

RADCLIFFE-BROWN LECTURE IN
SOCIAL ANTHROPOLOGY

UNDER THE LINEAGE'S SHADOW

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It was not long before he died that Meyer Fortes asked me if he could propose my name for the Radcliffe-Brown lecture, reminding me that it would be appropriate to offer something on the subject of kinship. That is why I come before you today to talk in general terms about a general subject.

The intellectual relation between Fortes and Radcliffe-Brown was of course very strong, beginning in 1931 at roughly the same time that Fortes got to know Malinowski. Although he had first worked for his Ph.D. upon cross-cultural intelligence testing (1932), he later undertook another study, also of considerable contemporary social interest, on the relationship between sibling order and delinquency, concluding that there was a tendency for the first born to be especially prone to behaviour problems (1933*a*; 1933*b*). It was no wonder then that when he formulated his third and major research project, this took the form of a psychological study of the African family using observational methods and a developmental perspective.

In this lecture I want to explore some of the consequences of what I may call the Fortes paradox (though it is in fact an anthropological paradox) and especially of this attachment to a particular intellectual 'lineage' for the study of kinship, marriage and the family. At first sight it seems bizarre that someone who started out with such a strong disposition towards the study of the family, towards psychology and towards a developmental standpoint should come, in anthropological circles, to be identified with the lineage ('descent theory'), with sociological variables and with synchronic analysis. The change occurred largely, though not entirely, through the personal mediation of Radcliffe-Brown and Evans-Pritchard, who placed

much stress on this particular constellation of interests. In what way were kinship studies affected?

Two of the most concise statements of work in this field were Radcliffe-Brown's presidential address to the Royal Anthropological Institute in 1941 entitled 'The Study of Kinship Systems', and Lévi-Strauss's Huxley lecture of 1965 entitled 'The Future of Kinship Studies' (1966); more extensive versions are to be found in Lévi-Strauss's earlier volume on *The Elementary Structures of Kinship* ('parenté') (1949), in Radcliffe-Brown's introduction to *African Systems of Kinship and Marriage* (1950), and in Fortes' Morgan lectures, *Kinship and the Social Order* (1969). I take the achievements of these authors as established, if only because of the many continuities in research following on from their work. Hence I do not share either the negative assessment of kinship studies made by Keesing (1978) nor the 'destructive' tendencies to which Barnes refers (1971, p. 265), much less one current line of thinking that tries to set aside the analytic concept of, for example, the lineage (Verdon, 1981; Kuper, 1982), without succeeding in offering anything substantial in its stead. As Lakatos has insisted, there is no refutation without a better theory (1978, p. 6). Where is this better theory? What better way of analysing the Tallensi or Nuer?

There are indeed interesting developments in the study of the lineage which have made little impact on anthropology because they do not deal with the usual range of societies. The fuzzy edges of the notion became more apparent in later studies which generated further questions, further refinement. A number of historians of western Europe (among them Guichard (1977), Duby (1972; 1981), Flandrin (1976), Herlihy and Klapisch-Zuber (1978)) have taken up this concept which after all was initially borrowed from Ibn Khaldun, Durkheim (1893), as well as from earlier medieval history. For Europe, this form of kin group is seen not as a continuation of some earlier structure, though clearly genealogical ties had been important, but as an aristocratic construction of late tenth and early eleventh centuries under specific social conditions. Equally J. Watson (1982), and before him Twitchett (1959; 1960-1; 1982), have seen Chinese lineages as composed of the male descendants in the male line of the founder of a specific charitable bequest (*see also* R. Watson, 1982). Such lineages are clearly 'corporate' in quite a different sense from those of Africa, from which they should be sharply differentiated. But the study of European and Chinese forms of what I have called *lignage* adds not only generally to our com-

parative knowledge but also specifically to our understanding of African institutions. This is not a matter for rejection or despair but an opening for creative enquiry.

I don't therefore see much to be said for that long-standing sociological tradition of intellectual *auto-da-fé*, the killing critique, the murder of the father, the rejection of the ancestors, nor yet to any alternative in abandoning general concepts and comparative analysis altogether. Even thick description is not enough to sustain the field without thick analysis. Like Heritier (1981, p. 104), I view such approaches as neglecting important kinds of theoretical interest if the discussion of concepts and the presentation of data has no further end (at least at the level of the subject, for it is always possible to accommodate individuals who wish to direct their efforts towards more restricted aims).

However, recognizing the interest of this work, there is nevertheless reason for thinking that the theoretical study of kinship offers too restricted a set of paradigms, and the result has been some disillusion with their application not only to complex societies but to simpler ones as well.

In his several accounts of intellectual developments Fortes separated off two lines of heritage in modern social anthropology, one going back through Radcliffe-Brown, Lowie and Rivers to Morgan and Maine, the other through Kroeber, Malinowski and Fraser to Tylor and Boas (1969, p. 14). These he regards as producing complementary frames of analysis, the first having to do with structure (and system), the second with culture (and custom). The separation of these two lines was related to the tendency to set up lineage, sociology, synchrony on one side against family, psychology, and diachrony on the other. The first point is touched upon in Fortes' reference to Malinowski, who was firmly placed in the other segment. He poses the question of why Malinowski objected to Morgan—it was, he says, because of the latter's 'purported disregard of the parental family as the source of all kinship ties and as the basis of social organisation' (1969, p. 5). Whereas Fortes, under the influence of Evans-Pritchard's studies of 'Nuer lineage organisation' and his own field experience, was forced 'to come to grips with kinship and descent theory'. 'Happily for me', he continues, 'Radcliffe-Brown was there to show me the way.'

This comment shows an obvious shift from his original plan of work, although of course he contributed to the study of domestic relations as well as of the lineage, even if others were more concerned with kin groups than with kin. But there is a yet firmer

rejection of past experience, at least at the level of ideology. Fortes was initially a psychologist who in later theoretical statements came to stress the divide, which he again saw Malinowski as neglecting, between psychology and anthropology; this view was also adopted from Radcliffe-Brown (and Evans-Pritchard), and farther back from Durkheim.

Radcliffe-Brown too had studied psychology under Rivers and even as late as 1922 asserted the necessity of a general psychological hypothesis in explaining or interpreting the beliefs and customs of the Andaman Islanders (1922, p. 232; Jahoda 1982, p. 21). Later he adopted the strictly Durkheimian point of view that the determining causes of social facts were antecedent social facts (1893 (1933), p. 349), though this assertion depended in part on a changing notion of the psychological, an indeterminate concept at the best of times. In any case these notions were passed on to Evans-Pritchard, Fortes, and most other British social anthropologists, though those more closely associated with Malinowski had a somewhat freer attitude. For Fortes, 'Everything he [Malinowski] wrote was riddled with psychological explanation' because of the nature of his reductionist functionalism (1957, p. 170). Once again, in practice, Fortes fortunately set aside his own implied injunctions.

These aspects of paradigm construction or constriction, the designation you choose will depend on your point of view, had to do partly with the segmentary opposition to Malinowski, partly with the attachment to Durkheim (to which Malinowski was not opposed), partly with the identification with a particular line of ancestors. That line was highly selective, omitting not only those mentioned as belonging to the alternative segment but more surprisingly, at least in the field of kinship and marriage, the great Anglo-Finnish socio-anthropologist, Edward Westermarck, as well as the fascinating study of E. J. Simcox, *Primitive Civilizations or Outlines of the History of Ownership in Archaic Communities* (1897).

Another aspect of ideological restriction with which all are familiar was shared with the opposing segment. This was not so much the rejection of history (*pace* Stocking, Radcliffe-Brown rejected pseudo-history) but rather the commitment, following Saussure, to a synchronic rather than a diachronic frame. I need only comment here that linguists were concerned with the priority of synchronic analysis, not with establishing this as the only form; and even this priority, as Lyons has recently pointed out (1981, pp. 64 ff.), has been considerably modified.

I will later return to the way in which the rejection of psychology and history has influenced the analytic frame for kinship studies. Meanwhile there is a further major aspect of paradigm restriction to consider. In discussing India Dumont recommends the average researcher to stick to the field of kinship for, he says, 'in contradiction to many others, here is a real, self-defining system, I mean a system whose boundary is objectively given' (1961, p. 76). Equally the importance of Radcliffe-Brown's notion of a self-contained system was often stressed by Fortes. The exigencies of any system require (a) a set of boundaries and (b) the inter-relation of elements or component parts. This means both inclusion and exclusion. Fortes is actually aware of the problem when he writes that 'structural analysis', descended from Morgan and Radcliffe-Brown, does not deal with 'all aspects and dimensions of kinship and social organisation'. For, as he goes on to say, 'the family is also the nursery of fundamental emotional dispositions and personality patterns, as well as being the media of economic and ritual institutions' (p. 84). Nor does this framework deal with historical and demographic factors, neglecting too the cross-cultural investigations of Murdock and the alliance model building of Lévi-Strauss. Nevertheless, he claims 'there is a distinctive range of problems presented by the facts of kinship and social organisation that can be better isolated, pooled and made sense of within the framework of structural analysis' . . . that is, by British Social Anthropology out of Radcliffe-Brown.

What does the distinctive range of problems consist of? I want to stress that I am talking about theoretical statements rather than theoretical practice, for there were substantial differences between the two. One of the topics included was obviously that of kin groups, especially lineages. As Leach has insisted, 'the achievements of British social anthropologists lay largely in the analysis of societies with unilineal descent groups' (1968, p. 485). Indeed even those who adopted an alliance approach were concerned with groups of a similar kind. For both approaches lineages were in.

But there is another topic that was especially suited to systematic analysis (in the sense of the analysis of systems), and that is deeply embedded in the intellectual tradition of social anthropology, in France and the USA as well as in this country. That is kinship terminologies.

At the beginning of his 1965 lecture Lévi-Strauss quotes from Rabelais to suggest that he had invented an imaginary Eskimo

kinship nomenclature, which is marked by an internal coherence that confirms its validity. He goes on to comment that the way of thinking behind this invention 'emphasises another theoretical requirement formulated by most of us when trying to explain a kinship system: namely, that the ways parents [*?kinsfolk*] and affines are allotted to specific classes have a meaning, and that from this allotment derive specific sets of complementary rights and obligations' (1966, p. 13). Internal coherence on the one hand, meaning and purpose on the other. And it is the first that he finds so well demonstrated in the study of kinship nomenclatures by Lounsbury (1964*a*; *b*), and Buchler (1964*a*; *b*), who have proved them to manifest 'a kind of logical perfection which makes them authentic objects of scientific study' (p. 13), by refining analytic tools and by reducing a collection of empirical data to a set of primitive elements on the one hand, and a set of rules for operating upon those elements on the other (p. 14).

If we compare this programme with that of Radcliffe-Brown, announced twenty-six years earlier, we find many similarities, though put in somewhat different words. Following upon such a process of reduction we can classify the results and reduce them to a certain number of elementary types. For Radcliffe-Brown considered that one of the first tasks of a theoretical study of kinship was to discover 'a certain small number of general structural principles' 'by a process of abstractive generalization based on analysis and comparison' and then proceed with such a classification and comparison of social structures (1950, p. 2).

This is not the place to list the 'structural principles', the primitive elements and the rules of operation, nor to comment upon their usefulness. But one conclusion that emerges from an examination of these systems is the enormous weight put on kinship terms as indices of social relationships, especially in the context of marriage choices and kin groups. Indeed not simply as indices. Lévi-Strauss is not alone in seeing rights and obligations as *arising from* (note the vectorial factor) the allocation of persons to linguistic classes. At times the study of terminologies comes close, perhaps by slippage, sometimes expressly, to being identified with the study of kinship systems themselves. And kinship systems in their turn (and here the usage is implicit rather than explicit) almost come to represent (at least in the context of simple social systems where, to put it in another form of words, kinship is seen as part of the infra-structure)—almost come to represent the social system (or structure) itself. As if, like Morgan (who collected them by world-wide correspondence, surely the most economical and

cost-effective piece of cross-cultural research ever undertaken), all we had to go on was a set of terms with which to construct the social universe. So it is in kinship terms that Radcliffe-Brown chooses to demonstrate his 'system analysis' (1952 (1941), p. 54), a choice justified by the statement that 'In the actual study of a kinship system the nomenclature is of the utmost importance' (p. 62).

I do not have the time to go into a detailed textual analysis to support these general statements about general trends. If you have doubts I would refer you to the discussion of the 'Eskimo' type of social organization in Murdock's *Social Structure* (1949) (the title itself is a give-away), a category which includes both the industrialized Yankees and the peasant Ruthenians. Or to look at some of the many discussions of Dravidian or Crow-Omaha systems. Here I take one particular example from Fortes' treatment of the Australian aborigines where he remarks that 'for ego his field of normal social relations is integrated from within, so to speak, by devices built into the kinship system and identified in the terminology' (1959, p. 111). From the standpoint of the political economy, we are dealing here with very simple societies. But just as Lévi-Strauss tries to define kinship systems by marriage choices and their related terms, so the tendency of this line of argument is to see the kinship system, even the social system, as 'identified in the terminology'. Writing of the Australian aborigines, he maintains that the 'internal factors of social organisation are the most important' and that 'these are all comprised within the classificatory kinship structure' (p. 103). Again in a note he says, 'I use "kinship status" to stand for the whole gamut of classificatory assignment mediated by the terminology' (p. 104).

The argument might be made that in simple societies of hunters and gatherers, terminologies, kin groups, and marriage choices effectively exhaust their kinship worlds, and that kinship effectively exhausts the social world. In which case a similar argument obviously cannot apply to more complex societies, especially those that are undergoing the rapid changes entailed by even partial incorporation in a world system. Yet the blanket statements are rarely modified to take care of the complex; no distinct claim is made for special theories for special societies, for two types of theory for two types of society as Sahlins proposes, later to dismiss, for utilitarian and cultural explanations (1976, p. 50); kinship is seen as a unitary field, with many internal variants. Hence the general argument of the anthropological armoury that we have to understand the elementary or simple before we understand the

complex (*see* Lévi-Strauss, 1966) must be viewed with much hesitation, since we clearly need drastically to extend our range of vision and of variables when we come to consider the latter. Beginning with elementary forms of kinship as they have been isolated in this way may well lead to a lack of theoretical comprehension of more complex systems—or anyhow a failure to contribute to their understanding—and even a possible distortion of the analysis. For example, writing of the Iban, Fortes tends to identify the kinship system with the whole of the domestic domain; even territorial relations, he claims, have their main significance in serving ‘partially to peg down kindred relations’ (1969, p. 126).

A stronger case could be made out for the paradigm if it were to be maintained that only by isolating certain elements of kinship systems, such as lineages, marriage arrangements, terms, could advances be made in their analysis. Such is perhaps the premiss behind Françoise Heritier’s subtle, intelligent and highly technical volume, *L’Exercice de la parenté* (1981), where she states that by kinship systems she means kinship terminologies—‘at the core of the formation of systems of kinship understood as systems of nomenclature . . .’ (p. 73).

Profit was certainly obtained by limiting the extent of the enquiry in this way. Studies of terminologies, unilineal descent groups, cross-cousin marriage, are among the topics that benefited from this tactic. But tactic it is, a heuristic procedure, not a definition of a persisting independent subsystem. We limit the variables for a particular, restricted purpose, but not to reveal the final truths about marriage, family, and kinship.

The profit entailed a loss. There are certain dangers in this heuristic limitation even in the extreme case of the first Australians; it may, for example, give rise to notions of the persistence of structures because of the longevity of component features. Fortes claims that from the time of Spencer and Gillen ‘Australian ethnography . . . overwhelmingly conveys an impression of relative stability . . . in the patterns of social life’ (1969, p. 104). In his support he quotes data from Meggitt and Hiatt showing that ‘critical marriage norms are upheld’ because they do not make ‘wrong’ marriages. We must accept these data. But it is difficult to believe that there are not other aspects of behaviour between kin (expected and practised) that have not changed during the most traumatic transformation that the ebbing social life of these people could possibly undergo. Did those who made ‘wrong’ marriages move? Has the substantive content of conjugal relations really not

changed with the collapse of the hunting economy in the face of colonial invasion? Do marriage choices and terminology tell all?

Turning from marriage to terms, stability is seen as reflected in the persistence of terminologies (and the implied marriages) over time. But this assumption needs careful reappraisal. If we look at shifts in social behaviour or institutions, not from the standpoint of the transformation of elements in highly generalized models but of the analysis of general aspects of actual changes over time in the light of specified hypotheses, we come to rather different conclusions. For this purpose we can do no better than examine what has happened to our own kinship terminology and marriage practices, the changes in which have been comparatively well documented over the last 1,000 years. Far from setting aside history, it seems essential to use our own past to evaluate hypotheses just because of this wealth of documentation; not in the rather unhistorical way that some have used the history of Teutonic kinship but more systematically.

It is an interesting fact that English kinship terminology has remained roughly the same over the last 900 years. There have been minor changes but none that would greatly disturb a person forced to adjust to changing verbal forms. Yet this was the very period that saw the advent and disappearance of feudalism, the significant changes of the Middle Ages, the Renaissance and Reformation, the agricultural and industrial revolutions, and the profound upheavals of the present century. Family relations have certainly been transformed. But little or nothing of this has been reflected in the terminology. In other words stability in this field is no indicator of stability in family or kinship relations—and certainly not in the wider society. The degree of entailment is apparently limited, a fact that an over-systematized view of the structure and functions of human institutions may fail to reveal.

Marriage choices are more closely linked to other socio-cultural events, but as I have recently tried to show (1983) they were controlled to a significant extent not so much by the religious ethic in the Weberian sense but by the economic demands and values of the Church as an estate of the realm, by the requisites of a social organization. A modification of the abrupt separation between the synchronic and diachronic, at least in this context, is essential to provide an adequate explanation of current and past practice.

That brings us to a related point. The attempt to limit the study of kinship to self-contained systems consisting of a restricted set of elements meant excluding the influence of, for example, the

economy. Lévi-Strauss makes this quite clear in the complaint he levels at Lounsbury whose account of Crow he sees as bringing in 'non-structural' factors in the shape of succession (inheritance). But what is non-structural is simply what lies outside an arbitrarily defined structure. So too Fortes includes politico-jural considerations but seems at times to want to set aside property in explaining lineage organization, partly for the same reason but also for the different, and more valid, one, that he does not want kinship reduced to the economy. Monolithic determinism is correctly seen as another form of paradigm restriction. We are interested neither in reductionism nor in determinism (in themselves) but in explanation. And explanations of kinship behaviour do not always confine themselves to so-called internal variables, let alone to the restricted array selected by the dominant theoretical traditions in anthropology. Tax laws, procedures of charitable accumulation, post-war population policies, all these factors influence the structure of domestic life, not only now but throughout European and before that Asian history.

I would not wish to deny the gains to be made from limiting and formalizing the analytic frame. But the idea that such a restricted array constitutes the whole study of kinship, even if maintained solely on the most general level, seems to me damaging, not only for future developments but for the past and present as well.

In the French *Encyclopaedia Universalis*, there is an article on *parenté* (kinship) which provides a useful summary of the discussion on descent and alliance, on elementary, semi-complex and complex systems. In other words, on kinship without the family, *parenté* without the parents. There is another article on *famille* which makes scarcely any reference to the discussion on kinship, though it does attempt some comparative review of the family and marriage. *The International Encyclopedia of the Social Sciences* (1968) is not much better. It contains three articles on kinship, one by Eggan covering roughly the field of the *Universalis* article, one by myself on descent groups, and one by Pitt-Rivers on pseudo-kinship. In this case 'Family' is entrusted to R. T. Smith who again attempts a comparative review. In the *Encyclopaedia Britannica* (15th edn. 1974) the article on kinship written by Barnes, does, as one might expect, much better since it offers an altogether more comprehensive perspective.

But the disastrous dichotomy between kinship and the family, at least at the disciplinary level, persists and has the effect, on the one hand, of primitivizing the study of more complex systems, and on the other, in simpler ones, of neglecting many aspects that

interest other students of kinship behaviour. As a result much of this kinship theory is almost useless for the study of the European family. That is not my conclusion alone. The position is clearly put by Augustins and Segalen, authors of two notable anthropo-historical studies of rural France over the past 100–200 years, the first on the Baronies of the Pyrenees, the second on South Bigouden in Brittany. Augustins remarks that ‘les grandes théories ethnologiques, filiation et alliance . . . sont ici d’un usage incertain’ (1982, p. 39). Segalen makes a similar comment in her study (1985).

I mention these two authors because, together with Claverie and Lamaison on the Gévaudan (1982), Zonabend (1980), Verdier (1979), Jolas *et al.* (1970), Jolas and Zonabend (1970), and Pingaud (1978) (*les dames de Paris*) on Minot in Burgundy, they have given us a remarkable series of studies of rural France using documentary material over the last two hundred years, as well as oral recollection for the recent past and observations on the contemporary scene. Although I would myself regard some of their extensive search for cycles of exchange, or even *renchainements* or *bouclages*, as being, partly peripheral, hangovers from the restricted ‘kinship’ tradition, they have made substantial contributions to the study of family, marriage, and kinship in Europe (partly as the result of the imaginative funding system of French social research which allows continuous work over many years). In this they have achieved close communication with other scholars, historians such as Collomp (1983) and Delisle (1981–2), sociologists like Bourdieu (1962; 1972; 1977), and with others in that wide spectrum of disciplines co-existing in the *Grande École* founded by Marc Bloch. They have exploited and explored rather than rejected the past; and they have examined the whole range of relevant kinship practices, inevitably concentrating on the household as a productive and reproductive grouping. In doing so they have had to consider psychological factors in the shape of motivation, strategies, and calculation.

I will not comment upon the use of historical material since nowadays only anthropological backwoodsmen have any objections to its use, even if they still struggle with the synchronic/diachronic dichotomy and with that unfortunate, personally felt, need for an exclusive, categorical identity—we are anthropologists, they are historians—that seems to afflict less developed fields of human knowledge, straws to drowning men who cannot stand the pressures of ambiguous ethnicities.

I go straight to the other two points, restrictions of the discipline

and topics. First, on the need to open up the institutionalized restriction of the theoretical field of kinship studies, a notion of past restrictions can be gathered by comparing the range of topics covered by Westermarck in *The History of Human Marriage* (1901) even with such a useful, current textbook (1967) as that of a subsequent teacher at the London School of Economics, namely Robin Fox, where Westermarck is mentioned only for his hypothesis on incest.

But what is a more basic gap in the official anthropology, especially regarding more complex societies, is any attempt to incorporate in that theoretical perspective, the systematic analysis of the household. This concept is not listed in Fox's index and appears only once in that of *African Systems of Kinship and Marriage* (1950). Here I return to the original paradox, which is critical to my argument. Clearly the authors of the articles in the latter volume, which included six of the first seven anthropologists to be elected to this academy, made some notable contributions not only to the study of the household itself (especially Fortes) but to other topics, such as divorce (Gluckman), that fell outside the restricted kinship paradigm. In this respect the book displays an almost schizophrenic approach to the subject. The Introduction remains, after thirty-four years, one of the best available. Yet not only is it remarkably restricted in its coverage but its focus is notably different from the contributions that follow. For example, despite the emphasis on terminologies there are diagrams of kin terms for only three of the nine chapters. Gluckman told me that it was Radcliffe-Brown himself who insisted he include the lists he did. Instead terms were discussed in the context of relationships rather than as abstract 'wholes'. Contextual analysis of this kind must inevitably complicate the problem of graphic representation and bring *la langue* closer to *la parole*. Certainly kinship systems are not treated as enclosed structures built up from a limited set of variables and then subjected to global comparison. Indeed such an enterprise runs right against the alternative, but more Malinowskian, anthropological notion of the holistic approach. But then if our paradigms are too restricted, we are forced to adopt a different approach for each and every season.

In the event, most authors considered relevant aspects of the political systems (especially Fortes and Forde), including the territorial dimension (Evans-Pritchard), while Richards and Gluckman took account of the influence of economic factors. Many of their more extensive monographic studies began with an

account of the 'ecology' and 'economy' as a background to their analysis of the 'social structure'. It is precisely in those monographic studies that the divergence between theoretical statement and theoretical practice (I need to avoid the simplistic opposition between 'theory' and 'practice') becomes most apparent. In his penetrating work on the Tallensi family, kinship, and marriage, in my view the most subtle such study ever written, Fortes analyses not only the complexities of lineage structure and local organization but the whole domestic domain of the household, family and farming. One of its strengths derives from his complete neglect of the Durkheimian dichotomy between psychology and socio-anthropology. So much so that Barnes (1979, p. 195) considers all Fortes' work to be 'based on principles that are basically psychological', an approach running quite contrary to the theory. As we have seen his interest in the developmental aspects of the family (and domestic groups) was also explicit in the research proposal he prepared in 1932 for the International African Institute and entitled *A Psychological Approach to the Study of African Society: the Social Development of the African Child*. In this project he criticizes those anthropologists who 'have confined their attention to a cross-section . . . at a given moment of time', the very point I have made about his later analysis of the Australians. For, like others, he came to insist, at the level of theoretical statement, upon a more restricted, traditional, paradigm of what constitutes kinship, or a kinship system.

As a consequence the world was notionally divided into that of primitive kinship and advanced family, bringing about a host of difficulties for communication between the various scholars concerned in their study. But this restriction also excluded half anthropology from the realm of discourse that was accepted for integration into the corpus of theoretical statement, their own work almost as much as that of others. Why do we exclude from that corpus, as summarized in the articles, texts, and reviews, the work of Bott (1957) and others who followed up her research (e.g. Oppong 1974)? Why does the work on matrifocal families, ageing, fostering, gender, etc., not get into many of the texts on kinship (though it is true that Heritier has attempted to incorporate the latter in an interesting way into discussions of alliance)? Why does classical anthropological theory give us no account of the interrelation of household variables that Chayanov (1966) and later Sahlins (1972) and others were interested in? Why is it that anthropologists find it easier to deal with the Amazonian Indian than the South American peasantry? Why does Segalen, like

Augustins, find so little to draw on in her study of France? Not entirely, I think, because anthropologists are so often dealing in primitive societies (Segalen, 1985, p. 118), but because their theoretical pronouncements on kinship are derived from a tradition that places too much emphasis on a narrow range of topics conceived of in an over-generalized manner. If we are to understand the kinship systems of even simple societies we need to widen that range, even to the extent of including in the domestic domain not only kinship but quasi-kinship and non-kinship too; otherwise we may neglect the domestic role of servants and slaves, equate sex with marriage or forget the concubines and the prostitutes. To treat domestic groups as a subject (one among others) for comparative study in their own right brings our interests much closer to those of historians, demographers, sociologists, and economists, closer to those of the peasantry of South America and rural France as well as of farmers in contemporary Zambia or Taiwan. Across the range of human societies there are a number of factors, clustered and weighted in different ways, that affect strategies of marriage, of inheritance, of succession, of continuity, and of social mobility (many of which are discussed in the works of Bourdieu and others, referred to earlier). As *potentialities* they cluster in particular ways in different types of society; and in any particular group they constitute a range of interlocking *possibilities* for the actors.¹ But such possibilities are rarely dealt with in synchronic accounts, whether structural or functional, since both the psychological element of intent and the temporal element of change necessarily take a back seat, indeed are more often shoved into the boot. A study over time, certainly a study of change, must allow for intention.

Let me be clear (or clearer than I have been) about strategies of kinship and of marriage (or marriage policy). Like others I have used the notion to cover a spectrum of social behaviour which for some purposes it is important to disaggregate. In every-day speech 'strategy' implies an intentional act. But clearly in marriage strategies the intention may lie at the level of the couple themselves, of the parental generation, of a wider group of relatives, or, at a somewhat different level, of actors, in the past or present, who have initiated the customary or legal norms that come to be enforced by sanctions (internalized or external).

In the first of these instances we have to take into account not

¹ We have in fact three levels of strategy, (i) the possibilities for an individual, (ii) the possibilities within a society, and (iii) the potentialities for a type of society (given a certain mode of production, communication, etc.).

only the different perspectives of the conjugal pair but also the fact that these may vary depending upon the age at marriage, and the acceptability of alternatives such as celibacy, pre-marital sex, and cohabitation. The parents too may differ over a marriage; among the LoDagaa of northern Ghana, women seemed more keen on their daughters returning to their natal homes (i.e. making a father's sister's daughter marriage) than were their husbands, who favoured the alternative of the mother's brother's daughter (who was probably of a more appropriate age). The likelihood of an identity of view between parents would seem to be greater in societies where marriage involves the establishment of a consolidated conjugal fund. At other times what is good for one son or daughter is not necessarily good for the next, perhaps because of the reasons Bourdieu (1977, pp. 56 ff.) brings out for the Arabs (one marrying in, one out) or Lamaison (1979) for the Gévaudan (heirs don't marry heiresses or vice versa). In any case the actors may be unaware of the strategic advantages that we as observers perceive, so that we then have to speak of the unintended consequences of social action.

The notion of intentionality becomes further diluted when we as social scientists guess at intention in the absence of any direct evidence of the actor's view of the situation. This is almost invariably the case in dealing with the past but often with the present too—yet it is a question which may need to be raised in both time perspectives.

One particularly revealing case is that of adoption. Widespread in Roman law, this practice (as far as children were concerned) was only reintroduced in France after the First World War (1926) as a mode of coping with the large numbers orphaned by that holocaust. In England and other western European countries it disappeared from the range of potentialities (that is, what I define as strategies at the societal level as distinct from individual possibilities) and I had long wondered why this institution was not available as a 'strategy of heirship', given its apparent utility (Cooper, 1976). The answer was provided by a sermon of a fifth-century priest from Marseille, Salvian, who regarded adoption as cheating the Church out of wealth that should rightly go back to God; to retain it in the family was bad enough, but to adopt a fictional heir was to create an 'offspring of perjury' (Goody, 1983, pp. 99 ff.). I do not know how Salvian's words were translated more generally into action, except that they were very powerful words and represented one view of, one compromise with, the family. But they coincided with the beginning of the

great build-up of Church wealth in western Europe; the following three centuries seeing the Frankish Church obtain one-third of the country's lands. I believe that these facts constitute part of an explanation of one aspect of the structure and functioning of European society over the next 1,500 years, an aspect associated with the alienation of property from families to the Church. It does so because I see adoption (or one facet of it) not as a thing in itself, nor yet as vaguely contributing to social continuity, but as one of a set of systematically interrelated mechanisms that you might expect to find under certain socio-economic conditions. If you do not find them, it is worth asking why.

I also see aspects of the levirate, the obligation of a man to marry his dead brother's widow and breed children to his name, as being similarly involved with the absence of 'natural' heirs. Hence it is interesting that scholars from quite different fields should have discussed the historical shift from levirate to adoption in both India and Rome as well as the fact that in other areas of 'advanced Oriental civilizations', namely Israel and Islam, the levirate exists and adoption does not (and in China vice versa). In other words there seems some 'clustering' of these features that I have called 'strategies of heirship', with societies being characterized by ('opting for') one or the other. This discussion has shifted to an observer's understanding of strategy as, in a sense, potentiality or mechanism. What is especially interesting is the way 'societies' shift over time, adopting one procedure rather than another. As if there was some long-term assessment of the costs and the gains of particular mechanisms, an assessment that must change as circumstances themselves change.

These circumstances include the requirements of both Church and state, of religion and polity. The impact of the Christian Church on the family was often 'unintended', being promoted in more general terms; nevertheless its ideology on kinship has been influenced by the continuing necessity for its own support. Today the state exercises even more overpowering pressures; for example, the 'pension' that used to be provided, in kind, by the child for one or both parent on taking over the farm or enterprise (how many sacks of potatoes, how many shirts) is now the pension provided in cash by the state, employer, or independent agency, so that our personal or conjugal, but no longer familial, income is spread over a whole lifetime, not just the period of the working life. The change is connected with the fact that in industrial society adjacent generations are rarely engaged in the same productive enterprise (though husband and wife sometimes are). The con-

sequences of this shift, not of terms but of meanings, is staggering. Note, for example, the conjugal solidarity combined with high rates of divorce, the dilution of fraternal and filial ties, though sometimes sustained by residential proximity. But this situation cannot be understood except by taking into account factors exogenous to the kinship system itself, factors that have a historical dimension and socio-psychological implications as well as demographic ones. Crudely, with the state providing the pension, the value of children diminishes, at least as contributors to the domestic economy. After an initial period, but less dramatically, the value of parents also decreases. The consequential increase in loneliness brings with it higher rates of suicide and senile dementia, both of which appear to be lower in China where familial responsibility continues to be important, more especially in rural areas (Sainsbury, 1955; Lin, 1953). What will happen to the old, to the sex ratio, and to kinship itself if the one-child policy is made to stick, is one of the fascinating questions for study, providing anthropologists are able to open up, yes, complicate, their systems, paying more attention to the theoretical practice rather than the theoretical formulations of their predecessors. For these two were often at odds, the latter changing if not seasonally, at least over the developmental cycle, precisely because of the inconsistencies to which they gave rise.

When I gave the title to this then unwritten lecture I did so with intentional ambiguity. The lineage referred not only to a term of art for certain forms of kin group but also to the very distinguished predecessors, under whose ample intellectual shadow we work. For some their achievements and their formulations appear to be causes for despair, rejection, or withdrawal. I have argued that such reactions are out of place. We can accept both critically and appreciatively, the work of Radcliffe-Brown and others of the line. But we need to enlarge its scope, following not so much their abstract theoretical statements and restricted paradigms but their hypotheses of the middle range. History and psychology may then become friends whom we marry rather than enemies whom we don't; the family and household may enter fully into the analysis of the domestic domain and its mode of livelihood; servants and hired hands may take their place at the table beside the family labour; production may intermesh with reproduction. The ancestral lineage will then be something to take advantage of, to build upon, not to feud with.

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