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THREE STYLES IN THE STUDY OF KINSHIP

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J. A. Barnes

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To Frances

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. . . the true road of anthropological science, which is to outline and explain differences and not to keep them hidden behind confused notions.

Lévi-Strauss (1957: 903)

Le preuve de l'analyse est dans la synthèse.

Lévi-Strauss (1960e: 140)

Acknowledgements

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Preface

Most books on kinship contain descriptions of selected social institutions – families, marriage patterns, clans and lineages, kinship terminology, and so on. There are no descriptions of this kind here. This book is on the study of kinship, not on kinship itself. The reader primarily interested in kinship, or in some similar substantive division of social and cultural behaviour, may well ask what need is there for a book whose subject-matter stands at one or more removes from the empirical world. I therefore begin with an attempt to explain and justify what I am trying to do.

This extended analysis of the writings of three anthropologists, Murdock, Lévi-Strauss, and Fortes, in the field of kinship was begun in an effort to assist the transformation of social anthropology from an intuitive art to a cumulative science. For several decades sociologists and social anthropologists have maintained as an article of faith that their dual discipline aims at the discovery of social laws, at the formulation and validation of significant generalizations about culture and society. I have no quarrel with this article of faith but find it chronically embarrassing to have to present such an apparently meagre set of works as our only claim to scientific salvation. In social science there are plenty of high-level tautologies masquerading as laws, and innumerable low-level generalizations which stand up well to test but which cannot readily be linked together in an embracing logical scheme. There are taxonomies galore, many of which sharpen our vision and enable us to see contrasts and connexions previously overlooked. But taxonomies in social science, unlike those in evolutionary biology, have no temporal implication and cannot be proved or disproved, only used or discarded.

There are many people who maintain that this state of affairs is inevitable. Man, they say, is born free, unlike the atom. There is no inexorable regularity in human affairs, and the only generalizations possible about social life are merely statistical summaries of past events, with no predictive power. Other people argue that the fault lies in the phenomena we chose to generalize about. We are too much concerned with accidental features that are the result of specific historic sequences, whereas if only we were to shift to using broad enough categories, everything really significant would be explained by ethology, general systems theory, or something similar. There is merit in both these views. On the one hand the laws of social science must necessarily be of a different order from the laws of physics, if only for the reason that men modify their behaviour in the light of knowledge of the laws to which it is supposed to conform, whereas the planets continue in their courses in blissful ignorance of whether Ptolemy, Newton, or Einstein wears the mantle of orthodoxy. On the other hand brains, men, and societies are nothing but gigantic configurations of law-abiding atoms. It is scarcely surprising that at a sufficiently empty level of generality there are similarities in all thoughts, all systems of interaction and symbolic interchange, all mammalian behaviour, all primate societies, and so on. Indeed, these two contrasted criticisms pose problems for social science that will become more acute in the future. We need to know to what extent the generalizations of social science are self-negating or self-fulfilling prophecies. Once we know how the system works, we think we also know how to make it work differently, and on the other hand, once the mode and mean are published, they become available as approved norms, as for example with Kinsey, so that *what is* changes into *what must be*. The tools of sociological analysis are continually being blunted with the patina of culture. From the opposite point of view, it is just as important to know what limitations are placed on us because we are merely human, what features of social life must be present if we are to survive as a species, what forms of social experiment must fail with our present set of genes, and what potentialities there are in the human psyche that are not yet fully used.

Between these limits lies a vast area of human behaviour where we can take man's membership of genus *Homo*, species *sapiens,* for granted and can also neglect the effect of feedback from the investigator to the societies he studies. This is the central area of social science, and it is in this area that the article of professional faith essentially applies. Here, if anywhere, can we hope to find that interconnected set of empirically validated and distinctively sociological propositions which was the goal of nineteenth-century positivism and which, in varying guises, has remained the target for social science ever since. So far there has been only modest progress. Two explanations immediately come to mind. Either we are not going about the job properly; or else the goal is illusory. Perhaps both explanations are true. The second explanation contradicts the basic article of faith, and most social scientists plump for the first. In a sense, this book also is based on the assumption that the absence of an ordered structure of propositions about one selected portion of social activity, kinship, is due to the inadequacies of the investigations that have been carried out, rather than because those forms of social action and thought that fall under the rubric of kinship are intrinsically unordered, random, chaotic, arbitrary, and unpredictable. But this is not because I reject the second explanation as heretical. I dislike articles of faith, and like to convert them if possible into tested propositions. I would very much like to know to what extent and in what ways, at what level of specificity and within what limits or probability, human affairs are orderly, predictable, and determinate. Articles of faith, like norms generally, as I argue in the first chapter, tend to be drawn in terms of black and white, all or none. We declaim either that we can one day have an all-embracing social science (despite the poor showing so far), or else that human ingenuity and freedom of choice will defeat all attempts at hard and fast generalization about human behaviour (despite the fact that everyday life depends on continual success in predicting the actions of our fellows). From this forced choice science offers us the hope of escape to some calibrated multidimensional continuum with which we can discover and describe how free man is and how much he is bound. My strategy is thus made plain. Assume that the reason for the non-emergence of an accepted, verified, logically organized social science lies with social scientists rather than with their subject-matter; put this to rights so that then, and only then, may we discover how much of a social science is possible.

Strategy is one thing and tactics another. I have no magic formula for building a brand-new edifice of concepts, methods, and techniques which would embody all previously discovered high-level and low-level generalizations in one logical deductive structure, whose design has eluded so many of my illustrious predecessors. Instead of starting afresh, I have tried to work with existing materials; even so, I have not got very far. I have selected three of my colleagues and tried to look at their work, not from a distance in terms of the substantive propositions they establish or suggest, but from close up, in terms of the kinds of problems they set themselves, the analytical categories and verification procedures they use, and the range of application of their results. Instead of looking at their work as a synthesis, I have, as it were, taken it to pieces, in order to study the parts and to see how they fit together.

This process of argumentative dismemberment must, I fear, be rather uncomfortable for the three authors I have chosen and I must crave their indulgence. Evans-Pritchard has remarked that in anthropology every writer tends to be closely identified with the views he advocates, and that it is difficult to criticize views without appearing to criticize their author as well. Therefore I must state explicitly that I am here concerned only with modes of analysis and not with personalities; I hope that the critical attention I have given to the works of these three anthropologists will be recognized as adequate evidence both of the importance I attach to them and of my admiration for their authors, even when I think they are wrong. At the same time I hope I can escape the ill-will of those many colleagues whose work I might have selected for review, but did not. This neglect should not be taken as indicating that I think their work unimportant. The three anthropologists whose work I examine were chosen so as to establish as sharp a set of contrasts as possible, to delineate, I hope, a triangle of extreme polar types that may serve to calibrate the views and styles of most other writers on kinship. Almost all contemporary students of kinship seem to me to belong somewhere within this triangle in respect of aims, concepts, procedures, and styles of argument. I do not attempt to develop a calculus for locating any given writer in relation to the vertices of the triangle, but I think this could be done if necessary.

The trio was chosen because it seemed that I could treat all three anthropologists as independent cases, a consideration that receives particular (and misplaced) emphasis in one of the analytical schemes we shall examine. During the first half of this century, while anthropology was becoming a professional discipline, studies of kinship were dominated by Boas, Rivers, Kroeber, Radcliffe-Brown, and Malinowski. Then in 1949 three books on kinship appeared simultaneously, though each had been in gestation before or during the years of the war. Because they appeared together, it was apparent that there had been no collusion between the authors. The books, *Social structure*, *Les Structures élémentaires de la parenté*, and *The web of kinship among the Tallensi*, differ radically from one another, yet during the couple of decades that have followed their publication each has had a major effect on anthropological thinking, an effect that has by no means been limited to the study of kinship. In the last few years other approaches to kinship, for example componential analysis and human ethology, have received increasing attention, and throughout the period other more centrally placed writers have written on kinship without identifying themselves at all closely with any one of the three I have chosen. But these three writers, and in particular the three books mentioned, provide as good a starting-point as any for a comparative analysis of post-Malinowskian studies of kinship.

It seemed essential to choose for scrutiny writers whose main impact has come after 1945. When I began to think about this book some twelve years ago the traditions of analysis dominated by Malinowski and Radcliffe-Brown were still alive and powerful, particularly in Britain. Although later writers had begun to make their influence felt, a great deal of undergraduate teaching and professional debate still centred on discussions of what Malinowski and Radcliffe-Brown really meant, and their books were an essential part of the literary culture of the subject. I thought that a decisive break with the past was called for, and that anthropology should change its style of publication and the organization of its findings so that it was no longer necessary to keep on going back to the classics for fresh enlightenment. I believed that in this respect, though not in others, we should strive to copy the natural sciences where, so Fortes (1963a: 424) tells us, 'Everything that has not been superseded, whether in theory or in method, or in the matter of experimental data, is embodied in the current body of accepted knowledge, the orthodoxy of the day.' I still think that this is a desirable goal. Nisbet (1966: 19–20) is quite right to argue that whereas a physicist can learn very little that is new about his discipline by reading its classic works, the sociologist can always go back to the classics for information and stimulus. But this is a reflection of the pre-scientific state of contemporary sociology, and I do not believe that it must always remain like this, even though the goal of emancipation from the classics seems to lie further away than I had thought. Indeed, even though I may have succeeded in making some sort of break with the past by excluding Radcliffe-Brown and his contemporaries and predecessors from detailed consideration, I have completely failed to replace or supersede the books and articles on kinship written by the three authors whose work I examine in detail. I hope that the long-term effect of my analysis may be the development of a truly cumulative theory of kinship, but in the short term I hope that its effect will be to encourage others to tackle the works of Murdock, Lévi-Strauss, and Fortes more effectively. Certainly this analysis is not a substitute for what they have written, nor is it in any sense a book of readings or snippets. It is intended as an extended commentary on works that still need to be tackled whole.

The field of kinship was chosen mainly because I thought that I would find here more glory and more knock-down argument, as Humpty Dumpty might have said. The study of kinship has been the central and distinctive feature of social anthropology ever since Morgan, and has reached a level of sophistication that makes it, more than any other branch of the discipline, impenetrable to the specialist in some other branch of social science as much as to the layman. In the study of kinship there are more specialized terms, more definitions, and more would-be theorems, than in, say, the anthropological study of politics or of religion. Here then, there seemed to be a greater probability of finding mature and developed logical structures that could be dissected and compared.

Of necessity I have been forced to treat each writer very much in his own terms, in his own vocabulary, and using his own range of interests and data. The goal of a unified theory of kinship implies the establishment of a single paradigm of concepts, theorems, and accepted procedures of investigation. Unfortunately, with three, and more, rival approaches currently in play, the study of kinship, like social anthropology as a whole, is still at what Kuhn calls the pre-paradigmatic stage of intellectual development (Kuhn 1962: 20). I have tried to put each of the three writers into intellectual and historical context (Scholte 1966), but I am well aware that I have not developed a meta-language into which all three bodies of writing might be translated and related to one another. To do this would be a major undertaking, but it may be a necessary step before the great leap forward to a paradigmatic stage in social science can be achieved.

All three writers are based in the northern hemisphere and their latest work has therefore not always been available to me while I have been writing. In particular this book has been finished before the publication of Fortes's Morgan lectures, *Kinship and the social order*. Had I been able to include this book in my analysis, many paragraphs of Chapter 3 would have been expanded, but almost all the arguments would, I think, have remained unchanged. Likewise Simonis's critique, *Claude Lévi-Strauss ou la 'passion de l'inceste'*, arrived too late to be used. I have tried to base my study not only on the better-known books and articles by the three writers but also on a wide range of their publications; nevertheless I have had to omit from my review several works which have not been available at all in Australia. A fairly complete list of Murdock's works is available in his festschrift volume (Goodenough 1964: 599–603) and an inaccurate list of Lévi-Strauss's works has appeared three times (*Arc* 1965a; *Current anthropology* 1966; Simonis 1968: 357–363). I have therefore included in my bibliography only those works by these two authors that I have referred to in the text. No bibliography of Fortes's writings seems to have been published previously and I have tried to list them all, including those I do not refer to. In the text, when indicating references or the source of quotations, I have often omitted the name of the author when no confusion is likely.

Although I have attempted to make an analysis of all the published work of these three anthropologists, I have probably often taken them much too literally and have divorced statements from the context in which they were made. For purposes of this analysis I have made the heuristic assumption that all three writers are consistently aiming at scientific, and not artistic, explanation (cf. Hammel 1968: 161), even though I think this assumption does not tally with the facts. Indeed, I argue elsewhere (Barnes 1971) that Lévi-Strauss can sometimes be better understood if we make the opposite assumption. In this enterprise my main exemplars have been Abelard's *Sic et non* (1122) and Parsons's *Structure of social action* (1937). Since the penalties of scholastic heresy are now not so great, I have been able to be more outspoken than Abelard; on the other hand I have tried to let my three senior colleagues hold the stage to a greater extent than, I think, is the case with Parsons's book. The product of a union of the expository modes of two such ill-assorted giants has proved to be decidedly rabbinical, but at least I have enjoyed watching it take shape.

When this study was planned several years ago, I intended to cover a much wider field. My polar triangle was to have been a tetrahedron, with the Manchester empiricists led by Gluckman forming the fourth pole. This chapter would have been the hardest of all to write, for I have been fairly closely associated with this group of writers, even if in somewhat antipodean and iconoclastic fashion. These four chapters were to make up only the first half of the book; the second was to consist of a series of case studies, showing how anthropologists from all four corners of the tetrahedron, along with centrally based colleagues, converged on delimited bodies of ethnographic data in tackling ostensibly common problems that had become controversial. I had planned chapters on the stability of marriage, the contrast between cognatic and unilineal, the relation between kinship and politics, and so on. Only one of the case studies was completed, and this has been published separately as *Inquest on the Murngin* (1967c). This appeared at about the same time as the second French edition of Lévi-Strauss's *Les Structures*, containing a revised chapter on the Murngin (1969: 181, f.n.1, 184, f.n.2; 185, 192–195). I have said nothing here about these revisions, and I doubt if and useful purpose would be served by doing so at this stage. I consider that my analysis of the issues raised over the Murngin by Lévi-Strauss, Murdock, and others still stands. In any case, despite Lévi-Strauss's plea for the study of hypothetical societies simulated by computer, it is scarcely possible to continue to discuss the Murngin in an artificially maintained ethnographic vacuum that would be quite alien to the fundamental anthropological tradition of empirical inquiry. For the recent field investigations in northeastern Arnhem Land made by Shapiro (1969 and references therein) convince me that the Murngin, as they are defined in the literature of the Murngin controversy, do not exist and never have existed.

As originally designed the book would have been a monstrous blunderbuss, and the fragments that remain are quite massive enough; nevertheless the volume would have been improved had it been possible to include an analysis of the Manchester group, whose object of study seems to be 'what actually happens'. But this chapter, and the other case studies, will have to be written by someone else, for I have now to direct my attention elsewhere.

1 Safety in numbers

In scientific anthropology, it would seem,
there is safety in numbers.
Murdock (1940a: 369)

1 Safety in numbers

1 Introduction

George Peter Murdock has had a major influence on theoretical studies in kinship and social organization. The publication of his book *Social structure* marked the establishment of a distinctive trend in comparative anthropological inquiry. His interest in comparative studies based on information about a large number of societies from all parts of the world led him to initiate the Cross-Cultural Survey, later to grow into the Human Relations Area Files. He founded the journal *Ethnology* to provide an outlet for publications in this field for articles which 'specifically incorporate or relate to some body of substantive data' (Murdock *et al.* 1962a: 2). His 'World ethnographic sample' (1957a) has been used by many other scholars for a great variety of investigations. Its replacement, the *Ethnographic atlas*, which appeared in instalments in *Ethnology* over many years before being published separately, continues as a sampling frame for general use. We can have no hesitation in identifying a distinct school or sub-branch within social anthropology, characterized by its own method of cross-cultural analysis. Two collections of papers, *Readings in cross-cultural methodology* (Moore 1961) and *Cross-cultural approaches: readings in comparative research* (Ford 1967), provide an ostensive definition of the school and indicate its range of interests.

Yet although quantitative world-wide cross-cultural studies 'have been appearing of late at a geometrically increasing rate' (Murdock 1967: 3), the fundamental assumptions common to these studies do not command unqualified professional support. Now that professional activities as a whole, and not merely cross-cultural studies, are expanding rapidly and new bandwagons threaten to create an indigestible intellectual traffic jam, it is no surprise that a line of inquiry marked out some thirty years ago should have many competitors. More surprisingly, those who have followed other lines have for the most part been content to ignore the cross-cultural method and to develop their own techniques without reference to it. A few writers have stated briefly their unequivocal mistrust of the method, but usually without examining Murdock's arguments and assumptions in detail. For some reason or other, most of the sustained discussion of cross-cultural method has been about blemishes and limitations in the practical application of the method rather than about fundamental principles. Criticism has been directed more at the way ethnographic data should be selected and coded for analysis than at the type of analysis performed. Yet there are many social anthropologists, in the United States as well as in France and Britain, who have no enthusiasm for the cross-cultural endeavours of Murdock and those who have followed him; the quantitative aspect of these studies has met with particular disapproval. A striking example of this lack of enthusiasm was the absence from Britain for many years of any copy of the Human Relations Area Files; this cannot have been due solely to shortage of funds. Silence among the critics cannot be explained by uncertainty about the stated aims and premisses of cross-cultural research, for Murdock has set out the assumptions underlying his inquiries, as he sees them, with great gusto and forcefulness. The technique of inquiry he has developed has its roots in the work of one of the founders of anthropology, E. B. Tylor. It is one instance of what Köbben (1952: 131, 137–138) calls the hologeistic method, whose practitioners seek to 'identify associated variables that transcend the vagaries of historical contact and local conditions' It aims at nomothetic, rather than idiographic or genetic, explanations and its statistical procedures are similar to those used very widely in cognate disciplines such as psychology and sociology, and in the natural sciences. The intellectual credentials of the school thus seem to be impeccably traditional and scientific. The results of applying the techniques of cross-cultural inquiry now form a substantial part of the contemporary literature in social anthropology. We cannot merely ignore them because, for example, we happen to find the structuralist dialectic more exciting, or the ethological approach more firmly based on verifiable fact. If we think that quantitative cross-cultural studies as now carried out are along the wrong lines, we should give our reasons. This is what I try to do in this chapter. I concentrate my attention on Murdock's principal theoretical work, *Social structure* (1949a), and on the strenuous efforts he has made since that book was published to improve his sample of societies and to meet other criticisms.

One distinctive feature of the cross-cultural movement, if we may call it that, is that it has acquired not only a common set of intellectual aims and research techniques but also specialized bibliographic institutions and several key published documents. The Cross-Cultural Survey was established at Yale University in 1937 under Murdock's leadership as part of the Institute of Human Relations. Extracts of published and unpublished ethnographic material on selected societies were classified according to a scheme set out in the *Outline of cultural materials* (Murdock *et al.* 1938, subsequently revised). Material in foreign languages was translated into English. During World War II several handbooks were produced with the help of the Survey. In 1949 the Human Relations Area Files were developed from the Survey. Whereas the Survey is confined to Yale, the Files were established to allow the extracted ethnographic material to be distributed to other universities (Murdock *et al.* 1950: xii–xiv). Both the Survey and the Files were designed to facilitate the formulation and testing of cross-cultural generalizations using quantitative methods. The societies included in the Files were chosen so as to form a fair sample of all known cultures (Murdock 1940a: 369). Later, as the number of societies increased, the objective shifted slightly; the societies in the Files are now seen as forming a collection from which a satisfactory sample can be drawn with minimum effort. Naroll (1968: 254) comments that recently societies thought to be of strategic interest to the United States government tend to have been selected disproportionately. The goal of the Survey is, or was, to cover 'a representative ten per cent sample of all the cultures known to history, sociology, and ethnography' (Murdock 1949a: viii). When writing *Social structure*, Murdock used a sample of 250 societies, 85 of them taken from those covered by the Survey at that time.

While the Survey and the Files may be seen essentially as bibliographic aids, a commitment to quantitative cross-cultural inquiry has also led Murdock to construct a series of standard samples of cultures and/or societies. His *Outline of world cultures* (1954a) establishes a list of all known cultures and suggests a suitable sample. His 'World ethnographic sample' (1957a) contains 565 cultures whose main characteristics are indicated in succinct coded statements. The sample has been used by many other ethnologists as a basis for their own inquiries. A revised version appeared in 1961 (Murdock 1961; cf. Köbben 1967: 9). Publication of the 'Ethnographic atlas' began in 1962. With the twenty-first instalment the Atlas reached a total of well over 1,100 societies. Finally, Murdock has constructed a standard world sample of 200 cultures. A new organization, the Cross-Cultural Cumulative Center (CCCC) will use this sample to re-test correlations found earlier and 'to intercorrelate the findings of different studies and thus raise the rate of scientific accumulation from an arithmetic to a geometric level' (Murdock 1968b: 306). Other institutions have followed Murdock's lead, and a Permanent Ethnographic Probability Sample is being established at Northwestern University (Naroll 1968: 254).

Although the principal stimulus to develop the Survey, the Files, the Atlas, and so on has been the requirements of the cross-cultural method, Murdock has claimed from the start that these research tools can have other uses. The Survey, he writes, 'should prove useful in nearly every type of research which anthropologists and other social scientists have hitherto pursued' (Murdock 1940a: 363). Some anthropologists who criticize the cross-cultural method are nevertheless ready to support the Atlas and similar documents as providing them with handy cues, directing attention to new portions of the ethnographic corpus that may merit closer scrutiny. The Files may be seen as a convenient set of indexed extracts from a huge body of scattered literature, and the Atlas provides an even more succinct key to the contents of ethnographic monographs. It is obvious that, as the amount of ethnographic writing continues to increase, we need more effective ways of finding our way around the literature; and it may well be that the Atlas adequately earns its keep as an index alone. However, the use of the Atlas as a pointer back to the literature is quite distinct from its use as a lead forward to statistical cross-cultural inquiries, and it is with the latter that we are here mainly concerned.

In the Files we have a relatively expensive and elaborate tool for library research. Murdock has been the driving force behind their development and he has been the obvious person to announce the achievements and possibilities of this undertaking. Typically, he has tried to assess quantitatively the efficacy of the Files as an aid to research. He states that with their help one of his articles (1950b) was written in a total elapsed time of twenty-five hours (1950c: 720; 1953: 485), whereas without their aid he would have needed at least twenty-five days. Similarly Udy (1964: 169) reports that he was able to extract all the information he needed ten times faster by using the Files than by reading through the source monographs themselves.

It is therefore not surprising that to many observers Murdock has become identified with a set of ethnographic data organized in distinctive fashion in the Files, as well as with a theory of functional relations between cultural items and a statistical technique for establishing these relations. Thus Nadel is led to note, rather peevishly, his suspicion that 'for Murdock, nothing anthropological is scientific unless it is (a) based on the

Human Relations Area Files and/or (b) contains some acknowledgement of Clark L. Hull's learning theory' (Nadel 1955: 346). A more accurate assessment is made by Leach when he says that, although Murdock may be generally associated with a particular style of cross-cultural comparison, the volume of his collected papers 'is a valuable reminder that Six-Gun Pete has had other aces up his sleeve' (Leach 1966: 1518). Similarly, the vigorous diversity of methods, range, and ethnographic content of the articles appearing in *Ethnology*, and the even wider range of interests shown by his pupils in the *festschrift* presented to their teacher (Goodenough 1964; cf. Fox 1966), give convincing evidence that Nadel was wrong.

In part, the scope of articles appearing in *Ethnology* under Murdock's editorship is explained by the division he draws between ethnographic accounts and comparative studies; we shall have more to say on this later. However, this is only part of the explanation, for he has always held that the cross-cultural method is not the only way to arrive at propositions that are valid transculturally. He has often expressed his approval of the inquiries conducted by Mead in Samoa into the biological and cultural causes of adolescent stress, and by Holmberg among the Siriono into sex anxiety in a society with chronically uncertain food supply, for these investigations were made in field situations where the appropriate variables occurred naturally in the combinations desired. If experimentation with human beings was possible, these are situations one might well construct artificially (Mead 1928; Holmberg 1950; Murdock 1950a: 573; 1951b: 1; 1954b: 27; 1957b: 252; 1966: 97). But since, like astronomers, we cannot experiment, we have to rely mainly on the other method distinctive of anthropology, that of subjecting hypotheses to quantitative comparative tests. Although most of his book *Social structure* is aimed at 'scientific results of universal application' (1957b: 249), Chapter 8 and Appendix A, where he discusses the evolution of social organization, deal with historical (or prehistorical) reconstruction, though some of the ethnographic evidence educed is expressed quantitatively. Elsewhere, as in his book *Africa*, he has pursued