of some Ndembu circumcision symbols by Turner. All three essays deal with Van Gennep's theory of 'rites de passage,' and this is discussed in detail in an introductory essay by Gluckman. The result is a very useful and stimulating set of ethnographical and theoretical material.

Each author adds something to Van Gennep's interpretation.

Gluckman points out that the main emphasis of Van Gennep's thesis was on a study of the mechanism of ritual rather than on the role which whole ceremonies and specific rites play in the ordering and re-ordering of social relations, and puts this down to the lack of an elaborated theory of society on which Van Gennep could work. Forde's essay shows in detail how, in the final transition rites of death and mourning, the Yakó stress the significance of social forces by putting the major ritual and practical emphasis not on readjustments among the kin, but on fulfillment of obligations to the village council of priests, the corporation of diviners and other associations to which the dead person had belonged. Forde thus sees the function of these procedures in the socially integrative and controlling roles played by the associations, which nevertheless must constantly guard against the disintegrative tendencies arising
from the ties of their members to corporate descent groups. Yako mortuary rites are thus not simply memorial feasts; they are also rites to ensure succession to offices not given by birth. The concept of office is the key theme in Fortes's treatment. He begins with the query, taken up by Gluckman also, of why transition rites have their ritual emphasis. Fortes's answer is that for an individual to play his social role effectively and receive his 'mandate from society' ritual is required to give the right kind of aura to the relationship. 'Actor' and 'part' have to be fused, and what ritual does is to place the technical fact within the moral order, with its social seal. Rites and taboos stimulate a moral commitment and 'keep the feeling of moral obligation alive.' In one way there is nothing very novel in all this, but the careful, sophisticated dissection of elements and the recombination of them in fresh forms of statement is a distinct aid to our analysis in this field.

Gluckman's essay does take up one new theme, namely, why it should be that there is more ritualization of social relationships in tribal society. His answer, as perhaps one might guess, is that in such type of society each social relation tends to serve manifold purposes. (It is not quite clear why he prefers 'multiple' to manifold, nor why indeed the term 'multiple' would not serve equally well.) On this view, ritualization is desirable in order to keep the roles distinct and avoid the practical effects of confusion. This theory, though suggestive, seems to lack something. To sustain it, Gluckman has to argue that tribal societies have a greater elaboration of ceremonialness in all (my italic) their relationships than modern society has, in the form of stylized etiquette appropriate to specific roles and relationships, and that this difference of degree passes into difference of kind (p. 49). It is doubtful if this assertion can be borne out fully; moreover, it takes no account of the very great differences in degree and type of ceremonialness in different tribal societies, even within the same cultural belt. The Samoans and the Tikopia in tribal conditions led very similar kinds of lives, with much the same set of multiple roles, yet the former have elaborated very greatly their social conventions of ceremony and etiquette. The ceremoniousness did not seem to exist in order to differentiate and segregate roles created by the social demands of modern life; on the contrary, new sets of roles were created and ritualized from within the ceremonial field. Another comparatively new note, though one familiar already to readers of Turner's other work, is the detailed and highly delicate interpretation of Ndembu symbols in a way which goes far beyond almost anything attempted in the earlier literature. His examination of the use of tree and plant material as symbols during and immediately after circumcision is given at three levels—of their exegetical, operational and positional meaning. The first level represents the interpretation of indigenous informants; the second refers to the use of the symbol in action, and the third examines its relation to others in the same complex. In general approach this is not so very different from Audrey Richards's study of the Bemba Chisangu, in which she differentiates between primary and secondary expressed purposes and inferred (deduced) purposes, based on considerations of the performer's actions and emotional reactions—a study to which, incidentally, Gluckman might have devoted more attention in his general essay. But Turner brings out very clearly in an original manner the values and norms associated with the symbols, and the manner in which they represent the separation of boys from the sphere where both sexes interact, and their incorporation in a community of male age mates who together bleed, heal and receive nourishment from the generation of their fathers. The very stimulus of Turner's analysis, however, sometimes makes it hard to distinguish assertions of high abstraction from what the Ndembu themselves actually believe. It seems clear that they do not regard these trees and other symbols as merely speculative or theoretical signs but as 'instrumentalities,' as Turner terms them, effecting as well as indicating changes of men's social status. But what then is one to make of Turner's statement that 'there are religious depths here that cannot be fathomed by the analysis of observational data'? And that the symbols have a 'fathomless lucidity of meaning,' which can be grasped intuitively by those who wish? This kind of exalted statement, whilst perhaps a dubious use of the term 'stations' for the stages of the ritual, is surely a comment upon the author's own view rather than that of the Ndembu.

In his general essay Gluckman states five times over that he is bored by Van Gennep. I did not always agree with the authors of the essays, but at least I was never bored in reading them.

RAYMOND FIRTH


The French edition of this book was published in 1958 under the title Anthropologie structurale. The present translation is a good, if at times somewhat overfree, attempt to render the literary style of Claude Lévi-Strauss. Unfortunately, the translators are sometimes quite careless with their choice of words, as for instance 'partial' for partiels (p. 327); indifferently 'rite' or 'ritual' for the French rite (p. 233); 'victim' for l'envoyé (p. 167), etc. However, the extremely precise and clear writing of Lévi-Strauss does manage to survive.

Although the English public must be grateful to the translators for their efforts, it seems to me that, as six chapters had already seen the light in English, it would have been more rewarding if the main work of Lévi-Strauss Les structures élémentaires de la parenté had been translated instead. Recently few English persons have read this work in the original although it has given rise to some of the major developments in kinship analysis (see George C. Homans' and David M. Schneider's attempt to refute Lévi-Strauss in their Marriage, Authority and Final Causes, the reply of Rodney Needham in Structure and Sentiment; no less important is Harrison C. White's An Anatomy of Kinship, which can be traced to Lévi-Strauss's pioneering work; and much of Edmund Leach's own writing on kinship). A hope is here expressed that some university press will finally undertake the translation of this major work.

It may be useful to comment on the concept of structure, the major conceptual tool of Lévi-Strauss's method of analysis, first used in his Les formes élémentaires de la parenté, and the connecting link in all the papers in the present book. According to Lévi-Strauss, what allows the social scientists to talk meaningfully about their research is the structural dimension inherent in the things that they analyse. By structure he means the system, or 'model,' latent in the object of study. The social scientists analyse the data which they collect in an effort to deduce what kind of structural pattern does exist. According to him, as the 'structural dimension' is the object of research, this gives social scientists the possibilities of comparing structures and their properties (see his 'La notion de structure en ethnologie,' in Sens et usage du terme structure, Mouton, 1962, pp. 40-5).

However, what Lévi-Strauss stresses again and again is that the notion of structuralism is not a return to a naturalistic kind of analysis, but essentially an attempt to solve the problem of symbolic representations and interpretations. As he has shown in his La pensée sauvage as well as in Le nénêmes de l'aujourd'hui, if there are logical structures to the perception of things, each logical system used is also an organizational concept of the society or the culture.

Until recently the use made by Lévi-Strauss of the concept of structure has not received from English-speaking anthropologists the same amount of attention as had his analysis of kinship; although the two are inseparably connected. Let us hope that his new publications in English will stimulate among English-speaking students a new interest in this aspect of Lévi-Strauss's thought.

PHILIPPE GARIGUE


According to his preface, Mr. McNeill contemplated this work for 18 years, and wrote it in the following eight. It is certainly an immense undertaking, and, in spite of the limited title, amounts to a universal human history. It is evidently intended to demonstrate the importance of the continuing contacts between the different cultures of mankind, by reaction
against the views of Spengler and Toynbee. It is indeed welcome to see the author stress the derivative nature of the civilizations of early Egypt, India and China, and the primacy of Mesopotamia; but instead of developing this theme, he often hesitates and draws back, or fails to make use of recent work which would support his thesis. His remarks on Cretan origins, for example (p. 94), are based on very old sources, and take no account of recent archaeology, which bears upon the subject, in Asia Minor and Syria. Again, he is commendably unimpressed by the subject of prehistoric contacts across the Pacific and Atlantic oceans (pp. 239-43), but fails to set out the most recent evidence, or to examine its implications.

In general Mr. McNeill is rather oblivious of physical environment, and in consequence he misses the crucial reasons for the rise of pastoral nomadism, and for the degeneration of the neolithic arts in the extremities of the world. His concern is rather with the history and influence of inventions, and above all of ideas. But even here he sometimes seems just to miss the vital point; for surely it was the first Easter rather than the first Whitsun tide which revived the hopes of the early Christians (pp. 341f.), and the nature of Islam can only be understood if it is recalled that it is linked to the written word rather than the spoken utterance (p. 421).

With over 800 closely written pages, this book is evidently too long for the clear exposition of a thesis. It could be used as a work of reference, or consulted section by section for general information. But the reader is left uncertain as to whether he is being instructed, enlightened or patronised. For there is a recurrent contrast between the didactic text, the somewhat mystical commentaries on the artistic illustrations, and the native line drawings, which are evidently intended to be taken seriously. The book is liberally provided with footnote references and clear maps, and is fully indexed. It is accurately checked and meticulously printed; although the dates seem to have gone slightly awry in the legend to the map on p. 173.

W. C. BRICE


In this re-evaluation of the religious concepts and their modes of expression among primitive peoples Dr. Jensen endeavours to come to terms with the earlier theories based on animism, animatism, magic and primitivistic monotheism, described as the 'Ur-Monotheism' hypothesis of Andrew Lang and Schmidt. These he finds inadequate and erroneous because they fail to recognize that in his intellectual and spiritual capacity man has remained essentially unchanged in spite of the heterogeneous nature of his cultural environment and historical situation. Throughout he has consistently exercised rational and logical judgments in appreciation of the different spheres of reality that have engaged his attention and awakened his cognition. Myths and cults, it is affirmed, are the result of creative acts of lively minds and spirits endeavoring to understand the world, its origin, order, organization and the place of man in it, and give expression to conceptions of reality concerning the 'mystery of life'; of mortality, propagation, food as the link between man and his surroundings, and the forces of destiny and providence, lying beyond scientific interpretation. These acquire a functional significance within the cultural framework in which they occur, but in the process of adaptation to varying conditions the original meaning of the myths is lost or obscured.

From these fundamental principles concerning the 'image of man', the conceptions of gods, spirits, the realms of the dead and the associated cultic activities are examined among the 'primitive-cultivators' found over a wide area of the tropics, with whom the author is intimately acquainted. The ingenious distinction between their innumerable mythical ancestors responsible for originating natural phenomena in primaval times, described as 'Demi-deities,' and the more otiose transcendent High Gods of the archaic hunters, cereal-growers and cattle-breeders is a matter for debate. But the significance of this provocative volume lies primarily in the stress laid on the 'meaningfulness' of the myths and cults of primitive peoples.

E. O. JAMES


Those unfamiliar with the author of New Nations may fall to read into this title its full meaning, which might well read 'The Social Anthropology of Socio-Economic Development in Africa, with Some Reference to Asia.' Yet, had Professor Mair brought herself to use such a monstrous designation, it would have alienated her intended audience of non-anthropologists. But, caveat lector: this is sophisticated social science written in an easy, pleasing style. Although Professor Mair would write her 'science' in minuscule, it is the search for broad regularities in social relations which leads her to include comparative materials from other areas than Africa. Through illustrative cases, the reader comes to appreciate the aspirations and anguish of people from traditional societies who must make choices between existing obligations and new opportunities. Such is Professor Mair's basic paradigm for understanding the impact of industrialization and new nationhood. Successive chapters deal with what happens to people in small-scale societies as Western influences draw them into wider relationships. Economy, kinship, polity, and religion provide institutional foci for analysis of the processes of change. Modern urbanization provides a situational setting within which to observe the maximization of these processes. The rubrics are familiar, but they signal clear summaries of the anthropological thought and data on these matters. Even the specialist can appreciate Professor Mair's flair for saying, in sharp, crisp form, things which are obvious only after they have been stated.

In a concluding, critical survey of the history of thought about the nature of culture change, Professor Mair's own stand does not always leave the impression that there is room for other legitimate, but different, theoretical interests. More crucially, the discussion becomes sufficiently abstract and technical to discourage non-specialist readers. But let not this possibility divert such readers from the volume. Professor Mair provides a notably sound and clear statement of anthropological contributions to the understanding of 'development' in the new nations, particularly in Africa. Although written for the non-professional, New Nations should be required reading for any student who aspires to become a specialist in this area.

HORACE M. MINER


In this compact book Schifra Strizower presents glimpses of five little-known Jewish groups: Yemenites, Indian Bnai Israel and Cochin Jews, Karaites and Samaritans. Each presentation is both historical and sociological: the author first summarizes evidence regarding the group's origins and development, and then describes some features of their social life. The Yemenites and many Indian Jews now live in Israel, but this description is of their traditional, pre-emigration communities. Among the five groups the Indian sections are certainly the best—Miss Strizower has done field work among the Bnai Israel and Cochin Jews, and this more lively commentary provides, as James Parkes writes in his Foreword, 'the real meat of the book' (p. 7). The other sections depend upon secondary sources, and although clearly written they are too sketchy to provide much illumination. Brief attention is given to each group's marriage and family organization, rituals and external social relations, but the descriptions are idealized, and there is only a limited sense of real social behaviour.

Why these five groups were selected, and then labelled 'exotic,' is not made explicit in the book. Surely there are other non-Western Jews that might have been included. The selective principle seems to be a search for groups tenuously affiliated with orthodox Jewish tradition. For example, Karaites and Samaritans are tiny sectarian groups, and the 'Jewishness' of the Indian communities was recently challenged by the IsraelI rabbinate (similar charges have also been made against the Yemenites, i.e., that they are not 'authentic' Jews). Focusing upon groups such as these could raise interesting issues for comparative research. The description of the Bnai Israel lends a fascinating glimpse into how the social
institutions of Jewish communities become modelled after their hosts—not only are Bnei Israel a caste in Indian society, they themselves rigidly distinguish between 'White' and 'Black' Jews! There are also hints of how occupational specialization, or informal social controls, perpetuate group ties and allegiances. But these are merely hints and glimpses; each chapter is independent of the others, and no attempts are made at comparison or generalization.

In introducing the book Miss Strizower writes that her intent was to write for 'the man on the street.' In this she has succeeded—the book is skilful and reads well, and it happily avoids any sense of condescension or excessive melodrama. It may serve as a useful introduction to several non-Western Jewries. Yet popular writing cannot proceed convincingly until more basic research has been done. We lack scholarly studies of non-Western Jewish groups—as this book shows, our information still depends upon the sketchy reports of travellers. It is no longer possible to conduct field research in most traditional Middle Eastern Jewish enclaves since many persons have emigrated to Israel, and the conditions of the host nations have also changed. However, it is possible to construct synthetic images of Jewish life in Yemen, Morocco, Syria or Libya—to name a few groups about whom we have inadequate information—through the use of documentary materials and informants. Additional research is urgently needed, in order both to record the traditions of these groups, and to understand how and why they have become changed. ALEX WEINGROD


This is the English edition of Manual de Antropologia Fisica, which was published in 1957 as a companion volume to Professor Comas's introductory course in physical anthropology at the National School of Anthropology in Mexico.

The treatment is mainly classical, with more than half the book devoted to human osteometry, racial descriptions and classifications by traditional criteria, biotopyology, and fossil man. There are full chapters on human growth and Mendelian principles, but only a short treatment of genetic characters in human populations. The sections on the history of physical anthropology and on its applied aspects are full and useful. Some topics, including fossil man, have been revised for the English edition, but others, including most of the genetic traits, are well out of date, although recent references are given.

Designed presumably as a reference book than as a readable introduction to the subject like Howell's Mankind in the Making, it has an imbalance of topics and, in some sections, a scarcity of factual data which restrict its value as a textbook. Footnotes do provide many references to detailed reports, but one would have thought it preferable to include more comparative material where it is available (fossil Primates, anthroptometry, human polymorphisms) at the expense of topics such as constitutional types, where there is so little.

The reader is warned against putting too much weight on differences in mean values of measurements and indices, but there is a lack of discussion on the nature and significance of variability within populations.

In many sections alternative indices and classifications are presented without sufficient critical discussion of their purposes or relative merits. The author's humility in failing to discriminate against particular indices of body build may be admirable, but the student needs some guide to discriminate between them. Again, Comas presents a great number of opinions on such topics as the affinities of fossil Primates, without offering enough information to show why opinions differ. The sections on the Australopithecinae, for example, omit all reference to the pelvis, apart from a drawing, yet include the full range of opinions on their taxonomy and relation to the human line.

The production is generally of a high standard, but there are not a few mistakes and errors of fact, both biological and ethnographical. Why should genus Homo be relegated to homo for most of its appearances?

J. P. GARLICK


An article by John C. Whitehead in the 'Doctor's Image of Man' shows that the relationship between doctor and patient and its change throughout history is also an account of the scientific approach to anthropology. The medicine man's intuitive knowledge of man was followed by the attempt to create scientific theories, the humoral theories were followed by demonology and then by the scientific approach of the Renaissance, which was in turn followed, under the influence of modern chemistry, by the anti-vitalistic 'crusade,' and this again in the nineteenth century by Marxian 'economic determinism' and Freudian 'libidinal determinism.' The article emphasizes the great importance of language in medicine, and in anthropology.

The second part of the book consists of articles on medicine and primitive man: magic, witchcraft, medical theory and primitive man by Paul Fejos himself; a search for the primitive by Stanley Diamond; and three articles on the primitive theory of medicine based on empirical knowledge, on the connexion with religion, and on public health in non-literate societies.

The third part of this book deals with present-day anthropological research on medical man and medicine man in three North American Indian societies.

The fourth and fifth parts, as counterweights, are devoted to psychiatric medicine reviewed in different present-day cultures. In psychiatry the orthodox psycho-analytical line represents the Judaic adherence to abstract principles. The therapeutic psycho-therapy has evolved from Christian charity and faith, and the constitutional and biological psychiatrist derives from the order and rule of the Greek and Roman tradition. The group therapists and child psychiatrists and interaction therapists on the other hand represent the Anglo-Saxon group tradition. Naturally the trends intermingle, but many contradictions and controversies in modern psychiatry and social anthropology may be the outcome of their interaction.

This book describes exactly what its title promises, and will be of equal interest to the doctor and to the anthropologist.

H. LEHMANN


The famous Berlin Phonogramm-Archiv of ethnomusicological recordings, inaugurated by Carl Stumpf at the turn of the century and directed for many years by E. M. von Hornbostel formed the centre for the studies of those early scholars who have come to be known as the 'Berlin School.' From this large collection, Hornbostel made a selection for demonstration purposes and it is the latter which is reproduced on these present records, the material representing about a third of the original, most of which was recorded on the old wax cylinders. The editors have made a point of including items which over the years have resulted in published studies and transcriptions so that reference may be had to the original sounds.

What we have here is, then, a historic collection whose value will increase as time passes, for they represent the musical performance of many peoples all over the world before the levelling and debasing hand of acculturation closed its grasp on traditional practice; for this enterprise the sponsors are to be congratulated.

At the same time they help us to form a judgment on older articles and transcriptions for we can now hear the sounds on which these were based, and at once we realize the limitations imposed by the young and undeveloped business of sound-reproduction. Time, of course, has taken its toll on the original quality of the cylinders, and
record the music at the correct speed—no mean feat when in most cases there is nothing to guide one except subjective judgment: but I suggest that Disc II, Side B, Band 5—which is Congo music—is recorded much too fast. If the turntable speed be reduced from 33 1/2 to 20 r.p.m. the music sounds intelligible and credible.

Apart from their historic significance, what value can the modern ethnomusicologist extract from this collection? He cannot, of course, use them for detailed transcription—for one thing, the words are not clear enough. But he certainly can profitably use them for typological and comparative purposes and he may be lucky enough in fieldwork to come across the same tune still being performed, in which case he could compare it with recordings made half a century or more ago.

What is quite certain is that these records should undoubtedly find a place in the library of every institution which has to do with ethnomusicology.

A. M. JONES

AFRICA

Un bref chapitre relatif à la coutume d’endogamie chez les habitants du Sénégal oriental, problème auquel R. Gessain s’attache particulièrement, termine la partie Hier.

En ce qui concerne la situation actuelle, elle se limite à quelques données statistiques sur la démographie, les densités et la répartition des différentes ethnies occupant le Cercle de Kédougou.

Les vingt dernières pages de l’article annoncent les modalités matérielles des travaux de recherche en cours, ainsi que le programme des travaux à effectuer sous l’égide et avec l’appui du Comité Analyse Démographique, Economique et Sociale de la Délégation Générale à la Recherche Scientifique et Technique de Paris: il s’agit en l’occurrence des études préhistoriques, historiques, anthropologiques (y compris des travaux d’anthropologie normale et pathologique), démographiques, ethnologiques et linguistiques. Comme on le voit, l’ampleur d’un tel programme nécessite évidemment une importante équipe de spécialistes et sera probablement de longue haleine.

Le deuxième article du premier volume, portant le titre Les populations du Cercle de Kédougou, est signé par l’Administrateur Abel Chataignier, excellent connaisseur de la question: sobre et technique, ce précieux texte présente, en somme, les fiches caractéristiques complètes des peuples Malinké (du Nioro, du Danfitt, du Sirimanna et du Bélédougou), Bassari, Peul (Bandé, Kamana, Tanghè, Bowé et Boñi) et Diakaré.

Les caractéristiques principales de chacune des ‘fiches’ portent notamment sur l’ethnonymie, les schémas d’organisation, la langue, la religion, le mode de vie, les principales activités économiques, la localisation géographique et sur l’ancienneté de l’établissement (en rapport avec les migrations).

Pour clore le premier cahier du C.R.A., Raymond Mauny a rédigé quelques pages, d’ailleurs richement illustrées, afin de dresser un bref inventaire des trouvailles préhistoriques et protohistoriques. Il s’appuie, ce faisant, tantôt sur des références bibliographiques disponibles, tantôt sur ses expériences personnelles. Un point important à retenir: à son avis, cette région ne semble avoir été peuplée, de façon appréciable, que depuis la période néolithique.


Marguerite Dupire, spécialiste des groupements peul, s’est enfin chargée d’une tâche, analogue à la précédente, mais qui intéresse les Matériaux pour l’étude de l’endogamie des Peul du Cercle de Kédougou. Le plan de cet estimable travail est composé de chapitres suivants: les migrations peul en haute Gambie, en guise d’introduction; les
Many extravagant claims have been made for the age of South African rock paintings and engravings. After critically assessing the available evidence, Willcox concludes, 'An age of five centuries for the oldest surviving art is I should say certain, an age of twenty centuries quite possible, but longer than this unlikely for rock paintings or petroglyphs under the conditions in which they are found.' Elsewhere he states that the available evidence indicates that 'no surviving petroglyph can be older than six or seven centuries.'

In the chapter entitled 'Who Were The Engravers?', Willcox supports the view that the engravers were 'Bushmen' and the same tribes (or at least culture) as the neighbouring painters. The reasoning advanced to support the thesis that paintings and petroglyphs were executed by the same people is far from convincing and in this section the scientific approach which characterizes the rest of the book is replaced by conjecture. In an attempt to answer the question, 'Why no engravings in the shelters?', the author suggests that the artists 'preferred to paint anyway and engraved only when conditions were not suitable for painting.' This of course is pure supposition. In the few cases where engravings occur in the same caves as paintings the treatment and subject matter of the engravings is quite different from that of the paintings, and these differences cannot be simply explained as being the outcome of using different media.

In several other instances Willcox has not made the fullest use of all the available evidence but nevertheless his book will certainly prove a standard work of reference for a considerable time. Author and publisher alike are to be congratulated for assembling this material in one volume and also for presenting it in such a lucid and attractive manner.

JAMES WALTON


The book comprises a short introduction and succinct, but incisive, comments about form, meaning, interpretation, on nearly 200 sculptures in wood, ivory, stone, metal, mud. This is an original, authoritative, challenging contribution to the study of African sculpture, which will benefit not only the 'wide public' to which it is primarily addressed, but everyone interested in African art and culture.

The authors stress the 'personal character' of their selection, which is for this reason called an anthology. But it is clear that this is a selection based on a truly comprehensive knowledge of African sculpture, which few if any could equal, and on a deeply rooted feeling for it. Altogether there are 53 tribal areas or tribal styles from which examples have been drawn—among them many smaller or less well known groups. The sculptural modes of individual artists are also examined. What is, however, unique about the collection is that we are presented here with a set of sculptural documents which have never, or very rarely, been reproduced in books. This feature alone gives a special flavour of originality and research effort to the book, since so many of the previous publications on African art are saturated with the endlessly repetitious reproductions of the same pieces.

The sequences and perspective in which these sculptures are presented and discussed are challenging. The usual geographical and tribal, or tribal-stylistic categories, have been discarded. Instead the authors present 'a new way of looking at African art' in that they attempt to identify the different types of sculptural form in Africa and to show that 'many of the styles and movements widely supposed to have developed for the first time in Europe really represent recurrent modes in the human arts, modes which have always been available to the artist' (p. 5). This is done in a passionate and critical manner; thus the authors avoid pitfalls of idle speculation and fancy interpretation. The attempt is exploratory and heuristic, in order 'to furnish one means of clearing our minds of our own preconceptions.' The authors repeatedly, and very judiciously, emphasize the point that formal correspondences and convergences seldom extend to similarities of function or content.

In the first part of the book, the vast range of forms produced by the African artists is examined in terms of classic European

Having lightly undertaken to review this publication, I was somewhat disconcerted to find, on the arrival of the review copy, that it was no book in the accepted sense of containing both text and illustrations, but a photographic catalogue of a number of works, with but single-line statements of their provenance, size and material. All that is useful for the reader were lacking; one could not see at the author's predilections nor his style, his bias towards an ethnographic or an aesthetic interpretation, the excellence or otherwise of his index, for none of these things were there for comment. Confronted with the photographs, one was left to one's own devices.

Perhaps this is no bad thing from time to time, and it is salutary to notice one's reactions to such a situation. At the first blush, the reaction was superficial. 'What a typical sixteenth-seventeenth century Benin that one is. 'This? No, I can't quite place it—must turn back to the index and look it up.' Ah! That's surely a Dogon, although I've never seen another quite like it,' and so on. Identification on this level is of no more value than a glance at the headlines of the morning paper. From that came the realization of the impossibility of making an aesthetic judgment on a piece of sculpture from a single photograph, for so one sees it from one angle only; and how much more small, cannot be appreciated in this way. This, of course, is inevitable; but my second feeling of irritation was, perhaps, more justifiable. Some variation in the scale of different reproductions set on one page is also almost inevitable, but it is confusing to contemplate a Warega ivory measuring on the page eight and a half inches high, standing check by jowl with a Baule figure measuring four and a half inches high; and only when one has thumbed one's way back to the index can one check the conviction that in reality the Warega work is six and a half inches and the Baule 14 inches.

Coming down to serious business, on what criteria are these things to be judged? The collector asks us to 'charge boldly into the cultural world' and judge the material 'with less intellectual prejudice and greater artistic discernment.' We are back at the old problem of form and content. Does not artistic discernment depend, partially at least, on some understanding of what the artist is after—what he is trying to say—and to whom he is saying it? Is it not important to sense whether the individual artist is working with vision and integrity, and not merely following a dead tradition or seeking to be with it in the latest local fashion? For this there must be some intellectual understanding of the social and religious background which has produced the work, or we easily become a prey to the hawker of faked antiques in ideas if not in material objects.

Happily although full appreciation of the arts in Africa cannot be attained without very considerable sympathetic intellectual effort, and despite the fact that all the latest gimmicks and -isms of Western art today have largely passed Africa by, it is easy when looking at any collection of African sculpture to pick out aesthetic qualities which are universal.

For perceptive observation and the resulting delicate modelling of facial planes the very differing heads of Benin (2), Ogowe and Dan (9 and 10) would be hard to beat by the most sensitive and highly skilled craftsmen of any art. Then when the total form of a sculpture is considered, the satisfying lumpy animal shape of the Bambara fetish (41) is splendidly conceived, as also the equal satisfactory Dogon figure (8): and as close runners-up come the Baule figure (31) and the solid little Ogoni mask (32). For the appreciation of texture the graining of the block used for the Dogon mask (14) is outstanding, so is the roughly textured Bundu mask on the previous page.

These are all striding from the point of view of form rather than content; but to be honest I find myself shying away from public pronouncements on the subject of content in African sculpture; for after spending 30 years in East Africa in close daily contact with African University students, I have learnt not to make cheap and easy judgments of what I do not yet fully understand. Let it suffice to pick out those works in this collection which I myself find emotionally moving. There is something very touching about the little Ashanti goldweight figure of the hornblower (24); and also the Shango staff of the Yoruba (3), although I have seen other similar specimens to this last which appeal to me more; and for sheer vitality the Dan mask (33) takes a lot of beating.

Am I wrong in feeling that the sixteenth century ivory salt cellar, Afro-Portuguese, (34), is a bastard art, and has some beautiful qualities, especially in the simple low-relief figures on the base, but as a whole I find it unsatisfactory. These are personal preferences rather than public pronouncements; for it would seem to me impertinent to pontificate on aspects which I still do not profess to feel positive about, but there is an appraisement of qualities common to the arts of all peoples, and so within the comprehension of us all.

MARGARET TROWELL

Dr. Salim will give detailed accounts of these in subsequent publications.

This insistence on brevity mars many parts of the book. His chapter on Family and Marriage is another example of the same kind of thing. The danger in this thumbnail sketch of so many aspects of social life as possible is that important facts requiring expansion and analysis come to be given the same weighting as incidental ones. Thus, a paragraph is devoted to Mit'a marriage—a practice whereby Shi'ite Muslims are permitted to engage in marriage unions for a specified period—but we are not told what sort of people practise it, what is its incidence or whether it is of little significance. The information offered is no more than that given in elementary textbooks on Shi'ite law and could have been omitted without damage. On the other hand the section dealing with the incidence of various types of marriage is much too brief, and the
most interesting information is included in the chapter on lineage. It is interesting to learn that 38.4 per cent. of the gross total of marriages are with the actual father's brother's daughter: a very high figure indeed—so high that unless some special factor is operating in this community, it is necessary to explain how such a high rate is demographically possible. The value of this figure (and the other figures, such as marriage) is partly destroyed because, although we are told that census data were collected from 120 families, the universe from which these families were chosen is not specified. Consequently, it is not possible to tell whether Dr. Salim just happened to alight on this number of first-parallel-cousin marriages, or whether the figure is statistically meaningful. My reason for suspecting the former to be the case is that the figure for other paternal parallel-cousin marriages is very low (12.8 per cent.) in the light of the fact that, according to the information, every attempt is made to achieve lineage endogamy. Why should the attempt be so successful wherever first-parallel-cousin marriages are concerned, and not elsewhere? Despite the fact that the term is obviously easier to achieve a much higher figure for parallel-cousin marriage in general than it is for first-parallel-cousin marriage in particular.

As with Arabs everywhere, although the sentiments are strongly in favour of patrilateral cousin marriage, endogamy is far from being achieved. Of the gross total, 37.6 per cent. of the marriages are with women of other clans or with strangers. This is far too high a figure to permit thinking in terms of endogamous lineages. Moreover, if the figure of 51.2 per cent. for marriages within the lineage had been broken up into a number of figures showing the incidence for the various categories of kin that must necessarily be contained within it, then the whole complexion of the question would shift from that of endogamy to a distribution of linkages within localities, to points of power and so on; this would have provided the author with a more significant reference than a lineage framework, and would have enabled him to comprehend the figure for marriages outside the lineage as well.

This preoccupation with lineage, with lineage solidarity and all the other components which go to make up the lineage cluster mars the analysis of social change also. The facts presented are fascinating, but the analysis is much too heavily weighted on the significance of traditionalism. Change comes about only inexorably, and in the face of the stillest resistance to it. Arabs in other lands are notoriously nimble in moving with the times. What is there about these particular Arabs which makes them so pedestrian? The answer which the author gives is the impediment of traditionalism. Clearly, this may appear, at least, to inhibit specific changes, but this sort of barrier is present in most societies. The nostalgia that these people express for the romany way of life—although it must be noted that it is some while ago since these boys were Bedouin—and the values which are said to accompany it may be given as a rationale for sticking to this or that; but however strong the sentiments about is by now surely little more than a vague romantic notion of what things used to be like in the old days, in most of the critical aspects of their lives these people are far removed from a Bedouin way of life. They are sedentary herd-gatherers: hardly a Bedouin occupation. What is of interest is that the local nobility have taken to reed-gathering but despite other occupations, particularly those connected with money-making. The author's view of the situation is that tradition is against these other occupations, particularly those concerned with getting rich. But tradition must surely be against reed-gathering also. Why then should this occupation be given a place of dignity? The answer lies not in giving primacy to traditionism, but in looking upon this not as a community of ex-Bedouin, but as reed-gatherers living on a number of very small islands in a marsh area. The question is, what limitations are imposed by the marshes on change? An answer to this would not give rise to the impression that the inhabitants are stupidly pig-headed, but that they are unable to move towards any other mode of living because the marshes prevent them from doing so. The emphasis in the analysis should have been placed on what the marshes permit, and less on what traditionalism obstructs.

The book gives pleasure to read. With it Dr. Salim has whetted the appetite, and it is hoped that he will publish much more of the store of material which this book suggests that he has in his possession.

E. L. PETERS


This is an excellent book, well written and arranged, dealing with the pattern of family and village and religious life in the Solu-Khumbu area of Nepal. It also pays some attention to the economic history especially in regard to the introduction of the potato within the memory of the father of one of the oldest living men in the area. This has resulted in an increase of wealth, which has also been due to the monopolistic position of the Sherpas in trade between Nepal and Tibet. This wealth has been partly spent on religious activities and the building of new religious houses within the last hundred years. The economics of the area require further study but the author has promised us more books dealing with economics and technology.

The author has also performed a useful service in showing how the rules of the monasteries are not enforced by the general moral rules of the Sherpa society. He gives an example of how a monk and a nun continued their religious way of life after their marriage even though expelled from the religious houses of which they were members. They still continued to keep the respect of the villagers. This is explained according to the author by the tolerant nature of Sherpa society.

In fact one of the author's aims is to paint the picture of this area in idyllic terms in contrast to the 'Hindu villages of Nepal' and the society of Tibet. The long-term immigration of Tibetans into the Sherpa society is partly ascribed to compensations for the lack of food in Tibet and the encroachment of Tibetan village headmen (p. 30). Nepalese Hindu villages are represented as being controlled by pollution concepts and closed in social activities in contrast to 'the intense pleasure the average Sherpa takes in the company of his fellow men' (p. 287). Thus the Sherpas are described with zeal and enthusiasm which tends to affect the reader as well as the author.

There are at least two endogamous classes in the society and possibly more but the author, following his argument in Contributions to Indian Sociology, Vol. IV, 1960, The Hague, pp. 121ff., refuses to call them castes. He also deals with the vexed question of polyandry and polygyny by showing that the question is largely a structural rather than a sexual problem and that children of polyandrous marriages tend to continue to marry polyandrously presumably for the reason that the property of one's father still requires to be undivided to support the family at the same standard of living. All brothers have a right in property and daughters have a right to dowry. Rather surprisingly the Sherpas allow a woman to be married only to one or two husbands at once but not more.

This is an interesting and thought-provoking book, although it seems likely that as more field studies are undertaken among the surrounding peoples many of the unique Sherpa characteristics will be found to be part of a general pattern of peoples living along the Himalayas at about the same height.

WILLIAM H. NEWELL


The Malar are one of the three tribes which inhabit the hilltops of the Santal Parganas in Bihar. They grow various crops by slash-and-burn cultivation. Matar is the most important crop; its cultivation is regarded as a sacred activity and is accompanied by many rites and sacrifices. They also subsist on game and the wild products of the forest.

Each village has a hereditary headman who is usually the religious leader, one or more priests, a shaman and a diviner. The sun is regarded as the supreme Gossaiyan (god or spirit) but has no cult; many other Gossaiyan have shrines and cults and are believed to help the Malar if the proper offerings are made to them. If neglected they may cause misfortunes. There are also evil spirits, who are mischievous unless placated.

There are no clans, but marriage is forbidden within the third degree on both sides. Sexual morals are strict in theory but loose in
practice, particularly in the dormitories in which the unmarried of both sexes sleep. Marriage is usually by bride price, in which case there is a ceremony at which a pig is sacrificed. A menstruating woman is under a variety of taboos which apply also to her husband, who also shares the pollution of childbirth.

The government is trying, hitherto without success, to move the Maler down to the plains. Its purpose is to improve their standard of living and stop the destruction of the forests.

The appendices include 13 autobiographies of typical Maler and an account of their customary laws, with examples of cases which came before the village court.

The author, who is head of the Department of Anthropology at Ranchi, paid, with some of his students, a number of visits to the Maler and, after initial difficulties, managed to get on good terms with these shy and suspicious people. His account is clearly written, but his English is far from being idiomatic. There is a foreword by Professor J. H. Steward, under whom the author studied.

RAGLAN


This translation is to be warmly welcomed. The Russian original was published as Narody Sibiri in 1956. It is leading Soviet specialists describe the material culture and social organization of 30 Siberian peoples, and outline the history of their contact with Russians— including, of course, the impact of the Soviet régime upon them. It is the most authoritative work available on the subject. Chapter 1, on the ancient population of Siberia and its culture, has already appeared in English in the Russian Translation Series of the Peabody Museum at Harvard; the rest has not been translated into English before, but a French translation of the whole book is said to be in progress.

Mr. Dunn, the editor of the translation, has put much work into it. Prolixities, common in most writings in the Russian language, have been sensibly docked. The Marxism, to be expected in such a work, has been muted by omission of the standard ideological passages. This policy may be questioned, but in my opinion is justified if the fact of omission is made clear, as it is. His other policies are equally sensible, allowing flexibility rather than imposing a rigid consistency. I would not personally follow his practice in either place names or tribal names, but there is a good index and most ambiguities can be sorted out. The very large bibliography in the original is of course retained, and its arrangement somewhat improved. The map uses monochrome symbols in place of the 20 or so colours of the original, and yet is quite comprehensible. The illustrations are as well reproduced as can be expected, given frequently poor originals; but the attractive colour plates of the original have not been reproduced in colour, no doubt for reasons of expense. The glossary of local and specialized terms might perhaps have been added to, in order to make clear to the English reader such words as 'pool' or 'klyn.'

The translation, which is by Scripta Technica Inc., seems to be of a high standard. Only small points were noticed in a series of spot checks: for instance, 'Ust'-Russians' (p. 106) would be more comprehensible as 'inhabitants of Russkoye Ustye'; and the standard difficulty of correctly re-transliterating non-Russian proper names has led to Borrow for Borough (or other variants), and Peet for Pet.

In this review, I have chosen to consider the translation and its editing rather than the contents. This is because much translation of specialist works is now undertaken, most laudably, but the standard varies greatly. It is encouraging to find one as competently and carefully done as this. It may be appropriate to mention here that a complementary volume, Istoriiko-etnograficheskii atlas Sibiri (Historical and Ethnographical Atlas of Siberia), was published by the Academy of Sciences in 1961. Arranged by the same editors and written by substantially the same team of contributors, it is richly illustrated and describes and classifies the material culture of the same peoples.

Is it too much to hope that this too may one day have the same treatment from the University of Chicago Press?

TERENCE ARMSTRONG


The Tibeto-Burman-speaking Taron (Taro) or Digaru belong to the group of tribes collectively known as Mishmi, inhabiting the Lohit basin in the eastern tip of Assam. Although the first published note on their language dates from 1837, till now only scantly data have been available. The present work, for which the title of dictionary is rather grandiose, lists some 2,400 words in two parts keyed by Taron and English respectively, and includes 16 pages of phonological and grammatical observations in its introduction. Unfortunately its usefulness is reduced by deficiencies in the transcription: tone, though lexically distinctive, is not indicated, and it is clear from the same to the vowel system itself has not been properly established. The use of different transcription systems in the body of the work and in the grammatical section is quite unnecessarily confusing.

It is to be hoped that the Department will continue to publish materials on the little-known languages in their area of responsibility, but that in their anxiety to make these available to serving officers they will not ignore the need for thorough descriptive analysis to precede the compilation of practical manuals.

H. L. SHORTO

OCEANIA


All those who have interested themselves in the study of cargo cults will give Road Belong Cargo a hearty welcome. Three years of fieldwork and access to documentary materials over a period of ten years and more have resulted in quite the best account of cargo-cult activities that exists to date. Indeed, if there is much, very much more to be said about the sociology of cargo cults and millenarian movements in general, as a study in contemporary history Road Belong Cargo will be difficult to match and will long remain an authoritative account.

Lawrence's concern is with the cargo movements that have taken place in the southern Madang District of New Guinea, an area which is historically relatively well documented, whose peoples have a certain homogeneity, and whose ethnographical description starts with the quite extraordinary adventures in the eighteenth-seventies of Baron Miklouho-Maclay—a man who would have been perfectly at home in the pages of Conrad. Five distinct phases of cargo belief from 1870 until 1950 are identified, and, treating each phase in its historical context, Lawrence relates the events that occurred to his main thesis: that native categories of understanding have changed but little over the period of European contact; that cargo activities are to be understood within all of these categories; that these latter, because they are self-maintaining and self-explanatory, are virtually impervious in the conditions obtaining. Since traditionally, Lawrence maintains, material goods were thought to have been brought into existence by a variety of deities, and access to these goods depended on establishing particular kinds of rapport with the deities, so now—as in the past—the problem of cargo from the point of view of the native peoples is how to establish a rapport with the Christian deities who are thought to be the cargo deities.

From the first impact of Miklouho-Maclay through to German rule, the Australian conquest and mandate, the Japanese invasion and the re-establishment of the Australian administration, Lawrence shows us in a splendid model of smooth-flowing narrative the natives' tremendous concern for cargo and how to gain access to it. Though more might have been made of the depression and of the
materialistic inclinations of Australians themselves, the major features that emerge are, first, that all the way through—with but one exception, Yali—cargo leaders have gone along with the general trend of cargo beliefs: a translation of traditional beliefs into a quasi-Christian idiom rather than the translation of Christian belief into a traditional idiom. Second, Lawrence’s evidence suggests that the political relevances of cargo cults are correlated with rejections of what are thought to be Christian values. Third, though the most effective cargo leader of them all, Yali, consistently endeavoured to steer clear of cargo beliefs, it was nevertheless through these very beliefs that Yali was able to exert the considerable amount of political authority that he did.

This last apparent paradox Lawrence hardly attempts to explain. Indeed, given Lawrence’s particular mode of address I do not see how it is possible to do so. The biography of Yali—which forms the inner hard core of the book, and which is a notable piece of writing and investigation—seems to me to fail in the end for just the same reason. Precisely at those points where Lawrence’s historical sense, crispness of style and knowledge and experience of the people come into relation with received anthropological theory—there do I find him least convincing. The real contribution of the book lies in the brilliantly sustained sociological account from the beginning of chapter II to the end of chapter VIII. But it would be churlish to dwell on the minor inadequacies of theory contained in summary form in chapters I and IX: they do their job succinctly, a second volume is promised, and there is richness enough in the present one.

Cargo cults might be very like omnivorous but servile chameleons. They gobble up positivist and mechanistic social theory and have plenty of spit left over; they are very good at appearing as any one might imagine them to be. Platonists, football-poolers, situational-logicians—they can all have a go. In this book there is a photograph of Kaum, a cargo leader. He is pictured in the act of simulated invocation, left arm outstretched and supporting an object described in the text as a ‘gunshell set on a base.’ A gunshell. Not a shellcase but a gunshell: political, martial, aggressive, phallic, authoritative, technologically ingenious, sophisticated weaponry—all aspects or qualities in which Europeans tend as a whole to excel, even though the gunshell itself [Japanese perhaps?] must originally have been something of a dud. Yet the artist who designed the jacket drawing for the Manchester University Press, and who has clearly based his drawing on the photograph of Kaum, depicts the act of invocation with what appears to be a shoe-last.... Perhaps it is something else again—What I wonder?

At any rate, with echoes of Gombrich in mind, a shoe-last seems as appropriate as anything.

A splendid book. K.O.L. BURRIDGE


The book under review consists of two parts: the first is introductory in character and treats general themes bearing on the subject, and the second, covering 262 of the 461 pages, is a gazetteer of art, styles and sub-styles. Part I is primarily meant to provide a background for the consideration of art in general and of works of art. In several respects, however, it offers much more. For instance, the paragraph on the material conditions of life in Oceania is an example of a sound ecological approach, and provides a quantity of interesting information such as that about the use of the soil in New Caledonia (pp. 12f.). The author does not consider the generally accepted subdivision of Oceania to have much value and particularly stresses the existence of a continuity, geographical as well as ethnic and cultural, between Polynesia and Melanesia, between Papua and Melanesia and between Melanesia and Micronesia (p. 14). With reference to the dating of man’s appearance in this remote part of the world ‘which took place earlier than was generally supposed’ a chronological table is presented based on the results of recent prehistoric research (p. 18). A very useful synopsis is given of groups in Melanesia and Micronesia which either claim Polynesian origin or speak Polynesian languages (p. 22). On the basis of the available data Professor Guiraud ventures to offer a hypothesis of a Polynesian *ethnos* having taken form somewhere in the area of Wallis, Futuna, Rotuma, Tonga and Samoa (p. 25). The treatment of the ecological, social and religious aspects of life in Oceania is mainly illustrated by examples from the Melanesian area with which the author is particularly familiar.

The variations found in the many style areas of Oceania are dealt with in the gazetteer of styles. The text is lucid and down to earth, the quality of the photographs, including the colour plates, is quite satisfactory. The harmony of text and illustrations is spoiled, however, by the fact that references to plate numbers are not given in the text so that the reader loses much time in looking for them. The art forms typical of each of the areas are illustrated. For a number of these—the Sepik area, the New Hebrides, New Caledonia, the Huon Gulf area, New Britain, New Ireland and the Solomon Islands—this picture is fairly complete. For some others, however, the presentation is inadequate. For instance, the art of the fifth continent is not sufficiently represented by four bark paintings (pp. 58f.), three rock paintings and a ceremonial fish figure from Arhinem Land, and an incised wooden panel from Victoria (pp. 140–2). As far as Fiji is concerned, whale teeth and whale-tooth ornaments as well as decorated tapa seem to be too important in ceremonial life to be omitted from a treatment of Fijian art. Samoan objects are not shown and one might ask whether illustrations of a *taupu* headdress and a kava bowl would not have been appropriate in a work in which so much attention is paid to the social function of *objets d’art*. The paragraphs on South-west and North-west New Guinea are definitely unsatisfactory. Of the former area only the Asmat region is dealt with; the Mimika area is mentioned incidentally and in quite another context, to wit in connexion with the photograph of a canoe prow (fig. 172, p. 207) which is presented in the middle of a series of illustrations of Sepik objects. The paragraph in question does not do justice to the rich and varied Asmat art. For instance, no mention is made of the monumental *bujj*-poles, although a photograph showing three of them in situ forms part of a kind of decorative page (p. 429), which is meant to break the monotony of the bibliographical list. Of the few illustrations accompanying this paragraph figs. 120 and 125 have appeared before in books on Oceanic art. The presentation of this style area would certainly have benefited if the author had acquainted himself with the collections in the museums of the Netherlands. The same holds good for the paragraph on North-west New Guinea. The art of this region, which should not all be included in the *korwar* style area, is represented by plates which are mostly rather poorly documented. Although they are referred to in the text (p. 306) none of the so-called skull *korwar* are shown; this is to be regretted for aesthetic reasons as well as for the sake of understanding and interpreting the *korwar* in general.

The division of the style areas into five main groups—in which, for instance, South-west New Guinea has been added to the Lake Sentani area, North-west New Guinea to Massim, and the Admiralty and Solomon Islands to Polynesia and Micronesia—is not accounted for by the author. It has probably been done on the ground of certain stylistic similarities which, however, do not seem to justify this rather drastic rearrangement.

In summary, the first part of Professor Guiraud’s book is an excellent introduction to the ethnology and the art of Oceania. The part dealing with style areas contains a number of good descriptive paragraphs, but the gaps in representation make it somewhat unbalanced and, as a whole, not quite satisfactory.

K. O. L. BURRIDGE


Dr. Ubersi’s study of the Kula begins with a reappraisal and resynthesis of relevant material in the published works of Malinowski, Seligman, Fortune and others—the list of sources is not quite comprehensive; e.g. it might well have included Austen (1945). My own paper (1960) was published after his book was written. Dr. Ubersi’s opening chapters present a useful survey of available data, which provides him with the basis for an attempt to formulate a theory of the Kula and its sociological
significance which would explain the apparent lack of political cohesion within and between the various Kula communities—or at least the lack of a coherent account of their political organization in the ethnography of the area, especially in the works of Malinowski.

In Chapter V the author proceeds to outline a theoretical approach combining Radcliffe-Brown's concepts of solidarity-contratual intragroup and friendship-joking intergroup relationships with Gluckman's theory of the derivation of social cohesion from conflict. He then suggests that the Kula can be understood in these terms, as providing on the one hand the ingredients of conflict in intra-lineage and local group relationships, by affording the members a context of action within which they can compete legitimately with each other as individuals; and on the other hand the means of uniting into a wider 'polity', through the ceremonial partnerships of their individual members, the 'segmentary' local descent groups of the various Kula communities. The remainder of the book consists in reinterpretation in the light of these theories of the available data regarding the political organization and other cultural aspects of the Trobriands, Dobu, etc. I am unable to accept either of these major hypotheses as to the nature of the Kula, or much of the argument on which they are based.

As regards the first of them, the author's discussion in Chapter IV on kinship and marriage particularly in the Trobriands (including the practices of uriguba and pokola) and in Dobu clearly recognizes the rivalry and competitiveness inherent in these relationships. It is not necessary to invoke Kula to explain or 'legitimize' competition between individuals or groups. Kinship and marriage, among other institutions, provide the structure and system in terms of which both the competitive and the cooperative aspects of corporate group membership and activities are carried on, especially in the economic and political spheres (cf. Powell, 1960, pp. 125–8).

Dr. Uiberoi's arguments concerning the magical and other competitive aspects of Kula in my opinion mistakenly see, in that the individual rivalries expressed in them are directed against a man's potential rivals for the favours of his particular Kula partners, who are not normally, as the author suggests, members of a man's own canoe crew or local descent group. With one common exception, these rivals are members of other canoe, local and descent groups than his own, since a man normally avoids having more than one partner in a given local group in order to spread his Kula net as widely as possible. The exception is that a local leader is likely to have among his many Kula partners all or most of the men of any importance in the Kula communities he visits. Thus the followers of a Dobuan 'Big Man' or a Kiriwina 'Chief' may be rivals of their leaders in Kula—but this is no more than an extension into Kula of the intra-group economic and political rivalries and competition which are basically carried on in terms of kinship and affinal groupings and relationships. There is of course a 'feedback' from success in Kula into prestige at home; but a man has to achieve power in intra-local group competition—in the Trobriands in terms of uriguba and pokola particularly—before he can hope to acquire influential Kula partners.

Again in regard to the Trobriands particularly I cannot accept the author's linking of rank with the Kula. Dr. Uiberoi suggests (p. 43) that rank is to be understood as an attribute of villages with rich garden lands and that participation in Kula is a further necessary condition of aristocracy (p. 125); and that it is the Kula relationships of the members which provide an 'external point of reference' in ranking the villages (pp. 97, 124). Elsewhere (pp. 396) Malinowski is taken to task for inappropriately accepting the 'traditions' of Tabula migrations as factual, whereas Dr. Uiberoi suggests that the fact is more likely to be that the people—'owners'—of wealthy Kula-making villages would tend to claim to be and to be recognized as Tabuali. In my opinion the sociological signification and explanation of rank in the Kiriwina social system is that it is an attribute of some descent groups which entitles their members rather than others to acquire wealth—to 'own' or control the best villages and garden lands, to acquire most wives and uriguba, to control the best fishing grounds, to preempt overseas trade and other prerogatives. The acquisition of wealth in that or other ways does not, on the other hand, in the traditional Kiriwina view, entitle people to claim rank as Guyau, or membership of the Tabalu dala (which is of course a descent group, not a rank). Pre-eminence, if not participation, in Kula was in olden times a consequence, not a condition, of rank (cf. Austen, 1945, p. 21).

War did not lead to the usurpation of the land, wealth and rank of the Tabalu by conquerors whose rank was inferior, and not all Tabalu leaders participated in Kula (both these points are noted by the author—pp. 35, 125); the ranking of descent groups was relative to the spatial and social relationships between them (cf. Powell, 1960, pp. 125 ff.). The claims to ownership and participation in Kula is thus misleading, unless it is treated simply as a historical conjecture, and as such it contributes little to the understanding of Kiriwina social and political organization.

Turning now to the suggestion that Kula provided the means of uniting the discrete local descent groups into a wider 'polity,' I find this equally misleading. Dr. Uiberoi hardly goes so far as to suggest that there ever existed a single overall Kula-unitied political community; rather he holds that Kula partnerships provide the basis of both political alliances and cooperation between and within different Kula communities. His arguments here are less clear; it seems, e.g., that Kula exchanges between other hostile groups exemplify this unifying function, and that participation in Kula tends to diminish the disjunction between the various districts. In my opinion, however, their participation in Kula emphasized the disjunction both political and economic between the various major districts, thus expressing their autonomy, while at the same time providing the means to maintain social intercourse between adjacent areas. Kula partnership as Malinowski showed gave safe conduct to participants in otherwise hostile areas, and thus made possible a degree of continuing economic exchange which was of more or less importance to different districts; but this hardly amounts to the making of political alliances.

The author's Chapter VIII on the trade of the Kula is useful; Kula was economically important to some outlying islands, and served to redress the inequalities of the 'balance of trade' between 'haves' (e.g. Kiriwina, particularly) and 'have nots' (e.g. the small islands between Kitava and Murua—cf. Austen, 1945, p. 26) by reinforcing economic with other incentives. But as the author also noted (p. 125) other Trobrianders—in Kavatara, Kailleuva and the small islands to the north-west—traded extensively without benefit of Kula.

As regards the political situation within Kula districts, my paper already cited is relevant. It was not as Kula participants that the local 'Big Men' or 'Chiefs' made varyingly successful attempts to spread their economic nets and with them their political power, but as kinsmen and affines of other lineage and local group leaders. In this process Kula partnerships might help by providing channels through which contacts could be made or help sought; but they appear never to have provided an adequate basis for organizing political relationships. The essence of success in establishing political dominance in the Trobriands at least was the obtaining of wives and through them 'tribute' (uriguba); Kula partners as such never paid tribute to each other; not even metaphorically, if the accounts of their personal rivalries to which the author refers are to be accepted.

The past tense has been used advisedly in some of the foregoing passages; the practice of Kula had changed considerably even in Malinowski's time (cf. Austen, op. cit.). One of my informants said of it that going on Kula was for him and his colleagues 'like you (Europeans) going on holiday.' But the Trobrianders still—at least in Northern Kiriwina—take their own political institutions and leaders seriously enough, and manipulate their contacts with Europeans, Government, Missionaries and traders—and also no doubt anthropologists—: and alternatives to indigenous political techniques such as war which are no longer available to them. In short, in my view Kula played only an ancillary part in local politics, at least in Kiriwina, and cannot be understood as primarily a political institution. Its most significant sociological aspect is in fact adumbrated by the author—that it served to motivate and maintain inter-island and inter-district cultural and especially economic exchanges, and to stimulate economic activities within the major communities; in the latter respect it provided the motive and the opportunities for fairly regular corporate activities between both the major communities and their component independent local
groups. It thus helped to overcome the conflicts and competition between both individuals and corporate groups inherent in their normal relationships as kin, affines, and neighbours.

Dr. Uberti's book is open to criticism as to the arguments used and the conclusions drawn, and to some extent as to presentation also—the absence of an index and the excessive use of direct quotations in some sections are at least irritating. It is when he sees in the Kula the major political institution of its practitioners that I believe him to be wrong. That his methodological approach is nevertheless basically sound, and its application able, is attested by the many more particular inferences that he makes concerning local demo-

**BOOKS RECEIVED**

It has been decided to publish in each issue of *MAN* a classified list of books received from publishers, many of which will be reviewed in later issues. This list covers the period from 1 January to 30 April, 1965. In order that publication of these lists should not lead to a reduction in other matter published, four additional pages will be published this year, and these are included in this issue.

**GENERAL**


**AFRICA**


designed by William Clayes and Sons, Limited, London and Beccles
A HUMAN FRONTAL BONE FROM THE LATE PLEISTOCENE OF THE KOM OMBO PLAIN, UPPER EGYPT

By Professor Charles A. Reed, Peabody Museum of Natural History, Yale University, New Haven, Connecticut

From October, 1962, into March, 1963, members of the Yale University Prehistoric Expedition to Nubia collected and excavated in the area (fig. 1) around Kom Ombo, Upper Egypt. The area is rich in prehistoric artifacts, particularly those of the Upper Paleolithic, hitherto studied almost exclusively by Vignard. His theoretical formulation of three successive cultural stages (Sebilian I-III) had often been criticized but for nearly 40 years was not reinvestigated. The stimulus for further prehistoric study in the area was provided by a programme of conversion of the land to agricultural use, which involved much levelling and ditching, and which was effectively destroying all evidences of prehistory.

In the process of this study, while excavating at a level rich in artifacts typical of Vignard's 'Middle Sebilian Industry,' a partial human frontal bone (figs. 2, 3) was discovered by Dr. Martin A. Baumhoff, of the University of California, Davis, on 4 February, 1963. Subsequently some other fragments of human skull bones were discovered immediately adjacent, but in ground that had been disturbed prior to our excavations, so that we lack stratigraphical evidence of contemporaneity of deposition of these human remains. Parts of at least two skulls are represented, but not necessarily more than two. These specimens have been catalogued as part of the mammalian collections of the Division of Vertebrate Paleontology of the Peabody Museum, Yale University, as follows:

No. 20333 (figs. 2, 3): a partial human frontal bone, to the description and discussion of which the bulk of this paper will be devoted.

* With three figures and a table

No. 21169: most of a parietal bone, but one with the surfaces so eroded that the bone cannot be identified as right or left.
No. 21170: a fragment of the interparietal portion of an occipital.
No. 21171: part of a left parietal.
No. 21172: two small parietal fragments.
No. 21173: the mid-squamous part of a frontal bone.
No. 21174: part of a right parietal.
No. 21175: an unidentified fragment.

The suggestion is made, solely on the basis of the relative massiveness of the bones, that Nos. 21169 and 21174 could be the parietals belonging with the frontal bone No. 20333, and that the other frontal and parietal fragments, much more delicate, could possibly belong together. None of these bones, however, retain any contiguous borders.

The site (Gebel Silsila 2A, Sebilian Area) where the bones were found occurs on the open plain, on the east side of the Nile River, approximately four kilometres north of Silsila Station, on the Cairo-Aswan railway, and one km. east of that railway. The layer in which the frontal bone (No. 20333) and associated artifacts were found is one of several successive water-laid strata deposited in the uppermost Pleistocene period on the right (eastern) bank of a branch of the Nile River since abandoned. The elevation is 96 metres above mean sea level, which is some 7·5 metres above flood-plain level of the present Nile. The detailed geology of the site in relation to the more general Quaternary geology of the area is to be published by Dr. Karl W. Butzer, of the University of Wisconsin, who was geologist for the Expedition.

Two determinations of age have been made by the Yale Radiocarbon Laboratory on materials directly associated with the Sebilian artifacts and the human frontal bone, as follows:

Sample Y-1375 Flecks of charcoal found directly above the layer of the artifacts:
13,070 ± 160 years before 1950 (= 13,230 B.C.).

Sample Y-1447 Shells of the fresh-water clam, Unio willokksi, taken from the level with the artifacts:
13,560 ± 120 years before 1950 (= 13,680 B.C.).

These two 'dates,' determined on different materials, are in essential agreement, particularly in consideration of the fact that fresh-water shell often yields an age-determination a few hundred years older than it actually was, owing to the presence of dissolved 'dead' carbonates in the river water at the time when the shell was produced.

A third age-determination was made on charcoal flecks gathered from an artifact layer in an excavation, the "Microlithic Area," which was about 50 metres distant from the spot where the human bone was uncovered. Unfor-
fortunately, the two areas could not be directly correlated, stratigraphically.

Sample Y-1376 Charcoal from a layer of artifacts of predominantly microlithic type:
15,310 ± 200 years before 1950 (= 13,360 B.C.).

At the time of our excavations, a group from the National Museum of Canada was excavating within a kilometre of us, in Nile silts which on geological evidence were of generally similar age to those in which our artifacts and the human frontal were found. Two radiocarbon age-determinations from these excavations (of 12,290 ± 370 years B.C. and 12,150 ± 450 years B.C.) have been published for a single cultural level, the Sebekian.3

These five radiocarbon ‘dates’ together indicate the general period at which the Nile, during the period of the Late Pleistocene, was still depositing silts over a wide floodplain several metres above its present high-water level, and also indicate the general validity of the two age-determinations of approximately 11,000 to 11,500 years B.C. for the human frontal and other skull bones here being discussed.

The frontal bone, as can be seen from the photographs, is not complete, the right side being better preserved than is the left. The specimen was from an adult modern human, *Homo sapiens sapiens*, undoubtedly male. No indication of neanderthaloid characters is present. The sutures preserved are the fronto-nasal, the right fronto-zygomatic, and a part of the coronal suture on the right side posterior and dorsal to the temporal lines. Antero-medially, the specimen is broken approximately at the level from nasion to the foramen cecum. A part of the orbital plate is preserved on the right side only.

The frontal sinuses were small and poorly developed, the left one rising slightly above the level of the frontonasal suture, the right one not so high. There is, thus, no pneumatization of the supra-orbital region. Such a rudimentary development of the frontal sinuses is, however, within the normal range of human variation, as shown by the following quotation from Morris’s *Human Anatomy*:

‘Not uncommonly the frontal sinus fails to develop into the squama frontalis, remaining related to the pars orbitalis only ... Rarely one or both sinuses are wholly wanting ... It is a well established fact that prominence of the superciliary and frontal eminences has no bearing on the size of the frontal sinus.’

This frontal bone was examined in comparison with the skulls (nearly 24,000) in the Musée de l’Homme, Paris, and several noteworthy anatomical details were evident:

1. The breadth of the bone from glabella to the nearest point on the right temporal line was 59.7 mm., indicating a greater width across the forehead than in most human skulls. For instance, of 133 skulls of Old Kingdom Egyptians from Thebes and Sakkara preserved at the Musée de l’Homme, only two (Nos. 7409, 7410), each 61.2 mm. from glabella to temporal line, had wider foreheads than the specimen here being described; of these 133 skulls, ten have the
Comparisons

There is little published material from the late Pleistocene of Egypt and adjacent areas, which can be used for comparison. The supposedly prehistoric 'Mokattam Skull,' from the Mokattam Hills near Cairo, was found to be parts of three skulls when studied. The time at which these individuals lived could not be determined, but there is no validity to the idea, as published, that they may not have been prehistoric because possibly they were originally purposely buried. One of these skulls, of a male, was stated to have had a particularly low forehead and another, also male, to have been heavy-boned, with a noteworthy massiveness of the frontal bone (thick of 9 mm. at the metopian). No photographs were published, and the skulls could not be found when a search was made for them in Cairo.

A heavily mineralized human mandible, found near the Nile in the northern Sudan, was with stone artifacts of Upper Paleolithic type, but the site of discovery was a wind-eroded surface, so that contemporaneity cannot be proved. However, the suggestion of a Late Pleistocene period is strong. This mandible is somewhat incomplete, and no useful anatomical statements can be made in a comparison of it with the skull bones considered here.

Much more important are the human skeletal materials from 38 graves excavated in early 1964 by the University of Colorado near Wadi Halfa, northern Sudan. Direct association in the graves with an artifact complex of mesolithic character is reported, and an age-estimate of 6000 b.c. is suggested. The skulls have thick bones, wide and low foreheads, and fairly heavy supraorbital arches, characters similar to those of the frontal bone described here. Pending publication of further data, little further can be said now except to suggest that the frontal bone deposited 13,000 years or more ago in Nile silts near Gebel Silsila appears to be quite similar to frontal bones in human skulls of some 8,000 years ago from the area of Wadi Halfa, southern Sudan. Although little was published of them, the Mokattam skulls may fit the same pattern.

Acknowledgements

The research of the Yale University Prehistoric Expedition to Nubia was supported in 1962-63 by grant No. G-23777 from the U.S. National Science Foundation and by grant No. SCC-29629 from the U.S. Department of State, the latter grant being in Public Law 480 funds. Dr. Martin Baumhoff, the original discoverer of the human cranial bones here considered, and prehistorian in charge of Yale's excavations at Gebel Silsila, kindly relinquished these human specimens for study by me. Dr. and Mme. Gessain, of the Musée de l'Homme, Paris, placed the whole of the Musée's magnificent collection of human skulls at my disposal, and were most cooperative and helpful. Dr. E. S. Crelin, Jr., of Yale University's College of Medicine, helped identify the smaller cranial pieces and furnished the comparative material reported in Table I. Mr. Minze Stuiver, director of the Yale Radiocarbon Laboratory, accomplished the age-determinations of the organic samples from the Gebel Silsila excavations. Numerous officials of the Government of the United Arab Republic aided our work, but particularly the members of the Department of Antiquities were helpful. To all these, and many others too numerous to mention, we offer our thanks.

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Notes
9 Photographs of the skulls mentioned were available for study at a meeting at Bellagio, Italy, in August, 1964, organized by the American Society of Archeology for a review of the prehistory of Nubia.

The Antiquity of the New Kom Ombo Skull. By Dr.
Kenneth P. Oakley, F.B.A.

96 After examining the human frontal bone discovered by Dr. Martin Baumhoff at Yale University’s site four kilometres north of Gebel Silsila station in 1963, I concur entirely with Dr. Charles Reed’s conclusions in the preceding article. Chemical analyses of the frontal compared with fossil animal bone from the same site left no doubt of its Late Pleistocene antiquity, as indicated in detail below. In so far as the whereabouts of the original Kom Ombo skull found in 1926 and of the Qau skull bones found in 1923 are unknown (Arkell, 1952), the new frontal is the only extant paleolithic human cranium from Egypt. Since both the 1926 discovery and that of 1963 were from sites on the Kom Ombo plain, I am proposing to term the original Kom Ombo 1 and the new find Kom Ombo 2.

The Gebel Silsila frontal bone was found in the Late Pleistocene Younger Channel Sands (Nilotic Unit III) at a horizon yielding a Middle Sebilian industry and a Late Pleistocene fauna including Equus cf. asinus, hippopotamus and Nile catfish. Radiocarbon dating at the Yale Radiocarbon Laboratory indicated an antiquity of 13,000 years B.P. (for further details see Dr. Reed’s article).

Analyses carried out in the Laboratory of the Government Chemist and in the British Museum (Natural History) gave the following results:

<table>
<thead>
<tr>
<th>N%</th>
<th>F%</th>
<th>100F/P2O5</th>
<th>eU2O3</th>
<th>p.p.m.</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.2</td>
<td>1.2</td>
<td>4.3</td>
<td>36</td>
<td></td>
</tr>
</tbody>
</table>

Hippopotamus patella from Sebilian silt

In their fluorine content the two bones are nearly identical. The nitrogen content in Late Pleistocene fossil bones in this environment ranges from nil to more than 0.2. The amounts of elements of the uranium family (determined by radiometric assay as ‘equivalent U2O3’) show considerable fluctuation within any fossil bone assemblage, but it is specially satisfactory when, as in this case, the human bone under test gives a higher radiometric value than the fossil animal control bone. In summary there is no doubt that the Gebel Silsila human frontal bone is of Late Pleistocene age.

References
2 C. A. Reed, ‘A human frontal bone from the Late Pleistocene of the Kom Ombo Plain, Upper Egypt,’ MAN, 1965, 95.

On the Ideology and Composition of Descent Groups.
By Dr. Marshall D. Sahlins, Department of Anthropology, University of Michigan. With a figure

I have a simple point to make. 1 One takes the gamble of simplicity: perhaps it is naive, something of general knowledge and little moment.

In major territorial descent groups, there is no particular relation between the descent ideology and group composition.

The most definite agnostic lineage systems do not conceal from us certain cognatic irregularities in membership. Now consider that these irregularities would have generated an entirely different descent structure, had they been enfranchised rather than ignored. As readily as they are discounted by a lineage dogma, the same ‘irregularities’ are accredited by a cognatic dogma, or perhaps employed in the service of dispersed unilineal clans. 2 A descent doctrine does not express group composition but imposes itself upon the composition. In the event, a variety of descent arrangements—specifically, segmentary lineages, dispersed clans, territorial clans and cognatic descent groups—may be similar in de facto constitution. They present themselves as different ideological permutations of the same genealogical affairs.

This is just to extend an argument which I made recently in the Journal of the Polynesian Society. 3 If I am allowed to recapitulate the original argument, it will be easier to follow its present extension.

In the J. Polynes. Soc. piece, objection was taken to au courant revaluations of certain Oceanic systems from patrilinear to cognatic—revaluations occasioned by ethnographical testimony of some non-agnostic affiliation, notwithstanding evidence from the same sources of an agnostic ideology. Between other, uncontented agnostic and cognatic systems—went the objection—there are no significant differences in genealogical composition. Agnostic systems may statistically approximate to cognatic in constitution. It is therefore unwarranted to read descent principle from membership for the cases in dispute.

We have to deal specifically with major residential descent blocs. Where descent is thus engaged in the political domain, as the charter of territorial communities and political actions, it is subject to vicissitudes unlikely to beset it in other contexts. It is compromised by residential practices. The ideals of common descent are more or less fiction, as people of different derivation are incorporated in the same group. Still, the genealogical impurities are irrelevant. This is policy, and therefore commitment, not recruitment. In ritual or categorical descent units, a discrepancy between doctrine and membership is unlikely. Small landholding corporations (basic lineages) similarly may be as agnostic in fact as in principle. But in political segments, principle and composition are out of joint.

A few empirical materials—from the New Guinea Highlands, Tiv, Nuer, Toambaits and Nootka—proved enough to show this disjunction between the constitution and the descent ideology of major blocs. (Very much so in Forde’s recent appraisal of the Yak̄o ye̖pun in MAN: ‘An analysis of recruitment to the Yak̄o...
ypum (patrilineal) groups shows that over recent generations 10 to 20 per cent. of members have not been affiliated patrilineally, that only a minority claim unbroken patrilineal descent from the apical ancestor of a lineage and that patrilineal ties are not claimed by all the lineage ancestors within a "clan." 8 The evidence is sufficiently clear. In (territorial) segments of agnostic systems, up to 20 per cent. and more adult males may be living with nonagnates; up to 30 per cent. and more of the members are discernibly non-agnostic cognates by virtue of a female linkage in recent generations; where genealogies do not trouble to conceal it, only the minority of members may descend in authentic agnostic lines. As a matter of genealogical fact, the discernible agnostic bias can be as great in classic patrilineal as in recognized non-unilineal groups. On the other hand, with a prevailing virilocal norm, the agnostic ingredient in cognatic groups (e.g. To'amba) may be greater than in lineages patrilineal by dogma. Political communities of approximately similar agnostic constitution may yet sustain contrasting ideologies of descent—maybe agnostic or cognatic.

(The same empirical materials intimated a connexion between the development of lineality in the intercommunity sphere and its significance within political segments. Where the superstructure turns on segmentary lineage relations, the internal doctrine of participating descent blocs is more definitely lineal. Where the superstructure is not descent-ordered—and alliance, feud and ranking develop more exiguously on other bases—the lineal dogma of political segments is underdeveloped, despite a clear-cut agnostic bias in membership. For here local agrarianism, distinctiveness, solidarity against the world, and freedom of manoeuvre are the overriding structural concerns. In the event, the facilitating doctrine is common descent whomever it may concern—the groups are non-unilinear.)

The ideology of descent has a career of its own, largely independent of internal contradictions in recruitment. Certainly if the doctrine is agnostic, this must affect modes of residence and affiliation. But the composition of the descent group is subject to additional forces. Demographic and ecological circumstances, including military threat and displacement, are known to induce instability of local affiliation in numerous instances. Facts of life overcome norms of membership. And if the facts be known, the ancestry is mixed, cognatic: purity of lineage has been undone. It is nearly impossible to maintain unblemished the agnostic composition in a set of territorial lineages. The system would need perfect freedom at its borders, a demographic moratorium, or else the capacity to intensify production at any and all locations. Lacking one of these requirements, and all are generally lacking, local imbalances in carrying capacity are inevitable; internal population movements therefore likely; hence, the likelihood that political segments fall below 100 per cent. agnostic (male) constitution—and the longer the system persists, which is to say the more successful it is, the less agnostic it becomes.6 Yet the same ecologic and demographic pressures that disturb agnostic constituencies may in the upshot consolidate agnostic ideologies. For under pressure agnation is functional, if not factual: as in defence of claims by authentic lines to scarce resources, in protective consolidation of territorial communities, in minimizing encroachments among coordinate ("brother") communities while maximizing their cooperation for (or against) predation.7 Then too, the agnostic dogma may only be strengthened by a deepening contradiction with kin-group membership, as ideology undertakes its Mannheimian function of keeping people from knowing what’s going on in the world. The most probable long-run condition is paradox, the ‘E.-P. paradox,’ an inverted relation between commitment to agnation in principle and commitment to it in deed—It would seem it may be partly just because the agnostic principle is un-

challenged in Nuer society that the tracing of descent through women is so prominent and matrilocal so prevalent.8 (Since such non-agnostic outcomes need not tarnish the agnic principle, the going explanations of cognatic systems on the basis of restricted resources and consequent flexibility of recruitment must be deemed insufficient. The explanation of non-unilinearity from restricted resources is logical, but it is not sociological.)

A series of diagrams will explicate the argument so far, which is as follows as it was taken in the J. Polyn. Soc., and then carry it to the conclusion that several distinct descent arrangements can be made of the same genealogical materials.

Fig. 1 (1) is matters of fact, how local groups in fact recruit membership, even though virilocal residence and patrilineal affiliation are prevailing norms. Almost necessarily the rules are contravened and agnic strains diluted by non-agnostic cognates. But society is at liberty to take cognizance of the ancestral facts in various ways. Different ideological charts can be enacted upon such a genealogy.

For example, Fig. 1 (2) dogmatically imposes non-unilinearity on the genealogical givens, yielding three cognatic descent groups. In this sociology of descent, ancestral connexions between local groups are discounted, as are external derivations ofincoming men. Neither is membership qualified by agnic pedigree. Common descent, whomever it may concern.

Fig. 1 (3) is a contrasting arrangement of the same genealogy, now fashioned to the design of a segmentary lineage system. Here lineage relations between local groups are politically critical. At the same time, the local groups are ordered as lineages by dogmatic dispensation: non-agnates are incorporated as accessory branches of the lineage. Fig. 1 (3), further, is drawn true to the usual status of accessory branches in segmentary systems, viz. ritual identity is maintained with the lineage of origin, albeit in political dialogue accessory branches participate in the lineage status of "dominant" lines.

Because the diagrams are schematic, it might be emphasized that Fig. 1 (2) and (3) are true also to genealogical circumstances in cognatic and lineage systems. Fig. 1 (2) does no genealogical injustice to the To'amba, for example, and Fig. 1 (3) would do none to Tiv or Nuer.

The classic dispersed clan (Fig. 1 (4)) presents itself as another permutation of dogma. Comparison with the segmentary lineage (Fig. 1 (3)) reveals the main innovations of principle. In segmentary systems, intrusive lines are absorbed as accessory branches of the territorial lineage, leaving only ritual connexion with lineages of origin. But suppose the assimilation of outside lines is inhibited, that instead these lines maintain, completely and exclusively, their original descent status. Although the territorial disposition of agnic lines remains the same, the descent structure is radically transformed—into a set of dispersed descent groups. Add in that common ancestry is only stipulated and not demonstrated, take away the genealogical links between clan ancestors, you come out with American Indians. A classic clan system.

(Notice, however, that a parallel permutation between matrilineal segmentary lineages and matriclans, while as easily diagrammed is probably not as easily documented. Given the analogous genealogical irregularity—a percentage of women attached to their husband's maternal group—matriliney appears to inhibit segmentary lineage formation [of a territorial sort], lending itself instead to clanship. This is the suggestion of empirical incidence: matrilineal segmentary organizations of the Nuer type are rare to non-existent; matriclans run-of-the-mill. It is consistent also with theoretical considerations, notably with the built-in disinclination of men of the matrilineage to loosen control over sisters and sisters' offspring.9 Irregular residence, then, extends

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the range of clans, at the expense of monolineage communities integrated by segmentary lineage relations.)

Returning to agnatic transformations, Fig. 1 (3) fits the base genealogy to a territorial clan-phratry charter. Again, this is a real thing. Over much of the New Guinea Highlands, an incidental inclusion of (non-agnatic) cognates in local clans is a condition of society as constituted, and absorption of said cognates within the agnatic idiom a condition of society as structured. Anomalies of agnatic derivation are eventually exterminated by genealogical amnesia. Cognate lines become by dispensation ‘brother’ to the older agnatic cadres. Clans are usually organized as stipulated brotherhoods of subclans, and phratries as stipulated brotherhoods of clans.

Still other exercises in social design could be performed on the same demographic data. But enough said; the meaning is clear. The overlying descent structure is no expression of the underlying descent composition. Something to the opposite: the major descent system orders genealogical facts in allegiance to its own principles. Subject to arbitrary interpretation, ancestral facts appear subordinate to doctrines of organization, not the doctrines to the facts. Wildly different descent groupings are contrived from fundamentally similar descent aggregates. Truth in a descent system is a specification of structure, not of birth, and in the event ‘true’ ancestry may assume the status of a sociological lie.

Therefore, a serious objection is in order to the popular tactic of perceiving structural principle (‘jurid rule’) as the outcome of how people are associated on the ground and in fact. In the conventional wisdom, structure is the precipitate of practice. Licence is therefore given to the discovery of descent principles from group membership: as from a genealogy; as when Oceanic groups are assigned to the cognatic class for a degree of cognatic affiliation through residence. (We hear talk of the supposed confusion between kinship and descent. What of the more egregious confusion between descent and residence? In the same vein, descent itself is commonly defined as a principle of recruitment. Reading from what people do to how they are culturally organized, received thought would develop the social system from the inside out.

But immediately we shift our sights to the major descent system, the received wisdom falls to the ground. Descent is not recruitment but arrangement and alignment, in the first place a principle of political design, exercising arbitrary constraints on the suppositions of ancestry. And modes of recruitment and affiliation at the lowest levels, these may merely reflect higher-level structural imperatives, thus injecting into everyday household and community life the relations appropriate to the working of the larger polity. A reversal of the conventional inside-out strategy seems indicated.10

We could even entertain the possibility that the descent superstructure is itself under outside influence. The functional context of political alliance and cleavage is after all the social world at large. The issues transcend homebred concerns of whom one associates with, and why. There is a life of society to consider, and of its several community segments as such; selective forces are in play on these levels.11 The idiom of descent as applied to territorial groups, and to the relations between them, proves arbitrary from the vantage of group composition. From the vantage of larger ecological circumstance, including relations to other societies, the design of the descent system may prove less arbitrary. Why not look from the outside in?

Notes


2 Ultimately all affiliation to groups depends on the currently effective ‘ideology,’ i.e., on the values which determine the actions of both the new and the pre-existing members’ (C. D. Forde,
Composite Descent Groups in Canada. By Elliott Leyton, Department of Social Anthropology, The Queen's University of Belfast. With a figure

In the past decade anthropologists have turned their attention to the study of kinship in 'bilateral societies': in particular, interest has been aroused in the nature of kin groups in urban western industrialized society. Previous to these investigations, sociologists had assumed the functions of kin groups in western society to be minimal. Kingsley Davis (1936), for example, wrote:

Modern society, characterized by an elaborate industrial technology, a high degree of urbanization, and a great amount of geographical and social mobility, has sheered away the extended kinship bonds. The sole effective kinship group is now the immediate family, and even this unit has lost in size and function.

While Firth (1936), Mitchell (1961) and Williams (1963) have modified this view, there is still little published material on complex networks of kin in western society.

This paper is an attempt to demonstrate that complex kin groups with important social and economic functions do exist in western society. It is argued briefly that in those sectors of the society which value the maintenance of close family relations, the control of a specific resource is possible. The formation of a kin group with important social and economic functions. Further, the isolated position of immigrants makes them particularly reliant upon kin for social and economic contacts.

The material presented below is a study of a Jewish family business in a Canadian city, Cherbourg. The study shows that kinship—i.e. consanguinity and affinity—can be the principle of recruitment to corporate groups in western society; further, that the activities of these groups, termed here 'composite descent groups', are economic as well as social.

The 'family' studied here is really the combination of two 'families', the Ostroffs and the Allans. From approximately 1939 to 1953 they existed as a single composite descent group, each member possessing corresponding rights and duties in the economic, social and religious spheres. In 1953 the group segmented along agnostic lines into two composite descent groups, and they continue as such as of 1963.

Both the Ostroffs and the Allans were small shopkeepers in Russia at the beginning of the twentieth century. Circa 1920, Morris Ostroff and Harry Allan exchanged sisters in marriage. In the early nineteen-twenties, Morris emigrated with his wife and children to Canada, settling in Cherbourg. During the following decade Morris brought the rest of the family shown on the chart to Canada, and established the Allans in London, Ontario. Harry Allan was a poor businessman and his family struggled on irregular earnings until his only son, Sam, quit school and took over the operations of their small wholesale fruit firm. Although the profits were low, it was nevertheless possible—with the Allans' frugal standard of living—to amass a small store of capital during the depression.

Meanwhile in Cherbourg Morris Ostroff collected junk with a horse and cart and slowly accumulated his own store of capital. In the late nineteen-thirties, Fred Kass—a non-relative—married Morris Ostroff's eldest daughter and Fred was virtually adopted into the Ostroff family. Soon after this, Sam Allan married Morris's daughter Louise and moved to Cherbourg. At this time the Second World War appeared imminent, and to avoid conscription (farmers were exempt) the Ostroffs and Allans bought and settled on a small farm near Cherbourg. On the farm the families congealed into a working composite descent group: they shared a common house; they worked together on the farm as well
as in the city selling junk; male work duties were apportioned by Morris Ostroff; the family shared income and expenditure; disputes were settled by Morris Ostroff; and they participated together in all religious festivals. During the war the primary occupations of the group—farming and junk-collecting—were excellent sources of income, and under Morris Ostroff’s leadership a considerable store of capital was accumulated by the end of the war.

As soon as the war ended the family sold the farm, set up a similar communal residence in the city, and purchased a small retail furniture shop in downtown Cherbourg. Morris Ostroff, with his son George and his virtually adopted son-in-law Fred, each assumed full partnerships in the newly formed Avon Furniture Co., Ltd. The younger sons-in-law, Sam Allan and Pete Gross, were high-salaried employees. In addition, all other adult members of the family assisted in the enterprise, the men acting as salesmen and delivery men, the women acting as bookkeepers, secretaries and cleaners. On the basis of this pool of labour and capital, sufficient resources were available to operate the store without hiring non-kin employees. The firm prospered and by the late nineteen-forties they were sufficiently affluent to provide individual housing for the nuclear families: a single apartment block housed the full partners, and the younger sons-in-law purchased two adjacent homes nearby.

However, a major power struggle had been developing within the corporation. The two younger sons-in-law—Sam Allan and Pete Gross—were dissatisfied with their salaried positions and demanded full partnerships. The full partners were not willing to make this concession and the conflict was at its height when Morris Ostroff died suddenly in 1955, leaving his estate in the hands of his full partners. The full partners sold the stores to the younger sons-in-law and left for the U.S.A., taking most of the Ostroffs with them.

In the new Cherbourg enterprise, Sam Allan, his wife and Pete Gross were full partners (Sam was older and had more capital than Pete, consequently Sam’s wife received an equal share), and the remaining cognates and affines were either salesmen or ‘directors’ (a directorship is a business post with virtually no restrictions on the qualifications, duties and salaries of its holders).

By 1963, both Cherbourg and U.S. corporations were multi-million dollar enterprises, based primarily in downtown property and residential apartments. The residence pattern is maintained in Cherbourg, two elaborate adjacent homes now housing the full partners, and one apartment block—owned by the corporation—housing three generations of cognates and affines. In the U.S. city, the pattern is similar.

At present, while personal feuds temporarily disrupt relations between the segmented composite descent groups, relations are in general still close. However, while relations between the groups are exclusively social, relations within each group are as much economic as social. Social relations between the groups take the form of frequent visits across the border; economic relations within each group continue as in the original single corporation, consisting of a nucleus of full partners utilizing cognates and affines as salesmen and directors.

While the Ostroffs and the Allans refer to themselves as ‘the family,’ there are restrictions on the range of kin permitted membership in the group. While cognates and affines of cognates of full partners are members, relationships are not normally traced further. However, the families have not been in North America long enough for large numbers to have been born and matured, and it has not yet been necessary for strict lines to be drawn. Ranges of kin can be divided into two broad categories: (a) The Full Partners: a group of brothers or brothers-in-law who control the business enterprise and who are obliged to contribute substantially to the support of (b) The Salesmen and Directors: all cognates and affines of cognates of the full partners can claim membership in the enterprise, but are obliged to participate fully in the social activities of the group, and affirm the generosity of the partners to the rest of the Jewish community. Relations between parents and children are rigidly authoritarian, while relations between grandparents and grandchildren are of a highly sentimental nature (as are relations between great-grandparents and great-grandchildren). Within a single generation, all members are at least theoretically peers, although some deference is paid by directors and salesmen to the full partners. In instances where an individual belongs to two generations (for example, Pete Gross is Sam Allan’s nephew and brother-in-law) the individual is placed according to his age and abilities (Pete Gross’s brother-in-law, a man of limited business acumen, was considered more a child than a man).

One of the major problems facing the Ostroff and Allan families is that they do not produce a proportionate amount of males. Consequently, the suitability of the in-marrying male is of
great importance. It is not surprising then that elaborate mechanisms exist to regulate marriage, *i.e.* to ensure that only suitable males are incorporated by marriage into the group. The suitable in-marrying male must be willing to accept the values and to immerse himself completely in the social and economic activities of the group (he is in fact encouraged to neglect if not sever relations with his own cognates). In the case of an unsuitable suitor, which has occurred twice in the past decade, the suitor is discouraged and the female is sent away to kin in the other city where considerable pressure is brought to bear (including appeals to family solidarity and warnings that a bad marriage would ruin the health of the parents).

This leads to an additional question: how is the unity of the group maintained today? Since the primary economic aim of the group, the accumulation of large sums of money, is already fulfilled, what devices exist to ensure that the younger generation will continue to participate in the activities of the group? Basically there are two devices: first, each adult is a potential negotiator and in times of crisis can be called upon to persuade any deviate of his error. Second, and more important, is the economic dependence of all members of the kin group on the full partners. This dependence is maintained by ensuring that any director or salesman enjoys and becomes accustomed to a standard of living out of reach of his own earning power. For example, in 1938, after great conflict, Sam Allan’s eldest daughter married a literature student who had academic ambitions and therefore little potential earning power. After his marriage, this student found himself living in a penthouse, with two cars, charge accounts at major stores, and a low-salaried directorship.

If an individual attempts to sever relations with the composite descent group, considerable pressure is also brought to bear. The student mentioned above attempted this in 1962: in the twelve-month period following his attempt he received weekly phone calls and visits from family negotiators and censure and persuasion from friends of the family.

Religious ceremonies, both private and public, were also approached co-operatively. Before Morris Ostroff’s death, most of the ‘family’ gathered for Sabbath dinners; after his death the regular Sabbath dinners were discontinued, but the family continued to sit together in a block in the synagogue.

Conclusion

Maintenance of this composite descent group offered costs and benefits, rights and obligations to all members of the group. In Jewish immigrant culture the family is considered a unit of fundamental importance, and much more prestige is gained from being the head of a large and wealthy composite descent group than from being simply the head of a rich nuclear family. Full partners, while obliged to contribute heavily to the support of the directors, could legally write off of the cost off on the income tax; in return they received the esteem of their family and of the entire Jewish community. Directors and salesmen, while obliged to participate in the social and economic activities of the group, received security, a high standard of living, and shared in the Jewish community’s esteem for the entire family.

The character of the furniture business and the prevailing state of the market were also important in determining the structure of the group. The furniture business requires a large number of employees with varied, but not necessarily highly developed, abilities: a family group makes available cheap and trustworthy labour for the initial stage of expansion of the business. Further, the state of the market during this period was in general one of expansion, and this made possible the inclusion of a large number of relatives in the enterprise. Finally the Provincial Corporation Law consolidated and maintained the group, providing as it did a legal mechanism for including large numbers at low cost.

One additional point should be stressed—the significance of the ‘leader’ in this type of composite descent group. The personal characteristics of the leader would seem to have considerable bearing on the functions of the group. Without Morris Ostroff’s charismatic leadership it seems doubtful whether the composite descent group in question would have attained its level of complexity and rigidity.

In sum then, we have a situation in which the composite descent group was the discrete unit in residence, property holding, income control and in social affairs; in which occupational roles were closely associated with kinship roles; and in which relations between kin tended to be obligatory rather than permissive. Membership in the group was obtained exclusively on the basis of kinship ties—consanguineal and affinal—and corresponding rights and duties were allocated to all members of the group. The coincidence of sentiment, economics and law allowed the formation of a composite descent group with important social and economic functions.

Notes

1 The author would like to thank C. S. Belshaw, J. R. Fox, S. B. Philpott, and Raymond Firth’s 1961–64 L.S.E. seminar for their criticism and advice. Opinions expressed are the responsibility of the author.

2 See Firth (1963).

3 ‘Cherbourg’ and personal names are fictitious.

4 The term ‘composite descent group’ was suggested by Professor Raymond Firth. It is used here to refer to a group using both cognatic and affinal ties as the basis for recruitment.

5 The material in this paper suffers, of course, from the inevitable distortions and inaccuracies of oral history.

6 Two sisters of Harry Allan were definitely left behind in Russia. It was not possible to determine why these two were left behind, nor if others were in fact left behind. This procedure of recruitment by emigration does however effectively limit—for a few generations at least—the size of the kin group.

7 Morris always referred to Fred—as distinguished from his other sons-in-law—as ‘my son.’

8 In regard to the future of the groups: the youngest generation is insufficiently mature to have decided its career or social affiliation, and no definitive statements can yet be made. However, menial tasks in the business are now being performed by non-kin employees; and the daughters are marrying professional men (against the objections of their parents) with little interest in the economic activities of the corporations. On the other hand, the eldest Ostroff son has completed his training as a lawyer, and now handles the legal arrangements for the U.S. corporation. It seems likely that the structure of the group will alter to form a more optative system, *i.e.* while participation in the social activities of the group may continue to be obligatory, participation in the economic activities of the group will probably be determined by inclination or need. In addition it will become necessary to distinguish ranges of degrees of kin and concomitant obligations. In fact, this process has already begun, the son (not shown in the chart) of one of Sam Allan’s sisters having taken up a career outside the family enterprises.

References


The Blood Groups and Hemoglobin of the Assyrians of Iraq.

By Drs. Elizabeth W. Ikin, A. E. Mounant and H. Lehmann. With four tables

The Assyrians are a widespread group of Nestorian Christians, mostly Syriac-speaking. Prior to the First World War their main home was in the mountains to the north of Mosul on both sides of what is now the Iraq-Turkey frontier. They derive their religious and cultural traditions, and probably to a large extent their physical ancestry, from the original Christian Church of Mesopotamia, founded in A.D. 70. This Church became renowned for its learning and missionary activities, sending missionaries as far as to China, and founding branches in Iran and India which still persist.

### Table I. The A, ABO Blood Groups of the Assyrians

<table>
<thead>
<tr>
<th>Group</th>
<th>Number observed</th>
<th>Frequency observed</th>
<th>Frequency expected</th>
<th>Number expected</th>
</tr>
</thead>
<tbody>
<tr>
<td>O</td>
<td>39</td>
<td>0.333</td>
<td>0.387</td>
<td>38.0</td>
</tr>
<tr>
<td>A₁</td>
<td>27</td>
<td>0.252</td>
<td>0.249</td>
<td>24.9</td>
</tr>
<tr>
<td>A₂</td>
<td>4</td>
<td>0.040</td>
<td>0.039</td>
<td>0.039</td>
</tr>
<tr>
<td>B</td>
<td>25</td>
<td>0.225</td>
<td>0.260</td>
<td>25.82</td>
</tr>
<tr>
<td>A₁B</td>
<td>7</td>
<td>0.070</td>
<td>0.060</td>
<td>0.060</td>
</tr>
<tr>
<td>A₂B</td>
<td>1</td>
<td>0.010</td>
<td>0.011</td>
<td>0.011</td>
</tr>
<tr>
<td>Total</td>
<td>99</td>
<td>0.999</td>
<td>1.000</td>
<td>98.99</td>
</tr>
</tbody>
</table>

**Gene frequencies**

- \( p₁ \) = 0.225
- \( q₁ \) = 0.775

The Church was protected by the early Moslems under the terms of a Firman of the Prophet himself, but as a result of the Mongol invasions in the thirteenth and fourteenth centuries many members of the main community in Mesopotamia were killed, and the survivors driven north into the mountains, where their descendants remained in relative peace until the First World War. As a result of their support for the British cause during the War, of the retention of the greater part of their homeland by Turkey, and of a number of subsequent unhappy events, they have become dispersed in Iraq, and have also formed communities in Syria, Lebanon and the United States.

### Table II. The MNS Blood Groups of the Assyrians

<table>
<thead>
<tr>
<th>Phenotype</th>
<th>Number observed</th>
<th>Frequency observed</th>
<th>Frequency expected</th>
<th>Number expected</th>
</tr>
</thead>
<tbody>
<tr>
<td>MNS</td>
<td>32</td>
<td>0.332</td>
<td>0.332</td>
<td>33.2</td>
</tr>
<tr>
<td>M₁M₅</td>
<td>5</td>
<td>0.955</td>
<td>0.962</td>
<td>9.62</td>
</tr>
<tr>
<td>MNS</td>
<td>32</td>
<td>0.332</td>
<td>0.351</td>
<td>34.77</td>
</tr>
<tr>
<td>M₁N₅</td>
<td>14</td>
<td>0.141</td>
<td>0.126</td>
<td>12.60</td>
</tr>
<tr>
<td>NNS</td>
<td>10</td>
<td>0.101</td>
<td>0.091</td>
<td>0.97</td>
</tr>
<tr>
<td>N₁N₅</td>
<td>6</td>
<td>0.066</td>
<td>0.061</td>
<td>0.61</td>
</tr>
<tr>
<td>Total</td>
<td>99</td>
<td>0.999</td>
<td>1.000</td>
<td>99.00</td>
</tr>
</tbody>
</table>

**Frequencies of gene complexes**

- \( M \) = 0.250
- \( M₁ \) = 0.750

In 1957 Dr. G. R. Hobday, then a student at St. Bartholomew's Hospital, who had been at school with his late Majesty King Faisal II of Iraq, arranged with the King for blood specimens, 99 in all, from Assyrians living in Iraq to be sent by air to us in London. Owing to the political changes which have since taken place it has proved impossible to ascertain from which of the Assyrian communities these were taken, but they presumably lived within relatively easy reach of Baghdad.

### Table IV. Sundry Blood Groups of the Assyrians

<table>
<thead>
<tr>
<th>Phenotype</th>
<th>Number observed</th>
<th>Frequency observed</th>
<th>Gene</th>
<th>Gene frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>P₁⁺</td>
<td>65</td>
<td>0.566</td>
<td>P₁</td>
<td>0.4140</td>
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<tr>
<td>P₁⁻</td>
<td>34</td>
<td>0.334</td>
<td>P₁⁻</td>
<td>0.5860</td>
</tr>
<tr>
<td>K⁺</td>
<td>3</td>
<td>0.036</td>
<td>K⁺</td>
<td>0.0153</td>
</tr>
<tr>
<td>K⁻</td>
<td>96</td>
<td>0.697</td>
<td>k⁻</td>
<td>0.9847</td>
</tr>
<tr>
<td>Fy(a⁺)</td>
<td>93</td>
<td>0.939</td>
<td>Fy</td>
<td>0.7338</td>
</tr>
<tr>
<td>Fy(a⁻)</td>
<td>6</td>
<td>0.060</td>
<td>Fy⁻</td>
<td>0.2662</td>
</tr>
</tbody>
</table>

At the Blood Group Reference Laboratory the specimens were tested for the antigens A, A₁, B, M, N, S, H, P₁, C, Cₑ, c₁, D₁, D, E, e, V (ce), Luₑ, K, Fy and Diₑ. All specimens were found negative for Luₑ, He and Diₑ. The results of the remaining tests are shown in Tables I-IV. At St. Bartholomew's Hospital, London, they were tested directly for sickling and by paper electrophoresis for abnormal hemoglobins and possible indications of thalassaemia, such as a raised amount of hemooglobin A₂. No abnormality was discovered.

**Discussion**

The present series shows a frequency of blood-group gene A considerably lower, and of B considerably higher than those found.
for previously tested series of Assyrians. This discrepancy is probably due to accidents of sampling, but could be due to an admixture arising from one of the many Moslem communities in Iraq and Iran which show similar trends.

The MNS frequencies, with rather high M and MS, are typical of the Near East, but the total S frequency is unusually high. The Rh frequencies are also typical of the region, the most important feature being that the frequency of the cDe or R̄_s complex, almost certainly of African origin, and showing the presence of an African component in the population, is, though substantial, considerably lower than that found in most of the Moslem communities tested in Iraq and the other Arab countries of the Near East (but higher than in the southern Turks and the Arabic-speaking Eti-Turks).

The very high frequency of the Duffy or Fy² gene is more like that found in India and further east than that usual in the Near East. The frequency of P, is, however, only slightly below the typical European level, though tending rather towards the low values found in Asia than to the high ones in Africa.

It may thus be concluded that the Assyrians of Iraq are a population not differing widely in their blood-group frequencies from those surrounding them, and that they have probably not been long subjected to any extreme degree of reproductive isolation.

The absence of abnormal haemoglobins is not unexpected in a population related to the Arabs of south-west Asia.

References


Problems of Ethnological Research in North American Museums. By Drs. D. Collier, Chicago Natural History Museum, and W. N. Fenton, New York State Museum, Albany*

The topic of this paper requires a brief discussion of terminology and of the organization of museums in North America, since these differ from customary usages in many parts of Europe. In the United States and Canada, both in museums and universities, the term anthropology refers to the science of man, which includes physical anthropology, archaeology, linguistics, and ethnology. The last is closely allied with social anthropology, which concentrates on social relations and social structure. In most departments of anthropology in universities training is offered in all these fields and candidates for higher degrees are required to have some general knowledge of all of them. Anthropology museums are usually organized along the same lines, with collections and research programmes in physical anthropology, prehistory, and archaeology, and ethnology (ethnography). Often these activities are organized as one department in a museum of natural history.

Prior to 1930 anthropology museums (and museum departments) played a central role in the development of anthropology in North America (Collier and Tschopik, 1954). Their staffs were a prominent segment of the small number of professional anthropologists and they performed an important function in teaching as well as in research (Kroeber, 1954, p. 76). The museums were the most important centres of ethnological research and the principal sponsors of field work. This research was mainly oriented toward problems of classification and culture history, and there was great interest in material culture and technology. There was emphasis on the salvaging of the disappearing cultures and languages of the Indians of North America (and of peoples elsewhere in the world).

During the intervening 35 years the centre of gravity of anthropology has shifted from the museums to the universities and today museums in North America occupy a peripheral position with respect to anthropological teaching and research. This shift has resulted from two major trends. One is the tremendous increase in the number of non-museum anthropologists attached to universities, government agencies, hospitals, and research institutions. During the past 15 years alone this accelerating increase has tripled the number of professional anthropologists in the United States, from about 600 in 1947 to over 2,000 at present (Some Foundations for Publication Policy, 1964, p. 1). In contrast, museum anthropologists have increased relatively little. The other trend is the tremendous growth of social anthropology, other disciplinary specialties away from traditional museum interests, which have not changed much. In anthropology as a whole there is a very little interest in studies of material culture and technology. As a result of these trends a very small proportion of anthropologists are using museum collections in their ethnological research, and museums have difficulty in recruiting able ethnologists for their staffs. Even museum curators have tended to shift their research interests directly into the most fashionable new fields rather than to attempt to use the ethnographical collections in new kinds of research related to some of the recent interests of anthropology (Mason, 1960, p. 345ff.).

In short, anthropology museums in North America, in spite of their very rich and extensive ethnographical collections, have become isolated from the most active areas of development in anthropology and they are not fulfilling their potential in the field. There is a great need for anthropology museums to assess and redefine their roles as institutions, to relate their research programmes both to their collections and to new developments in anthropology, and thereby to stimulate greater use of their collections for ethnological research by non-museum anthropologists (Fenton, 1960; Collier, 1962). These needs are important not only for the museums but for the balanced development of anthropology as a whole.

The current problems of anthropological museums in North America are not only intellectual but also technical. First of all, museums are inadequately financed. The traditional sources of support—private endowments, some support from government bodies, and gifts—are not sufficient in this period of rising costs. The new sources of money for science and education from the great private foundations and the federal government are directed mainly toward the universities. This lack of money has prevented the expansion of museum staffs and placed museums at a competitive disadvantage in maintaining their staffs. Research in museums is also handicapped by a lack of modern facilities. There is great need for improved methods of housing and preserving the ethnographical collections and new ways of storing and retrieving the documentary data pertaining to the collections. An index ethnographicum covering the museums of the United States and Canada would be a tremendous aid in comparative research (Fenton, 1960).

These and related problems were discussed by 20 museum and university anthropologists at the Conference on Museums and Anthropological Research held at the National Museum of Canada in Ottawa in October, 1963 (Collier, 1964). The conferences agreed that, regardless of current difficulties, research is a
basic function of anthropology museums and that without a productive research programme the collecting, exhibition, and educational activities of a museum cannot be effective. They also affirmed that anthropology museums still have a very useful function to perform in teaching and research training, although this may not be widely recognized in the universities. They believed that the intimate acquaintance of a student with the material culture of a native people adds a dimension to his understanding of culture that can be gained in no other way; this is an opinion shared by many European ethnologists (e.g. Lévi-Strauss, 1963, p. 375).

The Conference proposed several ways to increase the research use of ethnographical collections and to encourage field work on material culture by graduate students and non-museum anthropologists.

1. The establishment of joint museum-university research programmes. In such a programme an indispensable resource would be an ethnographical collection in the museum, the study of which would be tied into the field research.
2. Grants-in-aid and fellowships offered by museums to graduate students and professional anthropologists for research on the ethnographical collections alone or combined with field work. At least two United States museums have successful programmes of this sort.
3. The teaching of courses on primitive technology in universities or by museum staffs for universities. At present such courses are rarely given.

The Conference discussed various lines of research that would be appropriate for museums and agreed on a number of recommendations.

1. Museums should sponsor field work to add to the documentation of existing collections.
2. Anthropology still has the obligation to complete the ethnographical coverage of the world's peoples, and museums should sponsor field research in key areas.
3. In special areas like art, the field research needed to understand function and meaning in culture has hardly been begun. Anthropology museums should take the lead in such studies in Oceania, Africa and elsewhere before it is too late.
4. There is need for the systematic collection and study of the material culture of peoples undergoing rapid change. This would add to knowledge of cultural change in general and technological change in particular.

Research techniques and aids were discussed by the Conference at length. The three following topics were most important.

1. Data-processing. A detailed description was given by Erna Gunther of her use of punch cards in her study of Northwest Coast collections in European museums and by Philip J. C. Dark of his use of punch cards for the recording and analysis of stylistic traits in the total corpus of Benin art. The utility in museum research of such cards either for hand or machine sorting has been amply demonstrated. The permanent storage on punch cards or computer tape of basic museum documentary data is a promising possibility that needs further investigation.
2. There is great need for an inventory of research collections in North American museums. A pilot project in a limited area is the first step, and plans are being worked on to cover the museums of a single state, such as Rhode Island or New York.
3. Anthropology museums would be appropriate centres for the perfecting and promotion of the use of sound recording and still and motion photography for the documentation of culture, and museums should establish the needed archives for the processing and preservation of this class of documentary material.

Great concern was expressed by members of the Conference about the problems of preserving ethnographical collections as research resources of the future. The discussion centred around three points:

1. Since objects without cultural context are of very little or no research interest, standards of documentation for ethnographical collections should be established for all future collecting, and efforts should be made to augment the documentation of existing collections from living cultures.
2. Museums should be very cautious about disposing of their collections. Large series of specimens which might be considered 'duplicates' are particularly valuable for research and should not be dissipated.
3. Although the physical deterioration of ethnographical collections is a serious and pressing problem, few anthropology museums in North America have conservation laboratories. Every museum must have a scientific conservation programme. A North American centre for the conservation of anthropological collections should be established to help achieve this end.

The changing world and the growth and diversification of anthropology demand new roles for North American museums. The museums surely have vital functions to perform. They are an important link between anthropology and the natural sciences. They are the main remaining centres of interest in historical ethnology. They guard research collections of great potential importance. And they have the possibility of restoring to anthropology a fruitful interest in material culture and technology. We believe that the museums will find the resources to grow and change as they need to and that they will develop creative research programmes for the future.

Supplementary Note

A pilot project to make an inventory of collections in museums in the State of Oklahoma was initiated in April, 1965, under the direction of Dr. Alec Ricciardelli of the University of Oklahoma with a grant from the United States National Science Foundation.

References


Siriokwa Holes, Stone Houses and Their Makers in the Western Highlands of Kenya. By J. E. G. Suttion, University College, Dar es Salaam. With a figure

In MAN, 1964, 178, Mr. D. S. Noble described a double-circle stone house which he excavated at Tambach (0° 36' N.; 35° 31' E.) on the Elgeyo escarpment, which forms in spectacular style part of the western wall of the Kenya rift valley. His calling it a 'Siriokwa hole' or 'pit dwelling' requires some qualification, for, as he observed, the structures at Tambach 'are of a peculiar design'; in fact they hardly merit the name 'Siriokwa holes' in the normal sense. In elucidating this point the present paper briefly describes the results of recent study and excavations of Siriokwa holes and of the Tambach remains, and discusses their implications for the history of the Kalenjin, a group of closely related tribes sometimes referred to as the 'Nandi group' of Nilotic-Hamites. The Kalenjin constitute the principal population of the western highlands of Kenya. The Elgeyo are included among them.

Siriokwa holes proper do, it is true, vary somewhat from one locality to the next, but in their essentials they are sufficiently alike to bear an overall description (fig. 1, a, b). They are saucer-shaped hollows occurring in numerous groups on the hillsides throughout most of the highland region west of the rift valley, as well as in the higher parts of the rift itself around Nakuru, and certain areas further east. They have been variously described as 'hut-circles' and 'pit-dwellings' belonging to an 'Azanian civilization' or connected with the 'Gumban B' variant of the stone-bowl culture—acceptations that prove definitely unsatisfactory. Local traditions assert and excavations confirm that Siriokwa holes were enclosures for livestock, especially cattle of Zebu type. They normally measured between 20 and 50 feet in diameter and occasionally as much as 12 feet in depth. Each one was surrounded by a stout fence or stockade, broken by a narrow, usually sunken, and strongly defended entrance with a gate and sometimes a draw bar as well. The entrance always faced downhill and was often invisible from without, for the approach was commonly through a passage that curved round or between broad mounds formed apparently of mud and dung scraped daily from the enclosure. This scraping activity cannot, however, explain the hollowed shape of the enclosure, which was essentially the result of deliberate excavation, the upcast of which, be it earth or stone, was banked around the perimeter, especially on either side of the entrance. In stony country rough boulders might be employed for a neat dry-stone revetment lining the whole interior of the Siriokwa hole and its entrance passage (as in fig. 1b). There have been speculations on how Siriokwa holes were roofed; but it is now clear that they were open to the sky. However, one or two circular houses with stockade-type walls and presumably conical threshed roofs were attached to most or all Siriokwa holes. In the excavation of one at Kabyooyon Farm at the north-eastern foot of Elgon's foundations of two adjacent huts were revealed in the bottom of the enclosure. A more normal position for a house was outside the perimeter fence, with a single door connecting the house and the stock enclosure. Such house sites are sometimes detectable before excavations as small and usually vague annexe. But it is hardly correct to speak, as Noble does, of Siriokwa holes in Kisigis country consisting of a 'central pit with smaller pits around it, the whole housing a family group.' On the last point it appears from the excavations that the adjoining house or houses did in some cases serve a whole family, but in others merely a cowherd. For a rich family must have needed several Siriokwa holes for its cattle. This last can only have been a minor factor contributing to vast numbers of Siriokwa holes, which would have been occasioned more by each family's need to prepare new ones every few years.

On the Elgeyo escarpment there are to my knowledge no real Siriokwa holes (though several groups are known in the forest border along the escarpment crest). However, on the bushy shelves that break the steep drop along a short stretch of the escarpment around Tambah there are a number of the double-circle (or occasionally triple-circle) stone houses (fig. 1c), such as Noble investigated. At first sight they look quite unlike Siriokwa holes. The two circles, which lie side by side, somewhat sunken into the natural slope, are of about equal size, normally between 13 and 20 feet in diameter. The most obvious point of comparison is the internal stone revetting, which is virtually identical to that in the finest stone-lined Siriokwa holes of the forest border and the Uasin Gishu plateau. But besides this, Galloway's description of a stone house actually in use in the nineteen-thirties, and the results of those excavated by Noble and more recently by myself, show that basically they are derived from Siriokwa holes. The approach was from downhill, and led between banked-up, revetted, and probably fenced terraces into a narrow, sunken and curving entrance passage frequently with a draw bar. Through this passage livestock, including cattle, were filed or squeezed into the outer circle, which was paved and stockaded but not roofed. The inner circle, whose only access was from the outer one, was the house, which was roofed with a cone of thatch.

**FIG. 1. SIRIKWA HOLES AND TAMBAH STONE HOUSE**

Schematic plans, as revealed by excavations, of (a) Siriokwa hole without stonework, (b) Siriokwa hole with revetted lava stonework, (c) Tambah stone house (house, stock enclosure and entrance are all sunken)

The difference between Siriokwa holes and the Tambah stone houses, therefore, is mainly superficial or one of proportion. Whereas at Tambah the actual house circle and the stock enclosure were of equal size and prominence, with Siriokwa holes the enclosure is considerably larger and the adjoining house or houses are not normally discernible unless the site is excavated. There are some structures in the forest border above Tambah which are probably transitional between Siriokwa holes and the Tambah stone houses.

Both features were devices for guarding livestock and their owners, doubtless from thieves as well as wild animals—leopards in the woodlands, lions in the grasslands. They lack modern analogies, but appear more elaborate than would be expected of a purely pastoral people. More positive indications of agricultural
activities are provided by grindstones and rubbing stones recovered in the excavations, whilst the stone house which I excavated at Tambach was probably connected with some rough stone terrace walls, such as the Elgeyo still construct in preparing their fields on the escarpment. In general, moreover, the western highland region is conducive to a mixed agricultural and pastoral economy, and most of the Kalenjin rely more on their crops than their cattle, even though the latter may command greater emotional importance and prestige. Noble's statement that 'the later inhabitants of the region were pastoralists' is very misleading.

In many areas, and particularly Tambach, the grazing is insufficient to allow more than a few head of cattle to be kept near the home, and the same must have been true in the past. Hence, as is locally remembered, a grassland tract like the Uasin Gishu plateau was of prime importance as external grazing, and the Sirikwa holes there may represent the seasonal kraals to which the young men resorted with their families' herds.

The making of Sirikwa holes seems to have spanned a long period, perhaps beginning early in the Iron Age. This is not of great dating value, for the coming of iron to this region may have occurred several centuries either side of A.D. 1000 and was probably a very gradual process, as witnessed by lithic waste that occurs in Sirikwa holes, notably those excavated at Site II ('the north-east village') at Hyrax Hill by Nakuru. Radio carbon dates have been obtained from a Sirikwa hole site (not actually a Sirikwa hole) at Lanet, a couple of miles from Hyrax Hill, and from the floors of houses adjoining two juxtaposed stone-lined Sirikwa holes at Muringa, overlooking the Moiben valley on the northern side of the Uasin Gishu plateau. The Lanet date is A.D. 1380 ± 100 (Y-570); those from Muringa are A.D. 1650 ± 80 (Y-1385) and A.D. 1690 ± 60 (Y-1386). Whatever their accuracy, these dates merely fall within uncertain limits. The upper limit, however, cannot be less than 100 years ago, for enquirers at the beginning of the present century were unable to obtain eye-witness accounts of their construction and use. On traditional grounds a date for the end of Sirikwa holes around A.D. 1800 seems most feasible.

It is not just a question of when, but also of why, and is interwoven with the problem of who made the Sirikwa holes. The Kalenjin, on the evidence of linguistics and of their admittedly rather meagre oral traditions, are no newcomers to the larger part of their present territory, and there is little room either on the map or on the time chart to separate Sirikwa tribe, whether belonging to the Kalenjin group or not. Therefore the most reasonable explanation of these widespread remains is that they are the work of the various Kalenjin themselves; and that the name 'Sirikwa,' whatever its origin, is used by the later Kalenjin for their forefathers who made the Sirikwa holes. This is not inconsistent with the traditions and the archeological evidence. Pottery from Sirikwa holes (Kenyah highlands class C), especially some earthern forms, is best paralleled by indigenous Kalenjin wares. These include vessels both with and without handles; and Noble's 'sherd A' from Tambach is not necessarily atypical of the local Elgeyo work.

If this Kalenjin explanation of Sirikwa holes is correct, it seems that at some time the Kalenjin extended further east into and beyond the rift valley. This is further supported by Kipsigis traditions, by a few place names, and by the existence of scattered 'hordes' of forest hunters called Dorobo (or Okiek) speaking dialects of Kalenjin. The rift valley grasslands were probably lost to the Kalenjin about the seventeenth century when the Masai, a separate Nilo-Hamitic group, entered the Kenya highlands; and this displacement in the east is probably connected with the recorded movements of Kipsigis and a number of Nandi clans into their present territories. It may also have some bearing on the origin of the Tatoga, relatives of the Kalenjin who roam scattered grassland areas in northern and central Tanganyika. Better remembered than all this by the Kalenjin is their loss of the plateau grasslands in the midst of the western highlands to the Uasin Gishu section of the Masai at about the end of the eighteenth century. The clans claiming a Sirikwa origin now dispersed among the northern Kalenjin seem to represent the remnants of those who were driven from the plateau at this time.

Thus in the open grassland areas Masai expansion is the most feasible explanation of the end of Sirikwa holes; but it does not account for the abandonment of the practice in the more wooded and hilly country that Masai did not conquer but proceeded to raid, especially for cattle, on all sides of the Uasin Gishu plateau. Cattle-raiding in the nineteenth century, not only by Masai, but also by Karamojong from the plains to the north and by some of the Kalenjin as well, had, by accounts, become institutionalized, being undertaken on a large scale through a sophisticated military organization. If this had been the case in earlier centuries, it is difficult to see the point of Sirikwa holes, which may have provided reasonable protection from individual stock-thieves or small bands of rustlers hoping to succeed undetected; but to an army whose intention was to round up alive as many head of cattle as it could handle, Sirikwa holes would surely have been traps of easy loot. It is tentatively suggested therefore that the Masai introduced into the Kenya highlands large-scale raiding with new military organization and tactics, and that their overrunning of the Uasin Gishu plateau one and a half centuries ago demonstrated to the Kalenjin on all sides the futility of trying to safeguard their cattle in Sirikwa holes. Instead the Kalenjin were obliged to retreat into areas better protected behind the forests and the hills, and to rely on more coordinated and mobile methods of defence, with long-distance lookout and a relay system of alarms, so that cattle could be rushed to the cover of the forests in good time—that the Nandi responded to the British 'punitive expeditions' at the turn of the present century.

One of these better protected areas was the Elgeyo escarpment—which brings us back to the original problem of the stone houses at Tambach. It is not possible to say when they were first made, whether before the Masai threat from the plateau above or later. What is certain is that some of them were occupied by men and livestock till very recently, a hundred years or more after Sirikwa holes became outmoded. Presumably the Elgeyo, protected by the forest belt and the escarpment cliffs at Tambach, felt reasonably secure from being caught unawares with their stock by a large body of raiders. It remains unclear whether the latest occupants built their own stone revetments or took advantage of abandoned sites for useful foundations. But this point is now much less important, if it is accepted that the distinction between vanished Sirikwa master builders and latter-day Elgeyo squatters is false and meaningless.

Notes

1 A full report is to be published in due course.
5 By Susannah Chapman (publication forthcoming in Azania).
Traditional Yoruba Healers in Nigeria. By Edward L. Margetts, M.D., Professor of Psychiatry and Lecturer in the History of Medicine, University of British Columbia, Vancouver, Specialist Psychiatrist, Mathari Hospital, Kenya (1955–9). With three figures

The study of ‘native’, ‘traditional’ or ‘folk’ healers (medicine-men, sometimes confusingly called ‘witchdoctor’) has always been a subject of interest to medical doctors, anthropologists, missionaries and others who work in out-of-the-way places. In recent years, the countries of Africa particularly have been in the news, and there are many articles and books relating to African studies, including primitive African medicine and psychiatry.

A number of authors have suggested that native African medicine-men could be utilized as effective agents in mental health treatment programmes. In fact, they have been employed in such a way in Nigeria. I have had experience in East Africa and certainly would never align myself with such a policy. I have emphasized that native healers can do little good in a mental health programme and may do harm (Margetts, 1962). They can have no role in the modern Western world, and as the educational level of African natives improves and as time affords them cultural wisdom, it is expected that the people themselves will drift away from the superstitious beliefs and seek help in science. If African countries are to develop, to emerge (from primitiveness) the people must strive for equality in science and understanding with more enlightened countries. This is not to say that native African medical practitioners should be persecuted by law but the people need not be encouraged to seek their advice and ministrations, and they should not be countenanced with favour by health and legal authorities.

However, native healers exist in all countries of Africa, and they are appropriate and very interesting subjects for study. The magic and psychic techniques used by native African medicine-men are based on the classical theories and techniques of magic, and there is an abundant literature on the subject. In addition to the observation and description of native medical practice, investigators should seek to analyse the botanical and pharmacological properties of folk medicines used by practitioners and sold in the jujua stalls of the market places.

African medicine-men are much the same throughout Africa in that they usually employ a combination of ‘oral’ and ‘manual’ rites and of ‘accessory observances’ if one may extend and apply these terms advanced by Thomas (1911) for describing magic in general and by Gardiner (1917) for describing ancient Egyptian magic. ‘Oral’ rites would include the use of psychological manoeuvres such as hypnotism, positive suggestion, deception, curses, invocations, threats, intimidation and other fear-inducing methods. ‘Manual’ rites would include the use of physical contact such as the laying-on of hands, massage, physical violence, restraint, curses, and the use of material intermediary apparatus, such as magical images, symbols, sympathetic ‘medicines’, sacrifices, sharp objects, knots, magical barriers like circles, fences and repellant amulets, and herbal, animal and mineral substances. ‘Accessory observances’ would include time, place, manner of preparation for and conclusion of the magical act, etc. All combinations of ‘oral’ and ‘manual’ rites and of ‘accessory observances’ are discernible in African tribal magical medical practices. In healing procedures, these rites and observances are chosen as indicated in accordance with the practitioners’ beliefs and mental associations about causation of illness.

In general, for practical purposes, primitive African magical practitioners or ‘ritual experts’ (term proposed by G. W. B. Huntingford, verbal communication, 1965) may be classified as follows:

A. Practitioners of good intent (‘white’ magicians)
1. Medicinemen or healers or folk doctors (an archaic term for healer still used sometimes is ‘leech’).
   (a) Surgeons (who use cutting procedures and include midwives, obstetricians, genital operators—circumcisers, excisors and inflabulators, tribal-mark scarifiers, trepanners).
   (b) Physicians (who do not use cutting procedures and include herbalists, excisors, hypnotists, religious healers).
2. Other magicians: diviners, seers, soothsayers, mystics, mediums, priests, prophets, oracles, exorcists, dance masters, finders or ‘smellers-out’ of evil magicians, necromancers, rainmakers and other weather-controllers, blacksmiths, garden and hunt magicians, amulet-makers.

B. Practitioners of evil intent (‘black’ magicians)
1. Witches and wizards or sorcerers
2. Vampires

There is considerable overlap between ‘good’ and ‘evil’ intent (‘white’ and ‘black’ magic) as every field Africanist knows! However, this division has much to commend it because it indicates motive. The use of the terms *supra* vary from author to author and they may be very confusing unless specifically associated with the context. Anthropologists will sometimes differentiate witches (witch female, wizard male) from sorcerers, reserving the former term for one who practises evil without the help of material substances and appliances and the latter for one who uses them. The terms are all archaic and of European, not African origin. Moreover, they have changed their meanings over the centuries. There does not seem much practical usefulness in being too concerned about nomenclature. It is the motive (good or evil) of the practitioner which is important, not the label.

A further confusion about ‘white’ and ‘black’ magic and medicine is that, amongst the native healers in Nigeria at the present time, ‘white’ is the term used for European ‘medicine’ and ‘black’ for African, inferring a colour and racial difference!

In November, 1964, I visited the area around Akure, an urban centre in Western Nigeria. Eight Yoruba ‘African doctors’ were located. This was not difficult through local informants, and moreover, several of the healers indicated their presence by the use of signs which gave their names and in some cases their ‘registration numbers’ according to law (as purveyors of herbal medicines). Seven were pleasant and co-operative. They were interviewed and photographed. Most of them expected and received some ‘dash’ (recompense) and amounts of 20/- or 5/- were handed out depending on seniority and value of information given. The eighth healer was hostile and unco-operative, largely (he said) because of a previous unsatisfactory relationship with a research team interested also in native medicine, which had made plans and promises to him for his material benefit which had not materialized!

In Yorubaland, religion, divination, medicine, all ‘white magic,’ are mixed up, and often with a component of ‘black
magic' and of deception. The accounts and terminology are confused in the literature (useful basic references are Bascom, 1941–3, Clarke, 1939, Dennett, 1906–10, Farrow, 1926, Frobenius, 1913, Idowu, 1962, Lucas, 1948, Maupoil, 1943, Parrinder, 1951–61, Prince, 1960–64). Further research is required to clarify the distinction between priests, diviners and healers. Moreover, there are no detailed accounts in the literature of African 'quacks' or fake healers (Beier, 1960, Delano, 1937) and how (or if) they are very much different from the traditional healers. The term dokita (from the English, doctor) is used in Nigeria for native doctor and in these days of 'acclimatization' is apt to infer 'quack doctor.' The word Dokita was chosen as the title of the medical students' journal at Ibadan! The traditional tribal Yoruba healers use plenty of consciously motivated deception and dishonesty in their practice. The term babalawo is much used as a name for Yoruba native healer, but inaccurately, since the true babalawo (father or master of secret or mystery) is specifically a traditional priest of Ifa, a diviner. He is but secondarily a physician. Adahunse (person who caused something to be made—Dr. Raymond Prince helped me out with the translation of this word) and onisegun (maker of medicine) are more specific terms for healer or 'physician' (Abraham, 1958, Farrow, 1926, Lucas, 1948). They are not priests.

Yoruba healers usually had learned their craft from their fathers. This is different from some other tribes elsewhere in Africa where training is by payment and apprenticeship to a non-relative. Most Yoruba healers are male; this was explained on the basis that the healing craft is unpractical for women because of taboos relating to menstruation. This is not the case elsewhere in Africa, e.g. in Kenya many Kamba healers are women (my field work, unpublished).

All the Yoruba medicinemen used plant, animal and mineral substances in their therapeutic procedures, which, in addition to any pharmacological action, usually had a magical and suggestive function. Most of the 'remedies' were undoubtedly hit-and-miss and had no useful pharmacological action whatsoever—but see Prince, 1960–4, and his movie Were Ni (translated as 'He is a madman'; the film is available from the University of Ibadan), on the Yoruba healer's use of Rauwolfia as a soporific. All the healers treated mental illness except the one who was uncooperative. He said madness was not treatable and that any

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**Fig. 1. African Native Doctor with His Collection of Shackles**

Photograph: E. L. Margetts, 1964

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Nigerian healer who said he could cure mental illness was a liar!
One healer was particularly interesting, because he specialized in the
treatment of madness, epilepsy and leprosy. A brief account of
this man is considered to be informative and is the primary object
of this essay. He was about 45 years of age. He ran a private
‘asylum’ in Akure, which, at the time of visiting, contained two
patients, one an excited schizophrenic man and the other a manic
woman in remission. The healer claimed it usually took about three
months for the treatment of a patient. It is important to note that
many acute psychotics will ‘recover’ spontaneously, without
treatment, within this period of time, particularly if deprivation
methods like isolation, restraint and threats are used. The man’s
‘asylum’ was full of refuse and chicken feaces, unbelievably filthy
and unhygienic, a state of affairs not unusual in Nigerian slums.
He utilized most of the techniques noted above but he used
particularly the practices of isolation, restraint, threats and (he
claimed) soporific drugs (not identified). In addition to the two
locked rooms in which the patients were isolated, there was in
the house a room which contained restraints and bottles of medi-
cines and was generally used as a ‘consulting room,’ and yet
another room which contained in one corner a collection of
magical images, offering bowls, and other magic paraphernalia
usually referred to in Nigeria as ‘juju.’

This healer was notable because he made extensive use of
physical restraint by isolation in a locked room and by ankle
shackles of iron. Methods of physical restraint by the natives of
Africa vary from place to place, and include ropes, chains, iron
wristlets and anklets, log stocks, stretchers, iron, stone and cement
weights and locked doors. In the first photograph of this medicine-
man (fig. 1), he is shown posing with his collection of shackles—
ote four pairs of leg irons, a chain, padlocks and keys, and wire.
Some of these we ‘doctorized,’ i.e. treated with ‘medicine’—
usually consisting of or including blood and feathers from a
sacrificed chicken which had been used in magical ritual incanta-
tions. On the shackle at the left, the feathers are visible. The
shackle illustrated is the one on the right, and was purchased for
15 shillings. Above the shackles may be seen two ‘diplomas’
with photographs, the man posing with his whisk and amulet in
the same position as in this photograph. An amulet hangs on a
nail below the certificate on the right.

Fig. 2 shows the healer ‘treating’ the schizophrenic patient with
an amulet consisting of a horn filled with magic medicine and
with incantations and a generous and ostentatious flourishing of a
flywhisk. Aggressive action and threats were part of the ‘treat-
ment.’ The patient is shackled with the set of leg irons later
purchased. Fig. 3 is a close-up of these leg irons.

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**SHORTER NOTES**

**Man: The Journal of the Royal Anthropological Institute, 1966**

103 The present sixty-fifth volume of *Man: A Record of Anthropological Science*—as also the current volume of the *Journal of the Royal Anthropological Institute*—will be the last to be published in the present form. From the beginning of 1966 these two publications will be combined—as to both title and functions—in *Man: The Journal of the Royal Anthropological Institute*, which will be published quarterly (at the end of March, June, September and December). This decision has been reached by the Council of the Institute as the culmination of a thorough and protracted re-examination of the Institute's publication policy, which has in part been carried on in open meetings of the Institute, and it is intended to make possible a still more active development of the R.A.I. publications. The combined journal will publish at least as much material as the total amount published in the two existing periodicals, while the economies effected by rationalization—together with the increase in the combined circulations—will make possible for the first time the employment of staff to carry out much of the routine work and correspondence which have hitherto formed a heavy part of the duties of the Honorary Editors. The new journal will be produced by the present printers of *Man*.

The present Honorary Editors of *Man* and of the *Journal* will both retire at the end of this year, and the Council has appointed Dr. Adrian C. Mayer as Honorary Editor of the new journal, with Professor John Evans as deputy editor and Dr. Burton Benedict as reviews editor. A fuller announcement has been issued, covering also the changes in the Fellowship subscription of the Institute.

**Settlement of Action for Damages for Libel. Cf. Man, 1962, 110** The following account of proceedings in the Queen's Bench Division of the High Court of Justice is reprinted by permission from *The Times Law Report*, 15 April, 1965:

GAYRE OF GAYRE V. ROYAL ANTHROPOLOGICAL INSTITUTE AND ANOTHER

BEFORE MR. JUSTICE PAUL

The settlement of this action by Lieutenant-Colonel Robert Gayre of Gayre, of Darnaway Street, Edinburgh, and Professor Henry E. Garrett, of Winstown Road, Charlottesville, Virginia, United States of America, respectively editor and associate editor of the anthropological review *Mankind Quarterly*, against the Royal Anthropological Institute, publishers, and Mr. W. B. Fagg, honorary editor, of Bedford Square, W.C.1, of the anthropological review *Man*, for damages for libel contained in a letter headed 'The Mankind Quarterly' and published in *Man* for November, 1960.

Mr. Richard Hartley appeared for the plaintiffs; Mr. Peter Bristow, Q.C., for the defendants.

Mr. Hartley said that the third defendant in the action, Professor Bozo Skerlj—of Lubljana—died in November, 1961.

When the plaintiffs' magazine *Mankind Quarterly* was founded, Professor Skerlj agreed to become a member of the honorary advisory board, and his name was included in the list of the members of that board which was published in the first issue of *Mankind Quarterly*, in July, 1960. Unfortunately, on reading that first issue, Professor Skerlj realized that his opinions and some of the views expressed in the magazine were incompatible; he felt that he could no longer continue as a member of the honorary advisory board, and, accordingly, wrote to the plaintiffs asking that his name should be withdrawn from that board, and that his letter giving his reasons for taking this step should be published in the next issue of *Mankind Quarterly*. The plaintiffs added to his first request but decided not to publish his letter.

Professor Skerlj then wrote the letter, of which the plaintiffs complained, which was published in the issue of *Man* for November, 1960. This letter was severely critical of the plaintiffs, and of their opinions, and of their behaviour towards the professor himself.

He (counsel) had to say, at this juncture, that there was a considerable divergence of opinion in anthropological circles over matters concerning race and environment. Without attempting to go into the rights and wrongs of the matter it would, he (counsel) thought, be reasonable to say that the plaintiffs adopted one view, while the defendants favoured another.

The plaintiffs, of course, recognized how important it was that scientists should be able to agree and to air their opinions in public, and, in so far as Professor Skerlj's letter showed an honest disagreement with their views, the plaintiffs, of course, had no complaints. What they did complain of, however, was the suggestion, which they considered was made in the letter, that contributors to *Mankind Quarterly* had Nazi sympathies and expressed Nazi views. The plaintiffs felt that this was a libellous and exceedingly damaging assertion which they could not allow to pass unchallenged.

The plaintiffs, accordingly, issued a writ; but he (counsel) was now happy to tell his Lordship that the parties had, very sensibly, come to terms in this matter. The defendants, as they were entitled to do, disagreed fundamentally with the plaintiffs' views on the anthropological matters in issue, on which their views accorded with those of Professor Skerlj. While the defendants considered they had a duty to publish the professor's letter, they wished to make it clear that, if Professor Skerlj's letter bore the meaning that the plaintiffs had Nazi sympathies or views, they did not associate themselves with any such accusation, and regretted that it should have been made.

The defendants had paid an appropriate sum by way of damages to mark their regret, and had also agreed to pay the plaintiffs' costs. In those circumstances the plaintiffs were content to withdraw their action, which was brought with the intention of clearing their names.

Mr. Bristow said that he wished to associate himself with all that Mr. Hartley had said. If anyone had read into the professor's letter any accusation of Nazi sympathy or view it would be a matter of regret to the defendants and they felt it would be wholly unjustified.

The record was, by leave, withdrawn.

Human Fossil Remains Recently Discovered in Shensi Province, China. By Dr. F. P. Liu, Department of Anatomy, University of Birmingham

In August, 1964, I had the privilege of visiting the Institute of Vertebrate Palaeontology and Palaeoanthropology of the Academia Sinica in Peking as a result of an invitation from the Chinese Anatomy Society. Professor Tung Kung kindly showed me the rich collection of primate fossils discovered in China and in particular the fossil mandible of Sinanthropus type which had been discovered on November 19, 1963, at Chenchiaow (Chen - Chia - ou) in the district of Lantian some 45 kilometres south-east of Sian, the capital of Shensi province.

The mandible (for details see Woo, 1964a, b, c) was found in the basal parts of a layer of reddish clays which is some 30 metres thick and whose geological age is considered to be Middle Pleistocene. The specimen is well preserved except for small parts of the rami and belongs to a female of advanced age. According to the description of Professor Woo the Lantian find is closely related to Sinanthropus pekinensis, though there are certain differences. The former has a smaller angle of inclination, shows a greater distance between the height of the symphysial part and that at the level of the mental foramen, and has a larger angle of the molar rows than any specimen of Sinanthropus pekinensis so far found. Certain features, such as the prominence lateralis and torus mandibularis of the body and the crista ectocondyloidea and torus triangularis of the rami, are less marked. The teeth are intermediate in size, lying between those of the male and female specimens of Sinanthropus pekinensis. Owing to these differences Professor Woo gave new the provisional name of Sinanthropus Lantianensis. In addition radiological examination of the specimen revealed a congenital absence of the basal molars. An ante mortem loss of the first premolar tooth on the right side and a fracture of the mandible are the only signs of pathology.

Chang et al. (1964) have already produced a preliminary report on the Cenozoic formation and stratigraphy of the Lantian site. According to Dai and Chi (1964) more than a hundred palaeolithic made of quartz were found in sandy clay of the Late Pleistocene of this region. These consist of cores, flakes and tools, whose edges were trimmed unilaterally. Among the latter chopping and scraping tools as well as points are recognized. One quartz pebble showing traces of artificial chipping was found 1,000 metres from the Sinanthropus lantianensis mandible.

Many more fossils were associated with the latter find (Chow, 1964b), of which the forest-living types predominate. These occurred in the lowest part of the reddish clays, overlying a layer of basalt, which in turn rested on a series of Early Tertiary, probably Late Eocene, sandstone and clays. The association was so close that the fossil mandible was originally 'firmly adherent to and underneath a skull of Felis tigris.' The mammalian remains found, besides that of Lantian Man, were: tigers (Felis s. tigris), sika deer (Pseudaxis gray), elephants (Elephantidae), red dog (Canis cf. alpinus), boar (Sus cf. oyekeri) and Myospalax (= Spinnaeus) fontaniere. This assemblage, according to Chow, indicates a Middle Pleistocene or Choukoutienian fauna, since 'not a single species typical of the older Niowhan or the younger Loessic (Ting - sien of Sjarja - osso- goli) is present.' Though the fossils were found near the base of the reddish clays, it cannot yet be proven whether the Lantian fauna might be a little earlier in age than that of Choukoutien Loc. 1 (Chow).

Chow (1964b) also reported a single maxillary fragment of a new lemuroid primate which was found in the Early Tertiary sandstones and clays underlying the reddish clays of Chenchiaow. The horizon seems to belong to the Late Eocene. The specimen is well preserved and contains the second premolar and three molar teeth. Chow believes that it probably belongs to the Family Prosimiae and has named it Lantianus sinensis gen. et sp. nov. (these names refer to the two geographical names Lantian and Xiuchu). He considers it is still on an Eocene level of molar evolution, though somewhat more advanced than the better-known European forms of lemuroids. The find only shows remote resemblance to two other known prosimians in China, Hoanghongus (reported on by Woo and Chow, 1957) and Lushius, which are more tarsoid in character.

Later, after my return from China, Professor Woo (1964d) informed me that a further important discovery of a complete skull cap of Sinanthropus lantianensis had been made. The news of this was released at the Lantian Cenozoic Field Conference held from 3 to 8 November, 1964, in Sian. This specimen had been found in sediments in the open near Gongwangling (Kungwangling) in Lantian county at some distance from the Lantian mandible. Already in the previous May a pithecanthropine tooth had been discovered here. Whole blocks containing fossil material were removed to Peking, and on October 12 part of a skull was revealed. Preliminary work (Woo, 1964a and b) shows that the find consists of the frontal, parts of the orbits and nasal bones, both parietals, the right temporal and large portions of the maxilla with three teeth in situ. The semicircular canals and the cochlea of the right petrous part of the temporal bone can be clearly seen on roentgenograms. The strong continuous brow ridges, the low vault and the thick cranial wall are characteristic. According to Professor Woo the morphological features suggest that this discovery may be the most primitive Pithecantropus so far found.

Many extinct mammalian fossils were found with the skull cap, such as the giant deer (Megaloceros), bison, stegodont elephant, sabre-toothed tiger (Machairodus), Saamen horse (Equus saamiensis), ancient small bear (Ursus erucus) and tapir. From these and from a study of the stratigraphical sequence of the site it is considered that the find belongs to the Early Middle Pleistocene Period. And this, says Professor Woo, makes the specimen 100,000 to 200,000 years older than Sinanthropus pekinensis of Choukoutien. It is felt that, since the skull cap was not found in a cave, more Sinanthropus fossils may be discovered in wide areas in north China.

Chang (1962) and Yin et al. (1965) have already described a new assemblage of early hominid and hominid fossils from Choukoutien. The find has been made in the late Pleistocene in north China. The assemblage is dated by stratigraphical evidence. The find is of great importance. It is hoped that the assemblage will be described in an article by the end of the year.
A Note on a Recently Introduced Card System for Processing Numerical Data. By Dr. G. Kingston Garbett, University College of Rhodesia and Nyasaland.

During and after a recent field expedition I made use of a card system for systematically recording and analysing numerical data. The cards used in the system are known as Brisch-Vistem cards.1 They have only been in use in Britain since 1959 and may be unfamiliar to readers. B-V cards have a wide range of applications in commerce and industry as well as in research. They can be used to present time series and for the rapid retrieval of information from files stored in such general principles of the system and other applications of its are discussed in two articles by J. L. Jolley.2 To which further reference should be made. B-V cards are ideally suited for anthropological and sociological field work since they are simple to use, portable and cheap. In addition, not only can simple tabulations of numerical data be obtained, but also elaborate cross-tabulation and correlations may be made, using many variables. In operation I found the system efficient and possessing a number of advantages over other electro-mechanical and manual card systems with which readers may be already familiar.

Most cardsystems in common use depend upon each unit of enquiry — individual, family, household, etc. — being allocated one card. The attributes of the unit of enquiry are then punched onto the card, according to a predetermined code, either in columns or around the periphery, depending upon the system. The information punched onto the cards is then abstracted, counted and correlated either electro-mechanically or manually according to the system used. B-V cards, however, operate on a different principle. Each B-V card (or combination of cards) represents an attribute or characteristic which the unit of enquiry may possess. The cards have a series of numbered positions printed on them. Each unit of enquiry is allocated one of these positions which it occupies on all other cards. Information is then recorded by punching a hole in the allocated position through all the 'affect' cards. B-V cards thus form a particular unit of enquiry. When this exercise has been completed for all units of enquiry it is possible to take cards, align them together and obtain distributions of attributes, shown by the 'through' holes, which particular sets of units of enquiry possess.

For example, suppose we were to take individuals as our units and wanted to obtain distributions of men and women who were married and unmarried. For this simple exercise three cards would be punched: one for 'male,' one for 'female' and the other for 'married.' By placing the 'male' card on top of the 'married' card holes punched through both cards would be aligned and thus show the number of all married males. Holes which only appeared in the 'male' card but which did not appear on the 'married' card would show the unmarried males. If the 'male' card were now replaced by the 'female' card the numbers of married and unmarried women could be similarly obtained.

There are a number of sizes of B-V cards. The size which, I think, will be found most convenient and practical for field workers, is a card which is 11 inches long and 6 inches high, divided into 1,000 positions in blocks of 100, each hundred being arranged in columns of ten. Larger cards have 2,500 positions and 10,000 positions. These larger cards have wide applications in the field of information retrieval but for 'counting' purposes would probably prove uneconomical in terms of time and accuracy.

The only equipment required to operate the system is a set of cards, some means of filing them, an aligning tray, a small hand punch and, possibly, two or three coloured transparencies the same size as the cards. An efficient means of filing the cards is important for without it the cards cannot be used effectively.

To illustrate how the system is used I discuss the procedures necessary to code, punch up and then analyse simple census data of the kind that most social anthropologists collect. Let us suppose that

information on sex, marital status and age has been collected. The first step in utilizing the B-V cards is to create a code or key. We might let card 1 be 'male,' card 2 be 'female,' card 3 be 'single,' card 4 be 'married,' card 5 be 'widowed' and card 6 be 'divorced.' These cards are then numbered, titled and placed in the filing tray in an array. Let us assume, for the moment, that we have used ten-year age categories. We shall need, say, 8 cards to represent ten-year categories to age 80. These cards would then be numbered, continuing the series, from 7 to 14 titled and added to the array.

B-V cards are notched along their lower edges so that the notches correspond with a series of metal rods in the filing tray. It is a simple matter, therefore, to remove and replace cards in their allocated positions. The cards also overlap in the array, except for a margin of 4-inch on which numbers, titles and other information may be written. 'Out' titles spaced along the top of a card enable one to see quickly when preceding cards have been removed from the file. A large number of cards can be filed in rows in a relatively small space.

Once the array has been decided upon the next exercise is to draw up a form or code sheet on which the data for each individual are recorded. This done, each individual is allocated a code number which relates to one of the numbered positions on the B-V cards. The information for each individual is then recorded in the code on the code sheet. The following might be an example: Jones is a man, aged 25 and married. His code sheet, according to the simple code outlined above, would appear like this:

<table>
<thead>
<tr>
<th>Name</th>
<th>B-V Code No.: 219</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sex</td>
<td>M</td>
</tr>
<tr>
<td>Marital Status</td>
<td>Ma</td>
</tr>
<tr>
<td>Age</td>
<td>25</td>
</tr>
</tbody>
</table>

This procedure is then completed for each individual. Once this has been done punching can begin.

A code sheet is taken and the cards appropriate to it drawn from the array. In the case of 'Jones,' cards 1, 4 and 9 would be drawn from the array. The cards are then placed together in the aligning tray and punched through in the appropriate position. In the case of 'Jones' this would be the 219th position in the third block of a hundred positions. Note that each card is not punched separately. All cards appropriate to an individual are punched in one operation: a great saving in time besides making the alignment of holes a less critical operation. When the cards for an individual have been punched they are returned to their appropriate places in the file array, the code sheet being re-read at the same time to provide a check. These operations are then repeated for each code sheet. (The procedures are analogous to the coding, punching and verifying of I.C.T. cards.)

When all the information on each code sheet has been transferred to the B-V cards, analysis of the data can begin. Suppose we wished to obtain a distribution of sex and marital status against age. For the simple example given this would be done by placing the 'male' card in the aligning tray, covering it by the 'single' card, then taking one of the 'age' cards and placing it on top of the two others. The through holes would then give the number of single men at a certain age. The through holes are then counted. The first 'age' card is then replaced in the array and the second taken, placed over the other two cards, and the 'through' holes again counted. When this has been completed for all 'age' cards the 'single' card is replaced by the 'married' card and the same procedure repeated with the 'age' cards. In this way the distributions are obtained. In practice I found it useful to place a yellow transparency at the bottom of the aligning tray so that the through holes showed clearly by reflected light. It is not necessary to hold cards up to the light to examine 'through' holes.

In the simple example I assumed that age was presented in ten-year age categories. Suppose, however, that one wanted to represent exact ages to age 80. If each card were to represent one characteristic then 80 cards would be needed. However, it is possible to use cards in combinations. This reduces the number of cards required enormously. Using the general formula for combinations, it can be determined that by using combinations of three cards from nine, 84 separate combinations can be obtained. In the case of the example, each combination could be used to represent a particular age.
When devising a code for card combinations I found it useful to allocate letter symbols to cards and then work out the total combination before allocating the numbers which they would have in the filed array of cards. For example, given the cards A, B, C ... I, then let cards A, B, C, in combination represent age 1; cards A, B, D, age 2; A, B, E, age 3; A, B, F, age 4 ... and so on. This technique can be used whenever one wants to break down one category into a large number of sub-categories. Combinations of two cards from a given number are simplest to handle, though the number of cards taken in combination will depend upon the number of sub-categories. Combinations of two cards from nine, for example, could be used to represent 36 sub-categories; two cards from six, 15 sub-categories. It must be borne in mind when arranging a code for combinations of cards to represent an attribute and its sub-divisions that any surplus combinations cannot be used to represent other attributes. In the simple example given above the four surplus combinations for the age categories could not be used to represent any other attributes. This is no great drawback if the general formula for combinations is first applied to see what the range of possibilities is, and then the combinations appropriate to the required number of sub-divisions selected.

There are two other methods of reducing the number of cards required. The first is by the use of coloured transparencies and the second by the use of masking cards. Transparencies are supplied in primary colours. I found that yellow and green transparencies, which in combination give blue, could be used simply and effectively.

Suppose in our simple example we had not allocated a card to represent 'unmarried.' To obtain the number of unmarried males the following procedure could be used. A yellow transparency is first placed in the bottom of the aligning tray and on top of it the card representing 'males.' The green transparency is then placed on top of this card and on top of it the 'married' card. Two sets of holes would now become visible, 'through' holes appearing blue which would give the number of married males and green holes which would give the number of unmarried males. It is possible, therefore, to reduce the number of cards by the use of transparencies but, even more importantly, it is possible to compare units of enquiry which have an attribute with those which do not have it. The two comparisons can be made simultaneously without the cards having to be rearranged in any way.

Masking cards are useful in certain circumstances. Suppose that data from a census had been arranged on the cards by village and that there were 20 villages. By cutting our alternate segments from two cards so that the 'through' holes from ten villages showed in alternate blocks through one mask and the remaining ten in a similar way through the other, separate population counts could be made for each village quite simply without using cards in combinations. Masking cards, however, have only limited applications since they only screen out particular blocks of 'through' holes.

I have outlined only simple applications of B-V cards. In fact, complex cross-tabulations and correlations can be achieved. The system has great effectiveness when used for a relatively small number of cases where highly detailed information is available on each case: a situation which often confronts social anthropologists who use intensive rather than extensive research methods. As an example, take the following hypothetical problem. What proportion of men, aged 30, married with three children, owning two acres of land, with an income of £20 per month, are village headmen? All that is required to obtain this information is to draw seven cards from the filed array: 'Male,' 'Married,' '3 children,' 'Age 30,' '2 acres of land,' 'Income £20' and 'Village Headman.' A yellow transparency is then placed at the bottom of the aligning tray and covered by the 'Village Headman' card. A green transparency is added and on top of this the 'Male' card is placed. The remaining cards are added in any order. Two sets of 'through' holes will now appear: blue representing village headmen and green representing non-headmen. The proportion of village headmen as compared with non-headmen having the attributes under examination, can be found immediately. If other age, land-holding, or income and family-size cards were substituted systematically, other distributions could be quickly obtained. If this method is contrasted with the complex and sometimes, costly procedures involving many operations required by other systems, the advantages of the B-V card system are readily apparent. The disadvantages are few for census and survey work where the number of cases is under 1000. Above this number electro-mechanical systems possess advantages in speed and accuracy of counting and computation, though, unless the number of cases is very large, they may still prove expensive and, in any case, require access to special equipment.

In the case of my own field data I examined 35 attributes with a total of 149 sub-divisions of 140 individuals. This information was accommodated on 112 cards. The cost was a little over £3, excluding the filing tray, for the cards and ancillary equipment. The cards were coded and punched in about ten days. I have since added information on two other attributes to the array simply by punching appropriate holes in two additional cards. All this information is available at any time for quick and easy reference.

Because of its simplicity, cheapness, portability and flexibility, together with its ability to aid in solving quite complex problems, the B-V card system appears to me to be one which should find a wide application in socio-anthropological and sociological research.

Notes

1 The cards, together with ancillary equipment, are supplied by Carter-Parratt, Ltd., Visible Systems, London.


3 The number of combinations or selections of n unlike things taken r at a time is

\[ \binom{n}{r} = \frac{n!}{(n-r)!r!} \]

Ethnological Material in Early Organological Texts.

By Jeremy Montagu, London

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A number of important organological works have been reprinted in recent years, mostly in facsimile. These are standard works for the study of the history of orchestral instruments, but may not be so well known to the anthropologist with an interest in ethnographical instruments.

The knowledge of the authors of the sixteenth and seventeenth centuries was very variable, as was the skill of their engravers. The engravers often worked at second hand or worse and sometimes seem to have made things up as they went along. The writers are often quite accurate, but occasionally they misplace instruments by a continent or two. Praetorius, for example, believed that for an instrument to be really foreign it must come from America. India was accepted at a pinch, but Africa is nowhere mentioned although many of the instruments illustrated are African. Bonami’s engraver is unreliable in detail although the earlier book from which he copied is often recognizable. The user of these texts is warned that he should apply his knowledge of the instruments concerned, otherwise he may be easily misled. This warning applies equally to works compiled in this century, so that it does not reflect on the value of the early texts, properly used.

The great value of these works is the light that they throw on the contemporary knowledge of and interest in the music and instruments of other peoples. The works mentioned here are only those that are currently available in the shops (with two exceptions). Out-of-print rarities such as Hipkins and Gibb’s beautiful collection are not discussed. In each case, the publisher and date in parentheses indicates a modern reprint, in most cases by facsimile. A brief description of the contents is given. By folk instruments, those instruments used by Europeans but not in orchestral music are meant; this includes such instruments as bagpipes, jews’ harps and so on.

(i) Michael Praetorius, Syntagma Musicum, Vol. II, De Organographia, Wolfenbüttel, 1619, and Theatrum Instrumentorum, 1620 (in one volume, Bärenreiter, 1958). Possibly the most important of all sources of music and instruments of the early 17th century. All
drawings are to scale (beware, the foot is the Brunswick foot which
equals 11.25 modern inches. A conversion table to millimetres to
two decimal points will be found in Bessaraboff's great catalogue of
the Boston Collection, Ancient European Musical Instruments, p. 356.
This is still available and is everything that a museum catalogue
should be and that no other has approached except, in a different
way, Mahillon in Brussels). There are many folk instruments and
a number of non-European, including Java (lifted from the same source
as Bonanni), India, Africa (an excellent double bell and an ivory
horn) and many of the entertaining fictitious instruments thought
up by the early monastic writers and fathered on the classics. The
text is in German; as far as I know it has never been translated into
English.
(2) Marin Mersenne, Harmonie Universelle, Paris, 1636 (Éditions
du Centre National de la Recherche Scientifique, 1963). This
facsimile is in three volumes and slightly reduced in format,
a considerable advantage over the original one-volume text.
The copy used was Mersenne's own, so that there are many marginal
and interleaved notes in his own illegible handwriting. The third
volume of the facsimile contains all the material on instruments.
The standard of drawing is higher than in Praetorius but there is no
scale; two artists seem to have been involved, one much better
than the other. The text is of a very high standard. Again there are
many folk instruments and a few non-European, notably the Siamese
Kam, the first depiction of a free-reed instrument in Europe, and
a North Indian Vina (attributed to China and alleged to be shown
as a flute as well as played as a string instrument). The text is in French
(with $u$ for $v$ which takes some getting used to). There is a Latin
translation of 1640 which has not been reprinted as far as I know and
a modern American translation (published in Holland) of the
instrument part only; the original French is to be preferred (the
whole work is only slightly more expensive than the American part;
the original is a lot more accurate than the translation).
(3) Filippo Bonanni, Cabinetto Armonico, Rome, 1723 (Dover and
Constable, 1964, as The Showcase of Musical Instruments). A fascinating
collection of engravings in reduced facsimile. The original
text has been abandoned and a new one written for this edition. A
few of the pictures appear to be made up from whole cloth but
while the majority are excellent, always provided that the reader knows
what he is looking at. Hands are often in the wrong places and sometimes
the scale of the instrument against the body of the player is wildly
wrong. However, there are more non-European instruments in this
collection than in any other until the twentieth century (until the
Sachs Realllexikon as the editors, Joan Rimmer and Frank Harrison,
point out) and since at least 100 of the 152 plates are of ethnographical
material, this is a most important reprint. Its low cost should help
to make it available to all interested in the history of our discipline.
(4) Johann Christoph Weigel, Musikalisches Theater, about 1720
(Bärenreiter, 1961). A high standard of illustration ('art work'
might be a better term in this case). Folk instruments only, except
for the frontispiece which includes two somewhat inaccurate
African instruments. No text except titles to the plates.
(5) Sebastian Virdung, Musica Getutscht, Basel, 1511 (Bärenreiter,
1931). This is now out of print, but is included because Bärenreiter
say they intend to reprint it and because it is the first work of its
kind. It includes folk instruments, but no non-European. German
text only.
(6) Martin Agricola, Musica Instrumentalis Deudsch, Wittenberg,
1528 (Breitkopf & Härtel, 1896). Again out of print, but again on
Bärenreiter's list. Also folk only. Text is mainly in bad German verse,
but much more detail than Virdung.

Modern Works

(7) Sachs, Realllexikon der Musikinstrumente. This needs no introduction,
but it may not be generally known that it is now back in
print (Georg Olms, 1962). Another edition is announced (Dover and
Constable) with Sachs's corrections and revisions. Constable cannot
say what form the revisions take; the mind boggles at the thought of
resetting the type of this work, particularly at the announced price.
(8) Helmholtz, On the Sensations of Tone, translated by Ellis
(Dover and Constable, 1954). The main value to us is in Ellis's appendices
which give descriptions and tunings of many ethnographical
instruments. Remember that they were pitched as Ellis and
Hipkins found them (Ellis was tone-deaf and Hipkins did all the
pitching) and not necessarily as they were played by their original
owners. Many of the instruments concerned are identifiable in
public collections; most of those that were in the Victoria & Albert
Museum are now in the Horniman.
(9) Welch, Six Lectures on the Recorder (first three lectures only,
O.U.P., 1961). This is included mainly out of annoyance. The other
three lectures are far more important to us and include many
details of ethnographical instruments. Whether Oxford intend to
take a second bite at this cherry, I do not know.
(10) Panum, Stringed Instruments of the Middle Ages (Reeves, n.d.,
but original edition still available). In spite of its title this is one of the
most valuable sources of archaeological material for all instruments,
covering ancient Greece, Egypt, etc. There is also some modern
good, mainly of the Scandinavian 'Bowed Harp' type.
This list is not intended as a British Museum reading list, but as a
collection of working tools that one ethnomusicologist finds
valuable. It is given in the hope that it may be of use to those who
may not know some of its contents because their training has been on
the ethno-rather than the 'musicologist' side of our trade.

CORRESPONDENCE

The Content of Kinship. Cf. MAN, 1964, 130, 217; 1965, 38

Sir,—I am troubled to learn from Dr. Beattie's letter
(MAN, 1965, 38) just how much misunderstanding there
really is between us. There is too much, I think, to deal
with adequately here. I will not, therefore, take the space to discuss
his statement, which I certainly could not accept, that We all know
perfectly well what kinship is as a biological given.' Neither will I
discuss his suggestion that biological kinship can somehow be taken
for granted and treated as if it were independent of social or cultural
kinship, as is implied in his statement which begins: 'And my point
was simply that a social anthropologist ... ' On this see my 'Kinship
and Biology' in A. J. Coale et al., Aspects of the Analysis of Family
Structure, Princeton U.P., 1965. There are other points which I must
pass at this time.

I will concern myself here with what I think is the central mis-
understanding between Beattie and myself.

What Beattie said in his first paper, and repeats in this letter, is that
kinship must be treated as a social and cultural relationship and not
as a biological one. He says: 'I do not, therefore, "see kinship as
empty of content," and I did not mean to say anything to suggest
that I did. On the contrary, my argument was precisely that it is full
of content; social content: that is why social anthropologists study
it.'

With this statement I am in wholehearted, complete and un-
qualified agreement.

With this firm foundation of agreement between us, let us
proceed one step further, to the difference between us. We will both,
now, treat kinship as a social and cultural relationship. But now I ask:
How can I distinguish this social and cultural relationship called
kinship from other social and cultural relationships? How shall I
know, when I go to a society never before studied, that two men are
kinsmen and not tradesmen, or that they are both kinsmen and
tradesmen?

Beattie and I both agree that there are different kinds of social and
cultural relationships. Some social relationships are called 'economic'
and we define this in a certain way, and this permits us to distinguish
it from other relationships—those defined as 'political' and those
defined as 'jural' and those defined as 'ritual' and so forth. Beattie
gives us a definition of 'economic'—relations that have to do with
the production and distribution of resources. He gives us a definition

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of 'political'—relations having to do with the maintenance of the territorial order.

But what he says about kinship—as a form of social and cultural relationship—is I think quite inadequate. It is inadequate because it says that kinship has no content and is not a kind of relationship commensurate with economic, political, ritual, etc., relationships.

The central point of my note (MAN, 1964, 217) was to raise the question which I believe shows the inadequacy of Beattie's formulation. The question is this: If kinship as a social and cultural relationship has no content and cannot be set beside others as commensurate with them, then is it all about? For Beattie now to write this letter advising me that kinship is just full of social content evades the issue completely. I know that it is full of social content. We agree that it is full of social content. But how does that kind of social content (kinship) differ from other kinds (economic, political) of social content?

Beattie's unreasonable concluding remark requires comment. Beattie says: 'If Professor Schneider is saying that relations between people who are called and called one another kin have, as well as their social and cultural content, a further 'true content' or 'kinship content' will he please be a little less mysterious about it, and tell us just what he thinks this is'.

First, note again that Beattie and I both agree that kinship is best defined as social and cultural, and therefore to see other content over and above or different from its social and cultural content is not the point of my first note or of this letter. I did not say, and I did not imply, that kinship had content in addition to or different from its social and cultural content. I asked, and I continue to ask, what distinguishes its social content from the social content called economic.

Second, this passage has a quite unreasonable argument embedded in it. It implies that if I think that Beattie is wrong about the way he thinks about kinship, then I would do well to provide the answer which I think is right. This is like saying that Beattie wrote a paper in which he said $2 + 2 = 5$. I read this and wrote, 'Beattie is wrong! $2 + 2$ does not equal 5.' Now Beattie says that he wants to know just what I think that $2 + 2$ equals!

Suppose that I say that $2 + 2 = 6$. Is Beattie any the less wrong because I too have given a wrong answer? Nonsense! His request for my answer to this problem would only divert attention from the problem at hand, but it would not help him one bit if I were twice as wrong as he!

It is hard enough to be reasonable men—as this exchange clearly shows. Let is therefore stick to one question at a time. I found Beattie's statement to be inadequate in the ways I specified then and try to clarify here. I still find his statement inadequate, and the problem is of whether or not Beattie's statement is adequate.

University of Chicago

DAVID M. SCHNEIDER

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Sir,—If Professor Schneider [see preceding letter] will reflect upon his own fieldwork I shall be surprised if he does not see that the answer to his question is that we distinguish kinship relationships from other kinds of social relationships because the people whom we study distinguish them from other kinds of social relationships. Thus what kinship as an anthropological concern 'is all about' are those social relationships, whatever their social and cultural content, which the people who have them think about and talk about in the idiom of kinship. No doubt these relationships are, or are likely to be, between people who are physically related to one another, but this biological circumstance, which anyway is inferred and not observed, can evidently form no part of the social or cultural content of these relationships (though people's ideas about their biological relationships, which are cultural data, may certainly do so). If Professor Schneider and I are now in agreement that what social anthropologists study are social and cultural, not biological, data, I am perfectly content to leave the matter here.

J. H. M. BEATTIE

Institute of Social Anthropology, University of Oxford

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Topless in Portugal.

Cf. MAN, 1965, 73. With a figure

There are various traces to be found on the Portuguese Atlantic seaboard of very early—possibly Minoan— influences: each, when considered in isolation, could be dismissed as fortuitous, but taken together they make an interesting study. One of the most intriguing examples is the type of figurine shown in the photograph, variants of which are sold at a fair held near Janas, a village overlooking the Atlantic just north of the western end of the Sintra range.

Standing among pine trees some 500 yards from the village is the small round Church of São Mamede, probably dedicated in earlier times to the Dei Manes according to a Roman inscription found nearby. Once a year its doors are unlocked and the peasant farmers gather from the surrounding districts bringing their cattle, sheep, goats and donkeys, and these they drive or ride three times round the Church in an anti-clockwise direction to ensure their good health. Sometimes they buy a wax ex ino in the shape of an animal—a few years ago for a measure of grain, now mainly for cash—and offer it to the Church. Coloured ribbons may be purchased for dogs, and even tractors and motorcycles may benefit by circling with the animals. Stalls are set up for the sale of wine, roast sucking pig and other concomitants of an annual celebration, and items of local pottery—the figurines included—are laid out on the ground for sale.

Earlier in the century the figurines were snake-entwined or held in their hands a lizard!; and an old potter with whom I spoke confirmed that he used to make them with a snake around the neck. Vestiges of what may have been a snake can be seen in one of the pieces (fig. 1d). Other points of interest are the traces (also fig. 1e) of falling locks of black hair (though the moulded head has obviously undergone a change), and the 'double hoop' around the waist (fig. 1c) somewhat reminiscent of the smaller Snake Goddess at Knossos and other Minoan figures.

Fig. 1. Pottery figurines from Janas, Portugal

The design and shape of the figurines can hardly have been introduced by or for the benefit of tourists, for Janas is so remote and inaccessible from the landward side that my taxi-driver in Sintra last year had never even heard of the place. Nor is it likely that the style could have 'evolved,' as this is a land where public standards of female decorum are strict in the extreme and there are not, as far as I know, any examples elsewhere in the country remotely resembling those illustrated.

It is not easy to accept that a design could have maintained any recognizable degree of continuity over a period of 3,500 years, but Portugal is a country of very tenacious local tradition; and from Dr. Glyn Daniel's study of passage graves it appears that there may well have been maritime contacts between Portugal and the Mediterranean during the first half of the second millennium.

The name 'Janas' also permits of interesting speculation. Rodney Gallop had noted the use of similar-sounding words for spirits, devils or fairies—xamis, xanas (Asturias), janes (Algarve), xas (Galia), diatu and dianho. He had also noted a ninth-century decree attributed to the General Council of Ancyr condemning certain
women who, reverting to Satan, professed to ride at night with Diana: and concluded that at Janas the devil of the Christian belief had possibly preserved the very name of the god of the pagan. Whether he was right or not, one can add the coincidence of the dates of the festivals of Diana (mid-August) and at Janas (17 August) and the fact that in Sardinia also the word ‘gianas’ (pronounced as the Portuguese Janas or xanas) means fairies or witches and is to be found in the name given locally to the rock-cut collective tombs—the ‘domus di gianas’—Rhodes House, Oxford

RALPH FELTHAM

References

REVIWRS

GENERAL


The Revd. A. M. Jones has attempted to show that during the first five or six centuries A.D., Africa was colonized by successive waves of Indonesians travelling by sea south around the Cape and landing along the West Coast (pp. 222–6). Emphasizing the first 154 pages as ‘... the chief part of the book...', the author presents a cross-cultural comparison of xylophones under the following chapter headings: I, Introduction; II, Matter and Method; III, The Tuning of the African Xylophones; IV, Features of the Heptatonic Tunings; V, The Slenëro-Type Xylophones; VI, Organology and Orchestras; VII, Xylophone Terminology. The last 78 pages of text offer other musical and non-musical phenomena in support of his thesis: VIII, Other Musical Instruments and Practices; IX, Shipping and Canoes: X, Non-Musical Evidence; XI, Some Implications. In his final paragraph, Jones states: ‘It is above all the musical evidence which provides so cogent a complex of testimony. As a musician the writer finds it impossible to believe that this evidence can be explained away...’ (p. 233).

In my judgment it is precisely the lack of significant musical evidence that weakens Dr. Jones’ ambitious thesis. His arguments presented in the first five chapters are based on an outdated methodology designed to yield gross averages that do violence to descriptive, analytical and comparative method. Chapters VI and VII show his unfamiliarity with the musical ensembles and foreign terminology discussed. In Chapter VIII his extension of Kunst’s work on the kensade is more convincing. The last chapters are speculative, but make interesting reading. His index is very slight, the bibliography is modest for the size of his subject, the 65 photographs are good.

In the limited space available it is difficult to indicate the frequent mistakes, apparent misunderstanding, biased selectivity, and many non sequiturs that make up this study. We shall limit our criticism to a few examples taken from the first seven chapters.

Altogether Jones gives measurements of 212 African instruments, and only 14 from South-east Asia and Indonesia. Although he does make reference to Kunst’s gamelan tunings (Music in Java, Vol. II), he seems to wave them aside with the conclusion that ‘... the standard of tuning in the Javanese instruments is not very high...’ (p. 114). This lack of numerical balance between African and Indonesian examples weakens the foundation of his thesis.

The large majority of African xylophone tunings presented were measured by Jaap Kunst with a monochord. Kunst himself, in his well-known book, Ethnomusicalogy, discusses the relative inaccuracy of the monochord, and calls particular attention to the difficulty of matching the pitch of a vibrating string with the sound of a struck wooden key. Jones betrays his lack of experience in handling the monochord or knowing its limitations when he says: ‘... one strikes a note on the instrument being tested and matches it by bowing on the monochord and moving its bridge till the sounds are equivalent...’ (p. 15). The monochord is not bowed but plucked or struck. A few of the measurements were made by Jones himself with the Strobocount, a modern instrument that holds to an accuracy of 1/1000 of a semitone (1 cent); but on p. 30, Jones cautions us: ‘... one has to remember that the Cents system is a dangerous tool because of its extreme accuracy.’ In this same context, he refers briefly to India where a deviation of 20 cents (2/10ths of a semitone) is a discernible part of musical style. He allows that this may be a maximum tolerance of deviation in Africa and Indonesia. In his ‘equi-heptatonic tables’ (pp. 36–48), he has arranged the xylophone tunings from the best to the worst judged according to his theoretical equidistant 7-tone tuning and states: ‘The best xylophones (say Nos. 1–3) show a remarkable precision and even when we come to the “bad” ones, the equational basis underlying them is still observable’ (p. 49). An examination of his first 13 xylophones (p. 36) shows a deviation from his theoretical equi-heptatonic standard by 20 cents or more in the following percentages: (1) 45 per cent., (2) 20 per cent., (3) 30 per cent., (4) 15 per cent., (5) 20 per cent., (6) 50 per cent., (7) greater than 50 per cent., (8) 15 per cent., (9) 15 per cent., (10) 20 per cent., (11) more than 50 per cent., (12) 45 per cent., (13) 50 per cent. Not a very convincing start from the best to the worst of 150 instruments measured.

The provenance of the xylophones used for this study shows that most of them are museum specimens (pp. 234–6). There is no assurance that they were in tune at the time of collection, and years of museum storage, handling, and continuous fluctuations in humidity and temperature slowly change them from musical instruments to museum specimens. In attempting to explain ‘bad notes’ or ‘bad octaves’ the author sometimes concludes that the tuning wax applied to the underside of some xylophones has fallen off. Such an explanation given on pp. 113f. in connexion with the Javanese xylophone indicates Jones’ ignorance of the fact that the Javanese do not use tuning wax. In the same passage, he falsely reasons that wooden keys show more accurate tuning than bronze ones. Fine Javanese gamelan are precisely tuned to complex ‘tuning patterns’ made up of compressed and stretched octaves throughout a 6-octave compass of the orchestra. In Java, what Jones calls ‘bad notes’ and ‘bad octaves’ are intentional and account for the distinctive character of different gamelan. Although museum specimens are valuable for studies in organology, they are not reliable for comparative studies based on tuning.

Sections of this very important and ongoing work continue to appear. As in the earlier parts, the standard continues to be high, with much detail, numerous and good-quality illustrations, and ample references. My only grumble is that although all but one of the studies under review are in German, the editors have not considered it worthwhile to include at least one reasonably long summary in another language. If one does not read German (or whatever the language is) with ease, then such extensive studies can be rather off-putting.


It will be seen that the authors are drawn from various countries, and it is to be hoped that such a valuable work will continue to retain this healthy international flavour.

DON BROTHWELL


Linguistic problems play a very great role in many of the newly independent states. Though they may have historically old boundaries, some of these states are multilingual, such as India with her 845 languages and dialects; others, e.g. in Africa, may have boundaries which cut right through linguistic areas so that a language is divided between two states in which also other languages are spoken. Professor Le Page in some introductory chapters deals with the social role of language in general, its function for the individual and its role in society. The *exposé* is brief, but lucid, very well written and up to date. In the chapter on language in society he arranges the factors under such headings as government and law, education, religion and culture. Under education mathematics is the object that the demand for primary education in the 'mother tongue' must not be understood too literally. It is true that there exist bilingual children, and probably also trilingual, who do not know which language can be regarded as the 'mother tongue.' The point is that it is a bad method to teach a child to read and write in a language which it does not know so that it has to learn that language...
at the same time. The author has further a chapter on what he calls the 'colonial hangover' and then studies the situation in India and Malaysia. These two very different and very complicated cases are sufficient to show that there exists no universal solution of the national language question. In his conclusions the author shows how the different main solutions possible are (1) to use one or more of the indigenous languages for all purposes, (2) to give equal status to one or more local languages and an international language, and
(3) to adopt an international language for all purposes. All these solutions may have their drawbacks. It may seem paradoxical, but English will be a stronger integrating force in Malaysia than the 'national' languages Malay or Chinese. In India. The English language brought them (the Indians) together on a common platform and evoked their inherent feeling of "Indian-ness"; says a report of 1961 from 12 distinguished Indian leaders.

ALF SOMMERFELD

AFRICA


In 1957 the Historical Monuments Commission of Southern Rhodesia decided to undertake excavations at Zimbabwe with a view to determining the ceramic sequence and also the architectural history of the Elliptical Building. Excavations on the Acropolis and in the Elliptical Building were carried out during the following year, and the publication under review is a detailed report of the findings.

On the Acropolis five distinct occupations were recognized and dates for these periods were established by means of radiocarbon dating. The first period, pre-A.D. 300, is characterized by stamped wares ascribed to peoples ancestral to certain sections of the Bantu tribes of today, particularly the Sotho. No evidence is given for associating the pottery with the early Sotho, although the resemblance between Class I pottery from Zimbabwe and the pottery of the Gwembe, one of the first Sotho tribes to move south, is very marked. The Gwembe, however, were accomplished builders in stone, as their extensive settlements along the Vaal amply demonstrate, whereas at Zimbabwe no evidence of stone building during the first period was found.

The second period extended from A.D. 300 to 1085 and it marks a distinct change in the style of pottery. The authors regard this pottery as possibly belonging to the ancestors of the Karanga. Wall-building on the Acropolis began during the third period, A.D. 1085 to 1450, but the coursed walling which makes up the major part of the Zimbabwe structures, and of many other ruins in Southern Rhodesia, did not make its appearance until the fourth period, which extended from A.D. 1450 to 1833, when the Acropolis was occupied by the Duma.

The most important result arising from these excavations is the dating of the coursed walling from the middle of the fifteenth century. Schofield and Walton, on geological and historical evidence, had previously given the date of A.D. 1460 for the introduction of this type of walling. The radiocarbon date of A.D. 1450 ± 150 for charcoal of this period corroborates the evidence from other sources.

In associating these periods with tribal groups the authors do not give any supporting evidence. They state that it is impossible to be certain who were the builders of Period III walling, 'but perhaps the use of the term "Shona" is as suitable as any in describing this culture.' Similarly they state that Period IV 'marks the advent of some new and vital element at Zimbabwe, presumably the Rozwi.' Sufficient is known of central African tribal histories to establish more definite connexions than these vague statements would indicate.

If the coursed walling of Period IV dates from A.D. 1450 ± 150 then it cannot all be ascribed to the Rozwi, who entered Rhodesia about A.D. 1693. I have shown elsewhere that this period can be divided into a pre-Rozwi stage from A.D. 1460 to 1693 and a Rozwi stage from A.D. 1693 and 1833. The cultures of these two stages have sufficient in common to indicate that the peoples concerned were closely related and that they both came from the Congo.

There is historical evidence that the first Congo tribes reached Rhodesia within the period A.D. 1450 ± 150 (see James Walton, African Village, 1956; MAN, 1959, 92; and Journal of African History, 1960, pp. 19–30). A closer study of the historical evidence in relation to the radiocarbon dates would have enabled the authors to present a much clearer picture of Zimbabwe history than has hitherto been available. As it is they have provided a detailed excavation report which will be invaluable to other workers in the same field.

JAMES WALTON


The book consists of 11 papers contributed by delegates to the Leverhulme Conference held at Ibadan in December 1961. The subjects under discussion overlap to some extent with the colloquium on multi-lingualism, sponsored by CCTA/CSA, at Bracknell in 1962. At both conferences the speakers mapped out areas where important research ought to be done, but the report of one of the working parties at Ibadan is realistic enough to admit that the advice of experts is not very likely to influence government policy: 'we realize that the value judgments of a panel of experts are unlikely to coincide closely with those of politicians and administrators' (p. 133). Conferences of this kind, which are convened for their own sake and lack the financial and political backing for carrying out any particular proposal, are bound to have an air of unreality, like tactical exercises without troops.

The crux of the language dilemma which exists in the newly independent countries of Africa is stated very clearly by John Spencer, writing on 'Language and Independence' (p. 34): 'On the one hand stands the essential task of planning for and speeding up economic development, extending educational opportunities and beginning the task of unifying politically a multicultural people; this seems to require the retention of the imported colonial language if the desired acceleration of development and unification is not to be impeded. On the other hand, the full expression of the personality and cultural dignity of the nation, and of groups within the nation, appears to demand the development of the vernacular languages—more particularly since the vernaculars tend to be disdained, and sometimes despised, under colonial rule. It is this apparently conflicting linguistic demands, arising after independence, which require careful adjustment and compromise. And it is fruitless to pretend that language is not inflammatory matter; when language becomes a political matter it can become very inflammatory indeed.'

Many of the contributors to this book have had practical experience in Africa, either as language-teachers or as technical linguists in one of the African universities. The articles are written in a style that is easy and pleasant to read; what is more, the historical arguments and illustrations have a much wider range and perspective than is usual with writers on African problems today. At the end of the book there are four working party reports on subjects which are of general concern to all the territories represented at the conference. The book is extremely well printed and indexed.

GUY ATKINS


The theme of this publication is that tropical Africa is no longer an entirely closed book for the demographer; that, although care is necessary in dealing with the results of
censuses and other source data, it is now possible to perceive, albeit tentatively, population trends in the continent. The first part of the booklet, by T. E. Smith, summarizes the present demographic situation for the African Commonwealth countries generally, the second part by J. Blacker concentrates on Tanganyika and Uganda.

In Part I, the difficulties are first outlined of demographic investigations in Africa where accurate registration is non-existent and census results unreliable. There is a brief chapter on the non-African sections of the population, stressing the potentially low mortality that can be achieved with a sufficiently high standard of living—though age-specific instead of crude death rates would have been more revealing. This leads to a discussion of urbanization, or lack of it, in tropical Africa and the demographic features of urban communities, an extension of the calculation between degree of urbanization and population density, to the contrast of West Africa to the other Commonwealth African territories, and predicting that by A.D. 2000 one-fifth of the total population of Africa will live in towns of more than 20,000 inhabitants. Another major characteristic of African population, migration, receives a short chapter, in which its effect on demography is sketched, somewhat unsatisfactorily. In the next two chapters fertility is shown by no means uniformly high and some of the causes of variation are mentioned; how little is known about mortality is made clear, though the heavy infant and child mortality is demonstrated, and a difference in child mortality experience in urban and rural areas seems to be indicated. Comparison of recent censuses indicates widespread modest annual increases, but the conclusion is clear: 'unless fertility can be brought under control and the rate of population growth ultimately reduced to about half the present figure, the demographic outlook will become increasingly grim.'

Part II is rather less ambitious in treatment, Blacker restricting his discussion mainly to census material. The earlier censuses in East Africa are briefly reviewed, and the opinion is expressed that their unreliability can be exaggerated and that 'the figures piece together into a reasonably coherent picture.' Chapter II discusses in terms of regions the intercensal increases 1948–57 and 1948–59 in Tanganyika and Uganda respectively, showing that in many areas the expansion forced the population to seek their means of livelihood elsewhere and in the most dense areas this meant expansion only into peripheral regions less suited to human habitation. The chapter on fertility and mortality summarizing data from questions in the last sample census emphasizes again the variation in fertility and the inadequacy of mortality information. Finally a rate of population growth of approximately 2 per cent. per annum is arrived at.

This is a stimulating little booklet. It would have been more valuable if the authors had made more use of sources other than standard demographic works, for there is much detailed information relating to sample studies hidden away in the ethnological literature, which would have served to support the figures, which they do quote. But in a publication such as this it is necessary to be selective, and the material used does demonstrate the points. It is refreshing that the usefulness of small cross-sectional samples is appreciated and how these can implement the outlines given by standard demographic investigations. But it is the points that emerge from the synthesis that are the most important contribution of the present booklet, the ideas that they suggest for further investigation, which make it well worth reading.

D. F. ROBERTS


In the new African states, one of the urgent needs in the legal field is to record the 'customary law' of the region. There are many reasons for this apart from the danger that it may be lost. Though the colonial powers commonly imported their own systems of law, with or without modifications, native courts have retained or been given jurisdiction in certain fields, especially those of marriage and divorce, property and inheritance, and it is impossible to jettison or transform a system of legal rules overnight even if public opinion and the national economy could suffer such violence.

Perhaps the most important of those fields consists of the twin subjects of land and succession to land, especially where (as in Nigeria) all the reported decisions of the courts and the statements and opinions of text writers contain little of the law. The prevailing ignorance is a serious handicap to commercial development for unless it is known who has the legal right in respect of a piece of land and what is the nature of the right, credit cannot safely be given for any purpose on the security of the land, and where the stage of economic development is reached where sales of land are or should be common, it cannot safely be bought or sold.

And here is Dr. Lloyd, a social anthropologist who has made previous research tours among the Yoruba from 1949 to 1956, and from 1956 to 1959 filled the post of Land Research Officer in the Lands Department of the Ministry of Land and Labour of Western Nigeria, carrying out the task of preparing a report on the customary land law of the region. The book, which is in substance his report, is well conceived and well executed, and fitted to serve as a model for other undertakings of the kind. He writes as an anthropologist and his methods of research are those of an anthropologist, but his terms are, on the whole, easily comprehensible by a lawyer, and he writes especially for the lawyer and judge of the customary courts, illustrating his views by summaries of a large number of decisions of those courts. He has taken four large typical towns and sets forth their social and political structure and customary land law, as elucidated mainly by enquiry, and adds general chapters on succession, credit and the sale of land. Altogether a valuable work of reference and a very readable treatise which brings out a number of aspects of the law of land and succession which are not commonly appreciated.

A. S. DIAMOND


The Companhia de Diamantes de Angola has placed all anthropologists and naturalists interested in Southern Africa in its debt by the magnificent series of Monographs it has published, of which this work is the fifty-fifth to appear. It is well worthy to hold its place beside the earlier ones.

In a general introduction of 45 pages, Mlle. Bastin introduces us to the Jokwe (Bajokwe), particularly to their geography and history, their material culture, their social life and their art. Her approach in the first chapter is technological, and she classifies the variety of ornament according to the techniques of decoration, i.e. by drawing, by painting, by tattooing or by incising whether these techniques are applied to the human body, to wood, to pottery, to stone or to metal. She goes on to discuss the colours which are used (chiefly white, red and black), the materials from which these pigments are prepared, and various dyes which are also used. The third chapter discusses hair-dressing, whilst the fourth, the most substantial, is devoted to tattooing motifs and geometric patterns used as ornaments. These motifs are classified either as abstract designs, or as designs derived from nature (e.g. stars, cowrie shells, snakes, fish scales, footprints) or else as motifs derived from human beings and their activities and manufactures. The fifth chapter deals with 'physic-plastic' motifs, that is sculptural forms derived from plants, from animals, from human beings or from tools—much the same classes as those used in analysing decoration in the preceding chapter. There is finally a conclusion which discusses the frequency with which these motifs occur in the different parts of the area studied.

This study is based on an examination of eight hundred items in the Museum of Dundo of which half formed the principal basis of the work and are illustrated in 277 splendid plates in Volume 2, which also contains excellent descriptions of each item. The photographs are admirably sharp and clear, taken under fairly flat lighting which avoids dense shadows which often obscure details, although there is sufficient shadow to show the modelling. With this style of photography one feels that one is seeing the objects themselves, not an interpretation forced upon one by a photographer striving after dramatic effects. Moreover, having the financial backing of Diamang, Mlle. Bastin has been able to use more than one photographer.
of an object where this is desirable; there are, for example, six photographs of one xylophone, seven photographs of one chair and six of each of two others, in order to show the decoration on each part. Throughout the text the Jokwe name of everything referred to is carefully recorded, a practice which deserves to be more widely followed by art ethnographers. The text itself is carefully cross-referenced to the plates, which can be very conveniently examined as they are in a separate volume from the main text.

This is, in short, a very thorough study of Jokwe art and decoration. Exceedingly well illustrated and well produced, it forms a real anthology of Jokwe art which is only poorly represented in most books on African sculpture, and, indeed, in British museums.

FRANK WILLETT


This is a major work on the Tuareg, based on the author's field study, during several recent years, of North African pastoralism, and on an exhaustive perusal of relevant literature. In the first part of the work the author considers cultural traits connected with subsistence activities. Stock-breeding of goats, sheep, zebras, camels and donkeys is presented in great detail, both in its material and social aspects. Appropriately, shorter consideration is devoted to hunting, collecting, agriculture and caravan trading, with food preparation crafts, and dress details following; the author usefully draws attention to the similarity of social function of the men's face veil and the woman's head cloth without face veil. There is a valuable study and classification of dwelling-types on the basis of detailed analysis of the structure, which is very helpful in understanding their geographical distribution in this large confused area of north-west Africa, where migrations and trading expeditions have had such great influence. In the later part of the work, political systems of the pastoral Tuareg are ably considered in terms of drum groups, and noble and vassal classes; the economic importance and social function of slavery is well presented.

In the section on social organization, Tuareg matrilineal succession and inheritance are fully discussed, as well as relevant influences of contact with Arab patrilineal organization. This is the first time that a reliable account of Tuareg kinship systems has been published in detail.

The conclusion presents a valuable account of cultural developments in North Africa, and briefly discusses the origin of nomadic stock-breeding in the light of Tuareg culture.

There are interesting botanical lists of three groups of fodder plants in Ahaggar, as preferred by (a) camels, sheep and goats, (b) donkeys, (c) zebras and a glossary of Tuareg words.

The work is fully illustrated by a fine series of new photographs by the author. Furthermore, it is accompanied by a very comprehensive bibliography of the extensive relevant literature; the latter is mostly of little value, however, and this greatly emphasizes the importance of the present work.

The book is mine of trustworthy information and a first-class contribution to ethnographic literature in general, and to that on the Tuareg in particular.

It is regrettable that, in such an otherwise fine work, there are fairly frequent misspellings and some quite misleading misuse of English words (e.g. sticks for stakes, ranching for ranging, estimated for esteem) which quite often make the work more difficult to read. A résumé in Danish is provided.

WALTER FOGG


This is the third of a projected six-volume series from the Frobenius Institute which will say something, at least, about almost every cultural group in southern Ethiopia. Like the first two, it is beautifully and helpfully illustrated with many fine drawings, photographs and sketch maps. This volume deals mainly with the

Chako, Amaro, Dorze, Zala and Janjero. Since virtually nothing has been written about these peoples before, this volume is an important increment to our knowledge of this exciting and potentially fruitful area. There is much useful material on the distribution of peoples, crops, settlement patterns and material culture. Regrettably, it cannot be said that the book has much to offer those interested in social, political or religious organization. The data were collected from informants on trips too short to allow much depth (e.g. five weeks in Dorze, six weeks with the Amaro, eight weeks with the Janjero), and in the tradition of the German culture-historical school the authors are most concerned with cultural traits which they consider diagnostic for culture historical analysis. Readers not of this school may be less than satisfied with their conclusions.

A major thesis of the book is that the Chako (and other Gimirra peoples), although they speak Western Cushitic languages and have divine monarchies and many other traits common to Western Cushitic culture, actually represent an older, pre-Cushitic culture stratum. This conclusion is based primarily on the fact that they make greater use of root crops and have more 'Negroid racial elements' than most other Cushitic groups. Straube ignores the possibility that these divergences might be due to anything but the overlaying of successive Kolchenschichten. It seems likely to the reviewer that the Chako make greater use of yams, tao and ensete because they live in a dense rain forest zone while most other Cushites live in unfortsted grassland areas. Furthermore, the recent discussions of the botanists give no encouragement to Straube's assumption that tao and yams were cultivated in Ethiopia earlier than grains. No usable data on physical types are presented in the book, but it is perfectly reasonable to assume that the Chako have derived some genes from the people of the Sudan through recent gene flow. They are in direct contact with 'more Negroid' peoples on their west and south and it would be remarkable if they did not show signs of their intermediate position. Finally, linguistic evidence suggests that the Cushites were the earlier inhabitants of southern Ethiopia and that the Nilo-Saharan speaking peoples on the borders are the more recent intruders.

HERBERT S. LEWIS


This book, translated from German, forms volume XII in the series 'Art of the World.' It is intended to give 'a brief survey of the most essential facts and artistic monuments,' and attempts to survey not merely art and architecture from predynastic to Roman times but also to supply the necessary background material, history, religion, geography, etc. To do this a purely chronological arrangement has been adopted, and following a brief introductory chapter entitled 'Basic elements of Egyptian culture' is a series of chapters divided according to the chief periods of Egyptian history. In most cases a chapter outlining the most important historical, religious and social factors of each period is followed by a second chapter detailing and discussing the chief examples and features of the architecture and art. The book concludes with notes on the text, three pages of maps, eight pages of chronological tables which include information on history and culture as well as the chief events in neighbouring countries, a bibliography, a brief glossary, and finally a comprehensive index. Illustration is provided by sixty-two colour plates and seventy-nine small line drawings in the text. This strictly chronological arrangement of material has produced a picturesque and rather confused presentation, related facts being often scattered through several chapters. Whilst painting and sculpture are dealt with in some detail, architecture is given rather scanty treatment with little general explanation. Much space is devoted to aesthetic analysis and to discussion of motivation, and accordingly there is a great deal of subjective opinion and generalization, the authority of which is rather lessened by errors of fact, omissions, and a tendency to present hypothesis as established fact. The style, with its short staccato sentences and lack of continuous narrative, makes for rather dull reading. It is particularly unfortunate that there has been no attempt to adapt the bibliography, which contains mostly German works and carefully omits most of the books suitable for the English reader.

WALTER FOGG
The colour plates, while lavish in quantity, often fall below the standard one has come to accept as normal. BARRY J. KEMP


The University College of Fort Hare has done a service to ethnographers in publishing the catalogue of the Estelle Hamilton-Welsh ethnographical collection, which was presented to the College last year.

The late Mrs. Hamilton-Welsh was born in the Transkei and spent a large part of her life there. She had therefore ample opportunity for developing an interest in the crafts of the people among whom she lived. The oldest objects were collected by her mother in the eighteen-nineties, but the majority were collected by herself in the twenties and thirties of this century. By 1936 the collection was sufficiently remarkable to be exhibited at the Empire Exhibition in Johannesburg and two years later at Glasgow. Probably the most important are the items of clothing which include old skin garments that are both rare and beautiful. They are now out of fashion and like most of the objects in the collection, no longer obtainable. The collection is therefore of immense value, and it is fitting that it should have been placed at the College which serves the area from which most of the objects were collected.

The catalogue is well arranged and easy to use, but could have done with more careful proof-reading. It is introduced by a short biography of the collector and a history of the collection. Each class of object has a chapter to itself, with a short introduction. The text is somewhat personal and romantic in style, but contains useful information in a field that is not much published. It is a pity that in some sections where objects from different areas are included, the text often generalizes for the whole group of objects.

The spelling of the vernacular terms has been amended according to the rules of the International African Institute. One would have been glad to have been assured that the editing included a check as to whether they are always correctly applied to their objects, as in a few cases this seems doubtful.

E. M. SHAW


This book, described on the dust cover as a 'novella,' was written in Chaha, a dialect of Gурage in Southern Ethiopia, by a Chaha-speaker educated at the University of California, Los Angeles. It is a simple story, well translated, of the life from birth to marriage of a Chaha boy in modern Ethiopia, and includes an account of a visit by bus to Addis Ababa. Like other books of this kind, a good deal of explanation has to be worked into the narrative, but even without this help the reader can guess at the meaning of such terms as 'Mekel.' The book is hardly ethnological, but it does give an idea of part of the life of a Gurage family today.

G. W. B. HUNTINGFORD

AMERICA


The Kuskokwim River valley and delta of west Alaska and their inhabitants (the Kuskokwim flows into Bering Sea between the Yukon and Nushagak Rivers) deserve more attention than they received before Professor Oswalt fortunately became interested in them. For the past decade or more, he has made this his area of special interest in regard to prehistory, early historical contact and recent acculturation, with greatest emphasis on the middle period, which is presented in the above publication. In a triangular region bounded by a line from the mouth of the Yukon inland to a marginal zone between Athabaskan Indians and Yuit (southern or 'western' Eskimos), where the Yukon and its tributaries curve far southward and the Kuskokwim curves northward, coming close together, then down the latter river to Kuskokwim Bay, then northward along the sea coast to the Yukon Delta, there lies the greatest concentration of Eskimos in the whole domain of this far-ranging race. More than 6,000 Eskimos in about 30 villages—it is difficult to obtain an exact figure as they are increasing at an annual rate of 3-5 per cent.—live in the Bethel Triangle, named for the region's trade and transportation centre, which was founded by Moravian missionaries sent from Pennsylvania in the eighteen-eighties.

Although the book's subtitle suggests that only the influence of these missionaries is discussed, Mission of Change begins with the Russian priests and traders who preceded them. American traders who were contemporaries of the missionaries, especially those on the lower river, are not covered so well. The author tries to do several things together in this book: give a history of the mission (but in detail for its early years only), describe succinctly the Eskimo culture in the period, 1885-1925, when it first received vigorous, continuous pressure to change, and state the resulting changes. A final chapter gives a few suggestions as to why the Kuskokwim people changed in the way they did. The analysis is not, however, detailed.

Oswalt says that these riverine, fish-eating Eskimos lacked a strong religion well integrated with their subsistence—the coastal people's religion, centered on the sea mammals, was integrated with their more typical Eskimo economy,—and therefore did not have a religious bulwark against the zeal of the Protestant missionaries. He could have added that the middle river people lacked also the material wealth of their coastal cousins, as Eskimo wealth is reckoned, and may have had the dietary deficiencies that they have today.

In other words, they lived in a hungry country where no one starved—there was always some kind of fish,—where there were no large game animals and few challenges to the individual. The author mentions but does not fully explain the resistance of the tundra villages, between the Yukon and Kuskokwim Rivers, to Christianity (evidently they were simply more isolated from the Whites while at the same time having closer relations with the Bering Sea and Yukon Delta Eskimos), and does not explain the conversion of Kwinhagak and other communities at the mouth of the river, which were seal-hunting as well as fish-eating. On the whole, the summary of values that were similar in the Whites' and Eskimos' cultures and of those values or other aspects of culture that were different is supported by the ethnographical evidence presented. The ethnography, one should note, is not derived solely from the missionaries' recorded observations, which have been thoroughly and impressively combed by the author; it is considerably expanded by his own work in the area.

There are inherent difficulties in giving life to the subjects of a book of this kind. First, it is about missionaries, yet not a personal account, not written from the viewpoint of the Moravians. For this, one should read Eva G. Anderson's Dog-Team Doctor (Caxton Press, 1940) and Anna Buxbaum Schwab's Dayspring on the Kuskokwim (Moravian Press, 1951). Secondly, it is about Eskimos, yet not an account of any one settlement or group of settlements. For such a description, one should read Oswalt's Napaskiak (University of Arizona Press, 1963). Anyone desiring to get acquainted with the long neglected Kuskokwim area should read Mission of Change first, since it provides a historical and ethnographical framework, then the two biographical books on the missionaries, then Napaskiak, which is a study of a community as it was only ten years ago, and finally the topographical papers and monographs on art, folk medicine, diet, and other subjects listed for the area in the Arctic Bibliography.

One should note with gratitude that more than half the material of interest to ethnologists and social anthropologists has been provided by Wendell Oswalt.

The book contains a sketch map of the valley and an end map of Alaska but lacks illustrations and bibliography, and the authors
Anthropologists and sociologists have shown a good deal of interest in this heterogeneous society especially after the Second World War. Of course, most research workers in this field were Dutch. Annemarie de Waal Malefijt is an exception, inasmuch as she was already a United States citizen when, after her studies at Columbia University, she undertook fieldwork among the Javanese in Surinam in 1958 and 1959 under a grant from the Wenner-Gren Foundation. Her Columbia Ph.D. thesis was published afterwards in Holland with financial aid from the Netherlands Foundation for the Advancement of Research in Surinam and the Netherlands Antilles, Wosuna.

A society consisting of groups with very distinctive cultural backgrounds is an attractive object for acculturation studies. The author accordingly describes the extent to which the Javanese group in Surinam has preserved the cultural tradition of their motherland and in what way the Javanese are modifying the cultural tradition by their contacts with other segments of the total population. In analysing the situation she arrives at the conclusion that the group has developed a strong resistance against external influences. She demonstrates that the resistance to cultural change can be partially explained by the group’s marked internal cohesion, which implies a more or less isolated position of the Javanese in the country. Moreover, certain elements of the Javanese value system are indicated which oppose coercive culture change. In this connection Miss de Waal Malefijt extensively discusses the desire for harmony, rukun, as a living phenomenon.

Her ample anthropological study not only provides important material for further analysis of Surinam society, but is in itself ‘a unique addition to contemporary anthropological work,’ as Margaret Mead states in her preface.

The only objection which one could raise against this study is that Miss de Waal Malefijt has more or less limited her research to Javanese of the ‘districts,’ the rural area, while for an analysis of acculturation processes the urban Javanese would seem more important. In the town of Paramaribo the respective population groups are in closer contact. This does not imply, however, that the factors adduced by the author as influencing the low degree of assimilation of the Javanese are not active in the urban surroundings. Even so, I am of the opinion that the younger generation of Javanese in Paramaribo show much less resistance to cultural change than those of the districts. The author acknowledges these circumstances in her remark that in the town changes will be inevitable.

J. D. SPECKMANN


With the simplest of tools, the Maya of the Classic Period fashioned great quantities of carved jades which can stand comparison with the best produced by the Chinese. According to representations on steles and in paintings, Maya rulers and nobles were festooned in ornaments of that extremely hard stone, which was the most precious substance known to them.

In this modest and clearly written little handbook, the author tells of the cultural importance of jade to the Maya, of problems in the dating of individual jades, and of the lapidary techniques which were probably employed. A classification of different kinds of jade objects is offered, and some outstanding jade pieces in the British Museum are described.

It is to be regretted, however, that in a popular introduction to the subject, there is no definition of the word ‘jade.’ According to the late W. F. Foshag, to whose authoritative studies there is no reference, ‘jade’ is a generic term encompassing four different kinds of minerals: jadeite, nephrite, diopside-jadeite, and chloromelanite, all of which were worked in ancient Mesoamerica. Thus, one should put to rest the often reiterated claim that New World jades are not really ‘jade,’ a ridiculous opinion anyway since the word was first used by the Spaniards to describe the hard, green stones used by the Mexicans.

Drawing from his own curatorial experience and from the researches of A. L. Smith, A. V. Kidder, and Elizabeth Easby, Mr. Digby has covered his subject well. The handbook is attractively
produced, with excellent plates, one of which is in colour, and with clear drawings showing the methods of jade-working. We hope that Mr. Digby will continue to provide us with similar introductions to other aspects of Classic Maya civilization.

MICHAEL D. COE


The flow of books on ancient American art is rapidly becoming a torrent, and as the waters rise do so the prices. Dr. Dockstader, Director of the Museum of the American Indian, Heye Foundation, New York, has drawn very largely on the collections of that museum for the illustrations.

Apart from a ten-page introduction and a chronological table with many errors, the book consists of the plates, one to three to a page, and their short captions. These are of excellent quality. Three-fifths of them cover Mexico and the Maya area; lower Central America to Panama and the Antilles are allotted rather over a fifth; the remainder is given up to modern ethnographical pieces. It is good to have on record many important pieces which one remembers from treks to 155th Street.

As a host-and-hostess present the book is admirable, but archaeologically it is less praiseworthy. Dr. Dockstader makes no pretense to being an expert on the archaeology and ethnology of the areas that he covers, so it is a pity that he did not have someone competent to vet his text and captions. Very little effort has been made to relate cultures and periods to one another, and the dating is very unsatisfactory. Early Maya Classic vessels of the commensal types are dated A.D. 250–1000, and it is disconcerting to learn that Chichen Itza flourished from A.D. 455 to 1697. Cholula, we are told, was the centre of the Mixtec world; the two correlations of Maya dates differ by 650 years; and the peoples of Panama and Costa Rica were remnants of the parent Maya group. Archaeological sites and tribal groups are misplaced on the maps; glottochronology, so mal-treated by experts in recent years, wins high praise. A word in defence of Bishop Landa, so often castigated for burning Maya hieroglyphic books (there is even a modern mural in Mexico City with the satirizing-featured bishop beside the flaming pile). How many he burned is not known; the only figure, and that in a late source, is 27. In the introduction to this volume we read: ‘Only perhaps two score Mixtec-Aztec books still survive of the thousands burned when the notorious Bishop de Landa destroyed the libraries.’ If he destroyed thousands of Mixtec-Aztec books, whatever they may be, the burned Maya books must have run into the ten thousands, for he worked only in Yucatan.

J. ERIC S. THOMPSON


Professor Schaden sets out to study the relations between ‘social phenomena or important institutions observed in aboriginal cultures of different regions’ and the ‘hero mythology of the tribes in question’ (p. 171) and also the relations between the various aspects of single tribal cultures and the other tribal myths. This is a herculean task to attempt in about 130 pages of analysis, especially when much of this space is devoted to schematic outlines of the cultures to be discussed or even to establishing the data to figure in these outlines. Inevitably then the book has the character of an essay which only touches on the questions which it seeks to treat.

They are nevertheless questions of the greatest interest and useful insights emerge from the author’s handling of them. In discussing messianic movements, particularly those of the Tupi-Guarani, he points out that despair in the face of alien (white) intrusion is not a necessary and sufficient condition for them. Instead he suggests that in South America at least they are to be found largely among tribes with a belief in a culture hero who will lead his people to a terrestrial paradise. One might wish that he had chosen to elaborate this in a thorough comparative analysis instead of giving a series of illustrative examples in a manner reminiscent of Sir James Frazier; especially since the later discussions of the Kaduveu, Bororo, Kikangi, Apakouva and Mundurucú serve in the main to make the Malinowskian point that the hero myths of these people are charters for features of their social organization. It is of course gratifying to know that in each of these societies there is as a correspondance between hero myths and social institutions, since that is what one would expect; but some of Professor Schaden’s incidental remarks are more stimulating than his main thesis.

He discusses for instance the significance of the two pairs of Bororo twins, a crucial and hitherto neglected point in Central Brazilian mythology. He suggests that the prestige of the medicine-man among the Tupi-Guarani derives from his representing a hero. He notes too that, among the Kaingang and the Mundurucú, paint styles appear to be determined membership in certain groups, a principle which anthropologists have been slow to grasp. Yet these insights unfortunately remain undeveloped. Instead he tends to resort to ethnological explanations of which the following are two examples.

(a) The Bororo myth of Baitaogho who journeyed to a distant land and then returned to his community was probably based on historical fact, because the Bororo lived by hunting and are therefore likely to have wandered far afield in nomadic hordes.

(b) The Apakouva have a myth of a pair of hero twins who outwit the Anyá spirit. The spirit wears his penis tied up with a cord around his body. When he goes to wash it in the stream, the heroes throw a peeper in the water. The Anyá rushes from the place and falls over a precipice. This story probably serves to make fun of the Kaingang who are likely to be the only Indians known to the Apakouva who wear the penis bound up.

Professor Schaden is modestly frank about the tentative and somewhat fortuitous nature of his essay. He does not for example deal with Gê mythology on the grounds that Nimuendajú has already demonstrated the social significance of Gê myths. If we remember that Nimuendajú left us only a few cursory remarks on the copious body of myths that he collected, it becomes clear that Schaden is here concerned to fill in the gaps in a sort of ethnography of Brazilian myth, rather than to undertake a major analysis. If this seems rather an old-fashioned aim, then it is only fair to remark that the book was originally written 20 years ago and the author would probably write it quite differently if he had to do it again today.

DAVID MAYBURY-LEWIS


Contributions to Indian Sociology has by now established itself as a periodical of great significance in the field of Indianist studies. Its main merit has lain in the editors’ consistent effort to present, through a number of exploratory and review articles, a viewpoint regarding the nature and scope of ‘Indian Sociology.’ Their main thesis has been that it is not enough to study social facts as ‘things,’ in the manner of a natural scientist; that they must also be studied as ‘representations.’ Their approach has been serious and constructive, and their manner frank but often pungent. Inevitably they have not convinced everybody: critics as serious and able and outspoken as themselves have not been wanting. One is particularly reminded of Professor F. G. Bailey and Mr A. K. Saran. And the utility of the debate thus generated has been immense. It is, therefore, with great regret that I read the announcement in Contributions VII that the next issue is likely to be the last of the series. Whatever the reasons which have led to this decision by Professor L. Dumont and Dr. D. F. Pocock (the editors), Contributions will be missed.

ASIA

just punishment for them. The Gulistan was written in the year 1258, when Hulagu Khan sacked Baghdad, the city where Sa’di had studied as a young man. The episode is scarcely, if at all, alluded to: Sa’di was writing that a civilization and way of life might continue and so did not chronicle events that had made this artistic effort the more urgent and compelling.

His book is wrought like some finely enamelled decoration, rhyming prosaic of the kind called saj’, alternating with short poems, sometimes a single verse only, in a variety of metres and forms that embraces the whole gamut of Persian poetical modes. Therefore stylistically it presents a formal model of perfection and symmetry, composed when man’s hard-won gains over chaos seemed threatened with extinction and Shiraz, whose rulers the Salghurids (1148–1287) paid the Mongols heavily for it, had purchased a reprieve from Chinghizian destruction; while its geographical situation also helped to ensure it that peace which brought the wandering poet back to his birthplace.

Because the original, with its stories of tyrannical and capricious Sultans and ways to make them compassionate, of ascetics sincere and hypocritical, of love’s snare and the debilitating consequences of luxury’s indulgence, is so skilfully wrought as a filigree work of carefully chosen words, right for both sound and sense, it should not be thought of as precious and whimsical. The seriousness of underlying intent is too strong for it to be either and, while Sa’di provides the finest model of how the Persian language should and could be used, he writes a ‘hard,’ not a soft and precious, language in the speech of his people, raciness being raised to the level of artistry but not lost in the process. Until a modern crowded curriculum, based on Western syllabuses, overtook the Persian schoolboy, all educated men in Iran knew the Gulistan by heart and from it learnt how to write and speak; as well as much else as closely relevant to their Persian lives as their speech.

Clearly the translator is set a hard task and cannot hope to convey such linguistic subtleties of adornment as this book contains into another idiom. Idiom is, however, the keyword and if he remembers that Sa’di though artful is so on a firm bed of ordinary, idiomatic Persian speech, and remembers, too, to translate from one idiom into the other, his own, keeping his sailor-translator’s eye on the beacons of living speech, then the translator will be safe. If he does what Victorians did so often and attempts to make a ‘Persian’ book in English, then he will be wrecked.

Sa’di is great enough, his material continuously humanly relevant enough, to be translated afresh in the idiom of every age. Therefore, as Rehatske’s translation is dead, dead as Queen Anne, yes, though adequate enough for the student at least to see what Sa’di is talking about, a new version in modern English for today would have been far more welcome. Gratitude is due to Professor Wickens for his thought-provoking introductory essay and for such corrections and amendments as he has made, but Rehatske is not better than John Platt’s translation as an accurate rendering, and, instead of reprints of either, a modern living book is what is needed; and Sa’di deserves.

P. W. AVERY


The arresting title and table of contents of this valuable and important study relate specifically to a socio-ecological examination of what Geertz regards as Indonesia’s two predominant agricultural systems. These are the intensive, ‘involved’ wet-rice culture of Central and East Java, an area which he designates ‘inner Inner Indonesia,’ and the extensive swidden and cash-crop farming of ‘Outer Indonesia,’ by which term he means the rest of Indonesia, excluding Java, Madura, Bali and Lombok. The striking contrast between these two ‘systems,’ already well known to and depicted by Dutch scholars such as van Beukering, Bocke, van Gelderen, Gogosgepij and Terra, is made clearer by this new multi-dimensional approach whereby source materials from history, economics, soil science, geography and other fields are brilliantly marshalled and interrelated. But besides being an exemplary case study in the
interaction of history, physical environment and agricultural technology, this book represents a watershed between narrowly conceived ethnographies and the flood of verbose and ill digested post-war 'technology-and-social-change' monographs that are wont to aim high and hit wide. Geertz, with commendable brevity, has been able to cast widely his theoretical dragnet, and this, supported by a methodology both ingenious and rigorous, gives us a new genre of focused, empirical data presented in such a way that every stage is an indispensable logical continuation of a sustained analytical argument. The dialectic cohesion of the diverse materials presented for consideration is excellent and the strength of its logic should mark this as a model of comparative analytical writing for both ethnographers and sociologists for at least the next decade.

In a brief review it is not possible to list the author's major hypotheses and the tests of validation whereby he reaches the conclusions succinctly stated in the sixth chapter. There are, however, a few relatively minor weaknesses which should be noted. First, the term 'involution' is repeatedly used but is never precisely defined. In some places it refers to reversion, in others to undiversified intensification and in yet others to a ramifying elaboration of existing elements in a closed or static system. Secondly, Geertz takes as the polar centre or pivots of his comparative discourse the complex cane-sugar and rice-paddy ecosystem of inner Inner Indonesia and the plantation area of East Sumatra, with but a passing glimpse at the intermediate position of the Minangkabau in the Agam Highlands of West Sumatra. This yields rather striking contrasts, but they would be less striking if further regions of the Minangkabau Agam type were taken into consideration. Outer Indonesian rice bowls, such as Karo, Kerinci, Lembang, Redjang, Pasemah, Semendo, Ranau and Lampung in Sumatra, and South-west Celebes, all both self-sufficient in rice production and able to sell an exportable surplus to neighboring 'minus' areas, are of sufficient importance to blur the edges of Geertz's sharp contrast, a position which he finally admits towards the end of the book (p. 128). My third reservation relates to the prognosis that shared poverty—initially an 'inner Inner' phenomenon brought about essentially as a consequence of van den Bosch's mid-nineteenth-century Culture System—has in recent years embraced all Inner Indonesia (all of Java plus Madura, Bali and Lombok) and will, in due course (barring some major and at present unforeseeable reversal of the trend), include all of Outer Indonesia. Whilst this remains a possibility, I think it more remote than Geertz's tour de force would lead one to believe. On submitting the premises of his argument to close re-examination, certain errors of omission such as those noted above, and of inaccurate assessment, become pinpointed. For example, small-holder export-crop gardeners are not 'clustered in certain areas' (p. 129) and do not constitute merely 'a small minority of the Outer Island peasants' (p. 121). On the contrary, they constitute a majority of farmers in Outer Indonesia—where they grow the greater part of Indonesia's rubber, copra, pepper, cloves, coffee and vegetables.

My final remark relates to occasional lapses into rather diffuse value judgments such as a blanket description of Javanese village communities as 'dispirited' (p. 129) and 'hapless' and the characterization of their present prospects as 'discouraging' (p. 144) and 'melancholy' (p. 146). The criteria for such evaluation certainly do not belong to serious sociological discourse and come perilously close to current Eurocentric assessments of the highly mobile and labile condition of Indonesian society. History does not repeat itself but it would be unwise to repeat the assessments and judgments made by Western writers with regard to Russian society in the nineteen-twenties and early thirties, or Chinese society in the fifties.

M. A. JASPAN


This short monograph deals mainly with painted decoration on barkcloth garments collected at or before the turn of the century on the islands of Borneo, Celebes, Ceram and Halmahera, and stored today in Dutch museums. The purpose of the study is to interpret, as well as to describe, this material.

The 'function of ornamented barkcloth' is the main concern of the first of the three sections of the monograph, titled 'The Social and Ceremonial Aspects of Ornamented Bark-Cloth...' This section contains what the author feels is his main contribution, the other two parts (on the 'technical aspects' and on 'styles and style areas') being essentially supporting or background material. 'Function' is understood by Kooijman in a broad sense, and overlaps what Linton would have called 'meaning.' In spite of gaps in the data, Kooijman establishes that in all areas investigated, painted barkcloth was linked to head-hunting, and helped rank the wearer in terms of prestige acquired in that activity. Further links, on Celebes and Ceram, are seen with sun-worship and, for western Ceram, with a dual system that appears in other guises at other levels in the culture.

The section on styles guides the eye and the mind through numerous illustrations of designs and design elements. Here it is definitely 'meaning' that Kooijman is after, for while symbolism related to function is ubiquitous in the material, it is evident that, on many pieces (for example, women's jackets and hats), decoration merely indicates pervasive themes in the culture, not the wearer's status as related to head-hunting. This chapter might have been improved had the design elements and their inferred meaning been separated more forcefully: this would help future comparative work, and disentangle fact from conjectures of varying probability.

Geographically, style features and function alike suggest that the eastern islands form a unit, contrasting with Borneo. This fits well conclusions which I have reached on the basis of manufacturing techniques, tools, and other traits of the complex (excluding style of decoration).

The section on techniques of manufacture is perhaps the least satisfactory. It justly brings in Javanese paper-making, an industry which is a close relative of Celebes barkcloth-manufacture, and summarizes techniques used in applying decoration. However, Kooijman appears to be in error in ascribing fermentation to procedures on Halmahera, Buru, and Borneo. While soaking before beating is widespread and, indeed, of world-wide occurrence, fermentation as a distinct and purposeful operation is found, to my knowledge, only on Java, Celebes and perhaps some islands of Polynesia.

Minor shortcomings aside, Kooijman's paper is a welcome study of tangible ethnographical materials. As such, it shows a welcome concern for the as yet unfashionable. Studies such as this one, and others dealing with other Pacific areas, promised in the introduction, should help combat a widespread and fatuous illusion: that it is the short-sightedness of the Western traveller that causes our museums to be filled with objects for which most modern professionals cannot seem to find a significant use.

PAUL TOLSTOY


Changes that have taken place in China since 1950 have brought archeology into the limelight. Hundreds of students have been trained and organized into working parties. They have been digging in every province, exploring old sites, discovering new ones and producing reports, mostly by group discussion, with bewildering rapidity. To popularize the results of these activities it has been a common practice for each province or region to publish a selection of the important and interesting new finds.

The volume under review is one of these publications. It is devoted to Inner Mongolia and covers a collection of 184 specimens. The collection includes two paleolithic fossils, 26 neolithic artifacts, 20 Chou metallic objects, 41 Han pottery, bronze, wood and bone specimens, 14 silver, gold and bronze examples from the Six Dynasties, seven specimens of Tang pottery, stone and silver wares, 37 specimens of Liao and Chin pottery, bronze, silver and stone, six pieces of Sung porcelain and 31 Yuan relics in pottery and
porcelain, bronze and gold, stone and jade. A large number of the examples are inscribed and dated. The plates are clear and distinct and many of the specimens appear in more than one view.

The introduction, written both in Chinese and in Mongol, gives a brief account of the archaeological works conducted in this region and describes some of the key sites. It also stresses the fact that these cultural relics have thrown additional light on the written records of this outlying region. Students of Chinese archaeology and art will find this picture book enlightening and useful.

CHENG TE-K'UN

OCEANIA


(postage 11.)

Using as his central subject the sitting place (boti) of a clan (also called boti) in the village assembly house (maneaba), Professor Maude of the Research School of Pacific Studies at Australian National University has written the most comprehensive and useful description to date of the framework of a major division of the traditional social life of the Gilbert Islands, Micronesia, and shown how the social, judicial, political, and economic life operated within this structure.

The study is based on his long residence in the Gilbert and Ellice Islands Colony where the positions which he held until his retirement included those of Native Land Commissioner and of Resident Commissioner. Now he has drawn upon his first-hand knowledge, on published works, and on a long list of manuscripts which besides his own papers include those of the London Missionary Society, the American Board of Commissioners for Foreign Missions, the later Sir Arthur Grimble, and others.

Because so much of the old pattern of native social life has changed within the last century and one must depend heavily upon manuscripts describing an earlier period Mr. Maude refers to his study as an ethnographical reconstruction. Direct observation and interrogation in the present era would not be, he believes, yield sufficient data about the origin, development, and functioning of the clan. Prominent in the records that both he and Sir Arthur Grimble obtained in the Gilberts are the orally transmitted traditions of leaders of prominent clans. For his reconstruction Mr. Maude, to supplement his other sources, draws particularly on those sections of native tradition which relate to the time of man (as contrasted to the earlier era about the demigods and gods) which date from about A.D. 1400, a date based on genealogical computations, when the founders of the earliest clans left Samoa to settle in the Gilbert Islands. These narratives, and especially those from about A.D. 1650 to the time of European contact, Mr. Maude finds on comparing variants to show much agreement on the principal events although differing in details. Two major variants, one from Nukuani Island, the other from Beru Island, provide much information for the reconstruction of the form and functions of the clan and its sitting place as it existed in all but the geographically peripheral islands and those where local political factors caused numerous modifications of the more widespread form. Mr. Maude's careful but inspired use of native traditions shows how much about the processes of change in the clan can be learned from this type of oral material, and demonstrates, as he hoped it would, that one can obtain a true, or at least fuller, picture of a culture, or a part of it, through ethnographical reconstruction than by direct field observation and interrogation alone.

Using this variety of published and unpublished sources, Mr. Maude finally concludes that the number of original clans from Samoa was increased by the processes of conquest, fusion, immigration, and permission, and that the space in the village community house was repartitioned repeatedly to give each clan a definite sitting place in the when the clans of a maneaba district or the whole island assembled in ceremonies. The clan, exogenous, totemic, and patrilineal, included people descended from a common ancestor in the male line. Each clan originally lived in family homes (muenga) in a hamlet of its members (kainga) near the community house and other clan hamlets. When the kainga, associated with ceremonial and religious value to the clan, became too crowded, the overflow of members settled in the district in secular areas called kava, a term also used now for villages. When enclosed in a fence, the clan hamlet was called an O (fence). Varied as the functions of the boti (clan) were in Gilbertese life, land control was not included except for the kainga land and the large ponds and other properties too important to be managed and owned only by a kindred (utu). Most land was individually owned and inherited through the kindred.

The analysis is greatly clarified by plates, figures and a map. Four appendices succinctly present much additional detail on (1) the history and use of a large Nikunau pond, (2) the ceremonial distribution of food in two ceremonies in a major community house, (3) two special types of relationships, the tinau and the ciriki, and (4) the utu. Further, the main part of the study is helpfully divided into sections and subsections so that the main features of the author's presentation can be seized amidst the wealth of illustrations from traditions and field experiences.

The very richness of content may make reading this essay hard going for those completely unfamiliar with the traditions filled with the names of gods and ancestors who figure so prominently in them, but the effort is rewarding and the study is indispensable for filling in gaps in our knowledge about the place of the Gilbert Islands in the social organization of the Pacific. It is to be hoped that Mr. Maude will make the same kind of study for the village community house and its district as well as for the kindred, and lay out, as he has here for the clan, the origin and development of their major framework and operating processes. Only then will it be possible to fit in such later information as can still be obtained by fieldwork in the archipelago.

However, studies should also be made concurrently of local variations in structure and function of the social organization. There may be danger in reconstructing an ideal or classic form, or one that holds principally for one or two islands, and then interpreting deviations from that form in other islands or in different districts in a single island as incorrect, incomplete, and unworthy of intensive study. Also, what continues to operate in the present should, it seems to me, be regarded as more than a decayed survival of a past form; it should also be investigated as an adjustment to change that has meaning and validity in the present and represents native efforts to adapt older traditional beliefs and customs to bring them into harmony with the changes that have occurred since European contact and missionization. Otherwise, scholars of the future will be looking for manuscripts and memoirs about the present era to reconstruct the history of these adjustments, which as Mr. Maude has indicated led to the kindred becoming in place of the clan the 'sole recognized kinship.'

KATHARINE LUOMALA


The New Guinea Research Unit was established to co-ordinate research undertaken in the two Territories of Papua and New Guinea by departments of the Research School of Pacific Studies of the Australian National University, and to supplement their work 'by undertaking certain projects of an interdisciplinary character in relation to problems which are of both practical importance and scientific interest.' The Bulletin is to be published irregularly as field reports become available, the emphasis being placed on complete reporting of a given project rather than on consideration of theoretical implications of what are often related problems.

The first four numbers appeared between April, 1963, and May, 1964. R. G. Crocombe and G. R. Hogbin contribute accounts of a mechanical farming project in the Markham Valley, and of land tenure and use, other sorts of property, and the organization and productivity of labour among the Orokaiva. Crocombe alone reports on cash-cropping schemes among the Orokaiva. R. C. White discusses aspects of financial structure and policy in the two

This is the German edition of the Dutch original published in 1961. The author, anthropologist and missionary, has published elsewhere the results of his research work among the Tor people in the hinterland of Sarmi, North New Guinea. This little book is intended for the general reader. The writing of books of this kind by trained anthropologists is, indeed, a problem which deserves more and more attention from the scientists, for to my mind it is the function of anthropology in our modern society to convey understanding of non-European peoples and cultures to the broadest possible segment of our European society. In this regard the author has written an excellent book. The facts are soberly stated and well explained, and his style is admirably lacking in the tediusness of a scientific report, which could frighten away the very reader for whom the book was written. Additionally, however, it contains enough material to be of interest for anthropologists too. I have only a slight objection, to the term 'Naturvolk,' which, to my mind, should not be used any more, particularly in these popular reports. The term is a 'cultural lag' and its effect contradicts the very purpose of the book.

CARL A. SCHMITZ


In March and April, 1964, elections were held on universal adult suffrage for the legislature of Papua and New Guinea. This pamphlet contains a summary of the relevant legal provisions, a general introduction by Peter Lawrence, and brief accounts of the campaign and the reactions of the electorate in eight constituencies where anthropologists were at work at the time.

There was a common electoral roll, but ten seats were reserved for non-indigenous candidates (to be elected by all voters). The preferential voting system was adopted, as in Australia. These complications were assiduously explained to a largely indifferent electorate, many of whom supposed that voting was a kind of census registration.

LUCY MAIR


It has been a continuous source of surprise to me that the Solomon Islands should have been neglected in contemporary anthropological study. Although a number of American students have recently been at work, they have not yet published, and we are still dependent, with very few exceptions, on patchy studies. Santa Isabela, Choiseul, Guadalcanal, and large areas of Malaita and San Cristoval, to say nothing of the smaller islands, are still virtually unexplored terra incognita.

In this context, Dr. Hogbin's short and elementary study is welcome on two grounds. One is that it adds usefully to a series of inexpensive field studies available for the reading of first-year classes in anthropology. But the other is that it brings together in rounded form his observations on one Guadalcanal culture which he visited 30 years ago. Although some of this material has been presented elsewhere, the essay is a small but valuable contribution to knowledge. We now only wish that it had been one of Dr. Hogbin's major field enterprises, and that either he or other workers will give Guadalcanal the attention it deserves. It is, after all, now readily accessible, and easy territory to work in (as Melanesia goes) for budding Ph.D.ers.

CYRIL S. BELSHAW

BOOKS RECEIVED

The following is a list of books received from the publishers during May and June. Many of these will be reviewed in later issues of MAN.

GENERAL


AFRICA

Aldred, C., Egypt to the End of the Old Kingdom. London (Thames & Hudson), 1965. Pp. 143, illus. Price £1 10s. (cloth), 15s. (paper)


AMERICA


ASIA


EUROPE


Moustakas, C., The Internal Migrant: A Comparative Study in Urbanization. Athens (Social Sciences Centre), 1964. Pp. xii, 105, xv, plates, maps


OCEANIA


PROXIMITY PATTERNS OF THE URBAN JEWISH KINDRED*

By William E. Mitchell, Department of Psychiatry, University of Vermont

A primary interest of anthropologists is the whereabouts of men. We spend a good deal of time in the field just trying to find out where people are, where they have been, and where they are going. A related concern is to define the social and geographical boundaries of residence. We talk about people living, for example, in cities, villages, neighbourhoods, homesteads, bands and domestic groups. But our interest in people’s residence goes beyond the mere delineation of boundaries; we also want to know which individuals are living together and why.

The early ethnographers, working among primitive peoples, found that in many societies the newly married couple went to live in the household of a relative. Ties of kinship seemed to be the crucial factor in determining the place for postmarital residence. But the newlyweds didn’t live with just any relative. There was a culturally prescribed relative with whom they should live. This kind of residential expectation based on kinship was conceptualized as a ‘residence rule’ and has stimulated numerous taxonomic schemes and discussions, e.g. Adam, 1948; Ayoub and Lieberman, 1962; Bohannan, 1957; Carrasco, 1963; Fischer, 1958; Goodenough, 1956; and Murdock, 1957, p. 670.

Now helpful as the residence rule idea has been, it has tended to be a little muddled in its application. First, the geographical and social criteria denoting residence with kin sometimes vary in usage. For example, living with the husband’s parents and living near them can both be defined as living patriologically (Murdock, 1957, p. 670). However, it is obvious that the sociological and psychological consequences for a combined household are very different from those for separate but neighbouring households.

This also introduces the problem of how proximal or ‘near’ a married couple must live to kin to discuss their proximity in terms of residence patterns. Most of our profession’s conceptual distinctions are the result of fieldwork among small groups whose subsistence is based on agriculture, herding, and hunting and gathering. These are not urbanized or industrialized groups. Their technology, by comparison, is extremely rudimentary and simple. Communication is face to face and a person’s social life is usually restricted to his immediate locale and the distances his own legs can carry him. But in our comparative analysis of residence patterns, anthropologists yet have not fully realized the importance of technology as a structuring factor. In the urban-industrialized setting where our research was carried out, kin could live miles apart in different sections of New York City and still be in instant contact via the telephone. Some of our most interesting data are on the frequency and patterning of telephone contact with kin. Subways, buses and cars also make for easy and rapid contact with kin. Thus the sociological meaning of residence

* With four tables

patterns and geographical distances among kin cannot be understood apart from the society’s technology facilitating communication. In terms of miles what may seem a considerable distance to an Arapesh native might be regarded as ‘conveniently close’ to a native New Yorker. Secondly, it is only recently that ethnologists have recognized that an analytical distinction should be made between a culturally ‘ideal’ residence pattern, i.e. where couples should live in proximity to kin, and the ‘empirical’ residence pattern, i.e. where they actually live in geographical proximity to kin. The theoretical point that the present paper will make is intimately related to these problem areas.

The question that the classic residence rule asks is ‘With which relatives, if any, should the married couple make their home?’ In a society where they are not expected to live with relatives, we generally state that residence is ‘neolocal.’ But there is a further implication explicitly stated by some writers, e.g. Murdock (1949, p. 16; 1957, p. 670), that the wedded couple locate their domicile independently of the location of their close relatives. This is where the trouble begins. It is one thing to say that the couple does not live with relatives but quite another to say that where they do decide to live is unrelated to the whereabouts of their close relatives. The inference is that with ‘neolocality’ there is no patterned behaviour in respect to residence and the proximity of kin. I would like to show that for at least one group in our own ‘neolocal’ society, the foregoing simply is not so.

The data to which I will refer are from a group of Jewish families living in New York City and clients of the Jewish Family Service. The information on their residence patterns is from a larger study of their kinship behaviour and beliefs sponsored jointly by the Jewish Family Service of New York and the Russell Sage Foundation. Out research methods included interviewing, participant observation and the administration of several questionnaires. The data used in this paper are from a lengthy kinship questionnaire returned by 182 married couples with the husband and wife each completing a separate questionnaire. Most of the couples are native New Yorkers, the children of Jewish parents who immigrated from Eastern Europe. These are couples still very much involved in the care and education of their children. Of the husbands, about 60 per cent. work in the ‘white-collar’ occupations, e.g. professional, managerial, clerical and sales, while the remainder primarily work as craftsmen and operatives.

On the basis of our early experience with this particular cultural group—especially among the families which we regularly visited for interviews—we began to wonder just how ‘neolocal’ they were. Although few had relatives living with them, many seemed to have close relatives living just around the corner. Also, the proximity to relatives was not fortuitous but planned. Of course, other
factors were considered when choosing a new place to live, e.g. proximity to jobs and stores, rental costs, the reputation of neighborhoods and schools, or even the availability of a garage for the family car, but still, proximity to kin was frequently the crucial one. We decided, then, to augment our residence inquiry by asking the more inclusive question of 'Where do the research couples live in geographical proximity to their kin?'

But we should first point out that there are some definite methodological problems in obtaining data on the geographical proximity of kin from informants living in an industrialized urban setting. To secure information from a large number of married couples scattered throughout a metropolitan area, we found the questionnaire technique the only feasible way. But it was not practical by this method to obtain data on every member of the individual's kindred, that is, on all those who are in some form of kinship relation to ego whether cognate or affinal. Our data, however, are on what we consider to be the couple's basic genealogical network, viz. the cognatic categories of mothers, fathers, aunts, uncles, sisters, brothers and first cousins. Grandparents were not included because few had immigrated and, if they had, it would be unlikely for them to be still alive.

It was also important to decide upon a set of geographical boundaries that was sociologically relevant for our population. In our interviews with informants we found that, although the various political and neighborhood divisions were of some importance, what mattered the most was the actual distance between the informant and his relatives. First we obtained data on the number of kin for each couple living in the 'New York City area.' Once this was known, taking the married couple's home as the geographical center, we looked for kin living (1) in the same household with the couple; (2) in the same building but in a separate household and (3) within 'walking distance' as subjectively determined by each informant. These are, of course, mutually exclusive categories. Ethnographically speaking, these were the important geographical distinctions and our questionnaire data were collected according to these criteria.

Our findings show that none of the 182 research couples is without relatives in the New York City area: 86 per cent. of the couples have at least 16 cognates within the metropolitan area and over half of the couples, 54 per cent. have at least 31 cognates in the area; 87 per cent. of the couples have at least half of their cognates in the area; only 4 per cent. of the couples have less than one-fourth of their cognatic kin in the New York City area. These data clearly show that the research couples reside in an area where many of their cognates also live. More detailed documentation of this finding is supplied by Table I showing the number of husbands' and wives' living natal kin in terms of specific genealogical categories and the proportion of these living kin residing in the New York City area. Table II presents comparable data for collateral kin while Table III is simply a summary of Tables I and II.

But do the research couples, in fact, interact frequently with these kin? Looking at our data from an interaction criterion, the couples as a group list 5,938 cognates that they see more frequently than at big family gatherings, e.g. such ceremonial ad hoc assemblages as weddings, funerals and Bar Mitsvahs. For purposes of analysis we call these kin that are seen other than at large gatherings 'familiar kin.' When the criterion of residential proximity is introduced, we further discover that 68 per cent. of these familiar kin live in the New York City area. Therefore, a majority of the kin with whom familiar contact is maintained are in the same general geographical area as the research couples. That close to a third of their familiar kin are outside of the New York City area, also indicates that the factor of geographical distances does not preclude the maintenance of close kin ties.

### Table I. Husbands' and Wives' Natal Kin Residing in the New York City Area

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</thead>
<tbody>
<tr>
<td>All</td>
<td>593</td>
<td>463</td>
<td>82</td>
<td>76</td>
<td>474</td>
<td>622</td>
</tr>
<tr>
<td>Mothers</td>
<td>105</td>
<td>88</td>
<td>84</td>
<td>89</td>
<td>85</td>
<td>95</td>
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<tr>
<td>Fathers</td>
<td>78</td>
<td>69</td>
<td>88</td>
<td>85</td>
<td>49</td>
<td>58</td>
</tr>
<tr>
<td>Sisters</td>
<td>201</td>
<td>153</td>
<td>76</td>
<td>74</td>
<td>171</td>
<td>232</td>
</tr>
<tr>
<td>Brothers</td>
<td>209</td>
<td>153</td>
<td>73</td>
<td>71</td>
<td>169</td>
<td>237</td>
</tr>
</tbody>
</table>

### Table II. Husbands' and Wives' Collateral Kin Residing in the New York City Area

<table>
<thead>
<tr>
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</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>2,653</td>
<td>1,672</td>
<td>63</td>
<td>70</td>
<td>1,431</td>
<td>2,049</td>
</tr>
<tr>
<td>Paternal</td>
<td>295</td>
<td>211</td>
<td>72</td>
<td>84</td>
<td>189</td>
<td>226</td>
</tr>
<tr>
<td>Maternal</td>
<td>318</td>
<td>230</td>
<td>72</td>
<td>73</td>
<td>193</td>
<td>263</td>
</tr>
<tr>
<td>Paternal</td>
<td>1,040</td>
<td>616</td>
<td>59</td>
<td>70</td>
<td>531</td>
<td>731</td>
</tr>
<tr>
<td>Maternal</td>
<td>1,002</td>
<td>615</td>
<td>61</td>
<td>62</td>
<td>518</td>
<td>829</td>
</tr>
</tbody>
</table>

### Table III. Husbands' and Wives' Cognatic Kin Residing in the New York City Area

<table>
<thead>
<tr>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>All</td>
<td>3,258</td>
<td>2,135</td>
<td>66</td>
<td>71</td>
<td>1,905</td>
<td>2,671</td>
</tr>
</tbody>
</table>

What we now wish to ask is (1) whether there is a general tendency for couples to live closer to either the husband's or wife's kin and (2) whether there are any specific relatives who consistently appear to be more geographically proximate.

Of the research couples 53 per cent. have kin living within walking distance. Of these, 52 per cent. have only wife's kin nearby; 26 per cent. have only husband's kin nearby; and 16 per cent. have both wife's and husband's kin nearby. The remaining 6 per cent. are divided equally among those
couples with only married children and husband’s kin nearby, only married children and wife’s kin nearby and only married children nearby. In other words, 70 per cent. of these couples have some wife’s kin residing nearby while only 44 per cent. have some husband’s kin nearby. In regard to the relative proportion of specific genealogical categories of natal kin residing within walking distance, Table IV shows that there is no marked difference except in the case of sisters where there is a higher proportion of wives’ sisters nearby than husband’s sisters.

<table>
<thead>
<tr>
<th>Genealogical Category</th>
<th>Walking Distance</th>
<th>Same Building</th>
<th>Same Household</th>
<th>Totals per cent.</th>
</tr>
</thead>
<tbody>
<tr>
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<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mothers</td>
<td>19</td>
<td>10</td>
<td>8</td>
<td>37</td>
</tr>
<tr>
<td>Fathers</td>
<td>23</td>
<td>5</td>
<td>6</td>
<td>34</td>
</tr>
<tr>
<td>Sisters</td>
<td>15</td>
<td>3</td>
<td>3</td>
<td>18</td>
</tr>
<tr>
<td>Brothers</td>
<td>9</td>
<td>3</td>
<td>1</td>
<td>12</td>
</tr>
<tr>
<td>Mothers</td>
<td>18</td>
<td>1</td>
<td>1</td>
<td>19</td>
</tr>
<tr>
<td>Fathers</td>
<td>19</td>
<td>3</td>
<td>6</td>
<td>28</td>
</tr>
<tr>
<td>Sisters</td>
<td>6</td>
<td>3</td>
<td>6</td>
<td>6</td>
</tr>
<tr>
<td>Brothers</td>
<td>6</td>
<td></td>
<td>6</td>
<td>6</td>
</tr>
</tbody>
</table>

Only 13 per cent. of the couples have kin living in the same building with them but in a separate household. Although this is a low proportion, we are still interested in discovering any regularities within this group. Among these couples, the tendency to be geographically close to wife’s cognates is even more dramatically emphasized: 84 per cent. of these couples have only wife’s kin in the building; 8 per cent. have both husband’s and wife’s kin and 8 per cent. have only husband’s kin. Therefore, 92 per cent. of these couples have some wife’s kin in the building, whereas only 16 per cent. have some husband’s kin in the same building. Table IV indicates that only 4 per cent. of the husband’s parents live in the couples’ building in contrast to 15 per cent. of the wife’s parents. Furthermore, none of the husband’s brothers and sisters live in the same building with them.

The final proximity boundary is for kin living in the same household with the research couples. At the present time, only 8 per cent. of the couples reside in a household with relatives. Of these couples living with kin, 93 per cent. have only wife’s kin in the household; the remaining 7 per cent. have only husband’s kin. None of the couples have both husband’s and wife’s kin living with them. Table IV also shows that the couples have combined households only with their parents. None have siblings present. Also, the absence of a husband’s mother in her son’s home again indicates the importance of the feminine cognatic tie.4

On the basis of these data, let us now look at some of the residential proximity patterns which they reveal. First, all of the research couples have some cognatic kin residing in the New York City area while a majority of the couples have a majority of their cognatic kin residing in the area. When more proximal boundaries are introduced, 60 per cent. of the couples have kin residing within walking distance or closer. A second pattern is that these couples tend to live close to either husband’s kin or wife’s kin but not to both. Thirdly, they are much more likely to live close to the wife’s kin than to live close to the husband’s. Furthermore, the closer the kin live to the couple, the more likely are they to be the wife’s kin. From Table IV we can also discern an overall pattern of the relative proportion of natal kin that are within walking distance or closer. Arranged in rank order they are: wives’ mothers; wives’ fathers; husbands’ fathers; husbands’ mothers; wives’ sisters; wives’ brothers; and last, husbands’ brothers and sisters. Thus parents precede siblings while wives’ parents precede husband’s parents and wives’ siblings precede husbands’ siblings.

Here we should emphasize that these residential proximity patterns of the research couples’ kindreds are based on the empirical facts as reported directly by them. In this paper we have not dealt with their beliefs or attitudes about the proximity of kin. Our purpose has simply been to document the fact that, although these couples for the most part do live alone, the proximity patterns that we have just presented indicate that they do not locate independently of the residential location of kin. One wonders for what other ‘neolocal’ groups the same might be true. Finally, we would suggest that our thinking about ‘neolocal residence’ may need to be revised, as some of our older theoretical assumptions about ‘bilateral’ or ‘cognatic’ kinship systems are being revised, to fit the data and insights of modern ethnological research.

Notes
1 Members of the research staff included Fred Davis, Hope Leichter, Judith Lieb, Alice Liu, Candace Rogers, and Dianna Tendler. The data in this paper were analyzed jointly with Dr. Leichter although the responsibility for this particular theoretical interpretation is mine alone. This paper, in a similar form, was presented at the 62nd Annual Meeting of the American Anthropological Association, San Francisco, November, 1963.
2 A detailed discussion of the sample appears in Leichter and Mitchell (1965).
3 The term ‘kindred’ is currently a controversial concept, but the usage adopted for the present research is presented in two related articles (Mitchell, 1965 and 1963).
4 Acknowledging that ‘residence patterns are the crystallization, at a given time, of the development process’ (Fortes, 1958, p. 3), this paper, as noted earlier, is primarily concerned with the residence patterns of married couples with school-age children still in the home. Thus, although only 8 per cent. of these couples are currently living with kin, during earlier phases of the domestic development cycle, 61 per cent. of the couples resided with kin. This may be, in part, the cultural persistence of the Eastern European tradition of kest in a modified form. The kest is an arrangement whereby a young bridegroom-scholar and his bride live with her parents (Zborowski and Herzog, 1952, p. 82). I prefer to believe, however, that the structural prevalence of the feminine cognatic tie in our Jewish sample relates more to the incompletely understood function of feminine roles in urban-industrialized societies, since the importance of the feminine kin tie is a striking regularity of recent western kinship studies, e.g. Boll (1957), Firth (1956), Townsend (1957), and Young and Wilmott (1957).

References
Adam, Leonhard, ‘Virilocality and Uxorilocality,’ MAN, 1948, 11.

139

Bott, Elizabeth, Family and Social Network, London (Tavistock), 1957.


Murdock, George Peter, Social Structure, New York (Macmillan), 1949.


‘Le bâton de commandement.’ By Leon Underwood. With five figures

143 An object of utility has been before the scrutiny of scientists for the best part of a century, relegated to the limbo of incomprehensibles under this title of ‘magic wand.’ This stigma discouraged inquiry into the understanding of its practical purpose, beyond a speculative suggestion that it might have been used as a lever to straighten the shafts of javelins under heat. It still stands as a magical implement, used by hypothetical ‘sorcerers’ to wave plentiful herds into existence on the hunting fields of the Stone Age.

Prehistory flows from that underground source of artifact and art first tapped over a century ago. But even at this time the cultural judgment of the West was that imbalance which is characteristic of decline. In that state of spiritual starvation, the voice of subjective judgment was denied a hearing and science spoke for the whole cultural life of prehistory. Science qualified by only 50 per cent. of the dual intelligence responsible for art. Cave art was an enigma to the objective mind. Such idealization of nature had already been banished in the decline of the West, for the realistic ideal of the photograph. The loss of Greek idealism to Roman materialism was being reiterated, in the loss of Renaissance idealism. The succession of styles in rise and decline does not vary; the import of subject matter is the variable.

Scientific judgment on the value of prehistoric art, pronounced by the half-knowledge of the established authority in the School of Prehistory at Toulouse, claimed for the objective half a whole importance. ‘Magic’ became the label on a pigeonhole containing whatever material resisted analysis by objective judgment.

The characteristic of decline is that the dual understanding which was synthesized in the cultural rise divides against itself: objective knowledge (Science and Technology) divides against the tenets of belief (Art and Religion). It is not until Technology has taken the necessary time to exploit the new sources of provision (revealed by Science) that the confidence able to end division and restore a synthesis of knowledge permits the resurgence of a culture.

Sauvola, the discoverer of Altamira, recognized his discovery at once. A Spanish gentleman, a ‘man of parts’ born out of his time in a phase when intuitive judgment was discreditied by science, his intuitive-intellectual balance was as that of a Leonardo or a Michelangelo born out of time and place—isolated by his distinction. His intuitive recognition of the mural art of Altamira was totally rejected and ignored by the Toulouse School of Prehistory for 15 years. In the loneliness of his personal distinction, Sauvola’s balanced judgment had recognized the timeless significance of Altamira: but his presence was that of the one swallow that does not make a summer.

Prehistoric mural (‘parietal’) art was announced by Sauvola and Vilanova in 1880, and not accepted by the School of Prehistory at Toulouse until the beginning of this century—20 years later. Altamira, initially discredited by science without due consideration, was recognized only when similar prehistoric murals had been discovered in the French caves of La Moutie, Dordogne, in 1895 and Pair-non-pair, Gironde, in 1897. And ever since, the succession of leading savants of the School of Toulouse have insisted on the purpose of prehistoric art as ‘magical.’

As an artist and a student of prehistoric examples, I totally rejected the explanation of ‘magic,’ when I first heard it on my visit to the caves of France and Spain in 1925. Until recently I was unable to bring forward demonstrable evidence for my contrary view: that the visual form of prehistoric art has the conviction of the same belief as Classical or Renaissance art, but that the connexion of its subject matter with prehistoric life was obscure, on account of our loss of the technological know-how of those days. The loss of know-how is simply explained. Science may classify tools by their static and inert form; but these were invented and evolved only by the experience of their function, which is a faculty of the intuition or subjective judgment, stored as practical experience in the memory. Here is the link between art and artifact. Now, if we consider the surviving forms of pick or chopper (‘coup-de-poing’) in our descendant modern tools, objective judgment can easily identify them. The array of modern tool forms, however, presents no example of striking resemblance to the bâton de commandement. The surviving variant form still in use on the edge of civilization is obscurely related by a connexion which has somehow escaped notice. But, although the ancient tool form was not recognized, the evidence of its use was there to see, in the cave drawings.

The static realism as idealized in the drawing of Altamira is, stylistically, anterior to the dynamic realism as idealized at Lascaux. Between them lay an evolution in hunting-weapon range, which the bâton—as a spear-thrower—explains. The art of Altamira, when viewed as against the art of the historical past as a whole, is seen to be in the same phase on its cycle of style as the Elgin Marbles, and the Sistine Chapel paintings, on theirs. This prehistoric art cannot then be held to be ‘magical’ in purpose, unless Classical and Renaissance art are too. And this would give another meaning to magic.

What prompted the labelling of a tool form as a ‘magic wand’ in the first place? Was it the engravings of animals on the shaft? If so, then the term ‘magic’ was used to mask the true value of art—which is not in the currency of science.

Before describing what a reconstruction of the ‘magic wand’ revealed, a brief account of my approach to it will be given. As in my previous experiment with bronze artifacts of history (see MAN, 1958, 13, 39, 64), I attempted to re-enter the bygone and forgotten practical experience of Stone Age technology. The experiment amounted to no more than the knowing of four flint flakes, without previous experience, and the use of these to make a
wooden modelling tool, such as I had much experience in making with modern steel tools.

The experiment confirmed the existence of a connexion between the evolution of tool forms and the evolution of styles in art (or rather, of the subject matter of belief, which is what dictates style). The account of this experiment and conclusion was published in the Annual Report of the Royal Society of British Sculptors for 1961 as 'The Cycle of Style in Art, Religion, Science, and Technology.'

The loose and irresponsible relegation of a practical tool form to that oubliette of magic has hindered its recognition as one of the earliest and greatest of our ancestors' mechanical inventions. Had the bâton been more honestly labelled 'a tool form of purpose unknown,' then the pitfall of 'magic' could have been avoided—not only for this artefact, but for the whole body of prehistoric art too which was also cast into the limbo after it.

In prehistorical time as in all time since, culture depended on the evolution of tool forms. Hunting weapons and arms were developed in the continual pursuit of increased range. The step-by-step succession of increase in range relates directly to the rise-and-fall wave pattern of the cycle of style in art. Natural law being revealed and demonstrated, technology exploits it, in pursuance of the constant purpose of survival by keeping 'one step ahead'; and organized belief elects new subject matter for communication to the faithful by the visual imagery of art. This accounts for the change of style: the cultural rise and decline moves on the principle that the balance of means with ends is unstable. Renewed means keep the cycles turning.

Changing style is instigated by changing subject matter of belief, and not by the caprice of an art market—as, in the present decline, vested interests would have it. The religious belief expressed in the hunters' murals was vested in the power of Providence and manifested in the science and technology of tool form and function. Artifact-production is led by the intellect; tool function, and the production of art, are led by the intuition.

With this understanding of the relationship of art to artifact, we can now turn to the practical purpose of the bâton de commande.

When revisiting the caves and museums of France in July of this year, I was fully aware of a great change in the stylistic treatment of subject matter, caused by tool forms unknown. The evidence was incomplete. The great change in style of the Magdalenian period in the art of Lascaux indicated the existence of hunters' weapons of increased range—unknown, or unidentified. My visit to the Museum at Périgueux was to provide just this missing link.

In the Upper Gallery of the museum I examined a representative collection of plaster casts of the so-called magic wands. None of the originals had escaped breakage and erosion in part. It seemed impossible to reconstruct one in 'mint state'—working order—even from parts of every specimen in the collection. The one feature that was most striking in all specimens was the presence of one or two holes bored through the antler at the widening of the juncture of tine with main stem; those holes that were responsible for the vain attempt to rescue the object from the limbo of 'magic' as a 'spear-straightener.' Disapproving of that idea, I descended to the Lower Gallery.

There, in a wall case, my eye lit on an Eskimo spear-thrower; particularly, on the hole bored through its wide end. The museum director was good enough to open the case so that we could examine it, and both of us tried out the grip—which was indicated unmistakably by the form. The hole accommodated the first joint of the forefinger in a hook grip, while the second, third, and fourth fingers wrapped around the handle, in deep grooves provided for them; and a groove on the other side of the handle fitted the ball of the thumb. (Four Eskimo spear-throwers were illustrated by Bushnell in MAN, 1949, 160.)

In throwing spears by the aid of this implement, the fingers hold the implement while the thumb holds the spear on to the spear-thrower, with a cupped recess on the end of the shaft engaging with the nipple on the end of the spear-thrower. Such a firm union of the two is essential in the hasty taking of aim with the vigorous exertion necessary to propel the spear to its maximum range.

FIG. 1. THE HUNTER'S ACTION WITH SPEAR, SPEAR-THOWER AND BOW

The bâton or spear-thrower extends the range in Aurignacian days, as an intermediary stage between hand-thrown spear and Bowman's arrow. A, Early one-hole type called Aurignacian; B, one-hole type (later); C, two-hole type; D, lighter and definitely later type with slit-like hole for thong to pass around the wrist; E, grip on hand-thrown spear at point of balance on shaft; F, grip transferred further back by the use of thrower; G, the point of drive (as at E) is taken back to the rear extremity of spear-throw; H, use of the sailor's becket from Melanesia, serving the same purpose as cup on spear end and nipple on thrower.

It occurred to me that this Eskimo spear-thrower might be a descendent form of the prehistoric bâton, taken northwards as the latest glaciation receded by the Eskimo forefathers pursuing reindeer. The Eskimo implement may be a direct descendant of an ancient form, or a separately evolved parallel; but in either case, it seemed of little importance to the main issue—the connexion of art with artifact, on the first cycle of its appearance. The surviving prehistoric specimens of the bâton were indeed mere ruins, but I determined to make a reconstruction model in the studio on my return to England. Here was a prospect of finding out whether the magic bâton was introduced by the Aurignacian hunters to extend the range of their hand-thrown spears; indirectly giving rise to the abandonment of that heroic static in their style at Altamira and its replacement by the dynamic scurrying action of flight in the subsequent style of Lascaux.

In this altered light, the 'magic' attributed to the bâton by the School of Toulouse began to take on quite a different connotation. As a tool form the bâton would seem to have met a need far more urgent than 'sorcerers' magic,' and of incalculable antiquity. The ancient hand-held coup-de-poing having first been fitted to a heavy shaft for lunging, it was later fitted to a lighter shaft for throwing; and then, it seemed, came a magic from this bâton to extend the range again.

I made wooden models based on two examples; the one-holed form from La Madeleine and the two-holed from Laugerie Basse (see, e.g., illustrations on p. 6 of Encyclopédie par l'image, Paris, Hachette, 1930). In the superior grip of the prehistoric spear-thrower, forefinger and thumb were both free to encircle the shaft and hold the spear. The nipple at the end of the thrower was a
feature too fragile to survive, on any of the time-battered specimens that we have. I was agreeably surprised to find that the grip on my reconstructed models gave a very much better control in the hold on the spearshaft, than did the Eskimo spear-thrower; the Eskimo design, however, was limited, as might be expected been used by forearm thrust alone, to propel much lighter projectiles. The ornate and fragile examples of this kind are clearly ceremonial forms, symbols of the social status of a leader—sceptres, in fact.

Between Eskimo and reconstructed prehistoric forms, the points of comparison are the hole for hooking the fingers through, and the nipple at the end to engage with the cup on the spearshaft so as to give rear drive—extended range with more accurate aim. The points of difference are that: in the Eskimo type and the earliest prehistoric type, the position of wrist and forearm are weaker, and the grip on the spearshaft less firm; the design in straight-grained material like wood leads to a wrist position of horizontal

from the structural inferiority of driftwood as a material compared with reindeer antler. The form of the spear-thrower carries the implication that the action of using it is vigorous, like the overarm action of bowling at cricket. In contrast, the lighter and more ornate Magdalenian spear-throwers, already familiar to us, may have

section; in the improved prehistoric designs, antler being of more integral structure, the form permits the wrist and forearm a stronger position in action (with the wrist in vertical section), and better control on the spearshaft (with forefinger and thumb opposed).

The body of accepted evidence on tool form has hitherto not been sufficient to connect in a chain of increasing range with the changing styles of prehistoric art. The scientific evidence on tool form has been limited to the comparison of the penetrative power of the improving forms of flint points. But the flint point was such a small part of the hunter’s equipment that it did not represent support for the degree of stylistic change which the drawings

FIG. 3. RECONSTRUCTION MODELS OF THREE TYPES OF ONE-HOLED (A, B) AND TWO-HOLED BÂTONS (C)

Photographs by John Underwood, 1965

FIG. 4. THE RECONSTRUCTED SPEAR-THROWERS AND THEIR GRIPS
evince. An ample correspondence of the evolution of art with the
evolution of artifact was needed, to rescue the purpose of pre-
historic art from the oubliecle where modern science had heed-
lessly consigned it; and this practical function of spear-thrower
being ascribed to the misnamed *bidon* it fills in a lacuna in our
succession of technological evidence.

![Image of Eskimo spear-throwers, back and front view, and their grip](image)

**Fig. 5.** Eskimo spear-throwers, back and front view, and their grip

The bow-and-arrow of longer range was preceded by the light
propulsors of Magdalenian attribution; both these and the earlier
*bidons* drove the projectile forward from its rear end, as a great
improvement in range and accuracy on the drive of a hand-
thrown spear from the mid position of balance on the shaft. The
use of the *bidon* as a practical tool form can be extended before
the Magdalenian period into Aurignacian times. It gave a great
increase in range over that of the hand-held spear, while also im-
porting the necessary improvement in the hunter’s aim. It was a
true artifact, an inorganic extension of organic function, im-
proving on the function of the hand.

The development of flighted projectiles, as shown among the
mural animal paintings of Lascaux, may well have been begun
before the device of the bow-and-string as mechanical propeller
(see fig. 1).

From Melanesia comes a related design: a self-releasing knot,
or sailor’s becket, conducting the forward pull of the thrower’s
hand back to the rear end of the spear (see B. A. L. Cranstone,
This example suggests that the principle of end drive was ex-
ensively recognized as improving both range of weapon and accuracy
of aim.

The three types of prehistoric *bidon* are evidence of a long period
of slow improvement, from (A) the plain and earlier Aurignacian
through (B) to (C); covering its use up to the end of the Magda-
lenian period (see fig. 1).

There is a four-hole type of spear-thrower, which has appeared
in only one example (as illustrated in E. Lartet and
H. Christy, *Reliquez Aquitanier*, London, 1865–75). This example
appears to be most closely connected with the Eskimo spear-
thrower; and might, therefore, be responsible for it, in the event
that the Eskimo were descended from the prehistoric hunters of
Europe, as suggested above.

On the whole evidence, it now seems safe to assume that the
ends of thrown spears and lances were fitted with feathered flights,
as depicted in the murals of Lascaux. The feather flight and end
drive were features evolved before the bow, to which they made
indispensable contributions. The purpose of the feather flight is
original to that of the rifling of a gun barrel: to prevent the missile
from turning over and over in flight. The exertion of throwing
with the *bidon* would otherwise tend to make the unflighted spear
turn over and over in the air.

Leaving aside other prehistoric modes of hunting, on which the
evidence is too slender and hypothetical, the ‘magic wand’ alone
may now be recognized as an important lost link recovered in the
endless chain of technological evolution—the range race in which
our remote ancestors were committed as inexcusable as we are.

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**Notes on Boys’ Initiation among the Ngulu of East Africa.**

*By Dr. T. O. Beidelman, Laboratory of Social Relations, Harvard University*

In a recent paper (1964a) I have discussed the symbolic aspects of certain female initiation ceremonies among the
Ngulu of eastern Tanzania, East Africa; other papers on female
initiation are in preparation. Unfortunately, my Ngulu data on
male initiation are much more limited. I provide this infor-
mation here! in the hope that it may serve as supplementary
information to the data on Ngulu female initiation and that it
may also be of some use in the comparative study of ritual in
Bantu Africa, a study at present enjoying a wide revival of interest.

The Ngulu are matrilineal, Bantu cultivators living in south-
western Handeni and northern Morogoro Areas of eastern
Tanzania, East Africa. I have provided elsewhere an outline of
Ngulu society, as well as a bibliography of previous publications
on this people (1964a). I therefore make no attempt here to
discuss many of the implications of the symbolism involved or
the relation of these to Ngulu social structure. Some years ago,
the late Hans Cory published his findings on Ngulu (Ngusi)
initiation (1944: 1956); my own findings often disagree with his
and many of his texts appear to me, and to my Ngulu informants,
to be unintelligible or very garbled. However, these have
proved useful; I incorporate them into this paper, making textual
alterations where these seem necessary; I indicate these emendations
wherever they involve more than orthographical changes.

Ngulu boys are initiated some time between the ages of about
12 and 16; they undergo circumcision and some hazing and are
taught various songs and riddles providing traditional lore not
only about sexual relations, the nature of the sexes and the
problem of birth, but also about other values and traditions not
directly associated with these. Boys must be initiated before they
can assume adult responsibilities. Although they may be initiated
either before or after they show signs of puberty, today, at least,
most boys are circumcised before they have reached sexual
maturity. I was told that a sexually mature youth who was not
circumcised would be refused by women. While young boys
might go about naked on occasion, circumcised youths and men
must take care not to expose their genitals. In the past, they wore
grass or bark kilts; today men wear European or Swahili style
clothing.

Boys’ initiation ceremonies are carried out every year in most
parts of Ngululand, although there is no rule prescribing that this
be done. Sometimes ceremonies are held for a single boy but this
is very rare; usually a number of boys, perhaps a dozen, from
the same area are initiated together. Their fathers share the obligations
of providing food and beer to celebrate the various ceremonies;
they all share in providing a fee to the circumciser and hospitality to the various men who help him in tending the boys until their recovery. Boys’ initiation is always carried out in the dry season, preferably after harvest when there is much grain for brewing beer (cf. also Dooley, pp. 6f). In the past, these ceremonies required provisions of much meat, domestic or wild, which was often a serious burden to those sponsoring them, but such feasting is no longer observed in all cases. On the day of circumcision a dance and beer drink are held at the village of the father most prominent in arranging the initiation.

During the time of initiation a boy (or girl) is called mwali, pl. wali. Commonly the term means ‘girl’ but in the context of initiation, it refers to young people of either sex, and I therefore render it here as ‘novice.’ The series of ceremonies and the circumcision itself are all referred to simply as kumbi or lago, although, more precisely, these terms refer to the hut and bush camp, respectively, where the novices are quartered during these ceremonies.

The purpose of initiation is to make a boy clean, both physically and morally. It is to teach him orderly, proper behaviour. The Ngulu say that after circumcision, the glass is no longer moist (sika) because it is now exposed. The term sika means not only ‘moist’ but also ‘low’ in both the physical and the moral senses. Ngulu sometimes remark that one of the most important differences between men and women is that after initiation, men are clean whereas women are periodically unclean because of menstruation. Not even initiation makes them entirely clean and orderly. This ‘uncleanliness’ of women is associated with a certain moral weakness and lack of order (cf. Beideman, 1964a; 1964b, pp. 39-41). One can therefore say that the rules and knowledge provided by the elders to the novices are only the moral complement to the physical order already achieved by the circumcision itself.

II

On the day chosen to begin initiation, the various boys are collected by the assistants (makungu) of the circumciser (mwana-bakwa). Their heads are shaved (their bodies, too, if, as rarely happens, they have reached puberty) and they are then escorted to the initiation camp (lago) in the bush (nyika) far from any settlement. There a small hut (kumbi) has been built to shelter the novices. The shaving is said to be cleansing, again both in the physical and moral sense. Ngulu associate pubic hair and long, unkempt hair on the head with uncleanness and disorder, with an ignorance of proper behaviour.

One by one the novices are fetched out of the hut and circumcised. When the circumciser fetches a boy, he puts a string of white beads (king’ola) about the neck of the novice. After the boy has been circumcised, the necklace is removed and placed on the next one to be cut. After all have been circumcised, the necklace is given to the grandmother (maternal?) of the boy whose parents were most prominent in making the various arrangements for the initiation. Ngulu said that the white beads stood for the brightness and fortune which were hoped for the novice after he had recovered. White (azelula) stands for cleanness, strength, normality and masculinity (cf. Beideman, 1964a).

Ngulu say that a boy’s father can never circumcise his own son, nor should be closely associated with those involved in the actual operation. This seems to be related to the general Ngulu practice of rigorously separating adjacent generations, viz. those involved in authority relations, from all aspects of sex or informality. Thus, too, a Ngulu parent should never instruct his or her child in sexual lore, whereas a grandparent or age mate should. Ngulu sing: The small thing defeats the father (trying to) pull it out, oh! (Luwandende Isaminda tate, kung’ola, eee!). By this they mean that a boy’s father cannot cut off the foreskin (small part) of his son.

The circumciser is a doctor (mganga) in the sense that he possesses various medicines which protect his patient from both physical and supernatural dangers risked in such operations. After he has circumcised a boy, and he has applied his medicines, the circumciser gives him two eggs and some maize flour, which are said to represent the novice’s parent. The eggs are said to represent the boy’s mother and may be associated with stones (Beideman, 1964a), as well as having a more obvious relation to fertility and parturition; the flour is associated with the boy’s father. The whiteness of the flour seems to be the attribute governing its selection here, although maize itself also has certain masculine associations for Ngulu; maize flour is used in other Ngulu ceremonies to represent semen (Beideman, 1964a and maize cobs are used to represent the penis. These eggs and the flour are kept until the novice has recovered and are then buried in the bush by the boy’s instructors, along with the other goods used during the initiation. Maize flour is also sprinkled on all those who enter the initiation camp to visit the boy. The boys themselves are often sprinkled, too, with fine white ash. Here the symbolic attributes of whiteness and dryness are both associated with normality, with order, with cooling (khuza) the person. In many Ngulu ceremonies objects are applied in order to cool a person, to return him or her to order and normality, heat (moto) being associated with passion and disorder. Thus, too, a person who had killed another or had killed a lion was, in the past, also sprinkled with flour to cool him. There is further parallel in that the novices, too, are said to die and then to be reborn after the initiation, so that the circumciser is sometimes compared to a killer of a lion. Here, of course, there is a further symbolic association between the bush, the uncontrollable, and the wild and dangerous creatures in it (cf. later), and the novice’s ignorance and uncleanness, which are removed through instruction and circumcision. Water is also sometimes used in ceremonial cooling. On such occasions, it is not the liquidity or wetness of water (which are feminine, disorderly attributes) that are significant, but water’s coolness in comparison to the object on which it is poured.

One of the other expressions for circumcision is ‘to eat’, thus, the knife blade eats or devours (chadya) the boy. The idea of eating or consuming also has some negative associations with killing or subjecting a quarry, a connotation consistent with the usage here (cf. Beideman, 1964a). Thus, Ngulu sing: ‘The cutting of the knife’s eats (my) father and eats me’ (Kigela mankenya chadya tate name chadya). They mean that the father was circumcised and now it is the turn of the son; initiation is an expression of the continuity of Ngulu society (cf. a similar song in female initiation, Beideman, 1964a).

During the weeks following circumcision, the novices are confined in the bush camp and are subjected to considerable hazing. They are not allowed to wash or to leave the hut except under the careful supervision of their instructors. They are posed various riddles (mizimu) and mechanical puzzles (kikili pl. vihili) which they are later taught to understand and which are related to the sexual lore they must learn in order to conduct themselves as proper lovers and husbands. Here are some songs (mizimu) associated with these puzzles: ‘Solve the puzzles, solve the puzzles! We shall know the journey!’ (Fybambula fyamba, fyamba fyamba! Kinaman na)”. This refers both to various mechanical puzzles such as knots which novices learn to unravel as signs that they have passed initiation and also to the novices’ general instructions and new understanding regarding the sexual affairs of adults. The novices’ transition from childhood to adulthood, through initiation, is sometimes compared to a
taught at initiation. The use of a beast to represent the disorderly, negative aspects of passion and sexuality is also the motif of wild pig among the Ngulu and Kaguru, where this destructive creature represents female fertility, the womb (Beidelman, 1964a; 1964b).

While the boys are in camp, food is brought to them each day by their instructors. As any adult enters the bush camp, the children sing the following song: *Makilumbi! Makilumbi!* (the name of a bird) to which the adult replies: 'The bird responds feeding its chicks' (*Ndëge zumila wana wadye*).12

Here are some other songs sung with reference to these men who visit the camp: 'The millipede is poor, coiling up (when molested)! (*Nyongolo ni mkuwa, kisinga*). The first time a man enters the camp to visit the novices, he is supposed to bring a small token gift, such as a bit of food. The song is said to refer to a man who fails to do this, claiming in a self-deprecating manner that he is too poor to do so.

When a visitor brings something to the camp, the following song may be sung: 'Tell what you brought, what you brought!' (*Kikungjale, kikungjale kilinge*! And the visitor responds: 'I brought ——, oh! oh!' (*Nkungjale ——, eee! eee!*) The visitor may also sing: 'It is affection, it is affection for the novices!' (*Ni zinkhuwina, ni zinkhuwina za walli!* To this, too, the usual response is sung wishing the novices happiness and warning them against incest. The singer's 'affection' is said to refer to the joy which the adults feel when they see that the boys have safely survived circumcision. The novices are said to have returned to the adults, as from a journey (cf. above).

III

I noted that during this confinement many riddles and songs are taught the novices. Nearly all of the riddles and songs which I collected were said to be taught to both boys and girls. However, Cory presents some which he seems to imply are associated with only one particular sex. Some of these were also collected by me and were reported as appropriate to the initiation of both sexes; I deal with many of these in other papers published (1964a) or in preparation. However, I include here some songs and riddles which Cory ascribes to boys alone and which are not treated in my other papers. I believe that I can further clarify their meanings. In this, I hope to supplement Cory's findings and also to give the reader a sample of the type of songs and riddles that are taught to boys at initiation: (1) 'We spend our time in our home outside. Girls spend theirs at home (*nyumba*) but we are in the bush (*mambago*)'13 Ngulu boys are initiated outside the community, in the bush, in an area consistent with their anomalous, transitional positions as neither children nor adults. Ngulu speak of circumcision resembling death and rebirth and this, of course, involves a symbolic departure from the community. Ngulu themselves do not provide any clear reasons why boys should be initiated in the bush, whereas girls remain at home, except that boys are said to be 'stronger' and thus better fitted to the bush. Moral as well as physical strength seems to be implied. This would fit my interpretation: female initiation involves no transformation to a new physical and moral state such as that achieved by circumcision; rather, it is simply an effort to modify and control the new biological functions of nude girl, functions which, by their very nature, Ngulu see as never entirely controllable. Ngulu associate the bush (*nyika* or *mhuba*)14 with femininity, because of their common attributes of disorder (Beidelman, 1964b). Ngulu youths are placed outside their community while they undergo a transformation through surgery and teaching which is supposed to make them permanently clean and ordered, physically and morally.15 They must leave their homes if they are to return to them later as new persons. But Ngulu girls are retired within a
house, in the past immersed, as they are subjected to means of controlling the disorder and menstrual uncleanness which are henceforth constantly inherent in their fertility and sexuality. They too undergo surgery but even this does not make them clean, but, rather, merely tempers (softens) their sexuality sufficiently to facilitate their bearing children more easily. The surgery and ceremonies which they undergo are not designed to transform them into a new physical and moral state, but, rather, to contain and control the de facto biological state of nubility which, however vital to the society, poses a constant potential threat to its order. Their initiation represents no permanent resolution of the new demands of pubescence, but simply the inauguration of a routine of perpetual care by which a girl's dangerous and unclean sexuality is kept constantly in check within the orderly rules of the society. Thus, to initiate girls in the bush would seem to be inconsistent with the purposes of their initiation ceremonies, which do not involve transformation but control.

Here is another initiation song: 16
(2) 'Francolin on its back! What have we done wrong?' On the first night after circumcision the boys are forced to lie very still on their backs and are beaten if they move. The instructors sing the first line of this song, and the boys reply with the second. The novices are kept singing in order to stay awake. This enforced stillness allows the boys' wounds to heal somewhat during the night. The francolin has red feet and this provides a symbol for the circumcision wound, the choice of a wild, terrestrial bird utilizing the motif of the low, tangled bush with all its associated connotations of danger and uncontrollability (cf. Beidelman, 1964a). 17
(3) 'That which enters, enters without light. When it comes out, it sees with eyes.' Cory suggests that this refers to initiation and the knowledge gained through it. My informants maintained that the song referred to the penis which enters the vagina blindly, in passion, but produces a child which will have sight, viz. is a thinking member of a society, a person. Here, then, the contrast is between the violent, passionate, dark, dangerous aspects of sex and its results which are essential to maintain a society through the procreation of new members. Dr. Rodney Needham has suggested another possible meaning: the glans penis is covered up (blinded) before it enters circumcision, but it has its veil (foreskin) removed when it comes out. It then more nearly resembles a head with an eye.
(4) 'Instructor, make a fire! Let us eat meat!' 18 The boys are not themselves allowed to make a fire although one is an essential safeguard against wild beasts (cf. earlier) in a bush camp. The novices must ask their instructors to do this. The song is said to show the subjection of the novices to their instructors during this period. It may also be that this is a symbolic separation of the novices from sexuality (mato, fire, heat) from which the boys must remain apart until the completion of their initiation. We have already seen that 'to eat' is often a euphemism for sexual intercourse. The song may thus allude to the present asexual state of the novices and also to their forthcoming sexuality.
(5) 'The sun sets descending to the horizon.' 19 This song is sung at the conclusion of initiation when the novices and instructors are about to go home (cf. Section IV). It is also sung earlier during initiation but at that time its meaning is not explained to the novices.
(6) 'It has been cut, the euphorbia has been cut. The wise man keeps things to himself.' 20 The euphorbia has long, phallic-shaped branches and a milky sap spoken of as resembling semen. The allusion to circumcision is obvious. The implications of the second line are not only that the novices should keep the secrets of initiation but, also, that they should be orderly and dignified as befitting adult males.
(7) 'Over there in the bush a fire starts. The children of God escape.' 21 According to Cory, this song refers to the fact that pubic hair is always shaved, but care must be taken not to injure the genitalia. There are frequent associations both by Ngulu and Kaguru (Beidelman, 1964a; 1961) between bush vegetation and disorder and femininity, often epitomized by pubic hair or long hair in general. I suggest that there may be a further implication here of the distinction between controlled (shaved) sexuality and the uncontrolled (pubescent) and passionate (wild bush) aspects of sex. I would have thought that fire might well symbolize other, more sexual actions than the mere shaving of hair, but my informants did not know this song and I have relied on Cory's initial interpretation.

When the novices have nearly recovered, they are often taken out of the camp under the care of their instructors. They are allowed to bathe or play much as they would were they home. On one of these days, while the novices are gone from camp, all of the equipment used in initiation is removed or broken and buried. When the novices return from their day's outing, they find that everything has changed. Only the shelter hut remains. Although they may be frightened at this unexplained change, they say nothing because they have been taught never to question their instructors about anything strange that may happen in the camp and not to speak to their instructors unless spoken to about a particular topic. As it begins to get dark, some of the instructors line and make the lion sounds in the bush. One by one the instructors leave, saying that they are going home (as in the lion song quoted earlier). The boys begin to be frightened at the prospect of remaining alone in the bush without a fire or weapons. Then one of the few remaining instructors tells them that perhaps they had all better go home, walking through the bush despite the dangers of the reputed lion. When the boys arrive home with their instructors they find that bachelor huts (gane) have been built for them. 22 They are shaved and anointed with castor oil and given new clothing. Later, they are given new names by both their maternal and paternal kinmen. The next day a large dance and feast called mrungu (a type of bamboo) is usually held to celebrate their return as new, adult members of the community.

In this brief paper I have presented the few facts known about Ngulu boys' initiation. Many of my interpretations have been given without full documentation of Ngulu beliefs regarding the attributes of the sexes and the nature of sexual relations; much of this appears elsewhere (Beidelman, 1964a) or will appear later as part of a projected monograph on Ngulu female initiation. The purpose of this paper is to improve the available ethnographical data on these people, but not to provide a detailed discussion of Ngulu cosmology, symbolism and the place of these within Ngulu society. That I hope to attempt in the future.

Notes
1 Most of the data for this paper were collected in 1962-63 during a field trip to Tanzania, East Africa, made possible through a post-doctoral Ford Foundation Overseas Training Fellowship granted through the auspices of the Institute of Social Anthropology, University of Oxford. I am most grateful to Dr. Rodney Needham for many valuable comments and criticisms of this paper.
2 Cory states that on the day when boys are circumcised people dance about figurines called mungue or mnye which represent a composite of male and female sexual organs (1956, p. 97); I could get no report on this during my stay with the Ngulu; however, it may well be that such ceremonies are observed in areas which I did not visit.
3 Literally, Child of Bakwa; I could obtain no explanation of this term.
4 I refer only to unempt long hair; in the past, warriors appear to have let their hair grow long and then carefully braided it in Maasai fashion. But adult women and mature men kept their hair cut short. Today, Ngulu normally keep their hair cut short. I was told that only mad persons or witches would let their hair grow long. However, there were some significant exceptions to this rule: girls between initiation and their first child and women seeking to conceive sometimes wear a topknot which is associated with fertility and sexuality. This is shaved off after the desired child is born. I hope to discuss this further elsewhere.

5 Unfortunately, I did not attend such a ceremony and rely on informants' accounts. I was told that the foreskins were disposed of by the circumciser, but I was not told any details of this.

6 Cf. Beidelman, 1964; the Kaguru, who are the Ngulu's neighbours to the south-west, have very similar beliefs (Beidelman, 1961, 1963, 1964).

7 *Fyambula fyambo*, literally, 'to untie knots.'

8 It may be that this term (sanga) is what Cory means by the untranslated term *sankola* (1944, p. 365). He states that this is sung by visitors to the camp, to which the reply is: 'Enter and greet the Children' (Hugu kalamse wonea).

9 Mviko, pl. miko: a prohibition which causes serious calamity to oneself and one's kin if it is broken, e.g. prohibition against incest.

10 Mvizo, pl. mvizo: a prohibition against some act, such as sexual intercourse with a sister-in-law or serious insult of a kinship, which, when broken, is rectified by a fine. It does not involve supernatural danger and ritual impurity for those involved.

11 Kibwedo, a man's name.

12 In his book, Cory refers to this but provides only a rough translation and no text (1956, p. 100).

13 Cf. Cory's text, which is rather confused and appears not to be purely Ngulu (1944, p. 462). In his text, the term *nyumba* (house) rather than *kaya* (home) is used to refer to the place where the girl is confined. I believe that this choice fits the interpretation that I make in this paper.

14 Actually *nyika* means 'bush land,' while *mhage* means 'woods' or 'forest.' Both words are used to signify the wild land outside the settlement.

15 Kaguru legends concerning the origin of circumcision and related customs also emphasize these contrasts between bush and settlement (Beidelman, 1963b, pp. 758-62; 1965).

16 Cory (1956, p. 97); Cory gives a somewhat different text and translation elsewhere (1944, p. 463): *Huile ngongo. Kifendire ki?* This is mistranslated as: 'Francolin calls. What is our fault?'

17 Cory's translation takes extreme liberties with his text. He translates: 'A boy, blind and still wet behind the ears, has entered the well of wisdom. When he leaves he leaves around with opened eyes' (Makikologe laingita kisima. Kiikeza kulana lingele meso) (Cory, 1956, p. 105). However, kisima may, perhaps, not mean 'well' (kisima, Swahili, well), but may be a Gogo term meaning simply 'a dark place.'

18 Cory gives no explanation for this (1956, p. 100).

19 Cory's translation is somewhat different (ibid.).

20 Another somewhat confusing text (Cory, 1944, p. 460).

21 Although Cory indicates that this refers to shaving of pubic hair, he does not spell out the relation between the bush, vegetation and uncleanness (1944, p. 463).

22 An initiated Ngulu may not sleep in his or her parents' house, but must reside in a house for unmarried persons of the same sex.

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Archeological Reconnaissance in Southern Tripolitania and Northern Fezzan, Libya. By Dr. Louis Dupree, American Universities Field Staff, Associate Professor of Anthropology, Pennsylvania State University. With four figures.

In June-July, 1956, I journeyed to Libya as a civilian member of a United States Air Force team to test desert training, techniques and equipment for the Arctic-Desert-Tropic Information Center of the Research Studies Institute, Air University, Maxwell Air Force Base, Alabama. The tests took place in the Hamada el-Hamra (Red Desert), a hot, barren, largely uninhabited, pebble-strewn desert, located south of El Azizia, officially recorded as the hottest spot on earth. A few dune areas with camel grass types constitute the only zones of summer vegetation. Water is scarce and unpredictable. For example, although reported as being the centre of a live oasis on maps, the well at Bir ez Zauria (see fig. 4) had filled with sand, and the water table at Bir el-Chor had dropped to 120-130 feet below ground level (see map, fig. 1). Modern inhabited areas are generally limited along the main route leading south from Mizda. Seasonal migrations by nomadic groups do occur, however, especially in the spring.

While on a trek in the Hamada el-Hamra, we collected 60 flints on the surface of the desert floor. We travelled by foot most of the time, testing the Air Force survival system, so our collection is necessarily small. In spite of the limited sample collected, several flint implements are typologically, if not chronologically, similar to certain specimens discussed by McBurney and Hey.

The importance of this collection is that it extends certain typological elements south into the Fezzan. No chronological comparisons can be—or are meant to be—implied. I merely wish to record where the flints were found, describe them, and make pertinent comparisons with the McBurney and Hey typologies.

The 60 flints4 were collected from seven localities (see fig. 1). The numbers 1-7 on the map indicate the approximate geographical position of the localities.

Locality 1 (fig. 2) Located 3-3 miles north of Mizda in a wadi leading toward the major limestone scar. Four flints collected.

Stubbly, thick, rectangular, medium small scraper; undiagnostic; but freshness and lack of patina, generally small near size and angularity suggest relative lateness.

Small triangular flake with alternate opposite face edge retouch and/or wear mainly along one edge. Not diagnostic.

Two undiagnostic, utilized fragments; parallel scars on one, and parallel edges on other, suggest blades and relative lateness.

Note that the four specimens of Locality 1 all suggest lateness, typologically speaking.
Locality 2. North of Wadi Ussigh on plateau. One flint collected. Utilized flake or blade fragment; undiagnostic, but bladelike appearance and freshness of patina suggest relative lateness.

Locality 3 (fig. 3a, d, e). Wadi Ussigh, in front of cave to west of road; many flints in general area. Ten flints collected.

Seven utilized broad flakes with tan patina: two are more narrow and bladelike, but thick; two have faceted striking platforms, one of which exhibits distinct and predominant double patina, the edge wear or retouch being fresh (fig. 3d). The rest are typologically the broad irregular kind of flakes usually associated with Middle Paleolithic or Levallois-Mousterian industries (fig. 3e).

**FIG. 2. LOCALITY 1, NORTH OF MIZDA**

*Photograph: L. Dupree*

The other three flints are: a worn, undiagnostic fragment, probably utilized; a discoidal cortical flake of rolled or sand-blasted tabular flint with steep edge retouch or wear to form a poor circular scraper, suggesting relative lateness; an exceptionally fine retouched combination end scraper and side scraper, continuous flat near steep re-touch suggesting possible neolithic affiliation (fig. 3a; also see No. Tripoli. Surface, fig. 37, p. 265, No. 33 in McBurney and Hey).


In addition to a small undiagnostic flake, I picked up an elongated, medium-large fragment, coarsely flaked on two faces, possibly a core fragment, whose broadly set flake scars and milky patina suggest Middle Paleolithic, typologically.

Locality 5 (fig. 3b, c, f, g). Twelve flints collected near Bir el-Chor.

**FIG. 3. IMPLEMENTS FROM LOCALITIES 3, 5 AND 7**

*Scale: 1/4. Photographs: Fred Orchard, Peabody Museum, Harvard University*
Seven of the finds consisted of variously patinated, irregular small flakes and fragments with little or no edge wear.

A triangular-shaped, coarse bifacial flake-core (fig. 3f) with tan patina somewhat resembles the Middle Palaeolithic, Levallois-Mousterian type, but its small size and shape appear to be closer typologically to the flake-cores of the Upper Palaeolithic industry of Hagfa ed Dabba (McBurney and Hey, fig. 31: 1, 3, 5, 6, ‘possibly prototypes of Libyan Neolithic adze’).

Two side scrapers (fig. 3b, c) found at Locality 5 are typologically different: b might be called Middle or Upper Palaeolithic; c might be typologically later.

Aesthetically the finest piece is an excellently retouched backed point on an elongated fragment (fig. 3g). The tool looks late; closest comparisons are the Upper Palaeolithic backed blade industries of Hagfa ed Dabba (McBurney and Hey, fig. 37, p. 207) or Wadi Gan (McBurney and Hey, fig. 34, p. 231). The Libyan Neolithic relates more closely to the Egyptian style of pressure flaking.

A fragment of a steep scraper or core, undiagnostic, completes the Locality 5 finds.

Locality 6. Eighteen flints collected on foot trek between Bir el-Chor and Bir ez Zauria. Some dune sites, all blown out by winds and with no discernible stratigraphy, were encountered en route.

Mainly natural flint fragments with considerable desert varnish; all but three had tan patina. Four of the 18 show definite signs of human use; seven show signs of possible utilization; the rest are decidedly natural and worn by nature.

Locality 7 (fig. 3h, fig. 4). Dune sites, all blown out, at Bir ez Zauria. Thirteen flints collected, all of which sport strong desert varnish, tan patina and signs of extensive use by man.

Mathematical Models of Marriage Systems. By Dr. Frank B. Livingstone, Department of Anthropology, University of Michigan, Ann Arbor. With a table

The past 15 years have witnessed an almost explosive development in the analysis of kinship and marriage systems. Most of this analysis is the direct result of Lévi-Strauss’s monograph, Les structures élémentaires de la parenté. The development has taken two rather different directions, both of which, however, have centred around the concept of prescriptive marriage systems. One trend has taken its cue from A. Weil’s appendix in Lévi-Strauss and has been concerned with the description of prescriptive marriage systems by permutation matrices and other mathematical models. Bush (appendix in White, 1964), Kemeny, Snell and Thompson (1962), and White (1964) are examples of this development. The major contributors are not anthropologists, and their analyses have seemed at times to be quite divorced from current anthropological fact and theory. The other development has been the application of Lévi-Strauss’s analysis to specific societies and for the most part has been due to Needham (1962 and references therein).

Many of these prescriptive marriage systems are found in South-East Asia, where they are integrated into complex social structures. Apart from this area prescriptive marriage systems are most structured and hence discernible in rather primitive societies such as the Australian Aborigines in which reciprocity between groups consists almost solely of women-exchanges. By analogy it would seem reasonable to suppose that such marriage systems were more widespread among human groups in the Upper Palaeolithic and Neolithic. These systems are not reported from Africa (see Lane, 1962, for a possible exception); but among the Pygmies sister-exchange is the usual form of marriage (Putnam, 1949), the Plateau tribes of Nigeria have many types of women-exchange marriage (Meck, 1936) which to my knowledge have
never been re-analysed, and the Bushmen may have had a modified Kariera system. For the !Kung Bushmen Marshall (1959) states that there are a limited number of personal names, a man inherits his paternal grandfather’s name, a woman her paternal grandmother’s, and only certain names should marry. At equilibrium with constant family size and a constant population, this system would operate approximately like an Australian class system. Of course, the present situation of the Bushmen is quite unlike this ideal state. Winter (1956) has also stated that there are two forms of marriage among the Bwamba, the oldest or original form being exchange of women and the later form involving bride wealth. Other examples could be cited, so there appear to be many vestiges of obligatory women-exchange systems among primitive peoples, and even in Africa where prescriptive marriage systems are considered to be almost non-existent.

Although the theoretical models of prescriptive marriage systems are mathematically elegant, in their present form they apply to very few societies, and some societies to which they have been applied are almost completely unknown anthropologically (e.g., the Tarai in Kemeny, Snell and Thompson, 1962). This is a result in part of the fact that these models have been set up on the basis of genealogical relationships and not on ‘kinship’ relationships as that term is understood by most anthropologists. In addition the concept of prescriptive marriage system has been defined in such a restricted way that such marriage systems are very rare and in fact one type, prescriptive bilateral marriage, is considered to be non-existent. This definition, which is due to Needham (1938a), restricts prescriptive systems to those which require marriage with a particular relative. The function of this definition is apparently to delimit which societies have such a system and which don’t. The existence of various types of prescriptive marriage systems is thus simply a matter of whether the ambiguous, incomplete data which we possess on most primitive societies can be made to conform to the definition, and Needham (1962) has discovered that some types exist and others don’t. Many useful insights have resulted from this new approach to marriage systems, but at times it seems to be a new kind of anthropological butterfly-collecting.

Definitions are more than labels to paste on reality, and science is more than discovering what exists. In this paper several models of obligatory marriage systems will be presented, but these models do not exist in the sense that they are not exact descriptions of any living society. These models are similar to those which exist in population genetics and which are the foundation of that science. In the genetic models the concepts of breeding isolate and random mating are not meant as labels of certain aspects of reality. The further assumptions of discrete generations, infinite population size, and equal fertility of all marriages are such that no society on earth could possess these features. But taken together these concepts form a powerful model of the genetic process. We can express quantitatively the characteristics which are of interest, i.e., genes, and can predict with this model how they will change and how fast. When we apply this model to specific cases of reality it is sometimes possible to predict gene change with great accuracy, and at times it is possible to determine just how much each of our assumptions has affected our predictions. Social behaviour, particularly marriage, has many consequences for human population genetics, so it would be nice to have similar models for marriage systems.

Although many view prescriptive or obligatory marriage systems from the point of view of the individual’s obligation to marry a particular relative, they can also be considered as various kinds of alliances between exogamous groups within the total society. We will assume that a society is composed of an indefinite number of exogamous clans, say n, which contract alliances with one another by exchanging women. Other valuations may also be exchanged but here we will only be concerned with women (stone axes or shells have few genetic consequences). These exchanges can be either direct or indirect. In direct exchange the two groups simply take wives from each other, while in indirect exchange any particular group does not obtain wives from the same groups which marry its women. Direct exchange can be either simultaneous, which is usually recorded as sister-exchange, or it can be delayed in which case it is usually described as some form of patrilateral marriage and perhaps even cross-cousin marriage. Indirect exchange implies that one clan is always in the relationship of wife-giver or wife-taker to another clan and this in turn implies matrilateral marriage.

If within a society all n clans have alliances with each other, then there will be \( n(n-1)/2 \) such alliances. Since these alliances between clans are in the nature of contracts and imply some obligation to fulfil duties of wife provision, in the past I have used the term ‘prescriptive’ for them, but Salisbury’s (1964) term ‘obligatory’ will be used since it is applied to a wider range of actual societies by Salisbury and seems more appropriate. Although, in obligatory patrilateral marriage, the direction of exchange is reversed in every generation, which means in other words that the obligation between clans changes with every marriage, at any one time the alliance pattern can be represented in the same way for both matrilateral or patrilateral obligatory marriage. Since the relations between any two clans will be in one direction for both types of obligatory marriage, at any one time a particular clan will owe wives to (patrilateral) or give wives to (matrilateral) some number of the other \( n-1 \) clans. Thus, we can represent the alliances at one time by an n-tuple, \((x_1, x_2, x_3, \ldots, x_n)\), where \( x_i \) is the number of clans the ith clan gives or owes wives, and

\[
\sum_{i=1}^{n} x_i = n(n-1)/2.
\]

This description of the alliance pattern between clans is identical to Rapoport’s (1949) description of the pecking order of chickens, for which \( x_i \) is the number of chickens the ith one pecks. However, the equilibrium pattern is very different for the two phenomena. Many possible combinations of numbers for which \( \sum x_i = n(n-1)/2 \) are not possible alliance patterns. No \( x_i \) can be 0 or \( n-1 \), because this would imply that clan \( i \) could not find marriageable women or find spouses for its women. On the other hand the pecking order not only allows these values but would tend toward them since pecking orders tend to be consistent. That is, there is one chicken on top who is pecked by none, and a second in command who is only pecked by the leader, etc. In other words there is a strong dominance gradient. For example, in a flock of three chickens, if \( x_i \) is the number of chickens the ith one pecks, then there are two possible orders: (1, 1, 1) or (2, 1, 0). Although one might expect the first ‘democratic’ order at the beginning, in time the second hierarchical order would develop at equilibrium. However, for the alliance patterns in a three-clan society, the first set is the only one possible.

If we make the further assumption that the clans are all equal in size with equal numbers of the sexes, then the equilibrium order would seem to have all clans give and take wives from equal numbers of clans since this would maximize the alliances for a clan given a 50-50 sex ratio. If \( n \) is odd, then the perfect order would be \((n-1)/2, (n-1)/2, \ldots, (n-1)/2\), while if \( n \) is even, the closest pattern to equality would be \( (n/2, n/2, \ldots, (n-2)/2) \), with half the clans giving wives to \( n/2 \) clans and half to \( (n-2)/2 \) clans. With \( n \) odd, the variance of this order is 0. One step away from equilibrium would have one clan giving wives to \((n+1)/2\) and one clan giving wives to \((n-3)/2\) clans. There are \( n(n-1)/2(n-2) \) combinations of the clans which would result in
this pattern and the variance is 2/n. Two steps away with two
clans giving to (n - 3)/2 and two clans to (n + 1)/2, the number of
combinations is 6n!/4!(n - 4)! and the variance is 4/n. Three
steps away the variance is 6/n and at further steps it is 2x/n where
x is the number of steps away from the equilibrium order. Evolution
of the social structure toward equilibrium would tend to
reduce the variance in the number of clans each clan gives
wives to. This seems quite similar to R. A. Fisher’s Fundamental
Theorem of Natural Selection which states that the amount of
genetic change is proportional to the variance in fitness.

As an example of this model, Purum society has an ideal
alliance pattern of (3, 3, 2, 1, 1) which is about as far from equi-
rium as a five-clan system can get. However, Needham (1958b)
has produced evidence to show that these five clans are not the
alliance groups.

Since each clan at equilibrium gives wives to (n - 1)/2 clans, then
if they must exchange w wifes per generation to maintain the
alliance and the adult female generation is f per cent of the total
clan, the total population of such a closed society should approxi-
mate 100wn (n - 1)/2f. For example Needham (1958b) states that
the Purum had 13 alliance groups. If w = 1 and f = 25 per cent,
then the total Purum should have been 312. It was 309.

Of course the clans in any actual society are not all equal, so that
the society would be tending toward this equilibrium but there
would be disturbing factors tending to disperse the sizes of the
clans. Although no society has equal clans, when the clan still has
political, economic, and religious functions, there does seem to be
an optimum size to the clan. As clan size tends to drift by random
processes away from this optimum, the processes of fission and
fusion tend to keep clan sizes clustered around the optimum.

Leach (1959) and Meggitt (1963) have emphasized this continual
fissioning and fusion which must occur in any society for it to
remain viable or maintain viable clans. The dispersion of clan size
is almost the same problem as the extinction of family names which
Watson and Galton (1875) first solved almost 100 years ago in the
Journal of the Anthropological Institute. This was the first published
example of a stochastic process which is now one of the most
rapidly growing branches of mathematics. However, for family
names in Western societies or clans in tribal groups in which the
clans have lost their major functions, the variation in size is much
greater since there are no systemic pressures tending to push the
size back toward the optimum. These systemic pressures also
complicate the process so that I haven’t been able to get any
explicit expression for it. One can attempt, however, to simulate
the process on a computer for a specific clan optimum and maximum
allowable variation from the optimum.

Mervyn Meggitt has provided me with data on the clan size
among the Mae Enga of New Guinea. Since the adult males
constitute the core of any clan and are the individuals who defend
its territory and perform most of the clan’s functions, I have
divided the clan sizes by four in order to obtain a rough estimate of
this male core. On Table I are shown the means of the clans in
various areas and the ranges and variances of the number of adult
males per clan. The range is from about 25 per cent. of the mean to
about 250 per cent. of the mean. This seems to be about the vari-
ation which can occur and still have the group able to function as
a clan. Although the range as a percentage of the mean does not
seem to increase with an increase in mean size, the absolute value
of the range and the variance both increase with the mean.
This can be seen by the fact that the coefficient of variation re-
mains constant.

The distribution of clan sizes was simulated by a random process
on the Michigan 7090 computer. All clans were started with the
optimum number of males, which was taken to be the mean
number, and then succeeding generations were determined
randomly. When a particular clan became 2-5 or more times
greater than the mean or was reduced to 2-5 or less of the mean,
then this clan disappeared or became two clans with the optimum
number of males. The programme was set up so that the total
population was constant from one generation to the next and there
were always 10 clans. With an optimum size of 20 the average
variance was 100, and after the first few generations and the
distribution had stabilized the range of variance from generation
to generation was 25 to 200. Maramuni is the only zone area close
to this clan size and its variance of 68 is well within the range of
the model. With a mean clan size of 50 the range of the variance
was 200 to 700 with a mean variance of about 400. For a mean size
of 100 the variance ranged from 1000 to 3000, with a mean
variance of 1800. For these three different mean clan sizes the
coefficients of variation were 50, 40, and 43, respectively, which
seems to indicate no trend toward increasing proportional vari-
ation with increasing optimum clan size. Although the optimum
clan size can vary owing to land productivity, topography,
technology, or other factors, these simulation models seem to fit
the data quite well.

| Table I: The Approximate Mean, the Variance, and the Range of Adult Males per Clan in the Mae Enga |
|-----------------|----------|-----------|-----------|-------------|
| Area            | Number   | Mean      | Variance  | Range       |
|                 | of Clans |           |           |             |
| Upper Lai       | 106      | 2610      | 12-360    | 67          |
| Middle Lai      | 41       | 826       | 12-182    | 44          |
| Lower Lai       | 53       | 494       | 12-103    | 46          |
| Upper Sau       | 46       | 405       | 6-118     | 54          |
| Lower Sau       | 24       | 218       | 5-62      | 52          |
| Syaka           | 33       | 781       | 24-151    | 42          |
| Maramuni        | 27       | 68        | 9-42      | 36          |

If there is an optimum size for the clan, then for a closed society
within which all alliances occur, there must be an optimum
number of clans. But I have not been able to describe this situation
mathematically nor describe the process by which these optimum
units are formed. The clan model outlined at the beginning des-
cribes only the pattern of alliances. It may well be that the actual
numbers of women involved in the exchanges may be a more
suitable measure for the purpose of combining this model with
that of the fissioning and fusioning of clans. However, the clan
model is only a static description of the exchange system. In order
to predict how these systems change we need some model of the
process of the creation and dissolution of alliances. There are
many mathematical models which are attempts to describe similar
processes such as Barth’s (1959) use of game theory to describe
the alliances of the Pathans, but it seems to me that graph theory
offers the best possibilities (see Flament, 1963, for an outline of this
theory). The clan model is in fact a directed graph, which because
of the limitations of the possible alliance patterns, I think it can be
proved, is strongly connected. There have been many striking
advances in diverse fields which are based on graph theory, for
example economics (Avondo-Bodino, 1962) and in the analysis of
many kinds of networks and flow through them (Ford and
Fulkerson, 1962). The alliances between clans in primitive societies
seem to be a perfect fit for these models. But again the models
appear to be the description in another terminology of an alliance
system which we can describe adequately with the concepts of
anthropology. In any case this note is not meant to report concrete
results but only to outline an approach to marriage systems which
deserves attention.

References


SHORTER NOTES

A Note on Stone-Built Enclosures in South Nyanza, Kenya.

By Neville Chittick, Director, British Institute of History and Archeology in East Africa, Nairobi. With four figures.

There exist in parts of the South Nyanza district of Kenya, south of the Kavirondo Gulf of Lake Victoria, numbers of stone-built enclosures, now for the most part ruined, which do not appear previously to have been commented upon. I visited a number of these in January, 1962, and the present note gives some account of the structures observed during a cursory examination. The region is settled by (Nilotic) Luo people, but before their arrival probably some three centuries ago it seems to have been occupied by (Bantu) Kisii and related Kuria, now found in the areas immediately to the east and south respectively.

![Fig. 1. Plan of Boma at Minyere Hill](image1)

The remains concerned lie in the Kanyamkago chiefdom in an area some 35–40 miles south-west of Kisii; the most interesting group lies on a low hill known as Minyere, overlooking the Kuja river, about two miles south of the ferry on the Kanyamkago-Kanyadooto road which crosses that stream. Six enclosures (or bomas) were seen, but Luo elders who accompanied me asserted that there were some 20 in all. The bomas are fairly widely scattered over the hill and are for the most part enveloped in bushes and creepers. They are roughly circular in shape, built in drystone fashion of unshaped, usually flatish, blocks of the country rock. In some cases the mode of construction is very simple, with little selection of stone; it seems unlikely that such walls were ever of very great height. Others have been built with very considerable care, particularly the best-preserved of those seen, a description of which follows.

![Fig. 2. Minyere Hill, Boma No. 3, Interior of Main Entrance](image2)
This enclosure (numbered III) measured 29.50 metres from north to south internally, and about 32.50 metres (possibly rather more; the eastern wall is partly collapsed) from east to west (plan, fig. 1). The main entrance was at the northern end, and takes the form of a roofed passageway (fig. 2). This is carefully built of selected blocks fitted together, the roof being of large stone slabs extending across its width (0.80 metres). The height of the passage is 1.30 metres and its length 2.20 metres, this being the thickness of the wall at this point. There are slots for a squared beam to bar the entrance. There is a second entrance at the southern end, which was not roofed. The threshold of this was 0.75 metres above ground level; the opening extended the whole height of the wall, but was much narrower at the bottom (only 0.25 metres) than at the top (about 1 metre). The enclosure wall is 2.50 metres in height where not collapsed, and thicker on the northern side, adjacent to the door, than elsewhere (fig. 3). In the south-west quadrant of the circle are two drystone hut circles, diameters 5 metres and 2.40 metres internally, and remains of a third. The walls of the better-preserved huts are 0.75 metres thick and up to 1.30 metres high. West of the largest one are traces, flush with the ground, of a structure that appears to have been of pisé clay.

The elders asserted that the stone huts were for calves, and the other structure for goats; presumably the growing cattle were free to roam in the rest of the enclosure. The construction of the huts was ascribed to the Kabwoch Luo, and the enclosure described was said to have been occupied by one Alila, son of Migot, who died as recently as 1938. Indeed some of them seem still to be used as corrals on occasion; fig. 4 shows the simple entrance of another boma, with upright stones set against the ends of the ruined walls; rough timbers nearby appear to serve to close the gate.

There are traces of other stone huts about 1 km. south-west of Uriji market, which lies close to the Kisii-Migori road; these are said to have been destroyed in 1951 or thereabouts to obtain stone for a school or other buildings. The position of the works is marked by a luxuriant growth of bushes; the diameter of one examined was about 80 metres. A local informants averred that these huts were also built by the Kabwoch Luo, as a defence against Masai raids.

Mr. B. A. Ogot informs me that Luo elders report notable remains of apparently similar type at Nyaraya near Macalder’s mine, which lie about 43 miles south-south-west of Minyere Hill, and that there are others in the area bordering the Lango region; again they are said to have been built as a defence against the Masai.

Sir H. H. Johnston describes and illustrates mud-walled villages of the Bantu Kaviirondo. Of the Lwoo, he says: 'Formerly [viz. before c. 1900] they built mud or stone walls round their settlements in imitation of the Bantu Kaviirondo to the north.'

Despite the assertions of the present Luo elders, it seems probable that the structures described in this note, some of which display a high standard of craftsmanship, were either taken over from earlier Bantu inhabitants of the region, or built in imitation of such buildings. Their nature, which is foreign to Luo traditions of construction, suggests that the former hypothesis is more probable. It may be noted also that there are drystone remains further south near the lake shore, notably on the headland called Dwakahangara on Ukerewe Island. These, however, are concentric fortifications for the defence of a hilltop.

**Notes**

1 This entrance resembles a type found in the Inyanga area of Southern Rhodesia (cf. R. Summers, *Inyanga*, C.U.P., 1938, Plate IIIa); but the similarity is no doubt fortuitous.


3 Johnston, *ibid.*, p. 780


**The Hand Bolt. By Dr. M. D. W. Jeffreys, University of the Witwatersrand, Johannesburg. With three figures**

148 In defining the term 'hand bolt' the first element is readily disposed of; 'hand' is for my purpose defined in the *Oxford English Dictionary* as the 'terminal part of the arm beyond the wrist consisting of the palm and the five digits, forming the organ of apprehension characteristic of man.' To 'bolt,' however, I attach the meaning of a 'projectile'; something projected by force. This meaning for bolt is suggested by the *O.E.D.* where bolt is described as 'an arrow; especially one of the stouter and shorter kind with a blunt or thickened head, called also quarrel . . . .' The quarrel is also called a 'bird bolt.' However, the manner in which Shakespeare uses the term bird bolt inclines one to think that this weapon was not necessarily an arrow. Shakespeare in *Twelfth Night* (I, v, 100) wrote: 'To take those things for bird bolts that ye deem cannon bullets.' The *O.E.D.* states that a bolt 'is an an elongated bullet for a rifled cannon.' The impression gained is that a bolt is a projectile and this impression is strengthened when one recalls how Milton used the term in *Paradise Lost* (VI, 491): 'They shall fear we have disarmed The Thunderer of his only dreaded bolt.' Consequently a hand bolt could be described as a hand missile.

I maintain (a) that palaeolithic man among his missiles would use a special type of stone implement specifically shaped for its purpose; (b) that this stone implement has not been recognized by archaeologists as a missile; (c) that today this stone implement carries the misleading name of 'handaxe' or 'handpick.' An examination of these palaeolithic stone implements shows that they (a) are elongated tools.
with a pointed and a blunt end; (b) are worked on both faces; 
(c) have their margins trimmed to a cutting edge all the way round; 
(d) show no signs of wear and tear on the cutting edge (see fig. 1). 
On the principle of least effort these characteristics preclude the 
alleged 'handaxe' its use as a hand tool. A tool to be held in the hand 
would not be worked to a cutting surface all round its edge because this 
sharp edge would damage the hand as soon as the tool was used 
with any force at all. Furthermore the edges would then show wear 
and tear.

![Fig. 1. A typical 'handaxe' or hand bolt](image)

Length c. 15 cm.

In the cultural division of labour among the sexes, it is the 
occupation in food-gathering societies for the woman to provide the 
 carbohydrates in the form of seeds, roots, nuts, fruits. 
In the exploitation of these feral foods the so-called 'handaxe' 
would play no part. In these societies it is incumbent on the man to 
provide the proteins in the form of flesh and it is in this activity 
that the primary use of the 'handaxe' must be sought. As already 
suggested it was not a hand tool, yet it is a tool linked with the 
activities of the male in securing proteins for the family; as such its 
main function would be in the hunt. As a spear point or lance head 
it is far too heavy and clumsy. One must search for some activity 
in hunting where its 100 per cent. cutting edge would be purposeful. 
This purpose would be achieved if the so-called 'handaxe' were 
used as a missile. For a missile the term is seen immediately to be 
inappropriate, where 'hand bolt' would be appropriate.

If this hand bolt is held by its elongated end in the palm of the 
hand and five digits (fig. 2) and the blunt end aimed at the quarry, 
then, if hurled with the cutting edge horizontal, the missile will 
revolve around its centre of gravity with its elongated end having a 
high angular velocity. Its rotating movements through the air 
would resemble those of the following hand-thrown missiles:

![Fig. 2. The throwing hold](image)

boomerang, Sudanese throwing knife, South African knobkerrie. 
On its striking its quarry, the advantage of a hundred per cent. 
cutting edge becomes immediately apparent.

The areas in South Africa where I have found these hand bolts 
in abundance are around pools, vleis and lakes and along river 
banks. As these implements are readily made—I saw the late 
Professor Van Riet Lowe fashion an alleged 'handaxe' in a quarter of 
an hour—it occurred to me that stone-age man used these implements 
against flocks of waterfowl and that having brought down his quarry 
he did not always trouble to retrieve his missile which would have 
fallen into water or reeds. As a missile, this implement is better 
described as a hand bolt than as a 'handaxe.' (An association between 
the terms quarry and quarrel or bird bolt might repay investigation.)

The assumption outlined above would account for the great 
numbers of hand bolts found round water courses, vleis and pans 
in South Africa: it would also explain the almost pristine newness of 
the cutting edges on the hand bolts when picked up. These hand bolts 
show no sign of wear and tear or of continuous use. It would also 
explain the peculiar working and shaping of these implements.

The above claim for this tool runs counter to the claims of 
Margaret Shinnie in her book Ancient African Kingdoms (London, 
1965, p. 18), where she wrote: 'All over the [African] continent, 
except in the central forest area, rather clumsy, pear-shaped stone 
tools are found, which are called "hand axes." They have been 
chipped into shape, always the same general shape, rounded at the 
thicker end, which was the working end, and tapering to a not 
very fine point at the thinner end, which was held in the hand. 
They are called "hand" axes because it is assumed that they were 
held in the hand, and were used for a variety of purposes, mainly 
connected with food-gathering.'

Dr. H. J. Swart, of the Botany Department, Witwatersrand 
University, has provided an adroit sketch of palaeolithic man 
effectively using a hand bolt (fig. 3).

![Fig. 3. The hand bolt in action](image)

Drawing by Dr. H. J. Swart

In many parts of Europe the neolithic polished stone celt is known 
as a thunderbolt while in parts of Africa this celt is called a lightning 
stone or a 'god axe.' So far as is known the neolithic polished celt 
ever used as a missile but either as an adze or an axe for shaping 
wood. How then comes it that this celt is called a 'bolt' as in 
thunderbolt? Are we seeing here the transference to the obsolete 
celt of an earlier name for the forgotten palaeolithic hand bolt?

A Study of Foot Contour among the Sema of Naga Hills 
and the Lushai of Mizo Hills, India. By 
Sina Mukherjee and Deba Prasad Mukherjee, Calcutta.
With a table

The human foot is known to have undergone certain morpho-
logical changes due to assumption of erect posture. In adult man, for 
instance, the long axis of the big toe is nearly parallel to that of 
other toes, whereas in apes it diverges at a larger angle. Quantita-
tive variation of such adaptations in different habitats and races 
is not yet quite clear.
The study of foot outlines of some Indian tribes (Sarkar, Das and Uzir, and Ganguly and Pal) indicates relatively smaller feet among the nomadic populations. The divergence of the halluc and length-breath index are again greater among the Khasi of Assam, who speak a Mon-khmer language and show some physical resemblances with the Mongolid, and also the Negritoid Onge of Little Andaman.

We had the opportunity to trace the contour lines of the feet of 28 men belonging to the Sema tribe and of 20 men belonging to the Lushai tribe, following the methods adopted by Sarkar, during 1959-60 in Shillong. The former came from Mokukchhang district of present-day Nagaland and the latter from Aizol in the Mizo Hill district of Assam. The Sema fall in the Western group of the Naga in common with the Angami and Rengma tribes. Grouped in small communities the Naga have remained isolated on their hill tops (Hutton). The Lushai are included in the Kuki-Chin group of people and are similarly isolated.

The linear measurements from acropodion to pterion and from tibial metatarsale to fibular metatarsale were taken with the help of a sliding calliper on the contours. Angles between axis lines were read by means of a transparent protractor. The results are summarised in Table I for comparative purposes.

<table>
<thead>
<tr>
<th>No. of feet</th>
<th>Sema Naga</th>
<th>Lushai</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>36</td>
<td>40</td>
</tr>
<tr>
<td>Foot length in cm.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>23.7</td>
<td>24.9</td>
</tr>
<tr>
<td>Range</td>
<td>22.6-25.5</td>
<td>23.7-25.9</td>
</tr>
<tr>
<td>Foot breadth in cm.</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>9.7</td>
<td>10.1</td>
</tr>
<tr>
<td>Range</td>
<td>8.6-10.3</td>
<td>9.3-10.9</td>
</tr>
<tr>
<td>Length-breath index</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>42.1</td>
<td>40.5</td>
</tr>
<tr>
<td>Range</td>
<td>35.51-43.24</td>
<td>39.24-43.78</td>
</tr>
<tr>
<td>Hallux divergence angle</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean</td>
<td>7.96</td>
<td>5.97</td>
</tr>
<tr>
<td>Range</td>
<td>6.10-5.6</td>
<td>5.7-5.6</td>
</tr>
</tbody>
</table>

It appears from the data that the dimensions of the Lushai feet tend to be larger than those of the Sema, while the angle of halluc divergence and the width relative to length is more in value than among the latter. This seems to substantiate Hutton's comments on the branching away of the great toe from the others among some Naga tribes. The Sema are more comparable to the Khasi, and the Lushai come closer to the tribes of Eastern India in this character. Although the data are not sufficient for any definite conclusion, they may suggest the usefulness of further investigations on this character for genetic analysis.

**Notes**


**Myth and Environmental Change in Tanganyika. By P. W. G. and Beverley Brock, Entebbe, Uganda**

150 Anthropologists usually assume the physical environment of the peoples they study as something given, static; hence stories about changes in that environment are seen as mythical. A situation was found in south-western Tanganyika where such an assumption was not justified. In this case, a story of 'hills growing' may well reflect actual geomorphological changes.

The story was collected by the second author while carrying out an ethnographical study of the Nyilha of Mbozi in 1961. The Mbozi Nyilha are a Bantu-speaking people with patrilineal clans, formerly ruled by independent petty chiefs. Many of the Nyilha live on the Mbozi Plain, which is a gently undulating surface standing 3150 to 3300 feet above sea level. Apart from the rare steep-sided inselbergs, which stand 200 to 500 feet above the plain, slopes are very gentle. The broad valley bottoms are generally marshy and in a few places the marshes continue up the sides of the valleys onto the interfluves, some 100 to 150 feet above the valley bottoms.

The Nzwa chiefdom is situated on this plain. Historical traditions in this chiefdom all assert that the first people to settle there were the forefathers of the Shupa clan, who came long before the founding of the chiefly line. During an account of Shupa clan history, given in the presence of, and confirmed by, elders of other clans, an old, blind Shupa said that when his forefathers came to the place where they first settled (the steep-sided hill Nyeria) there were no trees; they burned a peat-like substance for fuel, gathered from the swamps. There were only a few large hills—the others have grown since.

Little attention was paid to this account until the first author, a geologist, began to study the geomorphology of the area. The geological evidence shows that the Shupa story could describe in non-technical terms and with much foreshortening a geomorphological process that has in fact been taking place in very recent geological time.

It is thought that until very recently (on the geological time scale) the Mbozi Plain was extremely flat and marshy, with only a few low hills of more resistant rocks standing up out of the marsh. As a result of the deepening of the Rukwa Trough, the Mlovo River began to cut more deeply through the rocks to the north-east of the plain. When this incision reached the marsh, the water-logged clays and deeply weathered rocks were rapidly denuded, producing broad shallow, and still marshy, valleys a hundred feet or so below the original level of the marsh. The higher-standing hummocks, originally more resistant to erosion and not subjected to the prolonged saturation, remained at about their original elevation. It is conceivable that the speed of the erosion and denudation was such that within a man's life span, some recession of marsh and increase in proportion of arable and tree-supporting land could be observed and interpreted as 'the hills growing.' The decrease in proportion of marsh, and to a less extent the increase in relief, might have been quite significant over several generations.

This geological evidence cannot be used to provide any dating meaningful in terms of human history. In interpreting it, due regard must be paid to the difference in geological and historical time scales; geologists are accustomed to working within a tolerance of plus or minus thousands if not millions of years. All that can be said is that the Shupa story is compatible with the evidence of a geologically very recent process that over several generations could have modified the environment sufficiently to allow it to support an increased population. In effect, the hills did grow, through the deepening of the broad valleys between them.

Because of the difference in the time scales involved, it is not possible to assert strongly that the process occurred in human times; nevertheless the geological evidence makes it impossible to dismiss the Shupa story as mere myth without any possible basis in fact.

**Notes**

1. For further ethnographical details, see Beverley Brock, A Preliminary Description of the Nyilha of South-Western Tanganyika, unpublished M.A. thesis, University of Leeds, 1963.
CORRESPONDENCE

Positive Zen Marxism. Cf. MAN, 1963, 21

Str.—In his article 'On Zen Marxism: Filiation and Alliance' (MAN, 1963, 21) Robert Murphy neglected the arguments over the interpretation of the minutiae of systems of descent and marriage, and there is a quite fundamental methodological issue that transcends in importance the subject matter of the argument: this is, quite simply, that Lévi-Strauss and his followers have taken a significant step away from the tradition of positivism, which is almost the equivalent of social science today, and reinstated dialectical philosophy as a method in social inquiry. While I agree with Murphy as to the importance of the methodological issue, I question whether Lévi-Strauss's use of the dialectical philosophy in social inquiry is a step away from positivism.

In the first volume of the Positive Philosophy, Auguste Comte, the founder of positivism, writes: 'It is true that every theory must be based upon observed facts, it is equally true that facts cannot be observed without the guidance of some theory. Without such guidance, our facts would be desultory and fruitless; we could not retain them: for the most part we could not even perceive them. Thus, between the necessity of observing facts in order to form a theory, and having a theory in order to observe facts, the human mind would have been entangled in a vicious circle, but for the natural opening afforded by theological conceptions.'

The necessity of a cognitive frame of reference for the observation of facts may be illustrated by the person who has never seen a photograph or heard of photography when first confronted with a snapshot. It may well appear to him as a meaningless melanage of black, white and grey until he formulates the theory that it is indeed a pictorial representation of some scene. Once having done this, his theory becomes unconscious and implicit in his approach to all subsequent experiences of a similar nature.

Comte recognized the necessity of formulation of a theory—any theory, regardless of its adequacy—prior to the study of empirical facts. Theories, as he said, do not arise from facts, they can only be tested against them. Having said this, however, Comte failed to recognize that this meant that all theories are necessarily metaphysical in origin and remain so. Theories are a part of the scientific method, but they are never 'positive' in nature.

We have here the problem: what is positivism? Which part of Comte shall we retain and which discard when there is inconsistency or disagreement in his own statements?

Comte's prejudice against metaphysical thought led him to regard his own theories as positive and not metaphysical, but we don't have to make the same error. He postulated three necessary stages of thought: the theological, metaphysical and positive, which could be roughly glossed as religious, philosophical and scientific, and then he proceeded to heap scorn on the first two, thinking he had disposed of metaphysics for all time.

It has been pointed out that the act of preference itself for the scientific over the metaphysical, i.e. philosophic, is necessarily a metaphysical choice. To put it another way, science demands the use of empirical observation and the application of rigorous logic, but the demand itself is not the result of reasoning by induction or deduction, but rather by seduction—it appeals to us only because we have found it the shortest road to predictability and order—which we like. In a word, it seems 'reasonable.' In a fundamental sense, our choice of method is always by seduction. Whether one chooses to explain the universe by theology, metaphysics or what one calls science depends ultimately on whether we like one method better than another. Thus the most avid positivist can only minimize the role of metaphysics in his approach to the study of reality; he can never hope to eliminate it.

This is embarrassing to scientists who, like Comte, wish to have done with metaphysics. But the first requirement of the method of science is the formulation of a hypothesis—a metaphysical procedure which is essential—whether it is performed consciously or unconsciously. Only the disproof of a hypothesis proceeds by the scientific use of observation and application of rigorous logic—not its formulation.

Thus I maintain that Lévi-Strauss is not departing from positivism when he uses dialectical philosophy; he is merely using it in the necessarily metaphysical procedure of formulating hypotheses with which to approach the study of empirical reality.

JAN W. MINNICK

Los Angeles, California


Str.—Dr. Bascom's letter raises an important question about the apparently 'puzzling' point which arises from my article on 'Kola Hospitality' (MAN, 1964, 53). The question is: Why in Table I (of this article) do lineages 3, 4, and 5 present kola to lineage 2 before the senior lineage?

The simplest answer to this question is that lineage 1 is not accorded any 'seniority' in the authority sense of the word. At the lineage-group level, Nsirimo manifests atypical lineage 'behaviour' for historical and political reasons. Like some other Igbo communities now commonly referred to as 'towns,' Nsirimo has no tradition of descent from a common ancestor. Two streams of population—one from the east and the other from the west—are said to have settled in the present territory at different times, in the remote past. The western stream, headed by lineage 1 (and some informants claim that it simply preceded in point of time), is accorded primacy of settlement but no seniority. The fact that lineage 1 retains the guardianship of Ala, the Earth deity, does not matter. We are, in fact, dealing with a fiercely egalitarian structure where the political autonomy of each lineage (village) is guarded and respected.

About five years ago, the question of 'seniority' among the five lineages became a burning issue following the disagreement among the local Councillors on the order of roll call in a wider political community erroneously termed a 'clan.' When the 'town' met to discuss this question, lineage 4 claimed to be Opara—lineage head, and should come first. It was challenged by an opinion leader from lineage 2, who wanted to know the names of lineage 4's father and mother, for 'an Opara must have had parents.' This proved an unanswerable question since it is generally believed that the 'founding fathers' of these five lineages came from different areas. Following this, the assembly dispersed without reaching any decision on the matter.

For historical reasons which my informants do not remember, lineages 3 and 4 co-operate and compete as a unit against lineages 1, 2 and 5. Fig. 1. of my article illustrates the territorial contiguity between these competing and often rival social groups rather than the proximity of their kinship relationship. It appears to me that what operates at the lineage-group level is a territorial principle phrased in kinship idiom. Within each lineage, however, the ancestors are not in doubt and the seniority principle is respected within the differentiated segments of each lineage.

The curious position of lineage 5 is resolved by the local tradition that it is a 'late comer' from the eastern stream of the incoming population which decided to co-operate with lineages 1 and 2 after its disagreement with lineage 4, the 'leader' of the eastern stream. In passing kola nut to lineage 1 through lineage 2, lineage 5 is applying the principle of geographical proximity rather than that of seniority.

Briefly stated, Table I of my article does not pretend to present a representative model of Igbo lineage structure. It rather documents the ethnographical reality of the presentation of kola nut in one Igbo village group where two different principles seem to be operating at different levels: at the village or lineage level, the principle of territorial contiguity and/or political convenience operates but is conceived only in kinship idiom; within each lineage, seniority is well defined and kinship principle and idiom are applied without any manipulation. But in other Igbo village groups where seniority is acknowledged, territoriality is irrelevant since the kinship principle is primary.

VICTOR C. UCHENDU

Crown Point, New Mexico
The Study of Beads. Cf. MAN, 1965, 3

53
Sir,—In the interesting and valuable paper by Dr. A. P. du Toit (MAN, 1965, 3) he refers to 'Indian beads,' 'Indian red,' etc. As I tried to point out for South-east Asia in a previous paper (MAN, 1964, 50), visual judgements of beads, even aided by the microscope, can be gravely misleading, as Dr. van der Steen has demonstrated more widely (MAN, 1956, 27, 154; 1963, 219).

What is the evidence that any of the beads Dr. du Toit describes from Rhodesia were actually made in India as he suggests? And what is the definition of 'Indian red' (not in the standard colour charts which we have been using for bead description)?

The great value of Dr. du Toit's paper is correlation of beads with Carbon 14 results. But if one objective scientific method is used, there is every cause to use others—in this case, chemical and spectrographical glass analysis. This would eliminate some doubts, and—in view of the extensive available literature in that field (not quoted by Dr. du Toit)—probably ensure some realistic localizations, in place of informed but essentially subjective guesswork.

TOM HARRISON
Sarawak Museum, Kuching

54
Sir,—It was with interest that I read the preliminary survey of the beads from the important site of Inonghe Illele (MAN, 1965, 3). In one respect it is, however, misleading. Gold beads are only mentioned as being present in Burial 3, whereas they were in fact also found with Burials 1, 2, 6 and 8.

In the interests of strict accuracy it should also be noted that the excavations which led to the finding of the main burials were undertaken by the National Monuments Commission of Northern Rhodesia, not the Rhodes-Livingstone Museum.

As postscript it might be pointed out that Dr. W. G. N. van der Steen has pronounced the blue glass 'Melon' beads found with Burial 3 to be of Romano-Egyptian origin.

Kampala, Uganda

JAMES H. CHAPLIN

Notes


55
In a previous issue of MAN (1964, 140) I offered a criticism of Livingstone's definition of prescriptive patrilateral cross-cousin marriage (MAN, 1964, 59), and cited Siane ethnography in support of that criticism. Mr. Andrew Strathern has since taken issue with some of my assertions (MAN, 1965, 37). I think, however, that Mr. Strathern is concerned with a slightly different problem from mine.

My quarrel with Livingstone was concerned with the applicability of constructed models to ethnographical material. Livingstone tried to show, by use of a model, that Siane was a mirror image of Purum. Because the model (or 'definition') he used would have distorted the Siane material beyond recognition, I argued against him.

Mr. Strathern appears to think that I also wished to pronounce on Siane social structure. However, my consideration of Siane social structure went only as far as was necessary to show Livingstone's faulty interpretations of it and its incompatibility with his 'definition.' It was not my aim to disprove cross-cousin marriage among the Siane. To do this I would have had to debate Salisbury's analyses and I did not undertake that.

I consider that my criticism of Livingstone still stands (Livingstone himself has not replied), and that Mr. Strathern's questions on reciprocity and patrilateral cross-cousin marriage have to do with altogether different problems. In reference to Siane social structure I would offer the following remarks for Mr. Strathern's perusal.

Salisbury, in an article (MAN, 1964, 213) apparently written without reference to mine (but which has praises for Livingstone's effort), suggests the same solution to Siane and Siane-like systems as does Mr. Strathern: namely that a patrilateral system is possible if there is a pool of sub-groups in a major group which can supply the required wife-givers and wife-takers for a potential spouse outside the major group. Exchange of 'sisters' in the kinship idioms of patrilateral cross-cousin marriage is then possible. This is a moot point, but even if it be granted, it does not solve the problems in analysis of Siane social structure. Salisbury, for instance, has had to go to the expedient of postulating two model systems in Siane to support his patrilateral interpretation.

I would remind Mr. Strathern—and Dr. Salisbury—that Lévi-Strauss's scheme is not limited to prescriptive systems (elementary structures), but is also intended to extend to other systems of kinship and marriage which Lévi-Strauss calls 'complex structures.' Lévi-Strauss characterized Africa as one of the territories of complex structures, and it is perhaps not fortuitous that African models have been broached in New Guinea. I would agree that Siane social structure can be looked at from the point of view of reciprocity, but I am not convinced that it can be explained simply as an example of an elementary structure (see my letter, MAN, 1964, 140, note 2). I believe that Salisbury's analysis is too restricted. Salisbury should remove some of the theory from his viewpoint and consider his facts in a more impartial light.

I am not now in a position to discuss this problem further. I am working among a people whose marriage system is indubitably 'complex,' and, unlike Mr. Strathern, I can obtain little relevant data; nor do I have the literature conveniently at hand.

WILLIAM WILDER
Tembeloh District, State of Pahang, Malaysia

The Rock Art of South Africa. Cf. MAN, 1965, 84

56
Sir,—In your May-June issue, Mr. James Walton, F.S.A., reviews my book Rock Art of South Africa. In reviews of one's books the rough must be taken with the smooth, but I would not like it to be thought that I have, taking my writings together, done less than justice to Mr. John Schofield, as Mr. Walton may seem to imply. His contributions including his 'Four Debatable Points' were fully dealt with in my Rock Paintings of the Drakensberg and also in a controversy with the Abbé Breuil conducted by letters in the Johannesburg Star in which I supported and defended Schofield's views. I did not say it all again as I do not like to repeat myself.

Mr. Walton has a point in referring to the use of canoes south of the Limpopo, but rather an academic one as the occurrence was not many miles south of the river, and what was reported in 1952 can hardly be called historic in the sense in which I used the word.

ALEX R. WILLCOX
Johannesburg

Fumigator or Altar? Cf. MAN, 1964, 137

57
Sir,—Mr. E. C. Lanning's article on 'Musiria Burial Caves' carries an illustration of what he calls a fumigator. I suggest that this object may be a miniature altar.

M. Bequaert reported on 'The Masaka Cylinder: An Interpretation of its Use' in The Uganda Journal, March, 1947, p. 24. In this connexion my own article 'Altars or Sacred Stools: The Ibo Tazza or Aka,' MAN, 1955, 46, has a bearing. If the object is an altar it becomes the seat of the soul and as such becomes a sacred stool.

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

This attractive volume intended not as a textbook for social anthropologists but as an introduction for the general reader, forms a useful complement to Dr. Beattie's Other Cultures. That study, also from the Oxford stables, tended to work outwards, so to speak, from a core of established methodology and ethnographical fact. This less exhaustive book, concerned frankly with the tribal world, tends rather to move inwards from the changing European literary and cultural heritage towards a comprehension of the institutions and modes of thought of exotic peoples. This difference in emphasis and procedure, appropriate to the series in which the volume appears, enables Dr. Lienhardt to suggest something of the correspondences which may be traced between the aims and overriding interests of social anthropologists and their own intellectual milieu. In these terms he discusses the evolutionists and writers like Lévy-Bruhl, as well as a later generation of fieldworkers who were often demonstrably imbued with a strong sense of liberal idealism. These last, as he points out, assuming a moral purpose and value in society itself, were not infrequently led to overemphasize the stability and order of tribal communities. Dr. Lienhardt's conclusion, however, that in the less ethnocentric world today moral problems are less likely to distort sociological analysis—since 'autonomous peoples do not require our "sympathy"'—will perhaps suggest to some readers a lingering touch of a similar idealism.

The book opens with a short account of the nature and scope of socio-anthropological enquiry and a sketch of its development up to the work of Durkheim. With this behind him, Dr. Lienhardt launches forth into a well illustrated discussion of the ecological circumstances in which many tribal peoples find themselves placed and the adjustments which they make to them. This is followed by a chapter on tribal politics which, very properly in the context, while stressing the novelty at first sight of uncentralized tribal systems, shows convincingly how these can be seen and understood as extreme types on a scale of increasing political centralization and complexity. The distribution of political power leads directly, in the next chapter, to an examination of the inter-play between economic interests and social relations, in which due attention is given to the ethnographically more colourful forms of conspicuous consumption and gift-exchange. Kinship and affinity, both in their formal and functional aspects, are dealt with next; and are followed by a concise but quite wide-ranging discussion of religious beliefs and values. Here religion is taken to embrace a wide spectrum of conceptions of ultimate power, and the intellectual element in the construction of cosmology is stressed. There is also a refreshing approach to the comprehension of voodoo beliefs through the study of the historical development of messianic cults; and Evans-Pritchard's analysis of Zande witchcraft is applied, too tentatively perhaps, to demonstrate the general properties of closed systems of belief on whatever premises they may be based. All this is capp'd, in the concluding chapter, by a brief assessment of the methods and assumptions of contemporary social anthropologists.

Throughout, the book is illustrated by well chosen ethnographical examples which fall neatly into place as the argument unfolds, and these are supplemented by a generous selection of telling and sometimes arresting quotations from often unexpected sources. The style is generally lively and interesting; and, what is ultimately most important of all, the work succeeds in conveying something of the excitement of the subject. The text closes, as it opens, with a quotation from Tylor commending social anthropology to the general public, as the catalyst which brings together in a manageable and meaningful whole 'the scattered subjects of an ordinary education.' This has a surprisingly modern ring. And if, as present indications seem to suggest, social anthropology is increasingly coming to be regarded in this light by a wider public, Dr. Lienhardt's introduction will surely play a part in making the matter further.

I. M. LEWIS


Social anthropology is an eclectic subject. In describing the societies and cultures which they have studied, anthropologists have written about their political organization, their 'law,' their economics, their religion, their cosmologies, and many other topics. But few of them have been professionally trained as political scientists, jurists, economists, theologians or philosophers. Does this mean that social anthropology is a pursuit for amateurs, for dilettante dabblers in fields professionally the preserve of specialist scholars? This most of us would indignantly, and rightly, deny, but the problem remains, and this book is the first serious attempt to deal with it.

The central question to which Professor Gluckman and his one-time colleague, the economist Professor Ely Devons, address themselves is, to quote from the blurb, 'when a social anthropologist's research leads him into a field which belongs to other disciplines, what line should he adopt? What use may be made of the results that other scholars have already achieved?' The question of course implies that social anthropologists have a field which they can be led out of, and this, 'defined operationally,' is 'the study and comparison of tribal societies and small fields of social life with emphasis on the role of custom' (p. 13).

The short answer to Professor Gluckman's question is that social anthropologists can and do make, and always have made, some use of the findings of other disciplines, and that they are bound to do so 'naively' since they cannot be experts on everything. Of course this is true of all social science, even of all science; after all, the fact that few of us are trained linguists or biologists does not inhibit us from using words. But what is original about the present work is first the incorporation of essays by scholars some at least of whom explicitly ask what use they are making, or should make, of non-anthropological data, and secondly the editor's and his colleague's detailed analysis of the ways in which such data may legitimately be used.

As well as a brief Introduction and a long Conclusion (over 100 pages) by Professors Gluckman and Devons, the book contains five essays by scholars trained wholly or partly in social anthropology at Manchester, illustrating more or less effectively the use of relevant but 'external' information made—or not made—in their researches. Thus in Chapter 2 Professor Turner, in another important contribution to his brilliant analysis of Ndembu ritual symbolism, concludes that although 'at one end of the symbolist's spectrum of meanings' the social anthropologist may join hands with the individual psychologist, the social psychologist and the psycho-analyst, his central concern is with what is normative and therefore 'cultural.' In the next chapter Professor Bailey, in an interesting comparison of inter-caste disputes in two Orissa villages, claims that adequate anthropological understanding of these disputes does not require an exhaustive scholarly knowledge of the Sanskrit texts which underpin Hindu culture (though obviously a social anthropologist working in a Hindu society should know something about them). Again, since social anthropologists study communities, our investigations stop, he thinks, 'at the point where relationships tend to become single-interest, specialized, and therefore of interest to ... political scientists and economists' (p. 73); where, in other words, they cease to be community relationships.

With the next essay, Dr. Epstein on the differences between the 'Mine Township' and the 'Government Township' on the Copperbelt, with particular regard to the emergence of African political institutions in the towns, we move out of the traditional field of social anthropology into urban sociology. But here again limits have to be drawn, and such 'outside' factors as the wider political scene and mine company policy cannot be fully analysed, though they must be noted. In Chapter 5 Professor Lupton and Dr. Sheila Cunnison, in their analysis of workshop behaviour in three Manchester factories, move even further away from traditional anthropo-
logy, but they are still concerned with the same problem. 'It is inevitable,' they write, 'that, in studying modern industrial societies, sociologists and social anthropologists will be dealing with the same areas of social life as economists, political scientists, social historians, and psychologists' (p. 127). They conclude that 'at least an elementary knowledge of the work of these other scholars is called for.

In Chapter 6 Professor Watson discusses social mobility and social class in a Scottish mining community, and his conclusion seems to be that 'the plurality of prestige systems' in which he is interested can only be understood if account is taken not only of local communities, but also of 'regional or national organizations' of all kinds and at all levels. Just how this fits in with the theme of the book is a little obscure to me, as indeed it seems to be to the editor.

In their concluding section, Professors Gluckman and Devons set out in detail some of the ways in which social anthropologists may take account of the findings of other disciplines, for example by the incorporation of certain events without bothering about their internal complexity, by the abridgement of theories and hypotheses from companion disciplines, and by simply making justifiably naïve assumptions. In the course of a critical review of their contributors' essays they go on to reconsider the problem of how to isolate an area within a larger whole for systematic study, and they point their moral (which is that social anthropologists should generally stick to their own laws) by a comparison of Evans-Pritchard's and Kluckhohn's studies of witchcraft, holding Evans-Pritchard's treatment more satisfactory, because he confined his analysis to cultural sphere ('stated' feelings), and did not 'trespass into the field of another discipline' by offering explanations in psychological terms.

This is an original and stimulating, if occasionally repetitive, book, and we should be grateful to its authors for having brought into the open some important questions which are only too often slurred over or taken for granted. Even though we may hope that the more adventurous among us will sometimes ignore Professor Gluckman's stern 'No Trespass' warnings, his discussion of the hidden traps and snags which lie on the other side of the fence will at least help us to keep our wits about us when we venture into disputed territory.

J. H. M. BEATTIE


This is a book of the first rank, and most useful to the worker in the social sciences that sets out deliberately to create a dialogue between the realist and the positivist and the methodological standpoint on the one hand and the behavioral scientists on the other. The author's grasp of the methodological issues, dilemmas, achievements and limitations of the latter group is first-hand and judicious, while apparently not having been achieved at the expense of technical erudition in his own field. His treatment of the entire range of methodological issues—experimentation, theory-building, model-construction, observation, the use of statistics, the value problems of explanation and application of finding, etc.—is unique in the degree to which it bridges the gap between the scholastic philosopher of science and the practical-minded social researcher. His book is one that can form a handy reference volume to lend depth of methodological rationale to the more practical handbooks of research method in the behavioral sciences— and lend comprehension in the framework of a single community of scholarship to such diverse works as the efforts of Freud, Weber, Jahoda, Mosteller and Lazarsfeld.

If the book has a fault from the point of view of the methodologically reflective anthropologist, it is its lack of balance in illustrative content as between the psychological and the socio-cultural levels of the phenomena. The former are far more thoroughly studied and used in illustrations. Durkheim is mentioned only once in the text, and Radcliffe-Brown not at all, though 'Structure and Function in Primitive Society' is listed among the references. The author seems unacquainted with Malinowski, Nadel, Redfield, Firth, Fortes, Gluckman, Parsons, Merton, Lévi-Strauss, etc., all having some interest in methodology. He relies, for his sociology, mainly on Weber, other references being in the area of the quantitative

methodologists rather than of the analysts of social structure and culture. The social psychologists who, in America, are perhaps the most methodologically conscious of the social scientists, fare better, in keeping with the author's generally greater sophistication on the psychological level.

If Professor Kaplan is somewhat off-balance in terms of the distribution of his attention as among behavioural-science disciplines, he is very well balanced in his treatment of the issues in the field generally. Indeed, balanced perspective is the most notable thing about the book for a whole, and anthropologists can certainly benefit from studying this book because many of the issues besetting scientists in the spectrum of approaches on the psychological level are very similar to those at the socio-cultural level.

Kaplan makes it quite clear that the process of discovery entails a degree of persistence in the face of vagueness, and trying out various techniques. The work of the clinician (astute case worker) thus has its place along with the psychometrician (quantitative social scientist). Both the cultivation of vagueness and the worship of technique are condemned as inexcusable. The problem, however, has not on the whole been one of charlatanry, but of the selection and emphasis of inappropriate models in behavioural science—e.g., the model of the cellist's mechanics with its emphasis on prediction rather than the model of evolutionary biology with its emphasis on explanation.

While this viewpoint is not new to the anthropologist, its reiteration in authoritative terms, richly rationalized and documented, can be quite useful in a period in which new methodologies must be accepted, but with a sense of perspective. As the era of computer technology and mathematical models advances upon us, rigidities both of over-espousal and of puristic rejection may hamper the proper absorption of the new methods into a broader armamentarium of the social sciences. As Kaplan points out, 'verbal assent may be given to the truism that each of the methods has its place, but the commitment of practice, very often, is to the attitude of defensive incorporation and exclusion: this is the only thing worth doing' (pp. 275f).

The role of the methodologist of science is to describe the overarching array of methods within which any specific approach—whether it be computer methodology or structural-functional methodology—may flourish in healthy interaction with the others or be restricted by a too limited view of its own nature and place in the world. Kaplan's service lies particularly in the attention that he has given to the special conditions governing the contemporary behavioral sciences in their adaptation to the methodological jungle. In this sense he is, as the sociologist Leonard Broom points out in his introduction, a 'most useful philosopher'.

ROBERT N. RAPOPORT


In these Terry Lectures (Yale University, 1957) Dr. Mead engages the issue of cultural evolution and comes up with something in the nature of social science fiction. Anthropologists were not able to find here any considered treatment of the course of evolution, for true to her major interests, Dr. Mead has concentrated on the bridge to the future and how the developing child and the individual should fit into the picture. The stroke of the brush is bold and impressionistic. This is quite in order, for Dr. Mead has always been crossing frontiers and blazing new trails. However, in detailing the omnipresent needs of anthropological research, too much space is devoted to discursive amplification. But anthropologists might well spend some thought on Dr. Mead's discouraging conclusion that sound advances in theory and method have been turned into cults by special in-groups or diluted and fragmented in subsequent application.

Dr. Mead's whole approach to the evolutionary issue is rooted in a confrontation of the threat of nuclear catastrophe. How are we to ensure that man and his descendants can get on with the present and get into the future? Hence, the concern with continuities in evolution and the deliberate attempt to plot 'new ways in which we

Of the theories concerning man's behaviour, psycho-analytical hypotheses are at once the most in need of cross-cultural testing and at the same time the most difficult to submit to a process of this kind. They are difficult partly because of their generality and partly because of their multiplicity; it is not always easy to be sure just what to test. Again, the very fact that these theories are concerned with unconscious processes, characterized by substitution, reversal, inhibition, disguise and other covert mechanisms, makes them yet more difficult to test against evidence derived from the usually very overt observations of the ethnologist. Nevertheless, in recent years a number of interesting attempts have been made to face these problems and to see what the available data from non-European societies (and the question of availability needs stressing here) have to say about such theories and their related constructs. The present study is part of a series of investigations carried out at Harvard Laboratory of Human Development, under the aegis of W. J. Whiting, which make use of various cross-cultural procedures, including the Human Relations Area Files, in order to try and assess the relative weight of certain components of human action in different societies (i.e. to measure variables).

This book is organized as a series of tests of the 'Oedipal complex'. In the simplest term (for purposes of validation), the central hypothesis of Freud and Fenichel is formulated as: 1. Young boys customarily become sexually attracted to their mothers. 2. As a result, they feel hostile and rivalrous towards their fathers. 3. This has lasting effects upon their personalities, taking the form of (a) unconscious fantasies, (b) sexual fears and avoidance. Stephens is concerned only with 1 and 3 above. He tries to test the hypothesis that the greater the attraction of boys for mothers, the greater the incidence of sexual fears of avoidance. This, of course, can be done directly from ethnological data, but few of us have much to say about either topic. The problem is to test and define frequently reported behaviour that can serve as indices of son-mother attraction on the one hand and sexual fears on the other.

For his major antecedent variable, Stephens follows Whiting and Kluckhohn (1958) in choosing the length of the period of sexual abstinence observed by the mother following birth (the post partum sex taboo). During this time of confinement a woman is child-centered rather than husband-centered, and the authors presume that the longer the abstinence the greater the attraction between mother and child.

The magnitude of the dependent variable, sexual fears, is assessed by the strength of various prohibitions surrounding sex, which are taken as indices of underlying anxiety. The most important are the extensiveness of menstrual taboos and the severity of kin avoidance. Menstrual taboos are seen as reflecting fear of genital injury (i.e. castration anxiety), arising from fear of the father. Following the suggestions of psycho-analytical writers, Stephens sees the fear of incest as lying behind most cross-sexual kin avoidance, and this he again relates to the Oedipus complex (mother-son seduction plus father-son rivalry).

The central hypothesis of the study is expressed in the prediction of a correlation between a long post partum sex taboo, and the severity of menstrual taboos (castration anxiety) and kin avoidance (incest phobia). The correlations turn out to be significant (though one must remember the usual limitations of such 'samples') with a P-value of 0.01 for the severity of the avoidance with the sister and the wife's mother, and 0.02 for the extensiveness of menstrual taboos.

As a result of these and other tests, Stephens concludes that the probability is high that this hypothesis, embodying several of the core-assumptions of psychoanalytic theory, is approximately valid.

The argument is ingenious, the tests interesting. It is a line of work that sociologists in this country will find only too easy to criticize, in terms of the limitations of the 'sample,' the relatively low reproducibility of the coding (80 per cent.), etc. But these criticisms are ones that apply to the instrument itself, which crude as it may be at least offers some prospect of testing assumptions rather than simply

FRED W. VOGET
Social Science and Political Theory. By W. G. Runciman. Cambridge (U.P.), 1963. Pp. viii, 200. Price £1 2. 6d. As distinct from social anthropology, sociology in modern Britain was until recently almost confined to the University of London. The last 15 years have seen a transformation—though there are still places where social policy and social work, admirable in themselves, are thought to be equivalent to sociology. One result of this has been that the tradition of sociology in London has been rather verbal than literary, and one is sometimes surprised, quite wrongly, to find books like this one from Cambridge which are largely concerned with 'what everyone knows.' Reflection soon shows that this is unfair and that besides giving, at their best, new rigour and specificity, such books are genuinely novel in being books, in existing and in being necessary to the development of the subject.

Mr. Runciman’s is, on the whole, an excellent example of what I mean, and also something more. His book is an introduction to discussion of the standing and justification of sociology, to political sociology, to the work of Marx and Weber, and to the bearing of all this on political theory. It is clear and careful, though Mr. Runciman assumes that logical alternatives are usually simpler than a more empirical consideration would suggest. I believe that his concentration on Marx is mistaken, not as exposition—there it is excellent—but in thinking that Marx was a sociologist. I feel that this in turn leads him to see Weber as larger than life and to fail to get as much from Durkheim as he might. I think that he misses a point about Pareto—the importance of the first two residues in political sociological models. I am sure that he neglects the ‘ethnographical’ element in sociology—what seems to me one of the main points of theory: its making possible of better descriptions and accounts. On ‘bourgeoisification,’ I judge Dr. Lockwood and therefore Mr. Runciman to be wrong and naive, because not Durkheimian enough—and this is a large matter.

More centrally I believe that epistemological and methodological problems are of very little importance to a discipline. They are problems of interest to philosophers and logicians, but they are subsequent and subordinate to sociology from the point of view of the sociologist. One cannot but make remarks that have epistemological resonances, but to mix this up with sociology is to muddle one’s categories and confuse one’s goals. But in most of these remarks on Mr. Runciman’s themes I am stating personal judgments. A consensus of opinions in sociology departments would not be with me. In terms of that consensus and in my judgment Mr. Runciman is brief, cogent and sensible. In his first three chapters and his last he is also original.

In the earlier chapters the originality is no less valuable for being mainly exegetical. (One is reminded that Mr. Runciman has been a classic.) But at the end Mr. Runciman is original in his insistence on the involvement of political philosophy with political sociology. He makes no muddle of categories here, but he drives home the lesson that a political philosophy is both possible and dependent, if realistic, on political sociology for the best available account of the nature and structure of political behaviour. This needs to be said, and Mr. Runciman says it well and defends it convincingly. Not only his instance from the Tallensi would have been a relevant use of anthropological data to drive his lessons home.

DONALD G. MACRAE


This is the first sociological survey in this country of the intergroup relations between the police and the general community, though such work has been done for several years in the United States. Dr. Banton examines in somewhat minute detail the police as an organ of social control and the relations that follow, but perhaps the special interest of the book derives from a comparison and contrast between the situation in Britain and that of the United States, and in this respect the work is new. For the author has studied and worked with the police in Edinburgh and during 12 months in 1962, while visiting Professor of Political Science at the Massachusetts Institute of Technology, and worked with police of various centres in that State and for some weeks at other police centres especially in Georgia City and Carolina City, and (most important of all) has done patrol work with all these forces. This comparison has brought out with clarity the differences of social control in the small-scale, stable society of a village, social control in a British city, and social control in the American society with its low social integration, its millions of immigrants of different national backgrounds, its far higher crime rate and the resulting differences in the functioning of the police and the relations between police and community.

The interest of the book is topical. The situation in this country in these respects moves daily nearer to that in the United States, and since Dr. Banton wrote his book the situation has considerably deteriorated. The extent of crime here continues rapidly to increase (as it has ever since 1938) and the social integration to weaken, with the same effects (i.e. the view of the law, the view of the interpersonal and the structure of the community). Nor should the British reader complacently assume that the police of this island are in all respects superior to those of America. There is reason to think that the British police stands a little too much apart from the general community and also that the American police organization is commonly more flexible and less wasteful of its numbers, and if its task is more difficult examines more searchingly its suitability for it and for changing circumstances. This is not an exciting study, but its quiet examination of
some relations fraught with anxious problems deserves to be widely 
read, and perhaps especially by the intelligent lawyer and the judge. 

A. S. DIAMOND

On Culture and Social Change. By William F. Ogburn, edited with 

The 25 essays in this collection are classified under four headings: 
'social evolution'; 'social trends'; 'short-run changes'; 'methods.' 
The earliest paper dates from 1912; the latest from 1961. The author 
deals with a wide range of social facts, including 'culture,' cultural 
lag,' 'social trends,' inventions, technology, race relations, the 
family, and business conditions, as well as making methodological 
pronouncements. Social anthropologists, censured for attempting 
to explain 'cultures' instead of 'culture' and defining the problem 
of social evolution incorrectly (!) (p. 31), cannot help but regard 
with apprehension the superficial treatment of social phenomena 
and the many generalizations which abound in these essays. 
The sociologist might find them of only slightly more interest. 
The majority of these generalizations are based upon undocumented 
results while quotations and statistics (used in figures, charts and 
tables) appear only rarely with an indication of their exact source. 
Otis Dudley Duncan, in the Introduction, attempts to explain 
these curious and unscholarly omissions in writings which have 
secure place in the history of sociology (p. vii), by saying: 'When 
in the course of an argument he appeals to a fact or proposition 
without giving data, the presumption is that the matter is one that he 
'once looked into' ...' (p. viii). A less benign critic will have 
less confidence in this presumed assurance. All the essays are easy, 
and most of them are interesting, to read, but their value would 
have been enhanced by the addition of an index. There is a useful 
bibliography of Ogburn's works and it contains almost all his 
principal writings.

DAVID HICKS

Mankind Behaving: Human Needs and Material Culture. By 
James K. Feibelman. Springfield, Illinois (Charles C. 

The planning and writing of this book must have involved a considerable and continued speculative effort; and so, 
to be sure, does the reading of it. This is not because it contains 
anything particularly novel—except some of the terminology—but 
mainly because the author attempts to cover, and to explain pretty 
nearly everything in what he calls 'mankind behaving,' and naturally 
this is a big job. The subtitle, which links human behaviour with 
material culture, is maybe the main reason why the general treatment 
can be claimed to appeal to anthropologists and other students of 
society.

Broadly the thesis is that all human modes of behaviour are 
based upon continuations of animal modes. Dr. Feibelman does not 
claim himself to have made any direct studies of animal behaviour, 
but he has read very widely in the works of those who have, and his 
principal authorities are Pavlov, Sherrington, Skinner, and, to a 
seemingly less degree, Clark Hull. Also, especially in the later parts of 
the argument, he relies considerably upon D. O. Hebb and the 
ecologists Lorenz and Tinbergen.

Animal behaviour is regarded as the immediate expression of 
'drives,' all the commonly recognized of which receive due attention. 
These continue in human beings, but can be, are in fact, much 
developed and their main determining conditions changed. This is 
where 'material culture' comes in, for most, possibly all, the changes 
are due mainly to human invention in the way of the production of 
tools and signs. The chief needs, or drives, that fall to be considered 
in regard to human behaving are: the need to know, the need 
to do and the need to be, which, it seems, would better be 
called the need to go on being. These three are discussed in very 
considerable detail and with very many illustrations, always within 
the headings of a general scheme, derived by the author from his 
studies in the literature of animal behaviour. These headings are: (1) 
the tropistic response—natural or unlearned reactions towards or 
away from specific objects; (2) the releaser, as conceived by the 
ecologists, (3) the preparatory response, which is whatever brings 
the objective into active relation to the person behaving, (4) the 
consummatory response, which is the actual behaviour, (5) the 
reward, which is whatever tends towards the repetition of the 
behaviour, (6) the reinforcement, which makes the behaviour 
progressive, and (7) the reverberating circuit, which gives to human 
behaviour its more or less constant and continuing pattern.

This very brief summary may make it appear that the book is 
even more formalized than it really is. But the volume is, in fact, 
a strenuous, undoubtedly able, and well informed, formal study of 
the entire field of human action. It will appeal to any students who like 
this kind of thing and are prepared to put up with a lot of not very 
easy terminology; but it is difficult to see that it does very much to 
develop the topics of its discussion by the addition of fresh 
knowledge or really original ideas.

F. C. BARTLETT

Theory of Relationships. By Stanford L. Silverman and Martin 

Applications of Graph Theory to Group Structure. By Claude A. Flament. Englewood 

The subject of this pair of books, the use of formal methods in 
the study of relationships, is of the greatest importance for social 
scientists. In principle great clarification of social theory can come from 
more careful definitions of concepts of social relationships 
and from more adequate deductions of the consequences of particular 
social relationships.

The two books provide a great contrast in the tactics for constructing 
formal methods for the study of relationships, and in doing 
so present a neat object lesson. A great advantage of formal methods 
is that the abstract properties of relationships can be differentiated 
from the particular concepts or metaphors that are ordinarily used 
to express these relationships within any particular subject matter. 
Thus a desirable formal method would: (1) be stated in symbols 
and rules that are widely recognized and used by students of formal 
metho;ds; and (2) avoid the use of subject-matter concepts in its 
formal statement, leaving the subject-matter concepts and 
interpretations to be specified in particular applications.

The book by Flament meets the criteria above; the book by 
the Silvermans does not. Flament employs the conventional 
mathematics for the study of relationships—set theory—with elaborations 
in graph theory and in matrix algebra. Flament does not attempt 
subject-matter-interpretation until formal definitions and theorems 
have been presented and analyzed.

In contrast, the Silvermans employ a private mathematics in their 
exposition (apparently they think that they know how to use vectors 
to compute resultant forces). Further, they use subject-matter concepts in 
hopes of improving the reader's intuition; worse, they use subject-matter concepts from physics in hopes of building one's 
intuition for applications in the social sciences (apparently they are not familiar with the conventional definitions of their own concepts 
in physics).

The serious social scientist should read Kemeny, Snell and 
Thompson, Introduction to Finite Mathematics, then read Flament. 
He will be richly rewarded in conceptual clarity, and will be able to 
construct his own subject-matter-interpretations in line with his 
own professional prejudices. Further, he will be able to comprehend 
the theoretical development of many other subject matters that have 
turned to the common fountaineau of mathematics in order to 
state relationships.

JAMES M. BESHERS

Family and Marriage. Edited by John Macey. Internat. Studies in 
Soc. and Soc. Anthropol., Vol. I. Dharwar, India (Dept. of 
15 gilders.

This book, originally published as a special number (Vol. III, 
No. 1, September, 1962) of the International Journal of Comparative 
Sociology, now appears in hard cover as the first volume in a new 
series entitled International Studies in Sociology and Social Anthropology, sponsored by the Department of Social Anthropology,
Karnatak University. It consists of ten papers by different authors on various aspects of marriage and family life. Among the first six papers, for example, there are those that propose hypotheses to explain variations in family forms, and raise questions about the root causes for the emergence and wide spread of the nuclear family. T. N. Madan deals with the term ‘joint family’ from the standpoint of comparative usage. Chie Nakane discusses alterations in the social functions of the Nayar ‘tarwad,’ and notes the emergence of the elementary family, but emphasizes the persisting strength of matrilineal ideology. J. Clyde Mitchell bases his paper on his own fieldwork among the Yao of southern Nyasaland, and shows how the distinction between uxorial and genital rights enables us to deal comparatively with many distinct family types. Chandra Jayawardenage limits his study of Indian families in British Guiana emphasizes the importance of post-marital residence and the tendency of variations in family structure. Emilio Willems examines some relationships between family structure and social class in contemporary Portuguese society. Norman Dennis reviews English studies and finds that goals sought in the marital relationship have changed in the course of this century (e.g. growing search for love and affection), and points to causes in parallel social changes such as greater specialization in industry and the mounting growth of large urban centres.

The remaining four papers comprise two on mate-selection by George Karlsson, and by Peter Jacobsohn and Adam Matheny, one entitled ‘A Cross-Cultural Comparison of Attitudes Towards Marital Infidelity’ by Harold T. Christensen, and finally a comparative study of support and power exercised within the family by Murray A. Straus and Solomon Cynthybaum. A very useful introduction by John Moggex brings together the salient points of the paper and indicates their relevance for future comparative research in this field. As a whole these papers represent fine scholarship, are well written, and raise useful questions for research.

M. L. PERLMAN


Man’s evolution, for us an established fact and a fascinating game (‘who is discovering whom?’), was a serious problem to our fathers and grandfathers; too many refused to swallow the truth. In what modestly is called ‘a historical survey’ K. P. Oakley, with a wide knowledge of the history of the discoveries, and the theories and the fallacies of scientists, traces with a fine sense of humour the various steps, which have made from flaked stones and broken bones and skulls a basic science. Because accidentally Man’s history is our own, we have reason to be proud of our ancestors who had the misfortune of living in damp caves before the central heating, the atom bomb and other household articles had been invented.

While Oakley has singled out a number of historical finds—Aurignac, Neanderthal, Cro-Magnon, Java and Heidelberg Man—, the fascinating problem is dating. He places the various finds against their stratigraphical setting, here called ‘relative dating’—, discusses Ice Ages and industries, and then switches over to the ultramodern laboratory asking for an absolute age—by him called ‘chronometric dating’—, an age that can be expressed in years. A bone can be measured by C14, a layer by potassium-argon. Possibilities are discussed and in the last section a list is given to reveal the birthday of a number of finds discussed.

The book, written for the layman, is a pleasure to read. And what is most important to every man in the study of archaeology but too often neglected—it contains ample notes and references to the literature used.

G. H. R. VON KOENIGSWALD


This book, one of the Global History Series, is a digest or anthology of prehistory. Its contents are culled from books and periodicals, of which the oldest is Child’s Man Maker Himself (1948). Nine others date from the nineteen-fifties and only five from the sixties. There is an introductory chapter by the editor and the others are arranged more or less in chronological order, from Evolution and the Origins of Culture (Sherwood F. Washburn) to The Nature of Civilization (Robert Redfield). Amongst the 15 authors, such names as Jacqueta Hawkes, Gordon Child and Kathleen Kenyon speak for themselves. Some of the others are ‘popularizers’ rather than professional anthropologists, but all are sound. They cover predynastic Egypt, the Indus valley civilizations, the Shang culture of China, and Zimbabwe, while there are no less than five articles on the New World, including very good accounts of Pre-Columbian Civilization (Eric R. Wolfe) and Prehistoric Cultures of the American Southwest (Paul S. Martin).

The chapters vary considerably in length and some of the chapter headings are misleading: for instance, Peasant Farmers in Europe consists of a three-page gem on Stonehenge by R. J. C. Atkinson; and The First Americans by Emil Haury, which is even shorter, merely describes the excavation of the Naco mammoth. The editor provides brief background sketches to the subjects, but gives no particulars about the authors or their work (which might well have been mentioned in the Preface). There is a table of dates and a short list of ‘Further Readings,’ but no illustrations and no index.

This is a good book to give the person who wants a potted world prehistory. Obviously there are enormous gaps; but the extracts have been chosen for their readability as well as their interest and they should encourage readers to look up the originals.

SONIA COLE


This is a comprehensive and very well illustrated compendium of paleolithic and mesolithic osteological and archaeological data. The author is a surgeon by trade and an anthropologist by affection. He has produced a very readable account of all the major and most of the minor discoveries in his chosen field; and he has also considered their cultural and sociological significance where this is possible. The main part of the book starts with the australopithhecines. It is preceded by a short but useful account of the relevant anatomy of man and the ape and of the glacial epochs pertinent to what comes after. There are good indexes of both authors and subjects.

The book can be thoroughly recommended to students as a companion to such works as Le Gros Clark’s Fossil Evidence and as a general introduction to human palaeontology. The author’s views upon the status of the African material will not command general approval, but he has presented the facts themselves very fairly. Apart from its pedagogic value this is an excellent work of reference for those who wish to refresh their memories upon the place, time and circumstances of particular discoveries in the paleo-anthropic field. Special praise is due to the publishers, especially for the very numerous illustrations in photogravure.

M. A. MACCONAII


This is a curiously ingenious handbook to some of the techniques utilized by the physical anthropologist in his study of human skeletal remains. It is divided into five chapters. The first three deal with the cranium, the long bones, and the remaining portion of the axial skeleton respectively. The remaining two chapters scarcely merit serious consideration, one dealing with what the author terms the ‘tricks’ of osteological anthropology, viz. various frankly subjective methods of assessment relating to traits such as dolichocephaly or orthognathism, and the other containing just over four pages of advice about writing a report on human remains.

The anatomical descriptions of the various bones and the relevant osteometric techniques are, in general, competently—if somewhat perfunctorily—outlined, but in many cases they are poorly served by the crude line drawings.

There is an old-fashioned air surrounding many of the tables of varieties, with a heavy reliance upon data originally supplied by
Broca and Topinard, and the author is content to rely upon Manouvrier's tables for the estimation of stature from long bones.

The index has many omissions, there being no reference, for example, to any or any sexing techniques, although these can be found by reference to the list of contents. The bibliography is by no means comprehensive and is somewhat dated: there is no reference to the new edition of Martin's Lehrbuch, for example.

Although the reader is disarmed by the author's suggestion that this is a simple handbook, intended for the amateur, and that serious study of human remains should be left to the expert, there are puzzling anomalies such as the inclusion in one table of Student's test for significance without a word of textual explanation or reference to another work.

"Pourquoi avons-nous écrit ce livre, alors qu'il en existe déjà de trois nombreuses," asks the author, at the beginning of his introduction. One reader, at least, is inclined to echo this sentiment.

D. R. HUGHES


This is in no sense a textbook. Neither does it develop any argument. Instead it seems that Dr. Calvin Wells is just talking about the various facets of a subject that fascinates him, talking informally and vigorously and with many a pleasant turn of phrase, and drawing on his extensive knowledge of the curious to enliven the tale the dead tell. From time to time a main theme is discernible. One is heralded in the opening quotation 'In the dead behold the quick,' that there exists an intricate relationship between a people's way of life and the diseases that they endure, for to Dr. Wells pathology is a sure guide to 'how people have responded to the aggression of their environment.' Another is that diseases themselves have histories which in some cases can be traced from remains of earlier populations.

Two brief introductory chapters discuss the significance of paleopathology and the nature and limitations of the evidence remains, skeletal and mummified, and human portrayal of pathological states in art and early writings. There follows chapter three, the main body of the book, listing the main types of abnormalities encountered, arranged on a semi-etiological basis; in each section example follows example, many illustrated photographically as well as verbally. The last seven chapters are very brief, devoted to skeletal adaptations, cannibalism, trephination, the uses of radiography in the examination of skeletal remains, artificial interference with the body in life, vital statistics, and historical characters.

It is an annoying book. The balance is inadequate—some of the later chapters are so slight that the book would lose nothing from their omission. Although there is a skeleton bibliography at the end, there is no documentation of the examples used in the text. Many of the sections on particular pathological states leave something to be desired; thus (p. 41) there is no indication of the distinctive cranial features of mongol individuals or how these are identified in burials; there is no reference in the section on tumours (pp. 70-6) to the well established examples known in earlier British populations; the high frequency of periostitis which Dr. Wells mentions among the Saxons (p. 78) is not compatible with other estimates in the literature, and his ingenious suggested explanation of their peculiar susceptibility of the tibia (chafing of the skin and underlying tissues by gartered leggings with subsequent infection) hardly accounts for the frequency with which the tibia is involved in inflammatory disorders in modern populations. The book recalls somewhat the 'strange facts about strange peoples' type of publication. Yet by its very vigour and the enthusiasm of the author, this book will awaken the interest of those with no knowledge of paleopathology, and will be read with lively, if critical, amusement by those who have some.

D. F. ROBERTS


Over three decades ago, Anthropologie, under the dynamic editorial direction of Professor J. Matejka, became an important organ of human biological studies in Czechoslovakia. The journal has now been revived by Dr. Jan Jelinek and colleagues, and from the nature of the articles which have appeared, the new series promises to be of considerable value to workers in this field. The first issue includes articles on aspects of bone growth in children, palaeodemography, new fossil remains, and late Predynastic Egyptian crania. As well as reviews and an obituary notice, there is also an account of recent anthropological meetings which have taken place in Czechoslovakia, useful to those interested in work in progress in eastern Europe. I look forward to further issues of this journal.

DON BROTHERWELL


This book is an elementary but sound presentation of the medical aspects of disorders of growth. It does not aspire to the inclusiveness of Wilkin's textbook, nor invite comparison with Bayer and Bayley's monograph on the same subject. It is written presumably for sale to medical libraries (at an excessive price) where it may be read with profit by final-year medical students and students for the D.C.H. It is of no particular interest (pace its all-too-flatulent blurb) to anthropologists or students of human biology.

J. M. TANNER


The tools which Darwin and de Candolle had a century ago to attack the problem of the origin of domesticated plants, viz. history, archaeology and geographical distribution, are made to appear antiquated by the overwhelming importance of the essential approach during the past 40 years through observation of the genome contained in the chromosomes, the study of breeding systems, and genetic experimentation. Besides, radiocarbon dating has greatly elevated the importance of modern archaeology. A survey of chromosome knowledge and its immense diversity occupies the first half of the present work. This concise digestion is a masterly survey with a wealth of correlations, fitting examples, and suggestions, urging the reader to great attention and convincing him of the staggering achievement of chromosome botany in elucidating descent and affinity. This first half of the work covers four chapters: (i) the chromosome 'morphology,' (ii) chromosomes correlated with systematics including reproduction modes, breeding systems and discontinuity, (iii) correlation with ecology, geographical distribution and migration, and (iv) plants in time, i.e. basic numbers of larger groups, adaptation, descent, and evolution. The second half of the book is dedicated to the application of chromosome botany to domesticated plants, centres of origin, centres of agriculture, culture, cultivation, and migration of cultigens, including also ornamentals. Almost all domesticated plants are polyploids in some way; more details are given on wheat, barley, oats, cereals, chrysanthemum, and dahlia. Conclusions are drawn on the mechanisms of improvement and this is of utmost importance for the understanding of evolutionary processes or principles generally, as these cannot be different from those occurring in nature, admitting that selection of man is of course mostly directed otherwise than selection in nature. My final remark is that the title should have read 'chromosome taxonomy,' since as I see it this is one of the branches of taxonomy sensu lato.

C. G. J. VAN STEENIS


Professor Father Paul J. Schebesta, S.V.D., the intrepid and indefatigable investigator of some of the world's most primitive peoples, has well deserved this birthday offering, prepared by his friends, colleagues and students. He is today the most senior of the distinguished anthropologists associated with the Anthropos Institute, and one of the last of the disciples and closest collaborators of Professor P. W. Schmidt. But unlike his former teacher, who originally inspired him with an interest in Negritos and Pygmies,
Professor Schebesta was always more interested in fieldwork than in the construction of theoretical systems, and his great and lasting contributions to anthropology are his painstaking investigations of Asian Negritos and African Pygmies. His career is a telling example of the interdependence of theoretical and practical anthropological work, for without P. W. Schmidt’s theoretical interest in the most archaic cultures (Urkulturen) Schebesta would probably never have had the incentive and the financial backing for his extensive fieldwork, yet in his theoretical works on primitive religion Schmidt extensively relied on the results of Schebesta’s fieldwork.

It is fitting that about one-third of this felicitous volume should be devoted to studies directly concerned with Pygmies and Negritos. The opening essay by Colin M. Turnbull reviews Father Schebesta’s work among the Bambuti Pygmies, and the subsequent studies by G. Hulstaert, Jean-Baptista Jadin, Anton Vorbichler, Johannes Fabian and Wilhem Dupré deal with various aspects of the cultural anthropology of African Pygmies and Pygmoids. The authors of the following three contributions are concerned with the material as well as the intellectual side of the culture of the Andamanese, the only major group of Pygmies of whom Schebesta had no personal experience. The late Lidio Cipriani, who had visited Little Andaman on numerous occasions and spent altogether nearly six months on the island, was the first anthropologist to undertake intensive work among the shy and elusive Ongi. On this is basis of this work and some excavations of kitchen middens, Cipriani concludes that both pottery and the pig reached Little Andaman some 3,000 years ago, whereas the dog, now used for hunting, got there only some 50 years ago. He concludes with the urgent appeal that this very archaic ethnic group, now counting approximately 500 individuals, should be studied in greater detail before the Ongi are lost to the fate of the inhabitants of Great Andaman and melted away under the impact of foreign settlers. Walter Nippold writes about the ‘Lebensraum und Welthilf’ of the Andamanese, and in the essay ‘Archaeology and Legend in the Andaman Islands’ some of Professor Cipriani’s findings are discussed by Robert von Heine-Geldern, who is of the opinion that the immigrants, who at one time must have brought pottery, the domesticated pig and canoes with single outrigger to Little Andaman, must either have come directly from the Nicobars, or have been a branch of the same people which coming from the mainland (or possibly Sumatra) settled on both islands.

The section which specifically deals with Pygmies and Negritos concludes with an article by Rudolf Rahmann on the reports of early Spanish missionaries on the Negritos of the Philippines and an annotated bibliography of Schebesta’s numerous writings in Czech, which are naturally far less known than his publications in German and English.

The remainder of the volume is made up of essays which have no direct bearing on Professor Schebesta’s work and range in the usual manner of a Festschrift over a wide field of ethnographical and anthropological research. Among the authors are such well-known anthropologists as J. Haekel, W. Hirschberg, H. Baumann, R. Boccassino, A. Hohenwart-Gerlachstein, G. Holker, E. Becker-Donner and M. Weninger as well as a number of missionaries belonging to the Societas Verbi Divini, Professor Schebesta’s own religious order. All these contributions are of the high scholarly standard which one expects from publications of the Anthropos Institute.

C. von FÜRER-HAIMENDORF


This work gives a wide range of speculations and philosophical deliberations on what German poets and thinkers have written about the phenomenon of man, of mankind, and human self-knowledge.

The book includes a great deal of classical Greek theory, especially as this is mirrored in German philosophy and poetry from the time of Goethe onwards ('seit der Goethezeit...'). It is almost completely void of any references to extra-European thought and European thought itself, beyond the German and classical Greek world, is mentioned only marginally.

The German philosopher may find here numerous descriptions of finesses, interpretations and re-interpretations. As for the anthropologist in the modern sense of the word (especially for one not deeply involved in German classics and philosophy), the book hardly offers anything positive beyond elaborates comparisons between man and animal.

U. R. EHRÉNFELS

AFRICA


From time to time Dr. Leakey makes tantalizing announcements about the fantastic fauna from Olduvai, but the only full accounts are in a few monographs (e.g. on the Suidae) and in his Olduvai Gorge published in 1951. To say that the present volume was 'eagerly awaited' is an understatement. It describes mostly the finds made in Beds I and II during the 1950-1951 season, after financial grants had enabled the Leakeys to work full-time at Olduvai for a whole year. Already, then, this book is four years out of date; but considering the numbers of entirely new species described for the first time; the vast amount of material collected and still being collected; and the fact that some specialists' reports are still awaited, it is astonishing that it has appeared so soon.

It includes a Foreword by Professor G. G. Simpson and chapters on the geological and climatic evidence, the mammalian and non-mammalian faunas (Olduvai is best known for its mammals, but it has also produced the richest finds of avian fossils known from Africa). Various papers from Nature on the potassium/argon dating are reprinted and there are Appendices by Dr. R. L. Hay on the stratigraphy and by Mary Leakey on the named sites.

Probably the most notable change in the Olduvai picture since the 1951 book is the discovery of a major break midway in Bed II rather than between Beds I and II. This is the boundary between Upper Villafranchian and Middle Pleistocene and between Oldowan and Chelles-Acheul cultures. It also heralds the disappearance of archaic forms of mammals and the appearance of many of the gigantic ungulates for which Olduvai is famous. There is little change in the fauna until the upper part of Bed IV, when desertic conditions probably interrupted hominid occupation for the first time (another new discovery).

Earlier reports on the fauna have been modified extensively and the 51 mammals listed in 1951 have increased to 152. Perhaps most progress has been made in describing the Bovidae, which existed in tremendous variety and in an astonishing state of preservation (some of the antelopes illustrated are so perfect that if they had horns instead of horn cores they would look more like modern sportsman's trophies than fossils).

Future volumes will describe the hominid remains and the cultural sequence, which are only very briefly summarized here. Volume I will be indispensable to all mammalian paleontologists; but all students of human evolution also will want to read this geological and faunal background to the most important early prehistoric site in the world.

SONIA COLE


Europe remained virtually unaware of the art and antiquities of Negro Africa until the end of the last century, when thousands of bronzes, ivories and wood carvings were brought back by the Benin Punitive Expedition of 1897. In the same year specimens of Congo wood carving attracted some attention at the Brussels Exhibition, and
in 1910, Frobenius’ Inner African Expedition revealed the existence of the remarkable naturalistic art of Ife. In increasing quantities, works of art from West Africa and the Congo flowed unheeded into the museums and private collections of Europe and America and, in Nigeria at any rate, little attempt was made to regulate the traffic, or to build up a national collection, until the nineteen-forties when legislation was passed to control the export of antiquities and, later, Kenneth Murray presented his own magnificent collection to the Nigerian Government.

Today as a result of the work of the Antiquities Service, the world’s most comprehensive collection of Nigerian traditional art is to be seen in the Nigerian museums, and during the past two years a sound basis has been laid for research into Nigerian prehistory by the establishment of chairs of Archeology at the Universities of Ibadan and Ife.

In his inaugural lecture at the University of Ibadan Professor Thurstan Shaw addresses the normally intelligent body of unacademic Nigerians rather than his fellow members of the University, who have long been aware of the part that archeology can play in correcting, what the Vice-Chancellor, Dr. Dike, has referred to as ‘the almost pathological unwillingness’ of the West to credit Africa with her own past cultural achievements. Professor Shaw briefly defines the aims of archeology, its scope and limitations, pointing out the recent important contributions made by African archeology to our knowledge of human prehistory and outlining the present position in Nigeria, where only a tentative beginning has been made to fit the spectacular finds at Nok, Ife, Igbo-Ukwu and elsewhere into a chronological framework which will eventually make it possible to build up a picture of Nigeria’s past and relate it to the prehistory of Africa as a whole.

One of the main aims of the lecture is to interest all thinking Nigerians in archeological research, and it forms a lively and inspiring introduction to the subject which should be widely read in Nigeria and be of valuable assistance in creating that informed body of amateur enthusiasts upon which archeological work in all countries so much depends.

The lecture is fittingly illustrated with a photograph of one of the remarkable bronzes excavated by Professor Shaw at Igbo-Ukwu, Eastern Nigeria, in 1960.

PHILIP ALLISON


The book here reviewed is one of half-a-dozen volumes, published or projected, in a series entitled ‘Library of the Early Civilizations,’ and it traces the growth of Egypt from a river valley of swamp and jungle sparsely inhabited by people still in the hunting and food-gathering stage to a civilized, highly organized state ruled by a divine king, the Pharaoh. In a sense the earlier chapters of this book could be described as ‘prehistory without tears,’ for the reader is introduced gently to the terms Tutan, Badarian, Amratian and Gerzean. The author goes on to discuss in turn the immediately predynastic period, the archeic dynastic culture of Dynasties I and II, and the full flowering of Egyptian culture in Dynasties III to VI as expressed in the architecture, sculpture, painting and small objects of the period. The provision of illustrations, a great many of which are in colour, is positively lavish, and always relevant to the topic under discussion. It lies in the nature of a book of this kind that many points which agitate archeologists are passed over in silence, but there is one point which I cannot pass over without comment: why is it suggested on p. 45 that the glyphs accompanying two fugitives (rather than ‘spread-eagled corpses’) at the base of the verso of the Narmer palette could refer to places in Western Palestine and Transjordania? Seeing that the northern kingdom in the Delta had barely been subdued, it is highly unlikely that Narmer could have carried his arms through Palestine and across the Jordan. This question apart, the author has portrayed very successfully the growth of civilization in the Nile Valley up to the end of the Old Kingdom, and his book will be of great use both to the student and to the general reader.

R. O. FAULKNER


This book consists of two distinct and rather tenuously connected parts, and the explanation for this would appear to be that in preparing for publication his inaugural lecture, delivered before a non-specialist audience, Professor Armstrong has sought to add something of interest to specialists by appending a list of 80 sets of suspected reflexes in a variety of West African languages.

The lecture itself is mainly a brief outline of the history and present status of comparative studies in West African languages. The various pre-Greenbergian theories of Fula are discussed at some length, with emphasis on the confusion of linguistic and etymographical data (and speculation) with which the problem was long bedevilled. To a résumé of the membership of Greenberg’s Niger-Congo family Professor Armstrong adds a few glottochronological findings of his own; Yoruba and Igbo, for example, are reported to be between 4,000 and 6,000 years apart. The language which he postulates as ancestral to the Niger-Congo family was spoken, in his view, not more recently than 10,000 years ago.

For the linguist the interest of this book lies in the appendix, in which the series of suspected reflexes are set out in the manner of Westermann’s Die westlichen Sursprachen und ihre Beziehungen zum Bantu. These lists, compiled from a variety of named sources, are a useful supplement to Westermann’s work, especially for the Kwa languages, and will be of value to comparativists.

G. INNES


This collection of essays on the Yakö contains nothing previously unpublished except the author’s brief but useful introduction, and it is perhaps only the last paper, ‘The Context of Belief,’ the Frazer Lecture for 1958, which has been difficult to find. Nevertheless far from condemning the republication of these essays as something unnecessary, the grouping of them here under a single cover is, I think, a very worthwhile undertaking which will be particularly welcomed by those interested in West African ethnography. We have here all Professor Forde’s writings on the Yakö apart from his book, Marriage and the Family among the Yakö, and two recently published papers, the analysis of Yakö mortuary ritual published in Essays on the Ritual of Social Relations (edited by Max Gluckman,) and the chapter called ‘Unilinear Descent, Fact or Fiction’ in Studies in Kinship and Marriage (edited by Schapera). The various papers, in Yakö rather than in English, have been pruned so that any repetitive references to the main features of Yakö social structure have been omitted and we are left with the major portions of 11 essays organized as nine substantial chapters on Yakö life. These deal in detail with the economy, the kinship system, the internal political organization and the religious cults of Umor (Ugep), the largest of the Yakö villages. Since therefore each essay has as its central theme some aspect of life in this village, and since the essays taken together serve to illuminate each other, there is every reason for publishing them as a single volume.

It is impossible to read these essays and not mourn for the monographs which might have been written had Professor Forde had sufficient time and funds to have studied not only Umor but the other Yakö villages also. Each of these is so large, so complex in its social organization and so subtly different from the others that a full-scale comparative study would have been enormously time consuming and yet, one feels sure, would in the author’s hands not merely have yielded fascinating ethnography but have proved extremely stimulating from a theoretical viewpoint. As it is we must rest content with these essays on Umor alone, but these in themselves contain both much sustained theoretical argument and ethnography of a high order. The theoretical interest of these essays is well known and need not be stressed here. The excellence of the ethnography, however, can perhaps be fully appreciated only by someone who like myself has benefited so much from it whilst working among the neighbouring Mbenbe. Obviously since my interest in the Yakö lay primarily in comparing them with the Mbenbe there were certain
As far as it goes the book will be useful as introductory, background reading to those who are interested in current affairs in Africa. Nevertheless, and doubtless unavoidably, the treatment is slight and many questions are left unanswered, including the questions as to why the revolution occurred and what are its immediate results.

P. H. GULLIVER


This is a political history of the Chagga. It gives a chronological account, area by area; the misty period of myth is briefly covered, the nineteenth century is treated in detail, including the establishment of German rule, and the twentieth century down to 1962 is, disappointingly, treated rather cursorily. There is almost no account of changing institutions, ideas and values (not even of chieftship, which so dominates the author’s constructions), and little analysis which could be described as political sociology. For the author, ‘it is personalities which have counted politically, not institutions . . . And of all the personalities involved, the greatest have been the chiefs’ (p. 336). Whilst many may, with me, regret this limitation, nevertheless it clearly was the idea of finding out what happened and the fascination of those personalities which continually inspired Mrs. Stahl in her six years’ work on the book. As a result she has produced a most interesting and well written account, packed with rich detail, which considerably augments our knowledge of this important East African people.

Although Mrs. Stahl frankly disowns anthropological pretensions, yet her methods of studying what is principally oral tradition, and the results which she has obtained, will raise as much interest among anthropologists as among Africanist historians. Mrs. Stahl utilized the comparatively plentiful written records of early Europeans on Kilimanjaro (beginning in 1848), and later missionary and government records (though she did not apparently gain access to material in confidential administrative files wherein more recent political data are kept). On the whole she did not find this documentary material any more trustworthy than oral tradition. She relied principally, and quite essentially as it seemed to her, on a lengthy series of semi-public meetings with older Chagga people in the many different areas of the country. (There were probably over one hundred politically autonomous units at one time, with some marked cultural differences among them.) At these meetings, with her interpreter-assistant, and by question-and-answer and discussion methods, the data were gradually accumulated. She tends, I think, to underestimate the problems and difficulties of her methods, assuming too readily, and with not a little romanticism, that old men’s memories are ‘clear . . . down to the minutest detail about some bygone stratagem’ (p. 13). The problems of hindsight, and of oral tradition being influenced by events and values of a later and very different age, are not directly raised. But of course it is essential to use oral tradition if we are to get a record of pre-colonial and early colonial history in Africa; and, despite some reservations, this book is an excellent attempt to do that.

PHILIP GULLIVER


Le père Hugo Huber, l’un des meilleurs missionnaires ethnologues d’Afrique du Sud, a consacré une partie de sa vie (1953-1957) en pays krobo (Ghana). Parlant la langue qu’il put observer les moeurs et coutumes des divers clans et recueillir les informations reçues au cours de son long séjour.

Après avoir situé l’ethnie krobo, il décrit les principaux traits de l’économie traditionnelle (agriculture, pêche et chasse), commerce, techniques et industries (forge, poterie, vannerie), puis parenté et mariage, formes générales de contact social. Les rites d’initiation et de sépulture occupent 108 pages, c’est à dire la ‘place très importante faite par l’auteur à ces éléments de la vie d’une communauté,’
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les rites agraires et les fêtes annuelles sont abondamment décrites ainsi que les cérémonies dédiées aux Dieux de la guerre. Une étude des divinités de moindre importance, des pratiques médicales, de la magie et de ses parades complètent cette fondamentale étude.

Dans son introduction, le R. P. Huber marque l'entrée des Krobo dans l'histoire de la région d'Afrique. Quant au récit de leur migration, il nous dit (p. 17): ‘Le seul écrit quelque peu détaillé sur les traditions Krobo se trouve dans l'Histoire Adangbe (Adangbe) de N. A. A. Azu. Après avoir parlé de leur ancienne migration depuis Saméh, une ile située au Sud-Ouest de la rivière Ogoun joignant Ladah et Dahome à Lolovor et de la sous la conduite d'Akro-Musa ou Akro-Natebi aux montagnes Krobo qu'ils trouvèrent inhâbiles...’ En note l'auteur cite Reinford qui dans son Histoire de la Gold Coast et de l'Ashanti parle aussi de same comme siège ancien des Adangme. Mais il le localise ‘entre deux rivières Et et Kpa par laquelle passait le fleuve Niger.’ Il est certain qu'il faut faire la part des déformations et que la désinence me dans les langues des groupes Evéh signifie ‘chez.’ Sa peut donc être un nom propre, peut-être celui du premier occupant dont on perd la trace.

Mais j'ai noté dans mon Histoire du Dahomey (p. 15) qu'il existait encore voici quelques décennies une immense nappe d'eau s'étendant sans interruption entre Lagos et Grand Popo. Porto-Novo était en relation par une voie d'eau de 12 kms. avec Sémé sur la Côte qui était la grande rade de cette région, et se trouve à une dizaine de kilomètres de la frontière de Nigeria. Sémé était alors la ville principale et figure effectivement sur une île délimitée par la lagune. Sémé, au Sud-Ouest de la rivière Ogoun, peut donc correspondre au Samé de la tradition Krobo. Quant à la rivière Et il peut s'agir de Afon qui est une autre appellation de l'Ourémé... Je donne cette hypothèse à tout hasard espérant qu'elle suscitera réactions et mises au point.

Peut-être les docteurs Akindelé et Aiguesy pourraient ils retrouver dans certaines traditions de Porto-Novo la clef de l'origine des Krobo.

ROBERT CORNEVIN


This is an ethnographical description of a village on the outskirts of Khartoum which Dr. Barclay investigated for 11 months in 1959-60, though the authorities did not allow him to reside there. It is thus a study of a settlement in transition from agricultural to suburban conditions, and there is much of interest and much that is relevant to other Islamic countries in the account of the effect of the change upon family life, marriage customs, devotional practice, etc. The author concentrates on family relationships and on the practical rather than the dogmatic aspects of religion. His experience of Egypt enables him to note some interesting differences. He avoids pretentious generalizations, and his modest and careful approach to his subject gives his reader confidence in his judgment and accuracy. Perhaps because the book is a revision of a thesis there are tables of facts which are sometimes too few and too disparate for such presentation to be useful. Dr. Barclay is, however, always intelligible, even to those who are not anthropologists. His use of words like ‘de-emphasize’ is to be regretted; it is to be hoped that ‘insignias’ (p. 166) is among the rare misprints. In connexion with the statement that ‘it is the usual practice to have the celebration of a naa’alld... on the evening before the holiday’ (p. 184), it would have been worth explaining that the Muslim day, like the Jewish, begins at sunset. Our Monday night is Tuesday night in Islam.

C. F. BECKINGHAM


This useful little publication serves the primary function of carrying on from where our two earlier bibliographies in this field left off (D. H. Varley, African Native Music, London, 1936; and A. P. Merriam, An Annotated Bibliography of African and African-derived Music since 1936, Africa, Vol. XXXI, No. 4, 1951, pp. 319-29). Geographical coverage is that of sub-Saharan Africa. The intention has been to list ‘all known articles’ that have appeared since 1930 and a ‘representative selection’ of earlier ones, as well as books wholly or partly concerned with music. A total of 513 articles from 144 periodicals are cited, and 84 books. Annotation is concise but efficient and informative. Included at the end are an index, an index of tribal names, and an index and map of language areas (after Cole). Articles and books have been treated in separate sections and the Articles section has been subdivided into attempted regional groupings. This is unfortunate in two respects. The page number at which the Book section and each ‘regional grouping’ commences has not been indexed, so the reader must flick his way through to the region of his choice. The grouping is: General; West Africa; Central Africa; South-West Africa; South-Central Africa; South Africa; South-East Africa; and East Africa, in that order. But it is misleading to have placed some Northern Rhodesian items in the South-Central list while others, together with Southern Rhodesian and Nyasaland material, turn up under the South-East Africa grouping. Confusion could have been minimized if the intended territorial coverage of each ‘regional grouping’ had been indicated.

DAVID RYCOFT

AMERICA


Carl Sauer is a humanist as well as a geographer of many facets. This book is a re-publication of selected writings covering a wide variety of subjects, bearing dates between 1916 and 1962—the majority, of course, towards the latter end of that scale.

Through all of them runs the thread of the author's interest in people and in their reactions to natural environments of the New World, often in the role of explorers or newcomers, adapting themselves to conditions quite different from those in which their cultures originated.

Part I deals with the New England pioneers in the Mid-West and the settling of the prairies and the grasslands of Kentucky, with some most interesting reflections on the origin and ecology of grasslands, which the author regards as largely of human causation, maintained by the former Indian inhabitants through their indiscriminate use of fire.

Part II contains three articles on the North American South-West and Mexico and shows how the hand of man, beginning with the Spanish conquest and coming right up to present-day Mexico, has changed the face of the country. It was opened up under the influence of several powerful motives of very varying nature—conquest and loot, religious proselytism, agricultural and mineral exploitation among others.

Sauer is conscious all the while of the role of time, history and culture in determining human land-use and the distribution of settlements.

Part III takes us back to the origins of New World agriculture and a discussion of the character and environmental preferences of the various species of cultivated plants and the areas in which their adoption by man is most likely to have taken place. The destructive effects of modern agriculture on both plant and animal
indigenous species and on the natural environment are noted, but it is pointed out that these processes began at the very first settlement by man, and not only with recent intensive exploitation. The special relation of human settlement to tropical environments is covered in a final article.

The question of the earliest colonists of the Americas and the relation of primitive hunting culture to environment occupies Part IV. First published in 1944, this appears to be somewhat dated, in view of the advances of the last 20 years in all fields relating to early man. Many of the basic concepts, however, are still valid and make instructive reading. The origins of the use of fire are discussed and the advantages of sea coast habitats for early man. These are pet theses of the writer and are interestingly treated.

The final section, four papers on the scope and philosophy of geographical research and teaching, though valuable in their field and representative of yet another side of the writer’s interests, seem to be somewhat out of context in a book with this title. To me, at any rate, they formed an anticlimax to a collection much to the point and full of thought-provoking ideas.

Not only geographers, but anthropologists, archaeologists and historians would read the book with profit.

I. W. CORNWALL


The problem of ‘early man’ in the eastern United States has become dependent upon hypotheses, assumptions and conjectures derived largely by extension from the better-known situation farther west. On the Great Plains the sequence of projectile-point types associated with extinct mammals has been stratigraphically established and adequately dated by radiocarbon: the ‘Llano Complex’ (including Clovis Fluted points) began about 12-13,000 years ago but the smaller Folsom Fluted points became predominant between 11,000 and 10,000 B.P. The Llano people frequently hunted mammoths while the Folsom people hunted extint bison. Somewhat later (until about 7,000 B.P.) several types of parallel-flaked unfluted points became characteristic, and occasionally they are found associated with extinct bison. These point types and associated artifacts are sometimes lumped under the term ‘Plano Complex.’ With the evidence for the associated extinct fauna in mind, most archaeologists lump all these Late Wisconsin and post-glacial evidences for ‘early man’ on the Great Plains under the term ‘Paleo-Indian.’

In the Eastern Woodlands, points of similar and occasionally identical form are found. Even though none of these forms have been found associated with extinct animals (although several mastodon localities, some containing evidence for the presence of man, have been radiocarbon-dated between 10,000 and 6,000 B.P.), the term ‘Palaeo-Indian’ is generally used to refer to both fluted and parallel-flaked (Plano) point types. Points which are notched or stemmed and generally less well made are usually termed ‘Archaic.’ Many occupation sites yielding such points and lacking pottery have been excavated throughout the Eastern Woodlands, and although most of these sites, when dated, prove to be later than the presumed ‘Palaeo-Indian’ time of occupation as projected from dated sites on the Plains, several deeply stratified sites have yielded dates up to 10,000 B.P. for ‘Archaic’ assemblages. As this situation means that Plano points are apparently contemporary with some early Eastern Archaic points, the suggestion has recently been made that all Plano remains should be classed as ‘Archaic,’ thus retaining the idea that an Archaic stage postdates the Palaeo-Indian stage or period. Such taxonomic confusion is further heightened by the fact that fluted points have been found in apparent association with ‘Archaic’ points in several excavations. Such awkward situations are frequently explained by invoking the idea that later ‘Archaic’ people picked up the earlier ‘Palaeo-Indian’ points as curios.

Acceptance of the assumption that ‘Palaeo-Indian’ assemblages must be older and economically more specialized leads to the practice of dividing surface collections and even excavated assemblages into what is presumed to be an ‘Archaic generalized fishing-gathering-hunting’ component from an ‘earlier’ ‘Palaeo-Indian big-game-hunting’ component. Overt recognition of the fact that several distinct technological traditions are present in such presumably ‘mixed’ assemblages would certainly seem to be called for. If assemblages were identified primarily as to technological traditions, any tendency to ‘read in’ relative time, or even the more subtle connotation of a sequence of economic stages merely by extension from the known situation in a different environmental region, could be avoided. Eventually, datable excavations will establish the true sequence of economic adaptations for each region.

Logically, there is no reason whatever why we must assume that the pioneer occupants in all parts of North America were big-game-hunters; and in fact, there are many obvious ecological reasons which would discourage an economic emphasis on big-game-hunting in a heavily forested environment such as was always present in most areas south of the Ohio River.

In the Ohio Valley region both Rolinson and Pruner and Baby found a high proportion of fluted points in collections made by amateurs. In north-western Ohio Pruner and Baby also discovered a definite concentration of Plano points, apparently marking the easternmost extension of this complex from the northern Plains across the so-called prairie peninsula of northern Illinois, southern Michigan and northern Indiana. The non-fluted types farther south are generally not parallel-flaked and may be somewhat earlier than the types from the prairie peninsula. Fluted points, including Clovis Fluted, which itself includes several different varieties, are concentrated along the margins of the maximum extent of Wisconsin (Tazewell) ice and farther south in most regions of Ohio and Kentucky except in the hilly or mountainous sections.

Unfortunately, Pruner and Baby’s long discussion of the dating problem is inconclusive. As some fluted points are found north as well as south of the Tazewell moraine, one can conclude only that occupation of the formerly glaciated area was not possible until about 17,000 B.C., a fact which is meaningless in this context because Clovis points were made that early anywhere. Within the Lake Erie basin a series of ice-dammed and post-glacial lakes prevented occupation until after their recession. Unfortunately a detailed and initially convincing argument that the Plano points in this area were probably deposited soon after 7,500 B.C. when glacial Lake Warren receded was admittedly negated by a geochronological re-analysis by Hough which placed the Lake Warren recession several thousand years earlier beyond the known range of time for Plano points. Of value was the discovery of Cumberland Fluted points within the Warren lake basin, a distribution which verifies the general belief that these ‘fishtail’ fluted forms, with their centre of distribution farther south in the Tennessee Valley, are relatively later than Clovis points with their centre of distribution in the Ohio Valley. The valiant attempt of Newcomb to place the ‘Paleo-Indian’ occupations of Ohio will simply have to await verification by stratigraphical excavations which yield samples for radiocarbon analyses.

In their handsomely produced monograph Pruner and Baby include an important chapter on the technology of fluting and surface treatment in general in addition to the usual discussion of typology on the basis of form alone. They recognize the presence of points which preserve old blade-core facits, although they unfortunately call them fluted points. The term ‘fluted’ should be restricted to those points on which one or more long flakes were removed from the base after the point had been flaked into shape. Pruner and Baby conclude from the fact that there are sites which yield only fluted points or only Plano points as well as sites that yield both classes that there was a period of overlap between the presumably earlier fluted-point-makers and the later people who made parallel-flaked points. They do not deal with the problem of the possibility of overlap with the Archaic technology because as yet only Late Archaic sites have been found in Ohio. South of the Ohio, however, Rolinson had to deal with this problem because Palaeo-Indian points are frequently associated with Archaic points, both in surface collections and from excavated sites. She concludes that the presence of artifacts attributable to both ‘Palaeo-Indian’ and ‘Archaic’ in the same deposits probably indicates a transitional period from the former to the latter. Summaries of three unpublished
WPA excavations which yielded Palco-Indian points are included in Rolinson’s report. It is to be hoped that full reports on these important sites will be forthcoming.

ALAN LYLE BRYAN


The Andes are surely one of the most interesting areas in which to study structural change in society. It is also one of the least adequately exploited for this purpose. It is an area in which a whole series of organizational changes have been attempted by force. The two conquest states, Inca and Spanish, and the indigenous governments since the Independence have all attempted major reforms by edict and legislation. Some have been accepted, some resisted, and of course some shifts have taken place of a kind neither politically anticipated nor intended.

Andean local history is available through conventional documentary research, but even more important for the sociologist, many fragments of past organization are preserved in scattered rural communities peripheral to the major centres of change. One cannot, of course, assume that these villages have persisted unaltered through the centuries, but there is enough to give substantial help in reconstruction.

Vellard’s book is subtitled ‘Évocation des populations du haut-plateau bolivien.’ Presented as a study in human geography, it seeks to describe, in some 261 pages, the peoples of the Bolivian highland, their natural habitat, their history, their present way of life, and their future as the Bolivian National Plan for Rural Development intends that they shall be by 1972. The compass of the book is very large. In a rather unfocused fashion it presents bits and pieces of demographic, economic, ethnographic and historical material. One has no intimation until nearly the end of the book that Vellard is concerned with tracing the history of the Andean rural community from pre-Incaic times to the present in order to explain the failures and successes of recently attempted agrarian reform.

The book is perhaps as interesting as a record of certain Latin-American attitudes as it is interesting as a local survey. For one thing the Spanish Conquest is spoken of as liberating the Andean peoples from the crushing Incaic administrative machine (p. 124). For another, there are some rather local scholarly preoccupations. Vellard on several occasions takes the time and trouble to disagree with Pater Schmidt, but does not see fit to discuss any of the growing literature on peasant society. The form of the book is strictly that of a descriptive overview. When Vellard interprets, he does so on the basis of theoretical assumptions which are never explicitly stated. He tends to describe society and events primarily in terms of the attitudes and ‘character’ of men. In spite of the space Vellard gives to history, economics, demography and so on, when he interprets, this material fades into the background. Initially, for example, Vellard speaks of the ayllu as an ‘unité spirituelle’ (p. 117), only later of its sociological attributes. When he talks about some of the failures of agrarian reform in recent years, he puts the matter in terms of the temperamental proclivities of the peasantry.

L’homme des Andes est avant tout un paysan fortement attaché comme tel à sa terre, à ses droits, mais ayant conscience de certaines obligations d’entre aide bien définies vis-à-vis des membres de son ayllu. De ces deux sentiments proviennent toutes ses réactions (p. 222).

‘La réforme agraire est une magnifique expérience pour saisir le véritable caractère de l’homme et des institutions des Andes’ (p. 224).

In the early nineteen-fifties Bolivian legislators drew up a series of laws directing a division and redistribution of large landed estates among the peasantry. The legislators also approved a programme of large-scale agricultural enterprises anticipating that Andean habits of communal co-operation would lead to its success. Instead, the programme met with a great deal of resistance, Vellard sees the legislators’ optimism as a misreading of ‘la véritable mentalité indigène’ (p. 218). The situation is thus interpreted in the limited terms of some characteristic local attitudes toward property, rather than in the broader terms of the concomitants of a certain type of social structure.

SALLY FALK MOORE


This consists of three papers which appeared originally as reports of the University of California Archeological Survey, and which are now out of print. The first two may be of some interest to anthropologists other than specialists in California.

The first, by James Davis, is a detailed summary of what the literature tells us of trade and trade routes in aboriginal California. He lists the items traded by each tribe and the tribes between whom trade took place. He records the various types of trading—barter, purchase by shell money, gift-exchange—but there are very few details of the actual process of exchange. These are of course more difficult to obtain than details of things exchanged. The sheer volume of trade that went on is impressive and of course extended beyond California. Shells, highly prized for decoration, were obtained by New Mexican Indians from California, and extensive trading trips were made through hostile territory to obtain them. Davis does not cite this and think that it will be no record in the literature. He does cite the long journeys made along the ‘Walla Walla road’ by tribes from eastern Washington. Put together, these make up a vast trading network embracing the whole west coast and the south-west almost to Texas. The bearing of this on diffusion studies is obvious.

The second paper is Kroebier’s description of the nature of land-holding groups in aboriginal California. It was written in connexion with a land-claims case and not for anthropologists, but is in consequence concise and non-technical and a good summary of tribal and political organization in immediate pre-conquest times.

It is hard to see what justifies the publication of the third paper in this company. It is the story, put together from a large number of sources, of the Lone Woman of San Nicolas Island. She was an Indian of an unknown tribe speaking a language that no one could understand who had been abandoned on an island (no one knows why). The fact that she stayed there for 18 years and survived is remarkable and perhaps deserves to be recorded, but it is surely not of the order of the material in the first two papers.

J. R. FOX


He is a bold scholar who sets himself the task of making a coherent presentation of the religious systems and major traits of the American Indians of the New World. Inadequate and indifferent documentary sources with few good, first-hand studies in this subject, difficulties in making comparisons which cover a large and varied continent and its assortment of peoples—these are the major obstacles which immediately spring to mind.

In spite of these sceptical thoughts with which I approached Les religions des Indiens primitifs de l’Amérique, I found it not only an interesting book but a rewarding one and in the reading of it I was converted to the desirability of a full-scale consideration of American Indian religions.

Dr. Hultkrantz asserts that there are no traits which are peculiar to New World religions, although they appear to be stamped more strongly by asceticism and a pronounced individualism than those of the Old World. Chapter by chapter he makes a concise, comparative examination of some of the most prominent and widespread themes. His first subject, for example, is the time honoured problem of the correct translation of the concepts of orenda, tralakana and mantum of the North American Indians. These, he concludes,
signify the supernatural or supernatural power. In succeeding chapters he discusses beliefs relating to supreme gods, nature spirits, the soul and future life: he also examines the practice of shamanism, outstanding tribal ceremonies, totemism and modern, messianic or prophet movements.

In Chapter 10 his comparison of some of the essentials in the religious systems of hunters and gatherers, as opposed to cultivators, is particularly interesting: in it he sets the individual spirit visions of the hunter, with the shaman system and stress on guardian spirits, against collective rites of the cultivators, with priests and stress on the seasonal rhythm of growth. Close links between systems of belief and habitat and economy have frequently been demonstrated for particular societies. That there are wider patterns of inter-relationship which take in a good many cultures is a hypothesis which requires careful examination and testing against documentary and field studies.

This book is a notable addition to the Stockholm Series, for Dr. Hultkrantz has produced a synthesis which is an excellent assessment of the present state of our knowledge at a general level. In addition, it provides a basis for more specialist research which is an obvious need in many parts of the continent.

AUDREY J. BUTT


In the Introduction to the Bulletin of the International Committee on Urgent Anthropological and Ethnological Research Dr. Reichel-Dolmatoff writes that there exist today approximately 160,000 Indians within the limits of the Republic of Colombia, most of whom have retained their aboriginal languages and much of their native culture. ‘While few of these tribes have been studied in adequate detail by trained anthropologists, an investigation of certain groups is of special urgency as they are threatened by extinction or rapid cultural change.’

The Cubeco are just such a tribe. They live to the north of the Rio Vaupes—a tributary of the Negro in the region of Mitu and were first reported by the naturalist Wallace who travelled up the Vaupes in 1853. They are a Tukano-speaking tribe, the term Cubeco being the Europeanization of the Tukano jest word Kebew’, literally ‘the people who are not.’ They are tropical forest dwellers whose main staple is bitter manioc; living in their giant matas (houses) close to the streams and rivers, they also fish and— to a less extent— hunt, using poisoned arrows and blowpipes.

Professor Goldman’s work is based on a study which he made from September, 1939, to June, 1940, of the Cubeco of the Cuduiari who at that time had the advantage of being accessible, yet almost completely unaculturated by European, Colombian and Brazilian missionaries, settlers, traders and latex extractors. Goldman notes that several valuable studies have been made in the area by explorers and museum collectors which are understandably preoccupied with a survey in space more than a study in depth. His own objective is to present the Cubeco in terms of fundamental principles of social structure and of basic cultural patterns.

He deals first with the Cubeco community which he estimates at approximately 2,500. Their social structure is segmentary—the sib being the basic segment. These sibs are exogamous, patrilineal and patrilocally descent groups that claim to share a line of descent through males back to the founding ancestor. Phratry are confederations of sibs closely bound by needs of exogamy, by common residence along the same river, by tradition of common origin and graded order of rank of sibs and finally by a series of ceremonies such as the drinking parties.

There is no tribal or phratral overlordship or council. Tribe is in fact a vague concept and refers to common identity of language, descent and custom. The only authority is at sib level where the sib— the maloca or common residence—is overlooked by the headman, whose position, however, is more as mediator than governor.

Professor Goldman writes of their economic life, kinship and marriage, religion, a chapter on the development of the individual, the ancestral cult and finally the drinking and mourning ceremonies, which he describes in fascinating detail, ceremonies which take a predominant part in their lives.

In his final analysis Professor Goldman explains the principles and patterns of Cubeco life in their terms of extending relationships and maintaining autonomy from central state level downward to household and ceremonial friendship. He terms this linkage and autonomy, which—in Cubeco terms— he considers to be more inclusive than the commonly accepted concept of segmentation.

In his Introduction Professor Goldman reiterates Reichel-Dolmatoff’s plea, stating: ‘One of the astonishing and saddening overights of twentieth century anthropological research has been its still continuing neglect of the Lowland South American tropical forest and Savannah.’ This study would seem to be an outstanding attempt to redress some of the balance in what he describes as this ‘anthropological terra incognita.’

DONALD TAYLER

The Indians and Métis of Northern Saskatchewan. By Helen Buckle, J. E. M. Kew and John B. Hawley. Saskatoon (Centre for Community Studies), 1963. Pp. 114. Price $1

This is the result of three years of research in the northern part of the province, sponsored by the provincial department of northern affairs and conducted under contract by the Centre for Community Studies of Saskatchewan. The problem, which is general throughout Northern Canada, was to investigate the human and social factors underlying the chronic lack of development in the north. This is a territory of 100,000 square miles whose population until 30 years ago was widely dispersed and had remained at about 5,000. Its subsistence economy was based on an historical monopoly fur trade and transport system. At present the Indian and Métis populations together have risen to 11,000. Such government as exists in this unorganized territory is a highly concentrated one whose welfare and supervisory services have increased greatly. Income from fur for the whole region in 1960–1 was $550,000 compared with government welfare grants and subsidies of one million dollars. These latter account for from 20 to 70 per cent. of cash income. And in the face of a diminishing total dollar value in furs, the population increase during the past decade stands at about 5.3 per cent. per annum.

This is a competent analysis of the social and economic system, together with a detailed set of recommendations for administrative changes. In these, mature judgment is shown about the limited scope of co-operative and community development programmes. Instead the expressed need is for intensive technical aid and capital as well as participation by the indigenous people. The study is a commendable one, and it is hoped that the reference work still in manuscript will soon be made available.

A query remains with respect to the recommendations about increased governmental help. As pointed out, the present range of behaviours has essentially ‘... reinforced the caste-like features...’ of society (p. 10). The implementation of some recommendations faces the possibility of increasing this disparity of behaviour, through the paradox of importing additional semi- and non-skilled persons into an area of chronic under-employment.

R. W. DUNNING


This is a reprinting of Hale’s original edition of the Book of Rites in 1883. There is an introduction by Fenton which provides the background of Hale’s life and work and his relations with Powell, Morgan and other giants of late-nineteenth-century anthropology. These two in fact represent the twin poles of Hale’s achievement. On the one hand there is his contribution to American linguistics, and on the other his contribution to Iroquois studies. In pursuing his studies of Iroquois dialects he virtually invented glottochronology (which Fenton keeps referring to as glottal-chronology), while in trying to sort out the details of the Book of
Rites he made valuable observations on the League and its organization—particularly on the relations between clans and councils—which supplement Morgan’s account.

Hale’s account falls into two parts, his introduction of ten chapters, and his translation (together with the original) of the ‘Ancient Rites of the Condoling Council.’ These were discovered in a manuscript dating from the mid-eighteenth century written in the orthography invented by Anglican missionaries. Hale translated them with the help of informants, but as Fenton points out, he published the book before seeing the rites performed, and had at a later date to make some modifications in his description. The rites are those of the ‘Condoling’ or mourning council which met on the death of a chief of the federal council to appoint—or rather to confirm the appointment of—his successor, and to install him in office. They should be of interest not only to specialists but to any anthropologists who are interested in pursuing the ideas in Gluckman’s Ritual of Social Relations, for example.

J. R. FOX


In this book the authors analyse data relating to the well-known fact that the dichotomy between the urban and rural ways of life (or between the ‘urbanized social organization’ and the ‘ruralized social organization,’ as they put it) in the U.S.A is being replaced by a general uniformity of social values and behaviour patterns. They describe, in good detail, various typical features of urbanized and ruralized social organization in the U.S.A. and draw useful comparisons between them. The changeover from ruralized social organization to urbanized social organization in the rural areas of the U.S.A. is made particularly apparent in social institutions such as the family and in such aspects of society as education, local government, and health services. Much of the statistical and sociological information has been gathered and summarized by Taylor and Jones, and a substantial amount is presented in maps and diagrammatic form. On the descriptive level the two authors have produced a study that will certainly interest the social geographer and, possibly, the sociologist as well, but it contains almost nothing that would be relevant to the social anthropologist. It is, I think, an unnecessarily large volume and some of the information which it contains is hardly directly relevant to the authors’ thesis (e.g. much of chapter seven could have been omitted). The terms ‘social organization,’ ‘social structure,’ and ‘society,’ which are used generously in the text, are never clearly defined and when the authors rise above the descriptive level, as they do on p. vii, one is too often uncertain as to what exactly they mean. The maps and diagrams are clear and informative, and the work is well documented.

DAVID HICKS


Volume VII completes the publication of ‘The Frank C. Brown Collection of North Carolina Folklore’ series which began in 1952. Brown started collecting examples of North Carolina folklore as early as 1913 and before his death arranged for Dr. Newman White to systematize the considerable amount of material which he and other collectors had gathered together. White selected associate editors to help him in this task and on W. H. Hand devolved the task of editing the material on the popular beliefs and superstitions in the State. This volume is the result. It contains almost 8,600 items with short and indications of the sources, and although incomplete, as the editor himself admits, is an interesting and valuable addition to the literature on North American folklore. Included are three illustrations and an index to the whole series. One generalization with which, I imagine, not all social anthropologists would agree is found on p. viii when, in the ‘Afterword,’ P. F. Baum writes, ‘For “superstition” in all senses represents more closely and more widely than ballad singing the mind of the folk.’ This is not always necessarily the case.

DAVID HICKS


Numinous is one of Joseph Needham’s favourite words. It may be somewhat embarrassing to apply it to his recent work, but that there is any temptation to do so is in itself a statement about its quality and utility. It should be clear even after such a brief statement that this is not going to be one of your nasty English reviews.

For me to be assigned to criticize a product of Needham’s remarkable scholarship is either an enormous compliment, which is unlikely, or an admission that the editors failed to find anyone whose learning was equal to the job. It is a formidable task. Needham seems to have read almost everything, forgotten nothing, and he has a talent for the discursive footnote rarely encountered since the nineteenth century. He creates an effect upon me somewhat akin to the reaction displayed by Murdock in his review of Wittfogel’s Oriental Despotism. There are similarities at that, despite the political gulf; Needham’s political opinions are less obstructive, but where are his references to the products of continuing scholarship in Taiwan? At any rate, reading Needham is in some ways, I should say in the best way, a nineteenth-century experience. There is such enormous delight in knowledge and information. One learns an extraordinary amount about the concept of time in the Chinese tradition, but also, by way of merest examples, that the idea of dating B.C. is quite recent and may have been initiated by Bossuet in 1681, and that the use of filing cabinets for data is at least as old as Li Tao, a historian of the thirteenth century. Also reminiscent of the nineteenth century is Professor Needham’s strong reliance upon rationalism and what appears to be an implicit assumption that things written centuries ago can be easily assimilated to modern concepts. Further probing along lines such as this will raise questions about Needham’s translations and quickly transcends my ability. It is significant, however, that Needham’s work has been so well received by noted sinologists.

Actually, the major points of presentation may interest cultural anthropologists and comparative sociologists even more than they will the sinologists. Needham’s thesis is that eastern civilization, far from being timeless, has had a long and exceptionally deep perception of time, including philosophical modes of handling it and mechanical means of recording its passage. It is perhaps overly to ask if any really important intellectual figures have thought otherwise within recent times. In any case, the heart of Needham’s argument is directed at this but at a more important thesis. He concludes that attitudes towards time had nothing to do with the Chinese failure spontaneously to develop modern natural science.

In coming to this conclusion Needham omits, perhaps because of charity, one strain of argument, the linguistic. He ignores that philosopher who would attribute the failure to achieving science to the tenebrous, non-inferential character of Chinese. Less happily, there is no reference to non-elite, non-intellectual concepts of time in Chinese culture. I am not sure what might be said upon this score, but I suspect that the peasantry would be found to have dealt, like the literati, in two kinds of time: linear and cyclical. The peasant view might have been overwhelmingly cyclical, with consequent deep emphasis of the kind of tinkering and observation that helped produce so many technologist-inventors in the West. This assumes that Needham is correct in his apparently well documented conclusion that the literati view was predominantly linear. Not all commentators on Chinese historiography would agree. Étienne
Balazs in 1961, in a paper cited by Needham in other contexts, published the opinion that Chinese historians were forced to think of history in terms of sharply distinguished dynasties, which forced their ideas into 'cliques etanches.' This, combined with attachment to the principle of cyclicalism, produced, in Balazs's view, a penchant for the collection of isolated facts and discouraged research into relationships, making it enormously difficult for Chinese historians to take a view of the whole.

Of special interest to social scientists concerned with macro-cultural processes, Needham's work offers measured consideration of some of the ideological factors that have been asserted to have thwarted the independent development of a modern China. He finds them lacking as determining forces and, although counselling further investigation of other ideological factors, implies that both necessary and sufficient causes may be found in 'concrete geographical, social and economic conditions and structures.' Even as a strong partisan of Professor Needham, I recognize that many more volleys will be fired in this aged but ever fresh controversy.

MORTON H. FRIED

Early Mesopotamia and Iran. By M. E. L. Mallowan. London (Thames & Hudson), 1965. Pp. 142, 142 illus., map. Price £1.10s. (cloth), 1s. 6d. (paper).

This booklet is a reprint of Mr. Mallowan's essay of similar title published several years ago (1965) in The Dawn of Civilization (pp. 65-96), with the addition of a number of new illustrations, a preface on the nature of civilization in the papers by Stuart Piggott, and a brief introductory note by the author calling attention to several more recent archaeological discoveries and philological insights relevant to the theme. In words terse and succinct, accompanied by superb illustrations in colour and in black and white, the book presents a bird's eye view of some of the significant archaeological finds in Mesopotamia and Iran, ranging in date from c. 3500 to 2000 B.C.: architecture, pottery, and cult objects, sculpture, jewels, ornaments, and written documents.

More than half the book is taken up with the Uruk period, or rather with the latter half of it when urban civilization seems to have developed and matured; much of its illustrative material therefore comes from the all-important Mesopotamian excavations at Warka, though such other relevant sites as Uqair, Khafajah, and Brak with its now famous Eye Temple, are not neglected. The Early Dynastic period (c. 3000-2400 B.C.) treated somewhat more sketchily, is represented by finds from the Diyala region, from Kish, and from the rich discoveries of the 'Royal Cemetery' in Ur. Following a very brief comment on the art and architecture of the Agade and Ur III periods, there are a few references to the later centuries, with a concluding section on the late Iron Age, written in an informative example of archaeological methodology, by comparing the finds belonging to the last phases of the lifetime of Tepe Hissar, in far-off north central Iran, with those of Ur III in Mesopotamia; the conclusion which he thus arrived at by inference and surmise has now been confirmed and verified by Carbon-14 dating of organic material excavated in an Iranian site not far from Tepe Hissar.

The book is directed primarily to the layman and college student, but the specialist, too, will find it of no little value; it is quite interesting and revealing to observe how many, one of the most experienced of Near Eastern archaeologists, in the process of examining, scrutinizing and sifting the archaeological data from ancient Iraq and Iran—archaeologically speaking the twentieth century will no doubt turn out to be Iran's century—and thus arrive at a comparative assessment and evaluation of the two cultures.

S. N. KRAMER


This book comprises a series in which the author has published descriptions of customs, traditions, sayings and songs recorded by her in the Muslim village of Artas south of Bethlehem; the previous volumes were Marriage Customs in a Palestinian Village, 2 vols, 1931 and 1935 (reviewed in MAN, 1932, 181, and 1937, 23, respectively). Birth and Childhood among the Arabs, 1947 (MAN, 1948, 106), and Child Problems among the Arabs, 1950 (MAN, 1951, 221). It can only confirm Dr. Granqvist's high reputation for accurate and painstaking field work. Like its predecessors it is primarily a collection of anthropological material. There is very little discussion or speculative comment, but numerous comparable practices are cited from such authors as Jaussen, Musil, Westernarch, and Dickson, and especially from the Bible. Indeed, the whole series will be of great value, not only to anthropologists, but also to students of ancient Hebrew and of Arabic literature. The book is well produced and misprints are rare. There are mistakes in the English and the spelling of a few place names is very odd. It is disconcerting to read of Transjordan as 'the East Banque' (p. 10), and not everyone will recognize Rampur in 'Rambour,' still less Aligarh in 'Alaika' (p. 173). In a book of this kind one is particularly grateful for such a careful and comprehensive index, and it may seem ungracious to suggest that the statement that 'in this problem [i.e. the effect of smoking on health] East meets West' (p. 39) did not deserve to be indexed under Geographical Names—Outside Palestine—West. Some relevant matter has been quoted from the Mishkat al-nasabih in the version by A. N. Matthews (Calcutta, 1809, 10). This has now been superseded by Professor James Robson's translation (Lahore, 1960-65).

C. F. BECKINGHAM


This short book has as its theme the evidence of ethnographical evidence of a historical account. There is in southern Iraq a group of people called Ma'dan, distinguished from the rest of the population by buffalo breeding. These are, in part at least, the descendants of people called Zutt, whom historical accounts show to have been deported to Iraq from Sind in the eighth century A.D. Do any descendants of these Zutt still live in Sind? At present, there is a group of Jat buffalo-breeders in the Indus delta area. The authors describe many of their customs and consider that they show enough resemblances to those of the Ma'dan to confirm that they are the looked-for descendants. In the course of the account, we are given some interesting material on the tribal organization of these Jat, though social anthropologists would wish for more localization of some of this material. At the same time, the authors wish to show that these Jat alone are the descendants that they are looking for, rather than any other Jat of the West Pakistan and northern India. Unfortunately, their discussion of the differences among Jat is confused by their lack of orthographical distinction between Jat or Jat and Jat (or Jat). This is a distinction which I have only found to be made in Sind and to be fundamentally connected to the difference between farmer and herdsmen, but since the authors only discuss the differences between Jat and Jat, it is often not clear when they are referring to the Jat, although the fact that they quote sources which draw this distinction shows that they are aware that it exists and that it applies to the people whom they are studying. One hopes that a clarification will be provided in the ethnographic and historical account of the 'Jat nation' which the authors promise us.

ADRIAN C. MAYER


The coverage of this book is not as broad as its title suggests. The rituals which it describes are religious, the more important of rites de passage being merely listed and dealt with in a few pages. There is a good detailed analysis of village Hindu beliefs, how they underlie religious rituals, and how these 'ritual beliefs provide the social prescriptions and prohibitions in
respect of behaviour' when villagers are involved, as members of casts, in inter-caste relationships.

Dr. Mathur says that he prefers 'Karve's classification of castes, caste clusters and varna' and he criticizes A. C. Mayer's use of sub-caste as 'the largest division within the caste' because 'Mayer's own analysis suggests that sub-castes behave like castes.' Dr. Mathur uses the term caste only 'in the sense of a jati or endogamous group,' although the term jati is applied by Hindus to groups of varying span, and their frontiers are not as easily identified as those of named sub-castes.

The discussion of caste adds nothing to present knowledge and gives only a scant idea of what others have said. It is a pity that a convention of monograph-writing drives a writer to embark on a general theoretical discussion when he has nothing new to say. What is valuable in the book, and makes it worth reading, is the careful exposition of the belief system of Hindus in Malwa village.

HUGH GRAY


This book comes mainly as a revelation. It was not to be expected that the Department of Culture of the South Sumatran Provincial Government would pay increasing heed to a regional language and literature and publish a brief comparative study of the several South Sumatran syllabaries. Neither was it to be expected that after the Pacific war and a lapse of a quarter of a century the author would refine Ali Akbar who before the war was a helper to Dr. Vooheoe, the distinguished linguist for Sumatra. We are grateful that after Rooolvink and Köhler attention is paid to a dialect of one of the biggest islands of the world and that clear syllabaries have been drawn or revived from Marsden. The notation of materials for writing and the photographs are useful, but I cannot help hoping for more textual material and philological notes, less misprints and a more handy typographical layout in the following volumes, to which I look forward. I hope that the source, tapped on the last day of a 21-month visit, may flow abundantly in the near future.

C. HOOTYKAAS


The state of our knowledge of Japanese society is such that the appearance of a specialized volume on almost any topic is welcome. Here is one whose title alone will intrigue the student of Japan, for paternalism in general and 'oyabun-kobun patterns' in particular are much discussed but little studied. The reader, attracted by the title, will find rather less than he will have hoped for. There are ten chapters, a 'Summary of Concepts,' and four appendices, covering inter alia the 'parent-status' - 'child-status' (oyabun-kobun) pattern of leader-follower relationship in several different economic contexts, a summary and analysis of Kawashima's seminal study Familial Structure of Japanese Society, some comments by Sugi on the concept of ninja ('human feelings') which has much to offer the student of Japanese values, and a linguistic analysis by Goh of some slang terms used in oyabun-kobun groups. With the exception of Chapter 10 and the 'Summary of Concepts,' the book is, in fact, a collection of essays. As the authors remark in the Preface, 'We have decided to present the materials in essentially their original form, and not to rework the entire series of studies in order to give the appearance of conceptual and theoretical unity.' This seems to me an unfortunate decision, for while the book is, as the authors promise, a record of their thinking about the problem, it is very far from being the contribution that it might have been to our understanding of the 'pattern' in question. The substantive materials remain enormously useful, for they are unique in English-language sources on Japan. Had the authors pursued a less narcissistic course, and come to grips with the issues raised by their own changing analyses, interpretations, and emphases (rather than simply presenting them), they might have given us a far more tightly organized theoretical statement.

ROBERT J. SMITH


In the summer of 1962 the author, a programme organizer at the B.B.C., together with two graduates in anthropology and a cameraman, from Cambridge, travelled in some of the less easily accessible parts of Malaya and southern Thailand. Their aim was to record, on film and tape, survivals of more traditional culture on the east coast of the Peninsula.

Mr. Wavell's particular and laudable purpose in writing his book is 'to sound the call to dig'; 'If the robbers can do it, so should we.' Few areas in the world, indeed, are more likely to reward archaeological investigation, and it is much to be hoped that this lightly written and enthusiastic account may further prompt intensive researches in this intriguing part of South-East Asia.

The author would not claim that there is very much ethnographical substance in his work, but there are numerous details which may well prove apt to future anthropological concerns, and there is a special interest in the reports that the Negritos in southern Thailand will readily draw blood from their shins and that their god (Mr. Wavell could not discover whether he is connected with thunder) is called Ka-ay.

RODNEY NEEDHAM


This little book is the Smuts Memorial Lecture for 1964 delivered in Cambridge last May and the author is Dr. C. D. Deshmukh, the distinguished Indian administrator, who has been successively Finance Minister to the Government of India, Chairman of the University Grants Commission and Vice-Chancellor of the University of Delhi.

It is as a man of affairs that Dr. Deshmukh looks at the Commonwealth and he is old enough to remember the earlier attitudes of the Indian Congress and to have taken part in discussions about them. A fact which he brings out is that these changing attitudes have to be related to the changing conceptions of the Commonwealth itself. Before the First World War Gokhale and the Moderates who controlled the Congress would have been quite content with Dominion status as then understood—internal autonomy but external subordination, a daughter nation in Kipling's phrase. After that war, which is the real dividing line, psychological as well as constitutional and political, between independent India and colonial India, this was no longer acceptable. Nor was it to the then Dominions themselves. But the Balfour formula expressing equality did not emerge until 1926 and its legal implementation until 1931. I think that it was this prolonged time lag which was largely responsible for the failure in 1929-30 of the Congress to appreciate the new concept of Dominion status as equality. They had grown up in the belief that Dominion status meant ultimate subordination and subordination to them it remained until the Second World War. They had been content with subordination as a Dominion before 1941; afterwards they were so no longer.

The decision to remain within the Commonwealth in 1947, and still more, to retain membership after becoming a republic in 1950, was evidence that the point had been taken. But what profit was the more equal but also more amorphous Commonwealth to India? An important attraction of the pre-1914 concept was both partnership with one of the Great Powers and protection from others. Now Britain was no longer great nor could she afford effective protection. Dr. Deshmukh finds the advantages of membership in the goodwill that it engenders in the opportunity that it provides for discussion with the widening circle of member states, and influencing them, and in economic co-operation. It is this last which he stresses most. What would happen in the event of a grave crisis Dr. Deshmukh does not speculate. But it is at least arguable that these largely intangible bonds might, like the thread of a
Indonesische Textilkunst. By Norbert Mylius. Vienna (Verlag Noting der wissenschaftlichen Verbreitung Oesterreichs), 1964. Pp. 93, 36 plater. Price 1.50 (schilling). The purpose of this small publication is to give a survey of the textile handicrafts of Indonesia. It intends to be scientific, but it is not even a popular representation of a fascinating subject. In fact, for the layman this work is relatively dangerous because of the numerous errors which it contains; and for the specialist it scarcely provides anything new or useful. I am tempted, therefore, not to take it at all seriously.

The author quotes many experts on textile research, but indulges in so much generalization and misdirected application, that one has to ask oneself how many of the almost 180 books referred to in the bibliography he has actually read. The bibliography takes up some 23 pages of the total 86, and many of the works are quite superfluous to the subject matter.

The text is not only unintelligible in many respects—full of repetitions and commonplaces—but is also written in very bad German, and slovenly printed. Some of the 36 plates are superfluous, nearly all are insufficiently captioned and confuse the text. To mention but one example: Plate 22 depicts a Bhum woman of India—a fact which is not mentioned—and one is consequently misled into assuming that the woman is Indonesian. Apart from this the Plate is taken from the Ciba 'Rundschau No. 111 (1953)' without mentioning the source, or even that it originally appeared in U. Mohr's 'Indien' (Hamburg, 1952).

In the light of such circumstances I feel it futile to attempt a review in any detail of the various parts of the book dealing with dresses, the ceremonial uses of textiles, the textile techniques and the materials employed, Plangi, Batik, and Ikat.

ALFRED BÜHLER


This welcome volume is a slightly abridged English version of Samekulturen, which was published in 1958 by the Tromso Museum and distributed by Oslo University Press. Its three main sections deal with the historical background, ecology and livelihood of the four major categories of Lappish community; with non-material aspects of traditional Lappish culture; and with the contact history of Lapps and metropolitan populations in the national states in which they dwell. Since I have already described the original work at some length and stressed its excellence (Amer. Anthrop., Vol. LXIII, No. 6, 1961, pp. 1366-8), I will only raise one further point about its content here. This concerns the present-day forest Lapps, a group particularly mysterious to the English reader since unlike the other major Lappish groups they have not been described in recent sociological publications. How many of them are there? Where precisely do they dwell? It appears that in dialect and dress they cannot be marked off from the older-established of the mountain Lapp groups in their regions. Dr. Vorren makes the ecological and economic distinction between mountain and forest Lapps reasonably clear, but what, if any, are the social boundaries between these groups?

Turning to a consideration of the English translation, one must say that this too is on the whole workmanlike and readable there are at least a dozen passages in which seriously misleading errors have crept in. These result from lack of sophistication in anthropological idiom or lack of familiarity with Lapp ethnology on the part of the translator, together with a general tendency to compress and combine what were separate sentences in the original. Thus a failure to appreciate the author's conceptual distinction between reindrift ('reindeer-herding')—an activity which can either be pursued alone or combined in other elements in a mixed economy—and reinnomadism ('reindeer nomadism')—an economy or way of life based entirely, or almost so, on reindeer-herding) makes nonsense of important passages on pp. 57 and 76; a similar lack of awareness of the technical usage of 'extensive' in discussion of agriculture and pastoralism reduces sense on p. 151; whilst the account of dowry and other marriage portions on p. 136 (a topic of special interest to the social anthropologist) is distorted and made ambiguous by unwarranted conjecture.

Translating is a thankless task and one would not wish to criticize the translator, who cannot reasonably be expected to be a trained anthropologist and expert in the Lappish field. One must complain, however, that an editor with appropriate academic qualifications was not employed, or, if one was, that he was negligent of his responsibilities. The useful 'Reading List of Publications in English about the Lapps' is also marred by inefficient editing and proof-reading.

Finally, one must object mildly to the editorial combination and compression of paragraphs as well as sentences, which frequently do violence to sense as well as style, and regret the omission of the very convenient marginal indexing of the original and the restriction in the number of photographic plates to only one quarter of those included there. In all, it is a little humiliating that O.U.P., with all its resources, should produce an edition so very much less attractive in its format than the Norwegian original, which was published by a Provincial Museum and printed in a small, remote provincial city.

Lapp Life and Customs is nevertheless a most useful volume. The small number of mistranslations will not detract from its value for the 'general reader' to whom it can be confidently recommended as much the best introduction in English to its subject. Its blemishes will however affect its usefulness to students and teachers of anthropology, which is a pity, since for them also this seems likely to be a standard work, indeed the standard introductory text, for several years to come.

RALPH BULMER


Professor Kriss's collection of religious folklore in the Bavarian National Museum, in Munich, is unique. The keeper of the collection has previously published a book on the votive images, but his latest book shows much better the astonishing many-sidedness of the objects. Dr. Kriss-Rettenbeck has a profound knowledge of the subject (popular religious images and signs) and the references seem complete. Especially gratifying are his observations on such familiar subjects as the living room (p. 14), the Magi (pp. 42-8), rosaries (pp. 276f), candles (p. 30), holy water (p. 31), keys (p. 46) and devotional pictures. Very useful are the statements about problems which have not yet been solved; e.g. why St. Leonard, who was throughout the Middle Ages the patron saint of prisoners and the persecuted, became in the seventeenth century the patron saint of domestic cattle (p. 20)?

The Breverl (a magico-religious protective letter) and the Haussgeset (illus. 135) might have been described in greater detail. Line 28 on p. 21 should read: 'das fassische Thomaskreuz (illus. 133).'

The remarks about the kritiklose Hitababene and naive Verallgemei-

The author's style is at the beginning rather cramped and involved; the second part of the book is written in a more fluent manner. Some misprints are rather tiresome, but the illustrations are magnificent.

ELLEN ETTLINGER

This important work gives a valuable conception of 173 Roman villas in the Pannonian provinces, as variable as in any of the western provinces. Regrettably, the main map illustrates physical features but no villa sites, although the sites themselves are clearly described and admirably illustrated, however divorced from geography or geology. The British reader will find stimulating parallels. Often, as with us, only a bathhouse has appeared. Small enclosed farms emerge, Regelsbrunn (p. 258) matching Ditcheley; also basilica houses, as at Smarje-Grobece (p. 346) or Königshof-Odokloster (p. 153), where the earthwork is no more a fortification than is that at Ely (Glamorgan). Fortified villas, such as Keszthely-Fenékpszta (p. 61), are very rare; indeed Sünig (p. 112) is probably a double Ekvastahusvilla, like Hambledon. The British type of large courtyard villa is matched only in its peristyli form, as at Southwick, which goes with Eisenstadt (p. 138) or Nemésvámos-Bálacpuszta (p. 75). The last site has yielded notable stuccoes and wall plasters, as has also Csiglészegy (p. 217), whose plan resembles that of the basilican house of Brading. Iron tools abound. Inscriptions, commoner than in Britain, include a veteran’s dedication (p. 195) and a client’s laudation (p. 272) of the fourth-century Gallic governor, Valerius Dalmatius.

I. A. RICHMOND


It is rather surprising that the Tarantella has never before been thoroughly examined, in view of the widespread interest which it is witnessed by numerous fascinating descriptions by illustrious men of letters. The problems were apparently too complicated and could be solved only by a strictly historical method. Signor Penna has at last put an end to some 300 years of confusion by concluding that the term Tarantella actually applies to two dances, whose origins are as different as their characteristics.

There is, first, the magico-religious solo dance from la Puglia, which was believed to cure, by means of profuse perspiration, the delirium and convulsions caused by the venomous bite of the tarantula during harvest time. This therapeutic dance survived in la Puglia as a cure for strange behaviour of neurotic women, it is also confined to the summer time. This 'Carnevaleto delle donne' was recently scientific investigated and the findings were published in 1961. There is, secondly, the Tarantella of Naples and its surroundings. It is a courtship dance which is performed by one or more couples. Its rhythms, melodies, gestures and accompanying songs are quite distinct: they are cheerful and much faster. The author suggests that the Neapolitan Tarantella is a fifteenth-century fusion between the Spanish Fandango and the Moresque 'ballo di giesanita.' The musical instruments used for the accompaniment of the two South Italian dances are very well described. The author might have been a little more explicit with regard to the vivid illustrations.

ELLEN ETTlinger


Critical analysis of a descriptive work is difficult. It requires a familiarity with the material at least equal to that of the reporter, as well as a serious application of theoretical concepts to the description under review. Sheila Patterson has recorded in this book her impressions of the first stages of settlement in Brixton of over 10,000 immigrants, the vast majority of them West Indian by origin. She calls this first stage 'accommodation' rather than assimilation or integration on the grounds that so far the natives and the immigrants have gone no further than putting up with each other. The book contains an examination of this 'accommodation' in the spheres of employment, housing and social and cultural life.

Including the officials, managers and others made necessary by this emphasis, the author interviewed about 290 natives and 150 immigrants. It might be accepted that the fluid social situation made sampling difficult (although at least residential sampling would seem to have been possible). Nonetheless there does seem to be a bias in the manner of selecting immigrants for interview. Mrs. Patterson says: 'Most of the white people interviewed were those found at various points of contact, official or unofficial, between the migrants and the bulk of the local population ... the coloured people interviewed were those who could be found at any of these points of contact with white people, and their relations, friends and fellow tenants' (p. 27). In a study devoted specifically to the extent and nature of 'accommodation' this would seem to be an error. How is the significance of these findings to be tested?

It is not only in this respect that the work suffers from the bias of social anthropology. In words, Mrs. Patterson accepts that the forces at work in the Brixton situation are operative at a national and international level. It is therefore difficult to understand how anything more than reportage can be claimed for a study along the lines of an anthropological field monograph. There is every probability that the 'absorption' or 'accommodation' visible to the naked eye in Brixton will be washed away in a political flood none of whose origins can be detected by studying the 'Brixton' situation, least of all at the 'points of contact' between natives and immigrants. The confusion arising from Mrs. Patterson's methodological bias seems to me to be further confounded by a general liberal bias. For example, on p. 401 she says: 'The trend in the world as a whole is towards an ideal situation in which "racial" affiliations, real or alleged, should no longer be a criterion for the allocation of rights, privileges and responsibilities between groups and individuals; in which the word "race" should in fact cease to have any but a biological significance.' In this context, she outlines the 'social action' which she thinks necessary to take steps in line with this 'trend.' One need hardly say that so many sociological assumptions are made here that comment is difficult in a short review: e.g. the assumption that the 'allocation' of privileges and rights flows from ideals or lack of ideals about 'race.' Surely the question is: what are the social bases of the dominance of racialism, and what factors exist which tend towards changing these objective bases?

In Brixton, one cannot begin to answer these questions, since the social roots of the problem are historical and international in their nature. The danger therefore is that one ends up with a long account of one’s impressions of people in Brixton—followed by a statement of ideals and modest prescriptions for their fulfilment. This is what Mrs. Patterson has produced.

C. SLAUGHTER

Atlas der schweizerischen Volkskunde; Part II, Issue vi.


In this first installment after the untimely and lamented death of Professor Richard Weiss, the new editors, W. Escher and E. Liebl, outline the steps which have been taken to assure a successful completion of the Swiss Folklore Atlas. The choice of the deceased’s close collaborators as his successors fills us with great confidence.

The contents of this issue are unusually varied. The first chapter and maps 233 and 234 record the changing fashions in grave monuments and grave plants. The dislike of yellow flowers in Swiss churchyards is perhaps unique; but the preference for or rejection of iron decorations, e.g., agrees with the tendencies in other countries. The fairly recent custom of placing Christmas trees on graves comes probably from Germany. In the nineteen-thirties I saw the lighted candles of tiny Christmas trees on Bavarian graves. It is the most lovable of all modern Christmas Eve customs.

The following chapters and maps register the various forms of bogymen, April fools, blessings which are pronounced when somebody is sneezing, the numerous attempts to stop hiccups and the different interpretations of the tingling of ears.

ELLEN ETTLINGER
BRENDA ZARA SELIGMAN, 1882–1965: A MEMOIR*

By Professor Meyer Fortes, President of the Royal Anthropological Institute

216 Brenda Zara Seligman died peacefully in her eighty-third year on 2 January, 1965. Of few people can it be so justly said that she fulfilled herself completely in her life. With an academic training that would be considered negligible by modern standards, she succeeded in making for herself a permanent place in the history of modern anthropology, by the side of her husband. In keeping up, during her 25 years of widowhood, the splendid collections of Chinese and Far Eastern art begun by her husband and herself, she satisfied both her perennial joy in things of beauty and her passion for knowledge. Without any pretensions to being a scholar, she was well enough versed in the technicalities of connoisseurship in the plastic and graphic arts of China and the Far East, and in the history of the civilization in which they flourished, to feel at home among the experts. She had what to the amateur seemed an encyclopedic knowledge of flowering plants. She loved gardens. Her own were laid out and managed with rare skill. Her iris garden at Toot Baldon, Oxfordshire (where the Seligmans lived in the thirties), was internationally known among the cognoscenti. One of the great treats of a midsummer visit to Toot Baldon was a conducted tour round the iris garden in its full blaze of glory. She was an assiduous reader, and her taste in general literature was catholic. A novel or travel book recommended by her was always worth the effort of finding. As to serious literature, the books and journals relating to her studies and hobbies, in anthropology or matters Chinese or iris culture, these were tackled with methodical care. She early learnt from her husband the useful habit of making critical abstracts of everything she read that had a bearing on her serious studies. To keep abreast of current literature, she regularly reviewed books for MAN and other journals, taking particular pains to be accurate and fair.

Until the death of Seligman in 1940, a great proportion of her time was given to their collaborative work and to the sidelines of research arising from it which led on to her own scientific publications. The war of 1939–1945 and its aftermath, her removal to London from Oxfordshire, and the rapid onset of the arthritis which progressively crippled her, made a gap in these activities but only temporarily. The publication in 1962 of Ainu Creed and Cult, the book which she made out of N. G. Munro’s unique but heterogeneous field records, was proof of this.

Much as these interests absorbed her, they did not, before her illness reduced her ability to get about, exhaust the boundless energy with which Brenda was gifted. She had been from girlhood a keen rider and before the war enjoyed going out with the local hunt. The Seligmans travelled a great deal, apart from their field-work expeditions, visiting museums and oriental collections and spending time with Seligman’s professional colleagues whenever possible. It was characteristic of Mrs. Seligman that in her late seventies, nothing daunted by the pain and lameness of her arthritis, she flew out to Switzerland to stay with M. J.-P. Dubosc, the distinguished authority on Chinese art, and talk about Chinese painting. A more exacting journey made at this time was to Stockholm, at the invitation of the King of Sweden, with whom she had been in touch over their common interests as collectors. This was to attend the opening of an exhibition which included loan items from the Seligman collection. The diaries which she diligently kept and the letters which she dashed off to friends and relatives, described the places, objects and personalities met with on these travels with a gusto which showed how she revelled in them.

Yet to anyone who had the good fortune of some degree of closeness to Brenda Seligman, what must live most in memory is her genius for friendship, her courage and her humanity. The secret philanthropy, which was a family tradition both on her side and on her husband's, brought hope to many. Unlike some aging people, she became more tolerant and more and more benevolent with the passing years, especially towards the young. Her zest for new ideas never flagged.

From the beginnings of their association with Haddon and the handful of devotees who followed him, the Seligmans had world-wide contacts with scientific colleagues, people working in colonial territories, scholars, collectors, travellers, beginners in field research as well as the established and eminent. Between the two world wars, the world community of anthropologists and ethnologists was still small enough for those with common interests to keep in regular touch by correspondence and occasional visits. The Seligmans were prolific writers of letters. But it was through the hospitality which they dispensed in their Oxfordshire home that their contacts were cemented; and many of their closest friendships grew out of this. These included anthropologists of all ages, as well as famous orientalists, collectors, psycho-analysts, physicians, artists, writers and men of affairs.

In the prime of her life and intellectual productivity, Brenda could be a demanding friend. She demanded attention—to some theoretical idea which she was working on, to some problem of a personal nature which she was grappling with, to a cause in which she was involved. On the other side, she was generous without stint, in her loyalty and affection no less than in material ways. In 35 years of friendship with her, I never heard her speak ill of anyone, though she was quick to condemn things she hated in people and institutions, be it humbug or cruelty or plain dishonesty. She understood physical and mental pain and suffering, having had a fair share of both, and knew how to help those in need. She was not one to take no for an answer, and spared herself no trouble in order to

* With a portrait
achieve something she had set her mind upon. She put months of work into preparing herself for the field expeditions in which she accompanied her husband, and took immense pains over everything she wrote. It was not only through her vision and her generosity in donating the proceeds of the sale of the Benin mask to the Fund, but as much on account of her pertinacity that the C. G. Seligman Endowment Fund was eventually established for the benefit of the Royal Anthropological Institute.

II

Brenda Zara Seligman was born in London, the youngest of the 14 children of Myer and Sarah Salaman. Her family was well to do, with interests in the City to which her father devoted his time. She came, therefore, from the section of middle-class Anglo-Jewry which, at the end of the 19th century, contributed many notable personalities to English public and intellectual life. Taught privately at home, to begin with, she was later sent to Roedean, but, as she sometimes modestly said, never applied herself seriously enough to her studies to qualify for admission to Cambridge as had been hoped. Instead, she became a student in the pre-medical biology courses offered at Bedford College, London. She gave this up however, in 1905, to marry C. G. Seligman, a friend and medical colleague of her brother Redcliffe—who was later to become distinguished for his scientific and historical researches on the potato. Thus began the lifelong partnership of the Seligmans to which Haddon paid such affectionate tribute in the 'Appreciation' which he contributed to the Seligman festschrift in 1934.

At the time of their marriage, Seligman was still professionally employed as a research pathologist. But on the side, he busied himself with writing up the results of the Cooke-Daniels expedition of 1904 to New Guinea; and it was through helping him with this task (mainly, as she was wont to say in later years, with the blue pencil!) that Brenda was first stimulated to take an interest in ethnology. Then came the expedition to the Veddah of Ceylon (1907). Reluctantly at first (for it meant leaving their small daughter at home with relatives) but presently with characteristic enthusiasm, Brenda yielded to her husband's wish for her to accompany him. It was this first experience of field work that captured her imagination and drew her into the dedicated circle of post-Torres Straits ethnologists. Twenty-five years later, in a lecture which she gave by invitation at Bedford College, she told how she had served this apprenticeship as 'Seligman's general assistant and bottle-washer' without any training. But, as she added, 'those were the days before the “now fashionable schools of anthropology had arisen”' (I quote from her notes) and the only guides to whom the field ethnologist could look were Haddon, Rivers and Tylor.

Yet untrained though she was, the notebooks and diaries of the Veddah expedition foreshadow the direction in which her interests were to develop. While Seligman was busy with his algometer testing for the pain threshold of the Veddah, she was noting the behaviour of a two-year-old child. Though she did not, till much later, grasp their full import, she recorded vividly the affinal and sibling avoidance behaviour which she observed. Thus from the outset Brenda's bent and interests supplemented those of her husband. Yet so close was their collaboration that it would take a microscopic investigation to tell apart their respective contributions to The Veddas, the book in which they reported the results of the expedition (1911). Subsequently, a more definite division of labour emerged in their joint work. The physical anthropology, archaeology and history of the peoples whom they studied were Seligman's speciality. Brenda took kinship and social organization for her province. Where their interests coincided and their collaboration was closest was in the study of magic and religion, and more particularly in the psychological and psycho-analytical enquiries which increasingly fascinated them after the First World War.

However, let me return to the chronological record. The report on the Veddas was hardly completed when the invitation came from the Sudan government for Seligman to undertake the survey of the southern Sudan which laid the foundations for scientific field work in what was, till then, ethnologically terra incognita. Brenda's part in the three Sudan expeditions of 1909–10, 1911–12 and 1921–22, was second only in name to her husband's. Side by side with him she prepared herself for them by reading, by discussion and correspondence with people who knew the Sudan, and by assiduously applying herself to the learning of Arabic. In the Sudan, they travelled together everywhere, by river, by camel, on foot or by bicycle. Brenda proved every whit as capable as he of enduring the arduous of climate, terrain and struggles with informants in this barely pacified territory. Strenuous as it was—a dawn-to-dusk schedule of work with informants, tramping around villages, attending ceremonies, moving from place to place, followed by evenings of writing up notes, was the usual routine—Brenda increasingly relished field work as her skill improved. With her lively and inquisitive disposition, she found the trimmings, such as dinners with officials and encounters en route with the exotic and sometimes shady personalities, stimulating and entertaining. The birds, the animals, the vegetation, the landscapes and the people—she responded to all, seeing new aspects in each successive expedition, supplementing Seligman's scientific objectivity with her more personal and aesthetic judgement. Characteristically, perhaps, the experience which she was most exhilarated by in the three expeditions was the three weeks' trip to the Kababish Arabs, subsequently (1918) recorded in the famous Kababish monograph in the Harvard African Studies series.

The Sudan expeditions were but one part of the Seligman's many-sided activities in the 15 years that followed the Veddas study. Seligman had by then committed himself to a career in anthropology and drew Brenda more deeply into the subject. There were winter visits to Italy, Greece and Egypt, the last primarily in pursuit of Seligman's enquiries into the prehistorical and historical background to what he later described as the Hamitic problem. There were visits to the major ethnographical and oriental collections on the Continent. There were sorties in
search of pieces for the collection of Chinese ceramics and bronzes which was beginning to take shape. Most important of all for Brenda was the consolidation, during these years, of her anthropological skills and knowledge. This was the period of vigorous theoretical controversy, it was at this time that, mainly through Rivers’s influence, Brenda began to pay special attention to kinship studies. Later, prompted partly by C. S. Myers and Rivers, and persuaded by his own experience with shell-shock patients in the 1914–18 war, Seligman began to make a serious

BRENDA ZARA SELIGMAN
A photograph taken not long before her death

centred partly on the Royal Anthropological Institute and partly on Section H of the British Association. Among the polemics of the diffusionists (Elliot-Smith), historicists (Rivers) and functionalists (Malinowski and Radcliffe-Brown) the Seligmans steered an eclectic course. However, study of psycho-analytical ideas and theories and, as usual, drew Brenda along with him.

These sombre years ended tragically for the Seligmans. Brenda nearly succumbed to a serious illness in 1918 and anxiety and exhaustion played havoc with Seligman’s
health. But the most devastating blow at this time, one which left a permanent scar, was the death in 1919 of their 13-year-old daughter. It took fortitude of spirit, as well as dedication to science, to mount the 1921 expedition to the Sudan.

Came the middle twenties, functionalism, Malinowski’s ascendency and the first batch of the younger anthropologists destined for mid-century leadership in social anthropology. Though the London School of Economics was the centre of this movement, Seligman's own interests lay elsewhere. Brenda, on the other hand, found new stimulus in discussions with Malinowski and the younger research workers, though she could not accept the functionalist position in toto.

The decade culminated memorably in a six-month tour (1929–30) of China and Japan, the history, arts and civilizations of which had a profound appeal for the Seligmans. Guided by professional colleagues and fellow connoisseurs, they missed little of note from prehistoric sites to the Peking palaces. Eight years later, invitations for Seligman to lecture at American and Canadian universities took them on a comparable tour of that continent, and it is of interest that Seligman’s main lecture topic concerned the history of early contacts between China and Europe.

Seligman died in 1940, his death hastened, I fully believe, by mounting despair over the world’s attitude to the Nazi atrocities and the probable outcome of the war. Brenda threw herself into charitable activities arising out of the war. She turned her house in Oxfordshire into a reception centre for young children evacuated from London and herself supervised the care that they received. For the time being anthropology was in abeyance, though many of us will remember with gratitude her encouragement and exhortations to keep the Institute and its publications going and plan for the post-war return to normality. When she moved to London after the war, the Institute became one of her main concerns. It was then that she conceived the plan of establishing the Endowment Fund which her own munificent gift eventually made possible. She served for many years on the council of the Institute, both as an ordinary member and as a Vice-President, rarely missed a Council meeting and regularly attended papers read at evening meetings. Next to the unique distinction of the award to her in 1963 of the Institute's first Patron's Medal in recognition of her services to anthropology, she particularly prized a gesture of homage from a different quarter. This was her election in 1959, in succession to Radcliffe-Brown, as President of the post-war Association of Social Anthropologists. The Festschrift (Studies in Kinship and Marriage) edited by Professors Schapera and Evans-Pritchard, both among her husband’s pupils, which was presented to her on her eightieth birthday, gave her great joy.

III

A word must be said, in concluding this memoir, about Brenda’s scientific publications. She looked on her own anthropological research as ancillary to her husband’s. Amongst her publications, she attached most importance to the contributions which she made to their two major works of collaboration, the books on the Vedda in 1911, and on the pagan tribes of the Nilotic Sudan in 1932, especially the latter. The tributes which Seligman paid her in the prefaces show how highly he prized her contributions, both to the field work and to the publication of the results. The sections dealing with kinship and social organization were almost entirely her work. In these and in other joint publications which arose from the Sudan expeditions, the principal aim of the Seligmans was, as Brenda often emphasized, to establish the facts as accurately as possible. We should remember, in assessing the significance of these publications from our position today, how little was then known about the life and customs of people whose names are household words in modern anthropology.

To fulfil their aims, the Seligmans supplemented their own field data with information garnered from all possible sources. They searched the writings of explorers and historians, from antiquity to their own day, and sought information from a multitude of people with first-hand experience of living and working among the tribes they were interested in, not only such professionals as Evans-Pritchard and others, but also missionaries and officials. Brenda’s files on African kinship systems made a sizable stack, though much of this material was not reliable enough to be used for publication.

Not counting the Ainu book, Brenda’s independent publications amount only to some half-dozen papers. But many of her book reviews in learned journals are of such high scholarly standard that they deserve to be reckoned among her original work. Apart from the paper on ‘The Part of the Unconscious in Social Heritage’ which she contributed to the C. G. Seligman Festschrift, where she discusses some of her ideas on the connexion between possession cults in non-western cultures and some unconscious processes postulated in psycho-analytic theory, all her papers deal either with aspects of kinship theory or with the theory of incest and exogamy. Those concerned with kinship theory are of special interest in that, much as she admired and set store by Rivers’ pioneering work in this field, she was strongly critical of his theoretical standpoint. In particular, in contradiction to his thesis that antecedent forms of marriage determined the structure of kinship groups and patterns of kinship custom, she maintained that the ‘recognized form of legal descent’ was the critical feature. The debate continues in the current controversy between ‘descent’ theory and ‘alliance’ theorists.

The papers concerned with incest and exogamy attempt to bring insights gained from psycho-analysis into line with hypotheses derived from Malinowski and placed in the context of descent theory. Speculative in tendency, they present arguments of basic importance for theory in this field.

Finally, mention must be made again of two heroic editorial tasks accomplished by Brenda Seligman. It was very largely, if not wholly, owing to her organizing and editorial efforts that Notes and Queries in Anthropology was
revised and brought up to date for its sixth edition in 1951. The authoritative team of contributors whom she managed to coax, persuade and sometimes bully into helping with this task made this edition the standard work of reference that it has become—and, deservedly, a best-seller. The book on *Ainu Creed and Cult* was a tougher undertaking, because of both her declining health in the last decade of her life, and the chaotic state of the Munro papers. It represents equally a triumph of will power and a notable feat of scholarship for a woman in her middle seventies. She herself said that she could not have accomplished it without the help of her devoted friends the late Lord Raglan and Dr. Arthur Waley.

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**FURTHER EXCAVATIONS AT IGBO-UKWU, EASTERN NIGERIA: AN INTERIM REPORT***

By Professor Thurstan Shaw, Institute of African Studies, University of Ibadan, Nigeria

217 An interim report has already been published on the excavations carried out at Igbo-Ukwu in 1959–60 under the auspices of the Nigerian Federal Department of Antiquities. The purpose of the present article is to describe the excavations carried out there by the Institute of African Studies, University of Ibadan, in 1964.

As the result of fairly extensive enquiries, information had been obtained that ‘things had been found’ to the east of Isaiah Anozie’s compound (where the original find was made) in the compound of Jonah Anozie. Accordingly arrangements were made to excavate in this area. Fig. 1 shows the relationship of the three sites excavated.

![IGBO-UKWU EXCAVATIONS](image)

**FIG. 1. SKETCH MAP OF EXCAVATIONS AT IGBO-UKWU, 1960 AND 1964**

Showing the areas excavated (shaded) and the relationship between the compounds of Isaiah, Richard and Jonah Anozie

It was said that a clay pit had been dug in this area to obtain material for building walls in 1922, and that in the course of this digging some bronze objects had been unearthed. Replies to enquiries as to what had happened to these objects indicated that some of them were added to those from Isaiah Anozie’s compound collected by the District Officer and Assistant District Officer in 1939.

Most of the area available for excavation in Jonah Anozie’s compound was uncovered, as a result of which the edge of the 1922 clay pit was found on its west and north sides, but it ran out of the site to the south and east. This clay pit was about 1.4 m. deep on its western margin and it had sliced off the top of an older pit, designated in the excavations as Pit IV. It was doubtless in the upper levels of this pit that the bronzes had been found. Not a great deal of interest was found on the rest of the site, although there were one or two shallow pits and one filled-in water cistern 5 m. deep.

Fig. 2 shows the section through Pit IV and how its upper layers were truncated by the 1922 clay pit. It was regularly circular in plan at the depth where its outline first became visible and had a diameter of 2 m. The sides were vertical, although there was some irregularity towards the bottom and on the floor, which was at a depth of 3 m. below the present surface. The infill consisted of layers of burnt and unburnt material, sloping down steeply from the northern side, and giving the impression of a succession of loads of material tipped into the pit from that direction all within the space of a comparatively short time (fig. 3). The pit also has the appearance of having been specially dug for the purpose of receiving these contents, unlike the filled-in cistern.

There were many traces of bone—and ‘traces’ is the correct word. Many dark ochreous yellow streaks and marks were noticed in the infilling, especially in the burnt layers, but it was some time before these were identified as being due to decayed bone, for of themselves they consisted of nothing but powder. Eventually, however, some small
fragments were recognized having the cellular structure of bone. Later one or two slightly larger pieces of calcined bone were recovered, but the only one identifiable appeared to be a human phalange.

The layers in the pit from the bottom upwards (and therefore in chronological order of dumping), together with the most important contents, are as follows:

(7c) Dark grey powdery material, prolifer in pottery; small bronze bell.

(7b) Reddish-grey sandy layer, no pottery.

(7a) Dark grey powdery material, prolifer in pottery; six bronze armlets, one short bronze rod, one semi-circular ‘knot’ bar.

(6) Red sandy layer, with yellow marks of decayed bone; no pottery except for one large complete pot and one probably complete (fig. 4); two bronze armlets, associated with bone fragments.

(5b) Light brownish-grey; prolifer in pottery; decayed bone; three bronze armlets (one associated with bone); the larger cylindrical object of bronze, decorated with birds, monkeys and fish; crotal with chain link; long bronze rod; iron funnel-shaped object.

(5a) Red sandy layer, with little pottery.

(4c) Light brown, greyish in places, with charcoal and pottery; in places much decayed bone; one bronze armlet, one small bell, one larger cylindrical object (with ‘ribbon loops’), one short bronze rod, two smaller cylindrical objects.

(4b) Reddish-grey sandy layer, with some pottery; five bronze armlets, two iron armlets.

(4a) Dark grey layer, with much charcoal, and pottery; one bronze armlet, one bronze finger ring; two pieces of (?) ceremonial iron sword; hooked iron bar with fragment of bone attached.

(3) Fairly homogeneous red sand, with occasional sherds of pottery, pieces of burnt sand in places, and some fragments of bone; bronze chain.

There were only 5 beads, all of plain blue glass; they came from layers 5b to 4c. This compares with 58,000 beads from Igbo Isaiah and 100,000 from Igbo Richard.

The pottery in Pit IV belongs to the general style of the pottery recovered from the other two Igbo sites, but restorations are still being carried out and detailed comparisons have not yet been made. One gets the impression that many of the pots were complete when first thrown into the pit. This is certainly true of the largest and most impressive pot, of which the only part missing is one out of its five original handles. This pot is a remarkable piece of ceramic art (figs. 3 and 6). It consists of a globular body below a neck surmounted by a widely everted, almost flat rim. It is approximately 36 cm. in diameter and slightly less in height, although this is deceptive, since it does not look squat but well proportioned. The five handles connecting the shoulder and the edge of the rim.
have a deeply modelled pattern of horizontal rows of small raised ribs arranged chevron-wise and reminiscent of basketry. Immediately below the neck an applied strip of clay has been indented obliquely with finger-tip impressions. Below this the top half of the body of the pot is covered with patterns of deeply channelled grooves and intervening ribs, often forming concentric circles, the largest of which rise in the centre to a protuberant boss. On top of this general pattern and between the five handles are modelled a coiled snake, a chameleon (probably), a ram’s head, another snake, and a curious humped rectangular object covered with cross-hatching. Unlike the other four objects this one looks as if it is something inanimate, such as a sleeping mat, but its correct identification remains uncertain. It seems reasonable to assume that this pot is not an ordinary domestic utensil but had some ritual significance—perhaps in connexion with title-taking.

The bronze or copper objects clearly belong to the same tradition as those found on the other two sites, although they do not include anything so large or so spectacular. Some of the very slender armlets are interesting in showing especially in the light of the straight bars with expanded ends of the kind out of which they could have been made. One wonders whether they are finished or only half made. The ‘cylindrical objects’ (fig. 7) were puzzling at first, their appearance of having been designed to stand upright on a flat surface probably being illusory, to judge from the projecting bird ornaments at both ends of one of them.
They seem to belong to the same class of objects as the larger and more elaborate ‘staff ornaments’ found at Igbo Isaiah, and one of them very much recalls the style of decoration on the smaller of the two bronze bowls excavated there in 1960 with its pattern of projecting loops. The other has a pattern of curled fish and monkey heads.

Clearly the objects from this pit belong to the general ‘Igbo complex,’ but one gets the impression—and it is an impression only—that this site is antecedent to Igbo Isaiah. Charcoal samples were recovered and the results of radiocarbon age determinations are awaited.

What sort of activity is represented by this pit and its contents seems much harder to suggest than in the case of the other two Igbo sites. If one thought it consistent with what was likely to have been the practice in the area, one would suggest a cremation or a series of cremations. If, however, we are right in connecting the Igbo finds with the Eze Nri tradition, cremation is, as far as I am aware, utterly foreign to it. The contents hardly seem like those of a domestic rubbish pit, nor even of an industrial one; in spite of the bronze rods referred to above, there was no waste material from bronze-casting or iron-smelting at all. A sacrificial purpose cannot be ruled out, although one wonders how the bronzes and the complete pots fit in with this. If it was the result of warfare, it seems either that it must represent the activity of conquering immigrants deciding to settle and getting rid of the debris of what they had destroyed; or, if it was connected with raiding activity, that perhaps it was the work of the original inhabitants returning to their devastated homes and clearing up the mess before making a fresh start. There are two other possibilities: the objects in this pit may represent the ‘disposal of a priest-king together with his shrine and his regalia, when it was considered he was no longer sufficiently vigorous physically or spiritually to ensure the wellbeing of the land and the people; or this site may represent the abandonment and destruction of possessions following a case of witchcraft.

Notes
2 As none of the objects have yet been analysed, ‘bronze’ is used here in a generic sense throughout to include objects of bronze, copper, and, perhaps, brass.
4 Fig. 5, p. 405, loc. cit.


This text was collected at Brugnowi village, in the Ambutu Sub-District of the Territory of New Guinea in May, 1961. I recorded this text as told by Kanumeri, a man of about 45, on tape and later transcribed and translated it into English. I have attempted to make the English translation as free as possible without losing the feeling of the Iatmul phrasing. Throughout the translation there are words in brackets which are added to overcome ambiguities resulting from translation. The text was related as a result of territorial disputes between the residents of Brugnowi village and those of Yesan. The text relates how the present residents of Brugnowi acquired the village site and surrounding lands and establishes the ‘legality’ of their claim.

Brugnowi is a Iatmul-speaking village which was formed by a group that broke away from Japanday.

This text mentions villages (see fig. 1) representing six separate languages—Wogamusin language: Kubkan and Wogamus villages; Yesan-Mayo language: Yesan and Mayo villages; Kwoma language: Amaki, Minow, Sereaman, Washkuk, Ndagava and Madivoi villages; Manambu language: Wonsiik, Yambon, Suang, Minsiyman, Ngwatu, Malu, Avange, and Avari villages; Iatmul language: Brugnowi, Yambon, Tugw, Pagw, Runw, Angwa, Japanday, Japanday, Mwewi, Sambag, Nyawong, and Palimbe villages; Sawos language: Burui, Nyaok, Werenam, and Ngeworivi villages.

We all originated at Ngeworivi. We made our home at Japanday. This was our fathers' and mothers' home place. The people of Mwewi were at Mwewi too, while we were at Japan—
day. There were two men of Japanday, one was named Ngumberiygumbon and the other was Kaambingombo. Ngumberiygumbon took a trip to Palimbe. When he had gone, the men of Mwewi began to have sexual relations with his wife in the Men's House. They all said, ‘Ngumberiygumbon's younger brother is asleep in the upper deck of the house. Go up and look at him.’ Having gone up, they saw him sleeping there. They took a coconut shell and having put it in the fire, they touched it to his leg, but he did not feel it or awake. So he remained as they talked. Then he told them all, ‘I heard all you said.’ When he had said this they were afraid and went outside.

So he went to tell his older brother. He climbed a tree and waited for his brother. He put white clay on his skin and so he remained. Then Ngumberiygumbon came by hunting for birds. ‘Is there a reason for your being up there?’ he said. ‘Yes, there is a reason. Do you know what it is?’ said his younger brother. ‘I want to tell you what all the men did to your wife, how they had sexual relations with her, that's what they did.’ ‘Tomorrow we will split bamboo and heat it in the fire and make spear points. We will make five spears each,’ said Ngumberiygumbon.

The man who had made trouble with his wife was by the sing-sing house and Ngumberiygumbon came to him with clasped hands and asked him, ‘What are you doing?’ ‘I am cutting grass,’ he said. He was cutting the grass with a bamboo knife. Ngumberiygumbon hid his spear in the grass. He [the grass-cutter], having gone to defeather, returned bringing sago and he [Ngumberiygumbon] shot him. He shot him with each of his spears so that he died. Having killed him, he shouted, ‘Men of Mwewi, come and get your pig!’

As a result, the Mwewiis came to shoot the two brothers. The two brothers went into a house and fastened the doors. So they were inside the house the day that Mwewi came. The Mwewiis threw their spears up toward the house. The two yelled, ‘If you want to shoot us, look out, for we will kill some of your number!’

By the time night fell, they had put the women and children inside the house. In the middle of the night he [the younger brother], lifting up the limbim floor, went down, he and his wife. Having gone out of the house, he went to Japanday. He told the men of Japanday what had happened so they told him he could stay there and that he should beat the two slit drums, Kombungun and Yambira, so that his brother would come too.

Having heard, he [Ngumberiygumbon] went down and filled up his canoe. His younger brother came to get him and the two went together to Japanday and there they remained. The men of Mwewi came up river to shoot the two brothers, but they hid in the house. The Mwewiis went back and forth looking for them but it was all in vain. So they [Mwewi] decided to shoot the people of Nyawronge. They made a road known as Tipmanimbu so that they might attack them. Then they made a league with Avatip to help them fight. Meanwhile the people of Japanday were eating breadfruit at Rungway.

The Mwewiis left Wapinnow and came up river in decorated war canoes. Going up river, they arrived at Runway. All the people of Japanday said, ‘Where are you all going?’ ‘We are going to Avange,’ they said. The Japandays answered lying, ‘We already knew that.’ The Mwewiis did not know that they were lying. The Mwewiis came to shore at Rungway. Japanday gave Mwewi a long target, and said, ‘We know all about your plan to fight with Nyawronge.’ So all Mwewi went back to their village and stayed there. Meanwhile Japanday gave a target to Seserman and enlisted them to help fight. Japanday told Seserman to meet them at the foot of the mountain Mosowinmibut and they would pick them up in a big war canoe.

Japanday and Seserman came to shore at Siririvi and disembarked. They all walked through the bush Kaaru by a trail and arrived at Japanday. They arrived at night carrying firebrands. They remained the night eating and chewing [betel nut]. ‘Tomorrow we will shoot,’ they said. One group will go by the road Wiyaggi and another group will go along the shoreline of the Sepik.

They went down and made a bridge over the barat near Mwewi and they shot them while they were sing-singing. They had sung all night and by morning they had not finished yet. A woman of Mwewi had climbed a coconut and saw the enemy coming down the road Wiyaggi. She said, ‘Mwewi, you no good men and women, look! There's going to be a fight! Their headdresses are as thick and numerous as the flowers of the cane.’ When she had said this, all the Mwewiis said, ‘It is not yet the time for the battle; this battle you talked of, it is for your vulva.’ This woman only, taking her two children and her basket of shells escaped in a canoe and went to Kuvurowu water hole, a place whose there was a tree to which the women fastened their fishing baskets. There they remained. So while Mwewi was singing in the houses, they surrounded them and they were unable to get their weapons. They could only come down [out of their houses] and be impaled on the upturned spears. During the day they finished Mwewi.

They returned to Japanday and listened for talk concerning some people from Mwewi who had escaped. Presently they heard that some had escaped to Wereman so they went and shot them there. At Wereman a man from Seserman named Marine went up into a house to loot it. A woman in the house threw down a shell and it caught him in the chest. He fell to the ground dead. So they carried the corpse away on a stretcher and decorated his body with shells. They [Japanday] gave a large amount of pay to Seserman. ‘It was our battle,’ they said. By this time the flesh was rotten and only the bones remained. They brought the bones in a canoe to the mountain Mowsownimbuk and Avatip helped them. Having brought the bones to their home village, they returned. They said, ‘I have shot my older sister. With whom shall I be?’ So they went up and down the river looking. At that time there was only one inhabited place above Japanday. That was Avange. So they went further up river. Where Malu is now, there was not a settlement. So they made houses there on a piece of ground known as Nsobowanyingi. There they settled.
and cut trees for building. Two important men were there, Ngumberiyighbon and Poriyighbon.

Many people came in three big canoes. At this time Yambon was not yet. It was only deserted bush. Going up river they came to Nguay. At Nguay these two men remained. 'We two will make a
house here,' they said. They two planted trees and came back down river. They also planted Dracaena and said, 'We will build houses.' It was an uninhabited place, just a bare mountain. They also planted trees and Dracaena at Pakisay. 'Man,' Ngumberiyighbon said to Poriyighbon, 'the men from up river put tree kangaoro skies in
t heir mouths and often come down here to fight. That is why our forefathers never came up here and settled.' Having said this the two men said, 'Let's go down river a little.' So they returned to
Pawiy. There at Pawiy they stayed a long time and built houses.

While they were there an important man from Malu, Bensiynduma, they called him, came up river to see the two men. 'Do you two live here?' he said. 'Yes,' they replied. 'I think I will live here
with you,' he said. So he stayed there with them. The two men later returned to Japanday. The men of Japanday asked them, 'Why do
you go around building houses everywhere? Do you have lots of
wealth to buy all this ground?' 'Why do you talk that way? Of
course I have pay to give,' he replied. So saying, he took a shell and putting it between his teeth went up river and gave it to Bensiynduma. He also gave his wife, Kapyneiyviga, to Kemi.4

They all stayed together again at Pawiy. The people of Japanday did
not all come there. They stayed at Japanday. Only his [Ngumberi-
yighbon's] younger brother was brought there. So the two stayed
at Pawiy. Everyone else stayed at Japanday. So it was that Ngumberi-
yighbon saw the Yambon people. Ngumberiyighbon, while walking
in the bush, heard someone talking. He asked Bensiynduma, 'Who are these people talking?' He asked, 'Why are the
houses so far apart?' 'Because,' said Bensiynduma, 'before the
people known as Sowimuyuganay who used to live near Misiymyan
always fought with them so they became scattered in the bush. They are wild.' 'I will bring them outside,' said Ngumberiyighbon.

The Yambon people lived in the bush near Ambunzi at a place
called Ngwatagu. They were not really people but the descendants
of bush spirits. Their houses were covered with vines and trees.
Their houses were not good. They were only built on the ground.
They slept like dogs and pigs. There were not good houses to sleep in. Inside their houses they made their fires on the ground. Putting
tree sap on their hair and covering their ans with cupped hands,
they would dance, saying:

\[\begin{align*}
\text{Nsuviy nsuviy nhemheng,} \\
\text{Nsuviy raviyo raviyo,} \\
\text{Mbanady raviyo raviyo,} \\
\text{Mbanady raviyo raviyo,} \\
\text{Mbanady avire avire.}
\end{align*}\]

That was the song they sang and danced in that no good place
where they lived like pigs.

So they brought them out and put them at Suang. They [Bensiy-
ynduma, Poriyighbon and Ngumberiyighbon] stayed together at
Pawiy and watched the Yambon people at Suang. There were
not many but after a time they became numerous and their ideas
became good. The three told them, 'You should live close to the
Sepik.' And so they did. This is the origin of Yambon. Before they had bad ideas, living like pigs and the children of dogs here and there
in the bush, children of bush spirits. Now having been brought out,

So Suang is not their real home. Before they were only here and
there but the forefathers of Japanday brought the forefathers of
Yambon out of the bush. Formerly they were not people who lived
at and from the Sepik. Their forefathers came from a place near
Amakii. Having been begotten of a vine, they [Yambon] came.
They came to Misiymyan and there those two remained.

At this time all Japanday came up river and they built houses at
Tugwan. Japanday gave fine women to Malu and Yambon. Their names were Kworuwiniwu, Maymay, Tinggaiyimongo, Kondoo-
viyimongo, Mandagawga and Kombrimawiyi. They also gave
pay for pigs for the ground. They put down a great deal of pay.
Then Ngumberiyighbon wanted to kill Bensiynduma so he gave a
tanget to the men of Serserman. Serserman received the tanget at
the market.

In times past there was no market there. But Ngumberiyighbon,
Bensiynduma and Poriyighbon brought Serserman out of the bush
and inaugurated the market at Minow.

The people of Washuk saw the people of Serseman eating shrimp
and fish and asked them, 'Where did you get that sweet food?'
We got it from Ngumberiyighbon and Bensiynduma,' they replied.
Having said this, they agreed to have a market at Ndogava.10

While they were at the market [at Minow] they both gave
tangents to the people of Serserman and Washuk. Bensiynduma gave
tanget to the Washuks so they would shoot Ngumberiyighbon, and
Ngumberiyighbon gave a tanget to Serserman so they would shoot
Bensiynduma.

Washuk and Serserman later met and said, 'Why should we
shoot these men? They are good men. Should we shoot one and
not the other?' That's all they said. The two tangents were of
the same length.11 They shot the two men at Ndogava on the appointed
day. They also shot the child of Ngumberiyighbon named Sawun-
awa but he did not die. He escaped and returned to Tugwan.
Many of Yambon's relatives whom he had brought to the
market, they also shot and killed.

The next year they [the people living at Tugwan] made friends
with the Washuku. Nsowat who had married Tyimbuk, the
dughter of Ngumberiyighbon, shot the killer of Ngumberiy-
ighbon. So he shot the killer of his in-law. The people of Tugwan
also fought against the Washuku and at Ndogava they took two
heads in payment for the heads of Bensiynduma and Ngumberi-
yighbon which Washuk had taken.

So it was that Yambon became numerous and they were at Suang
where they had been put. Yambon made a tanget and gave it to
Malu so that they would join them in shooting Japanday.12 Two
men of Yambon, Yambumwani and Kayawaya, brought the
tanget to Malu. Having done this the people of Yambon, being
afraid, went and stayed at Malu. They stayed together with Malu
because they were afraid of Japanday. They did not kill everyone;
they only killed the child of Kisakoyvi. They only shot the worthless
people.13 They did not shoot the good people for they were away
at work. If they had been there, they would not have attacked.
Some people were at the lagoon Tes gathering limbum. Some people
were at Avatip getting dogs. Some people were at Payanguard.
Everyone having gone, they attacked a deserted village.

The men of Japanday gathered together at Pawiy. All the canoes
gathered at Pawiy. They all killed pigs. They ate. Having eaten, they
said, 'It's not good that we stay here. Let's go back to Japanday.'
They went back down river and as they passed Malu they cried out,
'Is it now the time for fighting?' 'Yes, it's time to fight,' they said.
'Why do you want to fight?' asked the men of Japanday. They
replied, 'Yambon gave komriyin to us.'14 We are spirits and we are
going down,' said the men of Japanday.15 When we have put our
belongings away, we will come back. You all stay here,' they yelled.

They came to shore at Kaaru which is near Tarangawiy. Sawun-
awa took his canoe and went to Nyawronge and told his brother
Towenduma, 'My children and brothers have come from a no good
place called Tugwan having fought with Yambon.' Towenduma
arose and gave shell money to all the fighting men of Nyawronge
he shouted, 'They have killed all my people!' When his shell
money was all distributed, the fighting men said, 'All right, they
can stay here.' Towenduma replied, 'They will stay at Mopmon-
songu.'

Later Mwewii began repaying a dispute by poison so that all
Japanday was poisoned. Day and night, morning and afternoon,
people of Japanday died and were buried. Some Japandays moved
to Angwamiyine. At Malu they burned and wrecked the house
Tambarus. They shot them in the village; they shot them in the
bush.

The people of Malu said, 'Why are you doing this? It was not
our idea. Yambon gave komriyin to us. We are not your sago and
fish.'16 They shot them all morning and afternoon and all night.
After this they made friends with Malu. Having done so, Malu gave a canoe to Japanday, 'Go shoot Yambon in this canoe,' they said. 'It was not our idea.' The name of the canoe which they gave was Kombondonge. So they got in the canoe and went up river to attack Yambon. They went up river to the mouth of the stream which drains Payangot and waited there on the lookout. As they waited a big canoes came from Yambon and rounded the point called Wangok. When they saw Malu, they said, 'Be quiet! Wait until they get close.' Three of the men of Japanday who were in the canoe Kombondonge, which Malu had given, were Wonsiynduma, Mbangirindu and Ngambaranagwon. There were four canoes from Japanday that came. Three were hidden at the mouth of the stream and one along the shore of the Sepik. The canoe continued to come up river. Yambunwandi and Kayavayiwi were in the middle of the canoe. They were also bringing three people from Seserman; Mayinongwati, Kwarasandi and Siririwiy, they were called. They were bringing them up river to a market. They came toward the canoe Kombondonge. As they got near, the men of Japanday rolled their canoes so that they splashed in the water. When Yambon saw it, their canoe tipped over and they all fell in the water. They shot them all and they shot Yambunwandi and Kayavayiwi, those two who had given komuyiwi to Malu. They shot the people from Seserman and they shot those four Yambon people.

Going back down river they threw their spears at the houses at Yambon and sang a refrain from a song about fish. The men of Yambon came out in their canoes and they fought. The men of Japanday beat them so that the men of Yambon pulled to shore. Planting their shields in the ground, they ran away. The men of Japanday, lifting up the heads of Yambunwandi and Kayavayiwi said, 'These two men killed a pig at Madiway and they are there now smoking it.' Having shown them, the people of Yambon remained there crying.

They all went back to Japanday and had a sing-sing and then returned to attack Yambon again. At the lagoon named Tes they were about to shoot Ndaamasimin [a Yambon man]. He told them, 'I am only one person. Don't make a lot of noise when you shoot me. Shoot me well because there are more people coming behind me.' So they shot him and waited in hiding. They killed the other two. Having sung the spear song, they then went to shore and chopped up all their [Yambon's] paddles and spears and cut loose their canoes and sago and looted the village. All the people ran away to a hole in the ground that was in the bush. So it became a deserted village.

One man named Mangwameriwi went out of his house to talk to the men of Japanday. Wanganeryi shot him in the testicles with a forked spear. Having been shot, he went back inside. So all the people of Yambon said it was not good that Japanday should do to them what they did to Malu, so they quickly made friends. They became friends and ate together and chewed betel nut together.

At this time a dispute arose between Japanday and Yambon. Japanday was angry because they had given lots of pay for Tugwan and Papwiy; they had given many pigs and women. The people of Yambon gathered food at these two places. 'We will come and live with you,' [Yambon] they said, 'at Yambunwondu.' So they killed a pig and had a feast. The people of Japanday gave more pay to Yambon for the piece of ground called Yambunwondu. They also gave two women, Yeverinowiyi and Mamaworyi.9 They gave two women and one pig to Yambon. They got the pay without deserving it because they were not the real owners of the ground. The owners of the ground did not get the pay; all the people of Yambon got the pay undeservedly. 'The lagoon Tes and hunting fields are yours too,' they said. 'We will share it all,' they said. The true owners of the ground had long since gone to Wogamus. Kavuro9 already had gone away. So it was that Yambon got the pay undeservedly.

Yambon decided to fight again. They gave komuyiwi to Malu, Washkuk, and Nyawrongre and the four villages together fought. We were only one village and the four villages beat us. So we returned to Japanday. While we were discussing revenge, white men came. At this time the station at Ambunti was founded. So we followed their [white men's] ideas and the revenge was stopped by their law.

We said, 'When the white man leaves, we will attack them. For now, we will go up river and stay at Brugnowi.' We did not come and stay there without paying. We gave much pay to Yesan and Mayo. We gave many pigs. The true owners of the land did not get the pay. They all got the pay undeservedly; it was not originally their land. This is what we always say. We always say, 'You people of Yesan and Mayo, you are not river people. You are from the hills. You are penis and vulva, having no breech clouts or grass skirts. You are from the north. We are river people. We put on clothes and shoot fish and crocodile and pigs and we make canoes and paddles. You are from Amarki; your fathers and mothers came from there. They just got here and squatted. The people now at Kubkan had already left and your fathers and mother squatted on this ground.'

They [Yesan and Mayo] are another kind of people. We introduced them to women's cows, grass skirts, paddles and canoes. We taught them how to live, but they wouldn't learn. We said, 'Shoot fish like this. Paddle like this. Jump in the water and wash like this. Put on a grass skirt like this.' But they would not go near the water. They did not know how to swim; they just sank. We taught them everything and showed them our ways. But they were afraid of the water. They do not look at it. They do not put their feet in it.

About this time the mission [R.C.] was established at Ambunti. After the war with Japan was finished, they [Yesan and Mayo] finally came out and began washing themselves. They told us they were only bushmen and that we might as well use the river because they did not know how. So the pay we gave did not go to the rightful owners of the ground. They had long since gone to Kubkan. The pay they got was stolen. Had they been there before, our two forefathers would have seen them. They did not see them. They are begotten of pig hair and cassowary hair. They are only anus, vulva and penis.

'It is not good,' we said, 'that river people and bush people should live together. They are of one kind and we are of another. Our forefathers put Yambon on the river and Yesan on the river. They did not forget the place.' So the boundaries of Japanday are from Nibaringi up river to Konarag. The boundary of Mwewiyi is from Sabangit up river to Nibaringi. Mwewiyi's market is at Ngungusu. Japanday's market is Wereman. If Japanday were not where it is, Mwewiyi would kill all the people of Nyawro.

Notes
- The men of Rungway made Mwewiyi believe that the target they were giving was from Avange, putting forward the time of the fight between Mwewiyi and Avange versus Nyawrongre. The target is usually made of strong grass with knots tied in it, each knot representing one day which must elapse before an important event in which the recipient has agreed to play a part.
- Saccharum sp., elephant grass.
- Refers to Mwewiyi.
- Kemiwi is the child of Bensiynuduma.
- Poriygumbo, Bensiynuduma, Kapmevimyanga, Kerni and Ngumbreriyi."gum.
- Meaning, out of the bush, to the river, to the light of civilization.
- Became good like the ideas of the men who brought them out.
- Like the Japanday people.
- The forefathers of Yambon.
- Washkuk traded sago for fish at the market.
- Their obligations fell due on the same day.
- This shooting was to be carried out at Tugwan.
- Probably those incapable of fighting.
- Komuyiwi is a type of plant leaf given as a target which obliges the recipient to fight side by side with the giver.
- The meaning is obscure.
A possible reference to cannibalism.

They were taunting the people of Yambon with an explanation as to why the two men would be a long time in returning.

Sections of sago trees were lashed together in the river awaiting processing.

Mamawory is still living at Yambon.

The name of the Wogamusin people.

At one time the Yesan-Mayo people lived at Amarkiy with the Kwomas.

This is a reference to the fact that they wore no clothing.

The early trip of the two ancestors establishes ownership of the Brugnowi land prior to that of either Yesan or Mayo.

OBITUARY

Sir Alan Henderson Gardiner: 1879–1964. With a portrait

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The death of Sir Alan Henderson Gardiner is a grievous loss for world science. From his earliest years he devoted his whole life to scholarship in general, and to Egyptology particularly. A person of rare gifts and extraordinary erudition, this tireless toiler in science has created invaluable works the importance of which for further research will remain everlasting. The high standard of Egyptology nowadays is due to the unrivalled service to science of Sir Alan himself and his school. Science by its nature is international, and his school consists not only of his own pupils but also of scientists in many countries of the world who fully approved his numerous discoveries and strict methods of investigation. One can scarcely find a piece of serious research in any branch of Egyptology in which the reader will not find quotations from his works. And though he himself displayed a keen interest in all aspects of the life of ancient Egyptians and showed an unparalleled knowledge of these matters, his dearest subject of research always remained Egyptian philology and linguistics. His works in this branch of Egyptology are unsurpassed and all subsequent researches of other scientists are in one way or another based on his fundamental principles. This permanent and profound interest in Egyptian script and language brought him to studies in complicated problems of theoretical linguistics. His works in this realm of science are well known all over the world.

During his long life Sir Alan Gardiner estimated at their true worth the works of Russian Egyptologists and especially those of the eminent Russian orientalist V. Golénischeff, about whom he wrote: 'No living Egyptologist has made greater additions to our scanty but precious store of old Egyptian literature than M. Golénischeff of the Hermitage Museum in St. Petersburg,' 'New Literary Works from Ancient Egypt,' J. Egypt. Archaeol., Vol. I (1914), p. 20. His marvellous work Late Egyptian Stories (Brussels, 1932) he dedicated 'to Wladimir Golénischeff in old friendship.'

Many years later Sir Alan invariably expressed his kindness and friendship to Soviet Egyptologists, to Soviet science. An ancient Egyptian papyrus which he published (P. Chester-Beatty, IV, 2.10) contains celebration of wise men of old days: '... their names are repeated because of their books which they have created so far as they were good, and the memory of those who made them remains for ever.'

Undoubtedly Sir Alan Gardiner was one of the wisest men of the twentieth century, and his works also will remain tych dai.

MIKHAIL KOROSTOVTEV

SIR ALAN GARDINER, D.LIT., F.B.A.
Senior Fellow of the Royal Anthropological Institute at the time of his death. Photograph by Alan Chappelow

SHORTER NOTES

MAN

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With this issue there comes to an end the series of MAN: A Record (for most if its life A Monthly Record) of Anthropological Science which began publication punctually on the first day of the twentieth century (1 January, 1901—for the birth of Christ took place at a point in time and was not spread over a whole year, as is loosely implied by those who date the century from a year earlier); the retiring Honorary Editor regrets that for a number of reasons (some of them arising in the process of unscrambling a rather long editorship) the terminal issue will be less punctual, and will indeed be followed all too closely by the inaugural issue of MAN: The Journal of the Royal Anthropological Institute. This may, however, serve to emphasize the continuity between them in spite of the superficial changes of format.

This is no place for the adumbration of the policy of the new honorary editors; but it may at least be said of the new MAN as of the old—and as of all such learned periodicals—that its content is to a great extent in the hands of its readers, who are also its contributors. The fact that it is more and not less than before an organ of the Royal Anthropological Institute is the best guarantee that it will continue to represent the whole field of the anthropological sciences. Indeed, under the new arrangements made possible by the change, the practical equipment of the editors for the carrying-out of that task has been greatly improved.

Finally, the retiring Honorary Editor would like to record his
deep and sincere gratitude to all those who, during the past 19 years, have helped to make the editing of MAN much more a pleasure than a chore: to its printers, William Clowes & Sons, Ltd., and their staff at Bcecles—with a special citation of the extraordinary virtuosity of their readers—, for their constant efforts to make the periodical both typographically beautiful and accurate as a scientific journal must be, and for the unfailingly friendly and harmonious atmosphere of their collaboration; to MAN’s readers—about trebled in number during the period,—whose support made its publication possible, and who with great forbearance never but once sued for libel; and perhaps above all to its contributors, many hundreds in number, who actually wrote it, who amiably tolerated, e.g., his dogged conservation of the English (or British) spelling of ‘ethnographical’ and of the ancient principle that even a unanimous vote of all Anglophone anthropologists could not validate the qualification of one adjective by another (as in ‘social anthropological’), any more than the proposition that the Earth was flat, and who showed a surprising degree of understanding of the serious shortcomings as a correspondent of an editor who attempted in his ‘spare time’ not only to produce twelve issues (or latterly six double issues) a year but also—a task not yet fully complete—to maintain some communication with about 200 actual or would-be contributors every year. He sincerely wishes his distinguished successors an equal degree of co-operation from all on whom they depend and a greater resolution to deserve it.

A Carved Headrest of the Cushitic Boni: An Attempted Interpretation. By Dr. A. H. J. Pints, Department of Cultural Anthropology, University of Groningen. With three figures

The Boni hunters occupy the desolate and weird but certainly in parts scenic savannah hinterland of Lamu and the Bajun Islands. This habitat, stretching as it does between the lower Tana and Juba Rivers, offers them ideal conditions for their hunters’ and gatherers’ economy. The plentiful game includes elephant, rhino and buffalo, together with zebra, ostrich and a variety of antelope. Elephants alone are twice as numerous as the Boni themselves. Also a multitude of beasts of prey, including lion and leopard, are their daily and nightly companions in a very literal sense. The plenty that surrounds them permits them to be semi-sedentary and not nomads in the sense in which the Bushmen are. Also their symbiotic relationship with the Swahili-speaking townspeople on the coast and the Somali, wandering herdsmen thinly dispersed over the vast steppe and savannah, enriches the possibilities of exploiting their surroundings. They need not be self-reliant, merely relying on their own resources within a closed economy. Their hunting, though done mainly for food, yields a large surplus of horn and ivory which is bartered or sold against cotton piece goods, millet and maize and iron implements. Also they themselves engage on a limited scale in shifting cultivation, and have been doing so for at least two or three generations. Forest produce too is gathered and sold. Thus the relative abundance which they live in permits them not just to eke out a bare existence, but to have sufficient leisure to wander about visiting each other’s hamlets, thus weaving a web of kinship, affinity and friendship through which Boni culture integrates itself. It is this same leisure which enables them to attain a standard of craftsmanship which makes them comparable with Prairies Indians rather than with such hunters as the Bushmen, whose present paucity in material culture is, I believe, proverbial.

It is to a feat of such craftsmanship that I intend to draw attention. The wooden object itself deserves description, for it is a beautifully balanced piece. But also, Boni ornamental art (or material equipment in general) deserves to be brought to notice, because it is virtually unknown and not, as far as I know, widely (or at all) represented in museum collections.

The wooden headrest under investigation (fig. 1) was given to me, in exchange for a present, by a youngish man (of about 18 to 21 or a few more years of age) from the hamlet of Bolaa whom I met in Baragoni, a mixed Boni-Bajun settlement, about 20 miles inland from Lamu. He, together with an age mate, had wandered this far, about 50 miles from home, to visit a married sister. The headrest was one of the very few things which he carried with him in his travelling kit. His companion also carried one. They clearly were highly valued possessions, parted with reluctantly. But since he said that he could procure himself a new one (whether from some other craftsmen or by carving a new one himself I do not profess to know), he was finally induced to part with it.

FIG. 1. THE BONI HEADREST
Photographs: Rijksuniversiteit Groningen

To make a headrest the woodworker needs the proper kind of tree, one which at a certain girth bifurcates evenly into two branches each about as thick as a man’s arm. With knife and chisel he then proceeds to give it the required form. After the first rough shape has been blocked out, the centre is cut away. The headrest now consists of a base on which two struts support the curved concave board on which a sleeping Boni rests his neck. As soon as this process has been completed, that of decoration starts.

Viewed from any angle the headrest strikes the observer as beautiful, balanced and even graceful, but it remains a useful object with a ‘functional’ beauty. However, if he happens to look at it frontally, exactly in front, he is struck by an odd impression that it is something more than a headrest. He may wonder what exactly gave him this impression—until he sees the answer before him in the form of a cow’s head. The muzzle, the cheeks, the bovine front, the curved horns, they are all there. It is what may be termed a bucephalic headrest (fig. 2).

Now the observer may rest content with this observation and never look at the thing laterally. In fact he is rather unlikely to do so, unless he sets out to measure it exactly or draw its outline. If he does, he will probably undergo another sudden sensation, for, viewed from the side it is again something more than a headrest. To me it is beyond doubt that what he sees is a circumcised
phallus. Hence the object seems to be twice a symbol: first a symbol of cattle, although now at least the Boni are no cattlekeepers; secondly a symbol of circumcision, though I do not know whether the Boni circumcise.4

Hence it might also be called a 'phallic' headrest (though admittedly the idea of a headrest being phallic seems a trifle incongruous). What else should one call it then but a 'bucephalophallic' object?

![Fig. 2. Frontal or Bucephalic Aspect](image)

Thus far about the headrest as a form. But since it is not made of plain wood but bears decoration we must now turn to that aspect of pattern.

The decoration is applied on both outer sides of the two supporting planes and consists of a geometric pattern which can only have been carved with a pointed knife, held in a steady hand—as one would expect of a hunter of elephants.

The carved patterns on the two sides are not identical, although they reveal an overall similarity. This reminds one of the way in which the principle of duality is actualized in the social structure. There too one finds dyads, but not juxtaposition of two halves matching each other in every respect.5

The themes expressed in the carving are concentricity and angularity. These are the two general ones in which all the motifs are comprehended; or, to put it in a different way, every figure is angular and, whenever space permits, it is also concentric. This last theme already of necessity entails the use of repetition, a third theme. A fourth, logically dependent, is found in the use of juxtaposition. Juxtaposition again is inconceivable without the application of the theme of duplication. These five themes—

- angularity,
- concentricity,
- repetition,
- juxtaposition,
- duplication

may be placed in a more proper hierarchy as follows: duplication → juxtaposition → repetition → concentricity → angularity. In listing these five themes (omitting to mention as 'independent' themes those of mirroring and of alternation) I have not done enough. But though I would like to continue talking about duplication, I must admit that there is the basic difficulty of knowing whether two ones make two or two halves one; possibly both are meant to be true. I am now thinking in Boni terms, or at least trying to do so. For, in the top row (i) of the plane (A) two squares of three columns each (1, 2, 3 and 1', 2', 3') are divided by one single central column (4). Therefore as a first rule: one plus one makes two, or, conversely: two is divided to make twice one. In the same plane, however, rows ii and iii reveal that two triangles (i.e. half-squares) four times repeated make up one square. Hence, as a second rule: one divided in two is twice a half, or one half plus another half makes one.

The first rule is again exemplified in the top row of plane B, the second one in the middle row. In the third row (B iii) an anomaly occurs, not uncommon in primitive art: one triangle is left unfinished.

About juxtaposition (and mirroring) I need not say much. It occurs as between the halves of A ii and iii and B ii, in the first two cases as mirroring, a special case of juxtaposition, to be sure. The same holds for the left hand square of B i, where we find the only exact duplicates.

Repetition, including alternation, is apparent when we consider A ii and iii and iii and B ii, in the first two cases as mirroring, a special case of juxtaposition, to be sure. The same holds for the left hand square of B i, where we find the only exact duplicates.

About the other themes I will not speak: both the angles and the concentric occurrence of triangles and squares are sufficiently apparent.

The ordering of the motifs used in the decoration has become clear by now. But order is concerned not only with quality, but also with quantity, or rather numbers. The Boni, in their ordering, apparently make frequent use of twos and threes. Elsewhere I have demonstrated how they do this in their social order too. A village which I described consists of two demes but of three lineages. Here in their carving the numbers that crop up are: 27, 24, 18, 12, 9, 6, 4, 3 and 2, all multiples of two and three. Concentricity is expressed in threes, fours and sixes; angularity itself in three; a square in four. Duplication, mirroring, juxtaposition and alternation are all impossible without the use of the number two. The numbers 9 and 12 are apparent in A 1, where 18 also occurs; and 24 appears in B iii.

![Fig. 3. Lateral ('Phallic') Aspect](image)

It would require some measure of ingenuity to find other basic numbers expressed (like the 'five' of the Berbers, the 'seven' of the Israelites and the 'forty' of the Galla), and if they can be found I am sure that they are there without reference to what the Boni themselves might well regard as one of the self-evident ways in
which to express some of the values embedded in their own culture.

If we anthropologists were, for some taxonomic reason, to adopt a nomenclature akin to that employed in organic chemistry, we could refer to this Boni culture trait as a 2,3-bucephalophallic headrest!

Notes

1 Two more groups of Boni, not inhabiting this same stretch of country, both without doubt forming a northern extension, have been reported from the Lower Shebelli River in the Barawa hinterland and from the east bank of the Lower Juba, north of Margherita.

2 I think that I may have been the first anthropologist to do any piece of systematic investigation among them, in their own habitat. Grottanelli met a group of them on the coast; Battaglia’s paper is based on information supplied by Grottanelli. Other communications, with the one noticeable exception of Puccinio’s, are negligible. Art has been mentioned only once before.


3 Four somewhat similar headrests, but with entirely different ornamentation, were collected by Dr. L. G. A. Zöhrer, an Austrian U.N.E.S.C.O. expert in Somalia, and were shown to me in the collections of the Museum für Völkerkunde in Vienna recently. It would be worthwhile to investigate whether these objects are really Daret Somali (as the label states; Dr. Zöhrer collected them in Afmadu, as he told me in a later communication) and, if so, whether they were made by Somali men, or by Boni and traded for other produce. The same applies to some Somali headrests in the British Museum. Most headrests from the Horn of Africa that I have seen (Galla, Kaffa, Danakel, Gadbursi Somali) are entirely dissimilar, having some sort of central support, instead of the two planes in the Boni case. They are also invariably of cruder workmanship, hardly attaining artistic standards.

4 If so (and I think this very likely), they do not carry it out at an early age. Youngsters of six or seven running about naked have not been operated upon. When I say ‘circumcised’ I refer to circumcision proper and not to the operations known as incision and subincision. The representation, reduced to two dimensions, is quite naturalistic and easily discerned, the prepuc appearing to have been cut off.


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A Masai Text. By Dr. T. O. Beidelman, Department of Social Relations, Harvard University

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In this brief communication, I submit a Masai text (with English translation) which, to the best of my knowledge, has not been previously reported, perhaps because of its somewhat obscure nature. The text may prove of some value to those interested in problems in the symbolic expression of sexual opposition and antagonism. Although these motifs seem strikingly obvious, I make no comment since I do not consider myself sufficiently familiar with Masai society and culture to do so; I hope that others, better qualified, may provide some interpretation.

The text and translation were provided by a literate Masai from north-western Masailand, Tanzania; the informant desires to remain anonymous. Although both he and I are familiar with Tucker and Mpaaey’s orthography, we have retained our own.

In the beginning all were alike. The man had his manly part on the face and it was the same with women. Then one day the man asked God to make him [over] in a different way. He took off the manly part and put it between the [man’s] legs. When the man saw that the man no longer had his manly part on his face, he felt conscious of himself. So she asked God to put her womanly part between her legs. God agreed to do so and told her that she should come out into the cattle enclosure 1 and try to make something to meet him. When it dawned, the woman came out into the enclosure and met God. The woman said, ‘Since you have now agreed to make me [over], let me be a man.’ God refused.

‘Then the woman said [to herself], ‘I know what I’ll do. Just let Him take off this loathsome thing and He’ll see me.’ 4 The woman wanted to be a man, because the man was strong and he ruled over the woman. When God removed the womanly part, the woman ran away trying to enter the house. She left God holding her womanly part in her hand. You know Masai houses are very low so that one must stoop over when going in. God looked at the woman running and said, ‘Where is she running to?’ God waited until the woman stooped over to enter the house. As she bent over, God threw the womanly part [at the spot] between the woman’s legs. The man looked at God throwing the womanly part and exclaimed, ‘Gad! What a shot!’

Ore ape pee eng’as itoloren aponu engop kenyanyukoro apa pooki tung’ani. Ore lewa meeta olevaisho eillio te ngonom, nejyoake siinindie apa ekuunio ing’oryoky. Ore engongolong nabo nejyo olevaeng’i ayatavyara ekkunyiri ng’ojeek. Ore ape edol engito olevaisho ete olevaisho te ngonom, netum isura neonon olevaeng’i ape ekitayi enekuitiiahe ise apik arihsa ng’ojeek. Nényoare eng’i nejyoone olevaisho ekkunyiri ng’ojeek. Orevi olevaisho ekkunyiri ng’ojeek.


Notes

1 Although the plural form is previously used, here and in all subsequent parts of the text one finds the singular form.

2 The kraal (boo).

3 nakashiri.

4 In the same sense as the common Bantu expressions which imply that a person will learn of another’s anger or power, if that person is crossed or threatened.

The Plantar Main-Line Index and Transversality in Panjabi Khatri of India.* By P. K. Datta, M.S.C., Ph.D., Lecturer in Anthropology, University of Delhi. With a figure and a table

The proximal radiants of the four digital triradii a, b, c, and d, located at the base of the small toes in fibio-fibular sequence constitute the plantar main lines (fig. 1). The terminations of these main lines, placed in the reverse order, form the main-line formula. A general idea of the alignment of ridges and the transversality may be obtained from the main-line formula and the index. The index of transversality is useful in the studies of evolutionary process and ethnic variations, besides personal identification, like other dermatoglyphic traits.

Material and Methods

The present investigation is based on a study of sole prints of 270 individuals, drawn from the Khatri community of Panjab, at present settled in Delhi. The sample has been collected at random and consists of both the sexes, i.e. 130 males and 120 females. The technique followed in the collection of plantar prints and their

* This paper forms a part of the author’s Ph.D. dissertation accepted by the University of Delhi in 1961.
analysis is in conformity with Cummins and Midlo (1961). In order to study the generalized direction of ridges on the plantar surface, usually the collective main-line formula had been taken into account, but owing to a great number of combinations produced, Cummins and Midlo (1961) and Steggerda and Steggerda (1936) suggested the application of 'modal types' to reduce this number. However, Cummins has recommended the use of the main-line index as a better and simpler device for recording the most significant features of the total main-line formula. This index, calculated as a single figure, is a more useful measure for comparative studies.

From this scheme it may be observed that with an increase in the transversality of ridges there is a corresponding increase in the value. The range of index for a sole is from 4 to 14.

Results and Discussions

From a study of Table I it is evident that, considering the main lines D and A alone, the Indian sample from Panjub displays 18 different types of formulae. The characteristic feature of this population is the presence of formula 1°-1° in about two-thirds of the plantar prints. The mean main-line index presents a value of 11.70, with very insignificant sex variations. However, the males tend to possess a slightly higher index (11.74) than the females (11.67). The bimanual differences are indicated in the male sample alone, with right soles possessing a higher index than the left ones. This is in conformity with the study of the palmate main-line index and transversality in some European populations, viz. European American males (Cummins, 1941) and females (Steggerda and Steggerda, 1936), German males and females (Cummins and Clowes) and Jews of both sexes (Cummins). The right/left ratio of the plantar main-line index in the Indian population is 1.01 for the males and exactly 1.00 for the females. The standard deviation for the male group is 1.78 and for the female 1.91. The coefficient of variation (the standard deviation as a percentage of mean) is 15.16 and 16.36 for the males and females respectively.

Table 1. Frequency distribution of plantar main-line index in 150 male (300 soles) and 120 female (240 soles) Panjab Khatri's from India

<table>
<thead>
<tr>
<th>Main-Line formula</th>
<th>Main-Line index (D.A.)</th>
<th>Males (No. of soles)</th>
<th>Females (No. of soles)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1°-1°</td>
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Mean main-line index: 11.74 for males and 11.67 for females.
Mean main-line index for right sole: 11.91 for males and 11.67 for females.
Mean main-line index for left sole: 11.57 for males and 11.67 for females.
Right/left ratio: 1.01 for males and 1.00 for females.
Transversality (percentage): 2.93 for males and 0.00 for females.
Standard deviation: 1.78 for males and 1.91 for females.
Coefficient of variation: 15.16 for males and 16.36 for females.

Summary

1. The plantar dermatoglyphic prints of 150 male and 120 female individuals from Panjub (India) have been analysed with a view to studying their main-line index and transversality.
2. The mean main-line index and transversality for the Indian population from Panjub is 11.70 and 1.46 per cent. respectively.
3. In the sex differences, the males display a greater index than the females. The transversality is only 2.93 per cent. in the males and absolutely none in the female sample.
4. The bimanual variations are noticeable in the males only. The index is comparatively higher in the right sole. The right/left ratio for the males and females is 103 and 100 respectively.
5. The standard deviation for the male sample is 1.78 and for the female 1.91. The values for the coefficient of variation are 15.16 and 16.36 for the males and females respectively.

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A Note on Bali Jatra. By Jyotirmoyee Sarma, School of Oriental and 
African Studies, University of London

224 With reference to Professor P. E. de Joscelin de 
Jong’s Special University Lectures in Anthropology at 
the London School of Economics in February, 1963, on the spread 
of the Indian civilization and trade in the Indonesian Islands, I recall 
an interesting vestige of the days of trade between India and 
Indonesia in Orissa today. In the city of Cuttack in Orissa an annual fair 
exists which is called Bali Jatra. Literally it means ‘going to Bali.’ 
The fair is localized in Cuttack, and is held every year on the grounds of 
the Cuttack Fort called Fort Maidan, just to the side of river Mahanadi. 
It begins on the evening of the full moon in November or Rasa 
Purnima, and lasts for three days. The village craftsmen and traders 
from the areas surrounding Cuttack bring their merchandise to the 
fort, and large numbers of people from the villages come to Cuttack 
to see the fair, and to take boat rides on the Mahanadi afterwards. 
It is also customary for young girls and boys in many parts of Orissa 
to rise at dawn on the first day of Bali Jatra and float decorated paper 
boats on rivers or lakes, and to go for boat rides in the evenings in the 
light of the full moon. In Orissa Bali Jatra day is a State holiday, and 
although the young people it gives occasions for merriment, the 
intellectuals are aware that these celebrations are commemorative of 
the fact that going to Bali was at one time a major event in Orissa, 
and the traders went down the Mahanadi.

A Zemi Naga Legend of the Great Indian Hornbill. By 
C. R. Stonor

225 All of us who have had the privilege of knowing 
the Nagas have been impressed by the important place 
in their folklore of the Great Indian Hornbill (Dicerorhina albirostris). 
Some years ago I was told the following legend by the Zemi Nagas 
of the North Cachar Hills of Assam.

‘We Zemis have a tale of how the Great Hornbill first came into 
being. Long ago there was an orphan boy without relatives or friends 
to care for him. So he left his own village and went to live in another, 
far distant from his home. Here he was neglected and despised; and 
he went daily to work in the fields with the young men and girls. 
He made friends with a girl who was kind to him, and fell in love 
with her. One day, when they had stopped work to eat their meal 
in the field where they were weeding, he went to eat by himself, 
because the food that the villagers had given him was bad and dirty, 
and mixed with pigs’ dung, and he was ashamed to eat with the rest.

‘The girl came up to him and offered him her food, but he was 
soured by his harsh treatment and would not take it; and he even 
threw his own meal away in disgust. Then he asked her, “Will you 
give me your fine striped skirt?” And she gave it to him. He asked 
her for her black body cloth, and her rice-beer gourd. Both of these 
she gave him. He put the skirt and the cloth round his own body, 
and the gourd into his mouth, and changed into a great black and 
white bird with a long yellow bill.

‘And straightway he flew up into a tree. Looking down at 
the girl who had befriended him he said to her “How do I look now? 
Am I not handsome?” “Yes,” she replied, “indeed you are handsome; 
come down here.” “No, I will not come down; I am going away, 
but one day I will come back. When you hear me coming, 
run out of your house, and I will drop down one of the best 
of my tail feathers.”

‘Many years went by. The girl married and brought up her 
family. Then, one day she heard a mighty droning of wings. She 
ran out of her house, and the Hornbill flew over her and dropped a 
feather which she caught. A man was standing nearby who was in 
love with her, and he called up “What about me? Give me a feather 
as well.” “No,” replied the Hornbill,” you never befriended me or 
helped me.” And, so saying he defected, and the dung dropped 
into the man’s eye and blinded him.

‘The bird flew away, and from that day on the girl had all she 
wanted of money, clothes and rice. Even now, when we see a 
Hornbill we believe that it was once a man.’

Découverte d’une statue de bois dans la vallée de Parun (Nouristan). By Ahmed Ali Motamedi, Director, 
Kabul Museum, Afghanistan. With a figure

Au cours de l’été 1963 M. Palwal (qui étudiait le 
dialect Munjari) a averti la Direction Générale des Antiquités et 
Musées d’Afghanistan de la présence d’une nouvelle statue de bois 
dans la vallée de Parun au Nouristan (un des districts afghans au

Fig. 1. Wooden Statue from the Valley of Parun, 
Nuristan

Height 79 cm. Photograph: A. A. Motamedi

L’aspect général de la statue n’est pas nouveau mais de nombreux détails le sont. Parmi les statues du Nouristan actuellement exposées au Musée de Caboul, nous avons déjà deux exemplaires du même type, représentant une femme assise à califourchon sur un capridé (voir Edelberg, Lennart, *Artis Asiaticae*, tome VII, 1960, fasc. 4 p. 250, fig. 7 et 8).

Nous allons examiner maintenant les différences qu’on remarque en comparant la nouvelle statue aux deux autres du même style et parfois à l’ensemble de la collection. Ce qui frappe tout d’abord c’est la tête. Le sommet du crâne est châtré, mais on remarque des cheveux derrière la tête. La tête apparaît entre les cornes du capridé mais le menton ne repose pas sur la tête de l’animal. Le visage est fort différent: les yeux sont en relief (ce qui est rare parmi l’ensemble des statues) et fait unique, les pupilles sont figurées par des feuilles de mica. Le nez a des proportions normales (d’habitude il est beaucoup plus gros). La bouche est ouverte laissant apparaître les dents de la mâchoire supérieure (d’habitude la bouche n’est signalée que par un trait dans le bois). Les oreilles sont bien dessinées et le front est tatoué. Autour du cou, fait également unique, on remarque un collier à double rang formé de petits cailloux incrustés. Contrairement aux deux autres statues celle-ci a des épaules. Les mains saisissent aussi les cornes de l’animal, qui est beaucoup plus petit, mais les seins occupent leur place normale. La nouvelle statue a un bracelet au poignet gauche et un double bracelet au droit alors que les deux autres ont un double bracelet à chaque poignet. Enfin, aux dos des deux premières statues on voit le panier conique que les femmes portent généralement au Nouristan; la nouvelle statue n’en a pas.

A première vue cette statue paraît neuve. Or selon les renseignements que M. Palval a recueillis sur place cette statue a été découverte dans une maison qu’on vidait en vue de la démolir. Si la statue était ancienne le bois aurait normalement dû porter des traces d’altération, or il n’en est rien, ce qui nous amène à avancer les deux hypothèses suivantes:

1. Il y a peut-être encore au Nouristan des gens qui sont fidèles à leur ancienne religion et qui continuent la tradition de leur art ancien.
2. Il est possible que cette statue provienne de Chitral où la religion primitive de l’Hindou-Kouch est encore vivante, et qu’elle ait été amenée récemment au Nouristan.

Quelle que soit son origine cette statue est donc une découverte importante du point de vue ethnographique.

Note


Two Rock Engravings from South Africa in the British Museum. By Dr. Gerhard J. Fock, Alexander McGregor Memorial Museum, Kimberley, South Africa. With two figures

In 1963 I paid a short visit to the British Museum and saw two South African rock engravings in the Ethnographical Gallery. One shows a quagga and on the other rock are several animals. Lack of time and suitable material prevented me from making rubbings, and I only tried to establish the locality from which the rock slabs came. All I could find out was: ‘from a site about 8 miles north of Kimberley.’

Recently Miss Denise Gross of London was kind enough to provide me with rubbings which enabled me to search here in Kimberley. The Alexander McGregor Museum boasts a collection of copies of rock paintings and rock engravings, some dating back nearly 100 years ago. Among the copies of rock engravings made by G. W. Stow, the well-known geologist and author of *The Natives Races of South Africa* (published posthumously), I found one which depicts the quagga now in the British Museum. I do not know what methods Stow employed to obtain his copies, and whether he made tracings of some kind, as his copies are sometimes very accurate but sometimes show slight divergencies from the original. This is the case with the quagga. Compared with Miss Gross’s rubbing Stow’s animal looks like a three-horned creature, while on the slab the ears are in proportion and one can see a chipping (not man-made) roughly in the middle of the forehead and another above one ear. The tail is hanging down more or less parallel to the hind quarters. The stripes do not cover the whole body, thus indicating that this is the extinct quagga (*Equus quagga bontii*). As far as size

**Fig. 1. Drawing by G. W. Stow after Rock Engraving at Wildebeeskuil**

**Fig. 2. Rubbing by Miss D. Gross from Rock Engraving in the British Museum**
goes the copy is of the same size as the original, the stripes are arranged in the same way but in addition to the mistake about the chipping and ear the tail is also not correctly copied by Stow. On the engraving it is parallel whilst Stow let it fly horizontally to the end of the slab. But there is no mistake that he depicted the animal now housed in the British Museum. He wrote on the sheet of paper where he copied this quagga (and a number of other animals still on the site): 'Wild Animals from the Rocks on Bushman’s Koppie on the Farm Wildebeest Kiel.'

Wildebeestkui is a farm near Kimberley and that koppie (hill) is about nine miles north-west (not north) of Kimberley. We know that many engravings have been removed from the site; this was formerly not prohibited by law. But even so we still have about 200 slabs with engravings on that hill. As I learnt in the British Museum that both slabs are from the same site, I think that I am correct in saying that both come from Wildebeest Kiel. At the Colonial and Indian Exhibition in London in 1886 there were exhibited a number of Bushman carvings from Wildebeest Kiel: 'Zebra, ostrich, elephant, rhinoceros, hippo, hartebeest, crane bull,' so the catalogue informs us, according to Dr. Maria Wilman (The Rock Engravings of Griqualand West and Bechuanaland, South Africa, Cambridge, 1933). This would indicate that the slabs probably came into the British Museum after the exhibition closed. A 'zebra' is listed instead of the even then extinct quagga, but that is of no avail in this connection.

**A South Sumatran Corral. By Dr. M. A. Japans, Department of Anthropology, University of Western Australia. With a figure**

Whilst collecting lineage genealogies at the Redjang village of Sawang Libia in the North Bencoolen kabupaten in 1963 I was surprised to see a man setting up an ingenious portable springe corral. The device, known as an ajit, was said to have been common until a generation ago, but this was the last remaining example of it known in Redjang country.

**FIG. 1. SPRINGE CORRAL, SOUTH SUMATRA**

The corral (fig. 1) is designed to trap the wild forest cock (monok bugo; G. gallus gallus) found in the foothills of the Barisan Mountains. It consists of 14 rattan stakes (beis) to each of which a noose (skap) of lian or root lashing (pukul) is attached. The sliding end of the noose is attached to the stake by a woven rattan ring (pasan). The stakes are joined one to another by a thin cord (kichia) of twisted barkcloth; when they are stood up in the ground the kichia is stretched taut so as to form a circular obstacle or fence. The stakes when arched are 70 cm. high.

The corral, which has a diameter of about 2 metres, is usually set up in a small clearing of the secondary forest. The trigger places a domestic cock as a decoy in the centre of the corral, its foot tied to a stake. He then moves about 20 metres away and hides in the bush to await his quarry. The wild cock approaches the corral and gets caught either flying into one of the fourteen springs or in attempting to get out. Its loud clacking and cackling on being caught summons the trapper who is generally able to take his prey alive.

**Notes**

1 A. L. van Hasselt, in his Ethnographische Atlas van Midden-Sumatra, Leiden, 1881, Plate CXX, has left us a line drawing of a similar device that he saw in West Sumatra, there called a djuré balam.
2 Van Hasselt’s West Sumatran corral shows 35 stakes.
3 See van Hasselt’s excellent drawing (Plate CXX, 2, 1/1) of an almost identical beus, skap, pasau and kikia.

**A Unique Ghana Stool. By Dr. M. J. Field. University of Ghana. With a figure**

The common type of chief’s stool in Ghana is the well-known ‘Ashanti stool’ of the Akan peoples, which was always carried into battle to bring victory. In this capacity of a war medicine it was copied by the Ga, Adangme, Ewe and aboriginal Guan. Previous to this, these imitators ‘did not know chiefs and did not know war stools,’ but were under the leadership of priestly patriarchs. Their nearest approach to stools were tiny ivory headrests resembling the ivory headrest of Gua the Elder of Egypt, now in the British Museum. These venerated objects were never taken to war but were carried in procession annually on the heads of young virgins who were then sacrificed.

**FIG. 1. THE AWUTU STOOL**

However, the Awutu-Beretu of Awutu, a patrilineal people speaking a Guan dialect, have a stool (pru) of a type not hitherto described elsewhere in Ghana. The Awutu immigrated into their present district in the sixteenth century bringing this stool. On it every new chief (odefey) is custedled.

Unlike the Ashanti type of stool, which always lies on its side except on ceremonial occasions, the pru always stands upright. It is carved out of one cylinder of wood, has three handles and is kept white with an annual coating of new whitewash. It has never been to war, but is evidently a fertility object as it displays a carved phallus.
Rotating Credit Association among Indians in Malaya

Sir,—In her Wellcome Medal essay for 1963, Shirley Ardener (1964, p. 203) states: 'Rotating credit associations are said to be found among women, but not among men, in Indian communities in Malaya (Sharma, 1962, personal communication).' This seems to be no longer true (Jain, 1965). On rubber estates and other rural settlements of India in Malaya, there is a well developed rotating credit association known as citta or kuta. A typical citta has a mixed membership of both men and women, and not of women alone. Two main varieties of mixed citta are found: elor-citta (citta by auction), and kula-citta (citta by lots) corresponds to Ardener's model 4 (p. 214) and kula citta (citta by lots) corresponds to model 1 of the same page. The membership of the latter type of citta may sometimes be sexually exclusive, or even based on age. Men, just as women and children, may form kula citta (citta by auction) to buy from the citta fund an article of common interest to the members. About urban communities of Malayan Indians Charles Gamba (1958, pp. 41-42) writes: 'The kuthu, with or without interest, is common among urban Indians in Singapore and Federation of Malaya, and among lower-grade employees in the civil service.' That this kuthu has an exclusively female membership is nowhere indicated by Gamba. I am convinced that Ardener's information may be true for a small sector of the Indian population in Malaya and it should not be generalized for 'Indian communities in Malaya' as a whole.

I am in sympathy with Ardener's contention that the rotating credit associations may be useful be related to the traditional systems of mutual assistance found in a society. I had independently arrived at a similar view in trying to explain the pervasiveness of citta among Malayan Indians. It seems that the situation there is brought about by the interaction of two forces, namely, the traditional systems of reciprocity as exemplified in certain aspects of ritual prestations in Tamil society and the economic environment of immigrants working for small, though regular, wages or salaries.

R. K. JAIN

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References


Nigerian Traditional Healers. Cf. MAN, 1965, 102

Sir,—Professor Margrett's article on the traditional healers of the Yoruba of Nigeria recalled an experience of mine among the Ibibio of the Calabar Province. In 1926 I was the administrative officer in charge of the Ikot Ekpene division. An Ibibio laid a complaint that his wife, Adia Udo Ikpene, had been abducted and was now forcibly restrained by one Akpan Umo Idiong. I asked him what Native Court he attended and he replied Ikot Okoro. As I would be there in the next few days I told him to see me there. I arrived at Ikot Okoro in the early afternoon and inquired about Akpan Umo Idiong only to be told that he was a well-known abia ijob, i.e., herbalist. As he lived about two miles away I paid him a visit. Did he know anything about a woman Adia Udo Ikpene? He did. She had been brought to him by her parents because 'she did not know herself.' However, she was getting better. I asked him what his treatment was. He replied that he had her locked up in a darkened room, had shaved her head, was putting cooling lotions on it and giving her bland food and that he repeatedly went in and talked to her. She was improving, and would soon be returned to her parents.

I asked him if he treated everyone who came to him. He replied that he did not. He first examined the patients and if he saw that the person was incurable, he told the family to take the person away because, so far as he was concerned, that person was as good as dead.

I gathered that he specialized in what today are called psychosomatic afflictions rather than organic ones.

The next day the husband arrived. I told him I saw no reason to interfere.

M. D. W. JEFFREYS

Witwatersrand University, Johannesburg

West African Beads. Cf. MAN, 1964, 90

Sir,—In his article 'Modern Koli Beads in Ghana,' A. Sordinas has omitted to mention that I and r are interchangeable throughout many of the languages of West Africa (cf. D. Westermann and M. A. Bryan, Languages of West Africa, Part II, Handbook of African Languages, O.U.P., 1965, pp. 42-47); consequently the koli bead is also known as the akori bead. This matter of the nomenclature of the Akori—Aggrey bead was discussed by me in 'The Marginelle Currency of Timbuctu,' Bulletin de L'Institut français d'Afrique noire, Vol. XV (January, 1953), and in 'Aggrey Beads,' African Studies, Vol. XXX, No. 2 (1961).

M. D. W. JEFFREYS

Witwatersrand University, Johannesburg

The Corbelled Stone Huts of the Riviera. Cf. MAN, 1962, 46

Sir,—A new reading of James Walton's note on 'The Corbelled Stone Huts of Southern Europe,' has led me—though belatedly—into a brief comparison with my own recent observations (August, 1964).

Walton mentions the huts of Cervo territory; this report concerns those of the Diano hinterland, only a few miles from there—huts raised up by shepherds and peasants using slabs of local eo-oligogenic limestone, in the olive groves and upon the agricultural terraces which contour the valleys and heights. Most huts appear to belong to a 'free-standing' form, with a round plan, a single straight lintel and a double wall: the interior one, acting as a supporting wall, is often of the corbel type, that is of tiers of stones projecting increasingly on the inside; the exterior one is nearly vertical, the interval between them being filled up with a débris of stones or gravels. Both Walton and Casson (see MAN, 1963, 80) have described such a model. A wide horizontal stone plate, sometimes covered with fodder tufts, outlines the habitually truncated summit.

These buildings seem to be shelters against the inclemencies of weather, and nothing more. Walton refers, somewhat perplexingly, to some ancient beliefs attributed to the megalith-builders of the Mediterranean, according to the well-known opinion of Trites (J. Hell. Stud., 1943), namely that the second opening, immediately above the entrance, may be a survival of the widespread belief that after death the spirit of the deceased passes in and out of the house or tomb in which the body rests. Such a gap is infrequent and appears peculiar to the bigger buildings, in which a lightening is needed of the load of the heavy overhanging stones weighing on the lintel. This explanation is in fact the one offered by some artificers of the stone huts, who add that the same opening can serve as, or is also intended for, a true window, easily built and useful when the entrance is closed with its door. As far as I know, no vestige of superstitious reasons or metaphysical substrata is perceptible. The erection of these provision costs clearly represents an architectural inheritance nowadays divorced from any liturgical inheritance.

Moreover, I think that the linking of these buildings to the archaic Mediterranean and continental megaliths, in the sense of an almost direct derivation, is far from established. Neither the morphological studies so far effected nor the conjecture that both the French and the Italian (Ligurian) Riviera were areas of high
The retiring Honorary Editor of MAN would like to set right, so far as that is now possible, an injustice unintentionally done by him, jointly with the Deputy Keeper of Ethnography in the British Museum, to Mr. Henry Hodges some six years ago. It may be recalled that a remarkable series of three articles on Bronze Age technology in Europe and Asia by the sculptor and technologist Mr. Leon Underwood had led to some correspondence in MAN, in the course of which Mr. Hodges questioned Mr. Underwood’s main thesis of the extensive use of the lost-wax method in the mass production of Bronze Age weapons, and incidentally his interpretation—really an obiter dictum not germane to the main theme—of what he had described as a “blade-guard” on a shaft-hole axe from Ur. In a contribution to the discussion (1958, 173), Mr. William Fagg had implied the possibility that Mr. Hodges’s interpretation of this phenomenon might be accounted for by unfamiliarity with the object; having later learned that such an implication was unfounded, he withdrew it in a note appended to 1959, 209, while maintaining his surprise at the interpretation itself, which seemed to attribute an anthropomorphic purpose to the natural forces of corrosion. To this Mr. Hodges rejoined with the following letter, received by the Honorary Editor on 30 October, 1959:

Sir,—I am sorry that Mr. Fagg finds himself unable to make a tardy apology without indulging in a Parthian shot. Since Mr. Fagg has once again raised the subject of the “blade-guard” on the axehead from Ur, I would like to make the following observations, if only to clarify the issues to the readers of MAN.

The Honorary Editor at once formed the firm intention of publishing this letter, but, wishing to append a comment to it in his other capacity, only under the obligation of making a fresh inspection of the plow of the new reapings on my memory of the original examination, a year or so earlier, with Mr. R. M. Organ, then of the Research Laboratory of the British Museum. He accordingly enquired for it on two or three occasions over a period of months, but it was by then undergoing an extended course of treatment which made its examination at that time inconvenient. It may also be that this postponement was compounded by the fact that the Honorary Editor and the Deputy Keeper were too ready to make allowances for each other’s difficulties, then and later, in meeting their many commitments.

The apology for this unconscionable delay is reserved; yet Mr. Hodges would hardly expect (in spite of the implication of his first sentence above) that such a weakness of position in point of procedure should lead to a concession of the matter at issue.

Mr. Fagg writes: “The Ur axehead has now been found, after a search for which I am most grateful to the Department of Western Asiatic Antiquities, to be still ‘in soak’ in the Research Laboratory, and I have carefully examined it.” Interpretation is now further obscured by new deposition under treatment, though the fragments of the “guard” can now be examined on both their surfaces. Mr. Hodges’s metallurgical exposition is, of course, theoretically unexceptionable, and his interpretation cannot, I think, be ruled out; I will abstain therefore from any Parthian shots (e.g. that I am now looking forward to buying the first monkey-typed edition of Shakespeare’s works). Yet both surfaces of the “guard” fragments seem much smoother and more clearly artificial than the cutting surfaces of the blade itself, and the probabilities still seem to favour Mr. Underwood’s view.


However suggestive the facts of comparative anatomy may be, however reasonable the mechanism furnished by experimental genetics may be also, the hypothesis and consequent theory of evolution must be judged as fit or frail by the record of the rocks: it is an adventure in a history whose stages lack contemporary human witness. It is all the more important, then, that any presentation of this evidence should be as clear, and as judicial in tone, as that which determines whether a man shall hold or lose his life or liberty.

Sir Wilfrid Le Gros Clark has long been known for his outstanding ability in this field. This revised and enlarged second edition of his Fossil Evidence for Human Evolution is a further witness to it. He has, more than most, brought such order into our knowledge of the subject as its present state allows, first by providing a rational classification and terminology (without which argument is impossible), secondly, by a critical revision of all the older material in the light of the new. Not the least valuable part of this book is its first 50 pages, in which he sets out clearly the methodology and logical criteria that must always be remembered and applied in this realm of our thought; because it is these that constitute the truly scientific basis of any conclusions that we arrive at ultimately. To describe the rest of this book even briefly would be impossible in the space of a review. It is enough to say that it carries us as far in
the subject as a critical mention of 'Homo habilis,' whose entry in the anthropological peerage he refuses.

As most readers will know, he recognizes a human trinity: Sapiens, Erectus and Neanderthal; neither confounding the species nor dividing the genus. *Australopithecus* is discussed thoroughly and is assigned hominid rank, generically different from *Homo*. The time has not yet come, it seems, for assigning bodies and genealogical place to the dental finds at present labelled *Telanthropus* and *Meganthropus*.

This book is subtitled 'An Introduction to the Study of Paleoanthropology.' It is the best of its kind for this purpose so far. The treatment of its concepts and the factual data conforms to those standards that a scientist should demand and it is, moreover, easy to read because of the author’s prose: would that all wrote so well. It is excellently produced and so cheap that it should find a place in every physical anthropologist’s library, be he tyro or expert.

M. A. MACCONAILL


*Genetics and Man* is a new edition of the author’s *Facts of Life* first published in 1951. It is revised and taken account of the remarkable discoveries of the past ten years. The first ten chapters are historical and give a brilliant survey of the historical development of ideas on genetics and evolution from the Greek atomists to the present synthesis of the contributions of students of evolution, of heredity, of cell structure, and most recently of molecular chemistry. It was Lamarck who invented the term 'biology' and Herbert Spencer who introduced 'evolution,' 'heredity' and 'survival of the fittest,' the latter following Malthus’s ‘struggle for existence’ and Darwin’s ‘natural selection.’ Darlington makes the interesting point that sociologists learnt their Darwinism from Herbert Spencer and not Darwin; which may account for some of the failures of communication that still exist between sociologists and biologists. He shows how difficult men find it to accept the idea of genetic determinants or genes; there was a refusal to face the assumptions of material bodies in the germ plasm acting throughout life and determining the characters of the mature plant or animal. He might have added that even today the majority of educationists will assume that change in relative intelligence scores after the age of say 11 years must be environmentally determined.

The second half of the book is very different. It contains a series of articles, stimulating but often perhaps rather extreme, on current social problems and on fundamental philosophical questions such as the existence of free will. It is a pleasure to see the emphasis on the value of twin studies in determining the relative contributions of nature and nurture; though few would accept as literally as Darlington the results of series, such as that of Lange on criminal twins, which are based on individually reported cases rather than from a complete ascertainment of all cases in a population. It is a pleasure too to see questioned the currently fashionable doctrine that all ethnic groups have the same distribution of genes for mental qualities, a doctrine which can only be true if the overall selection pressures were the same in many different societies in many different parts of the world.

C. O. CARTER


Dr. Kraus, a Professor of Anatomy and Physical Anthropology at the University of Pittsburgh, has written this book because he rightly feels that every educated man should have some understanding of human heredity and evolution. The first section is a competent but simple account of human evolution and man’s place in the animal kingdom. It is somewhat dated in that it still discusses the two alternative theories of man’s origin, that he arose later from a common ape stem, or that he arose early from a ‘pithecoïd’ stem before any of the other great apes had separated off. Dr. Kraus is unaware that modern chromosome and immunological studies have shown not only that man arose late, but that he is more closely related to the African apes than these apes are related to the Asiatic apes. The human karyotype is in fact illustrated, but is not compared with that of the other great apes, and there is no discussion of that most critical mutation in human evolution whereby the 48 chromosomes of the common ancestor, a number still found in *Pan* and *Gorilla*, were reduced to the 46 of *Homo*. Again the author’s account is somewhat dated when he writes that there is a morphological and geographical gap between *Australopithecine* and *Hominine* fossils; recent discoveries in Africa and reappraisals of older discoveries in Java are filling the gap between *Australopithecine africanus* and *Homo erectus*. The account of the mechanisms and principles of evolution is good, except that a misleading table of human mutation rates is given without the necessary warning that such tables are quite unreliable, for example, in the case of recessive genes which confer heterozygote advantage. The medical practitioner will also smile at statements such as that ‘some cases of mongolism are examples of autosomal trisomy’.

There is a good account of the fossil evidence for human evolution with an especially good section on the evolution of teeth. The account of the origin of present-day races is also good and the author is to be congratulated on his schema of their development. Too often such diagrams show the present-day races and earlier fossils as blind ends, like branches of a tree. The author uses a diagram showing a criss-cross divergence and confluence like the streams flowing to the sea in a marshy delta, so indicating that the gene pool of a modern population of *Homo sapiens* probably draws from the genes of many different populations of *Homo erectus* type.

The final chapter on the biological and cultural status of modern man shows, oddly enough, a somewhat naive environmentalism. The author suggests that there is little or no selection operating today; whereas it is known that something like 50 per cent. of zygotes do not grow to maturity, that there are marked differences in fertility between those who do reach maturity, and that a substantial proportion of both fetal loss and infertility are genetically determined, a considerable part by cytologically demonstrable chromosome imbalance. He writes ‘... in education, not in genetic change, lie the potentialities for future adaptation of the species,’ which begs the question of the many individuals who cannot be educated to take a full part in modern technically advanced cultures, and of the extent to which the distribution of genotypes in population affects the future development of its culture.

C. O. CARTER


This book, which is one of the ‘Progress of Science’ series, is well written, attractively presented, and timely in appearance. Its subject is human variability—how it has come about, how it may be measured and described, and how an understanding of it can help to explain many of the apparent differences between human groups. The author moves at a brisk pace through many of the aspects of human biology, and reviews many of the numerous contemporary scientific aids that are utilized in that study. The chapter on human pigmentation, for example, contains references to the spectrophotometer, to melainin and the melanocytes, and to the structure of the skin. The text is accompanied by excellent line drawings and plates; the proper note of caution is sounded regarding interpretations of pigmented differences between peoples.

Other chapters are equally current in their subject matter and, amongst other topics, deal with hair types, facial morphology, climatic adaptation in man, hematochemical variability, P.T.C. sensitivity and the historical aspects of blood groups and disease.

The book ends with a chapter on the eugenic implications of some recent discoveries in the fields of human genetics, and with a brief but sensible section of advice to those wishing to make a career in physical anthropology and human biology.

There is one rather misleading figure that calls for comment, however, viz. the map showing possible entry routes into North America which appears on p. 42. Whilst the legend makes it plain that
neither fossil man nor apes have been found in North America (the author is presumably dismissing the Midland, Minnesota and Tepepan finds), such creatures are figured prominently on the map itself. It is also unfortunate, perhaps, that the spectrometer head figured on p. 13 is shown as being applied to the dorsal surface of the lower arm instead of the medial aspect of the upper arm.

This is a popular book rather than an academic text, but it could be used with advantage in sixth forms, teacher training colleges, and some first-year university courses, provided that the rather sketchy bibliography were supplemented.

D. R. HUGHES


This book meets a very real need. Its author is not only a distinguished physical anthropologist but also an embryologist who has added much to our knowledge of the prenatal development of the skeleton. This first-hand competence in two branches of human biology has enabled him to do more effectively what Arthur Keith tried to do in his Human Embryology and Morphology, one of our textbooks over 40 years ago. Professor Olivier's work is in fact an account of human variations, informed by the spirit of classical morphology and presenting postnatal differences as the continuation of prenatal processes. Comparative anatomy is employed as an aid to our understanding of the human thing; but always critically, a clear distinction being made between mammalian and premammalian developmental processes where these have now been established.

The intensification of embryological studies and the experimental and functional aspects of anatomy have, between them, very nearly extinguished interest in the morphological basis of the subject: except where it enters into evolutionary theory. Nevertheless, morphology—the study of homologies—is to anatomy as mathematics is to engineering or physics; for it is the foundation of our understanding of the structural ordering of our bodies. If only for this reason alone Anatomie anthropologique is to be recommended to those who teach or study human biology. It is pleasant to read and clearly illustrated. The bibliographies at the end of each section are both full and up to date. They are not the least valuable part of the work. Not the least pleasing feature of the book is the humorous adaptation of Rodin's Thinker on the front cover, an ape looking reflectively at a human skull. When a scientist is also a man of wit we are more ready to believe what he says.

M. A. MACCONAILL


This volume, spanning the interval between the suspension of the Yearbook of Physical Anthropology in 1952 and its re-establishment in 1962, reproduces in their original form a selection of articles from the literature of the subject. These are mostly reviews which themselves lead to a considerably wider field of original publications. The editor has selected for intrinsic merit, breadth of coverage and clarity of presentation, and he has prefaced each paper with comments which succinctly review them, in many cases with the hindsight of a decade's knowledge, and which provide links binding the parts into a coherent whole.

The volume commences with 'The Strategy of Physical Anthropology' by S. L. Washburn in which is described the need for the addition to classical descriptive investigations of 'analytic strategy, with its emphasis on theory, process and experiment.' Following this introduction are a series of articles on comparative studies of Primates—anatomical, geological and sociological—leading to assessments of early man and, in particular, to the now classical work of W. J. Straus and A. J. E. Cave on 'Pathology and Posture in Neanderthal Man.' Challenging, as it did at the time, unscientific orthodox views, the paper is a measure of a recent alteration of the philosophical climate that invests the study of fossil man.' It is also interesting to have side by side in this section the reassessments, originally published in Nature, of Leakey's find known as Zinjan-
thropus by J. T. Robinson (couched in the same non-quantitative descriptive terms as those used by the original finder of the fossil), and the welcome paper by J. S. Weiner on 'The Pattern of Evolutionary Development of the Genus Homo,' which reviews, among others, the metrical technique of description used by Weiner and Campbell.

The second part of the volume consists of studies of living man dealing with race-formation and genetic changes, and it includes reviews by Bentley Glass, M. T. Newman and S. M. Garn. The article by N. A. Barnicot on 'Human Pigmentation' is of particular interest, shifting as it does the emphasis laid down by many studies from skin colour to pigmentation. The final choice is 'The Efficacy of Selection and Domestication on Man' by L. H. Snyder and this stresses a cautious attitude to eugenics based on biological theories and the importance of cultural factors.

Together with the other yearbooks, the editor hopes that the series will keep professional anthropologists, students of physical anthropology and scholars in cognate fields abreast of current discoveries and new ideas.

CHARLES E. OXNARD


During the September of 1961, the 19th Congress of Czechoslovak Anthropology was held at Mikulov. This substantial and broadly spanning monograph is the result of the meeting, and consists of 28 separate contributions. The subjects covered in this symposium include the physique of children and athletes, studies on primate crania and internal organs, the analysis of numerous early skeletal series and comparative studies on such material, paleopathology, and metric and anthroposkopical problems related to the study of living populations. This is a useful reference work, and it is to be hoped that subsequent Czech congresses will result in further works of this calibre.

DON BROTHERWELL


The output of books dealing with archaeological methods and techniques shows no signs of abating; indeed, over-production often seems a potential problem. But while it is not a bad thing to be presented with an increasing choice, the exercise of that choice becomes an increasing problem. By concentrating, obviously, upon the serious works by competent authors we largely eliminate any real problem of actual quality of content, and the problem becomes one of scope and presentation. There are several excellent books dealing in various ways with the general range of archaeological evidence: detection, classification, analysis and interpretation. A second and more recent category deals more specifically with the so-called 'scientific aids' to archaeology, meaning (at least in the general talk of archaeologists) those detectable, classificatory, analytical and interpretative methods which require some mathematical or scientific comprehension on the part of the reader if he is to understand properly their application to archaeology. The Scientist and Archaeology is, as the title implies, one of these books: it has chapters on resistivity surveying, soil science, pollen-analysis, petrological examination, thin sectioning of pottery, fluorine, nitrogen and uranium dating, radio-activity, analysis and microscopic study of metals, and the various methods of chemical analysis. Inevitably many will compare it with Science in Archaeology: both are collections in which each chapter is contributed by an appropriate authority. This comparative approach may be most useful here, therefore.

Mr. Pydowe writes in his introduction: 'It is... the object of this book to give some insight into the work which is being done for archaeology by scientists... [and to] give some idea of how special tasks are tackled and conclusions are reached' (pp. xiv).
In other words, there is no claim to comprehensive treatment, but
rather an attempt to provide instructive introductory reading by
presenting a limited number of techniques, with discussion of the
problems involved in each. Regarding presentation, Pyndoke's
authors on the whole succeed admirably in writing clearly, simply
and as non-technically as possible. Science in Archeology does
not emphasize this aspect to such a marked degree. Compare, for
example, chapter 3 of Science in Archeology, by Dr. Willis,
with chapter 7 of The Scientist and Archeology, by Mr. Barker. Both
deal primarily with radiocarbon dating. Dr. Willis's chapter is studded
with equations, Mr. Barker's is almost entirely free from them.
The former concentrates primarily upon basic problems of the
method, and laboratory methods, while the latter is concerned mainly
with interpretation by the archeologist: thus Barker emphasizes
the interpretative difficulty posed by the different radio-active ages
of growth rings within the same tree trunk, while Willis does not
mention this problem at all. Again, Willis assumes that readers
will be familiar with the statistical basis of 'standard deviations,'
while Barker explains the problem simply, and at some length.
It is true that I have used this comparison since it presents perhaps
the greatest contrast, but this does emphasize the basic difference of
approach between the two books.

Turning to the scope of the two books, Science in Archeology is of
course the more comprehensive, offering something in the nature of
a reference manual: it is not encyclopaedic, but it is designed to be as
comprehensive as possible within the limits set by the editors.
The quotation (above) from the introduction to The Scientist and
Archeology makes it clear that this is not the aim of this book.
What, then, was the basis for selection?... 'by his choice of sub-
jects, the Editor has been able to give what he hopes is a reasonably
comprehensive view of the kinds of research which are being
conducted' (Introduction, pp. xiii). Is this an adequate basis for
selection? In part it is, since an awareness of the total range of
techniques is clearly important to all archeologists. However, while
this is so, it is most important to be familiar with those techniques
which are either of most fundamental importance, or of most
frequent applicability (or both). In a book of intentionally limited
content, should this factor not be considered also? It is often difficult
to assess the relative archeological importance of different tech-
niques, and additionally difficult to balance this with the problem
here the stated intention, of covering as much as possible of the total
range of techniques. But it does not seem an impossibility, and should
certainly be worth the attempt. For example, the potassium-argon
dating method, however ambiguous its application to archeology
may be at present, does have enormous potential for the global
correlation of Basal, Lower and Middle Pleistocene sequences, and
for supplying a time scale for much of the biological evolution and
cultural development of the hominids. Compared to this, the
fluorine, nitrogen and uranium dating methods are arguably of less
overall importance (though to say this does not imply, obviously,
any criticism of these methods). Yet chapter 6 (admittedly a short
chapter), by Dr. Oakley, is devoted to these latter methods, while
the potassium-argon method is not mentioned anywhere (at least it
might have been briefly outlined in a short section of chapter 7,
on radio-activity). In terms of both the range and importance of tech-
niques, magnetometer surveying seems a curious omission: this
represents a basic method not discussed elsewhere in the book, while
current evidence suggests that it is a powerful tool to archeology as the
resistivity surveying method. The applications of statistics to
classification problems also would have been a welcome addition.
Possibly the editor felt that this was not strictly a 'scientific tech-
nique,' but most archeologists would consider it as such, and
Brothwell and Higgs evidently would agree. These problems of
selection must have exercised Mr. Pyndoke greatly, as they do all
editors, and it is hardly likely that any reviewer would be entirely
satisfied with any given selection. But it does not seem unfair to
question the basis for selection.

The criticisms offered above should not conceal that both of these
books are extremely useful, and most archeologists will want both
on their shelves. Their difference in approach, indeed, makes them
to some extent complementary; certainly they do not coincide
sufficiently for one or the other to be declared redundant. Mr.

Pyndoke's authors have had, perhaps, the more difficult task, that of
more elementary explanation. They are to be congratulated on their
overall success, which makes one rather more optimistic about the
extent to which archeologists can master sufficiently the multi-
plicity of 'scientific aids' available to them. All are agreed that no
archeologist can be completely competent in every technique
applicable to his subject; the essence of the problem is that he should
understand their application, and this is just what The Scientist and
Archeology provides for in its deliberately limited way.

B. WAILES

An Introduction to Prehistoric Archeology. By F. Hole and

Though this is a welcome addition to the bibliography of prehistoric
researchers, the book is not particularly enlightening. The
翔fessor is a very good introduction to the subject, but
the book is more of an encyclopedia of prehistoric archeology than
a book for beginners. The author assumes that the reader is
already familiar with the basic principles of archeology and
archeology, and that he is able to understand the technical terms
used in the book.

The book is divided into two parts: the first part deals with the
time period of prehistoric archeology, and the second part deals
with the various methods and techniques used in prehistoric
archaeology. The author has done an excellent job of
explaining the various methods and techniques used in prehistoric
archaeology, and he has provided many examples from the
literature to illustrate his point.

One of the main criticisms of the book is that it is too
technical for the average reader. The author assumes that the
reader is already familiar with the basic principles of archeology
and archeology, and that he is able to understand the technical
terms used in the book.

Despite this criticism, the book is an excellent introduction to
prehistoric archeology, and it is highly recommended for
beginners in the field.
suppose that the four towers of a Roman fortress were put at the corners simply to provide for vision and defence each one in two directions rather than that they have any reference to ‘the four pillars of heaven.’ The drill and heath of wooden fire-making implements are certainly spoken of as male and female, but no one would postulate a ritual origin for male and female screws; and there is a good deal of evidence which makes it far from improbable that fire-making with two pieces of wood was accidentally discovered by observing boughs in friction in the wind. As for the statement that ‘no peasant, backwoodsman or savage ever started anything new’ (p. 7), it is demonstrably fallacious.

The Neolithic Revolution is some archaeologist’s ghastly misnomer for a process of change that must have taken millennia; rituals must have grown and developed as slowly as the discovery that seeds could be sown as well as collected. Man has ever tended to make gods after his own image, and Lord Raglan has offered no explanation of why men should have supposed that their gods needed beds or buildings until they had already found them desirable for themselves. In the absence of such an explanation his main thesis falls to the ground. This however is far from destroying the value of this original and provocative work, which emphasizes the loss that all anthropologists suffer by the death of an erudite, lively, and very independent thinker.

J. H. HUTTON


Subtitled ‘A Cultural Geography,’ this book forms an important landmark in the development of our view of the relationship between human culture and the physical and biological environment in which it is set.

Professor Carter has a central purpose, which is to deny categorically that the particular form taken by a culture is determined, or even closely directed, by the nature of its environment—still less by the racial affinities of the people concerned. He shows, with numerous examples, that the cultural preferences, traditions, and contacts of the inhabitants are far more potent determinants of the way in which they use, exploit, improve or degrade an environment than any natural factors. In this he is greatly in advance of many human geographers, who confine themselves almost entirely to the plane of the present day and scarcely at all take into account the third dimension of depth and solidity which is afforded by a recognition of time and history as environmental factors. If there is a ‘Geography behind History,’ there is equally a History behind Geography, which any environmentalist ignores at his peril.

In order to study at work the formative factors of culture and man’s influence on environment, Professor Carter first hypothesizes a uniform Earth, with racially uniform inhabitants, existing under uniform natural environmental conditions, with uniform traditions, and shows that any variation in cultural pattern, such as a simple technical invention (certain to arise somewhere, some time, man being what he is), will shortly diversify culture and land-use and so locally affect even the natural environment.

He then proceeds, in turn, to study examples of cultures in arid lands, the wet tropics, the Mediterranean climate, mid-latitude forest lands both of east and west coasts, grasslands, mountain lands, northern forest lands and the polar environment, showing that man’s degree of successful exploitation of these, at all times, has been due rather to his access to a rich cultural tradition than to the direct effect of any favourable or unfavourable natural environmental factor. He disposes, once and for all, of any idea of racial superiority or inferiority attributable to any human group, either today or in the past.

This book is full of ideas, as well as of facts—many of them, in my experience at any rate, original. It should be read and digested by every student of man, whatever his specialty. Very few will do so without much profit to themselves.

I. W. CORNWALL


The author, as the Director of the International Research Council, is well equipped for his present task and his work does valuable service in drawing attention to the many uses of tin from the earliest time up to the present day.

Tin is a metal which by itself is hardly ever used, but is capable of improving other metals. Its distinguishing characteristics are its high resistance to corrosion, its plasticity and its low melting point. It was one of the earliest metals known. The decisive advance in civilization came with the discovery of tin as an important ingredient of copper for the making of bronze.

The first chapter is concerned with the Bronze Age, early trade routes, tin mining in Malaysia and Cornwall, the English tin trade in the Middle Ages and its development. Chapter 2 deals with the international control of the trade, gives interesting figures of consumption and distribution of tin and tells us about the aims of the various trade groups. Chapter 3 gives us an insight on scientific and
technological research and its influence on the economics of the industry. The next few chapters deal with a wide variety of subjects such as coinage and counterfeiting. Roman pewter, Pewterers' Guilds in the Middle Ages, ceremonial observances in China, burial customs and the remarkable sarcophagi of the Hapsburgs in Vienna. In all these tin played its part, as well as in the decorative arts and in music. The last three chapters tell us about the use of tin in industry as in bearings and in the manufacture of tin plate, where the use of tin is essential because of its resistance to corrosion. In telecommunications, agriculture and for medical uses tin plays its part, and the book ends by describing research that is being carried out on new uses for tin in the future.

The author uses the archaeological evidence selectively and makes full use of documentary sources which are quoted at some length. The references given in the text are scanty but adequate, and a bibliography at the end would have been helpful. There is a good index.

The book is well produced and the illustrations are excellent. The author is to be congratulated on the original approach of his treatment of a single metal in the setting of the framework of social history. The work is highly informative and stimulating and there is no doubt that it will appeal to readers of MAN.

R. F. TYLECOTE


This remarkably comprehensive presentation of social anthropology as the subject is conceived and taught in British universities has already achieved a wide and much deserved popularity. At the moment there is certainly no better survey and assessment of the subject as a whole, nor is there likely to be one for a long time to come. Indeed Dr. Beattie has contrived the enviable feat of writing what is clearly destined to become a standard textbook indispensable to all students of social anthropology and at the same time of equal interest to their teachers. More than this, all those working in the social sciences generally and in other cognate fields could not expect to find a more thorough and satisfactory guide to what social anthropologists set out to do and what they have actually done. This is a solid achievement.

The book is admirably planned, lucidly and succinctly written, and has an elegance of presentation and style which is most welcome. It falls neatly into two parts. The first locates social anthropology in its proper historical and intellectual context, discusses the concepts which anthropologists use and abuse, and examines how these are applied in fieldwork practice and in theoretical analysis to the two main foci of interest—social relations and structure, and beliefs and values. This methodological approach is then illustrated in a rigorous and neatly economical exposition of such crucial areas of interest as: kinship, marriage and affinity, social control, economics, ritual, and social change. Throughout this discussion, the use of concepts and principles is pointed up by a judicious selection of ethnographical examples which are all from well attested ethnographical material. A special merit here is the way in which the same ethnographical examples have been used to illustrate different aspects of culture and social organization. This careful use of a relatively small selection of sources avoids the bewildering impression so often produced by a miscellany of references to disparate peoples, and at the same time gives the reader a more rounded picture of the communities referred to than is usual in works of this kind. This method of exposition has perhaps also contributed to the unusually high degree of accuracy and respect for source material which is maintained throughout.

What is particularly interesting, I think, at this stage in its development is the degree of consensus existing in the way in which the subject is conceived and taught in this country, which the book reveals. Partly, of course, this impression is conveyed because, no doubt wisely, Dr. Beattie has avoided some of the most controversial issues of current theoretical discussion. One aspect of this is perhaps that the book has few startlingly new things to say; but, and this is important, some of the ways in which Dr. Beattie phrases and expounds established knowledge are unusually succinct and thought-provoking. To my mind the most exciting passages occur, as we should expect, in those sections of the book which deal with types of explanation, methodology, and the study of symbolic systems and values. Here the author's exposition, none the worse for its frequent reiteration, of the common fallacies to which social anthropologists are prone is particularly valuable. My one unfavourable comment is that the penultimate chapter on social change is, on the whole, rather conservative and disappointing in its approach and range of subject matter. But this a small criticism to register of a wholly excellent work for which all social anthropologists should be grateful.

I. M. LEWIS


Does the appearance of two new American editions of Ancient Society over the past two years herald a deepening interest in social evolution or are these but vAGRANT sparks from the Darwin Centennial flareup? Probably the latter, for anthropology, now stolidly pedantic, has little taste for any kind of synthesis, let alone the evolutionary kind.

Yet were it now possible for an ancestral figure to lead anthropology onto the higher ground of synthesis, that leadership, said to say, would probably not fall to Morgan. Commanding as the core of his work may be, his reputation as a whole has suffered badly from the rigidity and ineptness of his theory of stages, and, even worse, from the spotness of his scholarship. Modified versions of this theory occupy the textbooks, but they lead nowhere. Morgan's contributions to kinship studies are universally recognized, but except for his interesting explanation of how matrilineality was replaced by patrilineal descent, his history of kinship, marriage, the family and descent has not survived subsequent research. Lowie credited Morgan with the basic discovery of the derivation of kinship terminologies from rules of marriage and descent, but even this still popular view will not bear close scrutiny. Morgan's most serious fault, however, and the one that really helped undermine the credibility of evolutionary theorizing was, of course, his blindness to contradictory evidence. Even White, the loyal disciple, is chagrined at Morgan's Polynesian blindness. If Morgan was not really aware of agriculture in Polynesia even though he had made painstaking inquiries from authorities in the field about kinship and family, we can only conclude that he did not take his economic stages seriously at all.

On the other hand, White—who has, naturally, tended to recreate Morgan in his own image—considers him important mainly as materialist and as ecological and technological determinist. Morgan was indeed a God-fearing materialist and he did consider ecological and technological factors. But the true centre of Morgan's interests and the core of his work that is still vital were in jurisprudence, in the laws of marriage, inheritance, descent, and in the emerging conflicts between kinship and territorial principles of authority. Above all he was interested in property relations, property laws and their pivotal role in social structure. His measure of progress was not mere technique but democracy. On these problems analysed in the concrete historical contexts of specific societies—Iraqqu, Aztec, Greek and Roman—what Morgan has to say is still fresh and alive. Morgan's consistent historical perspective gives to his work certain qualities that are unfortunately missing in current sociological studies based on equilibrium models—qualities of realism and of humanness.

IRVING GOLDMAN


Anthropology in the United States is afflicted with a schizoid personality—at one time calling itself Lewis Morgan and looking for universal laws of social development, at another believing itself to be Franz Boas, and absorbed in the minutiae of particular cultures. According to Professor Wolf, it is the spirit of Morgan, the founding father of native American
anthropology, that is now once more taking possession: as against the cultural relativism exemplified by the work of Ruth Benedict, the emphasis is again, as it was a century ago, on a search for general statements about all men.

Not all American anthropologists, one imagines,—not to mention their colleagues on this side of the Atlantic,—will feel able to subscribe to the 'new evolutionism': reading the signs offered by a collection of recently published anthropological works is to some degree an exercise in haruspication, and other and more complex interpretations are no doubt possible. A singular merit of Professor Wolf's synoptic approach, however, is that it emphasizes the role of historical factors in shaping anthropological preoccupations: the celebrated division between 'society' as the object of study in Britain and France and the concern with 'culture' in America issues in Professor Wolf's view from two markedly different social ideologies—the class-conscious Europeans producing models of opposed sub-groups within societies, the Americans emphasizing what all members of given 'cultures' have in common. (With the increasing stratification of American society, according to Professor Wolf, certain works of European social anthropology are acquiring a new and real significance in the U.S.) Finally it is the present world situation—pregnant, it would seem, with a unified culture and society embracing all humanity—that challenges anthropology to produce an integrated, universal theory to match the predicament (by Professor Wolf) holistic process. The author succeeds very well in communicating the somewhat breathless sense of urgency generated by his own vision.

R. G. WILLIS


One can now recognize three stages, though not, of course, sharply divided periods, in synchronic linguistics: the traditional stage, based on the classical model of Greek and Latin grammar, the structural stage, inaugurated by de Saussure and brought to its fullest development by Bloomfield and his successors in the United States, and the transformational-generative stage, initiated by Chomsky in 1957 by the publication of his Syntactic Structures.

This last development has been hailed by many, especially in the United States, as a definite advance from the Bloomfieldian position, and a number of illustrative grammatical statements and theoretical discussions have been published since 1957, but the book under review is the first explicit textbook on transformational-generative grammar to appear, and it is, for this reason, both welcome and important.

It contains step-by-step expositions, with examples from many languages, both natural and artificial, and the chapters are followed by problems and exercises. (The addition of solutions and discussions of these would have enhanced the value of the book for work with students.)

A transformational grammatical involves two specific aspects: as a generative grammar it is cast in the form of rules for generating (productive) sentences in a language, as contrasted with the inventory or listing of recurrent constants that characterized earlier Bloomfield-inspired descriptive linguistics; and an essential component is a set of transformational rules (hence the double-barreled title for this mode of grammatical statement). Specifically grammatical rules are of two types: phrase-structure rules generating sequences ('strings') of elements in structural trees, like the earlier immediate constituent analyses, and transformation rules converting one such string into another (active to passive, affirmative to interrogative, etc.): an important class of transformations is that of generalized transformations, which combine more than one string into a single string, as in coordination, subordination, nominalization, etc. It is a pity that Bach does not devote more space to these than he does (pp. 47, 73f.).

It is claimed that relations between sentences that are intuitively the base speakers, degrees of acceptability, and structural ambiguities are accounted for, and that syntax and morphology (as traditionally distinguished), lexicography, and phonology are all integrated into one (ideally) completely explicit account of the infinite possibilities of sentence generation in a living language. Word boundaries, with consequent graphic spaces and any phonetic juncture features, are generated at an appropriate stage in the rules, the lexicon is introduced in the form of lists (often unbound) of bases into which symbols such as N(oun) and V(erb) may be converted, and a final set of phonological (morphophonemic) rules converts the output of the phrase-structure and transformation rules into phonetic transcription, or utterances. These last may bypass the earlier independent phonemic transcriptions by which a set of symbols uniquely represented the utterance independently of its grammatical structure (pp. 126–32): as an example (not in Bach), the distinct representation, at a level higher than the final narrow phonetic transcription, of English noun plural suffixes by [-z], [-s], and [-z] (horses, cats, dogs), is unnecessary given the grammatical information 'noun plural,' and is not observed in the orthography.

Bach's exposition is, on the whole, clear and explicit, though fairly strenuous. He presupposes a general acquaintance with the methods and terminology of Bloomfieldian descriptive linguistics (p. vi). The chapter on the application of some modern logical and mathematical methods to linguistics (chapter 7), a subject attracting the increasing attention of some specialists, is much harder, and despite Bach's disclaimer of any prior knowledge of this field on the part of the reader, it is not entirely clear to someone like myself who is largely ignorant of modern mathematical methods. One may doubt, too, whether the bulk of this chapter is really necessary to the scope of an otherwise elementary and introductory book.

Throughout the book a good many questions are left open, and methods are sometimes put forward only tentatively. This is inevitable at the present time, as transformational-generative grammar, though firmly established in essentials, is in a period of rapid development, with many early viewpoints being challenged and modified. Any concealment of this, even in a textbook, would misrepresent the current linguistic situation.

R. H. ROBINS


In some ways, this is a most scholarly work. The breadth of the author's reading is indeed vast, including many English-language works as well as Russian, German, French and Swedish, to give a few examples and a by no means exhaustive list. From his reading, the author quotes extensively, often verbatim, and it is this that has led to one of the work's weakest features, namely the diffuse nature of the discussion with views differing only trivially given at length, quoted from several authors, so that the argument is not clear cut and incisive. Diffusion also derives from the author's undue regard for the writings of bygone demographers. In an appropriate context, it is very right and proper that we should recognize how great were learned men of the past, judging their writings in the light of the very limited information available in their day. But this is quite a different thing from regarding their sayings as absolutely sound, and the path to the most advanced views of today via all the misconceptions of the past is indeed winding and confusing. In this work such paths were inappropriate, and clear indication to the reader of the various facets of the subject could more easily have been given with much of this superfluous and dead wood cut away.

In view of the erudition shown by the author, it is not surprising that he shows only limited ability in the mathematical and statistical fields. The result of this is that, where Professor Rosset's argument requires a calculation to elucidate the point, unless a suitable quotation from an existing publication can be found, the point is baldly stated without either proof or elucidation and in some cases one is left in some doubt whether the point is really valid or not. For instance (p. 230) Hertzel's conclusion is quoted that, by reducing their fertility, 'people can ensure the well-being of themselves and their offspring.' The author's comment (based on the work of Srb) is that, on closer examination this conclusion does not hold water.
His argument rests upon the statement that the few children born to the generation limiting its fertility will, when they grow up, have to bear a disproportionate burden of old people. It is of course true that the ‘old-age dependency burden’ of the children will be greater, but the ‘young-age dependency burden’ will be correspondingly less so that, in total, the dependency burden will be barely changed at all. (And such change as there is is likely to be favourable as unfavourable, but in either case will change by only about one per cent.) Now these matters are capable of calculation, indeed in this particular case they have been examined in great detail in a United Nations study (and one quoted extensively by Professor Rosset), Population Study No. 25.

Incidentally, in this particular case, the full argument is really somewhat involved. From what has been said above, it might appear that Professor Rosset is wrong because the proportion of the population who are dependent is unchanged, not raised as would be required for him to be right. But it would appear that Hertzler is wrong too, since a rise in living standards would seem to require a fall in the proportion of the population who are dependent. The full argument is that a fall in fertility does not raise the dependency ratio, and it brings an advantage by lowering the proportion of children. This is because it becomes economically possible to raise educational standards and economically advantageous to do so since the resulting labour force becomes more sophisticated and able to achieve higher per capita output.

Again the author makes the obvious remark that the movement of predominantly young migrants to a country with a lower proportion of young people will lead to an increase in the proportion of young people in the receiving country. But he does not make the important test of the extent to which this proportion is increased. Frequently the change will be quite trivial and, apart from satisfying oneself on this point, the matter is then of little importance.

Whilst it must be pointed out that the level of mathematical and statistical argument is far below the standard of the literary parts of the work, it would be quite unjust to suggest that all the statistical and logical arguments are faulty. On the contrary, after (unduly lengthy) reference to the views of many other demographers, the author frequently puts his finger on the vital aspect of the situation and sums up the previous arguments precisely. This leads us to the main body of the work which is in reality a vast historical account of the main changes in populations (with particular regard to ‘aging’), restricted by necessity to countries for which there are data, but including less-known data relating to the remote past in cases where there are such data available. As a mine of such information, and the sources where it may be found, for those desirous of undertaking their own analyses of these data, this work must be quite unique as far as data relating to the remote past are concerned. (The main sources of data for more recent years—the League of Nations and United Nations publications—are so well known as to need duplication.)

The English of the translation (from the Polish) as such is impeccable, but, perhaps biased as a demographer, I was most disconcerted by the translation of the technical demographic terms, ‘Demographic Forecast’ for ‘Projection’ (although the projections of the Royal Commission on Population are correctly so named), and ‘coefficient’ and ‘rate’ indiscriminately used for ‘rate’ (as in ‘death rate’ and ‘gross reproduction rate’). The unsatisfactory nature of this feature underlines the value of the United Nations Multilingual Demographic Dictionary (not available in Polish) in which demographic technical terms, used in their correct contexts, are shown in various languages, the actual technical terms being given corresponding reference numbers in the various versions.

N. H. CARRIER


Conceptual advances in the history of psychology have mainly been made on an edifice of already existing philosophical standpoints. It was always appreciated that Freud’s emphasis on unconscious factors depended not solely on inferences from his own clinical observations, but also on the matrix of philosophical speculations in the nineteenth century made by people like Schopenhauer, Schelling and von Hartmann. Mr. Whyte extends our appreciation of earlier formulations about unconscious factors which influence our conscious behaviour, by exhibiting a variety of thinkers who had such notions. They are revealed to be a very mixed bag. We have poets—Goethe, Schiller and Dante—; physicians—Paracelsus, Carus (who shares with C. G. Jung both his initials and very similar ideas), W. B. Carpenter (who is responsible for the coinage of the term ‘unconscious cerebration’); essayists—Montaigne and de Quincey; and mystics like Jakob Boehme, as well as more professional philosophers. It is true that there is a wide interpretation of the concept of the unconscious by these and others. As Mr. Whyte shows it was regarded by the mystics as the link with God, by the Romantics as the link between individual and universal powers, and by the early rationalists as a ‘factor’ operating in the realm of memory, perception and ideas. It was not until the days of the psychoanalyst proper that a local geography is depicted with the bizarre scenery of complexes, archetypes and Latinized strata of the mind.

Mr. Whyte’s survey begins in A.D. 1600 although authorities previous to that date are mentioned. But it was about this time that gropings towards a formal idea of the unconscious began in earnest, and the reader is presented with numerous quotations from the seventeenth, eighteenth and nineteenth centuries to enable him to see and trace the origins of a concept which had such enormous impact in the early twentieth century. Particularly one is able to mark how the concept of ‘The Unconscious’ operated in observations or philosophical frameworks. There might have been more background of the epistemology and sociology of the times given in this book; indeed Mr. Whyte has paved the way for a more exhaustive enquiry. His style is somewhat jerky and the interpreters are presented in a rather hurried and spasmodic manner; nevertheless the presentation is far more useful and informative than lengthy discursive essays which analytical philosophers give us. Even if The Unconscious turns out to be a nominalistic fallacy, it is interesting to note that so many students of human nature have taken notice of it.

D. R. PRICE-WILLIAMS


The 15 short papers in this book mark the twenty-fifth anniversary of the publication of J. D. Bernal’s Social Function of Science. The contributors are eminent scientists and scientific administrators.

It seems to me that the authors have not tried to relate to a specific theme, or, except in a very broad sense, to illuminate aspects of a common problem. They are certainly united in admiration for Bernal and his work, and in their conviction of the social significance of science.

C. P. Snow’s personal note on Bernal is a nice mixture of anecdote and analysis of the personal qualities and influence of Bernal. E. H. S. Burrow traces the growth of social and political consciousness amongst scientists. There are three papers, one by Blackett, on the widening gap between the rich and poor countries, each emphasizing the role of applied science in economic development, a neglected field before Bernal’s Social Function.

A lively note by Haldane shows how the science of genetics is dispelling human superstition, and looks forward to a distant day when improved knowledge of genetics and social structure will make possible the improvement of society through control over human evolution.

Coblands and King take up, elaborate, and bring factually up to date Bernal’s earlier argument that archaic methods of scientific communication increasingly lead to waste and inefficiency. Pirie lists and discusses the current priorities for science; to feed the world, to control its population, and to conquer disease. He believes that scientists, given their head and the cash, are responsible enough to choose the right priorities. Syngue looks forward to a time when science has created relief from the grind of toil, and foresees leisure being used to satisfy the scientific curiosity of everyman.
Peter Kapitsa's optimism is striking. He recognizes no limits at all to the discoveries of science. His essay ends on an oddly naive note of condemnation of all social science for its failure to recognize the universal validity of Marxist laws. Korach's paper entitled 'The Science of Industry' I have not understood well enough to convey briefly its import.

My bias as a sociologist is probably responsible for my belief that Joseph Needham's essay is the most interesting and the most scholarly of the lot. Why, he asks, was early Chinese society so much more efficient at applying human natural knowledge to practical human needs than occidental society of the same period? And why did modern science nevertheless 'take off' in Europe? He dismisses hypotheses of chance and racial superiority and argues for analyses relating social, intellectual and economic structures. He demonstrates his point elegantly by reference to the influence of bureaucratic China's feudalism on scientific development.

Price, in the penultimate piece, seems to see emerging from the work of social scientists, historians and philosophers of science, a coherent new discipline, a science of science. This new discipline uses the methods of science to explain, among other things, the growth of science, and the motives and activities of scientists.

Bernal himself, with characteristic clarity and humility (I am also an admirer of Bernal), rounds it all off. There are, he says, still problems in the relationships of scientists, their patrons, and the generality of whom science benefits. He believes, however, that the situation has improved enormously and goes on improving. Science will increasingly be seen as the guarantee of a better future.

This has been a difficult book to describe because of its diversity of subject matter and patchiness of treatment. Its overwhelming message is one of realistic optimism. There is no blinking of the problems facing science in the modern world, yet this is accompanied in every essay by the belief that the scientific study of the uses of science will in the future lead to their solution. One hopes that the optimism will be justified by events.


It is a handicap to have to present only the first volume of a series of three. Perhaps the reader may be helped if he is told that the second volume is to be called 'The Negative Affects' (dealing with emotions like anguish, humiliation, self-contempt and disgust), and the third volume deals with cognition and ideology. Professor Tomkins takes a position versus both behaviouristic psychology and psycho-analysis. A novel approach back to the idea of consciousness is offered. This is done by conceiving consciousness as a "transmuting" process operated by a centrally innervated feedback mechanism. What is called 'The Image,' a term indicating a purposeful condensation of ordinary imagery operating in sensory and memory processes, is regarded as a key element in this transmuting process. If this sounds somewhat vague to the reader, let it be said that the first volume gives detailed description of the various concepts that are introduced. As the sub-title suggests, particular attention is paid to the part emotions play in the organism. What is perhaps of advantage to the student who is not at home with the language of biochemistry or neuro-physiology, is that the everyday and familiar world of excitement, joy, surprise, does not disintegrate into the symbols of stimulus-response theory. Whatever may be the final verdict on the underlying theory which is presented, one feels that new light is thrown on areas which are under-played in contemporary psychology. The social scientist in particular may find some interest in the chapter entitled 'The dynamics of enjoyment—Joy: The social bond.'

D. R. PRICE-WILLIAMS


The events herein related, says the jacket, are 'true stories gleaned from historical accounts' and 'should be of tremendous interest to those who have not read . . . how the proud civilized Cherokee were banished from their homeland.' Exactly so. The published sources are reliable, if occasionally misinterpreted (as when the Cherokee are called a Muskogean tribe, although their language is later given correctly as Iroquoian), and the author's moral structures amply justified. The make-up of the book, however, is somewhat repetitive and the text peppered with minor misprints, to one of which I must ascribe the fake information that our Cherokee visitors of 1790 were 'lavishly entertained at King George's place at Whitehall.' But if Mr. Peithmann's prose is sometimes less than expert, his poetic "envo" burns with the true flame of The Great McGonagall.

This then is not, nor I think is intended to be, a book for anthropologists. But to read it is to be reminded that there is room for an objective assessment of the role of mixed-bloods in American Indian acculturation. For this there must be ready store of case histories among the Five Civilized Tribes, and especially perhaps the Cherokee, with their rapid advance to vernacular literacy and constitutional government under such mixed families as the Rosses and the Ridges—and indeed the great Sequoyah himself, one-eighth Indian by blood if wholly so by upbringing.

GEOFFREY TURNER


This monograph defines the cultural sequence of the Piedmont area of North Carolina from approximately 8000 B.C. to eighteenth-century contact times, with primary emphasis on the pre-ceramic Archaic levels prior to 500 B.C. Data from three deeply stratified sites are presented in detail, supplemented by information from a fourth.

While the content of the various Archaic complexes is in itself a major contribution, this report has implications far beyond its geographical scope. Fully to appreciate the significance of Coe's contribution one needs to have experienced the frustration of working with the thin Archaic sites of the eastern United States, where stratigraphy is seldom clear and cultural mixture virtually inevitable. Coe convincingly demonstrates that if one knows where to look sites may be found where manifestations are separable and sequences may be empirically defined.

Such sites are found in the narrow valleys where the rivers cross the fall line, behind projecting rock formations which are the locations of large eddies during flood. Here sand and silt build up at a faster rate than elsewhere in the narrow valleys, sealing in the occupational levels. When such short-term occupations are discovered, Coe notes that 'the usual hodgepodge of projectile point types are not found—only variations of one specific theme.'

The latter is his second major contribution. It forces us to question sites that we might otherwise regard as culturally homogeneous; to beware of complexes in which 'the variance of hunting and fishing implements . . . rivals the selection of sporting equipment at Abercrombie and Fitch.'

JOHN W. GRIFFIN


This book is another addition to the 'Case Studies in Cultural Anthropology' series edited by George and Louise Spindler, and consists of a brief description of certain features of the culture of the Mexican-Americans who live in Hidalgo County (southern Texas) and of their relations with the Anglo-Americans. The author deals with such cultural aspects of Mexican-American life as the family, religion, sickness, curers, folk psychotherapy,
education, and prohibited and prescribed modes of behaviour. Particularly intriguing is the account of the way in which Catholicism and folk superstitions (e.g. the belief in witches) have blended.

The interesting descriptions of the relations between the Mexican-Americans and the Anglo-Americans constitute the most important parts of the book. The larger towns containing Latin-Americans and Anglo-Americans are divided into the Latin half and the Anglo half, and are usually separated by some physical boundary such as a railway line. Today, tensions between the two groups are much less intense than they used to be. Marriages between members of the two cultures are common, but marriage with an 'Anglo' often results in social and psychological difficulties for the Latin partner. These are fairly thoroughly described.

There is a bibliography of 125 items, a map that is only barely adequate (no key and no scale), six clear photographs, but no index.

This is a simple, yet competent account of one of the folk societies of North America, and is one that the first-year anthropologist or sociological undergraduate will find interesting and useful.

DAVID HICKS


This book is a most painstaking and thorough study of the pictorial symbolism of Mexican painting in the best group of the surviving religious codices. There are occasional references away from the Borgia group of Codices to sculpture, and to such later works as Codex Borbonicus, but in the main it confines itself to Borgia, Vaticanus B, Cospiano (Bologna in this book), Fejervary—Mayer and Laud.

The need for such a book has been obvious for half a century, but most of us had to be content with partial comparisons of this nature made for special papers. Now the work will serve as an excellent medium for beginners in the study of the codices. It is a pity that there is no possibility of a full colour edition, but in spite of the useful colour guide on p. 13, it would repay any researcher with sufficient patience to fill in the 1783 diagrams with colour.

Each section deals with the representation of one of the gods. They are arranged in the alphabetical order of their names. A short and very clear description of each deity is followed by an exhaustive concordance of the ornaments and face paint worn by the images in the codex. There are also other sections which deal with the pictorial grouping of the figures, their arrangement within the codices, and the basic iconography of the full-figure images.

Amidst all the confusion of the post-conquest informants the author seems to have picked a reasonably clear way. There may be a few corrections to be made later, but the directional linkages of some of the gods, but meanwhile this is a handbook of importance which will make the path of all future students much clearer.

C. A. BURLAND


This book deals with the morality system of the Mapuche (Araucanian) Indians, who live on scattered reservations in south-central Chile. They believe that ancestral spirits, if suitably propitiated, become hawks of the sun or other winged creatures of good omen.

The author begins with an account of Mapuche social organization, which is broadly patrilineal and patrilocally by reservations. Particular emphasis is placed on patrilineages and marriage arrangements which frequently call for a man to take a spouse from his mother's natal patrilineage. Thus an interchange is set up between a wife-receiving unit and a wife-giving group.

The two most important occasions for the propitiation of ancestral souls are the burial ceremonies (ausm) and the multifaceted fertility rites, tillatun. A tillatun may be led by a reservation chief, whom the author calls a 'ritual priest.' Participants and onlookers usually come from a number of nearby reservations. This assemblage Faron terms a 'ritual congregation,' and he finds that it corresponds fairly well to the total group within which intermarriages are customarily arranged.

There is an interesting discussion of handedness in relation to standards of morality. All good things are generally associated with the right hand or side, and whatever the Mapuche regard as worthless, inferior, or evil, tend to be linked with the left.

Faron is an ardent structural-functionalist, interested primarily in making synchronic analyses. In fact, he takes umbrage at the efforts of those who may prefer to take a diachronic approach. Despite the rather contentious tone, Hawks of the Sun makes a fresh analysis of the socio-religious activities of the Mapuche, particularly during the reservation period that lasted officially from 1884 to 1960.

MISCHA TITIEV


This is another of the growing list of descriptive works on Latin America. The author offers a geographically oriented description of what he calls the 'cultural landscape' of the community (defined as 'That composite of features visibly on the earth's surface which gives evidence of man's occupancy' (p. iii)), focused by his interest in economic underdevelopment and culture change.

The first chapter contains a general description of the physical makeup of the community with brief indication of the demographic composition, and in rural clothing, food and housing differences between the poor Indian and the less poor ladino.

Chapter II is devoted to man-land relationships in which the author describes the agricultural cycles of maize and sugar cane, as practiced in the near Villa las Rosas. He includes estimates of the cost of production and profit of these main crops.

The final two chapters are devoted to a description of changes in landscape used for crops, cattle and residential purposes. The changes are associated with recent construction of a new highway which has modified the relationships of the community to the outside world, resulting in the need for specialization.

From my point of view there are several shortcomings in the book. First, the problem of the ejido in Mexico, vital in understanding Mexican agriculture, is glossed over lightly, and secondly, the problem of deforestation due to charcoal-manufacture is not even mentioned. The limited treatment of these subjects in a description of a Mexican community is unfortunate as they are, in my opinion, among the most important factors to be considered in any prediction of land-use change, and therefore landscape change, in Mexico.

D. E. W. HOLDEN


In her foreword Dr. Jacqueline de Durand-Forest gives a general account of the work and its history. She stresses its importance as the most valuable source book for the Mexican language. The volume itself is a reproduction of the now rare edition of 1885. It has an introduction giving an outline of the grammar of the language, and then an immense vocabulary of about 25,000 words. The language is related to the Siouan group of North American languages, but has developed in centuries of isolation a character of its own which was conditioned first by the high level of civilization in Mexico and later by its use as the lingua franca of an empire which included peoples speaking many other languages of quite distinct types. Thus Nahua became a cultured language which easily developed into a literary language on the introduction of alphabetic writing in the early sixteenth century.

The book is an important source book for the linguist, but also contains many references which are of great value to the ethnologist.

This is one of the series of reproductions of rare and important scientific texts now appearing from the University of Graz. It is an important contribution to Mexicanist studies, and a welcome reappearance of a work which was in process of disappearing from the available sources of study material.

C. A. BURLAND

This 1959–60 surface survey, undertaken at the request of the Colonial Office, proved the physical and political difficulties of travel in many areas. The Western Protectorate consists of a coastal strip, a higher plain and the great scarp leading to the high, level tableland. Here sites are few and far between, the majority being medieval, ranging from the fifth century B.C. to the third-fourth centuries A.D. The most important sites lie in the Wadi Belhan (Baitani): (a) Hajar Kohlan formerly Timna, ancient capital of Qataban, where 1950–1951 excavations by the American Foundation for the Study of Man found levels of the eighth or ninth century B.C. according to C14 analysis; and (b) Karish (Am Adia), north-east of Makkara, at 7,000 feet, where a mass of tumbled stones included several courses of dressed local granite blocks. Inscriptions assign this site to the first or second century A.D.

The Eastern Protectorate is divided into a coastal strip, a high plateau and the great gorges of the Wadi Duan with its ancient irrigation complex, the Wadi Jirdan and the great Wadi Hadhramaut. Here are many sites, including Harraylah, Shabwah (claimed by Yemen) and Masghah. Naqb al Hajar in the Wadi Ma‘afah of the Wahidi Sultanates is a large walled town and Husn al Ghurab, the ancient port of Qana (Cane).

After describing the principal archaeological sites, Harding refers to three palaeolithic and one neolithie stations published by Dr. Gertrude Caton-Thompson as well as flint implements from Haburat (17° 15′ N. and 46° 20′ E.) on the Oman Border and from the southern fringe of the Rub‘ al Khali in Saudi Arabia.

The large number of graffiti from Yathut, Al Qurn al Haid, Makkara and the Wadi al Agabah depict the camel, ibex, human figures and three curious signs. Beeston (pp. 52–9) writes that these graffiti from the first two locations are ‘Thamudic,’ those from Makkara partly in normal Epigraphic South Arabian script, partly of a Thamudic type.

The absence of a site map is unfortunate despite ‘the unreliability of available maps.’ The index is inadequate for such a little-known region. A detailed subject index and selected bibliography would have been most helpful. The 56 excellent plates and text figures show the principal sites, sherds and pottery profiles, flint implements, texts and graffiti. No. 19 is missing from Plate XXXIV.

This monograph will not only prove extremely useful as a reference work but should act as a catalyst to encourage further research and to establish the newly proposed Aden Museum along the lines suggested. The present Museum, located far from the harbour, contains a quantity of archaeological material. The Government of Aden has acquired recently the Munchevich Collection of South Arabian antiquities with many unique pieces—all without provenance.

The Aden Protectorates form the geographical link between the Arabian Peninsula and the Horn of Africa. For this reason further archeological research here and on Socotra should receive early attention.

HENRY FIELD


This book forms part of the series which began 50 years ago with J. C. Stobart on Greece and Rome and continued with M. A. Murray on Egypt and A. L. Basham on India. It deals with the civilization not only of Babylonia (including Sumer and Akkad), but also of Assyria.

There are two main parts dealing with history and culture respectively, a good 25-page classified bibliography (no individual source notes), a six-page chronological chart (dates follow the new Cambridge Ancient History), and good indices.

The history provides a chronological framework from the Palaeolithic to the end of the Neo-Babylonian period (sixth century B.C.) in 150 pages. The second part has nine chapters on ‘Society,’ ‘Law and Statecraft,’ ‘Administration,’ ‘Trade and Economics,’ ‘Religion,’ ‘The King,’ ‘Literature,’ ‘Science and Art,’ and ‘Legacy and Survival.’ The anthropologist is familiar with the problems of describing the total culture of a people within a systematic framework, and will understand the added difficulties which arise when the description must be both synchronic and diachronic over a period of nearly three millennia during which many far reaching changes were introduced by intrusive population groups. In addition to these problems there are those imposed by the limitations of the evidence. While the assyriologist has been blessed with evidence, particularly in written form, of an embarrassing richness, it is not evenly spread. Some periods are illuminated by written documents and material remains, others have yielded little or nothing. Dr. Saggs has therefore tried to give a historical survey of the material within each of his sub-headings. This works best for Administration, Law, the King, Literature and Religion, and the author as a philologist gives a good account of all this material. The weakest chapter is that on society, a subject which has not received much systematic attention (though it might have been well to repeat a reference to Meissner, Babylonien und Assyrien, 1, chapters ii and 12) in the brief bibliography on p. 514. The full heading of the chapter is ‘The Foundations of Babylonian Society and the Babylonian Way of Life.’ While the ‘Foundations’ part fills in the political and economic background of the third millennium, of which the history has been dealt with earlier, and forms a useful supplement to this, the second part tries in 26 pages (169–195) to cover a variety of subjects from slavery to sewers and marriage to music, and seems indeed to include all the material which will not go anywhere else. The arrangement does not seem logical, and some topics only receive very summary treatment; music in particular gets about half a page. This example is significant, for most of the clear evidence on music is in the monuments and not the texts, which illustrates the essentially philological approach of the book. For a full treatment of such subjects illustrations of artifacts and artistic representations are necessary.

The philological bias is a good fault, however, since the non-specialist can inform himself on matters of material culture if he knows where to look, but he cannot read the texts. This book therefore provides a valuable guide to the texts for the layman, and one which makes some attempt at covering all the ground, rather than skimming off the show pieces.

There are 64 black and white and two coloured plates distributed in irritating waggles throughout the book (23 is upside-down; 62 is from Palestine, Middle Bronze Age), but they are not integral to the text and one suspects that the publishers had them put in for the sake of it, as seems to be the common practice nowadays. The binding is strong and the paper very tough, which perhaps partly justifies the rather high price.

T. C. MITCHELL


L’interêt que les services officiels américains ont porté, durant le dernier conflit mondial, aux problèmes de l’Asie Centrale étend ses effets loin dans l’après guerre: enquêtes sociologiques menées auprès des réfugiés, publications des matériaux réunis sur le terrain avant les troubles, reproduction de vieux récits de voyage, traduction d’études socio-culturelles russes, manuels d’initiation aux dialectes turcs et mongols se multiplient depuis vingt ans, sous les auspices d’organismes tels que The United States Office of Education, The American Council for Learned Societies, The Human Relations Area Files. En moins de cinq ans, plus de quarante titres (dont vingt-deux durant la seule année 1963) ont déjà vu le jour dans la collection
Urals and Altaic Series de l'Université d'Indiana, témoignant éloquemment de la vitalité et de la variété de l'effort de vulgarisation entrepris par les mongolistes et les turcologues américains.

Si les deux livres ici recensés sont destinés exclusivement à l'initiation d'étudiants, ils font cependant occuper une place très honorable dans ce genre de littérature et ils rendent maints services aux novices qu'ils introduisent dans le domaine de la sociologie, dans celui de l'histoire de l'Asie Centrale et dans celui des langues orientales.

Le premier travail, Social Organization, présente une analyse de l'organisation publique — bannière, clan, lignage, village — et de l'organisation familiale d'un peuple turc : les Kazakhs du Turkestân Occidental, et de quatre peuples mongols : les Orduos de Mongolie Intérieure (dans la grande boucle du Fleuve Jaune), les Buriats de Sibérie (à l'ouest et à l'est du lac Baïkal), les Kalmouks de la base Volga, les Mongous de la frontière sino-tibétaine, au Kansou. (Les trois cartes données en appendice sont malheureusement de simples schémas de piètre utilité.)

Des introductions historiques présentent, pour chacun des cinq peuples, des témoignages sur le passé des institutions et le degré de leur évolution. Les développements mettent en lumière la règle de la descendance patrilinéaire à tous les niveaux du groupe social, le principe de l'autorité patrilinéaire, et, du reste, de la résidence patrilinéaire, le caractère de personne morale du clan et du lignage. Les termes de parenté, spécialement ceux qui désignent les cousins parallèles ou croisés, sont longuement étudiés et résumés in fine en listes et en tableaux. M. Krader s'attache, en outre, à faire ressortir le rôle de l'échage des présents dans la formation du lien matrimonialement, spécialement le qaln (kalyn dans la transcription de l'auteur) remis par la famille du jeune homme à celle de la jeune fille, la situation de la femme mariée avant et après la naissance d'un fils, les tabous qui la frappent dans la demeure de ses beaux-parents, la pratique d'un lévirat junior (remariage de la veuve avec le frère cadet du défunt), le régime matrimonial (en règle générale, chez les Mongols, il est de tradition que les titres, fonctions et biens incorporels passent à l'aîné, que la demeure familiale et son mobilier échoient au fils dernier-né et que l'ensemble des biens corporels restants soit partagé également entre tous les fils, les filles recevant un petit dower lors de leur mariage). Il insiste, à juste titre, sur le rôle essentiel assigné aux généalogies pour établir, chez les peuples nomades, la position des clans, des lignages, des familles, individus, pour maintenir, à l'intérieur de tout le groupe agnatique, la conscience vivante d'une ascendance commune ; pour déterminer les limites du groupe endogame.

Quelques points intéressants ne manqueront pas de frapper le lecteur : ainsi de l'ancienne organisation sociale à base militaire qui survivent dans la forme circulaire des villages et dans une tendance nette à la division dichotomique, mais non antagoniste, des groupes étagés, surtout chez les Buriats (cf. par exemple p. 63). Signalons aux Africains, s'ils recherchent, hors de leurs régions d'étude, des parallèles à la pratique du mariage préférentiel entre cousins croisés et à la tradition de l'agressivité vers les orques utérins, qu'il existe chez les Mongols une prééminence au mariage soit entre cousins croisés soit plus largement avec les parents du clan maternel. Rappelons, à ce propos, l'exemple célèbre des unions préférentielles aux XIIIe et XIVe siècles entre le clan impérial des Borjigir et celui des Qongqirat après que le grand Khan Ogéodei ait pris en 1237 un décret en faveur du clan de sa mère : 'Chez les Qongqirat, quand il naît des filles, à chaque génération que [l'une d'elle] soit impératrice ; quand il naît des fils, à chaque génération qu'ils épousent des princesses impériales. Nous permettons que, tous les ans, à la fêre lune de chaque mois, on lise le décret que nous donnons. De génération en génération que [cette pratique] ne cesse pas!' (Yiim-shih, éd. po-na-pen, 118/1a-b, biographie de Dei-Seten). Mais c'est chez les Kalmouks et leurs voisins turcs les Kazakhs, qu'on rencontre un antagonisme entre neveu et oncle utérin qui, croyons-nous, rappelle assez bien, mutatis mutandis, l'exemple soudanais (cf. pp. 155 sq. et 214 sq. sur le droit au baronta ou pratique du vol légal).

Nous relèverons aussi l'existence de la règle Pater is et quem nuptias demonstit entendue, chez certains peuples mongols, dans son acception la plus large et la plus étendue (cf. par exemple p. 300) au point que les enfants conçus en l'absence du mari, éloigné ou défunt, lui sont attribués. On ne peut s'empêcher de se remémorer alors la curieuse solution de continuité qui rompt la généalogie de Chinggis, si l'on considère la génération de Dobun-Mergen (Histoire Secrète des Mongols, paragraphes 17 à 21) : les Borjigir sont en effet réputés descendre de deux ancêtres mythiques, Börte-Chin et Qo'ï-Maral, le Loup et la Biche, par l'intermédiaire de Bodonçar, fils naturel d'Alan-Qo'a et d'un rayon de lumière, mais officiellement enfant légitime du défunt mari de sa mère, Dobun-Mergen, le descendant direct du couple mythique.

Pp. 67-68, l'auteur note la lutte des chamanis burutis pour s'emparer du pouvoir temporel, mais il n'y voit qu'un phénomène isolé. Il n'en est rien. Les prétentions des chamanis burutis rappellent, semble-t-il, celles des chamanis pré-gengiskhanides et gengiskhanides dont l'opposition au souverain a culminé avec le célèbre Kokoqhi ou Teg-Teiggi. Une vocation à l'action politique paraît être origine- lement le propre des grands chamanis. Pp. 40-41 et 309, il distingue en Ordo et chez les Mongous deux catégories de marionnettes qu'il dénomme 'anomaux' : le mariage d'une jeune fille avec un objet appartenant au futur époux et le mariage d'une jeune fille avec un tapis de prière, un arbre, une terre cuite. En fait, les deux situations ne sont nullement identiques : la première est un exemple de mariage par procuration, la deuxième un cas de mariage fictif, comme le mariage avec le piquet de la tente, le linteau ou le jambage de la porte (voir A. Mastaert, Dictionnaire Orduos 176 a, s.v. liazza et Central Asiatic Journal II/4, p. 277).

Le deuxième ouvrage ici recensé, Peoples of Central Asia, couvre une aire : le territoire désigné comme la zone de contact des peuples turcophones en URSR (seuls les Kazakhs entrent dans le plan de l'un et l'autre travail). Après un aperçu de la géographie physique et économique de la steppe et du désert de l'Asie Centrale soviétique, M. Krader donne une classification des langues parlées dans ces régions et des types humains, une vue historique de la formation des peuples considérés et de leur évolution jusqu'à la formation des républices soviétiques d'Asie Centrale ; il passe ensuite en revue les religions et les systèmes sociaux des nomades et des sédentaires ; il analyse enfin les données démographiques et économiques modernes, groupées en tableaux. Deux cartes dépliantes sont jointes en appendice, de qualité assez médiocre (alors que d'ordinaire on réduit les cartes pour les rendre plus nettes, M. Krader a, au contraire, fait agrandir l'une des siennes).

Il faut louer M. Krader de faire une large place, dans ces deux ouvrages comme dans ses articles antérieurs, au cadre naturel et à la géographie économique. Peut-être même tend-il à en abuser et l'on se fatigue à la longue, des 'ecological regions' et 'ecological bases' dont il émaille son texte.

Le principal mérite des deux ouvrages recensés est l'ampleur de la documentation russe sur laquelle ils reposent (un peu au détriment, reconnaissances, le de la bibliographie occidentale). Les opinions personnelles sont, en effet, réduites au minimum et le travail n'est qu'une mosaïque de citations, de sorte qu'un lecteur ignorant du russe aura accès à une grande masse de renseignements nouveaux pour lui. Mais le spécialiste, sociologue ou orientaliste, est en droit de se montrer plus exigeant, à supposer toutefois que ces deux livres lui soient destinés. Peoples of Central Asia n'est qu'un manuel de références rapides dont il est prudent de toujours contrôler les données. Social Organization, par contre, aurait pu devenir un bon livre d'érudition si sa composition avait été plus soignée et ramassée, son plan plus approfondi. Un style diffus, des rédites constantes, des développements coupés et repris sont parfois penser à des séries de fiches mises hâtivement bout à bout. (Les sections les plus travaillées concernent les Kazakhs et les Kazakhs.) L'attention se lasse des exercices d'école et des lourdes démonstrations de faits bien connus. Quatre cents pages pour prouver que la société nomade est à descendance patrilinéaire et à autorité patriarcale, c'est trop.

Il semble que, dans le même espace, beaucoup d'autres faits passés sans silence auraient pu être évoqués : le problème de l'esclavage, du servage, de la domesticité ; les clivages sociaux en Ordos ; le rôle des monastères bouddhiques en Burjatie ; les droits respectifs des diverses époques après la dissolution d'un ménage poligame. Les termes d'origine identique, tels omoq, oboq, otoq, auraient gagné à être confrontés. La déception est vive de découvrir à la lecture que le projet de l'auteur est loin d'être aussi vaste que ne le laisse supposer le
titre. De l'organisation sociale, M. Krader ne retient que la parenté par le sang, l'alliance ou l'adoption, au niveau du groupe ou de l'individu. Parmi les pasteurs nomades turcs ou mongols, il opère une sélection peut-être justifiée qui retire au travail le caractère général que son titre semble lui conférer, et le défaut est d'autant plus grave qu'aucune note bibliographique ne renvoie à des études d'un plan identique qui permettraient de combler les lacunes.


Quant aux Turcs, ils sont plus maltraités encore que les Mongols: seuls les Kazakhs ont été retenus, en raison, probablement de certaines ressemblances de leur système avec celui des Kalmuks. Il aurait pourtant fallu inclure, au minimum, les Kirghiz sur lesquels on est bien documenté, ainsi que les Uzbeks. M. Krader justifie naïvement sa limitation par la croyance qu'il avait en 1953, à la suite de Radloff, d'une identité des systèmes Kazakhs et Kirghiz !

En fait, Social Organization présente les institutions de certains peuples mongols, comparées, pour l'un d'entre eux, aux institutions d'un peuple voisin. Malheureusement, l'auteur, obsédé par des controverses qui soulèvent actuellement l'hypothèse aralique, préfère parouvrages d'une uniformité certaine des systèmes turcs et mongols d'une extrême à l'autre du monde des steppes. Or son échantillonnage est bien malencontreusement choisi, puisque les ressemblances entre système kalmuk et système kazakh peuvent s'expliquer par des emprunts de voisinage. Vice plus grave encore, le classement des chapitres et certaines allusions laissent percer une théorie génétique: on nous fait entendre que les systèmes ordos, burut, kalmuk, kazakh et mongol représentent les étapes successives du développement d'un système originel unique.

La base du travail est donc critiquable et de nombreux détails de sa réalisation ne le sont pas moins. Les transcriptions sont inconstants. Certains signes, par exemple le signe g, ignorent les caractéristiques d'écriture turques, mongoles et chinoises (bien qu'il cite, p. 14, note 5 un ouvrage chinois, non repris d'ailleurs dans la bibliographie finale), mais il aurait pu demander conseil à des collègues mieux versés que lui dans l'orientalisme. On ne voit pas pourquoi il s'arrête dans l'introduction de Peoples of Central Asia qu'il utilisera tôt comme dans Kulja et tantôt dzh comme dans Tadzjikistan, ni pourquoi il rend par Tsaidin Bichik le titre du code kalmouk connu, en abrégé, comme le Çafın bıčık, alors que plus loin il écrit très correctement tańji. Pourquoi appelle-t-il wergeld l'institution germanique du wergeld, wafıj le wafij islamique ! Les exemples pourraient être multipliés sans peine. Pour dérouler encore plus le lecteur étranger au sujet, les mots orientaux, même en italique, reçoivent de temps à autre, mais pas toujours, le s du pluriel: dans Social Organization, on relève, par exemple, des tańji à côté des zaśangs, des noying, des mengese (notions, en passant, que ces trois derniers mots n'apparaissent pas dans l'index, aussi défectueux dans Social Organization que dans Peoples); dans Peoples of Central Asia, on trouve, entre autres, un absurde Tien-shan !

La liste des termes de parenté, sucre du travail de M. Krader, ne peut être utilisée qu'avec grandes précautions; celle tirée des inscriptions turques de l'Orkhon et de l'Ienisseï est même entièrement erronée, l'auteur écrivant: probablement appuyé sur le premier travail de Thomsen publié en 1896, sans se référer aux corrections ultérieures de 1916 et de 24-25.


On le voit, les deux travaux de M. Krader, malgré leur utilité certaine et des mérites évidents, ne doivent être consultés qu’avec la plus grande circonspection.

FRANÇOISE AUBIN


The Arctic Institute of North America is to be congratulated on taking the initiative in publishing modern Russian sources on Eskimo and Siberian ethnographic facts and ethnological problems. Papers which otherwise would not be accessible generally to Western students are fully rendered here in English. The present volume which deals with Siberian shamanism, a subject which recently has attracted renewed interest (particularly through publications by Djetszej, Eliaide and Findelis). Actually, only two of the articles refer directly to shamanism, the remainder being studies in soul beliefs and cosmology. These articles are, of course, indirectly concerned with shamanism, which dominates religion in the Siberian area. All the articles were originally published between 1951 and 1959.

The value of this book lies in the factual contents which it submits, the results of field research among some northern Siberian tribes, whereas the theories and generalizations of the authors are dogmatic and scarcely correspond to the facts. Vasilevich’s account of Tungus cosmology and Prokojeva’s description of Samoyed shaman costumes and their ideological background are, on the whole, very illuminating and objective. The former author’s discussion of the conceptions of dual creators is particularly instructive. Also Chernen-
sov brings in new and detailed material, concerning Ugrian soul beliefs. He disposes of Karjalainen's interpretation, according to which there is a functional soul dualism, and reintroduces the Wundtian approach from yesterday—which, in view of his own material, is less adequate (cf. also J. Paulson, *Die primitiven Seelenvorstellungen der nordeurasischen Völker*, 1938, pp. 79 ff.). Chernetsky further confuses the issue by confounding the souls of the living with the existence forms taken by the dead, and he even discusses 'soul souls.' Really, his own judgment of Munkácsi is appropriate here, viz. that the concepts set forth are of no scientific significance, but the material he has gathered is certainly valuable.

Chernetsky makes several references to a supposed totemic stage whose vestiges can be shown here and there in Ugrian beliefs. Anisimov, in his Mound of the Tungus shamanistic tent, sees the same thing. The process of evolution of totemic beliefs into the form of shamanistic ideology was, he says, conditioned by changes in the material content of society, and particularly in production. The shamanistic spirits, he assures us, represent different evolutionary stages, the oldest among them being the totemic stage; and even the west of the world tree originates in 'the totemic beliefs of the matriarchal epoch.' The shamanistic rite grew from the soil of ancient (totemic) clans, etc. It is a pity that so much first-class information on shamanistic performances is obscured by reasoning of this kind. Also in his second paper, on Arctic cosmology (which starts with quotations from Marx and Engels and finishes with a quotation from Comte), the same author dedicates much space to arbitrary evolutionary speculations.

Let us hope that future Russian field research will be conducted in a less doctrinaire atmosphere, and that the old-fashioned obsession of universally applicable genetic reconstructions will come to an end. The field material in the present volume testifies to the fact that Soviet anthropologists are very competent field ethnologists, particularly when they suppress their ideological loyalties.

AKE HULTKRANTZ


This short book comprises three lectures given by Professor Dumont in 1962, together with an Appendix containing a slightly different text of his inaugural address from that published in *Contribution to Indian Sociology* (Vol. I, 1957). It is important because it provides a further statement of Dumont's approach to social anthropology, and because it makes a significant contribution to the problem of how the social anthropologist should work in a civilization.

The theme running through the lectures is that a civilization can only be understood by taking account of its own ways of thought. Hence, the student must be aware of the differences between his own and his own categorization and interpretation of data, and must establish 'un rapport intellectuel correct' (p. 9) between the two. Professor Dumont maintains that this has not been done in sociology and other studies of India, and he therefore sets out what he believes to be the main ways of thought of Western and Indian civilizations. Only on this basis can truly comparative sociology evolve; anything less places 'les idées au magasin des épiphanèmes et confondre toutes les civilisations avec leur plus petit commun dénominateur' (p. 34).

Each lecture is devoted to a particular aspect of the theme, taking in turn the caste system, the role of history, and social change in present-day India. A short review cannot do justice to the masterly and subtle way in which the argument is developed. What emerges is that the basic distinction between the ways of thought of the two civilizations is said to be one between a social categorization of equal individuals in the West and a hierarchic totality of parts in India. Dumont sees the latter as running right through Indian civilization; the interdependence of opposites, rather than the worship of gods, is the 'heart of Hinduism' (p. 16), and it is also the basis for political and economic action, and of the relation of man to nature.

This is a stimulating approach, and for through it Dumont presents many features of Indian civilization in a different and convincing light. Some social anthropologists may wonder, it is true, whether it is possible to compare institutions in different societies in the ways in which they are thought of by the participants, and their relation to the value system of the whole society, must also necessarily be included. The difficulty cannot be resolved by agreeing upon different definitions and levels of analysis for different purposes, it seems. A restricted and non-ideological definition of caste, for example, can fit the Indian institution into a comparative study of stratification; but Dumont maintains that such a comparison, made in terms of the observer's own definition and categorizations, is false and does not give the real situation. This approach has already produced controversy in the *Contributions*, and the issue remains a live one. Here, the book makes a contribution to general methodological and to the discussion about the ways in which social anthropology can be a comparative discipline. In addition, it will greatly interest those who are working in civilizations. For it suggests a method of handling these, by abstracting from what has been called the Great Tradition its essential 'civilizational' categories, which are then applied to the small-scale situations in which the social anthropologist works. This does not necessarily supplant the analyses made of these situations alone; for instance, the hierarchical interdependence of castes has been stressed by those who have studied caste at this level only. But it may well illuminate them, and also relate them to other aspects of the civilization. This is, again, a controversial approach; for it requires a combination of disciplines which some think lie beyond the social anthropologist's proper sphere. It could not have found a more persuasive advocate, however, and one therefore wonders whether Professor Dumont may not herald an Indian phase in British social anthropology.

ADRIAN C. MAYER


The economic growth of underdeveloped areas is not simply an economic problem but embraces the whole social fabric of the populations. Since most of these countries are ex-colonies their face the difficult task of forming an efficient indigenous administration capable of planning and organizing economic development. The present volume, consisting of ten contributions, brings together papers by political scientists and economists, all concerned with administration and economic growth in India. Brabanti, Jagota and Malemba deal with problems connected with the civil service and political leadership, while Spengler and Mitra concentrate on purely economic matters: the former outlines Kautiya's economic treaties, whereas the latter discusses the tax burden for Indian agriculture and advocates a considerable increase in taxation as one of India's wealthy landlords.

The essays by Tinker, Park, Tilman and Sovani are of more immediate interest to the social anthropologist. Both Tinker and Park discuss the village in the framework of development, but they treat the subject as political scientists, rather than from the point of view of the social anthropologist. They discuss the concept of 'panchayati raj' in its past and present setting. Both emphasize the reluctance shown by panchayats to carry out developmental functions. Park observes the persistence of traditional panchayats as opposed to the inoperative newly elected statutory panchayats, but he does not take into account that the traditional panchayats are in most cases composed of hereditary office-holders and are concerned mainly with the settling of disputes and organizing of village feasts, rather than with administration or economic matters.

The impact of India's caste system on economic growth is treated by Tilman, who outlines the social, ritual and economic characteristics of caste, but ignores its function as a moralizing agent. This oversight is made good by Sovani, who stresses the 'deadening efficiency' of the Hindu concepts of dharma and karna in maintaining the status quo. Tilman points out that upward social mobility, crucial for economic modernization, may at present be hampered by caste restrictions, yet he is confident that urbanization will break down caste barriers very soon. He fails to observe, though, that caste loyalties may facilitate geographical mobility, which is also essential to economic growth.
The book makes interesting reading; however, it would have been helpful had it contained at least a brief essay giving it a common framework.

SCARLETT EPSTEIN


P. G. Shah's latest publication shows that at the age of 75, he has lost none of his wit, nor interest in Indian tribal life. As those familiar with his writings will guess, this book is very comprehensive. It embraces: anthropometry and serological studies; interpretations of the origin of tribal dialects on the basis of comparative philology; psychological evaluations; the supernatural; history and folklore; climate and culture; concept and conduct; and other fascinating subjects.

Sometimes the author pontificates about society and the nature of man. For example (p. 121): 'There are always a few wise and sober men in every community who form the pillars of society and who influence decisions . . . Well, one can think of some communities in which it would seem that the wise and sober, granted that they exist, are not regarded as pillars and have little effect on decisions. Again (p. 141): 'It is primitive man's ways, as soon as the nomadic tendencies give place to a permanent residence, as soon as sex relations get into control and co-operative economic endeavours becomes necessary, a family generally comes into existence.' Now it is not at all clear whether the author is theorizing as a social evolutionist or merely describing how the young tribal comes to settle down. If he former, he would appear to be in conflict with the accepted view that the family as a social institution is not only universal now but has always been a prominent feature of all human societies; if the latter, is he not merely describing a process common to all societies? Similarly (p. 142): 'The responsibility of the family life softens the wild nature of the tribal people . . . a tribal is seldom able to think of the next day until he has a house to keep and a family to maintain. But surely, irresponsibility prior to marriage is not peculiar to tribal people? (Furthermore, some people are infuriated rather than softened by exposure to family responsibility!)

However, Mr. Shah's chief concern is with the amelioration of the condition of these tribes and he pleads eloquently for their acceptability as an authentic section of Indian society. Moreover, he reports their various attempts to better themselves, citing long lists of the rules of behaviour which they seem to enjoy compiling. For example, rule 9, p. 122: 'Nobody should be allowed to sing obscene songs at the time of marriage.' Rule 8, p. 129: 'Develop competition but not jealousy.' Rule 8, p. 131: 'Cases of adultery by a woman are punishable by a fine up to Rs. 151, but cases of illtreatment by the husband are punishable by a fine up to Rs. 250.' (But afflicted by terrible poverty as these tribes are, how on earth can they manage to pay such punitive fines?)

Mr. Shah agrees that, when integrated, these tribes will have to pay a heavy price for keeping up with the Indian Joneses. Thus, the tribal woman 'enjoys greater freedom in betrothal, age of marriage, in divorce and freedom from compulsory widowhood than her Hindu sisters'—a freedom of which she might well be deprived on complete integration (p. 27).

To do the author justice, his glances at the future are cautious; and he can be commended on other grounds as well—industry, keenness of observation, and devotion to the ideal of service to the underprivileged groups in his country.

SCHIFRA STRIZOWER


This is the first volume of a work which tries to describe and interpret the 'magical-religious world-view of early man' (p. 518), as represented by Indian hill tribes. This book about the Bhils is organized under four headings. The first records creation and other myths, narrated by nine barus, or medicine-men, whose names are given along with those of the missionaries who acted as interpreters.

The following parts deal with the origin of 'Magical Guardian Spirits,' 'Barwodom,' i.e. the position and nature of medicine-men and the Pando, 'another type of sorcerer.' There are also female Pando.

Though an 'old manuscript,' apparently written by a Bhil convert, is frequently referred to and Fortunatu Bhabor is mentioned as the author's 'best informant' (p. 521), one would like to know more about the sources of the vast material presented.

The Bhagoria Bhil creation and flood myths sound very biblical indeed, except for the belief that among the first humans conception was caused by looking at each other or by eating blossoms from the heavenly tree. A brother and sister, after escaping the great deluge, were transformed into husband and wife by Bhagwan the Supreme God. This god bestowed white magic on the archetypal barua as a remedy against the evil influence of witches and black magicians. Hermanns perceives in this relationship between a creator and white magic the celestial archetypes to be re-enacted on earth in a macrocosmic interaction. Explaining the point, he quotes Jensen and refers to Spangler though he criticizes the latter for assuming a likewise magical content in original Christianity.

The major difficulty which the author faces in fitting the enormous amount of Bhil material into his magico-religious world view is the fact that almost all his Bhil data are Hindu mythology, slightly changed in the process of acculturation. The stories woven around Gangesa (pp. 114-31) make this particularly clear. Hermanns acknowledges that it is difficult to differentiate between what he considers original Bhil concepts and Hindu (or Christian) ones, which all go under the same, distorted Hindu terminology. He tries to overcome this difficulty by what seems to be at times arbitrary decisions, by comparison with other primitive peoples and by his observations in Tibet and Sikkim. The double interpretation of the word Bhagwan illustrates this. For the author Bhagwan is the supreme god, the world creator, when and where seen by the Bhil as a unique being. But he becomes a 'minor Bhagwan' (pp. 35, 181, 248, 261, 525, 527, etc.) when connected with his mother (Hinglas kumari, p. 182), or any other feature which does not fit into Hermanns' concept of a Supreme God. The fact that Bhakta, Advaita and other Hindu schools of thought also see Bhagwan as unique is overlooked.

The English summary (pp. 517-31) contains misprints which, however, are less awkward than some phraseology (cf. p. 35) and words in the German text. A Bhil choli, for instance, is translated as a Brustl or a Briestenhalter (pp. 322, 513, etc.).

However, apart from their presentation and interpretation, the large amount of collected material, as well as the lively photos, no doubt add to Jungblut's, Kopper's and Fuchs's publications on the Bhils in and around the mission station of Jhabua.

U. R. EHRENFELS


It is not easy to categorize Mr. Plath's kind of anthropology. He can read books and has discovered (as not all of his colleagues will admit) that Japan is not a thing. He is interested in the national picture and in national trends, but he also does his field work. He keeps his eyes on him and is concerned to report his close observations of what he sees—see his portraits of typical families in the urbanized-rural area of central Japan where he lived, and his well turned little accounts of a housewives' social club and a political meeting. His record is not, however, the workaday record of the social anthropologist concerned to prove a point of structural theory, nor even (though this is more nearly his line) of the cultural anthropologist concerned to illustrate a value analysis. His careful selection, and above all the fastidiously moulded sentences and highly wrought metaphors, show that these descriptions also have the travel writer's quality of ends-in-themselves. (And the quality is good.) Which is not by any means to say that he is uninterested in general ideas or the relevance of his observations to them. He discusses the different meanings of time in
traditional and industrial societies, the way in which the scheduling of leisure time has changed in the course of Japan’s industrialization, and the way new kinds and amounts of leisure foster new emphases within a basically unchanging system of values. And he drives home the point that civilization and its discontents look pretty much the same from either side of the Pacific.

But the discontents do not weigh heavily on him. Unlike most people who chart the emergence of mass culture in mass leisure, he is not alarmed about the debasement of standards and he does not foresee Japan of the future as a political zombieland of the manipulated masses. He rather likes what he sees. His book glows with geniality.

As such it is to be recommended for enjoyment as well as for information. In fact, he can so charm one into accepting his intentions that one doesn’t really mind coming across such technically accurate but substantially quite misleading statements as that village common land ownership was made illegal, or the assertion that the Heian period was ‘egalitarian.’ However, not all the best intentions to prevent scholastic scaffolding from spoiling the reader’s view of the façade excuse the lack of any supporting evidence for his intriguing throw-away assertion that in the inheritance of family headship ‘about one in five eldest sons is passed over.’ And I still have hopes that one day Mr. Plath or someone else in the Talcott Parsons tradition will explain to me what it means to say that, for the Japanese, group harmony is a means to goal-achievement, not to the maintenance of the status quo. How, for instance, does it fit the values of village society in which, after all, more than three-quarters of the Japanese population have lived for 95 per cent. of recorded history? If the goal to be achieved is regularly to produce a subsistence crop necessary for the maintenance of the status quo, what becomes of the distinction?

R. P. DORE

EUROPE

The Isle of Man: A Study in Economic Geography. By J. W. Birch. Cambridge (U.P., for U. of Bristol), 1964. 272 pp., xv, 204. Price £1 17s. 6d.

Dr. Birch is well known for his writings on the Isle of Man and here he has embodied some of his previous research with a considerable amount of new material and has presented us with a comprehensive study of the economic geography of the island. A short but useful account of the geomorphology is followed by a detailed survey of the growth and development of the Manx tourist industry. Almost one-third of the book is devoted to this study and this is an index of the importance of the holiday industry to the island’s economy; tourism currently provides 75 per cent. of the island’s income from external sources. Dr. Birch’s study not only affords a factual analysis of tourism in the Isle of Man but adds to our knowledge of tourism generally, a hitherto neglected subject.

A careful survey of the agriculture, fishing, forestry, mining and manufacturing activities, as well as the factors which influenced their development, concludes the book.

The work is well illustrated with maps and diagrams, and the aerial photographs are particularly pleasing, although this section could have been expanded to include such examples as the mining village of Foxdale and the farming settlement at Cregneash. The bibliography is a useful guide to further study.

Together with Professor Kinvig’s History of the Isle of Man, this book is an invaluable aid to the student and it will certainly become the standard work on the geography of the island.

WILLIAM ROLLINSON


International congresses involve the host countries concerned in enormous amounts of work, but they also provide them with opportunities for modest self-advertisement. Guidebooks, handbooks, brochures and such-like which are produced for these occasions serve to introduce countries to foreign visitors and to provide some indication of a host country’s contribution to the subject of an international meeting. Geographers are fortunate in being able to combine these two needs, since for many the physical and human environments of their own country provide the raw material for study. The two volumes under review were published specifically for those attending the 20th International Geological Congress in London in the summer of 1964 and the many field excursions and symposia which preceded and followed it. In determining the nature of a volume to introduce the British Isles to foreign geographers and also to present the work of British geographers, the organizers were influenced by the publication in 1962 of Great Britain: Geographical Essays, edited by Miss Jean Mitchell of the University of Cambridge. Her work provides a regional study of the geography of Britain and is complementary to The British Isles which, as its sub-title indicates, adopts a systematic approach. In the latter volume there are 18 essays, on various aspects of physical, historical and economic geography, all by established authorities, and four essays of a more general nature, equally authoritative. R. W. Steel writes on British geographers and their work on Britain during the present century and G. R. Crone on the mapping of the British Isles, a field in which practices and standards were established which have been followed in many parts of the world. J. Wreford Watson and W. G. East, respectively, begin and end the volume with essays on the individuality of the British Isles and on their importance in the world context. The essays illustrate the range of geographical interests and provide up-to-date statements of present knowledge on the various subjects discussed. Much of this knowledge has come from work in the field and Field Studies in the British Isles provides the background for what is also a wide variety of studies in many parts of Britain and Ireland. These range from the Channel Islands to the Northern Isles of Scotland and deal with subjects as diverse as Pleistocene geomorphology and National Parks. Both of these volumes are important permanent contributions to the geographical literature of the British Isles. They will be used and valued by all who are interested in, and concerned with, the landforms, landscapes and life of these islands.

R. MANSELL PROTHERO


The aim of this study is to explain the behaviour of people in a North Wales parish in terms of a series of equivalent oppositions: Welsh/English; lower class/upper class; rural/urban; ordinary people/officials, and somewhat more sketchily, colonized/colonizers. (To this list I feel like adding, on the basis of a graphic account of salmon-poaching, redskin-Irish faces.) The oppositions are conceptualized and felt by parishioners in terms of the first, hence the persistence and strength of Welsh culture. The book is a welcome addition to our scanty stock of studies in rural society, and much of the explanation is convincing. I feel that perhaps Miss Emmett has tried to do too much, to answer too many questions with her explanatory framework. For example, among various paradoxes in their social life for which she sought resolutions was ‘that a high rate of illegitimacy existed in a district where the influence of the chapel on peoples’ lives is relatively very strong and where the chapels frown severely on illegitimacy’ (p. 29). I do not feel that Miss Emmett really establishes that there is a paradox here. Granted that figures show a higher rate of illegitimacy than elsewhere, what factors have they for abortion, and what is their attitude to it? They could be obeying another of the Christian Commandments by refusing to take life.
The style of writing is pleasantly straightforward, though from some passages I suspect that Miss Emmett did not always have an audience of social anthropologists in mind when she wrote.

JAMES LITTLEJOHN


275 Searchers after the picturesque in the eighteen-twenties found, still surviving in the Welsh countryside, a dress similar to that of a modern woman, in Wales and England alike, in the late eighteenth century: in Wales local flannels and homespuns in checks and stripes, had resisted for longer than in England the spread of printed cottons with their finer patterns. The romantics of the middle of the century, headed by the enthusiastic and forceful Lady Llanover, tried to preserve and standardize this style and these fabrics as a national Welsh costume. This holding-back of the natural movement of fashion unfortunately led to a third phase, at the end of the century, a period in which the dress became falsified and prettified for the benefit of producers of souvenirs and picture postcards into the fancy dress which is still with us today.

This is the development which Mr. Payne traces in his article, gently unscrambling all the trappings which have accumulated on fact during the short history of 'Welsh' costume. The illustrations are particularly well chosen and show very clearly this growth of fancy dress from the honest archaisms which Lady Llanover saw and recorded in 1830.

ANNE M. BUCK


276 As an agency of socialization and social control, advertising is one of the most important institutions of modern society, and with the increasing proportion of our economy devoted to distribution and consumption will become even more so. Yet, except for the occasional denunciation, it has been largely ignored by social scientists. So Tunstall's study, limited though it may be, is most welcome.

To gather data for his research, the author worked for three months in an advertising agency in London, interviewed 45 advertising people, and read widely in the trade literature. The result is a knowledgable report on the workings of the advertising agency and the concerns of its personnel, with chapters devoted to the agency business; account executive; creative, media, research, and marketing personnel; and public relations in advertising.

The great merits of the study are its objectivity and its references to basic problems; the great deficiency, sociologically speaking, is the limited development of these problems. The author touches on the myths surrounding the advertising man, the importance of personal relationships, the career lines and status rankings of advertising personnel, the alleged restrictions on the creative process, the links between the United States and England, and especially—a point which runs through the volume—the insecurity which derives from the inevitable guesswork in evaluating advertising campaigns. But these references are not related to any broader theory, compared with other occupations or social structures, or even viewed in a context of social change. The focus is on the descriptive and the problems which loom important to advertising people. Nevertheless, the data are there, clearly and intelligently presented, and available to those of a more theoretical bent.

FREDERICK ELKIN


278 This is undoubtedly the best survey of Old Scandinavian religious which has been published in English up to now. MacCulloch's presentations are no longer up to date, and Branson's two recent books in the series 'Myth and Man' are too popular and concentrate too much on the mythological aspects of Scandinavian religion. Turville-Petre deals not only with mythology but also with cults and religious beliefs, and his discussion of the sources gives an excellent introduction to the Old Norse literature on the subject.

The present work certainly cannot replace the recently deceased Dutch scholar de Vries's Algetmanische Religionsgeschichte, since that book is more extensive, almost encyclopedic, in scope. Nevertheless, Professor Turville-Petre's judgments seem in comparison more sober, more cautious. For one thing, although he feels sympathy with Dumezil's tripartite system in Nordic mythology he does not, like de Vries, unconditionally subscribe to it (cf. also his contribution to Dumezil's Festschrift in Coll. Latomus, Vol. XLV).

Scandinavian religion is a field where many interpretations are possible, and the author brings in many new theories which, of course, are open to criticism. I am sceptical about the proposed theory of Odin's sacrifice; it seems here most likely that we have to deal with a mythical prototype of ritual regicides. The author finds it difficult to understand the story of how Odin lost his eye; he seems here have thought of the common idea of the one-eyed god, e.g. as discussed by the anthropologist Guttorm Gjessing. I object to the author's opinion that Tyr's name does not appear in Swedish place names, and that disir and fylgir are the same thing. There is no presentation of the character of Norse shamanism, or seidhr, and the importance of shamanistic ideology for the understanding of such phenomena as Odin's shape-changing or the sacred number nine has escaped the author. Finally, the different conceptual strata which make up Odin have not been analysed, and we miss—except for some few sentences—a thorough scheme of the development of Scandinavian religion.

In spite of these shortcomings the present work is a reliable and thoughtful presentation of ancient Nordic religion. It should be

This study of family relations in 300 villages in pre-war Yugoslavia is based on a survey by questionnaire collected by Mrs. Erlich's collaborators, mainly young teachers, students and doctors, that is village intelligentsia living among the peasants. Although the material cannot in a strict sense be considered representative, particularly as some parts of Yugoslavia were not studied (e.g. Slovenia), nevertheless it is of great value as it offers a description of family relations in the areas which have undergone most intense transformation. The Second World War and the subsequent social revolution have made tremendous changes in family life particularly in the mountainous areas. Thus this book describes what has already become part of history.

In the first part of the book the author deals with the position of the father as head of the family; the role of the mother and mother-in-law; brother-and-sister relationships; boys and girls; marriage; man and wife; birth of children; extra-marital relations; influence of alcohol; illiteracy and its impact on the family. The chapter on the changes in the zadruga will be of particular interest to sociologists who in many ways are still under the influence of romantic ideas concerning this type of extended family.

The second part of the book deals with patriarchal family systems containing the residua of the oriental and the tribal type, and changing under the pressure of such new elements as a monetary economy, education and democratic relationships which destroy the patriarchal authority. The author shows that the transformation of the family took place through the lure of emancipation, cultural contacts with West and East, Turkish and Austrian influences inland and Mediterranean on the Adriatic coast. She gives particular importance to what she calls regional types of family, which in fact is a spatially but a cultural concept, with a strong religious colouring. The ethnic element and national determination seem to have been underplayed in this survey. The last chapter is concerned with a comparison of Yugoslav family types with the American family.

The book is well written, full of direct quotations, proverbials and lively comparisons. The method was to classify villages, since villages rather than families were the units of observation. The data are aggregated into family types according to regional and religious criteria. These data sometimes show linear evolution, but the author rightly stresses that the level of economic development could not be used as the only independent variable. She found many examples of spiral continuity, e.g. the treatment of women in the family. This is best in the stabilized types of family, patriarchal or modern, and worst in transitional types.

As literature giving factual material on different types of family in developing countries is rather scarce this book could be of great pedagogical use for sociologists and anthropologists because it gives a large number of variants and types studied according to the same method. Many diagrams and statistical tables make the reading interesting and are of great help in the presentation of the material.

RUDOLF BIČANIĆ

BOOKS RECEIVED

The following is a list of books received from the publishers from July to October. Many of these will later be reviewed in MAN.

GENERAL


Africa


AMERICA


ASIA


EUROPE


OCEANIA


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THE POLYNESIAN SOCIETY
Box 5195, Wellington, New Zealand
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by John Greenway

This, the first modern study of verbal art among non-literate people, is the only comprehensive book on this important subject since the Science of Man came of age.

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Sacred and Profane: Some Thoughts on the Folk-Urban Continuum of this Dichotomy
Dr. C. Renate Barber

A Diviner's Apprenticeship and Work among the Bayaka (with a figure)
Professor Hugo Huber

Shorter Note
The Eagle Dance of At Sabe (with three figures)
Margaret King

Correspondence

Reviews
General: Africa: America: Asia: Oceania

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- Zinacanteco ‘Souls.’ Dr. E. Z. Vogt
- Some Glass Beads from the Malay Peninsula. A. Lamb. With four figures and two tables
- Rock Gongs and Associated Rock Paintings on Lolol Island, Lake Victoria, Uganda: A Preliminary Note. G. Jackson, J. S. Gartlan and Dr. M. Pognansky. With three figures
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- Traditional Designs in Some Modern Farm Tools. J. G. Jenkins
- Sacred and Profane: Some Thoughts on the Folk-Urban Continuum of this Dichotomy. Dr. C. R. Barber
- A Diviner's Apprenticeship and Work among the Bayaka. Professor H. Huber. With a figure

## SHORTER NOTE

- The Eagle Dance of At Sabe. Miss M. King. With three figures

## CORRESPONDENCE

- Cross-Cousin Marriage. A. Strathern
- The Content of Kinship. Dr. J. H. M. Beattie
- Tibet and Greece: Two Stories on the Power of Faith. H.R.H. Prince Peter of Greece and Denmark

## REVIEWS

### GENERAL

- The Concept of Freedom in Anthropology. Edited by D. Bradley. Dr. J. H. M. Beattie
- Primitive Art. By D. Fraser. Dr. J. Newton Hill
- A Million Years of Man. By R. Carrington. W. C. Brice
- Social Anthropology. By P. J. Bohannan. Dr. I. M. Lewis

### AFRICA

- Sociologie actuelle de l'Afrique noire. By G. Balandier. Dr. M. M. Douglas

### AMERICA

- The Inland Whale. By T. Kroeker. Dr. C. S. Brant
- The Menomini Language. By L. Bloomfield. R. H. Robins
- What They Say in New England and Other American Folklore. Edited by C. Withers. Mrs. E. Ettlinger
- An Introduction to Brazil. By C. Wagley. Professor T. de Azevedo
- Die Tacana, I: Erzählungsgut. By K. Honslet and A. Hahn. Dr. J. V. Jansen

### ASIA

- Social Structure in Southeast Asia. Edited by G. P. Murdock. Dr. J. R. Goody

### OCEANIA

- Plants and the Migrations of Pacific Peoples: A Symposium. Edited by J. Barrau. Dr. M. A. Towle
- Nga Moteatea: he marama rene no nga waka maha. By A. G. Ngata and Pei Te Hurinui. T. Barrow
- An Adjustment Movement in Arnhem Land, Northern Territory of Australia. By R. M. Berndt. Dr. P. M. Worsley

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