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(Kunkel 1974). Its conclusions are entirely based on analysis of data collected by Kroeber and other "older" ethnologists. In connection with my research into the basic ethnological literature of California, I had personal discussions with Kroeber, Gifford, and Barrett. In addition, I sent draft manuscripts to others, including (as I recall) Loeb, Driver, and McKern. My research was under the guidance of Ralph Beals. I worked especially closely with Barrett in my analysis of Pomo political organization. Barrett completely agreed with my interpretation of Pomo political organization. (I have a letter from him to that effect among my notes, stored back in the States). Of the others whom I consulted, only Driver offered a criticism of my political analysis. His criticism was essentially a caution against using, in too general a way, an interpretation he had himself made in his *Wappo Ethnography*.

My contact with Kroeber was brief, and I certainly would not wish to imply too much from it. However, he did (a) express interest in the fact that I was doing the political analysis; and (b) imply some disagreement with the social organizational analysis of one of his former students, in a manner consistent with my own thinking on the point in question. I honestly believe that Kroeber would have approved the "non-unilinear" aspect of my ultimate analysis, with respect to tribal groups in the northern portion of the present state. However, by the time my work was completed Kroeber had died and we were *all*, unfortunately, "post-Kroeberian." I would have been very interested in Kroeber's reaction to my final presentation and would have had great respect for his opinion.

Elsasser is naive if he assumes that all of the "older" authors he mentioned (essentially pre-World War II field workers) were in theoretical or methodological agreement with each other or, always, with Kroeber himself. Those whom I consulted were all happy to see their data being used for a new type of analysis and showed no resentment that a "young" person

was presuming to reinterpret their findings.

I have been working overseas for nearly three years and have not seen the Bean and Blackburn book. In fact, Elsasser's review is my first notice that it has been published. I have not read the articles of the other "young authors," so I cannot speak for them. However, I suspect that their work may have been misunderstood by Elsasser as mine has been.

It was especially amusing to reflect on Elsasser's implied "generation gap," using the age of fifty as the watershed dividing "young" and "old" California ethnologists. As of December 8, 1976, I am 60 years old.

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Reply to Kunkel

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In regard to Peter Kunkel's objection to parts (or all) of my review of *Native Californians: A Theoretical Retrospective*, I do indeed owe him an apology if he believes I was misrepresenting him. I can assure him that I

did not look at this book “on the run” and found little or nothing to comment upon adversely in any one of the articles of the volume, even if space were available to do so. What I chose to emphasize was what appeared to me as a sort of dichotomy between some “younger” and “older” scholars in the matter of relative confidence in handling of ethnological data. I am well aware that Kroeber’s students or associates did not always agree with him, or with each other, in methodological aspects of their work—it merely seemed to me that they were not deprecating directly or by implication the work done (or not done) by others. I realize also that historically there was little likelihood that any condescending attitudes could develop among these early scholars. No doubt the separation of “old” or traditional from “new” or innovative can be done in an approximate and figurative sense only, and I regret the suggestion that Kunkel was in effect fuzzily categorized as of the latter persuasion.

As to the context of the rhetorical question Kunkel posed on the nature of food-collecting peoples, I admit a possible misunderstanding of his intent. However, Kunkel states clearly that California ethnology is based *mainly* (italics mine) on “salvage ethnography” rather than “participation-observation.” I understand this to imply that the salvage (read “older”) ethnographers have somehow grievously neglected to treat dynamically theoretical questions of hunting and gathering peoples in favorable environments. If this is not what he intended, then I was wrong and regret the indiscretion. Certainly I have no doubt whatever of his respect for these older ethnographers and indeed believe that he has utilized the data pertaining to the existence of corporate residential kin groups among the Pomo most adroitly.

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Comment on Kowta’s Review of Fifty Years of Archeology in the California Desert

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Makoto Kowta, reviewing my recent *Fifty Years of Archeology in the California Desert* (Journal of California Anthropology 3[2]:93-94) has noted my positivist biases, commented that I have “covered the material well” and revealed “new and interesting historical details,” and expressed concern because I did not address “management of archaeological resources *vis-à-vis* the non-specialist public.” While I am always grateful for essentially commendatory reviews, I am both disappointed and a little disturbed by Kowta’s treatment of my work.

The archaeological overview as an element of National Park Service management was invented several years ago by Dr. Keith Anderson, who established the ground rules for and supervised my overview of Joshua Tree National Monument (the basis for *Fifty Years . . .*). The purposes of an overview are to figure out what archaeology has been done in and around a park or monument, to place these activities in a regional research context, and to discuss what might be done with the area’s archaeology in the future. This exercise gives park managers direction in contracting for inventory surveys, evaluating properties for National Register eligibility, and so on. In the Joshua Tree overview I do think I was able to dig out some “interesting historical details”—about E.W.C. Campbell’s research in the 1920’s and 30’s, about the differential distribution of pottery types among areas surveyed during the 1960’s, and about the effects of differing theoretical orientations on the observations of different archaeologists in the field, for example, and I would have appreciated