considerable slice of anthropological research. In the Introduction to his book on the Kafirs he describes how his imagination was stirred on first entering the extraordinary country of the Hindu Kush and Karakoram which lies beyond the Himalayas. He had crossed the Burzil pass and was rounding the Doian spur when

> a strange sight to the north-west startled us into open-eyed wonder. And indeed a wonderful picture lay spread out before and beneath us. . . . Above the pure sky domed over all, while in front a filmy veil of cloud was suspended, which seemed to magnify and accentuate, instead of dimming, the noble outlines which lay behind. Through this mysterious curtain could be seen a bold curve of the Indus flanked by mighty mountains, and the light yellowish-grey shades of the Sai Valley, which increased the general appearance of dream-like unreality. Beyond this, again, were the dark mountain ranges of the gloomy Gilgit region, divided by equally sombre ravines, while the eternal snows of the lovely Rakhipushi, calm and brooding, with a single cloud pennon streaming from its solitary peak, completed a background of surpassing beauty. The whole scene was illuminated by a dying afterglow. Swiftly, almost instantaneously, the light failed, and the translucent veil deepened and darkened so rapidly, that the vision-like picture was shut out almost as magically as it had flashed forth upon our senses.
>
> As we turned away silently, the fantastic thought arose in my mind that behind that transparency, that translucent cloud-film, a veritable faery country had been revealed to me, stretching far into the nothingness beyond; and an anxious doubt disturbed me lest I should never be able to enter that strange and enticing dreamland.[7]

But enter it he did, and to travel to its remotest confines.

Shortly after arriving in Gilgit for the first time, Robertson journeyed to Chitral, some 220 miles away, and this trip strengthened his resolve to visit Kafiristan. This area constituted what is now the Afghan province of Nuristan and a portion of the Pakistani State of Chitral. Its name—the land of unbelievers—comes from the fact that, alone in a strongly Muslim area, the inhabitants are pagans. In Robertson's time, and indeed today, the Kafirs, hidden in their remote and inaccessible valleys of the Hindu Kush, were objects of speculation, rumour and surprise. Before Robertson's visit, Kafiristan was known with certainty to have been visited only once previously by Europeans when the Lockhart mission entered the Bashgul valley for a few days in September, 1885. The first historical reference to the Kafirs may have been made by Alexander the Great's chroniclers,[8] for the enigmatic Nysæans of Swat, whom Alexander treated considerately as fellow countrymen, are thought by some to have been the ancestors of at least the Kamdesh Kafirs.[9] Mediæval writers, including Marco Polo,[10] speak of a country named Bolor, or Bilaur, or Belors, which probably comprised, *inter alia*, at least part of Kafiristan. But the first direct mention was made when the people of Anderab sought Timur's protection against the Kators and Siah Posh Kafirs. In response to their request, Timur invaded some of the Kafir valleys, and recorded his actions on a rock in one of the Kator defiles.[11] A hundred years or so later Baber writes[12] of Kafir invasions of Panjshir and comments on their love of wine, exemplified by the fact of every man carrying a leather wine bottle slung round his neck. Benedict de Goez remarked the same peculiarity—for such it was in a strictly Muslim region—while travelling in 1603 to Yarkand and Suchow (where he was to die) by way of Kabul and the Pamirs. He met a wandering hermit, presumably a Kafir, who offered him wine from the usual leather bottle, and whose alcoholic tastes made de Goez think that he must be a Christian.[13]

Mountstuart Elphinstone, in 1815, recorded some tolerably accurate information about them.[14] So did Wood, when writing in 1841 of his great journey to the Oxus.[15] Alexander Gardner, a soldier of fortune who served in many fierce and forgotten armies in Central Asia, may well have visited Kafiristan in about 1830,[16] but his papers about the country were burnt in Kabul after the murder of Sir Alexander Burnes,[17] to whom he had lent them. Only a few notes remain, one of which records that in about 1770 two Europeans, probably Catholic missionaries, lived in Kafiristan: one died in captivity, and the other was sacrificed. In 1885, disguised as a *hakim*, the gallant McNair was the first Englishman to reach Chitral by the route subsequently opened by Low's relieving force. It seems probable, however, that his report on Kafiristan[18] is based rather on visits to Kafir villages in Chitral than on experience of Kafiristan itself. Robertson was indeed justified in saying that Kafiristan was 'up to a few years ago, entirely unknown and unexplored, and still remains one of those few inhabited regions of the world only partially understood.'[19] These words, to a very large extent, are still true. Some publicity was given to the area in 1895 when that region of it which had been recognized as forming part of Afghanistan was brutally subdued and renamed Nuristan—the land of light—to celebrate the ruthless conquest and forcible conversion of the surviving inhabitants to Islam. The few works which have been published in the intervening years have added, but not very substantially, to Robertson's remarkable study.[20]

The Kafirs of the Hindu Kush was published in 1896, the year after Robertson's triumph in Chitral and while he was still British Agent, Gilgit. The book has 658 pages and 35 Chapters, of which half are devoted to the enthralling narrative of his field work, and half to the findings. It recounts how the Government of India acceded to his request to enter Kafiristan (moved, one suspects, by strategic rather than anthropological considerations) while he was actually on his way to Chitral in 1889. In October of that year he made a month's reconnaissance of Kafiristan, and then returned to England to prepare himself for a much longer visit. He took instruction in surveying and equipped himself with 'toys, photographic apparatus, compressed medicines, and miniature surgical instruments, together with various small articles with which to interest and amuse the Kafirs.'[21] The Indian Government supplied him with scientific instruments and books. His return journey from Kashmir to Gilgit, and thence on to Chitral was then, as indeed today, arduous and in places difficult. He left Srinagar on 29 July, 1890, crossed the passes and descended the deadly Hattu Pir to the Indus valley. Here a disaster overtook him which would have daunted most men. Seventeen porters were drowned and all his equipment was lost when the raft upon which they were crossing the river was swamped and sunk by the waves. His plight is illustrated by the fact that he was left with no footwear but a pair of tennis shoes! However, he pressed forward. On 17 August he reached Gilgit and left a week later (having collected a few stores in very partial replacement of what he had lost), finally arriving in Chitral town on 15 September. Then, after six weeks of travelling, 450 hard miles from any reasonable source of supply, he had reached base from which he was within striking distance of his objective. But this base was far from secure, a most unstable jumping-off place for a country of which nothing was known save that it was difficult and dangerous. Few contemporary anthropologists can have faced such hazards in their search for truth.

For the next year Robertson lived in Kafiristan, often in great danger, intrigued against and forced to hide and to flee, and eventually ill and weakened from privation. But he recorded his observations indefatigably and came to close friendship with many of the Kafirs. Much of his story is taken up with the difficulties which he encountered, but though retailed as narrative this has considerable sociological interest since the plots against Robertson were more the expression of a complex political system than of personal animosity. He suffered much from some of the Kafirs and indeed barely survived to turn the diplomatic tables on them at the end of his stay. It says much for the generosity of his character that no word of censure escaped him, and that he readily escorted a group of Kafirs to Gilgit, and subsequently to India, as the guests of the Viceroy. Knight gives an entertaining account of the journey with this party of 'village kings'—for the Kafirs had been joined by several young chiefs from Punial, Hunza and Nagir. Of Robertson he writes that he 'is not only a venturesome explorer and a clever Political Officer, but he makes an admirable travelling companion to youthful princes—parental in manner, not too severe, but maintaining due discipline.'[22]

The chapters in which his findings are described deal with political organization, marriage, kinship, religion, economy, warfare, slavery, folklore, and material culture, and a number of related or subsidiary topics, such as sport and morbidity. So many subjects are covered that a certain superficiality in some respects is inevitable. Nevertheless, the book as a whole is an amazing *tour de force*. Here was a man having apparently no knowledge of the existing literature, not referring to Tylor, Robertson Smith, or Herbert Spencer, nor to any speculations or facts outside his personal experience; a man who was either completely uninterested in or ignorant of sociological theory, but who nevertheless was a meticulous, sensitive and fascinated observer, and produced a comprehensive first-hand account of a primitive society. He cannot be considered as the first field worker in social anthropology. This claim could be contested by Bastian and Lewis Morgan, whose approach admittedly was far more intellectual, and more strongly by Boas, but the tenacity, concentration and long duration of Robertson's field work puts him in a class apart, the first of those—in my opinion—to show what anthropological field work may demand if the language is to be learned and the society understood. And today, 70 years after Robertson's sojourn in Kafiristan, his book is still the best briefing which can be given to an intending visitor to those parts.

In 1898 Robertson published his story of the siege of Chitral. A reviewer complimented him on the 'grace and power of its literary style' and this praise is well deserved. In both his books he displays an extraordinary ability to evoke atmosphere, and his account of people, places and events is vivid and forceful. This is how he describes the Chitralis:

> There are few more treacherous people in the world than the Chitralis, and they have a wonderful capacity for cold-blooded cruelty, yet none are kinder to little children or have stronger affection for blood and foster relations when cupidity or jealousy do not intervene. All have pleasant and ingratiating manners, an engaging lightheartedness, free from all trace of boisterous behaviour, a great fondness for music, dancing and singing, a passion for simple-minded ostentation, an instinctive yearning for softness and luxury, which is the mainspring of their intense cupidity and avarice. No race is more untruthful or has a greater power of keeping a collective secret. Their vanity is easily injured, they are revengeful and venal, but they are charmingly picturesque and admirable companions.[23]

His two books reveal an interesting picture of their author. Although he was an able exponent of the stiff upper lip the impression indelibly emerges of a man exceptionally sensitive although equally courageous and stubbornly resolute, who was genuinely humble and rejoiced to honour others, who was at one and the same time adventurously unusual and highly conventional, who was both an artist and an administrator, a scientifically minded investigator and an Empire-builder. Besides the two major works, he wrote little; a preliminary account of his explorations in Kafiristan was given to the Royal Geographical Society[24]; more sociological descriptions of his travels were given to the Anthropological Institute[25] and the Society of Arts[26]; and a rather stolid Presidential Address was read to the Geography Section of the British Association.[27] He never returned, as he had hoped to do until he was caught up in heavy administrative responsibilities, to complete his enquiries in Kafiristan, and so far as is known he did no more ethnographical work of any sort. Nor did he achieve further advancement in the service. His great period of efflorescence, of military and proconsular renown, of scientific endeavour and of literary creation lasted something less than ten years. He retired around the turn of the century and devoted his energies to politics. In 1906, after failure in Scotland, he was elected Liberal Member for Central Bradford. His strength and sincerity made him popular both in the House and in his constituency, but he spoke little except, in his last years, to protest against the inefficient prosecution of the war and in advocacy of more equitable and honourable conditions for the Indian army. He died in 1916, at the comparatively early age of 64 survived by his second wife, whose father was Samuel Lawrence the painter, and a daughter. *The Times* obituary makes no reference to his pioneering research, beyond mentioning that of his two books one was 'more technical.'[28] But to recognize the seriousness of his work was fitting praise, even if unintentional, for a man whose place among anthropologists has been consistently ignored.

Notes

[1] An interesting account of this event is given in Sir Mohomed (*sic*) Nazim Khan, K.C.I.E., Mir of Hunza, *An Autobiography* (privately circulated), pp. 61f. Hunza had up to that time paid tribute to China, but problems of the precedence of the Chinese envoys led to a deterioration of relations between the two countries.

[2] A. G. Durand, *The Making of a Frontier*, London, 1899, Chapter XI, p. 271.

[3] Sir George Robertson, K.C.S.I., *Chitral: The Story of a Minor Siege*, London, 1898. See also G. J. Younghusband and Frank E. Younghusband, *The Relief of Chitral*, London, 1897.

[4] W. G. L. Benyon, *With Kelly to Chitral*, London, 1896. See also Younghusband and Younghusband, *op. cit.*

[5] Fully described in Younghusband and Younghusband, *op. cit.*

[6] This is well illustrated by Robertson's speech at the installation of the new ruler of Chitral. *Correspondence Relating to the Occupation of Chitral, No. 4: Letter from the Government of India to Secretary of State No. 199, dated 9 October, 1895 (received 26 October), Enclosure 5: Report of proceedings of Durbar held at Chitral for the Installation of Mehtar Shuja-ul-Mulk*, London (H.M.S.O.), 1896.

[7] Sir George S. Robertson, K.C.S.I., *The Kafirs of the Hindu Kush*, London, 1891, pp. *vii–viii.*

[8] See Arrian, *Anabasis of Alexander*, Book V, 1–3, and (though he is sceptical) Strabo, *Geography*, Book XV, 1, 7–9.

[9] This view is expressed by T. H. Holdich, 'The Origin of the Kafir of the Hindu Kush,' *Geog. J.*, Vol. VII (January–June, 1896), pp. 42–9. His opinion is also implicitly supported by W. W. Tarn writing in *The Cambridge Ancient History*, edited by J. B. Bury, S. A. Cook, F. E. Adcock, Vol. VI, Cambridge, 1927, Chapter XIII, Alexander: The Conquest of the Far East, pp. 404f.

[10] Marco Polo, *The Travels*, translated by R. E. Latham, London (Penguin Classics), 1958, p. 49: 'The inhabitants live very high up in the mountains. They are idolators and savages, living entirely by the chase and dressed in the skins of beasts. They are out and out bad.'

[11] Ali Yazdi (Sharif uddin), *Zafarnama*, translated by P. de la Croix into French, 1722, and into English from French by J. Derby, 1723. See also the *Memoirs of Timur* (*Malfuzat Timury*), London, 1830, though some doubt has now been cast upon the genuineness of these.

[12] *Memoirs of Zehir-ed-Din Muhammed Baber, Emperor of Hindustan*, written by himself in the Jaghatai Turki, and translated by the late John Leyden, partly by William Erskine, with notes and a geographical and historical introduction, together with a map of the countries between the Oxus and Jaxartes and a memoir regarding its construction, by Charles Waddington, London, 1826.

[13] C. J. Wessels, S.J., *Early Jesuit Travellers in Central Asia*, 1603–1721, The Hague, 1924. Also J. Pinkerton, *Voyages and Travels*, Vol. VII, London, 1811, p. 579. These accounts are based on Matteo Ricci's commentaries translated from the Italian of the manuscript into Latin by N. Trigault and published under the title *De Christiano Expeditione apud Sinas*, Rome, 1618.

[14] The Hon. Mountstuart Elphinstone, *An Account of the Kingdom of Cabul and its Dependencies in Persia, Tartary, and India: Comprising a View of the Afghan Nation and a History of the Dooraunee Monarchy*, London, 1815.

[15] John Wood, *A Personal Narrative of a Journey to the Source of the River Oxus*, London, 1841.

[16] Hugh Pearce, editor, *Memoirs of Alexander Gardner*, London, 1898, pp. 33f., 109–12, 159–61.

[17] Hugh Pearce, *op. cit.*, p. 159. Sir Alexander Burnes was the author of *Travels into Bokhara*, 3 vols., London, 1834. He was also the joint author of *Reports and Papers Political, Geographical and Commercial Submitted to the Government by Sir A. Burnes, Lt. Leech, Dr. Lord and Lt. Wood, Employed on Missions in the years* 1835–6–7 *in Scinde, Afghanistan and Adjacent Countries*, Calcutta, 1839. Paper No. X concerns the Siah Posh Kafirs.

[18] W. W. McNair, 'A Visit to Kafiristan,' *Proc. R. Geog. Soc.*, 1883.

[19] G. S. Robertson's 'Kafiristan,' *Geog. J.*, Vol. IV (July–December 1894), p. 193 (a paper read before the Royal Geographical Society, 25 June, 1894).

[20] See R. C. F. Schomberg, *Kafirs and Glaciers*, London, 1938; A. Herrlich, *Land des Lichtes*, Munich, 1938; A. Scheibe, *Deutsche in Hindu Kush*, Berlin, 1939; H. Siiger, 'Ethnological Field Research in Chitral, Sikkam and Assam,' *Hist. Filol. Med. Dan. Vid. Selskab*, Vol. XXXVI, No. 2 (1956); E. Newby, *A Short Walk in the Hindu Kush*, London, 1958.

[21] *The Kafirs of the Hindu Kush*, p. 34.

[22] F. E. Knight, *Where Three Empires Meet*, London, 1893, pp. 485–95.

[23] *Chitral: The Story of a Minor Siege*, pp. 10f.

[24] See note [19].

[25] Sir George S. Robertson, 'Kafiristan and its People,' *J. Anthrop. Inst.*, Vol. XXVII (1897), pp. 75–89.

[26] Sir George S. Robertson, 'Kafiristan: Its Manners and Customs,' *J. Soc. Arts*, Vol. XLV (1897), pp. 573–81.

[27] Sir George S. Robertson, 'Political Geography and the Empire' (Presidential Address to the Geographical Section of the British Association, Bradford, 1900), *Geog. J.*, Vol. XVI (July–December, 1900), pp. 447–57.

[28] *The Times*, London, 4 January, 1916.

ROYAL ANTHROPOLOGICAL INSTITUTE

PROCEEDINGS

6

Cæsarean Section with Maternal Survival among Jews in the Roman Period. *By Jeffrey Boss, M.B., B.S., Ph.D., Department of Physiology, University of Bristol. Summary of a communication to the Institute, 13 October, 1960*

The operation of cæsarean section, that is, the delivery of a baby through an incision in the abdominal wall, is very ancient, being older than Cæsar by about half a millennium. Cæsarean section, as performed by the Romans, was the rescue of a fœtus from a moribund or recently dead mother, in the hope of saving the baby alive, and this operation was also practised among the Jews of the Roman period and in Vedic India. In addition, there is evidence that, in Roman times, the Jews not only performed this *post mortem* operation, but also carried out cæsarean section on living women in the justified expectation that the patient would survive and heal.

The oral tradition of the Rabbis during the two centuries before Christ and in the first 200 years of the Christian era is recorded in several collections, of which the most important is the *Mishnah*. The rabbinical discussions, recorded *verbatim*, include passages dealing with three topics relevant to maternal survival after cæsarean section. The first of these centres on the commandment in the twelfth chapter of *Leviticus* concerning the 40 or 80 days of separation and purifying after childbirth. The rabbis, as reported in the *Mishnah*, discuss whether a woman delivered by cæsarean section should or should not observe these days of separation, and it is clear that they supposed that she could survive the operation. Secondly, there is some argument in the *Mishnah* whether a woman delivered by cæsarean section should make the customary *post partum* offering at the end of the period of separation; on both sides of the argument it was implicit that a woman could be fit enough to bring a sacrifice 40 or 80 days after cæsarean section. Thirdly, there is evidence that it was taken for granted that a woman could survive the operation and have a further pregnancy ending with a normal delivery by the natural route. This is apparent from the discussions concerning the situation of two eldest brothers, of whom the first had been delivered by cæsarean section and the second naturally. It was clearly of importance to determine which had the rights of primogeniture. The biblical description of the first-born is 'that openeth the womb.' If the first son to be born was delivered through the belly wall, he did not 'open the womb'; the second, on the other hand, was not the elder. Hence the difficulty, a difficulty which arose only because their mother could survive the operation and bear again.

The internal evidence of the texts indicates that we have accurate reports of the relevant discussions and that the latter deal with actual situations and not speculations. Manuscripts older than the development of *ante mortem* cæsarean section in Europe contain these texts in a form materially similar to that in which we have them now and previous transmission followed an exacting tradition of reporting and copying.

Three commentaries, the *Gemara*, the commentary on the *Talmud* by Rashi, and the *Book of the Lamp* by Maimonides, comment on the Mishnaic passages on cæsarean section with maternal survival. The *Gemara* is a record of rabbinical discussions about passages of *Mishnah*, on which it thus forms a running commentary, *Mishnah* and *Gemara* together constituting the *Talmud*. The *Gemara* records material from the third, fourth and fifth centuries and, in its system of reporting and general methods of discussion resembles the *Mishnah* itself. Cæsarean section followed by the mother's recovery is commented on seriously in the *Gemara*, which raises no difficulty about accepting the obvious meaning of the *Mishnah* where the latter refers to the operation. This is clearly of importance when we remember that the *Mishnah* and *Gemara* are records of a single continuing tradition, with no clear time interval between the latest rabbis reported in the former and the earliest of those mentioned only in the latter. Rashi, writing in the eleventh century, records at least twice that the operation was carried out by what he terms '*sam*.' One of his comments runs as follows: 'By "*sam*" they opened the womb; they brought the fœtus out, and she healed.' A century after Rashi, Maimonides rejected the possibility of the mother's survival after operation. He did not, however, adhere consistently to this view and, where he put it forward, he was forced into providing an obstetrically weak explanation as an alternative to the plain meaning of the *Mishnah*. The commentators, taken together, support this plain meaning.

That the cæsarean section and its outcome were taken for granted indicates that our texts do not refer to the occasional miracle of a mother's recovery after she had been taken for moribund or dead and the baby rescued from her womb. And if the mother was evidently alive, her recovery must have been expected, as it was certainly not permissible to kill a woman to save her unborn child. The indication for cæsarean section is stated explicitly to be a three-day labour: that is, the operation was performed with one clear day separating it from the onset of labour. Whether an anæsthetic was used we do not know. Sleeping drugs were well known in the Talmudic period. The incision, according to Rashi, was carried out by '*sam*.' The identification of '*sam*' is itself a possible subject of long discussion. There is considerable material on the subject in the *Talmud* but many textual problems remain. The position of the opening cannot be exactly ascertained, but it was, on the evidence of the *Talmud*, lateral rather than central.

Cæsarean section was based on a foundation of considerable surgical skill and physiological and anatomical knowledge and the incentive to develop the operation was strong, since the alternative was embryotomy, which was found ethically hard to justify. The actual pathway of development was probably veterinary. Inspection of ritually killed animals led to a knowledge of anatomy and veterinary surgery included hysterectomy. About the offspring of animals as well as about human children, there was a discussion as to which was the 'first-born that openeth the womb,' if the elder was delivered by cæsarean section and the younger by the normal route. The same midwives attended both domestic beasts and women.

The Romans and Indians rescued infants from the wombs of dead or moribund mothers; Greece and Egypt did not, it seems, know even of this operation of *post mortem* cæsarean section. From Mesopotamia and its neighbourhood we have only a single legend: the birth of the hero Rustum. It is possible that the Jews themselves evolved the operation, probably at some time after the exile, perhaps in the Roman period.

The loss of the skill, like its acquisition, must remain a subject of speculation. Disuse would have resulted from an increasing number of failures. These could have been due to defective transmission of technique. The defective transmission itself could have been due to a need for secrecy. In the Moslem world the operation, even after the mother's death, was strictly forbidden. In the Christian world, the *post mortem* operation is not recorded until the tenth century, and to perform cæsarean section successfully on a living mother could have invoked the wrath of popular superstition. If the operation was driven underground, as a hole-and-corner procedure, the results might well have become more and more discouraging, until the operation was finally abandoned.

SHORTER NOTES

7 A Note on Bird Cries and Other Sounds in Zande.

By Professor E. E. Evans-Pritchard, M.A., Ph.D., F.B.A., Institute of Social Anthropology, University of Oxford

This note is written less for any intrinsic value of the information which it provides than in the hope that if others are thereby persuaded to publish similar information from other African peoples an interesting study may be made of how different peoples hear the same sounds in their different phonetic systems. Most of the cries and other sounds recorded can be heard over a great part of the continent. Only a few comments are required. The names of a number of the birds, like our 'cuckoo' and 'tit,' have an obvious onomatopœic origin, *e.g. titi, tutu, kukuru* and *zogozogo*. In the case of some of the cries of creatures the tone and syllables of the cries have suggested sentences to Azande, and the sentences react on the hearing of the cries so that though at first it may not, to a European ear at any rate, appear that the notes of the laughing dove sound very much like *urugu molu akpi akpi*, as the listener supplies the words to match them it begins to seem to him that that is what the dove is crying, and that is what he then hears. Likewise one begins to hear the sunbird saying *i na beda mangua*, 'they are sharpening an axe.'

Some of these sentences—in which grammar is subordinated to sound—derive from the habits of the creatures. For example, kites swoop down to the floor of homesteads to steal the termites which people have left to dry in the sun, so that it is a common practice to plant a spear near them to frighten the birds. The words *zinga* (*zanga*) *mangu*, which can be translated 'lack of bag,' in its cry derive from this habit—the bird is bewailing that it has no bag to put the termites in. Other examples are the reference to jumping away from a dog in the cry of the grey duiker, which derives from the fear which this animal has of hunting dogs; and the pearl-spotted owlet cries *rago ta gira si ni gbere ti re*, 'when daylight comes, I do not like it,' because it is a night-loving bird. Sometimes the cries contain what the Azande call a *sanza*, a sort of double talk, *e.g.* when the francolin cries *du wari du wari? u wari u wari?* 'Is where? Is where? Where is she? Where is she?,' she refers to herself and is saying to the people 'I am in dense bush, so catch me if you can!' I must add that the lists below are illustrative; they are far from being exhaustive.

Birds

(1) Black-and-white-tailed hornbill (*suangi*) cries *ki ki ki ki ki ki* and also *suangi suangi*. (2) Bulbul (*kpotoro*) cries *kpele kpele kpele* and also *geregada geregada*, *geregada* being a fruit on which it feeds. (3) Cisticola warbler (*titi*) cries *titi ti ti ti ti*, and it also mocks a wild cat (*dagbura*), crying *dagbura di di di di*. (4) Sunbird or honey-sucker (*ndege*) cries *zikui zikui zikui i na beda mangua peku peku peku peku*. (5) Pearl-spotted owlet (*koro*) cries *koo koo koo* and *koroo koroo* and *koroo, rago ta gira si ni gbere ti re*. (6) Pied wagtail (*ngbia, ngbiangbia*) cires *gui gui gui* and *gui toto gwi toto* and *ngbia gui ke*. (7) Swallow (*mbirambira*) only makes a cry in the season of termites, *pi pi pi pi*. (8) Francolin (*gbate*) cries *karu karu karu karu* and *du wari du wari, u wari u wari*. (9) Senegal coucal or cuckoo (*tutu*) cries *tu tu tu tu tu*. (10) Kite (*bakiki*) cries *kpi li zinga mangu* and *kiu kiu kiu* and *kiu kiu kiu kpii kpii kpii*, and the sound of its wings is *fo fo*. (11) Pin-tailed wydah finch (*balingilingi*) cries *kpi kpi kpi kpi*. (12) Black and white casqued hornbill (*ngongo*) cries *hua hua hua hua, wari o wari oo wari oo*. (13) Sudan brown parrot (*kukuru*) cries *kukuru o uu kukuru o uu*. (14) Eared owl (*bazizi*), a witch-bird, cries *hm hm hm*. (15) Vulture (*nguali*) cries *kokokoko kokokoko* and *kue kue*. (16) Babbler (*zogozogo*) cries *zog zog zog*. (17) Sparrow (*bavurukpolo*) chirps *kr kr kr kr* and *kuri kuri*. (18) Domestic fowl (hen)says *ku ku ku ku ku* and *kori kori kori kori*. When she has laid an egg she cries *mi na ka a ka kua kua kua, mi na oka a oko*, and when she calls her chicks she clucks *gbu gbu gbu gbu gbutu* (*gbu* is to hide or take cover). Cocks crow *ko ko li o ko yaaaa*. (19) Scaly francolin (*gurungbe*) cries *gurungbe sosoro, gurungbe sosoro*. *Sosoro* is another name for the same bird. (20) Mourning dove (*gbungu*) cries *ra giri mi zo tikpo wu wu, mi na mbu nga te*, meaning, At dawn I make salt wu wu, I do not tire. (21) Bronze mannikin (*nzoro*) cries *nzo nzo nzo nzo*. Plantain-eater (*koko*) cries *ko ko ko ko*. (22) Hammer-headed stork (*ete*) cries *ete ete ete ete* and also *gu zile na nye do yo mi a li ru na katakpa ru*, meaning, that bird which stays there, I will eat it all up. (23) Laughing dove (*mbipo*) calls *ku ku ku kuk* and *mi teterenge fuo ko, mi teterenge fue ko*, which appears to mean, I limp behind him, I limp behind him. It also cries *urugu molu akpi akpi*, meaning, the planter of eleusine will die, and *ra gili mo zo tikpo*, at dawn, make salt (an admonition to people to get to work early). When the dry season, its breeding season, approaches it changes its note to *rakataru rakataru*, and the bird is consequently sometimes named *rakataru*. (24) Ethiopian thrush (*taramani*) is the *bayango*, composer and song-leader of the birds. Azande say that it has no special call of its own but mimics other birds. However, they also say that it admonishes people, saying *he he he he de ni kuagi kuaga*, which is twisted into a sentence meaning that a woman is breaking off firewood; *tu ime tu ime, mo mungo mo mungo, de te de te*, meaning, draw water, draw water, you are idle, you are idle, [you are] no woman, [you are] no woman; etc. It sometimes calls out the name of the scaly francolin, copying the cry of that bird: *gurungbe sosoro, gurungbe sosoro*. (25) Guinea fowl (*nzengu*) calls *sukue sukue sukue* and *ngenge ngenge nge* and *uru wari? Uru wari?* This means, where is the sun, where is the sun? When it sees people it says *ke ke ke*, and when angry, *boro gbigbita pai, boro gbigbita pai*, a real bad business, a real bad business. When it roosts its call is a whistle followed by *mo ki yeri da gbura, mo ki yeri da gbura*, a phrase which has reference to the bird's habit of crouching to the earth (*gbura*) to avoid observation. (26) Honey-guide (*turugba*) is a great chatterer. It cries *kesa kesa kesa kesa* and *viki viki viki viki*. It also calls *turugba akpi ooo. no wo, mi na li nga anyege aliyo te. gbundagbunda boro mo yugu ra fe re go yo*, a sentence which starts with conventional exclamations and continues: I don't eat honey in the air. Mutilated man, you show it to me in a termite mound. The sound of its wings when it flaps them as bees attack it is *fuku fuku fuku fuku*. (27) Red-breasted cuckoo (*baduakporo*) cries *baduakporo*, a word meaning builder of homestead. (28) Black crested hawk-eagle (*mbadabare*) cries *o o o, de ne na wili nara*, sounds which are made to bear the concealed meaning, *mo na ndu na wilo ku uru yo*, you go with your child in the daytime. It is said that if one calls to this bird and tells it to look in one or other direction it will turn its head in that direction, a statement which I have often found to be correct.

Animals

(1) Hyæna (*zege*) cries *hi i i hi i i*. (2) Leopard (*moma*) breathes out through its nose *hhh* and then in through its nose *mmm*; and (3) lion (*bahu*) likewise. (4) Bushbuck (*gbodi*) cries *gboh gboh gboh*, and, when angry, *dio ooo dio ooo*, and when wounded *biau oooo oooo*. (5) Buffalo (*gbe*) cries *ho ooo* and, when angry, *uru uru fuu*. (6) Hippopotamus (*dupo*) cries *hu wo ho ho ho ho*. (7) The domestic dog (*ango*) barks *bau uuu* and *ho ho ho ho ho* and if struck it whines *hai hai hai i hai i*. (8) Wild cat (*dandala*) whines *niau niau*. (9) Warthog (*zigba*) cries *kuru kuru fu kuru fu* and, when excited, *wiii wiii*. (10) Red pig (*zukumbire*) cries *fu fu fu, eyuu eyuu* and, when angry, *hwo ho* or *wiii wiii*. (11) Waterbuck (*ngbaga*) cries *vu vu vu*. (12) Jackson's hartebeest (*nzungba*) cries *kisi kisi kisi* and, when excited, *fu u*. (13) Uganda cob (*ngbimo*) cries *huyu huyu* and, when excited, *ioo ioo*. (14) Gazelle (*gbangbalia*) cries *fiu fiu fiu* and, when excited, *dio ooo dio ooo*. (15) Grey duiker (*mvuru*) makes the sound *gubu gubu* when it runs. Its cry is said to be *niau niau mo guali ku ali yo be ango, niau niau*, you jump away from the dog, and, as it rocks to and fro when eating mushrooms, *baza o ngba nunguro*, tortoise o, good is your luck. (16) Colobus monkey (*mvugo*) cries at dawn *garr garr garr*. (17) Chimpanzee (*batangua*) cries *uur uur uur* and hits wood with hands *gidi gidi gidi gidi*. (18) Grey monkey (*mbiro*) cries *kisa kisa kisa miau miau*.

Other Sounds

(1) Stone grinding eleusine sounds *ke ke kiya kiya*. (2) Stone grinding sesame sounds *ke keke ke keke*. (2) Clapping hands sound *kpa kpa kpa*. (4) The wind says *gu gu gu gu go woooo*, and (5) a mighty wind *wiyu wiyu fiu fiu*. A whistle sounds *fia wa angbia*. (6) The sound of walking feet is *kata kata kata kata*. (7) An axe cutting down a tree makes the noises *ngbingi lili* and *go go go*, and (8) a falling tree *ndiiiii nduuuuu*. (9) A hoe says *ngbinginzili*. (10) Fire says *rorororoo*, (11) the burning thatch of a roof *guuuuu*, and (12) a bush fire *tutututu*. (13) A pestle pounding grain in an upright mortar goes *gibi gibi gibi* and (14) one pounding grain in the old-fashioned trough mortar goes *be be be be*. (15) Rain falls *bada bada bada*. (16) Beer ferments in pot *kpanguru kpanguru*. (17) Coins fall *ngbe ngbe, nge nge*. (18) Dark caverns at heads of streams pulsate *lililililili*. (19) Wailing at death sounds *guu* and (20) a crowd shouting, *wowo*. (21) *Guu* is also the buzzing of bees and the sound of rushing water. (22) *Gbugubugbugubu* is the sound a man makes when drinking gulpingly. (23) Roasting maize pops *kpuu*.

8 Prehistoric Hand Adzes from Gran Canaria: Contribution No. 6 of the Geochronological Expedition to the Canary Islands. *By F. E. Zeuner, D.Sc., Ph.D., Department of Environmental Archæology, Institute of Archæology, University of London. With four text figures*

It must be admitted that, on the whole, the stone industries of the prehistoric inhabitants of the Canary Islands were of a very poor quality. Whilst an assessment of the possible affinities of these industries can only be based on the analysis of complete collections from known sites, which is being undertaken in the Department of Environmental Archæology of London University, there are three types of implements which are unusual and deserve special consideration. One is a polished celt resembling very closely the European types of the Neolithic and Bronze Age, a report on which will be published separately when the study of the type of rock used has been completed. The others are large triangular stone tools, and of these two types exist, the ground type being the most remarkable. It is best called a hand adze, whilst the other is a flaked pick. It would be interesting to know whether similar implements have been found elsewhere, especially in Africa.

The hand adze is confined to the island of Gran Canaria, as are so many other cultural items. The red-burnished-pottery culture (MAN, 1960, 50) is peculiar to this island, too, and one is tempted, therefore, to suspect that the adzes belong to this same culture. Unfortunately, the pecked and ground specimens have so far been found singly, except for one from Roque Acuario, Los Coralillos, near Agüimes, encountered by Don José Naranjo of the Museo Canario together with stone tools and fragments of pottery. The stone tools comprise ordinary flakes, including long ones, a conical core of Upper Palæolithic aspect and a bifacially prepared discoid. These are in no way distinguished from the types found at Cuatro Puertas, near Telde, which will be described elsewhere. The pottery finds comprise 15 pieces which are coarse and yellowish, some with a wash, but no slip, whilst the six remaining pieces, four of which have a red or reddish slip, are pebble-burnished. Though of much coarser manufacture than the beautiful burnished ware from Tara-Telde, they establish a link. No chronological differentiation is as yet possible.

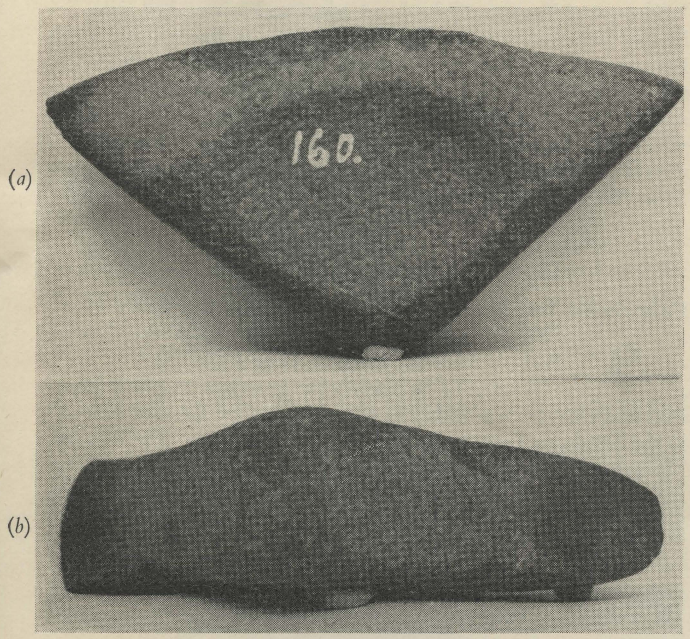

FIG. 1. HAND ADZE FROM LA GUIRRA, GRAN CANARIA

Side and top views of No. 160 (Museo Canario). Note the concave right-hand edge on (b). *Length 18·5 centimetres*

Unground specimens of triangular stone tools are much more frequent than ground ones. They could be regarded as rough-outs for the manufacture of ground specimens. Some, however, show evidence of use as picks, and they cannot be classed with the adzes. They have been found in association with both stone tools and pottery in several places, such as Cuatro Puertas. There is no indication that they are older than the ground adzes, and both appear to be connected with the red burnished-pottery culture. The only observation that might suggest an early phase is the poor quality of the pottery at Los Coralillos and Cuatro Puertas.

The hand adzes. In size the hand adzes (figs. 1 and 2) are remarkably constant. The largest two, one from Gáldar (No. 163), the other from Los Coralillos, are identical in size and shape to an extent that suggests that weight was an important property of the tool. Similarly, the smaller hand adzes are of the same size:

SITE	*Length, mm.*	*Height, mm.*	*Width, mm.*
Gáldar, No. 163	239	119	56
Los Coralillos	236	116	57
La Guirra, No. 160	185	98	53
Gáldar, No. 164 (ends worn)	172+	116	52
Tirajana, No. 162 (ends worn)	182+	97	53

All known specimens are made of a dense phonolitic or basaltic rock, by pecking and grinding. The maximum thickness (52–57 mm.) is evidently adapted to the hand using the tool, and so are the artificial concavities carefully worked on each side by pecking and grinding (fig. 1*a*, fig. 2*b*). The tools are very smooth, but not polished except at the two working ends. The long side of the triangular tool is always slightly convex and somewhat keeled. One working end is polished to form a straight cutting edge at right angles to the long axis of the tool. The other end, however, is left round below, and ground and polished into a broad concave groove above. There can be little doubt that this was meant to function as a gouge, and that the tool was thus used as an adze at one end and as a gouge at the other. In addition, one of the two large adzes (No. 163) has been chipped in the middle of the long convex cutting edge, presumably for use as a chopper. These tools were eminently practical; one could hardly design better ones for the trimming of large pieces of wood.

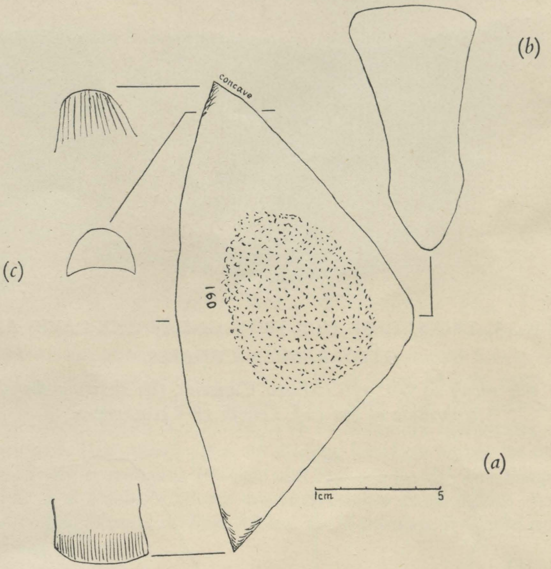

FIG. 2. FURTHER VIEWS OF HAND ADZE NO. 160

Side view, cross-section across middle, and cross-section one cm. from the tips. For size see text

When the tools had become blunt, they were either discarded (Nos. 162, 164) or else coarsely reflaked along the long edge and the ends, to be used as choppers (Nos. 159, 161, both from Gáldar).

So far, few of these remarkable tools are known, namely four from an unspecified locality near Gáldar in the north, one from La Guirra (Monte Lentiscal), one from Tirajana, and one from Los Coralillos, Roque Acuario, in the south of the island. This wide distribution suggests that they were generally known.

Triangular picks. The same idea of using the weight of a triangular tool held in the hand to produce maximum force is behind the shape of the implements here called 'triangular picks' (figs. 3 and 4). They differ from the hand adzes in being flaked only (not pecked, ground or polished), but basically their shape is the same, broadest where the two short sides of the triangle meet. They vary more in size, being often heavier and longer. There is no central depression for the fingers, and one end is often longer than the other. The long end is the working end, terminating (in complete specimens) in a point which is often worn smooth. The other end is often obliquely truncated (fig. 3*b*). It may have been used hammer fashion, but the truncation is not always present. The illustrations are self-explanatory. The sizes of large and small examples are as follows:—

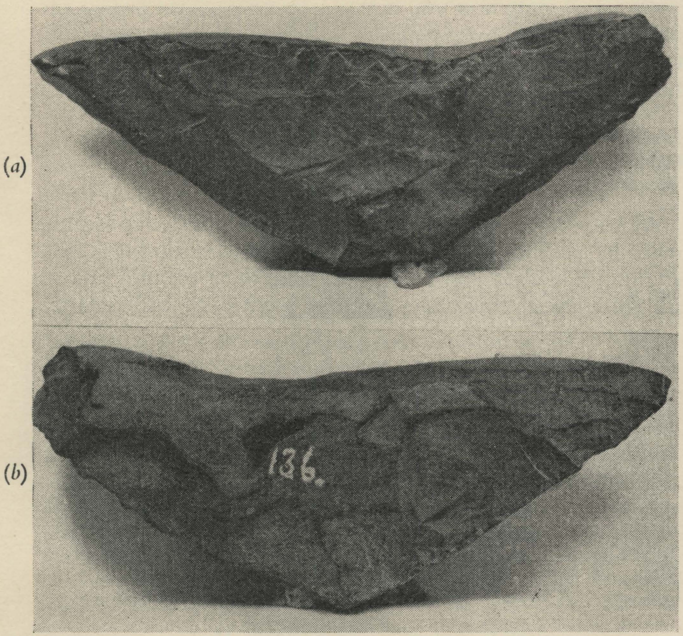

Fig. 3. TRIANGULAR PICK FROM ONE OF THE CAVES AT GÁLDAR, GRAN CANARIA

Two side views of No. 136 (Museo Canario), (b) *showing the oblique truncation of one end. Length 28·8 centimetres*

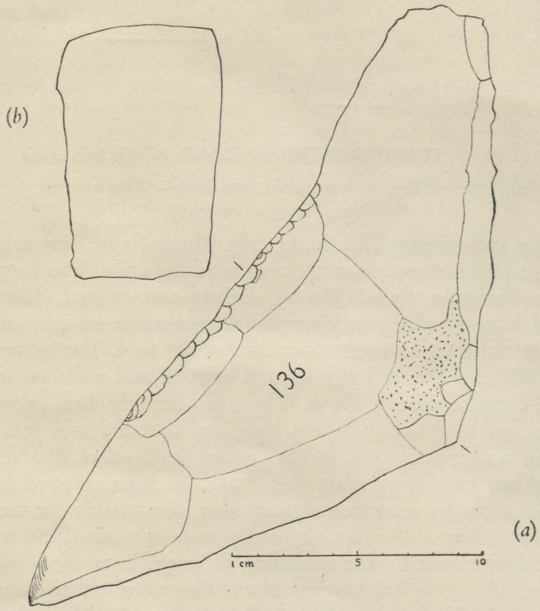

Fig. 4. FURTHER VIEWS OF TRIANGULAR PICK NO. 136

Side view, like fig. 3, and cross-section. For size see text

SITE	*Length, mm.*	*Height, mm.*	*Width, mm.*
Gáldar, No. 136	288	104	70
Gáldar, No. 131	170	101	86

The great interest of the hand adzes and triangular picks lies in the fact that nothing resembling them is known from the European Palæolithic or Neolithic. They may have been developed locally in Gran Canaria. On the other hand, since they appear associated with the red brunished pottery of that island, which is a late-comer and probably has African connexions, it would be interesting to know whether similar tools are known from North or West Africa.

Palæolithic Implements from the Rub' al Khali. *By Henry Field, B.A., M.A., D.Sc. Oxon., Research Fellow, Peabody Museum, Harvard. With a text figure*

9

A series of 65 palæolithic quartzite and flint handaxes and cleavers was found in March, 1960, by Edward Francis[1] in the Southern Tuwayq Quadrangle, Saudi Arabia. The locality, known as Nuhaydayn al-Qawnasah[2] (lat. 20° 27′ N. and long. 46° 33′ E.), stands in an area called Al-Qawnasat ibn Ghudayyan. This area lies about 65 miles east of As-Sulayyil.

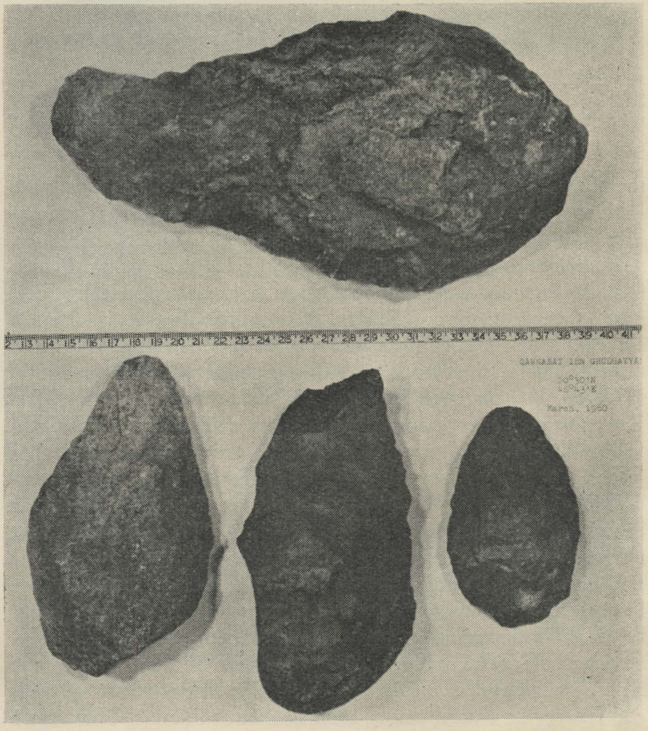

Fig. 1. LOWER PALÆOLITHIC IMPLEMENTS FROM THE RUB' AL KHALI

Photograph: Don Holm, 1960

In fig. 1 the largest specimen (14·0× 7·5× 4·0 cms.) indicates a powerful user. The quality of the quartzite in the smaller band-axes or cleavers is fine-grained. The smaller ovates are rolled and show evidence of æolian action. The flints are medium dark brown to mahogany in colour and reveal marked desert varnish and deep patina. This is the first Lower Palæolithic site in Saudi Arabia. However, a large handaxe from Duwadami[3] was described by Cornwall[4] in 1946.

In three localities, extending for almost 30 miles and lying south-east and east of Nuhaydayn al-Qawnasah, flint and quartzite arrowheads were found on the surface of low mounds standing 500 feet above sea level. The westernmost site is in the centre of a sandy ridge known as 'Uruq-ar-Rumaylah; the other two stand close together on the northern fringe of a sandy area (elevation 450 feet) north of the 'Irq Abu Faqar.

Attention must be called to the finely grained quartzite spear-point[5] (24·0× 7·5× 0·75 cms.) with long, wide pressure flakes on one side found east of As-Sulayyil (lat. 20° 3′ N. and long. 46° 15′ E.) by Don Holm of Aramco during October, 1949. This site lies south-west of Al 'Ubaylah midway between As-Sulayyil and Nuhaydayn al-Qawnasah.

Another archæological surface site,[6] yielding rather poor-quality artifacts of 'neolithic' or later periods, was located recently in the eastern Rub' al Khali at ST-17 camp (lat. 19° 41′ N. and long. 54° E.). Fragments of stone bowls or mortars and a piece of possible meteorite were found west of this Aramco camp.

These new discoveries supplement previous finds[7] in the Rub' al Khali, but now we know that the palæolithic hunters roamed this great area.

Lower Palæolithic implements have also been found[8] in Sinai, Israel, Jordan, Syria, Lebanon, Anatolia, Iraq, Iran[9] and the Caucasus. Presumably there were contemporaneous cultural contacts between the peoples of the Arabian Peninsula and dwellers in the Horn of Africa[10] extending into East and South Africa, north-west into the Nile Valley, north into Anatolia and north-east into Iraq.

This new palæolithic discovery is of the greatest significance and forms an important link in the chain of Stone Age sites now being plotted on the map of South-Western Asia.

Notes

[1] Geophysical Service, Inc., and members of Ruel D. Gierhart's seismographic party. Permission to publish these finds was granted by Aramco. Gratitude must be expressed to Mr. O. A. Seager for permission to publish these notes and photograph and to Mr. Don Holm for sending specimens and 20 photographs. All specimens are on loan from Mr. Francis in Peabody Museum, Harvard University.

[2] See Geographic Map of the Southern Tuwayk Quadrangle, Kingdom of Saudi Arabia, by Richard A. Bramkamp, Ruel D. Gierhart, Glen F. Brown and Roy O. Jackson, Miscellaneous Geologic Investigations, Map I–212B, scale 1 : 500,000, prepared by the U.S. Geological Survey and the Arabian Oil Company under the joint sponsorship of the Kingdom of Saudi Arabia and the U.S. Department of State. Published by U.S. Geological Survey, 1956.

[3] No. 30 on Map of Archæological Sites of Arabia by H. Field in *Ancient and Modern Man in Southwestern Asia*, 1956, University of Miami Press, Coral Gables, Florida.

[4] Peter B. Cornwall, MAN, 1946; *J. R. Geog. Soc.*, Vol. CVII, 1946, pp. 28–50.

[5] See No. 71 on Map in Field, 1956. This is the finest specimen I have seen from South-Western Asia.

[6] Described in a letter from Don Holm in Dhahran dated 8 May, 1960.

[7] F. E. Zeuner, '"Neolithic" Sites from the Rub' al Khali, Southern Arabia,' MAN, 1954, 209, with bibliography; H. Field, 'New Stone Age Sites in the Arabian Peninsula,' MAN, 1955, 145; H. Field, 'Stone Implements from the Rub' al Khali, Southern Arabia,' MAN, 1958, 121; H. Field, 'Stone Implements from the Rub' al Khali,' MAN, 1960, 30.

[8] H. Field, *Ancient and Modern Man in Southwestern Asia*, 1956 (see note 3), with bibliographies.

[9] Dr. Robert J. Braidwood and assistants found a handaxe near Kermanshah early in 1960. This was shown by Dr. Ezat O. Neghaban, Director of the Institute of Archæology, University of Tehran, to the members of the Fourth International Congress of Iranian Art and Archæology in New York City, March, 1960.

[10] Dr. Gertrude Caton-Thompson writes that the palæolithic evidence from South Arabia seems to be definitely against any connexion with East Africa after, or during, handaxe times, let us say —tentatively correlating Pluvials and low sea levels—the Kanjeran Pluvial. If a Pebble Culture reached Arabia from Africa during the preceding Pluvial of East Africa, the Kamasian, it would not be surprising.

CORRESPONDENCE

Music and Diffusion. *Cf.* J. R. Anthrop. Inst., *Vol. LXXXIX, Part* 2 (1959), *pp.* 155–68

10 SIR,—I feel that some of the musical evidence in the Revd. A. M. Jones's recent article 'Indonesia and Africa: The Xylophone as a Culture-Indicator' ought to be commented on; the other non-musical evidence, convincing as it is, is not essential to his theme.

Firstly, it must be remembered that we are dealing with peoples most of whom are, musically, extremely sensitive and pitch-conscious, and able to reproduce at a distance not only the degrees of a scale but also its exact pitch to an extraordinary degree of accuracy. Such peoples as the Chopi, when they leave their homes for the mines, do not normally take their xylophones with them; they make them on the mines, and with nothing more than memory, or 'perfect pitch,' they tune them to within one vibration per second of those at home. Others are equally fastidious in the precise tuning of their instruments. Jones's main thesis, that Siamese-Indonesian *pelog* and *slendro* tunings are 'the same' as African tunings, is not borne out by the figures which he quotes himself, some of which differ from each other by such relatively large amounts as 74 cents, or ¾ of a tempered semitone. To any of the musicians involved, the other scales would quite definitely feel 'out of tune,' and as such, it is doubtful whether they can immediately be considered as 'the same.' Jones's theory depends on the supposed colonists, and the Africans themselves, having a particularly sensitive ear for pitch, yet the differences in the scales are easily appreciable by any musical ear. They make the whole scale sound different. Jones himself explains that the 'makers had no scientific instruments by which to regulate their tuning,' thus indicating that the choice of scale lies in the ear alone.

Throughout there is the assumption that the African normally uses 'nature's own scale,' *i.e.* a Western scale, one whose octave has seven intervals containing five whole tones and two semitones, and that anything which differs from that is automatically unnatural or artificial and must have been 'influenced' from somewhere else. This is starting from the wrong premise. Measurement of a large number of African scales, such as has been made by the International Library of African Music, shows that there are a myriad of different scales in common use among Africans, of which few approach our diatonic major scale, either in mood or in the exact tuning. One of the main reasons for this is that only approximately 40 per cent. of African tribes use heptatonic scales at all, the other 60 per cent. using one-third hexatonic and two-thirds pentatonic. (And there are a few border tribes which use more than one type of scale. The figures are from the I.L.A.M.'s measurements of about 80 tribes in the area south of the Sudan.) However, anyone who regularly listens to any type of African music will tell you that it is all too easy to approximate one's ear to the notes which one hears, imagining them to be those of our own scale. It is only when one comes to playing transcriptions of them onto our stave, on a piano, or some such fixed-tone instrument, that one hears how completely wrong the tuning can sound.

Much depends on what factors are considered to decide on the choice of a scale.

Choice is the right word here for, from our evidence in Africa, scales, or tunings, are inherent in a person, and *cannot be learnt*, so that a man will naturally choose the scale that seems 'right' to him whether he is judging someone else's music or making his own. There may be some cases where scales have been learnt to a tolerable degree of accuracy but then only with extreme difficulty. You only have to hear any Negro singing, whether he has been trained in Western music or not, to know immediately that he is a Negro. Whether preference for scales is caused by early training or by heredity it is almost impossible to say, but one cannot discard the possibility of it being the one or the other. Whichever it may be, the only practical way of passing on scales is by intermarriage. Therefore, Africans cannot have *memorized* Indonesian scales exactly; they would straightway have reverted to their own set of scales, or at least have co-ordinated the new scales into their own until they were almost unrecognizable—that is, unless the two scales were similar to start with. This is to judge by their reaction to both Arabic and the various types of European music which have impinged upon the continent from very early times. There is no reason to suppose that Indonesian scales are any more like African scales than European ones, in fact the opposite is probably more true; and yet look what liberties they take with our scale!

Therefore, assuming Indonesian colonization, these particular tunings must have been caused by intermarriage. Again, assuming that the choice of notes in a scale is hereditary—we do not have to, but it is convenient (also the faculties of musical ability and musical appreciation—Otto Ortman in *The Effects of Music*)—this particular choice will depend on a gene, or a combination of genes. Now the area which is supposed to have been 'influenced' so radically in its musical practice is a huge one, the number of peoples in it vast, and only a relatively small number of Indonesians must have been able to come over the Indian Ocean, in whatever the size of boat (unless a mass migration over a land route is envisaged; but the distribution of the xylophones and tunings does not indicate this). Does Jones suggest then that all those peoples whom he has quoted as using equitonal heptatonic or pentatonic scales, numbering millions, have inherited the genes from this relatively small number of people who settled around the coasts of Africa, introducing possibly rice, and other items of material culture? Or is it not equally possible that these musical genetic traits may be shared equally by Africans and Indonesians, and date back far earlier than any possible influence from one country to the other?

In what sense does Jones mean that the equitonal scale is 'artificial'? The definition means not natural. But to several million people it is the only natural scale, and by no means all of them are able to explain their musical system as explicitly as the Chopi. Is it so 'astonishing' or 'staggering' to find *any* scale in the world that is not our 'nature's own scale'? And that itself is not in fact so natural as it seems to us. Although founded on the simple ratios of the harmonic series, it yet needs quite a process of abstraction to reach our major scale. The South African pentatonic tribes have also, according to Kirby, evolved their scales from the harmonic series of a plucked string, and there are considerable variations amongst them in tuning, as well as between any of their scales and say, our Hebridean scale. Further, although it may be a matter of opinion, one cannot without further evidence assume, following the ethnological principle, that the equitonal principle is so complex and sophisticated that it could only have originated in one part of the world. On the contrary, the principle itself is easy to grasp, and I believe that it is one which any people with any degree of conscious musicality could think out. There may even be something in the construction, lay-out and appearance of a xylophone, the African instrument whose tone is most clearly definable, which is conducive towards an equitonal tuning, in something like the same manner that flute tuning in Africa frequently depends on the symmetrical placing of the finger holes, rather than the conscious desire of the maker to produce a known scale.

So it should not be unreasonable to assume that two peoples might evolve separately a similar equitonal principle, especially where they are both to an unusual extent musically minded, which, from the opinion of many African observers, could well apply to Africa. But, as Jones's figures adequately show, the Indonesian and the African scales are not exactly the same thing, but differ even among themselves very considerably, although one may like to classify them loosely under the heading 'equitonal scales.'

In regard to the use of thirds in vocal music, one must not assume that because two peoples sing in thirds, they are necessarily connected. Here may be another example of separate development. After all, we Westerners make frequent use of thirds, and who would suggest Indonesian influence on European music? Another difficulty here is in defining the third; whether it is to be any interval of approximately 350 cents—our ear is notoriously tolerant in this respect—or the interval between any note in a scale and the second note from it. Thus some pentatonic scales have an interval between two adjacent notes which could be quite easily classed as a third in a heptatonic scale.

This leads on to the two types of Indonesian scale mentioned by Jones, the heptatonic *pelog*, and the pentatonic *slendro*, which are supposed to have been adopted in different parts of Africa. But why should Uganda and parts of the Congo, among others, have chosen the pentatonic xylophone to adopt while other parts chose the heptatonic xylophone? The only possible reason must be that those people who adopted each kind of xylophone must have already had a similar scale themselves, and that they chose the instrument most suited to their abilities. But in that case how were the notes of their primitive scales distributed?

Further, if, as Jones suggests, Indonesian influence has been present not only in the tuning of xylophones, but in their presence in Africa at all, why are not all the African xylophone scales equitonal? There are many African xylophone tunings that are not approximations to equitonal scales. Jones says (p. 158): 'It would of course be possible to adduce other xylophone tunings which are not so near.' There is no logical reason why the tunings which Jones has chosen to give should not be an approximation towards those other tunings 'which are not so near,' just as much as towards an Indonesian tuning. Any conclusions based on such a minutely variable, psychological thing as tuning must be supported by more than a few examples selected seemingly at random.

This seems to be the case also with Jones's theory of 'perfect pitch' on both sides of the Indian Ocean. For all the examples given, one could quote many more where equitonally tuned heptatonic xylophones have no note approaching 184 v.p.s. or where pentatonic xylophones have none approaching 270 v.p.s. In fact the I.L.A.M. has several Ganda and Nyoro xylophone tunings that have no note within as much as a semitone of the 284.5 v.p.s. quoted for one particular Ganda xylophone. If the particular frequency to be chosen as featuring in a large number of scales appears as the lowest note in one, the second in another, and the fourth in another, does that not indicate that that note is thought of in a quite different way in each case? We need to see examples of the actual music performed, as well as of the tuning. We could find thousands of instruments, not only in Africa, which use a frequency of around 184 v.p.s., but we could not then deduce a common influence.

This should not be construed as a categorical denial that there was ever any Indonesian influence in Africa; there is plenty of good evidence to the contrary. But I would suggest that such confident statements as 'The consistent evidence of the musical features points to one conclusion; that Indonesian colonists settled on the East coast of Africa . . . etc.' are not supported by the facts at the author's disposal.

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Descent, Filiation and Affinity. *Cf.* Man, 1957, 59; 1959, 309, 331; 1960, 6

11 Sir,—May I be permitted to comment on the discussion between Dr. Leach and Professor Meyer Fortes, taking it as part of a debate between two theories, namely the theory of lineage and political systems on the one hand, and the alliance theory, or, I should prefer to say, the structural

theory of kinship, on the other? There are signs that the debate is in the air at present, as the fact, for instance, that one issue of the *Journal of the Royal Anthropological Institute* (Vol. LXXXIX, Part 1) contains three major articles bearing directly or indirectly on the question seems to show. At the same time we have reached a stage where what is meaningful for one school sometimes seems to the other to be devoid of interest. The dialogue is timely. I think that the more fully and thoroughly it develops, the better for the future of our studies, and I am concerned with trying to suggest some conditions and possibilities of its development. From this point of view, Prefessor Fortes's position seems unfortunate in so far as, in response to Dr. Leach's effort towards reciprocal understanding, he restates one theory in such a way as to leave no room for the other: in effect, I conclude from his double article that, according to him, for theoretical purposes positive marriage rules do not exist.

Whatever the relative weight which one likes to give to these theories and their achievements to date, it is wiser to recognize that in their essentials both are here to stay and are not incompatible (although, as they involve different assumptions, they may appear to be so). This is because they are intended for *different purposes*, and achieve their purpose for *different types of societies*. In the first place, the clash is possible only when, having successfully detached from kinship proper a theory of lineage systems (and of a certain type of political systems) one claims to turn back upon kinship itself and reduce it in essentials to those elements which had been detached from it on a different level. If one renounces this doubtful procedure, there is no incompatibility in principle between a structural study of kinship on one level, and a study of the lineage system on the other. But then comes the link between a certain type of analysis and a certain type of society: one type has been developed in Africa, while South-East Asian ground seems to favour the other. This is typical of anthropology at the present stage. The science has not yet reached universal formulations which would hold experimentally; we are as yet on a 'low level of abstraction.' A rigid exclusion of one theory by the tenants of the other, a tendency to generalize from one's own experience while disregarding others' will not help.

On the contrary, each theory can gain from the other an awareness of the basic assumptions which it involves and of the limits and conditions of its experimental validity. Regarding assumptions, there are differences between one author and another; for instance within the lineage theory Professor Evans-Pritchard is more of a structuralist than others; in particular he stresses segmentation where Professor Meyer Fortes stresses corporateness (*cf.* M. G. Smith, *J. R. Anthrop. Inst.*, Vol. LXXXVI, Part 2, 1956, pp. 59 f.).

Regarding the limits of the theories, Dr. Barth's remarkable article on the Pathans (*J. R. Anthrop. Inst.*, Vol. LXXXIX, Part I) offers an example, as it presents apparently a stumbling block to both. On the one hand the Pathans' 'lineages' never fuse, on the other they do not distinguish in the structural sense consanguinity from affinity, as they marry with ease their father's brother's daughter (so that we cannot speak of a 'marriage preference' in the ordinary sense). But are not these two aspects connected? One could say that their lineage is such only by a great extension of the term, as one loses membership by losing one's share of the group's land: it is a land-owning corporation based on a unilineal chart, and this is instructive. At the same time the absence of exogamy, the presence of what would be called affinal links within even small segments of the lineage is probably related to this lack of unity in the lineage (*cf.* Kasdan Murphy, *Amer. Anthrop.*, 1959, No. 2, p. 24). Quite conjecturally, I would conclude that the theory of lineage groups cannot be divorced from their affinal background.

It is probably too early to speculate whether the two broad models which the theories deliver could be taken as particular cases of a more general, and more complex, theory. But it is perhaps permissible to wander about a little in order to suggest such a possibility. Let us suppose that some people (say, the Kachin) put a stress on affinity and others (say, the Nuer) on lineages and their permanence. This does not prevent the former from having lineages and the latter affinity relationships. Then one might suggest, at one's own risk, that the symbolic significance of cattle among the Nuer has its roots not only in the conscious identification of the lineage with its herd, but also at the same time in the empirical fact that the lineage herd materially embodies the marriage exchanges, so that descent and affinity are present or implied in it. Whatever may be the case, one can conceive a future combination and generalization of the theories developing along such lines.

One difficulty for the dialogue which I advocate lies in the technical terminology. As the controversy in MAN itself shows, there are signs of uneasiness on that score, even among the most extreme adherents of the descent theory. I think that it would be possible to show that the accepted meaning of the term 'descent,' *i.e.* 'transmission of membership in a group,' is too wide and leads to queer situations: what is for instance, according to that definition, a 'local descent group,' but a local group in which membership is transmitted in a certain way, *i.e.* simply an enduring local group? It is, however, obvious that in most usages of this expression 'descent' is actually taken in a more restricted sense. Among the wide variety of forms of 'descent' and 'descent groups,' and apart from considerations of localization, corporateness, etc., which receive due recognition, the criterion of exogamy is often, the structuralist feels, neglected to a large extent. For a structural study of kinship at any rate, it is absolutely essential, I believe, to distinguish between (membership in) an exogamous group and (in) a non-exogamous group. *Cf.* Dr. Leach's remark about 'lineage' in Arab societies, MAN, 1953, 279 (p. 180, col. 1).

On this point, the simplest way to make possible a dialogue between the two theories would be to restrict 'descent' to what is in fact its most frequent use, *i.e.* membership in an exogamous group, and use another word, say, 'filiation,' for the other cases. This would make immediately apparent the place of affinity which, as Dr. Leach has stated and Professor Fortes once more demonstrated, some descent theorists have a tendency to underestimate or to disguise.

In general, the time has perhaps come when the terminology of kinship studies can be refined on the basis of existing works and in view of future developments. It should not be impossible to adapt it to the essential requirements of both the points of view which I have referred to, not as a compromise between them, but rather as a sort of 'double entry' scheme in which both could accommodate the distinctions which they insist on making. The structuralist's needs can be easily stated, but to explore fully the possible alternatives and assumptions requires team work of a strenuous kind. At best perhaps a few staunch adherents from both sides, junior members of the same Anthropology Department, determined to lay the basis for reciprocal understanding, could initiate it. If the attempt were successful, the scheme would impose itself by its usefulness. If not, or not completely, successful, the attempt would have been enlightening for those taking part in it. If nothing is done, then every worker will have to solve the problem for himself, with the risk of communication becoming more and more difficult and the two theories blending unawares (in fact this process is already on the way), instead of a fertile difference bearing fruits in full daylight.

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Egypt and Africa

12 SIR,—It has become the fashion for modern writers on ancient religions to attribute to Egypt most, if not all, of the beliefs and rituals of the past. The writers of these books display ingorance of the physical and climatic conditions of Egypt and have clearly made no personal investigation into the evidence. They merely copy from books, whose authors have also been copyists, and with the usual perversity of human nature it is the mistakes and misinterpretations which are copied, generally because they are more dramatic. Consequently the chaotic pictures of Egyptian religion now presented 'surprises by itself.' I would point out that Egypt was and still is two separate countries. Upper Egypt consists of a rainless isolated valley watered by the single stream of the Nile, to which valley access could be had only along the course of the river. Whereas Lower Egypt is a plain watered by branches of the Nile, and having a long Mediterranean seaboard with many harbours. It is surely obvious that the influences coming into Upper Egypt would be African, the influences into Lower Egypt would be Mediterranean. Upper Egypt has been the scene of so many spectacular discoveries that little attention has been given

to the equal importance of Lower Egypt except to interpret the religion of Upper Egypt by the description of it by writers of a late period who knew only the Delta. But the evidence for the African influence is clear in Upper Egypt. The people of the predynastic Amratean period are proved by their pottery to be of African origin; the scented plam oil of the Gerzean predynastic period is African; the throne was regarded as a deity, so that when personified in a woman she became a deity, and the man who sat on the throne also became a deity. Us-yri (Osiri) the occupier of the Throne. This great field for research is entirely untouched. But the investigation should be undertaken not by a copyist, but by an Egypto-archæologist, specializing in the pre- and proto-dynastic periods, who will understand the evidence and interpret it correctly. Until some sure foundation of the Egyptian religion is laid, the copyist will reign supreme.

London M. A. MURRAY

REVIEWS

GENERAL

13 **The Antecedents of Man.** *By W. E. Le Gros Clark. Edinburgh* (*U. P.*), 1959. *Pp. vii*, 374. *Price* £1 1*s*. **The Foundations of Human Evolution.** *By W. E. Le Gros Clark. Eugene, Ore.* (*Oregon State System of Higher Education*), 1959. *Pp*. 77. *Price* $1

These two excellent volumes treat of human phylogeny and evolution in different ways. The shorter book is composed of a series of lectures, and traces, with the utmost clarity, the relationships of the various major natural groupings of the Primate order. The longer work, on the other hand, discusses the evidence for man's relationships with other primates by organs and systems, each chapter dealing with a variety of creatures. The two books, therefore, support one another in a most useful and convincing way.

The lectures, of course, are more suitable for the intelligent but previously uninformed non-specialist. Problems which are still in controversy, such as the taxonomic position of *Oreopithecus*, and of the Tarsiiforms, living and extinct, are treated most judiciously. Even the illustrations, showing the presumptive appearance of a number of extinct forms, are acceptable: the details shown are obviously the result of carefully reasoned, rather than of wild, speculation. The only weakness of this volume is its lack of explicit attention to the connexion between behaviour and evolution. In every other respect, it is completely up to date in attitude and breadth of coverage, and a pleasure to read.

The Antecedents of Man is a much fuller treatment, and far more technical, yet also written with clarity and vigour. As the author states, the treatment follows the main lines of *The Early Forerunners of Man*, but the book can in no sense be considered a revision of that earlier work. Indeed, the degree to which our knowledge and understanding have increased during the past generation is well demonstrated by a comparison of these two books by the same author. Advances in our knowledge of ecology and of population genetics have done much to clarify the palæontological evidence which has, itself, increased in volume. Many gaps in the record have now been filled in, and a much improved theoretical framework for understanding the processes of primate and human evolution now exists. The results of these advances show clearly in Professor Clark's work.

Following two introductory chapters, the evidence of the dentition is assessed in the third, and of the skull in the fourth. Inasmuch as from these parts of the body the bulk of our palæontological data must be drawn, the author quite properly discusses details extensively. Yet here, as throughout the book, the importance of relationships and of the total morphological pattern is stressed. It seems a pity that comparisons of *Australopithecus* and other primitive hominids are always with the Gorilla or Chimpanzee, rather than with the Orang-utan, of which the skull is, as the author says (p. 162), 'more human in aspect.' It is certainly to be hoped that one day a Dryopithecine skull may be discovered.

Chapter five, the longest of the book, deals with the evidence of the limbs; and, as in the previous two chapters, the characteristics of *Proconsul* are discussed at some length. In all known details, this extinct genus seems intermediate between apes and monkeys, and therefore, as close to the family of man himself as one can expect to find from the lower Miocene. The author is properly cautious in his discussion of the question of whether or not man's ancestors brachiated, but does not hesitate to class them as less similar to monkeys than to apes.

The four succeeding chapters, on the brain, the senses and the digestive and reproductive systems, are shorter. Fossil evidence for these is, of course, scanty if not completely lacking, so that comparisons have to be made between living forms. In all respects, however, this evidence is in agreement with that derived from the skeletal system. I, at any rate, found the discussion of the anatomical basis of stereoscopic vision to be the best that I have encountered. The final chapter summarizes the evolutionary radiations of the primates, insofar as these may be inferred at the present state of our knowledge. The book is excellently illustrated throughout, with clear, concise charts, diagrams and line drawings.

FREDERICK S. HULSE

14 **Men and Cultures: Selected Papers of the V International Congress of Anthropological and Ethnological Sciences at Philadelphia, 1956.** *Edited by a committee, chairman A. F. C. Wallace. Philadelphia* (*U. of Pennsylvania*) (*London, O.U.P.*), 1960. *Pp. xxxi*, 810. *Price* £6

This is a large collection of summaries of contributions from many parts of the world, including statements of present positions and trends in thought in many countries, U.S.S.R. being co-operative. Trends in U.S.A. are discussed rather too much in the light, or shadow, of Ph.D. dissertations, often, alas, on subjects chosen by university teachers rather than by the graduands. For Britain Firth reviews recent years, and wisely pleads for quiet digestion by critical examination of enormous masses of field notes. For U.S.S.R. Potekhin makes clear the political effort infused into the study of man.

Cultures and culture change are the dominant topics, with physical anthropology, archæology and linguistics as, in the main, accessories. One can do no more than note a few subjects. M. Jacobs is critical of habitual folklore studies and pleads for more comprehension of the dynamics of the entirety of the verbal arts, for folklore is the expression of a very largely oral tradition, and Margaret Mead also pleads for a new approach to folklore, emphasizing the vast changes now going on with migrants, refugees, urbanization and industrialization of peoples living until recently under ancient tradition. Loeb thinks that the Kuanyamu Ambo peoples of South-West Africa are an enclave of survivals of early Mediterranean African life brought by cattle-herders. Bratanič compares ploughs without mould boards in Yugoslavia and Scandinavia. Steensberg in the main supports F. G. Payne's view that field systems are often the results of plough types. J. S. Weiner argues for the evolution of both the Sapiens and the Neandertal types of man from a *Pithecanthropus* form. Olderogge studies the Hausa language and finds that in addition to recent borrowings of words from the Hamite-Semite group, there are deep grammatical resemblances, and he suggests a very ancient link of pre-desert Saharan days followed by long separation. H. J. FLEURE

15 **On Human Communications: A Review, a Survey and a Criticism.** *By Colin Cherry. Boston* (*Technology Press, Mass., Inst. of Techn.*), *New York* (*Wiley*) *and London* (*Chapman & Hall*), 1957. *Pp. xvi*, 333

During recent years, with the rapid development of telecommunication, the problems and results of linguistic science have become important also to the branch of physics which deals with communication. Those who want to obtain original and important results in the general science of communication must combine a knowledge of natural science with that of linguistics, a rather rare combination, at least in Europe. The present book by the Reader in Telecommunication in the Imperial College of Science of the University of London, is most welcome, the author being well acquainted with linguistic research. The book is written in a lively manner, with interesting glimpses of the history of the different devices and problems. It will probably be more useful to specialists in telecommunication, who will find in it a sure guide to the linguistic problems of importance to them, than to linguists, who will need quite a good knowledge of mathematics in order to be able to understand it completely, but even the reader who has not got that knowledge is able to see the nature of the problems raised though he will not be able to follow how they are solved.

The book consists of seven chapters. In the introductory chapter the author discusses the general nature of communication and of communication and organization, what we communicate—he has chosen 'sign' for any physical event used in communication. The chapter contains a good survey of the individual and social character of communication and goes on to show how there are three aspects of communication which have received clear mathematical treatment: the theory of networks, statistical communication theory and the theory of feedback (sometimes called cybernetics). The author is well aware of the general difference between physical and social phenomena. The concepts of time and space, for example, are highly abstract and universal; whereas form and space in sociology means history and geography (p. 25). He also shows how in modern science the synthetic point of view dominates: it is significant that the idea of the topographical graph has been applied to social groups and led to the setting-up of sociograms. The next chapter, devoted to the evolution of communication science, deals with language and codes and shows how from very early times information has been conveyed in a two-state code (*cf.* the bush telegraph or the Morse alphabet), a method which is a precursor of present-day binary coding (*cf.* also the 1,500-year-old Celtic Ogham script). With the need for a statistical view of language and code there is now a renewed interest in the principle of the Morse alphabet in which the most-used letters of the language are allocated the shortest dot-dash symbols. The chapter goes on to expound the mathematical theory of communication, the measurement of what is called information capacity, which may be defined strictly on a mathematical basis. The following chapter is for the most part devoted to purely linguistic matters, the question of form and meaning, speech and writing, phonetics and phonemics, object language and meta-language (that is language as an object of research and the scientific language discussing the object language). The author stresses the importance of the binary description of language, the distinctive features characteristic of phonemes and their binary attributes (according to Roman Jakobson and his collaborators). As far as the analysis of meaning is concerned I think that it is necessary to reckon not only with a *referent* (according to Ogden and Richards) or a *designatum*, but with what Hjelmslev calls a *content* between the sign and the referent; the same referent may have not only different *signs* but also different contents in different languages. Language form has also been the object of statistical research. 'Zipf's law,' that the most frequently used words are the shortest, can only be said to be a tendency. When conjunctions, for example, become too short as a result of phonemic reduction they are replaced by longer words. The author insists in this chapter and also elsewhere on the importance of 'redundancy.'

The next two chapters deal mostly with physical and physiological problems—the analysis of signs, especially speech and the statistical theory of communication. The author deals with the newest methods, with 'visible speech,' the spectrograph and spectrograms. He protests against the comparison often made between brains and *existing* digital computing machines—they have never been intended to serve as models of the brain.

The problems discussed in the last two chapters are of a more psychological and philosophical character. The author emphasizes the difference between the language systems set up by the logicians and the natural ones. He distinguishes between *events* and *types*; sign-events, word-events, tokens, signals denote physical transmissions on specific occasions, sign-types and word-types are linguistic concepts, the signs in the language (*cf.* the opposition *langue: parole* de F. de Saussure). He does not think that the Wiener-Shannon measure of the selective information rate of signals in terms of their statistical rarity is all that is needed, but he does not try to set up a different measure of information. He surveys some of the theoretical studies and arguments in the field, deals amongst other things with Pierce's pragmatic theory of signs, the recognition of universals and quantitative experiments on the intake of information by the senses and the search for invariants in pattern-recognition.

Two minor remarks: What is said on p. 70 about the character of dictionaries is just in general. The modern definition dictionary, however, dissolves the meaning of the word into what has been called its 'spectrum,' a group of other words which can take its place as far as distribution is concerned. And on p. 77, gesture-like signs are not altogether non-arbitrary: smiles and bared teeth, for example, may have different implications among different peoples, but everywhere they belong to a special category of signs.

The book is an excellent synthesis of the results of different sciences and ought to be studied not only by communication engineers and linguists, but by sociologists as well. It contains an excellent list of definitions of special terms, a good index and a very full bibliography.

ALF SOMMERFELT

16

Beyond Psychology. *By Otto Rank. New York (Dover Publ.) (London agents: Constable)*, 1959. *Pp.* 291. *Price* 14*s*.

This book was written by Rank shortly before his death in 1939, and the present edition is unaltered. The first part is devoted to his theory of 'The Double as Immortal Self,' in which he brings in twins, the shadow, Jekyll and Hyde, and other aspects of dual personality. The trouble is that his theory seems to have an inadequate basis in fact. Thus he says that 'Among primitives in Africa, where the custom [*sc.* of king-killing] has been preserved into our own times, the term of the king's office is usually limited to two years. When that period expires he is killed, usually by strangulation—a sacred duty performed by two executioners. In former times two slaves were killed in front of the royal tomb. It seems to me that the number two running through this ritual like a "*leitmotiv*" is reminiscent of the twin origin still preserved in the two lictors or consuls of historic Rome' (p. 105). But there were many more than two lictors, and I can find no example of an African king who reigned for two years or was strangled by two executioners.

After a discussion of Christian theology he goes on to totemism. This, he says, is 'the earliest system of soul-religion, called totemism from the Australian "totem," which signifies an animal, plant or object adopted as a symbol by certain tribes who consider themselves descendants of one and the same "totem." This totem animal is made responsible for the necessary supply of food.' In spite of his mention of 'object' he discusses totemism as if the totem were always a food animal and as if such totemism were universal among primitives (pp. 205f.).

When he comes to incest he assures us that 'the individual's inner resistance to the biological sex urge must be taken as one of the most fundamental facts of human life' (p. 213) and that 'because of his inner resistance to biological procreation primitive man practically tabued sex, or, at least, had to be induced by "law" to marry and produce children' (p. 213). We are not told who imposed this 'law' upon primitive man; perhaps it was the kings whom he strangled.

RAGLAN

AFRICA

17

David Livingstone: Family Letters, Vol. I, 1841–48; Vol. II, 1849–56. *Edited with an introduction by I. Schapera. London (Chatto & Windus)*, 1959. *Pp.* 320 *and* 266. *Price* £3

Professor Schapera has already demonstrated that he has no peer in the editing of the journals and letters of early visitors to South-Central Africa. His editions of *The Early Cape Hottentots* (with B. Farrington—from the writings of Dapper, Ten Rhyne and Grevenbroek); of Burchell's travels; and of Robert Moffat's *Apprenticeship at Kuruman*, are learned in a way that is, I think, unique in African scholarship. For he alone among editors of original documents such as these seems to bring to his task both comprehensive knowledge of literary and archival sources and also detailed knowledge of the African peoples among whom the authors of the documents moved. Hence he has the ability to show his characters inside the coming together of two different social systems: neither the world of the Africans nor the world of the Europeans is thrust into the background and obscured out of the editor's ignorance. His present edition of David Livingstone's family letters shows the same skill and scholarship, and it will enhance his already great reputation. I find it difficult to remember that these carefully edited letters, with the masterly and concise Introduction, are a by-product from Schapera's main work as a social anthropologist.

This collection of letters is designed primarily to present Livingstone in his roles as son, brother, husband, son-in-law and father. Schapera warns us that Livingstone's extended opinions on missionary work and his reports of his scientific discoveries appeared in letters written to those more concerned with these activities, rather than in his letters to his relatives, save for some to his father-in-law, Robert Moffat. Hence these letters only give us on the whole one particular view of Livingstone's complex character; but it is a fascinating view. There is a tendency in early biographies of Livingstone to skate over the difficulties which he thrust on his family; and here we see through his own letters how great were these difficulties. He appears certainly as the loving son and brother, always interested in his parents and siblings, always prepared to stint himself in their interest, sending medical advice even when he knew it would take may months to reach the patients. His love for his wife and children is manifest. But there was an apparently even stronger streak of obstinacy and self-will, as well as self-righteousness and egotism, within his interest in the lot of others; and here lay largely the basis of the determination which drove him on to ever greater discoveries, of lands and tribes.

Glimpses of these lands and tribes, especially of Bechuana tribal politics and their relations with the Boers, emerge incidentally, and Schapera's careful notes whet our appetite for a full study of Bechuana history from himself. He shows conclusively that it is dangerous to rely on published books, when letters like these are available. For example, he notes that the letters show that Livingstone only learned from hearsay from Bechuana that the Boers sacked and looted his home at Kolobeng, and that he never visited the site of the reputed sacking himself. Did it in fact occur? This is but one graphic example of how carefully we must learn to use our literary sources. Schapera has shown yet again, as he did on Moffat's sojourn at Kuruman, that all Livingstone's letters and journals about his travels and sojourns will require equally devoted scholarship by other local specialists; and that similar attention, based also on knowledge of the local peoples, should be given to the records of all early pioneers in Africa. It is welcome news to all students of South and Central Africa that Schapera has already in press an edition of Livingstone's records of his relations with the Makololo, and that he plans to edit the first great traverses of Africa, as well as other letters. He adds another vast dimension to anthropological studies, and he has set a challenging standard.

Aside from its value to all scholars of the region and of anthropological method, this collection of letters stands by itself as an absorbing account of the developing character of one of the most complex men in the history of an expanding African frontier.

MAX GLUCKMAN

AMERICA

18 **Culture and Conquest: America's Spanish Heritage.** *By George M. Foster. Viking Fund Publ. in Anthrop., No 27. Chicago (Quadrangle Books), 1960. Pp. 272. Price $6*

For about ten years, Dr. George Foster has been contributing to our knowledge of the Spanish side of the Middle American conquest situation and the present work summarizes many of his conclusions. It is claimed that anthropologists working among Amerindians have concentrated overmuch on the recipient culture and insufficiently defined the nature of the donor culture. In so far as no culture ever presents itself completely to another, Foster suggests that a definition of a 'conquest culture' involves recognition of two 'screening processes.' First, the formation of a conquest culture is characterized by a 'stripping-down' or 'reduction' process in which many elements of the donor culture are eliminated and many configurations become simplified. This, in turn, is screened by the various local aspects of the recipient culture which accepts some elements and rejects others according to its own needs. The production of a conquest culture is further characterized by a dichotomy of process: formal, wherein institutions and individuals in authority play a positive planning role, and informal, wherein a multitude of personal preferences and habits are selected and maintained in the new country.

Simplification in the first screening process is explained partly by sheer difficulty of transportation of certain items, partly by the fact that Spaniards in the new countries were of various origins and could not simultaneously preserve all the customs of all their native places. Foster points out that, while it is true that Andalusia and Extremadura provided most culture elements, this is not because they were more numerous in New Spain, but because they were more numerous in the first waves of immigration. Late comers from other parts adapted themselves to the patterns decided upon by the first waves in the crucial moments of the early conquest. This process of 'cultural crystallization' based on a time factor Foster finds as important as the explanation of acceptance or rejection through social and psychological phenomena previously stressed by most anthropologists.

Foster shows that the diffusionist outlook inhibited consideration of Spanish culture as more than a grab-bag from which items somehow had to be selected, as well as a global view of Hispanic-American variants. To these different visions of Spanish culture, according to the viewer's position in Chile, Mexico or Peru, the body of the book opposes a very readable compendium of Spanish ethnography, based partly on field work, partly on historical sources. While this does not replace the books of such Spaniards as Julio Caro Baroja, with whom the author worked, it will be a boon to English-language students both of Southern Europe and of Latin America. A good 20-page bibliography covers the Spanish sources.

While Foster amply shows the diversity of regional cultures in Spain, under a blanket politico-religious dispensation, there is no space for the corresponding complexity of receiving cultures to be examined. His simplified conquest culture, still available to study through the cultural conservatism of Spain, provides the framework of a relatively standardized donor culture in the place of what is usually one of two variables. Now that this has been given, it may be time to proceed further with the second variable: the Indian cultures in their own different stages of evolution, taking one item and refusing another in a seemingly loose, jigsaw-puzzle fashion which remains both exasperating and fascinating to the Middle Americanist.

E. MICHAEL MENDELSON

ASIA

19 **The Ainu of Northern Japan: A Study in Conquest and Acculturation.** *By Takakura Shinichiro, translated and annotated by John A. Harrison. Trans. Amer. Philos. Soc., N.S., Vol. L, Part 4. Philadelphia,* 1960. *Pp.* 88. *Price* $2

This work, which has been written with great erudition, is a study of Japanese colonial policy compiled from numerous Japanese records. It is not a history of the Ainu people, but it is the latter aspect, which is carefully documented, that will interest anthropologists. It deals with the Ainu of Hokkaido, here called by the ancient Japanese name Ezo, though there are early references to the Ezo of Honshu (the main island of Japan) and frequent subsequent references to those of the Kuriles and Saghalien. From the beginning of the ninth century there are official references to the powerful Ezo of Ou (the Ainu of north Honshu) with whom there was constant fighting. By the end of the century, after heavy Japanese losses, they were conquered, and towards the end of the eighteenth century they were completely assimilated. The history of the Ainu of Hokkaido begins in the sixteenth century with the immigration of Japanese into South Hokkaido. There were numerous clashes between the immigrants and the natives; the Japanese built forts to keep the Ainu out of the territory in the south-east where they had settled. From 1515 till the middle of the century warfare continued; then, the chief of the Matsumae clan realized that a change of policy was necessary and tried conciliation, giving the Ainu Japanese objects which they appreciated as treasures, and appointing two Ainu as chiefs respectively of eastern and western Ezo. Rules concerning taxes and the rights of Japanese merchant vessels to call in Ezo waters were laid down. In 1604 the head of the Matsumae clan was granted a decree of enfeofment in an area in the south of the island.

At this time the Ainu were living by hunting and fishing. They bred dogs and cultivated millet, but their use of agriculture was probably due to outside influence. They made clothes of bark cloth, skins and feathers. Their tools were of bone and stone. From early times the Ainu had trading contact with the Japanese and Manchurian and Siberian tribes. It is not clear when the barter trade began, but in the early Matsumae period it was already important.

It is difficult to obtain a clear picture of chieftainship, as doubtless some of the Japanese authorities quoted refer to the officially appointed chiefs, while others refer to the elders who were both heads of the families and of local groups—a system which existed till recent times. Several chiefs are said to have had large numbers of retainers—several hundreds in one case. Slaves are also mentioned, but Professor Takakura points out that the word *utare*, translated in the Japanese texts as slave, means primarily kinsman. Whether the Ainu had paramount chiefs of their own or not, they were able to unite in large enough numbers to create a formidable menace to the Matsumae.

Trade increased, and various trading centres were opened by the Japanese. The Ainu received *sake*, rice and tobacco in return for salmon, dried meat, skins, etc. The traders cheated the Ainu and there were numerous complaints of ill treatment. Hokkaido became a commercial colony in which the native population were harshly exploited. The Imperial Government became aware of approaching trouble, and edicts were issued forbidding injustice to the Ainu. In 1669 a dispute over fishing rights and game grounds led to open revolt, and nearly 300 Japanese were killed and a number of vessels sunk. After a punitive expedition the Ainu were forced to sign a hard treaty, and in 1685 the island was divided into an eastern and a western district, and each division was compelled to pay tribute to the Matsumae. Trade, which had begun as an exchange of gifts, had become exploitation. But, possibly even more harmful to the Ainu, they were now bound to supply labour to their masters.

More and more coastal trading posts were established till 1735, when a marked decrease in exports of deer and seal was recorded and some traders began to teach the Ainu more advanced techniques. The various products salable at a profit in Japan affected the Ainu in different ways. Those who lived away from the coastal posts supplied hawks, eagle feathers, skins and bear liver, and remained independent hunters, but the fishers were employed as labourers by the merchants, and were treated more like slaves. Cash had not been introduced, and when loading and unloading cargo Ainu were given rice and *sake*. The merchants indulged in easy tricks to cheat the Ainu, and those employed in fishing and pressing fish to make oilcake never earned enough to keep them through the winter. Revolt began in the spring of 1789; two guard houses were attacked and the guards killed, as well as the crew of a merchant vessel. The rebels fortified themselves in readiness for an attack by the Matsumae troops, but were persuaded to surrender without a battle.

In 1790 control from Japan was initiated, and the feudal rule of the Shoguns, known as *Bakufu*, began. A few years later Hokkaido came under direct government contol. One reason for this was to prevent further infiltration by the Russians, who were already trading in the islands. The contractors, *Basho*, were given instructions concerning fair treatment of the Ainu. Ainu chiefs were to pay tribute, and ceremonial exchanges of gifts were arranged. Buddhist missionaries were sent and some temples were built and paid for by the government, but Buddhist influence was slight. The Bear Ceremony was prohibited, but the prohibition was repealed as the Ainu maintained that hunting would not be possible without the ceremony—a clear indication of the strength of Ainu culture in spite of powerful foreign influence. A policy of assimilation was introduced with regard to agriculture, dress, manners and ethics, and miscegenation was encouraged. However, when it was found that, instead of appeasing the natives, enforcement of Japanese ways of life incited rebellion, the policy was reversed, and only those measures in favour of economic benefit to the government were upheld. A special iron currency, not in use in Japan, was made and circulated.

The detailed account of Japanese policy and its effect on the Ainu ends about 1820, though in the author's introduction he lists three more periods: the later Matsumae clan period, 1821 to 1854; the later Bakufu period till 1868; the Meiji period from 1868 until 1899 when the Hokkaido Aboriginal Protection Act was passed.

The translator in his preface states that Professor Takakura spent the best part of 20 years in documentary research before publishing *Ainu Seisaku Shi*, and that his devotion to the subject was due to his reaction to the harsh colonial policy from which the Ainu suffered, and his desire that understanding might lead to improvement in their lot. But he does not state whether the work in Japanese covers the five periods mentioned in the author's preface or stops at the end of the second period as in this translation. On a few occasions 'God' is written with a capital G; one would like to know which of the many Ainu *kamui* is translated as God. In spite of Batchelor's speculation there is no evidence that the Ainu are, or ever were, monotheists.

Mr. Harrison states that the bibliography is primarily for the specialist, but even a specialist might be given more help, and as an anthropologist I found myself completely bogged. There are seven pages with double columns and a little over a hundred entries; footnotes refer to these authorities. I could find no system on which the bibliography was arranged; it is not alphabetical or chronological, nor are the works of the same author listed together. For instance, there are seven entries for Matsuura Takeshiro scattered apparently at random through these pages. Further, there is no index. It is a pity that a work of such importance should be so difficult to read.

BRENDA Z. SELIGMAN

20

India's North-East Frontier in the Nineteenth Century. *Edited with an Introduction by Verrier Elwin. Bombay (O.U.P.),* 1959. *Pp. xxxii,* 473. *Price £2 10s.*

This latest contribution of Dr. Elwin to the ethnography of Assam consists of passages selected from the earlier literature in English dealing with the history and peoples of what is now known as the North-East Frontier Agency before the end of the nineteenth century, though the bibliography of relevant works includes authorities down to 1959. As many if not most of the passages here transcribed are only to be found in articles in periodicals, or in volumes or reports long out of print and often very difficult to come by, it is clearly of great advantage to have them assembled in a single volume from which irrelevant matter is excluded.

These exerpts are preceded by an introduction in three parts. In the first Dr. Elwin explains his object in publishing, and gives some estimate of the outlook of these earlier writers, many of them disparaging to the people of whom they wrote.

In the second part Dr. Elwin gives some account of nine of his 27 authors. 'Very little,' he writes, 'is known about the men whose writings are reproduced in this book: only two of them find a place in the *Dictionary of National Biography*, and I have had to collect what information there is from chance references in journals and occasional autobiographical passages.' The two in the *D.N.B.* are T. T. Cooper and W. Griffith, both distinguished botanists;

they, and Father Krick, and Dalton, as also Needham, Robinson, and Wilcox, are dealt with at some length; likewise Major Butler, though besides the works mentioned he had published earlier (1839) '*A Sketch of the Services of the Madras European Regiment during the Burmese War*. By an Officer of the Corps' (later the Royal Dublin Fusiliers) in which he served before he was posted to the 55th B.N.I.; and his son was the 'Johnny Sahib' so well known and beloved in the Naga Hills that his name was given to a mountain—Jani-sebu ('where Johnny sat')—to the complete obliteration of its original Angami name. Errol Gray was, no doubt, a tea-planter, but he was best known to his later contemporaries as an elephant-catcher, who knew no less about elephants than about Singphos and got on equally well with both. Beyond the briefest of footnotes in one or two cases nothing is told us of the other 18, although information is certainly available about some of them. Peal was a tea-planter who made frequent visits to the Naga Hills over a long period of residence in Assam; in the *J. Anthrop. Inst.* for 1881–2 he published a note on 'Platform Dwellings in Assam' with specific references to Bor Muthun, and to Khamtis, Singphos and Miris which is, like some others of his papers, unrecorded in Dr. Elwin's bibliography. An article of Woodthorpe's on the 'Wild Tribes Inhabiting the So-called Naga Hills' in the same volume of the *J.A.I.* likewise escapes record; it contains sketches of (among others) a Chang 'canoe-gong,' a Konyak village, the Vangam of Senua, of Chopnyu, and his wife, and a Naga of Borduria. Before serving in the Naga Hills and Upper Assam Woodthorpe had served on and published an account of *The Lushai Expedition* 1871–1872 (Hurst & Blackett, 1873). He gave a lecture on the Lushei and Chin tribes to the Royal Anthropological Institute. He accompanied Captain Williamson on the Garo expedition of 1872–3 and later served on the boundary commission that demarcated the frontiers of Burma, Siam and Indo-China on the Mekong; he took part in the Chitral expedition of 1895, and retired with the rank of General. He was a clever artist with an exceptional gift for catching types as well as individual likenesses, and a number of his admirable water-colour drawings of Assam tribesmen and others used to hang in Government House at Shillong until an undiscerning Governor's wife banished them to the 'Chateau' on the Peak; his portrait of the two babus of the Forest Department carried off by Akas in 1883 (he accompanied the Aka expedition of 1883–4) and several other sketches are in private possession in the United Kingdom, but the bulk of his drawings are now in the Pitt Rivers Museum at Oxford. R. B. McCabe, we are told in a footnote, was ultimately Inspector-General of Police, but before that he was Deputy Commissioner of the Naga Hills and compiled the standard grammar of the Angami language. He was killed in the earthquake of 1897. Captain Dun, whose initials appear as G.W. or (p. 199) as G.M. seems to have been really E.W. and the author of *The Gazetteer of Manipur* (Calcutta, 1886). Captain St. J. F. Michell, author of a *Report* (*Topographical, Political and Military*) *on the North-East Frontier of India* (Calcutta, 1883) with information on the Abors, Mishmis, Singphos, Khamtis, Miris, and Daflas, as well as on the Eastern Naga tribes, escapes reference altogether.

In the third part of Dr. Elwin's introduction, which is very short, he deals with confusion liable to arise from earlier uses of the terms 'Abor' and 'Naga.' In regard to the acknowledgements also made there, it should be made clear that Mr. J. P. Mills's *Bibliography of the Ethnology of Assam* has never been published; in regard to the terms 'Abor' and 'Naga' he leaves room for enlightenment. The term *Abor*, long applied to a particular tribe, now apparently known as *Adi*, is derived from an Assamese use which classified all hill tribes into *bori* (*bǎri*) or 'Tame,' with which the Ahom government maintained regular relations, and *Ābori* (*ābǎri*), the negative of *bori* applied to the unvisited tribes behind them. Thus earlier accounts often speak of 'Abor Nagas' meaning Nagas of the remoter ranges who had no relations with the plains. The term 'Abor,' for the specific tribe so called, was probably for 'Abor Miri,' the bulk of the very nearly related Miri being dwellers in the plains or foothills and all regarded as *bori*. 'Naga' on the other hand has been rightly applied to the large body of tribes south of the Brahmaputra and Dihing rivers with discernible common elements of race and culture doubtless once more uniform than they are now: the fact that nowadays some are called Wancho or Nocte, etc., without the qualifying 'Naga' does not alter the fact of their relationship, which was recognized at an early date by those who spoke of the more northerly and naked Nagas of the Patkoi first met with (*c.* 1835) as *pukka* or 'genuine' Nagas, while the more southerly and clothed tribes encountered later (*c.* 1839?) were labelled *kachcha*, 'unbaked,' no doubt as being less naked—*nanga*—from which the word Naga is probably derived. The Wancho of Ninu and Nisa are even better entitled to the term 'Naga' than the Kabui, Nzemi, or Angami of the south, and are just as much Nagas as Konyak, Chang and Phom.

The extracts that follow Dr. Elwin's introduction are in no way new. On the contrary they have been lost to view through age and a dissipation from which this useful volume effectively redeems them.

J. H. HUTTON

21 **Aspects of Caste in South India, Ceylon and North-West Pakistan.** *Edited by E. R. Leach. Cambridge* (*U.P.*) 1960. *Pp. viii*, 148. *Price* 18*s.* 6*d.* or $3.50

The second volume in the Cambridge Papers in Social Anthropology is a worthy successor to the first, and the four papers by Drs. E. K. Gough, M. Banks, Nur Yalman and F. Barth are all of a high standard. Dr. Gough's essay, the longest of the four, is about 'Caste in a Tanjore Village,' Dr. Banks writes on 'Caste in Jaffna,' Dr. Yalman on 'The Flexibility of Caste Principles in a Kandyan Community,' and Dr. F. Barth on 'The System of Social Stratification in Swat, North Pakistan.' Each essay combines description with theory in the tradition of British social anthropology. Dr. Leach contributes a brief and provocative foreword. The space at my disposal does not permit me to summarize the essays and I shall have to rest content with commenting on a few points which struck me as significant. The volume needs a long review article to do justice to it.

I find a view expressed by Dr. Leach, and shared by Dr. Gough, puzzling: 'Everywhere in India and Ceylon today whole caste groups are tending to emerge as political factions but it is misleading to think of such behaviour as a characteristic of caste as such . . .' (p. 6). Again, 'If a caste group turns itself into a political faction does it then cease to be a caste?' Dr. Gough implies that it does (p. 44) and at the end of her essay (pp. 58f.) she cites the formation of a 'caste labour union' as one among many symptoms of caste disintegration, but Dr. Yalman (p. 84) cites the formation of a 'caste welfare society' as one among many symptoms of caste resilience to changing social circumstances! (pp. 6f.).

Dr. Leach adds: 'My own view is that wherever caste groups are seen to be acting as corporations in competition against like groups of *different caste*, then they are acting in defiance of caste principles' (p. 7). I am at a loss to understand why the starting of a labour union, welfare society, bank, co-operative society, hostel, hospital or journal on the basis of caste should be regarded as evidence of caste disintegration. Over 25 years ago, Professor G. S. Ghurye mentioned in his book, *Caste and Race in India*, some characteristically modern and urban expressions of caste, and these continue even today. In recent Indian politics, especially at the State level, castes (in the traditional sense) have entered into alliances with other castes to further their interests. Even the Communists have seen to it that everywhere they put up candidates having a 'social base,' *viz.* belonging to the locally dominant castes. (See in this connexion my essay 'Caste in Modern India' in the *Journal of Asian Studies*, August, 1957.) I find it difficult to understand why this is not to be taken as evidence of caste resilience even if it is *assumed* that such resilience carries within itself the seeds of the destruction of the caste system.

There are other points made by Dr. Leach—that the caste system represents a form of division of labour from which the element of competition among workers has been largely excluded (p. 5) and the cut-and-dried distinction which he makes between caste and grades within caste (p. 7)—which space does not permit me to take up for consideration here.

Dr. Gough's essay is written with her usual competence but none the less I must confess that occasionally I had difficulty in following her. This is partly due to her use of three explanatory systems, *viz.* the structural, the psycho-analytic, and another which may be called, for want of a more accurate term, Marxist, and each in a deterministic way. Dr. Gough's desire to have no loose ends, and her tendency to explain social events in straightforward cause-effect terms results in great underestimation of the complexity of the data which she is handling.

Dr. Banks's article is a good study of the caste system in Jaffna and the part played by the dominant caste, the Vellalas, in it. He contrasts meaningfully two types of social stability, *viz.* Tanjore and Jaffna. In the latter area the '. . . total system of multiple checks and balances does not produce much in the way of village unity, but neither does it disrupt the village utterly. On the contrary, it is a system of great stability' (p. 75).

Dr. Yalman stresses the flexibility of caste principles in a Kandyan community and I would say that flexibility is a characteristic of Indian caste too. Without great flexibility Indian caste could not have survived till today. The traditional picture of caste as a rigid, immutable and clear-cut hierarchy is now giving way to a much more flexible hierarchy in which mutual position is left undefined and nebulous over a wide structural area. It is this nebulousness which makes for mobility, and mobility was not entirely absent even in the traditional system.

Dr. Yalman explains to us the sources of caste endogamy and the 'internal' barriers against caste mobility. He discusses illuminatingly the *raison d'être* of caste endogamy, and also the process of familial mobility. But individual or familial mobility is not typical of traditional caste, in which mobility when it occurs is of groups which are big enough to be endogamous or lucky enough to persuade the parent groups from whom they fission off to agree to a one-way movement in girls.

Dr. Yalman attributes enormous power to the symbols of caste status. Symbols of status differentiation appear to have great power in Ceylon as in India, but should not the acquisition of symbols be preceded or accompanied by the acquisition of economic or political power?

Dr. Barth's account of caste in Swat is fascinating, but I find his concluding section turgid, and his ethnographical data not at all detailed enough to support his theoretical conclusions. On p. 125 he says that the strength of the patron-client tie in Swat 'prevents the castes themselves from developing corporate administrative functions in any system like that of the local and regional caste panchayats of India.' If this statement means that caste panchayats develop only when patron-clients ties are weak, I would like to point out that all over India patron-client ties are very strong and were probably even stronger in historical times.

The four essays which go to form this book may be looked at in a different way: in Tanjore a caste which is ritually dominant, and which was formerly also dominant economically and politically, is gradually losing ground. In Swat the Pakhtuns are a secular caste who are dominant but the saints have higher status especially when they also own land. In Jaffna the ritually high caste is dependent utterly on the secularly dominant Vellala while the Goyigama in Kandy do not have a caste of priests at all. The comparison of the four dominant castes and the implications of each type of dominance for the total social structure would have made these studies even more illuminating than they are at present. M. N. SRINIVAS

EUROPE

22 **Recherches sur l'anthropologie des françaises.** *By Suzanne de Félice. Paris (Masson),* 1958. *Pp.* 316, 8 *plates. Price* 30 *N.F.*

By comparison with the amount of information available for men, anthropometric data for women are few. This detailed metric and somatoscopic study of a sample of French women undertaken by Dr. Suzanne de Félice is therefore useful. It relates to 140 women, mainly young adults—all were under 49 and the majority under 30—drawn from almost all parts of France; half the sample however was born in Seine and the adjacent departments, and only a minority in the south, but it is claimed that birthplace has no real significance since the sample relates to a shifting population. For it is a highly selected sample, consisting essentially of professional women—doctors, medical students, masseuses, teachers, etc.—and only three working-class women and no country women are included. There was moreover fairly rigorous physical selection of the subjects; all were required to be in good health, and any with quite minor postural variations, such as shoulders of unequal height or perceptible curvature of the spinal column, were eliminated. It is hardly surprising to find, therefore, that the average stature (5 feet 3 inches) approaches that of British women, or that when regional subdivisions of the sample are compared, there is no correspondence with the regional variations in body size, head shape, and pigmentation noted by Vallois (*Antropologie de la population française*, 1943) in males. The argument of Mme de Félice that characters of body size in her sample are bimodal in their distributions is not convincing. Graphical comparisons, with which the book closes, demonstrate the general similarity of most body proportions in European female samples of different provenance.

D. F. ROBERTS

OCEANIA

23 **Kapauku Papuans and their Law.** *By Leopold Pospisil. Yale U. Publ. in Anthrop., No.* 54. *New Haven (Yale U.P.),* 1958. *Pp.* 294, 8 *plates. Price* $4

The very useful descriptive first half of this work serves as a basis for the even more valuable second half, which deals with the general theory of primitive law. By virtue of that theoretical part, the book becomes much more than a competent fieldwork monograph: it is important for every anthropologist who has to face the thorny problem of defining such general concepts as law (or customary law) and religion, and applying them to his ethnographical material.

The Kapauku inhabit the central highlands of western New Guinea. The author describes their religion and life cycle, and then their socio-political organization. Particular attention is paid to the *tonowi*, the man who by his personal characteristics and wealth occupies a position of authority in the village. There is a splendid description of the *tonowi*'s financial dealings in pigs, beads and cowries, by which he establishes and strengthens his position (pp. 83–6).

After this descriptive part comes the theoretical, which deals specifically with law. Pospisil gives 121 'Rules,' *i.e.* statements by the Kapauku themselves on how conflicts should be settled, and 176 'Cases,' in which one sees to what extent these rules are applied in practice.

This brings us to the problem of defining, or finding a criterion for, law. The author rejects earlier definitions which select one single dominant principle, and proposes that the core of 'the social phenomena which we call law' is to be found in the coexistence of four 'attributes' (p. 258). These attributes are: Authority, *Obligatio*, Intention of Universal Application, and Sanction.

It is remarkable that European residents in the Kapauku area had doubted the existence of any indigenous authority. Pospisil's field research brought to light the role of the *tonowi*, and this became a cornerstone of his four-attribute theory.

The next point the author had to consider was whether it is meaningful to distinguish law from customary law. His solution is to discern two foci within the field of law, *viz.* customary and authoritarian law. Other relevant matters, such as the delimitation of law where it borders on custom on the one hand, and on political decisions on the other, are dealt with in an equally lucid manner, and the final result is a theory of law which in my opinion makes better sense than any previous effort.

Here in Holland, at any rate, this book has given rise to a most lively controversy. Although it has converted me to an ardent pro-Pospisilite, I admit there remain several points which require further thought and elucidation. For instance, the conclusion (pp. 274, 277, 289) that in the last instance there is no Kapauku Law as a single entity seems too extreme, and has dangerous implications. I should say that some societies clearly distinguish law as a category within their culture, and others do not. In studying the law of societies of the first type, it is probably best to adopt the native criteria for law (this, incidentally, does away with the paradox (p. 278) of the 'legal gangster' in western civilization). For those of the second type, Pospisil's four-attribute theory is an excellent tool—but it is a tool forged by the anthropologist, not by the native. If we allow the anthropological theorist this scope, then we may surely also permit him to discern a Kapauku culture, and Kapauku law within that culture, even if the Kapauku themselves do not.

Other points needing clarification are: to what extent can 'customary' and 'authoritarian' be equated with 'just, according to the Kapauku' and 'unjust, according to the Kapauku'? How must one reconcile the ideal character of the rules (p. 145) with the possibility that the application of a rule is considered unjust (p. 281)? How exhaustive is the list of rules, and should one not take into account the elements of vagueness and time lag in their wording? And, finally, is the English language not responsible for some confusion, as it allows the word 'law' to be used for 'a law' (French: *loi*) as well as for 'the law' (French: *droit*)? Some conclusions which are valid for a Kapauku *loi* appear to be rather rashly applied to the Kapauku *droit* (p. 281).

On the other hand, Leach's criticism (*Amer. Anthrop.*, Vol. LXI, No. 6, p. 1096) of an earlier publication dealing with the crucial case No. 33 is effectively refuted by the present book: there can be no doubt that the *tonowi* who wittingly broke an incest rule was creating a new pattern of behaviour, and not conforming to an existing one (pp. 109, 166, 274, 282, 284).

Pospisil's book, I am convinced, means a highly important advance in the study of 'primitive' law, not least by establishing customary law as a focus within the field of law as a whole. And if ever it is necessary to prove that social anthropology is not merely 'primitivology,' but contributes to the understanding of human culture everywhere, there can be no better testimony than this book.

P. E. de JOSSELIN de JONG

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24 **Das Floss in Ozeanien: Formen, Funktion und Verbreitung des Flosses und flossartiger Schwimmkörper in der Südsee.** *By Dieter Schori. Völkerkundliche Beiträge zur Ozeanistik, Vol. I. Göttingen,* 1959. Pp. 274

This work has been presented as a thesis for a Ph.D. degree at the University of Göttingen, Germany. It treats of the function and the distribution of rafts and raft-like vessels in the South Seas.

In an introductory chapter (pp. 2–10) the three basic types of Oceanic rafts, *i.e.* the platform raft (*Plattformfloss*), the bundle raft (*Bündelfloss*), and the raft boat (*Flossboot*) are listed and described. A special type of platform raft is the *katamaran*, consisting of an odd number of logs, the longest in the middle, symmetrically arranged in an organ-pipe fashion. In contrast to the loose terminology of various other writers who also use the word *katamaran* to refer to vessels of an entirely different type, *i.e.* the Polynesian double canoes, the term is employed by Schori in its original meaning only. Next to the *katamaran* two other special forms are mentioned, *i.e.* the raft-like counterparts of the outrigger canoe and the double canoe. The author emphasizes that only a few sources deal with the matter more or less elaborately (J. Cook, J. J. Labillardière, F. W. Beechey, W. Ellis, R. Parkinson, and G. Friederici). His data had therefore to be painstakingly gathered from 430 books and articles which is certainly an impressive number for a study of this size.

The book proper consists of a presentation of the basic material, arranged according to the usual tripartite division of the Oceanic island world—New Guinea being included in Melanesia (pp. 11–149)—, and a summarizing chapter with a number of cautiously formulated conclusions (pp. 150–173). It is demonstrated that rafts in Oceania had their own functionally determined position. They were used for coastal traffic, for fishing, for the transportation of heavy loads (stones), for river traffic on the larger islands, for funerals at sea, for surf-riding, and as children's toys. Mention is made of long raft voyages in Eastern Polynesia, *e.g.* on page 131. According to the author these voyages had an involuntary character, the group in question having been either exiled or put to flight after defeat in battle. In the majority of these cases boats will not have been available nor will the people have had time to make them. Drift voyages of rafts blown away by gales from safe beaches and lagoons are also reported. One would expect the author to mention in this connexion Andrew Sharp's *Ancient Voyagers in the Pacific*. The book has evidently escaped his attention, however, which is to be regretted.

Schori advances the theory that the Oceanic peoples made their long voyages by means of boats and that there already existed in their lands of origin a division of functions between boats and rafts similar to that in the Pacific as it appears from eighteenth- and nineteenth-century sources (pp. 167f.). He differs from J. Hornell and Th. Heyerdahl, who both consider the part played by rafts in the peopling of Oceania to have been an important one. He particularly disagrees with Heyerdahl's opinion that on account of their technical inferiority in comparison with boats rafts cannot possibly have formed an integral part of the nautical complex of Oceania and must therefore have been of South American origin.

With regard to the *katamaran*, which is met with in America as well as in the South Seas, the writer suggests that judging from its occurrence in Southern Asia, along the coast of East Africa, in Indonesia and in Oceania this vessel has been distributed by people coming from South Asia who eventually also carried it to South America. It is emphasized in this connexion that the long voyages were made by boat but that the emigrants knew of the *katamaran* and were able to make it wherever need arose. Contrary to the way Heyerdahl puts forward his opinions, this idea of the writer as well as his other conclusions are cautiously advanced as hypotheses. This certainly is one of the merits of his study which is, moreover, illuminating with regard to a hitherto rather neglected field of the nautical history and ethnology of Oceania.

The 82 photographs and drawings of rafts and raft-like vessels are not very well reproduced, but an intelligent use is made of them in illustrating the text.

S. KOOIJMAN

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25 **Oceanian Art.** *By Tibor Bodrogi. Budapest (Corvina) (U.K. agents: Heffer),* 1959. *Pp.* 41, 160 *photographs,* 10 *colour plates,* 19 *text figs. Price £*2 2*s.*

The Curator of the Ethnographical Museum in Budapest has written a sound introduction to his fascinating subject. After a general discussion of the nature of primitive art, Bodrogi describes the exploration of the Pacific and contributes a brief but lucid analysis of various regional types. He follows Speiser's classification scheme in Melanesia and the Linton-Wingert style areas as supplemented by Kooijman. To the list Bodrogi also adds his own discovery—Astrolabe Bay. Thanks to the efforts of L. Biro in the eighteen-nineties, the Hungarian Museum has many splendid examples of this little-known style.

The novel accomplishments of this volume reside mainly in its illustrations. For the 160 black-and-white plates of *Oceanian Art* reproduce a congeries of objects ranging from Tami masks to Balinese wood carvings. Especially important and impressive are a mask from Modon (Fakfak) in West New Guinea, a carving from the newly identified Arambak area of the Sepik, figures from the D'Entrecasteaux, masks from Tanga, Witu and northern New Ireland, and a rare Philippine image of the 'cubistic' type. Polynesian, Micronesian and Indonesian material offers the viewer less interest but shows surprising scope. There are in addition ten colour plates which, while a trifle garish, add to the overall value of the work. In sum *Oceanian Art* makes an excellent impression. It is well written, well translated, and handsomely illustrated. Scholars especially will rejoice that extensive material heretofore beyond the Pale now lies within easy reach.

DOUGLAS FRASER

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