

BOOK REVIEWS

Logik und Leben. Kulturelle Relevanz der
Didinga und Longarim, Sudan. Andreas Kronenberg.
Wiesbaden: Franz Steiner Verlag, 1972. Pp. 191,
14 illustrations, 31 diagrams, 3 maps.

As Government Anthropologist to the Republic of the Sudan, Herr Kronenberg worked among the Lango-speaking Didinga and Longarim between 1958 and 1960. Those were years of comparative peace for the Sudanese Nilotic peoples: it seems likely that the ethnographic data contained in this book have, by now, mainly historical value.

They are presented in a deliberately idiosyncratic way. We start with theoretical considerations, and an elaborate mathematical analysis of kinship terminology and end (after chapters on the cattle idiom, sexual relations, age-stratification and magico-religious beliefs) where the authors of most field monographs still feel obliged to begin, namely the geographical and economic base. Theory first, ethnography later; the pitfalls are obvious, but the method does have the advantage of honesty: Herr Kronenberg at least makes no bones about where his real interest lies.

It is a philosophical interest - Herr Kronenberg's intellectual patrons are Frege and Wittgenstein, Lévi-Strauss being not so much as mentioned in the bibliography - in what Evans-Pritchard used to call problems of translation. Empirically observed facts can have significance, "cultural relevance" only within a given semantic system. But semantic systems are culturally discrete; thus, even within Western anthropology, "common-sense terms" bear the imprint of Empiricism in England, of Romanticism in Germany, of the Enlightenment in France... And even to the extent that a common "scientific" terminology has been evolved, that terminology is still a classificatory system of the same order as those it seeks to interpret: a system, in its own way, as closed and self-confirmatory as that of the Azande. Seen in this light, whole volumes of carefully checked ethnographic data can have no more significance than so many compilations of statistics about individual moves in chess: they will not, Herr Kronenberg argues, enable us to deduce the rules of an unknown game - the rules, say, of a kinship terminology which will enable us to produce the correct term for a particular genealogical specification as unerringly as the native speaker.

Put in these terms, the problem is not soluble; which may lead us to suspect that it is not in fact correctly presented. It is possible to discover the rules of an unknown game, but only by the same kind of mental process that makes possible the discovery of an invisible molecular structure in chemistry: namely by the methodical testing in the light of observed facts of what started out as a series of intelligent and consequent guesses. But there does remain the problem of finding a language for the guesses, such that they can be tested in other semantic contexts; and Herr Kronenberg believes that he has found such a language for Didinga kinship terminology.

The Didinga themselves express such things, as good Nilotes should, in terms of cattle; or rather, they express agnatic relationships in this way. They recognize, in fact, two modes of kinship. "Natural" kinship is given by the fact that women bear children; it is thus definable only in terms of female fertility, and can be transmitted only from mother to child.

All other kinship is "cattle kinship" - i.e. it depends on a woman's fertility having been legitimately acquired in exchange for bridewealth cattle. It can therefore be expressed in terms of such cattle, or rather in terms of proportions of an ideal unit, the total herd, which corresponds to a brother-sister pair. At each generation, half a man's inherited cattle are given in exchange for a wife; but he, in turn, receives half another man's herd in exchange for his sister, thus reconstituting the ideal complete herd.

Agnatic kin are thus "cattle" kin: the father-son relationship, for instance, has been created only by the father's legitimate acquisition, by means of cattle, of a woman's fertility (as among the Nuer, there is nothing to prevent a woman from becoming a pater by the same means). And the term applied to a category of kin denotes, precisely, the number of "cattle-links" (i.e. agnatic links) between members of that category and the speaker; "natural" (i.e. uterine) links being immaterial for the purpose.

This Herr Kronenberg chooses to express, ingeniously and elegantly enough, in binary notation; the symbol 1 corresponding to agnatic or "cattle" links, and the symbol 0 to uterine or "natural" ones, while positional values correspond to what European terminology would call generations. Thus:

<u>Genealogical specifications</u>	<u>Binary expressions</u>	<u>Number of agnatic links</u>	<u>Term used</u>
B (FS) , Z (FD)	1		
FDS , FDD	10	one	A
FDDS , FDDD	100		
etc.			
BS (FSS), BD (FSD)	11		
FSDS , FSDD	110	two	B
FDSS , FDSD	101		
etc.			

This, is, in itself, a pleasing solution, certainly more illuminating than labelling such a terminology "Crow" or explaining it by a "skewing rule". Herr Kronenberg rather handsomely attributes his reasoning to the mathematicians of ancient Egypt (who, he believes, derived binary notation from their own kinship reckoning) and backs it up by a great many pages on the nature of binary series. These can, of course, be skipped by the mathematically unsophisticated; but the skipping, inescapably, brings up another question.

Could not the binary notation itself be skipped? Is it, on the principle of Occam's razor, really necessary? Or has Herr Kronenberg simply succumbed to a more seductive form of the mathematical fata morgana that seems at times to bedevil modern anthropology? He has rightly condemned uncritical statisticism; he has had the good taste to ignore Lévi-Strauss's propensity for littering his pages with pseudo-algebraical formulae; but if the Didinga can operate their system by differentiating between "natural" and "cattle" links and counting only the latter, could we not get along with some pair of terms like "uterine" and "agnatic"? Natural language may have its limitations, but the premature importation of mathematical symbols can only compound the problems of translation, besides leaving us no language in reserve for higher levels of abstraction if we should ever reach them. So keep your powder dry, Herr Kronenberg, and save up the binary numbers; with luck, we might really need them some other time.

Eva Gillies

Rethinking Kinship and Marriage. Rodney Needham (ed.)
A.S.A.11. London, Tavistock, 1971, £4. (hardback),
£1.95 (paperback).

Whatever the merits of the rest of this book, clearly most interest is going to be aroused on the gossip circuits by Needham's long, long "Introduction" (p.xii to p.cxvii, as it were), and his following chapter of "Remarks". The first takes a swipe at a number of targets but the bulk of it consists of a history of the prescriptive marriage argument and the various indignities that have been perpetrated in the name of truth, theory, and the like. On this let us record that Needham is unquestionably right, his opponents are undoubtedly disreputable, and the whole thing is becoming a bore. The temptation to use such an opportunity for elaborate self-vindication is obvious, and one can sympathise, but there is a time to leave well alone. Needham is not content simply to slay the dragons that have so ridiculously plagued him over this issue, but he feels it necessary to turn his sword, in the name of 'competence and authority' on Radcliffe-Brown, Lévi-Strauss, and so help us, Fortes. Perhaps one can be forgiven for thinking that the lady doth protest too much in this instance. Needham's response - that he is 'right' - is of course unanswerable. But then so was Joan of Arc and no one thanked her for it.

Having destroyed the classic authorities in the first salvo Needham then murders kinship itself in the next; Southwold kills kinship terminology in chapter two; and Rivière finishes off marriage in chapter three. Since we are now left with nothing to discuss it is not surprising that in chapter four Leach has to resort to another piece of convoluted cleverness on the subject of phonology and affect, or the "mama/papa" syndrome. It's all good clean fun, and at least he concludes that there might be some

signals of a 'species-specific, cross-cultural type'. Incidentally, it borders on the hilarious that after so many shining reputations have had to go down in the name of competence and authority, that the book should be dedicated to Edmund Leach!

Francis Korn continues the needless Lévi-Strauss bashing that is so² fashionable nowadays, and is, I suppose, a natural reaction to the rather silly adulation paid earlier. It seems we don't know what an elementary system is anymore, or rather that Lévi-Strauss doesn't know. Bateson showed that the Iatmul had a series of contradictory structures of marriage, and Korn nicely demonstrates that only one - marriage with FMBSD - is totally compatible with the structure of five assymmetrically related descent lines as given in the terminology. 'Sister exchange', for example, and FZD marriage are not. This is very interesting, and clearly reflects a transitional system as Bateson saw (although he got the transition the wrong way round, I think). In The Keresan Bridge I describe a similar situation among the Keresans where terminology reflected both "Crow" and symmetric tendencies, and marriage was preferred between a man and a woman of his mother's father's clan (MFZDD). Of course, in a simple symmetrical system MFZDD, FZD, and FMBSD are one and the same, and therefore, following Lévi-Strauss, I interpreted this as a transition from elementary to complex structure (i.e., in this case, Crow.); these two categories being 'trends' in any case. What I think Korn has here is an "Omaha" version of the same trend. I cite all this, (and I could go on at length about the details), simply to protest against using this kind of material merely to put Lévi-Strauss down, rather than, as we are so often admonished to do, "getting on with the job". This arid 'anti-classificatory' business largely misses the point and I suspect its motives.

Forge adds some notes on the Sepik to Korn's article. McKnight clears up some points about the Wik-mungkan, showing in yet another instance that at base there is a simple, symmetric ("two-line") system. Wilder works out how many descent groups the Purum have. Well, that's the way the Old Kuki crumbles. It's a bit of a relief to get to Beidelman on Kaguru incest notions and Fox (the other one) on sister's children as plants and other analogies. All very lively, and a good read if you can digest the indignation.

Robin Fox

African Culture and the Christian Church: An
Introduction to Social and Pastoral Anthropology.
Aylward Shorter, W.F. Geoffrey Chapman, London,
1973. (also in paperback edition)

In his introduction, Father Shorter tells us that the term 'pastoral anthropology' is likely to cause some confusion, especially among professional anthropologists. But, he explains "the term aptly symbolizes the marriage of two disciplines: social anthropology and pastoral theology". We would ask who officiated at, and who consented to, this marriage?

There are missionary groups, apart from the Pastoral Institute of Eastern Africa at Gaba, who are trying to find out how the study of anthropology and sociology might help to broaden Christian pastors' understanding of peoples of the world and to set new developments in pastoral work into motion. These groups would not wish to make an underlabourer out of the discipline of social anthropology, nor would they simply moralize with reference to various church practises and legislation, going against the spirit of Vatican II, which represented a sincere appreciation for genuine religious and ideological values of non-western and non-Christian peoples.

This book represents a double problem: from the pastoral theological point of view, the Church in the past has not always handled problems of traditional belief and religious practice in the way Fr. Shorter suggests. From the social anthropological point of view, hardly any of the points raised would be recognizable as social anthropology to a professional anthropologist. A concrete example of this is the way in which the notion of marriage is handled. We would have thought that the contribution of social anthropology to the question, 'can other forms of marriage be Christianized?' would be (1) the facts about other forms of marriage and different forms of kinship systems throughout the world and (2) the questions about forms of marriage which an anthropologist might raise. Most modern anthropologists would hold the view that all forms of marriage are honourable; the Christian theological view is that only monogamy is acceptable. The anthropologist would ask, 'what were the forms of marriage existing in the Holy Land at the time of Christ's birth?' and 'to what extent is monogamy an imposition of Roman law onto Christianity?' In such cases, the pastoral question largely boils down to the anthropological ones, but the pastor would ask (going further than the anthropologist), does the present legislation of the Church take account of the fact that among many peoples of the world, marriage is not a contract between two individuals, transferring "uxorial" rights, but an alliance between two groups of people involving an intricate constellation of mutual rights, duties, assistance and prestations? In fact, it is these kinds of considerations which recently prompted Professor D'Arvack in Rome to brand the Roman Catholic Church's legislation on marriage as "anachronistic, inhuman and grotesque".

From the point of view of the pastoral theologian, Fr. Shorter does not go to the root of the problem, he merely skates over the surface of the problem within the framework of present day Church legislation, although he seems aware (p.176) that other suggestions have been made. In so doing, he does both pastoral theology and social anthropology a disservice and lumps it all under the heading of a "new discipline" called "pastoral anthropology" which does not exist. In one way, this book can be seen as a missed opportunity; it could have been an attempt at a breakthrough, instead, it is a re-hash of old norms and legislation with new names attached.

While we can readily understand that Fr. Shorter's book is directed towards an audience of non-anthropologists, we regret that in the name of social anthropology (which we understand he studied at Oxford) he does not demand of laymen that certain intellectual efforts be made to understand such complex notions as, for example, "social facts". We know of few anthropologists who would attempt a three paragraph 'potted definition' of such a fundamental social anthropological concept (pp.6-8). Many anthropologists would object to the looseness with which terms like 'social fact', 'behaviour', 'network', 'structure', 'adaptation', 'product', etc. are used. It does not seem to matter to Fr. Shorter which anthropological theory or methodology he uses to "prove" his points. Each chapter represents

a curious mixture of ideas: in one paragraph, Malinowski is brought to the fore, in the next, Lévi-Strauss, then we might find R.G. Lienhardt or Evans-Pritchard cited to "prove" (p.45) that non-western religions are really religious! What we protest against can be summed up in this statement of Fr. Shorter's, made on p.7:

In social science it is customary to speak of sociology and social anthropology, but these terms refer to differences in emphasis and method rather than to any really significant differences between the two.

We are here told that theory and method (which we thought selects what 'facts' are, which selects what facts will be examined, which selects how they are examined) are not really significant. This is simply naive and we disagree, especially in cases such as that in the chapter on ritual where we find such disparate theoretical and methodological views as those held by Malinowski, Mary Douglas, Radcliffe-Brown and Victor Turner all cited to "explain" the same phenomenon. A study of each of these anthropologists' methodology and explanations of ritual might well enrich the understanding of any non-anthropologist; might introduce him to the complexity involved in explaining sequences of human actions. If nothing else, such study would demonstrate the care and concern which anthropologists have for the categories and classifications of others, or it might at least indicate that for some, anthropology is a life-work. But Fr. Shorter, no doubt unintentionally, neglects to give such impressions; rather, he gives the impression that social anthropology is something which can be "mugged-up" over a short period of time and then applied, carte blanche, according to the whims of such 'students' of the discipline.

For example, on p. 152 ff., we find that the foreign western classifications of religion/worship/sacred and medicine/magic/secular are superimposed onto African religious traditions. Moreover, Fr. Shorter suggests how judgements may be made by the African pastor with reference to traditional forms of belief and worship as to whether they are against "faith", i.e. Christian religion. Any consideration of the genuine "salvation-value" (in the Christian sense of the word) in non-western religions, clearly recognized by Vatican II, seems to have escaped his notice. He does not, anthropologically or theologically, really go into African religions. He merely puts forward a framework for judging what is "good" and "bad"; a regression to antiquated missionary attitudes. Anthropologists have long known, at least in principle, what the difference is between making judgements and describing what happens in a society.

Finally, we should like to point out David Pocock's clear distinction, made in Social Anthropology in 1960:

It is evident at the outset that the anthropologist working in another society (or in his own society regarded as "other") must take a certain stance quite different from that of, say, a government official or missionary, who is concerned to bring about change in accordance with certain beliefs which he holds (p.86).

of optimism - which seems appropriate. One would have liked a long reflective conclusion. Instead, the last chapter (which is only ten pages long) hurriedly brings us up to date, and quickly glances at the contrasting situations in France and America.

Some will be slightly disappointed that the survey does not end with a more positive or theoretical section. But all research is conducted with certain limits imposed, and it would be asking too much to have expected a two year period of investigation to have lead to a bolder type of climax. Miss Henson has served us well in covering the ground in a more descriptive way, so we now have a useful starting point from which to raise larger issues. Indeed, her book is never a catalogue, so many of the problems one would like to follow up have already been indicated by the author. We could ask, for example, what is the significance of inquiring about the relations of social anthropology and language? What have been the consequences of their separate development and why is it important to relate them more closely? When the latter problem is answered, which is the best means of linking the two fields?

There are plenty of facts in Miss Henson's history to set us thinking about these difficulties. If we say, for instance, that language is important to us because we need a source of ideas to help us tackle semantic problems more efficiently, then the framework for our research obviously becomes social anthropology, language, and meaning. And now we are presented with some rather more precise problems. Malinowski's linguistic work did not lead to an anthropology more concerned with meaning whereas Evans-Pritchard's more general sensitivity to language did. On an international scale, the discipline has been transformed by Lévi-Strauss' idiosyncratic vision of the role of 'linguistic' models in the human sciences; nothing of equivalent potential has been produced by the 'linguistic anthropology' of the United States. It is difficult to envisage 'ethnographic semantics' revolutionising the subject. If social anthropology and language are to be related, 'linguistic anthropology' is perhaps not the best way of doing it. Even so, we can still ask whether Lévi-Strauss' use of language has led to a better understanding of meaning, or whether its dominant tendency has been anti-semantic. Perhaps, too, it is not out of place to remember that linguistics has been less successful with semantics than with any other phenomenon. This being so, might we expect more valuable guidance by looking to linguistic philosophy than to linguistic theory?

There are lots of large problems like these that need to be considered. That they are not part of the problem Miss Henson set herself does not lessen the value of her book. One could not begin to grapple with such topics without a knowledge of the historical background. Social anthropology and language was a slice of our development which had not previously been charted, and those interested in the sorts of issues I have outlined will be in Miss Henson's debt for her covering this ground and laying bare some of the landmarks.

Malcolm Crick.

J.W. Berry & P.R. Dasen (eds), Culture and cognition: Readings in cross-cultural psychology.
London: Methuen, 1974.

The publication of this collection of papers should allay the fears of those anthropologists who find themselves overtaken by visions of Human Relations Area Files, tortured ethnographies and coughing computers whenever the word "cross-cultural" is muttered by their uncautious colleagues. It demonstrates once again that not all psychologists are totally insensitive to the problems of translation and cross-cultural comparability, that some are aware of the role of situational factors, and that there are even a few psychologists who feel that numbers are not essential to the conduct of a science.

This volume has brought together some of the more important papers on cross-cultural studies of thinking. It contains Fishman's impressive review of work on the Whorfian hypothesis, and, by including papers by Gladwin and Sturtevant, may serve to remind psychologists that ethnographers are also interested in similar topics. There is also a very striking paper by Cole and Bruner on the potential problems of inferring cognitive differences from differences in behaviour. The last third of the book is devoted to papers on Piagetian developmental psychology. Among these there is an early essay by Piaget and a good review of the field by Dasen.

The introductory essay by Berry and Dasen raises several important theoretical issues. Instead of engaging in empty name-dropping and the passing genuflections that cross-cultural psychologists usually make to aspects of theory, these authors have taken the trouble to detail the contributions of early writers on the subject, and have been so bold as to offer some thoughts on the problem of cross-cultural comparability. The problem of comparability is without doubt the most serious problem confronting cross-cultural psychology. Yet for all the attention it has received one would think that researchers in the field regarded it as the piffling decision of an over-zealous touch-judge rather than what it really is, the rule upon which the entire cross-cultural game depends. In discussing the problem of comparability, Berry and Dasen elaborate some ideas presented in an earlier paper by Berry. They propose among other things, that behaviours in different cultures be matched in terms of their "functional equivalence". This solution is clearly inapplicable in several areas. What is more, where it might be appropriate, it would merely serve, like slum clearance, to remove the problem to another quarter.

This volume will certainly be of value to anthropologists and psychologists. It will show anthropologists what has been happening in the study of cognition and, more importantly, will enable psychologists to take stock of what they have produced thus far. It seems likely that cross-cultural psychology will begin to abandon its strictly comparative approach, and that it will turn instead to the study of relationships between behaviours within a community. We may still see the day when cross-cultural research is conducted by people who enjoy a psychological way of thinking and who, but for their concern with data and the elimination of competing explanations, could be taken to ethnographers.

Peter Collett.

F.G. Bailey (editor), Debate and Compromise: The Politics of Innovation. Oxford: Basil Blackwell, 1973, 343 pp., £5. (Pavilion Series).

A critique of Shakespearean humour based exclusively on synopses of the first acts of the "comedies" would match this book for intellectual teasing. Each ethnographic chapter begins in a new location; few readers will want to be "just getting into it" so often in a single work. On the one hand we are told that, in effect, ethnography is theory (p.254); on the other, there is little to convert the reader who suspects that, instead, he is being led to ultimate generalities distilled from these ethnographic "shorts". There is clearly more to the complete ethnographics than such synoptic presentation displays; some, at least, surely deserved separate and full-scale publication. Moreover, so strong a resemblance to Gifts and Poison suggests the birth of a new methodological oligarchy (compare "ekistics", "cantonometrics"); though, encouragingly in a sense, the vagueness of the concept of "innovation" and the brevity of the editor's summary show that no restrictive constitution has as yet been drawn up.

There is clearly much here that is worth having, and in far more detail. As it is, arguing from Wolf to the peasant will not suffice: the peasant should be at liberty to falsify Wolf (and others). Specifically, the symbolic universe of each community tends to receive stunted treatment at best. Heppenstall suggests that in St. Martin the "fringe members'" inability to "participate in traditional customs of reciprocity" leads them to "think of their labour in terms of cash return". With a more exhaustive discussion of traditional reciprocity customs, we should perceive the "fringe" in diachronic context. One wonders whether some so-called innovations might not be most usefully considered in relation to changes, not in the reciprocity system itself, but in the mode of its realization. "We" and "they" remain opposed, while the specific range of reference changes; in the essays on Saburneda and Gema we see how new means can serve old ends (such as family solidarity). But to relate "change" to a continuity of any kind requires fuller ethnographic presentation of traditional practices and beliefs. In the East Tyrol Essay, the whole emphasis on the "local council" demands supportive data on the composition of such a council and on the traditions which the members support or contravene. Similarly, in Barrett's assessment of informant views on the failure of the Aiyetoro fish-ovens (pp.260 ff.), the ethnographer's "practical" objections may be valid, but we have no means of judging their acceptability in Aiyetoro terms. Barrett surely does not mean that one type of explanation is exclusively wrong or right; but the inference might easily be drawn here.

Bailey's view of crisis as loosening the minutiae of normative behaviour, thus creating greater freedom to innovate, is falsifiable and clearly delineated, and could provoke useful discussion of other ethnographies. However, the relation of the society to the source of crisis raises further questions of bounding. Crisis may lie in internal smugness as much as in internal threats: external grandeur may go with internal stagnation. But Parkinson's Law is falsifiable too.

Michael Herzfeld

SHORTER NOTICES

When the Golden Bough Breaks: Structuralism or Typology? Peter Munz. Routledge and Kegan Paul. London. 1973. £2.25. xiii, 143 pp.

Professor Munz's historical approach to myths consists of classifying them into typological series. The progression in each series from more general to more specific versions yields the historical and inherent meaning. There are some insights on the way, but on the whole they do not justify the journey.

Urban Anthropology: Cross-Cultural Studies of Urbanization. Aidan Southall (ed.). Oxford University Press. London. 1973. £2.25. vi, 489 pp.

A collection of essays hoping to deal with questions like 'What is meant by terms such as 'urban' and 'urbanization'?' and 'Is there a clear rural-urban dichotomy?' Particularly interesting are the contributions by E. Bruner and O. Lewis; but even these are of small-scale interest, dealing with the minutiae of social life. The 'cross-cultural' emphasis of the title is noticeably absent, but the book does offer a collection of sound ethnographies.

The Translation of Culture: Essays to E.E. Evans-Pritchard. Thomas O. Beidelman (ed.). Tavistock Publications. London. 1973 (paperback).

£2.50. ix, 440 pp. This excellent collection of articles (reviewed JASO vol.2 no.3) is now available in paperback.

Cause and Meaning in the Social Sciences. Ernest Gellner. Edited by I.C. Jarvie and J. Agassi. Routledge and Kegan Paul. London. 1973.

£3.25. ix, 228 pp. A collection of well-known articles and unknown book reviews in the essayist tradition of Voltaire, Heine, Twain, Butler, Shaw and Borges, according to the editors. Anthropologists will not find much of any interest here. Social scientists, no doubt, will be fascinated and impressed. English style sui gellneris and irritating in the extreme.

Race. John R. Baker. Oxford University Press. London. 1974. £6.50p. xviii, 625 pp. Baker's interest is 'with the question whether there is reality behind the idea of race'. From the point of view of sheer industry one can have nothing but praise for his attempts to rectify this state of affairs. If only his appreciation of social anthropology was better founded: 'A language may reveal its superiority or inferiority...by the scope of meaning attached to its words!' Remarks like this throw doubt on his controversial conclusion, 'One must deny...the "fine dictum of morality" that men are everywhere the same'. Criticism aside, however, Race contains as comprehensive a survey of the subject as could be expected in one book. The work is also entertainingly illustrated.

Urban Ethnicity. A.S.A. conference edited by Adrian Mayer £4.50p. Tavistock Publications. London. 1974. xxiv, 391 pp. 'Urban ethnicity' involves the study of 'the anthropology of the complex structure of the new state'. The anthropologist's job is therefore to deal with 'the socio-cultural problems raised by the developing interdependence between these parts and by the processes of socio-cultural change involved in this development'. Such an approach suits the current interest in group/group relations, but is hardly likely to provide 'heuristic and theoretical considerations' of 'major importance' to our discipline: the functionalist tone of many of the contributions, the general concern with 'definition', the existence of much jargon, and the occurrence of simplistic remarks of the type 'I (Deshen) have operated with a conception of ethnicity as a strategy whereby people set bonds of inclusion or exclusion', all suggest the extent to which 'urban ethnicity' is a species of that type of sociology which dates from Homans' The Human Group (1950). What is so surprising, given the 'heuristic' and 'theoretical' pretensions of the work, is that so many contributors write as though their subject is only just beginning. There are articles, amongst others, by Mitchell, Parkin, Lloyd and Schildkrout.