

AUTHORS RESPOND

We thank the participants in this review forum for expressing their opinions of *Anahulu*.

Atholl Anderson, however, has misread and misrepresented our discussions of the ways in which the historical ethnographic and archaeological investigations mutually influenced each other during the course of the *Anahulu* study. Hence we take this opportunity to make the necessary corrections to his review. Anderson rewrites the history of our collaboration, casting the archaeologists in the role of mere technicians in the service of ethnohistory, seeking material results that were “desired on documentary grounds.” (His reference to “processual archaeology” is also odd, as this term is generally associated with the so-called New Archaeology and with an eco-technodemographic determinism that we specifically reject in *Anahulu*.) We are thus characterized as having “set out to find evidence” of the influx of people into *Anahulu* associated with the occupation of O‘ahu by Kamehameha’s forces in 1804. In Anderson’s version of our interdisciplinary methodology, the archaeological results are therefore criticized as having been interpreted “to fit the historical evidence, without rigorous consideration of alternative hypotheses.”

Nothing could be farther from the reality of how archaeology and ethnohistory were reciprocally engaged over the many years during which this collaboration evolved. As Sahlins recounts on page 1 of volume 1, and Kirch reiterates in volume 2 (49), the

initial ethnohistorical interpretations roused no suspicion of a radical restructuring of land use and settlement following Kamehameha’s conquest and occupation of O‘ahu. Consequently we originally expected that the archaeological excavations would reveal a deep sequence of permanent residence and irrigated agriculture extending well into prehistory. To our complete surprise, archaeology revealed that the landscape of stone-faced house platforms and permanent irrigation plots and canals was a phenomenon of the early contact period—thus sending Sahlins “back to the historic record and to the revaluation of certain known facts about the occupations of O‘ahu in 1795 and 1804 . . . whose significance for the history of *Anahulu* had been overlooked” (1:1). In short, the archaeologists did not go about seeking evidence of an influx associated with the 1804 occupation; they went seeking a deep prehistory of intensive land use and found the material evidence of the 1804 influx instead! This general failure of Anderson to appreciate how the archaeological investigations influenced the historical ethnography is reflected recurrently in his mistaken commentaries on details of the excavations.

Take the case of Mailou’s house, situated in the far interior reaches of the valley, where Anderson confuses the specific hypotheses generated by ethnohistory and archaeology, erroneously suggesting that it was the archaeologists who attributed the site to Mailou’s father after test excavations failed to reveal the presence of exotic (foreign artifacts). Quite to the contrary, the

association between this site and Mailou's father is unambiguously given by the documentary testimony of the Land Commission record, Mailou testifying that his parents (*makua*) received the parcel "at the time of Kamehameha I" (2:60). What the archaeological investigation did was raise a new hypothesis—not indicated in the *Māhele* records—that Mailou's claim in 1848 was of a house site that had already been abandoned by that time (rather than still inhabited as Mailou's testimony that "at this time I am there" might imply). This archaeological interpretation of a house not regularly occupied by the time of the *Māhele* received documentary support in the absence of 'Ili Mikiyai as a unit of taxation in the *konohiki* La'anui's record book of 1841–1842.

True, the absence of foreign artifacts in site D6–40 was curious (although quite likely a function of our small sample size). To us, the most likely explanation is that the Mikiyai household did not have access to rare western goods at this early date. This lack of western artifacts in early house sites is illustrated, for example, by the D6–27 house in Kāloaloa, which yielded only a single iron nail (easily missed if our trench had been differently aligned). But our dating of the Mikiyai house was not restricted to arguments based on documentary evidence; it was directly tested by radiocarbon dating. Anderson notes that a radiocarbon sample from D6–40 was dated, but ignores a second sample from alluvium underlying the adjacent D6–41 irrigation system, which gave near identical ages of 160 ± 60 and 160 ± 70 BP (2:64). To be sure, both

carbon 14 "dates" have multiple intercepts on the calibration curve, but in both cases the highest probability is for the period AD 1716–1886, with specific intercepts at 1739 and 1804. The congruence of the two dates is striking, and strongly suggests that both the house and the adjacent irrigation system were constructed at the same time. Furthermore, the highest probability calibration of the dates is congruent with Mailou's testimony that his father received the land in the time of Kamehameha I. None of this "proves" that the site was built in 1804; such "proof" is beyond the methodologies of either archaeology or ethnohistory. But to suggest, as Anderson's review implies, that we biased our interpretation on a privileged reading of the documents is flatly wrong. Rather it was through the testing of several alternative hypotheses—on the independent data sets of archaeology and historical ethnography—that we reached the interpretation set forth in our monograph. We continue to hold that it is the most plausible interpretation, surviving the scrutiny of both archaeological and ethnohistoric tests.

Take further Kainiki's house (D6–34), on which we expended a great deal of effort in careful excavation (2:70–75). Again, Anderson blithely implies that the archaeological interpretation is "made consistent" with conclusions desired *a priori* on documentary grounds. He accuses us of basing the interpretation of Kainiki's house chronology on a single glass bottle! This is curious (not to say ridiculous), for as we report in painstaking detail, Kainiki's house was *stratified* (indeed, the only house to be so stratified), with an

earlier deposit containing a mixed assemblage of both indigenous Hawaiian and exotic, western materials. The lower stratum is the one we attribute to Kainiki's father, Koali'i. The contrasts in artifact assemblages between the upper and lower strata in D6-34 are very carefully documented in volume 2. In the case of Kalua's house (D6-38), it is true that a single bottle with a manufacture date of AD 1790-1810 is believed by us to be a "curated" artifact. But Kalua's house was not stratified, it had only a single component occupation, dominated by bottles with manufacture dates from 1845 to 1881, and was therefore entirely consistent with the historic records indicating that Kalua had received his house lot during Kīna'u's reign (1832-1835), and that the site had not been occupied from the earlier period of Kamehameha's 1804 occupation. In short, *on strictly archaeological criteria* of stratigraphy and artifact assemblages, sites D6-34 and -38 are shown to have chronologies consistent with the independent evidence of historical ethnography. There is no privileging of documentary sources as Anderson's review implies.

A further example of Anderson's erroneous reading of our text is his remark concerning the D6-51 house, which he says we "attributed to the big man Kamakea" on the basis of an assemblage of artifacts with a "variety and abundance of exotic goods." How totally wrong! We knew that site D6-51 was Kamakea's house before even beginning to clear the weeds from its surface, for this and the adjacent structure are sketched in their correct positions on the 1852 map of Kamakea's Land Commission award made by

Emerson, and reproduced by Sahlins in volume 1 (13, fig. 1.3). Moreover the evidence that Kamakea was a big man—and the only early church member in the upper valley—was well and truly developed from historical documents. In this case it was unnecessary to make archaeological arguments concerning the status of the occupant; rather, archaeology could be used to independently test the ethnohistorical hypothesis that a big man such as Kamakea might have a richer array of foreign material culture.

Anderson is again wrong in his assertion that the restructuring of the upper valley's settlement landscape following the occupation of O'ahu in 1804 by Kamehameha's forces depended heavily on radiocarbon dating. In fact, with the exception of site D6-40 ("Mailou's house"), every open habitation site tested or excavated in the upper Anahulu Valley yielded foreign (western) artifacts. None of these sites contained earlier components with indigenous artifacts, as one would expect if there were permanent prehistoric occupations in the upper valley. In the single case where a site was stratified (the Kainiki house, as described above), the earlier stratum was also demonstrably of postcontact age, based on the presence of glass beads, flints, and other foreign items. The presence of these foreign artifacts provides far more precise means for dating this settlement pattern to the postcontact period than would any corpus of radiocarbon dates. Where the radiocarbon method was useful was in the dating of the irrigation systems, particularly at Kāloaloa. The very young ages obtained from these

irrigation-context examples were entirely consistent with the artifactual evidence from the open site excavations. There is absolutely no basis for Anderson's claim that "many of the house sites, and the irrigation systems, were first occupied or constructed prehistorically by people of undocumented identity." The only sites within the study area occupied prehistorically were the rockshelters, and the chronology and sequence of their use is thoroughly documented in Chapter 2 (vol. 2).

Beyond having to expend valuable journal space on such a detailed refutation of Anderson's unjustified criticisms, we are deeply disappointed that Anderson's review accords no mention at all of several broad anthropological themes resulting from this collaborative engagement of archaeology and historical ethnography. For example, the analysis of levels of surplus production, and of the sociology of canal hydraulics deriving from the irrigation system study (wrongly attributed by Kame'eleihiwa's review to sole authorship by Spriggs—it was a collaboration by Spriggs and Kirch), are matters of some significance for Hawaiian and Polynesian prehistory. The radical transformation of land use in the upper valley following Kamehameha's 1804 occupation is a matter that Kirch subsequently relates to other settlement transformations throughout the archipelago in late prehistory (2:53–56). A further theme is that of architectural changes in Hawaiian housing during the early nineteenth century, an issue largely ignored by archaeologists until recently. The restructuring of burial patterns during the prehistoric and

historic periods is also an important finding, not without relevance to contemporary Hawaiian cultural practices. Yet none of these or other major themes receive the slightest comment from Anderson. Too bad. We are left with the impression that an obsession with radiocarbon dating, as demonstrated by Anderson's work on New Zealand moa-hunting sites, is the organizing general perspective of Pacific archaeology.

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A critique of the ethnographic enterprise, which may be dated for convenience as beginning in 1986 with the publication of Clifford and Marcus's *Writing Culture*, has now spread to works about Melanesia. (Make no mistake: the "anthropology" in this book's title really means "ethnography." No serious attention is paid to archaeology, much less biological anthropology, though these subdisciplines might tell us something useful about "history.") However, although the criticisms in the collection under review are sometimes phrased in such trendy terms as "historicism," "essentialization," and "Orientalism," the