the Pomo might actually have been in a social/ecological situation that really called for a mixed residential/political strategy. But, on the other hand, they also might have been in a situation that called for a clear unilineal strategy, but wherein individual actors or groups had particular problems (e.g., quarrels or micro-demographic variation) which interfered with their execution of that strategy.

It is this missing information, in the context of California's ecology, which would allow California Indian studies to make the kind of important substantive contributions to social theory that Kunkel and I would both like to see. Kunkel's article, by highlighting the theoretical issues and by clarifying the ethnographic facts, has materially increased the likelihood of such a contribution.

University of California
Riverside

Reply to Kronenfeld

PETER H. KUNKEL

Kronenfeld raises the question of whether the existence of ambilocal residential kin-groups among the Pomo is necessarily inconsistent with the existence of unilineal kin-groups. He bases his argument for the possibility of Pomo lineages on analogies between Pomo and Nuer residence patterns. Implicit, however, is a hidden assumption, namely that "primitive peoples" are not capable of establishing social order beyond the level of the "band," without recourse to the principle of lineal descent. My paper on the Pomo kin-group, etc., was intended as an attack on Service's hypothesis of the universality of the patrilocal band among truly aboriginal food-collecting societies. Unfortunately, I did not also make explicit my opposition to the inevitability of unilineal descent at the "tribal level" (another hypothesis of Service, apparently shared by Kronenfeld).

A detailed rejoinder to Kronenfeld will probably serve as a convenient vehicle for demonstrating the weakness of the unilineal bias. This is particularly convenient since Kronenfeld chooses his stance as an "Africanist of sorts." Because of the prevalence of lineal descent systems in Africa, and because of the considerable respect we all have for the theoretical contributions of Evans-Pritchard and other British social anthropologists who have analyzed African political and kinship systems, it is not surprising that some American theorists have taken the African data as "an intuitive reference point" from which to look at "tribal," kin-based social systems elsewhere. Thus Service, in *Primitive Social Organization* (Random House, 1962), posits a universal evolutionary progression from societies with patrilocal bands to societies with patrilineal lineages. In this scheme, matrilineal systems are viewed as a possible logical derivative, somewhat later in the evolutionary sequence. Still later, according to Service, non-unilineal systems emerge as quite ineffective adaptations to early acculturation circumstances. Even then, Service argues, non-unilineal systems have underlying lineal organizations, or traces thereof, presumably representing their prior conditions.

The basic reason many theoreticians assume a kind of inevitability for unilineal descent is that such systems are relatively easy for members to "remember," as compared with "bilineal" systems. This is so because a non-unilineal system is not really bilineal at all; the number of potential lines through

which to trace ancestry doubles with every ascending generation. The result would be a very intricate maze of descent lines which would be impossible to analyze, except with a computer. The reasons for favoring patrilineality over matrilineality as the "ideal" kind of lineal descent system seem to me to be less logical—essentially a subtle expression of male chauvinism.

So long as one assumes that corporate kin groups must concern themselves with descent models for legitimizing a structure (and for recruiting members) the basic argument outlined above is very persuasive. However, if one considers the possibility that "lateral" models may have been equally logical and effective for these purposes in early times, the argument for inevitability may be weakened. By a "lateral" model, I mean one with stress on recognizing contemporary kin as potential members of corporate groups. It is possible to know a rather large number of persons related to you *ambi*laterally without establishing connections through "depth analysis" of descent. Tracing relationships ambilaterally has the same disadvantage of potential vagueness that Murdock has pointed out for kindreds. Furthermore, groups so constituted will tend to be different for different individuals to such an extent that only full siblings would have exactly the same group co-members. Under such circumstances, there would be no "groups," properly speaking, only ego-centered categories of persons.

The above objections are not, however, insurmountable, as Davenport, Goodenough, and others have been able to demonstrate. If there is a criterion of some kind which can be introduced as a means of including or excluding members, then ambilateral corporate groups are possible. One such potential criterion is residence, specifically the existence of a flexible residence rule. Alternative residence choice is, of course, a rather flexible norm, and this may bother some theoreticians. However, the flexibility of the norm is, I submit, an advantage, not a weakness, in actual life circumstances. If a married couple can make a choice between two or more possible places of residence they will be better able to make that choice which seems advantageous under a particular set of circumstances. Such circumstances might involve, for example, eligibility for secular office, inheritance of occupational specialization, succession to sacred office, or population pressures. All of these conditions were taken into consideration by married couples among the Pomo.

Let me now turn to a consideration of Kronenfeld's counter-hypothesis and his arguments for it. Essentially, he is saying that the Pomo probably had lineages, and he implies by his use of the Nuer analogy and certain other aspects of his logical argument that those lineages were patrilineal. Apparently he assumes, Service-like, that lineages will inevitably be patrilineal and that everybody would know that without being told.

Kronenfeld bases his argument for Pomo lineages on an alleged weakness in my argument—my use of post-marital residence statistics. He rightly objects to the use of such statistics, alone, to determine presence or absence of lineages. In essence he is pointing out the well-known sociological fact that norms are never without exception in any society. Therefore, even if the norm is patrilocal residence there will likely be some deviations from the norm in actual practice. (He could also have argued that when the "real" norm is lineal descent it does not follow that a norm for unilocal residence must inevitably accompany it.)

In stressing his objection to my use of post-marital residence statistics, Kronenfeld ignores other evidence I presented, though he might argue that it too is vulnerable to the deviation-from-norm explanation. However, he puts main emphasis on the comparison of Pomo and Nuer household patterns. Since

Nuer "actual" residence patterns deviate from the patrilocal norm, he argues that they are much like the Pomo residential pattern. However, in order to do this he must be assuming that there really was a Pomo norm for patrilocality. There is no good basis for this assumption. Pomo informants vary considerably in their statements on this point. Some make statements to ethnographers that indicate a preference for patrilocal residence; others make statements that indicate a matrilocal preference; still others make statements that indicate either patrilocal or matrilocal residence is acceptable. I have inferred from all these statements that alternative choice was itself the norm. The choice seems to have been limited usually to two alternatives—either matrilocal or patrilocal residence. The result of this choice, as limited, meant that people who lived together were usually related either patrilaterally, matrilaterally, or affinally.

Now, it can be argued that, despite the lack of a norm for unilocal post-marital residence, there could still have been a norm for lineality in tracing descent. Again, actual statements of Pomo informants are ambiguous (e.g., when dealing with chiefly succession, inheritance of sacred or other occupational specialties, and the like). Therefore, I have come to the conclusion that there was no unilineal descent norm in Pomo society. However, other conclusions could be drawn. For instance: (1) there could have been a system of double descent; (2) some Pomo groups may have had patrilineal descent while others had matrilineal descent; (3) all Pomo groups may once have had patrilineal descent but more recently some tendency to shift toward a matrilineal (or non-lineal pattern); or (4) some or all Pomo groups may have been originally matrilineal, with a recent tendency toward a patrilineal (or non-lineal) pattern. After detailed examination of the ethnographic data I have come to the conclusion that none of these alternative hypotheses seems likely. Instead, I prefer the hypothesis that the Pomo had an ambilateral system of relationships. Their rules of exogamy seem to support this, since cousin marriages of all types were forbidden to the degree of third cousin.

None of this should be regarded as conclusive. There is room for some doubt, and a reexamination of all evidence available would be useful.

University of Maryland
Far East Division
Yongsan, Korea

Pomo Social Structure: Problems of Ethnohistory

NELSON H. H. GRABURN

Kunkel's (1974) article on "Pomo kin groups" and Kronenfeld's response, in this issue (Kronenfeld 1975), bring to the fore once more the interesting problem of the ethnohistory of social structure. Kronenfeld is quite apt in his comparison with the Nuer whose residential groupings and seasonal movements do not exactly reflect the fact that the Nuer use the *metaphor* of unilineal kinship to describe and understand their own socio-political organization. Kunkel seems to have confused social organization for social structure and while he has much of the former type of data in hand the latter only seems to have existed in the minds of long deceased Pomo for it was not recorded by ethnographers or other reporters. Social struc-