

# Ancestor Worship in Anthropology: or, Observations on Descent and Descent Groups<sup>1</sup>

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THE DEFINITIVE CHARACTERISTIC of "ambilineal," "non-unilinear," or "cognatic" systems of descent-ordered grouping is often stated to be that each person may choose his own "descent group" affiliation. Whether this is accurate is not the concern here, nor shall we consider whether so-called ambilineal and unilinear systems are truly contrastive in this respect, as has occasionally been suggested (by, e.g., Fortes 1959; Leach 1962; but cf. Schneider 1965). The matter of concern here is that because of this presumed feature some anthropologists argue that these groups ought not to be called descent groups and, further, sometimes question the use of "descent" itself in this context. For these authors (e.g., Goody 1959, 1961; Fortes 1959; Leach 1962), "descent" denotes the genealogical criteria for membership of descent groups, and the latter are those corporate bodies which are "closed" or mutually exclusive on genealogical grounds. Thus, only unilineally bounded groups may be called descent groups, and only unilinear genealogical criteria for membership of groups may be called descent. In contrast are the usages of, for example, Davenport (1959), Firth (1957, 1963), Goodenough (1955, 1961), Murdock (1960), Peranio (1961), and many others, who, despite some differences in phrasing, would describe a social unit as a descent group if one of the criteria of entitlement to membership is demonstrated or accepted genealogical connection with (i.e., descent from) its founder or some less remote member of it. Some of the latter would continue to denote by descent itself the genealogical

criteria for entitlement to membership of such groups (and see also Goody 1961:7-8; Leach 1962:132). Others, however, would denote by descent "relationship by genealogical tie to an ancestor" regardless of the social significance assigned to such ties or to specific forms of them (but cf. Fortes 1959:206).

Choosing between these usages requires consideration of several matters, one of which is heuristic: How are the terms descent and descent group best used to facilitate comparative analysis and the formulation of useful sociological generalizations? One's answer to this question must depend upon one's sociological perspective. For instance, Fortes' exclusion of groups having other than unilinear genealogical criteria of entitlement to membership from the category descent groups and his exclusion of cognatic descent-constructs from the category descent derive from his holistic structural-functional perspective (see Fortes 1959, 1963; and for further discussion see Leach 1962 and Schneider 1965). But no matter what one's sociological perspective may be, an argument for terminological usage is also obliged to be coherent and logically defensible. If it is not, then distinctions made for heuristic purposes may lead only to the replacement of one confusion by another.

The Fortes-Leach-Goody usage of descent and descent groups signifies and emphasizes a number of differences in group and "total" social structure which are perceived as related to the jural attributes of different genealogically-phrased organizational principles. Fortes perceives these differences as so radical as to require equally radical terminological and typological separation (see especially Fortes 1959: 210-12). However, the displacement of "ambilineal principles" and "ramages" (Firth 1957) or cognatic descent groups from the categories descent and descent groups on these grounds is at best unnecessary. We need not draw such radical dichotomies in order to recognize that unilinear descent is, or may be, different from "ambilineal" or cognatic descent as an organizing principle and that the unilinear descent groups may differ in certain structural and functional attributes from

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"ambilineal groups". We need only be aware that for some sociological purposes the important issue is not whether a group is in some sense a descent group but whether it is a unilineal or other form of descent-ordered grouping. Furthermore, since no group is a descent or kin group in any "absolute sense" (Fortes 1959:211), for yet other sociological purposes the important issue may be whether the groups compared are political, economic, or religious corporations, or all of these, rather than descent groups of one kind or another (see, e.g., Befu and Plotnicov 1962 for an unintended example of the analytical irrelevance of descent group typology).

The apparent necessity of the Fortes-Leach-Goody dichotomy derives from conceiving descent as the genealogical criterion for entitlement to membership of groups. Much of the fuel sustaining the polemical fire on this subject has been provided by W. H. R. Rivers' (1915, 1924) early comments on descent (see e.g., Goody 1961; Leach 1962; Salisbury 1964). These hermeneutic endeavors have been relatively unproductive, however, for although some attention has been paid to the problems Rivers had in mind, little attention has been given to how these problems might better be resolved in the light of a considerably more informed comparative ethnology than Rivers had at his disposal. Rivers was particularly troubled by the then common practice of reporting as descent, without further specification, various "social processes... which are entirely distinct from one another"; i.e. descent "has been used indifferently for the way in which membership of the group is determined, and for the modes of transmission of property, rank or office" (1924:85). To resolve the resultant ethnographic confusion, Rivers proposed that these processes be called, respectively, descent, inheritance, and succession (1924:86-88). Descent would thus denote "the process which regulates membership of the social group," and, Rivers added, "the term is most appropriate when the community is divided into distinct social groups" (1915:851). He offered scant justification for the latter point and seems to have been concerned only to distinguish units which are socially defined from those which are not, but in 1924 he expressed his argument more strongly: "the use of the term [descent] is only of value when the group is unilateral" (1924:86). Rivers offered no justification for this usage other than the desirability of establishing an unambiguous convention.

This proposed convention was of some value for dealing with the fact, not sufficiently appreciated at that time and not adequately conceptualized by Rivers himself, that various kinds of kinship connection may be recognized in the same society, each for different purposes. But Rivers' solution is, nevertheless, inadequate and indefensible, for he failed to take into account that descent, then as now, also denoted "relationship by genealogical tie to an ancestor," so that terms like patrilineal and matrilineal had, and still have, as their minimal connotations particular *kinds of genealogical continua* connecting persons with their ancestors. Wherever such forms of genealogical connection are *recognized* in a particular society, we may speak of "descent-constructs." The confusion between ties of filiation and ties of descent (see Fortes 1959: 206-7) also escaped Rivers' notice. Finally, he failed

to recognize certain terminological problems which necessarily arise out of his solution: What are we to call those genealogical criteria for group affiliation which are not unilineal and those unilineal and other forms of genealogical construct which are not criteria for group affiliation? These are important problems, for they have led others, attempting to accommodate to Rivers' dicta, to coin a variety of terms labeling the genealogical criteria for and the processes of group affiliation in societies without unilineal descent groups (again cf. Leach 1962: 131).

A further difficulty is that if it is admitted that unilineal genealogies may serve as other than criteria of group affiliation, and Rivers clearly thought they could, then his usage is arbitrary inasmuch as it would place identical genealogical constructs in different descriptive-analytical classes on the basis of the social functions with which they happen to be associated rather than on the basis of formal similarity or dissimilarity. Moreover, even if unilineal genealogical constructs were always and necessarily criteria for the formation of groups—and one could define "group" in such a way that the argument could be made—there would still be no need to restrict descent itself as Rivers did. We may continue to recognize affinities at one level (the cultural) while also recognizing that members of subclasses at that level may, or perhaps even necessarily do, have different social functions. This is, in effect, what Leach does when he says, "to be pedantically accurate one might perhaps say that, in [ambilineal] situations, the *potentiality* of kin-group membership is based on an ideology of descent" (1962:132). (This statement clearly reveals that Leach, too, has difficulty in doing away with the purely genealogical connotations of descent, even in the context of a sustained polemical effort to read such connotations out of anthropological discourse!)

To put this argument in another way, the problem facing Rivers was three-fold: In the ethnographic world we find various and numerous kinds of *recognized genealogical connection*, and these are incorporated into, or sometimes only manifest in, *norms* for the regulation or validation of participation in numerous kinds of *social transactions or processes*. Since a single type of genealogical connection may have normative significance in several different transactional spheres, we need terms to describe each of these three phenomena separately. At times we may want a term to describe some empirically common complex of them; but it would be logically indefensible and certainly confusing to label a complex by reference to one of its elements (e.g., unilineal descent) unless it could be demonstrated that the complex is unitary in that its elements occur only in the context of that complex (this appears to be Fortes' argument). Even if the latter could be demonstrated, it would still be an error to label the complex by a term (e.g., descent) more commonly used to designate the superclass of which one of its elements is a member; and this is what Rivers' solution and some of its derivatives have done.

The simplest and most defensible solution to the conceptual problem facing Rivers would have been to distinguish between (1) cultural or ideological constructs and the social processes they may regulate or validate and (2) types of social processes and types of cultural forms, e.g., rules or their components. If

we do this, we may distinguish group affiliation, succession, and inheritance *processes* in terms of the kinds of status involved in each, as Rivers had to do in any event; we may recognize as descent-constructs some of those genealogical forms which may occur in each of these three contexts of status transmission; and finally, we may designate as descent-phrased rules those norms which incorporate, or in which are manifest, the various genealogical forms we call descent-constructs. All of this Rivers failed to do because of deficiencies in the conceptual apparatus of his time. The necessity of distinguishing between ideational (cultural) forms and social transactional structures or processes was not yet apparent, and, as a consequence, Rivers could not adequately express his highly significant recognition of the fact that the same cultural form may serve various social purposes or functions.

These observations should suffice to dispose of Rivers' demand, and others like it, that descent should denote the process regulating membership of social groups, and, one trusts, his ghost may be laid to rest, no longer to trouble an anthropology which has outgrown the "theoretical" basis of his terminological dogmas. We must now consider how descent-constructs may be distinguished from other genealogical forms.

Descent-construct, as understood here, refers to formulations of genealogical connections between persons and their ancestors; these constructs are recognized types of lines of serial filiation or genealogical continua (cf. Fortes 1959:206-7). Seriality or continuity is the critical notion here, for it is essential to distinguish, as Fortes does, between ancestor-descendant and parent-child ties. We may speak of such constructs construed as criteria for group affiliation as one form of descent-phrased rule, as suggested above, but we must reject as misleading and uninformative such phrases as "descent rule" and "rule of descent," for they only perpetuate the confusion that Rivers failed to resolve.

Descent-constructs should not be confused with the simple recognition of several successive filial steps, which is a more general phenomenon constituting the basis of what we call kinship. Descent-constructs necessarily subsume filial ties and depend upon the latter for their very conceptualization, but they are different from the latter, being another form of abstraction from or conceptualization of genealogical data. A parent, however parentage may be defined in the cultural system under consideration, may be both a "parent" and an "ancestor" (or either/or), depending upon the context, and a person may validate his descent identity by demonstrating his parentage or siblingship. Likewise, a grandparent may be either the "parent of a parent," linked only by successive filial ties, or an "ancestor," linked by descent, again depending upon the context. In other words, a parent may be linked to his or her child by descent ties or filial ties, and the former occurs where their position in a genealogical *continuum* is at issue (again see Fortes 1959:206-7).

Descent-constructs are readily recognized (by the analyst) where they are conceptualized as such—given a label—by the actors, but at times they are only implicit in other concepts. The latter may often occur in "lineage systems," where agnatic or uterine descent may be seen as "naturally" entailing the right and

obligations of group membership. Among the Tallensi, for instance, "the generic concept of kinship, *dcyam*, subsumes all kinds and degrees of genealogical relationship, however remote" (Fortes 1949:16), but more specific forms of kinship or "consanguinity" are recognized. *Soog* stands for "matrilineal descent [not simply filiation] . . . in contrast to patriliney." *Saaret* (plural) are "uterine kin." Fortes does not give a comparable term for "patriliney" or for persons who are agnatic kin, but he does note (p. 31):

People who are *saaret* define themselves biologically as 'the offspring of one womb' . . . but far less commonly, one hears people defining themselves as patrilineally related by the corresponding phrase 'to be the children of one penis.'

It would appear that Tallensi may contrast kinship in general with matrilineal kinship and this with patrilineal kinship, but to them patrilineal kinship naturally entails common lineage membership with all of its associated rights and duties; they find few occasions to divorce the concepts of lineage-mate and patrilineal kinsman and perhaps fewer still upon which to divorce the notion of agnatic descent from lineage membership. A *concept* of agnatic descent is none the less implicit in many Tallensi concepts.

It is not surprising to find that, from the point of view of the actors in a "lineage system," patrilineal descent and kinship are intrinsically associated with lineage membership; but it is a normative association, *and both may be conceived apart from lineage membership and apart from the presence of lineages* (as socially recognized and corporate solidary groups). This is, of course, particularly true where patrilineal or matrilineal descent is solely a principle of succession to a single or a few political offices (e.g., kingship or the headship of groups without unilineal constitutions), but where patrilineal descent is relevant to all persons in a society this statement will still hold true.

Its truth depends upon definitional matters, the resolution of which may depend upon either ontological or methodological issues. Let us consider the methodological issues.

When membership of a descent group, so-called, designates "only rights and duties to individuals and these do not require mutual action or entail making decisions" (Davenport 1963:181)—which would most certainly be true of a unit whose "corporate possession is as immaterial as an exclusive common name" (Fortes 1959:208)—no one's definition of descent is at stake, but the concepts "groups" and "corporate group" are. To be sure, such units may be jurally discrete and they may even be held to be corporate, if one is willing to so dilute the sociological value of the concept; but it is doubtful that they are "groups" in any significant sense, for they may never "convene," and they lack that host of organizational features which many sociologists and some anthropologists find essential to the concept "group" (see e.g., Goffman 1961:9-13; Spratt 1958:9-22; Freeman 1961:202-3). If this is accepted, then many lineages and clans are not "groups," even though they may

be socially recognized and have names (cf. Goody 1961:5), and there is thus, by definition, no intrinsic connection between unilineal descent and descent "groups."

The issue, then, is which concepts of group and corporate group are sociologically more useful? Is Fortes on firmer sociological ground when he argues, "theoretically [descent groups] are necessarily corporate groups, even if the corporate possession is as immaterial as an exclusive common name or an exclusive cult" (1959:208), thus suggesting that he would denote by group a unit with even fewer organizational attributes; or when he argues for a concept of corporate group which requires of the unit that it convene and be concerned with matters of strategic economic or political or religious significance about which decisions must be made (1953:36)? If our typologies of social forms are to be useful in generating hypotheses and generalizations about the behavior of classes of persons and classes of aggregates of persons, then there can be little doubt that we must affirm the wisdom of the second choice; and consequently we must reject any attempt to define descent by reference to "groups."

Since comparative ethnology is not a pastime of peoples in the so-called primitive world, it would be difficult to imagine why forms of descent would be conceived in a society without assigning to them some social significance. They must almost certainly always pertain to the establishment of "pedigrees for some social purpose" (Fortes 1959:207). Unilineal descent may well pertain to the rights and obligations of membership of established groups—commonality of descent does not itself "make" a group—and these groups may be, for jural or legalistic purposes, "mutually exclusive" on genealogical grounds as a consequence. Other forms of descent-construct may be of such a nature that they cannot serve as criteria of entitlement to membership of groups or, if they may do that, cannot allow the groups to be mutually exclusive on genealogical grounds alone; such constructs are none the less descent-constructs, and patrilineal and matrilineal genealogical constructs remain descent-constructs even when they do not regulate membership of groups. What forms might non-unilineal descent-constructs take? There is, of course, a decided limit to the number of kinds of descent-construct that may be conceived, just as there are practical limits to the uses to which different kinds of descent-construct may be put. The former limitation derives from the bisexual nature of human reproduction and the latter from the logic of the constructs themselves.

Cognatic descent-constructs are those in which sex of the linking kinsman at each step is immaterial for the tracing of the continuum and the continuum itself is significant. Such genealogical continua may relate to membership of groups.

"Ambilineal," "multilineal," "optative," "nonunilineal," "omnilineal," and "bilateral" have been used in reference to the type of descent-ordered grouping here termed cognatic. Firth (1963:26-27) has noted that certain of these terms are not congruent with "unilineal," which denotes "one form or type of [genealogical] line in consistent principle," for ambilineal and multilineal denote not "many types of principles of tracing descent, but many lines combining

two principles," and ambilineal is "antithetical to unilineal" because it means "the use of descent lines involving both principles, descent through the mother and through the father." By "both principles" Firth apparently alludes to his frequent characterization of "ambilineal" descent groups ("ramages") as those in respect of which "both parents are available as points of attachment." This characterization is questionable, since it would seem to have been the case among the Maori (see, e.g., Firth 1936:583; 1963:27) that validation of membership claims by reference to parental ties presupposed eventual juncture with the apical and founding ancestor of the group and that, ultimately, what was involved even in a claim on grounds of filiation (in Fortes' sense) was linking oneself with a *line of descent* (for a similar situation in Melanesia see Scheffler 1965). Furthermore, where descent is reckoned through males and females, or through both parents, we have one principle—cognatic descent—not two. Either parent is "feasible" as a link for purposes of group membership, or "sometimes a parent is ignored and recognition claimed through a grandparent" (Firth 1963:27), because the criterion is cognatic descent, not filiation (cf. Fortes 1959:210-11).

Firth's characterization of the genealogical grounds of group affiliation among the Maori and in similarly structured societies unfortunately confounds Fortes' valuable distinction between descent and filiation. Also, Firth's usage of "ambilineal" to denote "the maintenance of group continuity through the generations by using male or female links without set order," thus indicating a social process, not a type descent-construct or even "principle," further confounds principles and processes. (For further comment on "ambilineal" see Service 1962:163.)

The only other type of descent-construct logically possible is sometimes called "alternating descent," and here the genealogical line alternates regularly between males and females as links. That this form of descent-construct is rare is not surprising, for it offers considerable problems for the transmission of status of any kind. It would be difficult indeed for the "alternate descendants" of any particular ancestor to form groups, and it is difficult to imagine what advantage there could be to an alternating descent arrangement. Mead's (1935:176-88) account of the Mundugumor "rope" is perhaps the sole ethnographic example of alternating descent-ordering of certain transactions; it is reported that inheritance of all property, "except a share in the patrilineally descended land," is regulated by "ropes," but there is no indication that "ropes" form corporate groups or even groups. They seem to figure most prominently in complex and protracted marital alliances which are, however, "never carried out in practice."

"Parallel descent," where for certain purposes men trace patrilineal and women matrilineal (see e.g., Maybury-Lewis 1960), is not a form of descent or descent-construct, as these terms are employed here. The descent-constructs involved, if any, are quite simply patrilineal and matrilineal ones, and parallel descent refers to a feature of the *system*, the way in which its components complement one another, not to the components themselves. Such arrangements also appear to be rare, though a few occur in South America. Perhaps, as Maybury-Lewis notes, "the only known in-

stances of it are in conjunction with ordinary unilineal systems" with parallel descent as an "ancillary institution." (There is also the possibility that some so-called parallel descent systems are not *descent*-ordered at all [see Lounsbury and Scheffler 1965].)

Similarly, "double descent" refers not to a type of descent or descent-construct but to the way in which patrilineal and matrilineal descent-constructs and descent-phrased rules complement one another within particular social systems.

One final aspect of Fortes' comments on descent demands attention here:

It is obvious that in systems where a sibling succeeds or inherits 'in preference to,' i.e., by priority of right over, a child, descent is the critical factor, for a sibling is closer to the source of the deceased's 'estate'—a common ancestor—than is a son or daughter. But where succession and inheritance devolve on sons or daughters 'in preference to' siblings, this is governed by filiation. The rule of so-called primogeniture is in fact, analytically speaking, a rule of succession by filiation (1959:208).

But if this were accepted, what would we do with those situations in which a senior son is, from the actors' points of view, closer to the ancestral source of claim to office? Such was perhaps the case among the Maori, where succession to headship of a *hapu* devolved upon senior sons since, in the Maori view, they were closest to the ancestral source of *mana* or power through which group welfare was maintained. *Mana* was vested initially in group founders and thereafter descended in the *lines* of senior sons. A senior son had priority of right not only because he was his father's firstborn son, but also because he belonged to a *line* of firstborn sons descended from the group founder, and the *mana* which validated office-holding always remained in that line. A firstborn son could be disqualified from most of the political duties of the office if he were incapable of performing them to the group's benefit, but the religious duties always remained his (Firth 1929:95).

Why is this any less a descent phenomenon than succession by sibling seniority? Could not primogeniture and senior sibling succession be viewed as *variant expressions* of descent? They may be so viewed if descent is defined as "a genealogical connection recognized between a person and his ancestors" (Fortes 1959:206); but Fortes departs from this simple and sound usage to a concept of descent as it is manifest among the Tallensi, and it is the Tallensi model that becomes the preferred analytical model. A concept or complex of concepts designed to fit one society, or at best a few, cannot fit others very well, if at all. If our analytical concepts are to prove generally useful, they must be relevant to a greater range of societies than a Tallensi-based model allows.

The confusion surrounding "descent" is somewhat akin to that surrounding "marriage," "social structure," "religion," "magic," and other terms basic to social anthropological discourse. The tendency among (notably) British scholars has been to argue that such terms are best left underdefined, since any rigorous definition is bound to result in inapplicability of the terms in relation to some societies where, perhaps intuitively, we feel that the terms are really appropriate

(see, e.g., Leach 1961a on "marriage"). The problem is that each new culture or social system presents a new structure which must be understood, at least at first, in its own terms. Happily, however, each new culture is not unique, for if it were, comparative and general studies of any kind would be precluded. Rather, each new structure offers another conditioned variation, another more or less different organization, of already more or less familiar materials, and one of our problems is to develop a metalanguage which will facilitate comparison and generalization while at the same time maintaining some respect for both the integrity of natural systems and the similarities and differences among them.

(A critical difficulty with the Leach-Fortes-Goody usage of descent, which would confine it to "unilineal genealogical criteria of entitlement to membership of groups" is that it dwells too much on the integrity of some cultural systems at the cost of an inability to recognize and to deal with intersystem similarities and differences using the same body of terminology and theory. Fortes (1959) offers us a holistic or unitary model "descent system." It is one whose parts, if one may speak of parts in such a context, are aspects, rather than elements, of a system and definable only with reference to their roles in the whole system. The parts are defined structurally and functionally, and their function is inevitably *the social function* of system maintenance, more immediately the formation of discrete, exclusive groups. Yet the more immediate function or operational significance of similar genealogical constructs in societies other than those considered by Fortes may not be group formation, and, this being the case, it is, as argued above, unsatisfactory to link structure and function by definitional fiat. No matter how convenient it may be from one point of view [see e.g., Leach 1962:132], it is both logically and empirically indefensible.)

Social and cultural forms are mutable, transposable, and variously combinable, though surely not infinitely so. Some are more or less substitutable for others without requiring that the *whole* systems of which they are parts be changed as well. Therefore, models of systems of social and cultural forms ought to be mutable and transposable (see Lévi-Strauss 1960, 1963: 279–80). Mutable structural models are of such a nature that one may be viewed as a transformation, permutation, or conditioned variant of another in the same series. The models share some elements, and the elements shared may be identical or understandable also as variants of one another. Models of whole systems—and these need not be of whole societies—may be defined in terms of the ways in which the elements or their variants are combined, and what would set one model off from another would be not so much the elements shared or not shared as the conditioned variation in the ways in which the elements are combined.

There is much to be gained by regarding descent as a primitive element of such structural models; or, to put this in a more useful language, the genealogical elements of certain models may be labeled as types of descent-construct. We may then go on to construct

models which vary in the pervasiveness<sup>2</sup> of descent-phrased rules of one or another type as well as in the types of descent-construct involved (as in so-called parallel and double descent systems). This should allow us to overcome the problem noted by Stanner (1961: 21), who observed that "we have not yet found a way to generalize adequately the degrees of recognition" given to different forms of descent in one and the same society; nor have we been able to discuss adequately the matter of variation in spheres of relevance of the same or different forms of descent-construct. These difficulties have been needlessly perpetuated by adherence to the logically and empirically indefensible "conventions" proposed by Rivers.

Here we must return again to the problem of "descent groups." Leach has asked, "if we are to distinguish societies with unilineal descent groups as a special type, where do we draw the line?" (1960:117). He offers no solution to this problem, but perhaps he would agree that no immutable line is possible; where one draws the line depends upon the problem or problems at hand. The difficulties of drawing the line have to do with "group" rather than "descent." Unilineal descent-constructs may be present or absent, whether explicit or implicit, and a rigorous formal analysis of the relevant indigenous conceptual system should be able to demonstrate which is the case in any particular instance. But social units exhibit varying degrees of formal and informal organization and pertain to various activities and interests, and which of these one wishes to dignify with the label "group," or perhaps to aggregate into some other class (e.g., social field or category) must depend upon the problem one has in mind. In particular, analysts with different problems may find it useful to draw up more or less different lists of the formal or informal attributes a social unit must have to be included in the group class (cf., e.g., Goffman 1961:7-14 and Spratt 1958:9-22). But we can perhaps agree that the unit constituted by all those persons who "share a common name" and nothing more is hardly worthy of the label in any event.

The question of how descent-constructs must be related to groups before we shall call them descent groups involves similar issues. For some purposes there may be considerable analytical value in regarding a group as a descent group if the members of it understand the unit "as being composed of descendants of a common ancestor or pair" (Service 1962:31), regardless of the kind of descent considered relevant to membership. Beyond this, descent-phrased rules may variously prescribe or merely confer entitlement to membership of established groups and descent-constructs may variously conceptualize group structure, composition, or even interrelations. To speak of descent groups in all of these instances, as anthropologists have, can be confusing and uninformative; but this need not lead us to accept equally difficult restrictions upon usage. It should instead serve to remind us that it is never sufficient simply to speak of "descent groups" (or of "marriage," "religion," etc.) in an ethnographic description or in ethnological comparison; it must always

be specified in what way the group is a descent group, i.e., how a descent-construct or descent-phrased rule relates to it. We should also remember that the classification of groups on the grounds of their descent ideologies is not on a par with classification of them on "functional" or "operational" grounds, as, e.g., political, economic or religious groups. These latter may be forms of corporateness, and they pertain to the substance rather than the ideology of group membership. Since it is probable that the substantive nature of groups is a better index of the conduct of members (and non-members) towards them than are any other defining features, "operational" typologies may prove to be more generally productive of sociological generalizations than ideological typologies. If so, descent ideology cannot be a primary taxonomic consideration, but it does not thereby become unimportant. In this view, to say of a group that it is a descent group, or even a corporate descent group, is not to say very much; it is only to indicate in a very general manner an aspect of group ideology.

It should be obvious, then that the Maori *hapu* and some of those Oceanic social units described as "nonunilineal descent groups" by Goodenough (1955) may be called descent groups. They are descent groups because, first and foremost, they are, as localized and major economic, political, and religious factions, *groups*, and because their members conceive of themselves as sharing common ancestry and as being variously obligated to one another by virtue of that fact. To call them descent groups is merely to note the ideological aspects of their organization which are generally salient for their members; it is not to imply that they are groups by virtue of their descent ideology. But for that matter, neither are unilineal descent groups (or lineages and clans) groups by virtue of their descent ideologies.

The possible objection that there is no guarantee that the class "descent groups" so defined is of any sociological significance has been anticipated. It has been suggested that an "operational" typology of these and other groups, utilizing criteria or combinations of criteria such as forms of and degrees of corporateness (see also Schneider 1965:47-49), would better facilitate the formulation of significant generalizations about such groups, their operations, and the social systems of which they are constituents. Similarly, Leach (1961a:104) has suggested that our comparisons "must start from a concrete reality—a local group of people—rather than from an abstract reality—such as the concept of lineage or the notion of kinship system." Even from this perspective, however, the concept of descent group, as here defined, is of some value.

Fortes (1953:36) has observed of African lineages that "it is not surprising" to find that they are "generally locally anchored," for "a lineage cannot easily act as a corporate group if its members can never get together for the conduct of their affairs." It might also be argued that without this "getting together" lineages would not be groups to begin with, much less corporate groups. Therefore, where lineages are groups they are such, and they are solidary, to the extent that they are sustained and multifunctional gatherings, that is, local groups. Yet, Fortes argues, lineages are "not necessarily territorially compact or exclusive"

<sup>2</sup> Fortes' (1953, esp. 25-26) discussion of variations in unilineal descent group structure could well be phrased in terms of such models.

and a "compact nucleus may be enough to act as the local center for a group that is widely dispersed" (1953:36). How then might we formulate the relation between lineage and local group?

For many African societies, especially those described as "segmentary lineage systems," the distinction between lineage and local group is largely a cultural and also an analytical fiction. As the observations of Middleton and Tait (1958:3-8) suggest, one is not dealing so much with two kinds of grouping as with concrete groupings on the one hand and the *idiom* of or for their organization on the other. It is only where the lineage idiom orders relations within and between local groups that it may be argued that "the lineages... compose a total structure" (Middleton and Tait 1958:5) or that the total social order may be analyzed as a lineage system. In these societies (for an enumeration see Middleton and Tait 1958:12-30), those units which "act as lineages" in most contexts are in fact local groups which are variously composed (see also Firth 1959:214-15; 1963:23-24); their members relate "as lineage-mates" and the local groups relate "as cognate lineages." In other types of "lineage system," lineages continue to provide the "cores" of local groups, but relations between these groups are not so pervasively phrased in the lineage idiom and, as among the Yakö (see Forde 1963:41-42), modes of affiliation other than unilineality may be deemed perfectly legitimate (for further discussion of variation in lineage systems see Fortes 1953:25-26; Middleton and Tait 1958:1-3; Lewis 1965).

The point here is that if we do as Leach suggests and begin with a "concrete reality, a local group of people," we are soon led to consider the "abstract reality" which serves as the organizational idiom for the former, and we note that although the elements of the idiom—in this instance, agnatic descent in the formal genealogical sense—persist throughout the range of societies under consideration, the elements vary in their degree of *pervasiveness*. The element of agnatic descent is commonly found in rules relating to the organization of local groups, and in African lineage systems one of these is a rule of affiliation, though it may not be the only rule of affiliation (again see Forde 1963).

The element of agnatic descent is also found in systems of cognatic descent-grouping.<sup>3</sup> But in these societies agnatic descent is not a qualification for membership; it is rather a qualification for privileged status within groups, the genealogical qualification for membership of which is cognatic descent (for the data on Choiseul see Scheffler 1965 and for the Maori, for instance, see Firth 1936:582-83 and 1957). In many ways these groups are similar to lineages, though there is certainly no foundation to the argument that "ramages are the precise functional equivalents of lineages" (Murdock 1960:11; emphasis added). Both unilineal and cognatic descent groups are corporate and multifunctional residential groupings which are the primary political, economic, and religious factions of their respective societies (see also Firth 1963:36), and rigorous comparison would probably reveal many

additional operational similarities which could be traced to these common features. But differences in individual group and larger societal structure would doubtlessly remain, and these might well be relatable to differences in group ideology.

However, in view of the fact that group structure and operation are only in part ideological matters and in view of the possibility of explaining differences in group ideology in terms of differential adaptation to material environmental, technological, and, for instance, political factors,<sup>4</sup> it would seem the superior tactic to attempt to compare and perhaps to explain differences in group and larger societal structure and operation in terms of, for instance, ecological matters, *before* attempting them in terms of ideological matters (see also Leach 1961b:296-306).

The essential issue here is that there would appear to be a range of societies composed of local and residential groups which, for many if not most sociological purposes, may be defined as the primary segments of these societies (see also Gray 1964). As such, they share perhaps many other operational features, but they differ in organizational ideology. The differences, however, are matters of degree rather than of absolute kind, for certain ideological elements are more or less pervasive. In this discussion, the typological focus on these ideological elements has been secondary, and the primary emphasis has been on the notion of groups and kinds of groups substantively rather than ideologically defined. It is supposed that the sociological value of a typology which might be formulated on these grounds would be guaranteed by the primary rather than the secondary emphasis. In addition to elucidating the structure of individual societies, the anthropologist may also attempt to explain differences in structure, including ideology. Thus we may attempt to account for the fact that some peoples conceptualize the concrete segments of their societies, or at least many rights and obligations in respect of them, as unilineally ordered whereas others conceptualize them as both unilineally and cognatically ordered, the different kinds of ordering having different domains of relevance. The concepts and terminology suggested here are designed to facilitate consideration of such problems.

## Abstract

Recently, a number of prominent anthropologists have cited with approval W.H.R. Rivers' observations on "descent" and "descent groups." Rivers has been credited with considerable insight and foresight into matters terminological and conceptual. Goody and Leach favor his usage of descent to refer to the genealogical criteria for membership of descent groups, the latter being defined as corporate bodies which are mutually exclusive on genealogical grounds. Fortes agrees but does not cite Rivers. Other scholars favor a broader usage of both terms, and there is considerable

<sup>3</sup> Here I refer to what Firth (e.g., 1957) has described as the "patrilineal emphasis" in systems of "ambilineal" grouping or "ramages."

<sup>4</sup> See, for instance, Forde (1947), Steward (1955), Arbele (1961), Leach (1961b) and Sahlins (1961, 1963) for attempts to do this.

disagreement over whether or not we ought to speak of "ambilineal" or "cognatic" descent groups. The Leach-Goody-Fortes position holds that it is unsound to do so. It is argued here that their argument is indefensible because it, too, fails to meet several terminological and conceptual problems posed by Rivers' usage. Rivers did not distinguish between indigenous *ideological* forms, such as concepts and rules, and the forms of *social transactions* which may be conceptualized and regulated by enunciated rules. Thus Rivers defined descent structurally *and* functionally, as do Goody, Leach, and Fortes. This leads to "holistic" analytical models, which are inadequate to the representation or explanation of social forms because these forms are mutable in structure and function.

Descent is here defined in terms of ideological or conceptual phenomena, as a generic label for a variety of forms of genealogical continua. Descent-constructs are distinguished from descent-phrased rules and these from descent-ordered units or sets of social transactions. The concept "group" is also considered, and it is noted how some usages deprive the term of sociological utility. It is suggested that models of social structures should be mutable; typologies of groups with descent-phrased organizational ideologies should 1st deal with the operational aspects of group structure and then focus on their idioms of organization. In this way, it may prove possible to formulate and test generalizations about the complex material and other conditions to which various organizational idioms may be differentially adaptive or adjustive.

## Comments

by ROBERT F. GRAY\*

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Scheffler has done social anthropology a signal service in sorting out the differing, even contradictory, usages and definitions of "descent" and related terms. He levels serious charges against certain anthropologists, describing their writings on this subject as "confusing" and "indefensible," and some of them will certainly offer rebuttal. Therefore, as I am not personally involved in these controversies, I shall limit myself to some critical remarks on secondary aspects of the article.

Throughout the article runs a note of polemic which seems out of place in dealing with what I consider an unemotional topic. Excessive zeal in argument, while perhaps harmless in itself, tempts to exaggeration, and this can introduce inaccuracies in criticisms which, in turn, may distort or weaken the points being made. Scheffler accuses Rivers and some of his recent followers of handling "descent" so badly that the concept is now surrounded with confusion. If this were indeed the case and not an exaggeration, then the writings of these people on this topic ought to be unintelligible, or at least frequently misunderstood; but this the author does not even claim. Their demonstrated errors—failure to make certain possible distinctions and unduly restricted definitions—hardly add up to a situation in which one can speak of "the confusion surrounding 'descent'."

It seems to me that in criticizing definitions of "descent" the author implicitly compares them to some independent standard of meaning, by which they are found wanting. The impartial referee to which he makes implicit appeal can be none other than the dictionary, that is, the usage of the term in everyday language.

"Descent" as used in this article, meaning roughly "ancestry," is one of several metaphorical senses of the word, all deriving from its primary meaning of movement from higher to lower in space. These senses are all in wide use outside anthropology and are more or less adequately codified in dictionaries. Therefore we can treat the term in one of two ways: we can adhere as closely as possible to the dictionary meaning, or we can alter or restrict the meaning to make it more suitable for a special purpose. The results of this second method are the diverse definitions and usages that Scheffler criticizes. Different students may disagree on the best way to restrict the meaning, and then they can only appeal to the dictionary for an impartial ruling, as Scheffler has in his criticism of Rivers. In that case, why not stick to the dictionary meaning in the first place? I do not see how it could possibly produce confusion or misunderstanding.

The dictionary meaning of a word sometimes may not be flexible or precise enough for all situations arising in anthropological research. In these cases it is probably best to abandon the common term and search for, or invent, a technical term sufficiently accurate and precise. Scheffler refers to such terms as constituting a "metalanguage" and gives us an example by coining the term *descent-construct*. Now a newly coined term must be defined clearly, because here the puzzled reader can expect no authoritative help from the dictionary. The term *descent-construct*, while it eliminates the inconsistencies in the usage of the non-technical term "descent," introduces into my mind a new confusion: Does it designate something constructed in the minds of the tribesmen, or in the mind of the ethnologist? My confusion might provide Scheffler with a useful cue. In further analysis of his problem, he should consider whether the confusion sur-

rounding "descent" that he finds in other writers is not partly caused by the same failure on their part to distinguish between *folk interpretation* and *anthropological interpretation* in their definitions of descent-related concepts.

One final point: I read the title hastily and then read through to the end in anticipation of some new insight into the ancestral cults of descent groups. If my comments have been insufficiently appreciative of the good qualities of the paper, this is no doubt because of my disappointed expectations and chagrin at being taken in.

by EDMUND LEACH\*

Cambridge, England. 12 II 66

I can hardly fault Scheffler's extremely lucid reader's guide to the minefields of Cambridge anthropological scholasticism. He is generous in treating the Fortes-Goody-Leach position as a chorus rather than as a raucous discordancy; all told, we appear to have survived the treatment much better than I should have expected. Scheffler correctly diagnoses my own views at the bottom of p. 546: "no immutable line is possible." What constitutes a good definition will depend upon the nature of the problem; when problems change definitions may have to change also. Fortes' discrimination between *descent* and *filiation* threw a flood of light upon a variety of rather special sets of ethnographic data, and it posed new questions about other sets of data to which it proved ill-adapted. I cannot agree with Scheffler (pp. 545 and 546) that the Cambridge viewpoints is inextricably linked with "whole system" analysis with consequential tautological assumptions about the relation between *structure* and *function*. I myself have been a most persistent critic of the Radcliffe-Brown whole-system organic analogy, and, in so far as Scheffler's conclusion depends on the proposition that Goody, Leach,



and Fortes can only operate with "holistic analytical models" (p. 548), I must quite firmly disagree.

by LEONARD PLOTNICOV\*

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Scheffler's paper, like some of the social groups of his concern, has many purposes, some explicitly stated, some implied; one must suppose it to be, among other things, a contribution to the analysis of corporate kinship groups and to the history of anthropological theory, and an exposure of the quasi-religious behavior of some British social anthropologists. In addition, the author reminds us of certain caveats: some groups are not groups at all but only categories of persons, and these should not be confused; we must always specify in what way a group is a descent group; and some descent groups are cultural and analytical fictions, being actually local groups. I will confine my remarks to two issues: the proof of Scheffler's main thesis that some definitions of descent and descent groups preclude comparative analysis, and the nationalistic background against which he has set his exposition. I will take up the latter first.

I would not be surprised if our British colleagues took offense at the manner in which Scheffler frames his argument. The line-up of proponents in the controversy sets American scholars on one side and British (Goody, Leach, Fortes, and Salisbury) on the other. (Firth is excepted, but he might also be regarded as not being British.) This division in itself is innocuous, but the implication that Goody, Leach, and Fortes hold their views by virtue of their veneration of a distinguished British pioneer in anthropology may be regarded as insulting. If this was not intended, then it is unfortunate that this article was entitled "Ancestor Worship in Anthropology." Readers may feel that the author is insinuating that Goody, Leach, and Fortes, whose anthropological reputation is that of recognized

excellence, cannot distinguish between blind hero-worship and views held according to the strictures of scientific principles.

Scheffler could have avoided this invidious implication by altering the title of this paper and restricting his remarks to the logic of his argument. I am sure Professor Scheffler did not intend to be offensive, and I am confident that he will indicate this in his reply.

I cannot think of any anthropologist today who would take issue with Scheffler's statement that "one of our problems (could we say "aims"?) is to develop a metalanguage which will facilitate comparison and generalization while at the same time maintaining some respect for both the integrity of natural systems and the similarities and differences among them" (p. 545). Differences of opinion do exist regarding the difficulty of maintaining the integrity of unique cultural systems when these are subjected to description and analysis, but division on this issue does not follow clear-cut national lines. While British social anthropologists tend to be more concerned about cultural uniquenesses than American scholars, Bohannan (an American) is one of the strongest exponents of this position, while Gluckman has long pleaded for a "metalanguage" that would facilitate comparison and generalization. However, on the whole, Scheffler is correct in pointing out that British social anthropologists are inclined toward holistic, unitary analyses and not toward comparative studies. They have tended to view social systems as consisting of "aspects, rather than elements," and these parts as "definable only with reference to their roles in the whole system" (p. 545). This viewpoint indicates a fundamental difference between British and American ethnologists and underlies the antipathy of the former toward cross-cultural studies. It would be misleading, however, to attribute an anti-comparative attitude to all British

social anthropologists. There have always been attempts at comparative analysis and generalization (Radcliffe-Brown's and Richards' kinship analyses, Gluckman's essays on law and politics, and Turner's studies of religious symbolism are some instances), and the tendency to make comparisons and generalizations is increasing. Nonetheless, the issue of the comparability of data derived from unique social and cultural systems remains an issue of strong debate, and to my mind this basic problem has not yet been satisfactorily resolved. Perhaps it never will be.

Scheffler's main point is that corporate kinship groups are analytically comparable, provided Rivers' restricted definition of descent is discarded. Since I support Scheffler in this, I regret that his article is not more persuasive. He could have strengthened his argument in several ways: (1) Following the elegant analyses of Goodenough (1955) and Davenport (1959), he might have indicated his appreciation of the implications for the total structure and operation of corporate descent group systems of the basic difference between recruitment based on unilineal descent ideologies and recruitment based on optative conditions. These two papers point out the kinds of implications that I think Fortes, Goody, and Leach would stress. (2) He might have dealt with the Leach-Goody-Fortes position in more detail. The non-initiate to this controversy would be forced to turn to the cited sources in order to gain an adequate appreciation of the issues in dispute. As it now stands, Scheffler's exposition tends to be esoteric. (3) He could have attempted to demonstrate the superior utility of his approach by contrasting it with that of Goody-Fortes-Leach in application to particular empirical situations. A demonstration attempting an analysis of some problematic situations by means of each of the contrasted approaches would have provided substance to the outlines of the argument.

## Reply

by H. W. SCHEFFLER

I wish to thank Gray, Leach, and Plotnicov for their comments on my paper. Leach's introductory remarks are particularly gratifying. They should serve to allay the fears of Gray and Plotnicov that I may have damaged Anglo-American relations.

I am fully aware, as is any anthropologist reasonably conversant with

the history of his discipline, that Rivers' ghost has been evoked not out of "blind hero-worship" but in order to suggest that a point of view now popular among some social anthropologists has a lengthy and respectable history. Nevertheless, that point of view has definite limitations: I merely pointed them out as concisely as I could. Perhaps I dealt with some matters too concisely, for I believe that the answers to most of the questions raised by Gray and Plotnicov are contained in the paper. To reply to them would be redundant. For

example, I did not set out to write a treatise on societies which are in part ordered by descent-phrased rules, and I said so at the outset.

I am puzzled that Gray and Plotnicov find it offensive that I refer to certain arguments in the literature as confused or as logically or empirically indefensible. Such are the normal hazards of scholarship. I submitted this paper to CA in the hope that, by continuing an important controversy, it would draw some pertinent and substantial criticism that might be of value to me and to others.

Leach has indeed been a most persistent and compelling critic of the Radcliffe-Brown whole-system organic analogy, and it should be apparent that I agree with most of the criticisms that have been brought to bear on that approach to the study of human behavior in society. My argument is not, however, that "Goody, Leach, and Fortes can only operate with 'holistic analytical models'." I know they can do otherwise—witness Leach's *Pul Eliya* and his "Rethinking" essay, which formulated and discussed transformable models of cultural subsystems. My argument is that their "descent" and "descent groups" are derived from and belong to a structural-functional model of "descent systems," either as total social systems or as unitary subsystems ordered in such a way that at least certain transformations or permutations are difficult to conceive. One can, of course, use the terms as they do without wishing to commit oneself to a structural-functional or total social model—Rivers did just that—but I think it would be inconsistent to do so.

Gray's discussion of the formation of concepts is, to my mind, not adequate to the problem. A great many words in any language are *polysemic*, to use a term somewhat more precise than "flexible"; they have several semantically related senses. A dictionary is little more than a device for aiding in the determination of the sense of an unfamiliar term in the context of a particular utterance, or of a familiar term in an unfamiliar context. A good many technical utterances, however, contain several ambiguities or so little information that it would be difficult to determine, even with the aid of a good technical dictionary (and there is none in anthropology), precisely what the utterance is meant to convey. Descent is a highly polysemic term in anthropological discourse, as in "ordinary language," and is thus susceptible of ambiguous usage. It was my intent to sort out some of the sources of the ambiguity and to suggest compound labels for some of the derivative senses of the term. To judge from Gray's initial observations, I achieved at least a moderate degree of success. Therefore, I am particularly puzzled by his later suggestions that I either created the muddle myself or exaggerated a relatively minor one.

This paper is now better than two years old, and I find that I would now rephrase, though not seriously modify, some of the argument. In particular, I would stress that in many societies with corporate descent-ordered groups the generic terms for those groups are polysemic. The terms designate (a) *classes* or socially significant *categories* consisting of agnatic or

uterine or cognatic descendants of a specified "founder" and also (b) the operant groups (sometimes "localized") which form around some of those category members. Moreover, the same terms may designate categories and groups at several levels of segmentation, resolution of referential ambiguity being left to linguistic and social contexts of usage. Middleton's account of Lugbara social structure (in Middleton and Tait 1958:207-13) offers an excellent example of this sort of thing (see also Scheffler 1965:39-42).

Groups and categories designated by the same term may show relatively little correspondence in their personnel. As Sahlins (1965) suggests, and as much ethnographic data confirms (see Meggitt 1965; Bulmer 1965; Sahlins 1963), correspondence or lack of correspondence between membership of category and group may be explicable by reference to what we may call "ecological" factors, including intersocietal relations. Moreover, these and other ecological factors may help us to explain the ideological differences themselves. But the relationships between ideology, group composition, and ecological variables are quite complex, and we have, as yet, no simple formulae in which to express them.

These considerations are pertinent to the issue of "choice" raised by Plotnicov. I noted this issue but did not discuss it because I considered it one which could not be resolved until others had been dealt with, and because I have considered it elsewhere (Scheffler 1965), as have Schneider (1965) and Sahlins (1965). This much, I think, is now clear: Since there is no simple association between structural ideology and group composition (or between category and group "structure"), there can be no simple association between unilineal versus non-unilineal ideologies and no-choice versus choice of affiliation with the groups that are the significant (i.e., politically and economically strategic) social units. It is true enough that many unilineally phrased ideologies of social order appear to preclude two or more group affiliations of the same general order of significance and that, in contrast, cognatically phrased ideologies appear to invite such arrangements. Yet as I have shown for Choiseul Island (Scheffler 1965), the choices that are open "in theory" are not usually open "in fact" (see also Firth 1957). Many choices are effectively precluded by political and economic circumstances beyond any one person's control, and there, as in many unilineally ordered societies, a person's interests and commitments are usually confined to a single group. Thus, I doubt that on Choiseul there

was greater interlocal mobility, or a greater rate of change of political and economic allegiances, than there is in some reportedly unilineally ordered societies, where the choices closed "in theory" may be open "in fact." Moreover, as I demonstrated in my study of Choiseul Island descent groups, *affiliation with a group conditions one's membership of descent categories*, even though category membership is a criterion of eligibility to membership of descent groups. It is well known that similar processes (e.g., genealogical charter revision) operate in so-called unilineal descent systems.

The ideological and transactional domains are in some ways ordered independently of one another (Sahlins 1965), though under certain conditions there may be considerable correspondence between them (see Meggitt 1965 and also Groves 1963). But in other ways there may be dialectical relationships between the ideological and transactional domains. Thus, a cognatic descent-phrased ideology does not necessarily in and of itself permit "flexibility" of group affiliation any more than a unilineally phrased ideology in and of itself precludes such "flexibility." Such "flexibility" as may occur is, I suspect, as much a function of political and economic interests and organization as it is of the kind of descent-phrased ideology present.

Unilineally phrased ideologies, it seems to me, are often devices for legitimating closure and cognatically phrased ideologies devices for legitimating openness of groups or access to their resources. These devices may be employed with vigor or largely ignored, as circumstances and perceived needs seem to demand and allow. The conventional analytical emphasis on the literal phrasing of descent-phrased rules, as recruitment principles, is useful enough when one is merely characterizing ideologies, but when the concern is with the complex relationships between ideology and action such treatment leaves much to be desired (see also Scheffler 1963).

One problem, as I perceive it now, is to understand why some societies have found it advantageous to institutionalize a device for legitimating closure and others a device for legitimating openness, or both devices in different domains of social life, though as Sahlins', and Fortes', work suggests there is certainly more to the problem of unilineal versus non-unilineal than this. I do not doubt that ideological differences have consequences in and of themselves, but I have reservations about some formulations of those consequences, especially those formulations which have not sufficiently taken into account the complex relationships that may obtain between ideology and action.

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