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COMMENT

Further on J.P. Harrington

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Kathryn Klar in her redargution appearing in the last issue of the *Journal* seems to have gotten her dander up a bit over what she sees as my "bitterness" in some remarks I made about John Peabody Harrington, a man who I barely knew and who she knows only through the aggeration of his field records. My apologies to all readers for not acknowledging Tom Wolfe as a qualified judge of JPH as a genius and book sales promoter—I stand corrected on Wolfe and by Klar. Am I faulted for remembering only Harrington's unusual typewriter? But wait; I also recall a lot of gravy stains on his shirt, though this little intimacy is perhaps of even less interest. I am also cast, unfairly I think, in the role as an apologist for C. Hart Merriam who was admittedly as eccentric as JPH, though CHM carried a lot of weight with North American naturalists, was the founder of the U.S. Biological Survey, and was a member of the National Academy of Sciences. I think that CHM also had many of the faults of JPH, among these a suspicion of professional anthropologists, or perhaps better, anthropological linguists. And surely Merriam was no linguist at all, but rather an abecedarian word list collector.

I still think one of the main reasons why Harrington can be held at fault is that as the improcreant collector of phemic data (which I admit are infinitely precious since in many

cases they will be our main record of a vanished world) he should have prepared it for publication—he alone knew its intricacies and intimacies and interpretation far better than some student a half century later possibly could. To make JPH, through some kind of 20-20 hindsight, a kind of latter-day linguist folk hero may come to pass, but if it does then that will only tell me something I refrain from trying to state here. If all scholarship had to await the amanuenses of one or two generations later, we would still be waiting for the electric light and the airplane, as well as critic Klar.

A considerable collection of Harrington's Chumash myths has recently been published under another person's name as editor, but the credit by readers will not go either to the true authors (Native Californians) or to the real collector (JPH), and the book royalties will go to neither. JPH seems to be a bit like an oil well which a lucky macrographic petroleum geologist has found. Any reader interested in this unimportant matter should compare Harrington's barebones Chumash myths with Kroeber's *Yurok Myths* published a year later by the same (unnamed) university press.

Since we are letting it all hang out, let me suggest to Ms. Klar and "several other young California specialists" who are known to her and who are poking through the bones of Harrington's records that they be careful to acknowledge their gratitude to that man's work by making him the author, or co-author, of any publication which results.

Let me also, since Ms. Klar, who I think I never met, takes a personal tone, take exception at her gratuitous characterization of me as

a person of "outstanding reputation and scholarly ability." I do not possess, nor have I ever sought, either. She is as wrong about me as she alleges I am about JPH. Most of what I have discovered I have published in the hope that it would be of some help in my own generation. And that, I learned by example from my teacher, Kroeber, a thought which supports me in my senectitude.

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"The Development Of Pinyon Exploitation In Central Eastern California"

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Robert Bettinger's (1976) article in the preceding issue of this journal represents one of several recent sophisticated treatments of surface distributional data in the Great Basin (see also Thomas 1971; O'Connell 1971). The results of such work have opened up a whole new dimension of patterning in the archaeological record. However, sophisticated methodology in itself is no substitute for rigorous application of scientific method.

Bettinger attempts to demonstrate that pinyon nut exploitation began in the Owens Valley at approximately A.D. 600. This he has

failed to do. Instead Bettinger's argument only demonstrates the *introduction* of a stone-ring feature associated with the exploitation of pinyon—not necessarily the exploitation of pinyon itself.

Bettinger classifies 21 sites as "pinyon camps." The criteria used to define this functional site type consist of three primary characteristics: (1) location of a site in the pinyon-juniper zone; (2) presence of milling equipment; and (3) presence of circular floors. The presence of these attributes at a site "suggests that pinyon collecting and processing was the most important activity at these sites" (Bettinger 1976:86). These "pinyon camps" were dated using time-marker projectile points found on the surface of these sites. All but one of the sites date from a period of time ranging between A.D. 600 to A.D. 1850. From this Bettinger declares that pinyon exploitation commenced at approximately A.D. 600 and prior to this pinyon exploitation was negligible or nonexistent.

We wish to address the contention that the physical remains described by Bettinger are the only possible manifestations of pinyon exploitation. If such is the case, one would expect to find similar manifestations in other areas of the Great Basin. Fortunately, a similar surface survey has been accomplished. Thomas's (1971) study of the Reese River Valley employs an almost identical sampling design based on 500-meter random transect tracts which cross-cut biotic communities.

Thomas (1971:47) indicates that "actual harvesting of pinyon nuts leaves only perishable artifacts . . .; the physical remains population resulting from the pinyon harvest is nil. Ancillary activities, such as food preparation took place in the winter village, not in the pinyon grove." Archaeological data from the pinyon-juniper zone in the Reese River Valley support Thomas' hypothesis since there is an extreme paucity of both stone circles and milling equipment as indicated in Table 1.