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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

THOMAS F. KING

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

Kowta's comments on the reliability of my historical interpretations. The overview made it possible for me to do more, however. I was able to synthesize interpretations of California desert climatic change and discuss the possible demographic effects of such change. I was able to comment on possible reasons for differences between Serrano and Chemehuevi settlement organization. I was able to consider the social meaning of different forms of rock art. I was able to speculate about what caused the expansion of Uto-Aztekan into the Great Basin. Considering Kowta's long and deep experience with southern California prehistory, I would especially have appreciated his comments on these attempts to synthesize, interpret, and formulate hypotheses. I am disappointed that he did not see them as important enough to justify comment.

Kowta in fact seems uncomfortable with the fact that I emphasized the research value of archaeological sites at all, insisting that my paper should have considered matters of "management *vis-à-vis* the non-archaeological public." Since the paper was written in standard English, with a glossary of technical terms, and since it includes recommendations for survey, considerations of National Register eligibility, and so on, I assume that what Kowta misses is some sort of recommendation concerning public use and interpretation of the Monument's prehistory. I object. We do not expect biologists who study the Monument's snakes and cacti to include a public interpretation element in their research reports, nor do we expect geological researchers to include recommendations about how to display rocks. We assume that their research itself serves the public interest, and that it provides useful information for management purposes. Does Kowta think that archaeologists should become public interpreters just because they do research in a park? Is archaeological research itself so illegitimate?

My concern about Kowta's position rises from the fact that his opinion is shared by

many in Park Service management. Despite the fact that National Monuments are created under the Antiquities Act precisely to preserve "objects of historic or scientific interest" (34 Stat. 225; Sec. 2), and the fact that the National Park Service itself was initially created as a preservation agency, an orientation toward recreation and lowest-common-denominator "interpretation" has increasingly come to dominate National Park Service upper management thinking. This has served to justify the sacrifice of archaeological sites that could not be effectively put on display within the parks, the "restoration" of historic structures at the expense of their archaeological integrity, and the employment of Park Service archaeologists and historians whose scholarly abilities are nil. It is not only disappointing but rather frightening to see the same philosophy being adopted by non-Federal archaeologists. The National Parks and Monuments *should* provide our best bank of preserved research resources, and if we are not willing to argue for the priority of research and the preservation of research value over public use and interpretation, who will?

Washington, D.C.



Reply to King

MAKOTO KOWTA

The limited space available for my review precluded a full discussion on all aspects of the solidly executed overview in question. Its author's comment above provides additional details which readers will find useful in arriving at a more complete comprehension of its contents.

The concern alluded to is not so much that King's study should have undertaken the task, but that management problems *vis-à-vis* the