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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 patrilocally, matrilocally, 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-linear pattern); or (4) some or all Pomo groups may have been originally matrilineal, with a recent tendency toward a patrilineal (or non-linear) 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.

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Pomo Social Structure: Problems of Ethnohistory

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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-

ture consists of the conscious and unconscious models in people's heads, as a guide for behavior but not related to behavior in any one-to-one fashion. In much the same way, one would not expect to find out that the speed limit was 30 miles per hour by timing all the cars and trucks that passed down a city street.

Though Kronenfeld's argument holds open the possibility that lineal forms of social structure may have existed, it is not very useful for it does not direct us how to find out whether they did or not given the possibilities of present day research. Similarly, Kunkel's claims for California Indians as the correct models for pre-agricultural hunters and gatherers is interesting but does not help us solve the specific problem of the Pomo. Since one cannot ask the relevant Pomo any longer, one is dealing in a kind of guesswork or conjectural reconstruction for which one must consider two main kinds of evidence. The first kind is the nature of the reports available for the light they throw on the problem. For instance, it is well known that Kroeber was very much against emphasizing the importance of unilineal descent groups (see especially Kroeber 1917) and to some extent this rubbed off on Gifford, in spite of his reports on the Miwok and other groups. I am surprised that neither Kunkel nor Kronenfeld mentioned the very thought provoking work of Aginsky (1935) who found himself in the field among the Pomo at the same time as Gifford and who described the latter as not being very interested in "sociology" but as interesting himself in other matters. Aginsky agreed that the largest autonomous groups of Pomo socio-political organization were "valley groups" of which there were seven. He worked with three, and was particularly concerned with the relationship between kinship terminology systems and rules of preferred marriages—parts of social structure—and found considerable variation. The Hopland Valley

Pomo had an Omaha type system, the Rincon Valley a Crow system, and the Ukiah Valley group a Dakota system! All of these are very compatible with unilineal descent systems—of different kinds—though Aginsky preferred to explain the terminology systems as solutions to the problems of expressing conflicting marriage rules and says nothing about descent. If he was right, then I might guess that all three systems were perhaps also compatible with Pomo social structure as portrayed by Kunkel, but explanations such as Aginsky's have fallen on very hard times, especially since Murdock (1947).

The other line of evidence to be considered is the relevant history of acculturation of the Pomo and comparable groups. It is all very well for Kronenfeld to say that "it is impossible to rule out any unilineal basis," but the burden is still on him to show that it is still worth entertaining the proposition. If the Pomo had a unilineal type of social structure once but not in recent times one must look at the character of the authorities who might have overlooked the facts (see above) and one must also ask why it disappeared, especially as it did not disappear from the social world of the Indians of Central/Southern California. We know that differential acculturation pressures might account for different degrees of loss of aboriginal social structures, amply documented by Eggan (1937) and Spoehr (1947), and perhaps such is the case with the Pomo. It is surprising that neither of the authors took this into account.

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Further Information on the de Cessac Photograph

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In the Winter, 1974 issue of the *Journal* (p. 186), there appeared a photograph, originally taken by Léon de Cessac in 1878, of an Ineseño Chumash man dressed in ceremonial regalia. Since the individual shown is somewhat misleadingly identified in the *Journal* as a "Chumash shaman," and since the particular photograph has been reproduced several times in recent years (Reichlen and Heizer 1964; Grant 1965), I feel that it might be useful to

correct the record and add further data to what little is presently available.

I am reasonably certain, on the basis of various bits of information contained in the ethnographic field notes of John P. Harrington, that the particular man shown in the photograph is Rafael Solares. Rafael is known to have worked closely with de Cessac when the latter was at Santa Ynez (even assisting him in the excavation of the cemetery at *soxtanokmu?*), and was most probably de Cessac's primary Ineseño informant. In addition, Rafael is known to have been a dancer; in the 1860s, for example, he accompanied Marcelino to Saticoy to take part in Pomposa's fiesta (Blackburn 1974). It is therefore most likely that the photograph (which shows a man dressed in the typical attire of a Chumash dancer) is of Rafael Solares.

Whether Rafael could legitimately be described as a shaman is a moot question. Although as a dancer he was almost certain to have been initiated as an *?antap*, he is unlikely to have ever engaged in the kinds of practices usually associated with the term shamanism. However, it should be noted that there is one brief reference in Harrington's notes to pictographs painted in a cave by Rafael and Joaquin Ayala at the time of the winter solstice, so the problem must remain open for the time being.

Relatively little is presently known about Rafael Solares' life. His date of birth is unknown, although he apparently assisted in the construction of Mission Santa Ynez. He died in 1896, and is buried in the mission cemetery in an unmarked grave (along with 1700 other Chumash Indians). His son, Manuel, became the third husband of María Solares, Harrington's primary Ineseño informant (Blackburn 1975). A letter of inquiry was sent to Mission Santa Ynez regarding mission records on both Raphael and María Solares, but unfortunately no information was forthcoming. A photograph of Rafael Solares (wearing a bear-skin