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## CA☆ FORUM ON THEORY IN ANTHROPOLOGY

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# Cook, Lono, Obeyesekere, and Sahlins<sup>1</sup>

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by Robert Borofsky

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The current cause célèbre between Marshall Sahlins and Gananath Obeyesekere involves more than a tempest in a teapot of exotic details. Behind the obvious issue—whether Captain James Cook was perceived by Hawaiians in 1778–79 as a manifestation of their *akua* (a term at times translated into English as “god”) Lono—are broader ones critical to anthropology today: To what degree, for example, do the present cultural politics of identity demand a rethinking of anthropology’s ethnographic effort? Who has the right to speak for whom across the present borderlands of difference? Also: How does one evaluate conflicting claims about someone else’s past? Must politically charged events in other societies at other times generally remain enigmas to Western scholars, or can those scholars, while outsiders, still make sense of them? And, looking at the controversy from still another angle, is anthropology simply a matter of vexation and debate, or is something approaching a common, cumulative understanding of others possible? What can one say about anthropology given the way the current controversy has proceeded? However we frame the controversy, one point is clear: Behind the surface simplicities, behind the antagonizing arguments, illuminating issues exist that demand anthropological attention.

### Contexts

In contextualizing this controversy, one might reasonably begin with a rarely cited piece by Sahlins (1977)

1. I thank the following for their thoughtful suggestions and comments on earlier drafts and/or presentations of this paper: Nick Dirks, Dick Fox, Valerio Valeri, Clifford Geertz, Greg Dening, Jonathan Friedman, Stanley Tambiah, Deborah Gewertz, Fred Errington, Ravina Aggarwal, Andrew Lass, Debbora Battaglia, Renato Rosaldo, Jane Collier, Akhil Gupta, John Fleckles, Susan Hirsch, and, particularly, Gananath Obeyesekere and Marshall Sahlins. I owe a special debt of gratitude to Karen Peacock and Nancy Morris, two great librarians, who facilitate my research on things Pacific. To Tambi.

sketching out an intellectual direction for anthropology in the 1980s. Here Sahlins first uses Cook’s apotheosis as Lono to illuminate broad themes of cultural process in which efforts to reproduce the social order lead to changes in it. As Sahlins later wrote in *Historical Metaphors and Mythical Realities* (1981:8), “The great challenge to an historical anthropology is not merely to know how events are ordered by culture, but how, in that process, the culture is reordered. How does the reproduction of a structure become its transformation?” The events set in motion by Cook’s visits to the Hawaiian islands in 1778–79 became a prominent example of this thesis: Hawaiian efforts to cope with the anomalies of Cook’s visit—by incorporating him into their cultural order—led, over time, to transformations in that order. While not necessarily stressed in key publications, Sahlins’s discussion of the identification of Cook with the Hawaiian *akua* Lono and his subsequent murder attracted much attention from others interested in understanding these well-known events. “The killing of Captain Cook was not premeditated by the Hawaiians,” Sahlins wrote. “But neither was it an accident, structurally speaking. It was the [religious celebration of the] Makahiki in an historical form” (1981:24). In *Islands of History* he added, “Cook’s death at Hawaiian hands just [after the Makahiki could] . . . be described as [a] . . . ritual sequel: the historical metaphor of a mythical reality” (1985:105–6).

Two sets of concerns initially raised about Sahlins’s analysis of Cook are relevant to the current controversy. The first, by scholars such as Greg Dening (1982), questioned the tightness of Hawaiian cultural structures. To what degree these structures shaped, as opposed to simply providing a meaningful context for, human action was for Dening an open question (cf. Friedman 1988). While Sahlins does not always amplify the point in his writings, a careful examination indicates that he remains sensitive to this concern: cultural structures, he states, are indeed negotiable (see, e.g., 1977:25; 1981:35; 1985:144; 1995:204, 251). Sahlins also observed in *Islands of History*, however, that those with power could enforce certain structures on others: “Whatever the people in general were thinking, the Hawaiian powers-that-be had the unique capacity to publicly objectify their own interpretation. They could bring structure to bear on matters of opinion” (1985:121–22). The second concern—raised by Jonathan Friedman and his students—challenged Sahlins’s analysis of the historical data. Friedman suggested that “Captain Cook was not treated as a ‘god’ but as a chief” (1985:194). Bergendorff, Hasager, and Henriques (1988) questioned Sahlins’s interpretation of the 1778–79 Makahiki. (The form depicted by Sahlins, they suggested, evolved only years after Cook’s visit.) In reply, Sahlins (1989) presented such a wealth of documentation to support his contentions that these suggestions tended to fall by the wayside.

Not long after, however, Obeyesekere presented a related analysis in *The Apotheosis of Captain Cook* (1992) that turned Sahlins’s thesis regarding Cook al-

most on its head. Instead of interpreting Cook's apotheosis as Lono in terms of Hawaiian mythology, Obeyesekere interpreted it in terms of European mythology. Instead of focusing on Hawaiian rituals and symbols, he emphasized Hawaiian pragmatics. Critically, he asserted that the Hawaiians did not see Cook as the god Lono; rather, he was viewed as a chief named Lono.<sup>2</sup> At the core of Obeyesekere's analysis were two points: (1) that Cook's apotheosis was based on European, not Hawaiian, myth making: "To put it bluntly, I doubt that the natives created their European god; the Europeans created him for them. This 'European god' is a myth of conquest, imperialism, and civilization" (p. 3); and (2) that the plethora of sources cited by Sahlins (in confirmation of his thesis) could be interpreted in a number of ways: "The very possibility of a plausible alternative interpretation is at the very least a demonstration of the folly of attempting any rigid interpretation of symbolic form" (p. 82).

Obeyesekere suggested that Western anthropologists such as Sahlins had taken away Hawaiian voices by portraying their cultural categories in a manner that separated them from rather than united them with Europeans. He pointed out that Hawaiians possessed as shrewd a sense of the pragmatic—what he termed "practical rationality" (i.e., "the process whereby human beings reflectively assess the implications of a problem in terms of practical criteria" [1992:19])—as Europeans. Obeyesekere felt that as a Sri Lankan—as one from a country only recently freed from colonialism—he had a certain insight into the colonial politics affecting Hawaiians in times past that let him grasp their experiences in ways that Western scholars such as Sahlins might not (pp. 8–9, 21–22).

Sahlins's first reaction was not to respond to Obeyesekere. He preferred leaving that task, he said, to reviewers (1995:ix). But the overall tone of the 29 or more reviews of *The Apotheosis of Captain Cook* that have appeared in print has been fairly positive. In fact Obeyesekere won two awards for the book, the Louis Gottschalk prize from the American Society for Eighteenth-Century Studies being the more notable. Clearly, if someone was going to defend Sahlins, it would have to be Sahlins himself. Only he knew the primary material in enough depth to answer the specific charges leveled at him. (Being unfamiliar with key aspects of Hawaiian ethnography, most reviewers tended to evaluate the controversy in fairly broad terms.)

*How "Natives" Think: About Captain Cook, For Example* (1995) is Sahlins's response. Hacking (1995:6) calls it a "work of refutation and revenge, judicious and remorseless." It focuses on two central concerns. The first is to restate Sahlins's position regarding various specifics on which Obeyesekere questioned him, and the second is to refute Obeyesekere's criticisms point

2. Obeyesekere did not acknowledge similarities between parts of his thesis and Friedman's (1985). Rather, he preferred citing another predecessor—the part-Polynesian anthropologist Te Rangī Hiroa, Peter Buck (1992:75).

by point. Sahlins considers a wide range of historical documentation involving, to quote Hacking, "an immense amount of detail" (p. 9). Throughout the book, Sahlins is critical of Obeyesekere's criticism. For example, "Obeyesekere often alleges I failed to say things I did say—and just as often attributes statements to me that I did not say" (p. 29).

Sahlins's second theme relates to broader issues raised by the controversy. Where Obeyesekere emphasizes transcultural aspects of Hawaiian thought (in relation to practical rationality), Sahlins focuses on its culture-specific qualities. "Epistemologies," he states, "vary . . . with world views (cultural ontologies)" (1995:179)—different cultures, different rationalities" (p. 14). Sahlins also accuses Obeyesekere of conducting "pidgin anthropology"—"substituting a folkloric sense of 'native' beliefs for the relevant Hawaiian ethnography" (p. 60): "When I say . . . [that Obeyesekere's] distortions amount to a 'pidgin anthropology,' I mean that they have the quality of ad hoc fabrications based on a sort of generic primitivism, like Fenimore Cooper Indians. They appeal to a popular sense of common average 'native' thought" (p. 62).

## Differing Readerships, Differing Styles of Knowing

A careful examination of Obeyesekere (1992) and Sahlins (1995) suggests that they are partly talking at cross-purposes.<sup>3</sup> No matter how much evidence each presents to buttress his case, the other does not concur because he uses a different though related perspective to demonstrate different though related points.

Two central concerns pervade Obeyesekere's analysis. They are ones that most readers today would readily accept, and for those not deeply familiar with the Hawaiian data they are concerns that indicate that Obeyesekere is on target, so to speak, in his analysis.

The first is the problematic nature of the historical material. "One must probe into the hidden agendas underlying the writing of [historical] . . . texts," Obeyesekere notes (1992:66), and in *The Work of Culture* he says, "A text does not exist by itself; it is embodied in a context" (1990:130). For Obeyesekere, historical accounts "have to be *deconstructed* before they can be ef-

3. Readers interested in additional references on points raised in this section may wish to consult (1) *on Obeyesekere's perspective: historical data problematic*, Obeyesekere (1992:xiv, 67, 69, 112, 116, 159); *Western misperceptions*, Obeyesekere (1992:20, 120, 123, 137, 140, 147–48, 173); *creating doubt*, Obeyesekere (1992:78, 86, 90, 98, 144); *selective*, Obeyesekere (1992:182, 154, 163, 212 n. 54, 215 n. 78); see also Obeyesekere (1993, 1994, 1995a); (2) *on Sahlins's perspective: concern for evidence*, Sahlins (1995:2, 100, 115–16, 117); *command of material*, Sahlins (1995:199–285); Geertz (1995:6); Hacking (1995); Fagan (1995); Powers (1995); Corrigan (1995); *weighing of evidence*, Sahlins (1995:21, 27, 43, 45, 51, 71); *implicit concern*, Sahlins (1995:8, 10, 11, 14, 32, 38–39, 86, 97); *explicit concern*, Sahlins (1995:98, 100, 108, 174, 279–80); *bold, daring*, Sahlins (1995:22–23, 24, 45, 70, 71, 83, 218, 228); *ambiguities and flexibilities*, Sahlins (1995:104, 106–7, 221, 222, 228).

fectively *reconstructed* as reasonable history" (1992: 144). While Kotzebue, for example, tends to be sympathetic to indigenous Hawaiians, Obeyesekere observes, his account cannot be accepted uncritically. To assess its value, one must carefully examine its contexts of production (p. 144). Such caution is particularly important in respect to "on-the-spot" reports written by the British during their stay. The unpublished journals and logs of the visit differ in significant ways from later published versions, Obeyesekere notes. Rickman's published account at times "widely deviates," for example, from Rickman's unpublished log (p. 214 n. 73). Similarly, King's official account of the voyage differs from his original journal (pp. 124–25).

The second concern is the misperception of Hawaiians' understandings of Cook by various agents of Western expansion—explorers, traders, and missionaries. Obeyesekere asserts that the apotheosis of Cook "was created in the European imagination of the eighteenth century . . . based on antecedent 'myth models' pertaining to the redoubtable explorer cum civilizer who is a god to the 'natives'" (1992:3). The "idea that the European is a god to savages is . . . a structure of the long run in European culture and consciousness" (p. 123). Also, accounts written by Hawaiians under missionary guidance as statements about the Hawaiian past—such as *Mooolelo Hawaii*—show considerable missionary influence, Obeyesekere suggests. *Mooolelo Hawaii* could be seen, he indicates, as "a mythic charter for the new vision of Hawai'i of the evangelical missionaries" (p. 162). This is why Obeyesekere, in various conversations, has suggested that his book is more about European than Hawaiian society. It involves exploring the distorted lenses through which Westerners see Hawaiians. In a way, certain of Obeyesekere's criticisms regarding Sahlins derive from this point: Sahlins, as a Western scholar, continues earlier European "myth models" of Hawaiians (p. 177). Embedded in Obeyesekere's statements is a certain moral positioning. Given the gaps and silences that exist in various historical accounts, modern scholars need to give new voice to indigenous perspectives, he says, by "reading across the grain" of previous history-tellings: "One of the disconcerting features of contemporary scholarship on Cook . . . is the cavalier manner in which bits and pieces from the missionary and *Mooolelo Hawaii* narratives are taken to prove the hypothesis of the apotheosis. I think these procedures are endemic to the scholarship pertaining to nonliterate people who cannot strike back" (p. 154). And later he adds that there is much in the *Mooolelo Hawaii* "that is hidden, waiting to be brought to the surface. . . . an examination of [the Hawaiian] Kamakau's text with a gaze of suspicion sheds considerable light on the nature of an indigenous Hawaiian discourse that is the very opposite of the evangelical" (p. 168).

In emphasizing these concerns, Obeyesekere seems more intent on creating doubt about previous analyses and what else might be possible because of them than on defending a particular position. Thus, for example,

he questions Sahlins's interpretation of the initial "thefts" at Kauai (in 1778) on the basis of Cook's limited knowledge of Hawaiian and notes that "alternative interpretations are possible" (1992:70). He hazards his own "guess," but it is only a guess. He never suggests it as something definitive—presumably because he is sensitive to the ambiguities of interpretation (p. 82). Again and again he questions earlier (especially Sahlins's) accountings of Cook's visit (e.g., pp. 86, 95). Again and again he suggests alternatives with such phrasings as "hence my hypothesis" (p. 78), "my own guess" (p. 95), and "it is likely that" (p. 103). Rarely, however, does he take a definite stand regarding the provocative possibilities raised.

Because Obeyesekere perceives a host of biases in the data, he is selective concerning what he does and does not consider reliable evidence. "I do not treat all texts the same way," he writes. "I am suspicious of some and treat others more seriously. I try to disentangle fantasy, gossip, and hearsay from more reliable eyewitness accounts" (1992:xiv). He relies, for example, more on Ledyard's than on Rickman's account of the British stay at Kealakekua Bay (p. 215 n. 78, n. 83). Sahlins's reliance on accounts by Kamakau and Malo for a description of precontact beliefs he finds "untenable" (p. 164). Obeyesekere is cautious about taking a host of sources and, despite their various limitations, piling one on top of another to get some overall sense of what transpired at a particular time. By the way he contextualizes sources, by the way he evaluates texts, it is clear he weighs the evidence with deliberation (see p. 67).

Finally, I would add that Obeyesekere's analysis often resonates with our own understandings and our own times. The notion that European explorers would see themselves as gods to Pacific islanders (1992:123), for example, makes sense to many in the context of today's postcolonial critiques. When Obeyesekere suggests that something "is therefore entirely possible" (e.g., p. 86), this often makes sense to many Western readers. When he uses other Polynesian chiefs to make deductions about the Hawaiian Kalani'ōpu'u's motivations, such deductions fit with anthropological notions of comparison within a common cultural area (see Salmond 1993). And when he talks of conspiracies (p. 203 n. 29), "shredding of damaging evidence" (p. 216 n. 29), and a "cover-up" (p. 112), many scholars, I have discovered, think it makes perfect sense, given our times.

Sahlins takes a different tack. On the surface, he appears less concerned with the relation between text and context than in the specifics of the evidence. "An anthropology that defines itself as 'cultural critique,'" he suggests, "too often dissolves into a 'pseudo-politics of interpretation'" (1994:41). He approvingly quotes Lucian: "This, then, is my sort of historian . . . in his writings . . . [he lays] out the matter as it is" (p. 41). Compared with Obeyesekere, Sahlins speaks with more confidence regarding what the documentary material suggests. There is less hesitancy, less guess and hypothesis: "It will be easy to show," he writes, "that, in word and deed, Hawaiians received Cook as a return of Lono"



(1995:2). And in respect to the nature of the Makahiki celebration at the time of Cook, he suggests that it is "an empirical issue for the most part, to be settled by comparing the Cook documents with the later Makahiki corpus" (p. 31).

In contrast to Obeyesekere's selective embracing of the documentary evidence, Sahlins seeks to be more inclusive. One can hardly read Sahlins (1995) without recognizing the enormous command he has over the material. One can see it in details. He points out that Obeyesekere's depiction of Lono's canoe, for example, is a misinterpretation based on a missionary mistranslation of a Hawaiian text (pp. 105, 109). One can also perceive it in citations. Sahlins repeatedly makes reference to a number of sources in developing a point: He cites Ellis, Mariner, Dimsdell, the Vancouver people, and Little, for instance, in respect to the role of Cook's bones in post-Cook Makahiki celebrations (p. 110). Seven lines later, in relation to Cook's divinity at the time of his death, he considers Ellis, Judd, Kotzebue, Bachelot, and Kamakau. (Sahlins's bibliography contains 316 references, Obeyesekere's 152.)

Rather than weighing one context of production against another, Sahlins weighs one piece of evidence against another to ascertain general patterns, to verify a particular assertion. Thus he notes that the Cook journals and the *Moolelo Hawaii* "corroborate each other" in respect to Kalani'ōpu'u's fighting on Maui and that both are "consistent with" the classic description of the Makahiki calendar (1995:36). In discussing the historiography of the Makahiki, he refers to Malo, K. Kamakau, I'i, and Kepelino, noting that "none of them seriously contradicts the others or is in any way aberrant" (p. 209). And in respect to two of Ledyard's assertions (regarding the dismantling of a Hawaiian *heiau's* palings for firewood) he states, "Neither . . . can be corroborated from other accounts and the second is clearly contradicted by later events" (p. 268). Rather than dismissing this or that text because of biases in its production, Sahlins prefers to see textual biases as cultural information. He notes that "a report may be historically inaccurate . . . yet still structurally revelatory"—such as the claim by Hawaiians that Cook slept with the daughter of Kamakahahei at Kauai (pp. 43, 280; cf. Beaglehole 1967:266).

Sahlins does not deny the problems that Obeyesekere deals with regarding the relation of text to context. But his weighing of information, his examining of the contexts of production, tends to be more implicit than explicit, or, perhaps better phrased, text and context are not so consciously tied one to the other as they are with Obeyesekere. Sahlins indicates in respect to the Makahiki, for example, that "the evidence shows substantial continuity and regularity of the celebrations" (1995:27), but he does not elaborate on the point in ensuing paragraphs. He waits until a few pages later to provide relevant documentation (pp. 31, 208). Similarly, in reference to the Makahiki he uses the phrase "according to the classical rules," implying that some sense of definitiveness is involved (p. 37). On the next page, in another

context, we learn, however, that "Hawaiians knew how to overcome their ritual scruples" (p. 38). And later still it is clear that Sahlins is well aware that ritual can be flexible in nature (pp. 39, 251). The one notable exception to this general style of presentation concerns his replies to Obeyesekere's criticisms. Here he looks very intently at the relation of text to context. In respect to Obeyesekere's use of Chamisso (and Kotzebue) as sources, for example, Sahlins considers in detail the various editions of the text, even comparing English translations (p. 99). And in defending himself against Obeyesekere's assertions regarding the Makahiki, he contextualizes the basis of various Hawaiians' knowledge claims, especially K. Kamakau's (pp. 208–9).

Sahlins's assertions are commonly bold ones. He suggests that Cook's return (on February 11, 1779), for example, "presented a mirror image of Makahiki politics" (1995:81). There are none of Obeyesekere's qualifiers here. Later he states, "The Hawaiian schema of things can be understood as a unitary system of two dimensions" (p. 167)—again few cautions and hesitations. "Perhaps's" and "maybe's" do occur. Regarding the opposition between the Lono priests and "the king's party," he states, comparing accounts from Cook's visit with those of Portlock and Dixon seven years later, "The opposition thus seems to have been recurrent, perhaps structural" (p. 71, see also, e.g., pp. 24 n. 10, 83). In comparison with Obeyesekere's, however, doubt, qualification, hypothesis, and uncertainty are less central to Sahlins's *modus operandi*.

It is here that Sahlins appears the most brilliant and, at the same time, the most vulnerable. His powerful synthesis allows others to make better sense of old confusions and complexities. He brings diverse materials together in an insightful, thoughtful manner. But it is also this clarity of vision that sets off alarm bells for scores of postmodern scholars sensitive to the ambiguities of interpretation and the complexities of life. These alarm bells constitute a central element in Obeyesekere's critique (see, e.g., 1992:67). While Sahlins may seem out of step with current scholarly trends, a careful analysis of his work shows that he remains sensitive to the ambiguities of interpretation and the flexibilities of structures. He simply does not emphasize them to the same degree as Obeyesekere; they are not, as noted, always closely enmeshed. One needs to search a little. A number of anthropologists continue to insist that Sahlins seems insensitive to such issues, but a careful reading makes clear that he is not. For example, "Nothing guarantees that the situations encountered in practice will stereotypically follow from the cultural categories by which the circumstances are interpreted and acted upon" (1981:35). Or again, "Every reproduction of culture is an alteration, insofar as in action, the categories by which a present world is orchestrated pick up some novel empirical content" (1985:144). Or again, "To say that an event is culturally described is not to say it is culturally prescribed. To conflate the cultural structuration of events with the necessity of one particular ordering is abusive" (1995:251).

Still, we might wish to ask Sahlins certain questions: When he states that "'laying out the matter as it is' . . . [involves] the historical issue of understanding people's cultural constructions of events" (1994:41), what pitfalls does he see to the process? How are they different from the ones Obeyesekere takes up? Why quote Lucian rather than Carr, Foucault, E. P. Thompson, or Denning as an anthropological model for history-telling? Why not weave text and context more closely together?

## Evaluating Conflicting Claims

Given the different perspectives involved in the controversy, how do we make sense of the different knowledge claims?<sup>4</sup> If one thing is certain, it is that we must move beyond first impressions.

4. Readers interested in additional references on points raised in this section may consult the following: *similarities, chiefs*, Obeyesekere (1992:86, 91, 197) in relation to Sahlins (1995:2, 99, 128, 144, 196, 192, 194, and passim; cf. 136); *similarities, practical rationality*, Obeyesekere (1992:10, 18, 19) in relation to Sahlins (1995:152, 154, 169, 170); *divergent accounts*, e.g., Beaglehole (1974:674–75; 1967:547, 561, 567); cf. Obeyesekere (1992:234 n. 66); *making sense*, Sahlins (1995:9, 118, 119, 121, 151, 152); *problems with Obeyesekere's arguments*, Sahlins (1995:110, 192, 236): criticism without analyzing the context of Cook's "terror," Obeyesekere (1992:xv–xvii, 27, 29, 30, 41, 80, 139); cf. Beaglehole (1967:589), Rodger (1986:205–51); basis for choosing Obeyesekere's interpretations over others unclear, e.g., Obeyesekere (1992:73, 155 in relation to 83–86); degree to which the Kāli'i was a coherent ritual and what constitutes "all sources" as the basis for deductions regarding the Kāli'i, Obeyesekere (1992:182 in relation to 199); basis for perceiving and/or deducing major social divisions, such as between priests and "counselors," in Hawaiian society, e.g., Obeyesekere (1992:91, 93, 171) in relation to Sahlins (1995:72, 197–98, 227, 256–63); *Obeyesekere's imprecise scholarship*: not tolerating resistance, Obeyesekere (1992:6 vs. 27); date of Sahlins's talk, Obeyesekere (1992:8) vs. Sahlins (1995:3); Cook's violence "unrecorded by any" and full Gilbert quote in relation to what is cited, Obeyesekere (1992:32); context of Obeyesekere (1992:33–34) regarding Cook at *inasi*, e.g., limited citation of quote involving n. 47 in relation to Beaglehole (1967:151); headshaving, Obeyesekere (1992:36 vs. Cook and King 1784, vol. 2:82), Obeyesekere (1992:44) vs. Beaglehole (1974:648); Watts in relation to the more complete quote in Beaglehole (1967:479–80), Obeyesekere (1992:45); "every biographer and historian," Obeyesekere (1992:49); "most of these versions," Obeyesekere (1992:50); basis for statements regarding Makahiki's occurrence, especially in relation to later comments, e.g., 1992:95, Obeyesekere (1992:58); "fabrication" overstates what Beaglehole reference asserts, while Ledyard's book is not cited Ledyard clearly is in Beaglehole (1974), Obeyesekere (1992:116); citing of Stokes rather than actual (and full) quotation from Dibble 1909:iii–iv), Obeyesekere (1992:159); basis for assertions regarding Hikiau ritual, e.g., "it is clear," "it is reasonably clear," Obeyesekere (1992:83–84); Denning in relation to Sahlins (1995:282–83), Obeyesekere (1992:198); Cook's beatings in relation to Bligh, see full Denning citation (1988:22) or Denning (1992:61–62), Obeyesekere (1992:14); "curse the scientists" in relation to full Zimmermann (1926:48) quote and context, Obeyesekere (1992:14); basis of statements regarding "unreliable by modern standards" and "plagiarized" as a dismissal for Beaglehole (1967:ccviii–ccix), also the degree Obeyesekere then heeds such statements in documenting his argument, Obeyesekere (1992:203 n. 29); basis for "silent conspiracy" given citations relating to Cook's role in his own death in Beaglehole (1967:clxxvi n.1, 537, 1536), Beaglehole (1964:305), and Gilbert (1926:11), see also Fisher and Johnston (1979), Obeyesekere (1992:203 n. 29); "not one of the ships' journal writers" in relation to

One of the intriguing aspects of the controversy is that the differences between Obeyesekere and Sahlins—on certain issues—are not necessarily that great. This is true regarding Cook's status as Lono and the notion of "practical reason." It is a small step, for example, from saying that Cook was perceived as a chief named Lono (Obeyesekere's position) to saying that Cook was perceived as a manifestation of the *akua* Lono (Sahlins's position) if one accepts that some chiefs possessed divine qualities. Obeyesekere acknowledges that "it is possible that Hawaiians had some notion of divinity inherent in chiefs of high descent" (1992:198; cf. p. 91 and Sahlins 1995:128). Valeri—who is steeped in the material—perceives "no necessary contradiction between the view that Cook was Lono the chief and the view that he was Lono the god. A contradiction only arises when a non-Hawaiian view of 'divinity' . . . is introduced into the situation" (1994:126).<sup>5</sup> To be a human chief, then, did not preclude the possession of divine attributes. Or, to reverse the statement, to be seen by various Hawaiians as a manifestation of Lono did not mean that Cook was perceived by these Hawaiians as somehow less human.

In respect to "practical rationality," one might note that neither Obeyesekere nor Sahlins disputes that "magical" and "practical" reasoning can be intertwined (see Obeyesekere 1992:15, 21, 205 n. 48; Sahlins 1995:6, 155). Nor would either disagree, I presume, with Obeyesekere's statement that a "common humanity . . . [underlies] formal differences" (1990:218). But how does one give this statement concrete form? If one seeks specific examples of "practical rationality," there is a rich literature in Polynesia on pragmatic perspectives (see, e.g., Borofsky 1987; Howard 1970, 1974; Levy 1973; Marcus 1995; Shore 1982). But isn't such cultural specificity the very specificity that Sahlins stresses (see, e.g., Sahlins 1995:155, 169)? The question really involves an empirical issue: How does a postulated pragmatic trans-cultural tendency work itself out in a specific island en-

Samwell, King, and Clerke (cited in Beaglehole 1967:1536), Obeyesekere (1992:75–76); basis for deductions regarding appearance of (given Malo 1951:83; Valeri 1985:325, 327) and language spoken by Hawaiian *akua*, Obeyesekere (1992:61); *misrepresentation of Sahlins's position*: "not much difference between King Kalaniō'pu'u . . . and the god Kū," also on what basis Kalaniō'pu'u is interpreted as "king" rather than paramount chief, Obeyesekere (1992:21); implication that Sahlins indicated "rituals exactly paralleled," Obeyesekere (1992:53); "Sahlins does not explain why . . .," Obeyesekere (1992:56); Sahlins never indicated that intra- and interisland Makahiki variations did not exist, Obeyesekere (1992:59); "virtually no instances in Sahlins's corpus where a source is critically examined," Obeyesekere (1992:67); "information from any test is used as long as it fits the structuralist thesis," Obeyesekere (1992:67); if *Mooololelo* was a product of . . . as Sahlins implies," Obeyesekere (1992:159); "empirical accounts . . . have been subtly, and sometimes not so subtly rephrased or altered," Obeyesekere (1992:177); see also Sahlins (1995:29, 49, 193, 239–40).

5. Friedman notes: "Divinity is . . . an attribute of high status. . . . The Western concept of god is inapplicable to a context where humans can be gods incarnate in a universe where there is a genealogical and functional continuity between gods and chiefs" (1985:194–95).

vironment? What seems to be culture-specific? What might be viewed as transcultural?

A careful reading of the published and unpublished accounts of the British visit to Hawaii makes another point: The British did not grasp everything that was happening around them (see, e.g., Beaglehole 1967:506). Whatever their linguistic and ethnographic abilities, it is clear that they did not fully comprehend certain Hawaiian perspectives and practices (see Sahlins 1995:275–77). Most sought to report events as they saw them—as was their task (cf. Smith 1960:2; 1992:25–26). But different people saw different things, and people seeing the same event at times reported it differently. The result is a set of overlapping but divergent accounts. This means that modern scholars can comb through the material, selectively choosing quotes here and there, to support different arguments. While reading eyewitness accounts of centuries-old events may impress some anthropologists, it is important to be rather cautious about relying too much on any single account. Each account must be viewed within the context of the whole corpus of material. The plausibility of any assertion has to be judged in relation to what others reported at the same time in the same place (cf. Sahlins 1995:117; Obeyesekere 1992:203 n. 29).

Parenthetically, I would also add—and this will be obvious to some but not others—that whether Obeyesekere's or Sahlins's analysis makes more sense to us is not the central issue. What we need to ask is which analysis accords better with Hawaiian and British understandings in 1778–79 as they have come down to us today (cf. Sahlins 1995:127, 151–52).

As one works one's way deeply into the material, first in terms of the logic of the arguments and secondly in terms of the supporting documentation, certain points, I believe, become clear.

First, there are serious problems with Obeyesekere's argument. Geertz's statement that it follows the "beat-the-snake-with-whatever-stick-is-handny" (1995:4) strategy catches the sense of Obeyesekere's presentation. His subarguments do not necessarily tie together as a coherent, cogent whole. Important discrepancies and contradictions exist. Obeyesekere's central premise that a European myth of the long run depicts Europeans as gods to savage peoples faces, for example, a basic contradiction. Sahlins and Obeyesekere agree that nowhere else in Polynesia did the British describe Cook as being taken for a god (Obeyesekere 1992:87; Sahlins 1995:178), even where indigenous populations seemingly did hold such an opinion of him (see, e.g., Salmond 1993:51). If Cook's apotheosis was a European myth rather than a Hawaiian assertion, should it not have been noted elsewhere as well? The one related example that Obeyesekere mentions for the Pacific—involving Wallis at Tahiti (1992:123; Robertson 1973:43)—is ambiguous and incomplete. It amounts to a single phrase. It needs to be supplemented by a host of additional cases, especially from the Cook voyages. The myth, I would add, also runs counter to a sense among many in England during this period, particularly among those of

"middling" rank, that it was improper to place oneself at the level of god. What is intriguing is that documentation for this point—a frequently cited passage by Cowper (see, e.g., Beaglehole 1964:289), a popular poet (see Davidoff and Hall 1987:92, 157)—is in Obeyesekere's own volume (1992:126; cf. Sahlins 1995:200). In other words, to counter his thesis one simply needs to sift systematically through the data he presents. More generally, for a book that focuses on the dynamics of a European myth, relatively little space is taken up with examining the European contexts of the myth (pp. 120–37; cf. Robertson 1981). Which Europeans at what time adhered to this myth, before and during Cook's years of exploration, is left vague. Linking the Spanish Cortés with the British Cook (two and a half centuries later) is a fairly broad stretch, especially when so few other examples are given from these or other countries and contradictory information clearly exists for Cook's time. A little investigation will also indicate to readers that on various occasions Obeyesekere uses the same source in contradictory ways. He notes that King's published account differs from his shipboard journal (p. 68), for example, and uses this difference to discount a passage in Cook and King (1784), the official admiralty account. Yet on the page before that he has indicated that the shipboard journals may well be biased (p. 67), and a few pages later he cites both King's journal and Cook and King (1784) in discounting Rickman's account (p. 72). As for Rickman, Obeyesekere doubts his linguistic ability and reliability as a journalist (pp. 72–73) but then, shortly afterwards, cites him as a definitive source (p. 81). (Rather than citing Rickman's ambiguously reliable journal, in fact, he cites the still less reliable—by Obeyesekere's assessment—published account [pp. 217 n. 48, 71–72].) And he asserts that S. M. Kamakau "has excellent accounts of native cosmology" but then indicates that these accounts display a range of biases that makes Sahlins's reliance on them "quite untenable" (p. 164). Yet he himself cites Kamakau in respect to pre-Christian Hawaiian understandings of *akua* (p. 140).

Intriguingly, though these contradictions and gaps in argumentation are fairly self-evident (I selected these examples for that reason), few of the 29 reviews of Obeyesekere's (1992) book that I have read refer to them. Of the reviews examined—Alter (1992), Levy (1992), Burce (1993), Ernst (1993), Hanson (1993), Gough (1993), Knauff (1993), Lamb (1993), Linnekin (1993), Martin (1993), Rose (1993), Salmond (1993), Smith (1993), Thomas (1993), Campbell (1994), Carter (1994), Friedman (1995), Frost (1994), Hanlon (1994), Kaeppler (1994), Kame'eleihiwa (1994), Lindstrom (1994), Linnekin (1994), Modell (1994), Osborne (1994), Parmentier (1994), Thomas (1994), Valeri (1994), and Parker (1995)—only Linnekin (1994), Parker (1995), and Valeri (1994) discuss the second problem noted above and only Hanlon (1994), Linnekin (1994), Knauff (1993), Parmentier (1994), and Parker (1995) the third. No one refers to the first. Part of the reason for this dearth of comment, presumably, is that reviewers must be highly selective, in the space allotted them, regarding their remarks. But



shortage of space cannot, I believe, account for the largely positive tone of most reviews, especially when such contradictions are reasonably clear on close reading. The dearth of critical comment on Obeyesekere's arguments stems, I suspect, from two other factors. The large number of citations to unpublished and/or unfamiliar material can be intimidating to reviewers, and, as noted above, Obeyesekere's style and perspective very much fit with current trends. The rush to review and the acceptance of current scholarly trends, I am suggesting, tended to lull many reviewers, particularly those unfamiliar with the primary documentation, into accepting Obeyesekere's arguments on trust. After all, they do make sense to us.

What of the specific details that few reviewers could readily delve into? Obeyesekere (1992) contains much imprecision in this regard. Sahlins accuses Obeyesekere of "selectively ignoring or misrepresenting the primary documents" (1995:117; cf. 193), and in my opinion that is true. Let me cite a few examples (for others see n. 4): Obeyesekere asks, "Who would have expected Cook, even in his first voyage, to be a bit of a crook?" (p. 23). The reference is to Cook's adding his two sons to his ship's rolls—a practice that Beaglehole admits is "chicanery, but accepted naval custom" (1974:141). Obeyesekere, while citing Beaglehole's reference to chicanery (and paraphrasing the reference to accepted naval custom), emphasizes that this practice was "in flagrant defiance of an act of parliament which threatened the penalty of permanent dismissal from the service" (p. 23). The fuller quote reads that in wanting his sons able to be naval lieutenants before they were 40 "he was willing to follow the example of post-captains and admirals innumerable, in flagrant defiance of an act of parliament" (Beaglehole 1974:141). The fuller quote makes Obeyesekere's question a bit too dramatic. Cook seems much less "a crook" given the British context and period—with such a pervasive practice and such distinguished company. And it makes the comparison (and generalization) that follows to "Italians and other Third World peoples" (pp. 23–24) puzzling, especially given that the above example appears the sole basis for Obeyesekere's analysis of 18th-century British morality as "moral familism." A closer study of British laws and their violation would be in order (see, e.g., Hay and Snyder 1989, Linebaugh 1992, Gilmour 1992, and Thompson 1963). Obeyesekere (p. 206 n. 10) indicates that Beaglehole does not refer to the prize for discovering the Northwest Passage, but it is dealt with fairly extensively (1974:478, 484)—more accurately and in more detail, in fact, than in the reference Obeyesekere cites. Obeyesekere (pp. 44, 209 n. 118) quotes Beaglehole (1972:646) as indicating that "it begins to look as if Cook . . . had lost touch with his men." Such an assertion may exist, but it does not exist on the cited page. Obeyesekere refers several times (e.g., pp. 44, 53, 64) to Cook's going "round and round" the island of Hawai'i. Beaglehole (1967:268, fig. 8) and Cook and King (1784, vol. 3:map facing p. 1) indicate that this is incorrect. The British sailed around the island once. And finally,

Obeyesekere cites Todorov's *The Conquest of America* as the "immediate intellectual precursor of Sahlins's own work" (1992:16). Todorov's book was published in French in 1982—a year after Sahlins's initial major statement (1981)—and in English in 1984. (The reference Obeyesekere cites for Todorov is a 1987 edition.) The statement makes no sense as presented.

Obeyesekere at times significantly misrepresents Sahlins's work. For example, he argues that "Hawaiian culture . . . Sahlins says, . . . is given to 'stereotypic reproduction'" (p. 55). Yet Sahlins actually says, "As for stereotypic reproduction, strictly speaking, it does not occur" (1977:23), and later, in a book Obeyesekere repeatedly cites, he writes, "I argue that . . . the theory [of stereotypic reproduction] is better reversed: plus c'est la même chose, plus ça change" (1980:7). When Obeyesekere finds data contradicting "stereotypic reproduction"—such as in regard to the Makahiki's ritual schedule in relation to Cook's visit (pp. 64–65)—he claims that it casts doubt on Sahlins's position. It might more reasonably be construed as the reason Sahlins never held that position in the first place. On p. 181 Obeyesekere states, "Sahlins has to alter the British accounts to make them fit his myth. . . . Sahlins has to distort the evidence . . . [and] Sahlins again misunderstands the evidence." A careful reading of the cited references will indicate that none of these statements is true. Readers might at first glance perceive these commentaries as significant critiques of Sahlins's work, but a careful examination of the documentary material, reference by reference, indicates otherwise.

I have asked Obeyesekere on two occasions why he wrote the book in such a polemical style. (He agrees that it is polemically written.) And both times I received the same answer—to stir things up. Yet what he has done, more than simply stir things up, is show how academic scholarship often depends on appearance and trust, as the reviews make clear.

We are thus left with some significant questions: With so much going for him—in terms of general concerns most modern scholars would concur with—why did Obeyesekere frame his arguments and supporting data so much at odds with key portions of the documentary material? Concerned as he is with text/context relations, why did he take so much of Sahlins's work out of context? And why did he make *The Apotheosis of Captain Cook* so polemical that the chance for meaningful dialogue with Sahlins about a host of critical anthropological issues was essentially destroyed?

## Reexploring the Documentary Data

If we set aside the controversy's polemics and work our way once more through the documentary materials and the contexts within which they were produced, we can, I believe, make considerable headway in unraveling certain issues.

In respect to Cook as Lono, a few points shine through the data. First, there is considerable ambiguity



regarding what "Hawaiians"—as some collective unit—thought of Cook. For example, Valeri suggests that Lono may have been associated with the color black (1985:15), and Malo indicates that the Makahiki image, associated with Lono, involved white tapa cloth (1951:144), but on a host of occasions Cook is associated with the color red, especially being wrapped in red cloth (Cook and King 1784, vol. 3:5, 7, 13, 18; Beaglehole 1967:504, 505, 1, 195; Obeyesekere 1992:46, 65; Sahlins 1995:69, 224). What did it signify? Red may not have been specific to a particular *akua* (Sahlins 1995:54; Valeri 1985:390 n. 79), but Valeri suggests that it might well have been associated with the Hawaiian *akua* Ku (pp. 12, 15, 270, 322). Cook's identification with Lono in respect to color, in other words, is not necessarily clear-cut. We might add that given that Hawaiian *akua* tended to be transcendent, appearing in various forms (Sahlins 1995:196; Malo 1951:83; Valeri 1985:325, 327), many Hawaiians were presumably uncertain as to Cook's relation to Lono no matter what color he was wrapped in. Nor is the ritual involving Cook at Hikiau necessarily that clear in respect to Cook's association with Lono. The concluding rite, the Hānaipū, is definitely associated with Lono (see Sahlins 1995:55–58), but what about the rites preceding it? They "most probably" involved, Valeri says, "an *ad hoc* creation that combines the crucial rite in the cult of Kū with the crucial rite in the cult of Lono" (1994: 129). Nor, as Kane (1994:19) points out, did chiefs prostrate themselves before Cook in the *kapu moe* position; only commoners did. In their gift exchanges, there was often a sense of equality (see Beaglehole 1967:513, 517–18). A careful reading of the documentary material suggests, then, that ambiguity exists concerning who believed what about Cook during which period of the British stay (cf. Obeyesekere 1992:65 and Sahlins 1995:65, 66, 279). The real problem here lies not with the data, I would suggest, but with our efforts to make sense of the data, with our conception of Hawaiian conceptions—with our believing that Hawaiians possessed some consistent, collective "group mentality" regarding Cook.

But if not everyone seemingly concurred on Cook's status, we need to ask who, at Kealakekua Bay, most people would have turned to—or felt obliged to defer to—in respect to such matters. If a belief in the *akua* Lono existed among Hawaiians—and neither Obeyesekere nor Sahlins has ever suggested anything to the contrary—then who had the authority to specify Cook's relation to this *akua*? The documentary material makes clear that the *priests* of Lono at Kealakekua Bay (e.g., Kanekoa, Kuakahela, Ka'ō'ō, Keli'ikea, and Omeah), because they were the priests of *Lono*, had this authority. They were, as Sahlins notes, Lono's "legitimate prophets" (1985:122). But we would add that, given the oppositions that clearly separated chiefs from priests (see, e.g., Beaglehole 1967:510, 543, 560; Cook and King 1784, vol. 3:69; Sahlins 1995:80, 256–63; cf. Obeyesekere 1992:171), apparently not all Hawaiians accepted these priests' authority all the time.

One other point seems clear. We know that these

same Lono priests continually supported the British, both during and *after* the Makahiki at Kealakekua Bay. From their repeatedly providing food (Beaglehole 1967: 510; Cook and King 1784, vol. 3:14–15) to their returning a piece of Cook's "hind parts" (Beaglehole 1967: 560; cf. Sahlins 1995:68), the British noted "the very extraordinary marks of attention & disinterest'd proofs that the fraternity of Priesthood had paid the Captain & we who liv'd on shore" (1967:560, 509). It was these same priests, moreover, who continually reinforced Cook's association with Lono: "whenever Captain Cook came on shore, he was attended by one of these priests, who went before him, giving notice that the *Orono* had landed, and ordering the people to prostrate themselves" (Cook and King 1784, vol. 3:14). The documentary material indicates that not everyone was so deferential or so loyal: "We had not always so much reason to be satisfied with the conduct of the warrior chiefs . . . as with that of the priests. In all our dealing with the former, we found them sufficiently attentive to their own interests" (Cook and King 1784, vol. 3:15).

The controversy thus revolves around who among the powers-that-be had the power to objectify their interpretations of Cook (see Sahlins 1985:121–22; 1995:65). With the onset of the Makahiki—especially if we follow both Obeyesekere and Sahlins and assume that there was some flexibility in its scheduling (Obeyesekere 1992:99; Sahlins 1995:32–33, 220–22)—we might assume that the Lono priests were at the relative apex of their power for the year. Many others deferred to their interpretations. After the Makahiki, during Cook's second stay at Kealakekua Bay, it was a more open matter. This would explain the varied attitudes toward Cook on February 13 and, especially, February 14. Cook's status at this time was an open question for negotiation not between the British and the Hawaiians (though that clearly went on) but between the priests of Lono at Kealakekua Bay and other Hawaiians. The controversy thus hinges not on Western versus Hawaiian conceptualizations of Cook but on different Hawaiian conceptualizations of Cook. The British (and their mythology and/or rationality) had relatively little to do with it. Only a sense of European self-importance would suggest that Hawaiians were the supporting characters in a British play rather than that the British were the supporting characters in a Hawaiian play at the Bay in 1778–79. (For accounts of how the dynamics of this Hawaiian drama unfolded in ensuing decades see, e.g., Sahlins 1995:85–116, 134, 256–63; Valeri 1982.)

And yet, it is only fair to say, the British did have a part to play in this Hawaiian drama. They selected who among the British received deference from Hawaiians. When a Hawaiian chief (on December 1, 1778) came on board the *Discovery* looking for "our Arrona" (see Edgar's journal of this date), he, unbeknownst to himself, got the wrong ship; Cook captured the *Resolution*. Similarly, King and Bayly (Beaglehole 1967:504–6) were at the Hikiau ceremony, but neither of them reported being the focus of Hawaiian attention. When Clerke

was given various signs of respect/adoration (see Beaglehole 1967:1165), again, this did not upset the equation. From a British viewpoint, the British Lono had to be Cook; he stood at the top of the British social hierarchy.

In a way it might be said—though space does not really allow for an elaboration of this theme here—that the British had a drama of their own to play out. There seems little doubt, for example, that the British—with their weaponry, astronomical navigation, and ability to manufacture daggers prized by Hawaiians—viewed themselves as technologically superior to Hawaiians. We see, on the colonial periphery, an important result of the industrial revolution: it gave Europeans a sense of intellectual superiority over others (cf. Adas 1989:7, 134–98). One might also note the British use of outward mobility—movement to the colonial fringe—as a means for upward mobility. Cook’s explorations, Smith notes, “provided the material . . . [for] a new kind of hero. . . . Cook is the self-made man. While hidden among the obscurity of the vulgar, he . . . raised himself above his station in life by assiduous application” (1992:225, 228). The British, in brief, were also involved in a play—regarding deference, technology, and social mobility (see, e.g., Dening 1992, Borofsky n.d.). But such upward mobility had its limits and, most certainly, in the late 18th century clearly stopped short of the divine, even when it was framed in terms of those on the colonial fringe.<sup>6</sup> The closest one probably ever got—in the very best of circumstances—would have been the “demi-god or hero and distinguished title” status given to Cook in *Omai* (see Dening 1986:114; cf. Obeyesekere 1992:129) or given to Nelson after his death. More would have been offensive to most British sensibilities.

## Deciding Who Can Speak for Whom

This brings us to one of the central, as well as one of the most problematic, aspects of the controversy: the way various people (including myself) claim insight into what earlier Hawaiians and Europeans thought. How can others evaluate such claims? What does a person need to know—to experience or learn—in order to possess such authority? Obeyesekere feels he can understand earlier Hawaiians through an intensive examination of the ethnohistorical sources as well as through his Sri Lankan experiences (e.g., 1992:xiv, 8–9, 21). Sahlins feels he can understand them from more than two decades of ethnohistorical investigation (see Kirch and Sahlins 1992:ix and references under “Sahlins” in 1995:299).

Such claims are not isolated, academic assertions. They occur within a broader context that complicates

their clarity. The right of Western anthropologists to translate or speak for others (in Evans-Pritchard’s [1962:148–49] sense) is very much under attack (see Asad 1986). Standing at the margins of one culture and speaking for those across the borderlands of difference in another—speaking, that is to say, for those who are deemed “different” from “us”—is not the politically innocent experience it was perhaps once considered to be.<sup>7</sup> A host of indigenous scholars now standing at these borderlands find an anthropological presence intrusive. Western anthropologists compete for the authority that these scholars would claim as spokespeople for others. Such is the case in Hawaii. Anthropological authority in matters Hawaiian is often viewed with suspicion in the archipelago, particularly by Hawaiian activists (see, e.g., Trask 1991, Kame’eleihiwa 1995).

The issue is further complicated by the fact that the intellectual authority to make statements regarding who can speak for whom across these borderlands is often based on criteria that explicitly or implicitly exclude others. It involves less a discussion among claimants than a setting up of barriers. Thus, for example, Obeyesekere implies that his experiences as a Sri Lankan provide him with insights into Hawaiian behaviors that Sahlins lacks (1992:9, 21), and he suggests that key portions of Sahlins’s argument cannot necessarily be taken seriously because Sahlins does not really consider text/context relations (pp. 67–73). Sahlins accuses Obeyesekere of misrepresenting historical documentation to the point that he “systematically eliminates Hawaiians from their own history” (1995:116; cf. pp. 117, 193, 233). Both would speak for Hawaiians, and each would generally dismiss the other’s claims to do so (for Obeyesekere, see, e.g., 1992:21, 66–67, 155). As for Hawaiians, Kame’eleihiwa asserts, “Natives have often wished that white people would study their own ancestors . . . instead of us, whom they generally misunderstand and thus misrepresent” (1994:112; cf. Trask 1993:161–78). And Kane sees anthropologists as mostly talking *about* rather than with Hawaiians. He notes that they “seem to be in a system which rewards ability to spout current fad theory” (personal communication, 1996). He suggests that Sahlins’s (1995) book might be better entitled *How Anthropologists Think: About Polynesians, For Example*.

Such barriers mean that little sharing, little conversation, takes place across the borderlands of difference. This is a shame, because there are serious issues to be addressed here. There is the question of to what degree being born and raised in a locale leads to effective knowledge of that locale’s history. Reading references such as Denham (1912:259), da Silva Cosme (1990:279–80), and Abeyasinghe (1966:76–77) suggests ambiguities

6. In the late 1700s, Britain’s class barriers opened slightly but remained closed to most from below (see, e.g., Colley 1992:191; Stone 1984), and, as E. P. Thompson (1963:177) notes, barriers increased significantly following the French Revolution.

7. A host of people use the term “borderlands” in overlapping but slightly different ways (see, e.g., Anzaldúa 1987, Rosaldo 1989, Thelen 1992, Alvarez 1995, Gutiérrez-Jones 1995). I use it here in a general sense, involving the ambiguous zone of transition between differences deemed important, a zone where hybridity flourishes (see Bhabha 1994, Gilroy 1993) and a zone where those who police border crossings by others assume much authority.

in Obeyesekere's assertion regarding Sri Lankans' not treating Europeans as gods (see 1992:8–9). There is also the question of whether one should apply the same standards of morality to Polynesians as to Europeans. In discussing "terror" (pp. xv–xvi), Obeyesekere, for instance, is fairly critical of British violence toward Hawaiians (e.g., p. 41) but seems to downplay similar "terror" espoused by Hawaiian chiefs (see, e.g., Beaglehole 1967:589; cf. pp. 29–32 in relation to Beaglehole 1967:101). There is, moreover, the intriguing situation of various Hawaiian scholars' siding generally with Obeyesekere against Sahlins (e.g., Kame'eleihiwa 1994, Kane 1994) but siding with Sahlins in relation to key specifics against Obeyesekere (see, e.g., Kame'eleihiwa 1994:116; Kane 1994:20). Kame'eleihiwa conveys both in print (1994:117) and in personal conversation that Obeyesekere's perspective is acceptable to her as long as he attacks Sahlins but that he needs to tread more carefully—and with more knowledge—if he is going to probe further into Hawaiian history and challenge other people's interpretations.

Furthermore, without dialogue we lose the ability to evaluate scholarly works. In the social sciences and especially in anthropology (where few can "check" an ethnographer's observations), intellectual authority tends to reside not in scholarly assertions but in the interactions of scholars with their audiences through time. It is something that gradually gets established through collective conversations, through "comparison, evaluation, and debate" (Kuper 1989:455). Without such interaction, we can only whistle in the dark, trusting our own impressions as to what is (and is not) credible. We have seen this in respect to the reviews of Obeyesekere's *Apotheosis*; reviewers responded in terms of what made sense to them. Just as critical, the loss of conversation means that we rarely learn from our differences. We become frozen into positions and less able to move beyond the complacency of our own constructions toward increased knowledge (Borofsky 1987:155). Scholarship then often becomes a matter of political positioning, of power and control. What can result can be seen in the case of *Anahulu* (Kirch and Sahlins 1992), one of the most sophisticated anthropological works ever produced in the Pacific and arguably one of the best anthropological works produced anywhere in the past decade. By suggesting that Hawaiian chiefs as much as foreign *haoles* were responsible for the immiseration of the *maka'ainana* (commoners) in the early 19th century, Kirch and Sahlins have run afoul of certain Hawaiian activists. (They have stepped rather hard on Kame'eleihiwa's political toes, for example, in documenting this point.) Sahlins is now deemed to "misunderstand" Hawaiian culture (Kame'eleihiwa 1995:16). Before *Anahulu*, Kame'eleihiwa perceived Sahlins in much more positive terms (1995:16); not now (see also Kame'eleihiwa 1994). The irony—tragedy would perhaps be a better word—is that as Obeyesekere is being drawn into the meat grinder of Sahlins's determined scholarship—what Hacking calls Sahlins's "revenge"—Sahlins is being drawn into the meat grinder

of Hawaiian political correctness—where one might perceive Obeyesekere as having his "revenge." Where will it end?

It might end with conversations with one another across our differences to build common points of reference. What the Obeyesekere-Sahlins controversy (and the larger controversy regarding who can speak for whom) emphasizes is the need for common points of reference regarding how we might (or might not) judge the credibility of scholarly assertions about the Hawaiian past—or, for that matter, about any other past (or present). What are the common points of reference that unite us in evaluating divergent knowledge claims? What seems clear is that excluding others' accounts as less than credible on the basis of one's own self-appointed criteria, one's own self-appointed authority, shuts down meaningful conversation. In claiming to speak for others we seem mostly to speak to and *for* ourselves (Kane's point).

I am not dismissing the pervasive presence of politics and power in intellectual controversies, but I am saying that controversies such as this one cannot only be a matter of politics, because this destroys our ability, through shared, open conversation, to learn from our differences, to move beyond appearances of the moment to deeper understanding. The "real" issue behind this controversy for anthropologists, I would suggest, is not how deep cultural differences go—with Sahlins stressing difference, Obeyesekere cross-cultural similarities (cf. Geertz 1995:5). It is, rather, to what degree we (and others) can converse across our differences, whatever they are, however deep they go.

The common points of reference are there to be built on. I have suggested that we can make considerable headway in clarifying the present controversy by focusing on three critical points: consistency of argumentation, comparison of particular assertions with material printed elsewhere, and attention to events people generally agree occurred (such as the Lono priests' consistent support of the British). Though fairly familiar with the unpublished logs of Cook's visit to Hawaii, I have purposely avoided citing them here (except in the case of Edgar [n.d.]) so that others—reading this forum—can readily check the material as well. As the controversy continues (and it seems bound to continue), we might ask how we might expand our common points of reference so that we can effectively evaluate—together—the credibility of conflicting claims about Cook's visit.

We are left with questions for *both* Obeyesekere and Sahlins (as well as, more generally, for anthropology): What should our shared points of reference be? How can we best check on each other's assertions? In what ways might we encourage further conversation? And for all of us—as we try to make sense of a more globalized, transcultural world—there are critical questions as well. What is anthropology's role now that indigenous scholars from Hawaii to Haiti challenge the right of Western anthropologists to act as translators and spokespeople across the borderlands of difference? (Is there room for all of us at the borderlands?) How shall



we judge the credibility of our own and others' constructions in such an environment? How shall we converse in ways that encourage shared learning?

## Comments

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In a superb summary and analysis of conflicting views, Borofsky demonstrates that the Cook story has become a Rorschach test, perceived according to the viewer's cultural programming. How Sahlins and Obeyesekere differ in their interpretations of the events of Cook in Hawai'i is made clear. Borofsky also comments on views expressed by Hawaiians today and pleads eloquently for conversations across our differences which focus on common points of reference such as "events people generally agree occurred."

We have eyewitness accounts by Cook's men; unable to comprehend all they saw, they were nevertheless good enough to write it down. In their view, Hawaiians saw Cook as someone of special status and killed him when he attempted to take their king hostage against the return of a stolen boat. Later writings decline in credibility with the passage of time—the fluffed-up official publication of the voyage, the tales told to European visitors by Hawaiians adept at anticipating what their visitors wanted to hear, and accounts collected more than 50 years later and rewritten by Anglophobic American missionaries. All must be interpreted within the context of the European vision of the writers and their motives.

We also have cultural facts, known premises and traits of Hawaiian culture, some of which may be corroborated by their distribution throughout Polynesia. These, in the absence of credible Hawaiian accounts of Cook's visit, may help us view events through Hawaiian eyes.

One cultural fact is the absence in the Polynesian language of equivalents for such Western religious terms as "divine," "god," "adoration," "holy," "sacrifice," "supernatural," and "religion." As used by Cook's men and by some anthropologists today, such terms misinterpret Polynesian thought.

Polynesians of 1778, having no vision of the supernatural as a *separate* sphere from the natural universe, could not have seen Cook as a "god." To Westerners, "god" means a supernatural being. An *akua* is a being of nature, one of immense power, which may be an invisible spirit or a living person. Of the Marquesans, Handy (1923:244) succinctly wrote: "The native does not distinguish supernatural and natural, as we do. *Atua* were simply beings with powers and qualities of the same kind as those of living men (*enata*), but

greater. Some men and women were *atua* in this life; most became *atua* after death."

By interpreting Hawaiian thought in Western religious terms, anthropologists step on semantic land mines. Cook's men may be excused for their religious vocabulary, but social anthropologists may not be excused for perpetuating it as a scientific lexicon. The challenge to anthropologists is to develop a glossary of Polynesian terms for which no precise European equivalents exist and use those terms in their writings.

Take "adoration," as voiced by Cook's Lieutenant James King: In Polynesia authority was based on seniority. Authority/seniority hierarchies extended beyond the living, progressing in power as in seniority to the original creative spirits of Nature, the ultimate sources of the *mana* which motivated everything in the universe, whether it be the talents of a man, the growth of a plant, or the forces that moved winds, waves, and stars. Chiefly families derived authority from a special form of seniority—genealogies aligning them to the major *akua*. The relationship is evident in the terms for parent (*makua*), ancestral spirit guardian (*aumakua*), and *akua*. What Cook's men perceived as "adoration" and "worship" may have been expressions of the compliant, filial respect of the junior (or commoners) toward the authority of the senior (or chiefs). Throughout Polynesia, commoners treated European officers as chiefs; however, chiefs addressed the same officers as equals.

How should we interpret the lavish hospitality given to Cook at Kealakekua Bay by the local priests, apparently without request for payment? Very likely it was Polynesian gift giving, with the recipient considered to be honor-bound to reciprocate in some way (there was no Polynesian term for "trade" or "merchant"). It is doubtful that the priests could have been so generous on their own authority. Kalaniopu'u, as king and ultimate decision maker, must have seen Cook's arrival as fraught with marvelous opportunities as well as unknown political dangers. In awarding Cook a prestigious title (*rono*) and showering him with gifts, the king would have believed that Cook would be bound to reciprocate with some gift of service.

Having no writing by Cook's hand of the events at Kealakekua Bay, we can only speculate about what Kalaniopu'u wanted. It may have been a sharing of the visitors' wondrous technology with their hosts. Obeyesekere's suggestion that the lavish hospitality was to oblige Cook to reciprocate with military assistance against Maui is supported by a historical fact: the proposal most frequently made by Pacific island chiefs to European captains was one of joint military adventure. From the beginning of European exploration of the Pacific, when Magellan fell for such an invitation and got himself killed, to the proposals made by Kamehameha to Vancouver, it was common for an island chief to propose that his guest bring along his muskets and cannon and join him in a lovely little war against the chief of a nearby district or island. The hospitality at Kealakekua Bay was the classic set-up.

Historians have passed over Kalaniopu'u, seen

through British eyes as old and ineffectual, but S. M. Kamakau (1961: chap. 7) describes him as a “clever” and ruthless chief who had seized Hawai‘i by force and preserved it by political intrigue and prowess on the battlefield. A conquest of Maui was his great obsession. There had been four campaigns, each a disaster, each increasing his rage and frustration. Now in old age he got the news—the British were coming, hungry and without women. Word had come from Kauai of their weapons. They had met his enemy, Kahekili, a few days earlier, but he could offer them a deal that the Maui king could never match; Maui’s harvest was wasted by war, but Hawai‘i’s bounty was at hand, and in his hands.

Before negotiations were possible, Cook had to be made visible as an entity within the known Hawaiian world; hence the chiefly title *rono*—one which conferred great prestige within the domain of the benevolent *akua* Rono but apparently little if any power to threaten the king’s paramount position within the realm of the *akua* Ku, patron of politics and warfare. At Kealakekua Cook’s men met a resident high chief named Omeeah, who was also called *rono* and who received the same acts of respect given Cook, including prostration before him by the commoners; yet Omeeah, as *rono*, was no threat to the king.

James King was clearly informed of the distinction between Rono as an invisible *akua* and as a chiefly title: “Sometimes they applied it [*rono*] to an invisible being, who, they said, lived in the heavens. We also found that it was a title belonging to a personage of great rank and power in the island” (King and Douglas 1784:5). This is a distinction I find blurred in Sahlins’s writing. That a living man holding the title *rono* may have been regarded to some degree as a manifestation of the great *akua* Rono does not imply that Hawaiians confused the man with the “god.” Similarly, some Roman Catholics may regard the pope as a manifestation of Christ, but no Catholic regards the pope as Christ.

Moreover, in a society driven by precedent, Hawaiians had no precedent within the annals of the chiefs for Rono or any of the primary *akua*’s walking upon the earth in human form.

Missionaries translating the Bible into Polynesian dialects had difficulty finding equivalents for their religious terms. Searching Hawaiian for a word for “holy,” missionaries settled for *hemolele*, “perfect,” and put their own spin on it. There being no Polynesian word for religion, one had to be invented. *Haahi* is a Maori rendition of the English “faith.” *Ka ho‘omana* became the Hawaiian term.

Is it possible to stop projecting? So long as we ignore the realities of language and the premises by which a people shape their conclusions about their world, we tend to perceive everything in our own terms, and empathy is impossible. Much of Hawaiian history, written by foreigners with Western bias and personal ambitions, lacks empathy. As Barbara Tuchman observed, the difficulty of empathy is the supreme obstacle for the historian. A Chinese painter once said to me, “To paint a tiger one must be a tiger, to paint a flower one must be

a flower.” As an artist, I find that a depiction of an event in Pacific history requires more than painstaking accuracy of setting and details; one must also try to see the world of the participants *through their eyes*; otherwise human figures will seem lifeless on the canvas or, as we often see in Hollywood historical spectaculars, as modern people in period dress.

Polynesian and European premises, logic, and conclusions about the universe and humanity’s station in it were worlds apart, but with empathy founded upon known facts of both cultures we may improve our grasp of the events of Cook’s visit and his death.

After European contact was renewed, some Hawaiians, finding visitors fascinated by the subject of Cook’s death, were quick to confirm European notions about it, but among the chiefs, as well as early Hawaiian writers Kelou Kamakau, David Malo, and John Papa I‘i, there seems to have been an amnesia about Cook. Sixty years after Cook’s death, Hawaiian accounts not unkind to Cook were disregarded by American missionary writers. Seeking to discredit the British, they put their own spin on the Cook-as-Lono myth, condemning Cook for self-deification—a blasphemy by which he supposedly incurred the wrath of the Almighty and brought about his own death. Bingham (1847:75) described Cook as “contemptible” and as a “worm” guilty of “disgusting lewdness.” The missionary history text (Dibble 1838) taught to generations of Hawaiian children and parroted by later Hawaiian writers created a feeling of revulsion toward Cook (Stokes 1930:100) that persists today.

*Lono* (formerly *rono*) has several meanings. It can mean news or announcement and, with the prefix *ho‘o* (*ho‘orono*), a warning to be attentive. *Ho‘orono* may have been what the British heard when Cook landed and was led to the temple by a herald crying what sounded like *orono*, for it was the command by which commoners were warned of the approach of a chief of the highest rank, requiring them to prostrate themselves as an act of deference (Mary Kawena Pukui, personal communication). The British had previously witnessed this act, *kapu moe*, on Kauai, when commoners prudently prostrated themselves before Cook and, later, before the high chief Kaneoneo. Clerke, captain of the *Discovery*, was given the same honor but declined it. Commoners referring to Cook as *orono* during the circuit of the island may actually have been saying *ho‘orono* to caution others of the British chief’s presence.

*Lono* is also the name of one of the four major Polynesian male spirits, patron spirit of fertility and healing, manifested in the clouds, thunder, and rain that renewed the fertility of the land each year. *Lono* has been the name of famous chiefs and today is a Hawaiian surname as well as a given name.

Two stories told to Westerners in the 1820s gave rise to the idea that Hawaiians were awaiting the promised return of *Lono* and saw Cook as their *Lono*. In one, *Lono* was a king of Hawai‘i of the “fabulous age” who became offended with his wife, Kaikilani, and killed her. Driven mad by remorse, he went about boxing everyone he met and then sailed away, alone in a canoe, promising some

day to return. He was deified as Lono-i-ka-makahiki, and sporting events were instituted as part of the Makahiki in his honor. When Cook arrived, "it was supposed . . . that the god Rono was returned" (Ellis 1963: 85–86). Ellis, confused, begins a tale of a king and ends it as one about a god.

In another version, Lono is the major "god" Lono who comes down to earth and takes human form to wed Kai-kilani. The rest of the tale is the same as the first (Thrum 1907:108–16).

These tales are variously interpreted by both Obeyesekere and Sahlins, but both versions are fragments of a larger story which, unaccountably, neither author considers. The full account of the Lono story, along with a traditional chant, is found in S. M. Kamakau (1961:47–63):

Lono was a ruling chief of Hawai'i, not one of the "fabulous age" but one who lived nine generations before Kamehameha. He murdered his wife, Kaikilani, in a jealous rage. Mad with remorse, he went about boxing all comers and then sailed away.

But the story continues: He eventually regained his sanity, returned to Hawai'i, and resumed his kingship. He defeated an invasion from Maui in a great battle. He instituted sporting events as part of the Makahiki celebrations and after death was named Lono-i-kamakahi. A complex of rock walls and platforms at Keauhou, Kona, cleared in the early 1970s for construction of a hotel, was reputed to have been one of his residences. His bones were reputed to be in a sennit reliquary recently stolen from Bishop Museum.

Hawaiians of 1778 were not awaiting the return of Lono; he had returned centuries earlier.

Addressing the disparity of interpretations, Borofsky correctly points out, "The real problem here lies not with the data . . . but with our efforts to make sense of the data, with our conception of Hawaiian conceptions." Our conception of Hawaiian conceptions can be improved, I submit, by interpreting the data through what is known about the Polynesians' view of their world, reconstructing that unique lens with cultural and historical facts.

As one result of the worldwide surge of nationalism and cultural revival among peoples formerly dominated by foreign powers, the question of who can speak for whom, Borofsky observes, has become a barrier across the borderlands of difference which challenges "the right of Western anthropologists to translate or speak for others." There is no doubt that in the mood of the times, the popular view of anthropologists making forays outward from the civilized center to study the exotic cultures of others (presumably less "civilized") is one which lumps anthropologists with missionaries, fortune hunters, and snake-oil salesmen and may arouse resentment. Moreover, all peoples, Westerners included, continually reinvent their history and culture within the context of their times. In the search for a glorious past to buttress self-esteem, unpleasantness is too often obscured with romantic illusion, past horrors locked away in closets of amnesia, and past outrages

nursed to feed present hatred. All peoples feel proprietary about their own histories and resent outsiders who, uninvited, would search secret closets and strip away veils of cherished tradition, despite the possibility that such veils may obscure historic truth.

A more positive result of the new nationalism is an emergence of indigenous scholars. As these gain expertise and authority, the tradition of Western outsiders conversing with indigenous informants may become obsolete. What Borofsky perceives as a barrier may become a borderland of a different kind, a threshold from anthropology's past to a future in which useful conversations occur between anthropologists representing their own cultures. Such a trend need not result in "balkanization" if anthropologists can find common points of reference in conference with others.

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Marlow in *Heart of Darkness* opens Kurtz's testament thus: "He began with the argument that we whites, from the point of development we had arrived at, 'must necessarily appear to them [savages] in the nature of supernatural beings—we approach them with the might as of a deity,' and so on and so on" (Conrad 1968: 328). I hold that *The Apotheosis of Captain Cook* is primarily about the European Enlightenment and its self-imaginings. "Polynesia" itself is a European invention, and Polynesian places were filtered through Eurocentric prisms. Cook, in my account, was the exemplar of the Enlightenment, but he also reflects a deeper myth model of the long run that Kurtz also imagined. I show how this latter "myth model" was constituted and contrast it with the terrorism that the voyages initiated. Borofsky thinks I should also have mentioned Polynesian terrorism. I don't think Polynesian chiefs can be considered terrorists; also, such issues were irrelevant to my thesis.

I agree with Borofsky that my whole argument is doomed if it can be shown that the apotheosis is not a Western myth model. For Borofsky such apotheoses "would have been offensive to most British sensibilities"; they "stopped short of the divine, even when it was framed in terms of those on the colonial fringe." Apparently, the only real evidence I have for the European apotheosis is Wallis's sailors, who, even before they landed in Tahiti in 1766, thought (in one sentence) that they were "demigods" to Tahitians. But Borofsky does not tell us why that one sentence should be dismissed out of hand. He also neglects other evidence I submitted: from cartoons, novels, school texts. Then there is that marvellous "eyewitness" Zimmermann, who said, "[Hawaiians] raised Captain Cook to the dignity of a god, and set up an idol in his honour which they called after him, 'O-runa no te tuti,' 'O-runa' meaning god, and 'tuti' Cook. This idol was made exactly like the others, but was adorned with white feath-



ers instead of red, presumably because Captain Cook, being a European, had a white complexion" (Zimmermann 1926:36). Now, Borofsky might want to agree with Sahlins and say that Zimmermann heard it right, proving that Cook was apotheosized by the Hawaiians. But these statements are extremely opaque: Zimmermann probably heard what Hawaiians often reiterated, namely, that Cook was called Lono. But that Lono meant "god" was his invention, and the image in white "exactly" like the others was the Hawaiians' Lono rather than Zimmermann's; it was Lono's color, not Cook's. One can imagine how easily ordinary seamen could have placed the momentous events of a few days in the frame of their own myths. On the next page Zimmermann adds, "The death of our quartermaster destroyed their previous belief in our immortality, and, this belief being lost, their reverence for us was gone" (1926:37). Thus, according to Zimmermann, Hawaiians believed that all Europeans were immortal (read "divine"), a tradition reflective of the discourse of ordinary sailors in Wallis's expedition. Borofsky goes on to ask: If Cook as god was a European invention, then why was he not represented thus in other Polynesian cultures? The answer is simple: The rituals of prostration that Europeans interpreted as "worship" occurred only in Hawaii, and Cook was not given the name "Lono" elsewhere. Borofsky neglects to mention that the ships' major journalists made no reference to Cook's apotheosis; clearer hints appeared in the *official* edition of the voyages, which, I suggested, was published in a European context in which the apotheosis of the redoubtable civilizer was already taken for granted. Further, apotheosizing the civilizer is not the dominant or the only response that Europeans attributed to savages. The more common language of Otherness is found in the discourses on cannibalism during these voyages (Obeyesekere 1995*b*, 1996).

Two important papers on "attributions of divinity" and "imagined apotheoses" by the historian William Hamlin, the first written before he was familiar with the anthropological debate (Hamlin 1994, 1996), demonstrate that "the motif of Europeans perceived as gods—a common feature of Renaissance discovery narratives—appears to work differently in different kinds of ethnographic documents" and that virtually every Renaissance explorer or traveler to savage lands claimed to have had divine status conferred on him by natives (1994:418). Let me give the reader a feel for this material: here is Jacques Cartier on Canadian Indians, for whom "God was descended and come downe from heaven to heale them." Drake reports that when he bestowed goods on the Miwok, of whose language he was ignorant, "they supposed us to be gods, and would not be perswaded to the contrary." André Thevet claims that when wild men first saw Christians they "esteemed them as Prophets and honored them as Goddesses." And Captain John Smith asserts that the Susquehanna were "with much adoe restrained from adoring us as gods," the word "adoring" echoing the lan-

guage used in Cook's voyages (1994:430–31 n. 36). Hamlin's extensive documentation shows that Renaissance thinking was *saturated* with this myth model, both literally and as an extended trope, with the result that "European voyagers habitually perceived, interpreted, and represented these peoples within the confines of a thought universe so densely configured that scarcely any space remained for imagining alternate realities" (1996:424). He also shows that some educated sea captains tried to persuade natives that they were not gods, but he rightly says that these denials, like Cower's protestations, only affirm the power of the underlying myth model. Borofsky seems to favor the position of Conrad's Kurtz: that the apotheoses represent an empirical reality such that everywhere Europeans went (in Sri Lanka, for example) they were in fact treated as gods by natives. How does Borofsky reconcile Hamlin's two papers and the Zimmermann data with his position?

I am surprised that Borofsky places such inordinate emphasis on my tongue-in-cheek reference to Cook as a crook and my "puzzling" aside on "Italians and other Third World peoples." These references were not meant to be "precise scholarship," which Borofsky himself admits is problematic in argumentative disciplines such as ethnography.<sup>1</sup> I must recommend that he reread that part of the text as "fun" or *jouissance*. It contains my alliterative predilections (crook-cook), the joking reference to Banfield's outrageous book about Italians, and my facetious comment on white-collar crime in asking my readers to forgive Cook's crookery because "what upright citizen has not fudged his income tax returns?" (1992:14). Borofsky may not like my strategy, but I introduce irony, sarcasm, satire, bad jokes, and occasional vulgarity into the heart of the ethnography, juxtaposing (if I may now call on T. S. Eliot) levity and seriousness. The "revenge" that Borofsky refers to is alien to my stylistic mode.

Space does not permit a detailed response to Borofsky's important section "Reexploring the Documentary Data," but I think its methodological flaws can easily be exposed. First: having paid lip service to flexibility in interpretation, he can make such statements as "the documentary sources make clear" or "one other point seems clear" about the friendly role of the Lono priests. My question: Can documents produced by sailors in the late 18th century after a short spell in a tense situation be that unequivocal? What about the priest Omiah, who was also called Lono? The British thought he was as sacred as the king, and they suspected him of being hostile to them, so much so that they regretted not having captured him (Obeyesekere 1992:109). Borofsky's problem is also Sahlins's: he fits the diverse, often contradictory information into the straitjacket of a single interpretation. He cannot imagine Hawaiian priests' having debates among themselves and taking sides vis-à-vis the foreigner in their midst. Further, how can he maintain

1. I have addressed this issue at length (Obeyesekere 1990:225–44, 269–84).

that the present cause célèbre (or cerebral cause) “hinges on different Hawaiian conceptualizations of Cook” and that British myth making and rationality had little to do with it when all the information that we have for this period comes from *British* sources? Hence my third question: Given that hermeneutical scholars such as Gadamer think that there is no way even modern scholars can escape a “fusion of horizons,” how is it that British journalists writing in the 18th century escaped that predilection and emerged relatively “prejudice”-free?

My final critique of this section: Borofsky performs the methodologically impossible feat of “reexploring” *documentary* data with only the barest reference to “documents”! His strange rationale is that these documents are readily available to readers of CA when he ought to know that most of them are located in archives. Further, outside of a few references to Cook’s journal, Borofsky overwhelmingly relies for his reconstruction of the Hawaiian past on *secondary* interpretations by Sahlins and Valeri (with a little Beaglehole added), which are already saturated with objectivized interpretations. Hence my fourth question: How can he justify this special methodological procedure of relying primarily on Sahlins and Valeri in attempting to show that Sahlins and Valeri have it right? He also wants to impress on the reader that Sahlins and Valeri present a unified front, ignoring contradictions in their relative positionings, as for example when Valeri says that “probably there was nothing preordained about Cook’s identity” and that “performance of rituals [at Hikiāu] was an attempt to orient and fix this identity in a direction favorable to the Hawaiians” (1994:130). Not exactly mythopraxis, is it?

I am quite prepared to accept factual and bibliographical errors in *The Apotheosis* and elsewhere in my writings, though I have not checked Borofsky’s specific criticisms. To me such errors are normal, mostly innocuous, and eventually correctable. (Until I pointed it out, a mistaken date of 1980 for “Sahlins’s major statement” [1981] made it appear that a reference had been omitted from Borofsky’s list.) I do treat different parts of a journal differently, as he claims, but with cause. For example, on pp. 71–72 I doubted Rickman’s ability not only on linguistic grounds but also because of the discrepancy between his log and the written text, the latter written without the benefit of the former. Even poor journalists such as Rickman could sometimes be trusted with accounts of their own people, but not about Hawaiians. So with Kamakau; like all of us he could be sometimes right and sometimes wrong. Does Borofsky think that our judgments regarding texts should be an all-or-nothing game?

Borofsky himself can be wildly wrong, for instance, in arguing that “two decades of ethnohistorical investigation” can give integrity and authenticity to any account. Equally dubious is his bibliographical arithmetic—“Sahlins’s bibliography has 316 references, Obeyesekere’s 152,” conveniently ignoring (apropos of

“precise scholarship”) his own excursus into the Sri Lanka culture of apotheoses with 3 references.<sup>2</sup> I know plenty of colonial scholars who have worked in South Asia for decades and produced scholarly tomes with little empathetic understanding of the local culture. In this context, Borofsky’s apotheosis of Kirch and Sahlins’s *Anahulu* as “one of most sophisticated anthropological works ever produced in the Pacific” is at best methodologically irrelevant. I have not yet read this work, but if Jonathan Friedman’s judgment can be defended, *Anahulu* has little to do with the vitiating assumptions about native mentality implied in such terms as “stereotypic reproduction” and “mythopraxis” (1996:383–84). Different books, different rationalities.

Equally flawed is Borofsky’s defense of these terms by showing that Sahlins introduces some flexibility into their use. My response: There is no way in which “stereotypic reproduction” and “mythopraxis” could be applied to the ethnographic data without distortion, and those who use such rigid terms are eventually forced to qualify their usage. Let me state at the outset that I am not hostile to parallel terms located in other contexts: thus Freud used “repetition compulsion” as a generic neurotic propensity found among all human beings, not just primitives; so with Nietzsche’s ideas of an “eternal return.” Sahlins’s usage, however, is specific to the Hawaiians and others like them, deliberately excluding Europeans. Hence my next question to Borofsky: If “stereotypic reproduction, strictly speaking, . . . does not occur,” why use the term in the first place? What are the implications of using such terms to describe Hawaiian mentality?

Now to the unfortunate question of “who has the right to speak for whom”—a phrase that Borofsky lifts from the notorious blurb for Sahlins’s book by the University of Chicago Press and withdrawn after my protests. I have no answer to this question, which I think has been badly framed in terms of “rights.” I shall leave ethnographic gurus to speak on issues such as these. As I see it, anyone ought to be able to speak on the political issues that beset us today; thus Borofsky has a perfect right to talk about colonialism and ethnicity in Sri Lanka if he can present reasoned, evidentially supported arguments. But I would be appalled if he arrogated for himself the “right” to speak on behalf of Sri Lankans, whose voices are multiple and disparate. This must compel me to ask him yet another question: Where on earth have I said or implied that I “speak for Hawaiians” or that I have arrogated a “right” to speak on their behalf?

Let me put the record straight. I introduce my book with a fantasy: I hear Sahlins speak about Cook’s apotheosis, my mind wanders to my Sri Lankan experi-

2. The original documents are ignored in Borofsky’s reference to a Portuguese captain general who was accorded the treatment given to Sri Lankan kings or high chiefs, called *deviyo* or “god.” This is similar to the Hawaiian attitude to Cook; the reference also contains a wonderful joke, easily literalized.

ences, and these free associations make me think that the apotheosis is a European construction. This is exactly my strategy in *Medusa's Hair*, which also starts with the anthropologist's own self-deprecatory fantasy of castration but ends up by saying that "the essay itself is not a fantasy, since the original fantasy was mediated through my discipline and my critical faculties into its present form" (Obeyesekere 1981:192). Interpreting a text or writing an ethnography is not speaking *on behalf* of the people represented therein. I do believe, however, that I can bring some insights that are a product of my historical placement as a native ethnographer and a former colonized subject, but such personal insights must eventually be woven into the ethnographic narrative. I neither assert nor imply that these insights provide privileged access into Hawaiian culture. I would willingly recant such statements if Borofsky were to document them for me.

While I think anyone has a right to speak on issues that affect our times, Borofsky raises important questions regarding conversation across borders, however permeable such borders may be. Without pontificating on them, let me open the debate by affirming what I think ought *not* to prevail in any kind of reasoned conversation across academic, cultural, and political borders.

Conversation cannot be sustained through the language of revenge and the denigration of the Other which Kerry Howe (1996:116) has neatly spotlighted in Sahlins's work:

Obeyesekere's work is variously described (to give but a small alphabetical sample) as: absurd, anti-anthropological, blundering, blustering, careless, contradictory, defective, distorted, dubious, evasive, epistemic murk, falsely accusative, fictitious, highly righteous, helter-skelter, implausible, misrepresentational, pidgin-anthropology, quixotic, scattershot, slanderous, scholier-than-thou, solipsistically fallacious, stratified palimpsest of confusion and contradiction, symbolically violent, ventriloquism, willy-nilly, wrong, unaware, unhistorical. Then there are the dreaded (and I hope not obscene) acts of *petitio principii*, *suggestio falsi*, *suppressio veri*, and *ignoratio elenchi*.

The reader might also remember the University of Chicago Press blurb mentioned earlier, which says that Obeyesekere is guilty of "wholesale fabrications of Hawaiian ethnography." This kind of crude, dismissive language betrays a certain attitude to the Other which is hardly conducive to any form of conversation.

In this regard I am distressed by Borofsky's own attitude to the Hawaiian scholar Lilikala Kame'eleihiwa, who is purported to have told him that "Obeyesekere's perspective is acceptable to her as long as he attacks Sahlins"—thereby dismissing her as the irrational female and native. Borofsky isn't very charitable to his colleagues either, who apparently were unable to deal with my book because "the large number of citations to

unpublished and/or unfamiliar material can be intimidating to reviewers," who can thus be "lulled" and gulled into "accepting Obeyesekere's arguments on trust." His colleagues have also betrayed a tendency to "rush to review" and are guilty of "accepting current scholarly trends." This singularly patronizing stance can only inhibit the very conversations he wishes to initiate. Borofsky ought to credit his colleagues with critical judgment, particularly when virtually everyone who wrote about this debate was a Pacific historian or anthropologist. More pernicious still: having denied Pacific scholars the capacity to make critical judgments, he praises two big names, Clifford Geertz and Ian Hacking, who, in spite of their lack of familiarity with Pacific history and ethnography, were not in the least intimidated by arcane references and "primary documentation." Why this double standard?

However, I do not wish to make a Sporus of Borofsky, because it is the larger issue that interests me.<sup>3</sup> He says that scholarship "often becomes a matter of political positioning, of power and control." But how does he escape such things in *his* paper? And how did the British when they wrote their texts? And did Hawaiians escape the political realities of the British arrival by seeing Cook as the prophetic realization of their returning god? Developing Borofsky's thought, I would add that the ethnographer is not immune to his own culture's traditions containing its unstated assumptions or doxa. Thus, while Sahlins writes in the "revenge" mode, I too, wittingly and unwittingly, write in the "satiric mode of shaming" well established in Sri Lankan literature and culture. This obviously has offended Sahlins; nevertheless, I perversely think that expressing one's position in a satiric mode is a much more civil form of contestation. I may well be wrong, a victim of my own presuppositions. Yet, I want to remind the reader that, cynical though I am about Sahlins's work on Cook, I did acknowledge that he is "one of the most creative thinkers in our field" (1992:9). I might also add that colonized subjects like myself may wear their own peculiar kinds of blinders. So it is with Sahlins. The native idea of the divinity of chiefs (if such a view existed) has been assimilated by him, unwittingly, into the Eurocentric vision of the apotheosis of the white civilizer. When this happens on a grand scale, ethnography becomes a kind of doxography.

Borofsky's version of ethical relativism is, I believe, a hindrance to intercultural and other understandings. He seems to think that we should judge Cook in terms of the standards prevailing at that time. What standards? Enlightened Enlightenment thinkers did not subscribe to the everyday squalid standards prevalent in the period, for example, that it was acceptable for natives or ordinary seamen to be brutalized. How then can we apply Borofsky's criteria for ethical judgments and make any criticism of colonialism or imperialism or cannibalism or nazism or any other form of deadly

3. My (I hope) not so arcane reference is to Alexander Pope's "Epistle to Dr. Arbuthnot."



"ism"? I would go even further and say that in the context of the native Hawaiian political struggle, Borofsky's kind of ethics can easily be used to deny or ignore the dark side of the United States involvement in Hawaii's political demise in the late 19th century.

Finally, for me, a further hindrance to intercultural communication is the ethnographic notion of "the natives' point of view" developed by Malinowski. Because I have dealt with this issue in *The Work of Culture* (1990:220–25), I can only briefly restate it here. The native viewpoint is a fiction, an ethnographic device to represent the other culture, an ideal type, if you will. There is no "native point of view"; yet it is impossible to write an ethnography without such phrases as "how Hawaiians think" or "Sri Lankan thinking" or "according to my informants." This rhetorical strategy can be deadly if reified. Such a reification is exemplified in Valeri's account of Hawaiian "sacrifice": on the basis of later texts Valeri has reconstituted early Hawaiian kingship and sacrifice and confidently formulated how Hawaiians think "according to the classical rules," to use Sahlins's phrase. I find it utterly presumptuous that ethnographers should tell native peoples how they think or formulate their "classical rules" for them, without an iota of skepticism or tentativeness. My guess is that this kind of appropriation and arrogance, which refuses to acknowledge the invented nature of ethnography, has made it a "pariah discipline" in the eyes of native intellectuals in spite of our enormous contribution to the knowledge of small-scale societies and "subaltern" peoples hitherto debarred from history. While one cannot please everyone and while one should not appease lunatics and fanatics, whether natives or civilized, I generally think that reasoned, evidentially supported, and unpatronizing arguments are the best tactic to employ and one that some native intellectuals at least can respond to on their own terms.

How, then, can one escape the dilemma of writing a critical ethnography without giving offense to local sensibilities? Again without pontificating, let me note a few of my own "prejudices."

One way is to accept explicitly the convention-bound nature of the ethnographer's "natives' point of view"; another, which Borofsky and I both approve, is to emphasize tentativeness and indeterminacy in our writing; another is to represent native voices, even though such voices are inevitably framed in terms of authorial authority.

What about the difficult task of creating spaces wherein one can think of the Other in human terms? I am not in the least averse to imagining that both Hawaiians and middle-class Europeans possess practical rationality that impels them to "reflectively assess the implications of a problem in terms of practical criteria" (Obeyesekere 1992:19) or that both can engage in debates or contentious discourses, as we scholars also do (p. 21). Such a position can only enhance my argument for providing spaces for intercultural and other forms of communication. Here at last I can agree with Borofsky when he says that everywhere in Polynesia there is evi-

dence for practical rationality, but I am amazed that he reads Sahlins as also subscribing to this idea when the whole thrust of *How "Natives" Think* is in the opposite direction. "Practical rationality" in my usage also underlies Hawaiian interisland voyaging as described in Ben Finney's important book *Voyage of Rediscovery* (1994), which reduces the level of otherness and exoticism endemic to Hawaiian ethnography. Thus "Kahiki," which Sahlins thinks was a mythic land beyond the horizon, was also a real land (Tahiti) for Hawaiians (Finney 1994:317).

Making a different kind of move, I would advocate employing *other epistemologies* that might help question those we have inherited from the Enlightenment, perhaps even reversing some of our time-honoured presuppositions or even creating new forms of social theory apposite to a time when cultural and other boundaries have become permeable. For example, I think that the Buddhist notions of absence of "self," its convention-bound and fluctuating character, or the Buddhist theory of "conditioned genesis," which stipulates that the world is permeated with causality, might help problematize Western notions of selfhood and causal understanding. Thus instead of saying "different cultures, different epistemologies," I would encourage *cross-epistemological thinking* whereby ethnographers can experimentally use other epistemologies to interrogate European society and its forms of thought. If it is good for us to understand other cultures in terms of Western epistemologies, it might be a salutary move occasionally to reverse our strategies. For example, instead of relegating Hawaiian or native American epistemologies to the "new age religions" as some patronisingly do, I would use them to hold a mirror to the terrible times we live in.

I will conclude by spotlighting a troubling feature of our discipline as it prepares to position itself in the crosscurrents of the coming century. Ethnography poses an enormous paradox: there is virtually no one, from Boas onward, who really believes that natives are biologically different from Europeans. But this universalist assumption cannot be brought directly into our writing nowadays because it essentializes human beings, and "essentialism" is a dirty word. Instead we have moved in the reverse direction and have celebrated *difference*; thus each culture is different, and we have proclaimed a doctrine of cultural relativism to rationalize that difference. If the older positivists thought that one could erect *universal* institutions on the basis of a universal human nature, we have made a case for cultural *particularism* on the same basis. Thus we think that common human nature produces different cultures, reversing the positivist position. Some also assume that while cross-cultural differences exist, intracultural differences are minuscule and that this is especially true of isolated, small-scale societies and "islands of history." It is this kind of assumption (and I myself have been guilty of this) that has led anthropologists to formulate uniform modes of thinking ("the classical rules") of small-scale societies instead of positing multiple rationalities

within each culture. Further, given our common neurobiological nature, the possibility exists that different cultures might also display similarities, both substantive and structural. In Wittgensteinian language, I would say that there exist "family resemblances" in "forms of life" within and across cultures and that historians are correct when they show that phenomena such as sorcery, witchcraft, spirit possession, and similar exotica also existed in the history of the West. The very existence of the iron cage of Weberian rationality may compel people today to reinvent the magical garden in their fantasy lives in such things as the "psychic readings," [holy] spirit possessions, and UFO-type visions which have exploded into popularity recently. These similarities, historical and contemporary, blur the distinctions between Western and native cultures and in turn can result in the "humanization of Western man" (to turn Ernest Jones's phrase [1924:49] on its head).

Cultural relativism is I think the charter myth of American cultural anthropology, a naive credo even if idealistically motivated. It is not surprising therefore that ethnographers often forget that cultural relativism has had a long run in European thought and was wonderfully expressed as early as 1580 in Montaigne's "On Cannibalism." The long preoccupation with difference has also had a dark side in European thought, for example, when, following the voyages of discovery, medieval notions of the wild man were foisted on the native. I am suggesting that *both* reflect European attitudes to the Other in terms of difference, the one characteristic of humanistically motivated thinkers, the other a pernicious popular one expressed by a multitude of persons and texts. These two attitudes can sometimes effect an unholy alliance. For example, in the interests of difference and relativism, ethnographers have literalized cannibalism as the savage propensity to eat human flesh during "cannibal feasts," mostly on the basis of narratives such as sailors' yarns that have not been critically examined (Obeyesekere 1996). More important, the doctrine of cultural relativism isolates the other as a species and enhances our predilection for exoticizing the other culture. This does not mean that ethnographers should not document practices that are different from theirs. That is their duty; but they should make them intelligible in terms of mechanisms that are common to us human beings. The hermeneutical principle of making the familiar strange and the strange familiar would be another way of dealing with this issue. It is no paradox, I think, to make the claim that cultural differences can coexist with family resemblances and structural similarities. It is cultural relativism that inhibits this recognition.

I am not ashamed of certain kinds of essentialism. I say in *Medusa's Hair*: "Our informants are not passive objects out there, nor are we anthropologists tools or objective others. They must think in some fundamental way as we do, for *we* and *they* are constituted of the same essence, our human nature, our species being" (1981:192). I can come to this conclusion from different

positions, for example, from Buddhism. The Buddha repeatedly told us that, while animals are differentiated according to species, humans are not (Horner 1989:381–82):

Not in the hair or head or ears or eyes,  
Not in the mouth or nose or lips or brows,  
Not in the throat, hips, belly or the back,  
Not in the rump, sex-organs or the breast . . .  
Nothing unique is in men's bodies found:  
The difference in men is nominal.

What is wrong with this kind of essentialism? Our contemporary knowledge of neurophysiology and cognitive thought processes can reinforce such a humanistic vision, obviating the necessity to insult people in other cultures by denying their capacity for common modes of thinking, perception, and cognitive functioning, and at the same time not fall into the trap that such functioning is immune to cultural influences. Hence my emphasis on modes of thinking rather than modes of thought (1992:21). Common modes of thinking, such as practical rationality, can underlie different modes of thought. I am not for erasing differences; I am for blurring them, seeing structural and other similarities behind substantive differences and, when differences exist, trying to understand them theoretically, though not in terms of a theory that isolates cultures as species. To move ethnography into the uncertain borderlands of the future one must unfreeze the world of the native and open up the multiple worlds contained therein. At the same time one must open up the boxed-in world of ethnographic theorists who would draw chalk circles around islands of history and thereby unwittingly esotericize their cultures, ignoring the human suffering and pain and bypassing the political struggle, colonization, or conquest on which native activists justly look back in anger. But I think I have slipped into a futuristic guruism myself, and it's time I put a brake on these dry thoughts in a wet season.

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I am responding to the plea in Borofsky's admirable article to raise the level of discussion to something more useful intellectually. This is not hard to do, since it seems to me that the several debates over Hawaiian history during the past decade have put the very possibility of a historical anthropology—maybe even the possibility of a comparative cultural anthropology—at issue. Some very odd things of a vanishing sort have happened in the course of these disagreements, and for a very good reason. The very good reason is criticism of the alleged ethnocentricism and imperialism of Western studies of Hawaiian history, including a witting or unwitting support of Western violence—something that no one with any sense of justice, or merely a judicious concern for

her or his academic skin, would want to defend. Yet, oddly, what has tended to be lost in the criticism—or, indeed, by means of the criticism—is the specificity of 18th-century Hawaiian culture and history. Sometimes, as Borofsky indicates, it is because of an adventitious relation between anthropological work on the Hawaii of 200 or 150 years ago and the current sovereignty movement, since so much depends on the immediate political context of the latter as well as its aspirations, values, and organizations. But more generally the problem seems to lie in historiographic arguments that, in the name of decolonizing Hawaiian history, recuperate it in standard average Western notions and ideologies. Thus the testimonies of scores of Hawaiians about Cook from in and around his time, as recorded in numerous historical documents, are dismissed on the *petitio principii* that the *haole* author or editor of the document must have been responsible for the idea or else that it had been put into the Hawaiians' heads by Europeans, especially Christian missionaries (who arrived in 1820 and became successful in 1825). The documentary record is reduced to repeated acts of ventriloquism. Something similar then happens to the cultural values entailed in Hawaiian people's discourses: their distinctive patterns of belief and action get swallowed up in common Western senses of rationality. Their own scheme of things is resolved into a philosophy of empirical realism that pretends to be human and universal—or else, in other arguments, distinctive of “natives” by contrast to Western-colonial mythologists.<sup>1</sup>

Yet the issue is not, I think, the one Borofsky proposes—who can speak for Hawaiians, whether of back then or now. Reference to Evans-Pritchard's discussion of translatability does not mitigate the plain, obvious, and ugly meaning of this phrase. To assume the right to speak for Hawaiians would be morally repugnant as well as epistemologically mad. Nor is the problem whether they—the so-called (by Gayatri Spivak) subalterns—can speak. The problem is whether they can be heard and understood. And more fundamentally, it is whether anthropology can enter into a dialogue with what Hawaiians have been saying and doing and in this way transcend all the (respective) understandings of history that are limited to an awareness of self.

My own view of anthropological practice is that we can push back these limits, at least to some higher order of impossibility. I mean the reciprocal practice in which the anthropologist submits his or her scholarly resources—already by training partly cosmopolitan—to the words and acts of other peoples, thus creating an “objective” manifold of symbolic relationships that will also situate the cultural schemes of one's own society. No need for post-Enlightenment fears about the “objective” manifold. As I have recently argued in these

pages, the International Phonetic Alphabet is an “etic” grid, an objective apparatus of description, precisely as it is composed of known phonemic distinctions in natural languages—thus, in a sense, a meaningful system of discernible sound contrasts. In the same way, anthropology as cultural practice involves a synthesis of ethnography and comparison. More than that, it requires an intellectual interchange of subject positions of the sort one must make in any and all symbolic communication. Since in speaking the speaker constructs the world from his or her own vantage, an interlocutor has to adopt that standpoint in order to understand and reply. So likewise, as a mutual reflection of meaningful logics, anthropology struggles to go beyond its membership in a particular society by virtue of its relationship to others. Utopian and Sisyphean? Perhaps that, or even worse, for by struggling to synthesize cultural and historical diversity in a unitary field of knowledge—that is, as a means of understanding the particularities of this diversity—anthropology has before it the ultimate illusion of a self-consciousness of humanity, that is, as a species being.

The other impossibility consists of the limitations of self-awareness. The problem here is, that as an intersubjective field of which the people concerned have different social experiences and local perceptions, a cultural life in its complexity, let alone its totality, involves reasons and relationships that no one who lives it can be expected to express. Significant differences (heteroglossia) there will always be. (The whole historical analysis I have done about Cook and the following decades in Hawaii turned on conflicting views and behaviors of ruling chiefs and Lono priests, chiefs and commoners, men and women.) But the important anthropological question is, *Are there any significant relationships in and of the differences?* Moreover, given that any such cultural life is both natural and presupposed, neither can those living it be expected to give an adequate account of why they say what they are saying or do what they are doing. One may relate why one fought in the Viet Nam war, but this is no explanation of why there was a war. One can give reasons for marrying this or that person, but such is no explanation of monogamy. All this is an argument *for* what postmodern anthropology has made us allergic to: ethnographic authority, the so-called construction of the other. A better phrasing would be *construing the other*. And whether ethnographic authority in this sense turns into Orientalism or some such imperialist conceit depends on how it is achieved rather than whether it is attempted. There is no choice here. The attempt is a necessity: Either anthropology or the Tower of Babel. Hence the two moves of ethnography: submission to the understandings of the others and the integration of what is thus learned in a general anthropological understanding, corresponding to the two-stage project of comprehending cultural work that Todorov has read in Bakhtin.

First is the scrutiny of the work as the author comprehended it, without leaving the limits of that comprehension. Basically this is the arduous, *empfinden* aspect

1. I am told that the use of quotation marks bracketing “Native” in *How “Natives” Think* (Sahlins 1995) is not generally understood as intended, that is, as a reference to the use and concept of this noun by others. In my anthropological vocabulary *native* is an adjectival form, most often used in connection with indigenous European practices and ideas.



of the ethnographic encounter. Call it endotopy, by contrast to the complementary phase which Bakhtin labeled "exotopy": the inclusion of what is said in an alien cultural context, thus revealing rather than repeating a meaning by setting it in novel positional relationships. Curiously, then, the process Bakhtin knew as exotopy is profoundly analogous to the construction of value in the semiotics of Saussure. A foreign cultural practice acquires an anthropological value by relationships of contrast in a comparative field (Bakhtin, quoted in Todorov 1984:109–10):

There is an enduring image, that is partial, and therefore false, according to which to better understand a foreign culture one should live in it, and, forgetting one's own, look at the world through the eyes of this culture. . . . To be sure, to enter in some measure into an alien culture . . . is a necessary moment in the process of its understanding; but if understanding were exhausted at this moment, it would have been no more than a simple duplication, and would have brought nothing new or enriching. *Creative understanding* does not renounce its self, its place in time, its culture; it does not forget anything. The chief matter of understanding is the *exotopy* of the one who does the understanding—in time, space, and culture—in relation to that which he wants to understand creatively. . . . In the realm of culture, exotopy is the most powerful lever of understanding. It is only to the eyes of an *other* culture that the alien culture reveals itself more completely and more deeply (but never exhaustively, because there will come other cultures, that will understand and see even more).

Bringing to bear on the one culture an anthropology of many, this tacking between the inside and the outside aspires to a singular intellectual union of truth and method. Its ambition is the identity of another cultural logic and one's own thought—whose condition of possibility is a common human symbolic capacity. Lévi-Strauss says somewhere that the distinctive project of anthropology consists in transforming the objectively remote into the subjectively familiar. What he means, I imagine, is that one addresses a practice, say, cannibalism in ancient Fiji, that is initially distant from our experience and perhaps even repugnant to our feelings. But then, ethnographic texts which seem resistant to our own cultural specifications may yet, by the nature of the mental operations involved, provide an opening to intelligibility (cf. Sahlin 1983; Thomas 1991:69–70). A. M. Hocart relates the apology of a Fijian chief to the builder of his sacred double canoe that he could not reward the craftsman as in the old days with a cooked man or a "woman brought raw," for Christianity, he said, "spoils our feasts" (1929:129). Confronted with this violation of our own dietary laws, we can nevertheless grasp the operation of equivalence that links cooked men and raw women, even as we can metaphorically expand our native notions of exchange, appropri-

tion, and consumption to what for Fijians are so describable. It also helps that Fijians describe these categories of persons, together with certain objects such as the teeth of sperm whales that were traditionally transacted with and against them, as members of a general class of "great things" (*ka levu*). Indeed, all these "great things" have a common finality—if by means ranging from birth to sacrifice—which is the reproduction of society through the appropriation of divinity.

Clearly, an intercultural understanding does not rest simply on a common biology, a physiology of perception that will allow anthropologists and their interlocutors to agree on the empirical referents of their otherwise different talk. Something to the contrary: the possibility of anthropology consists in mutual and communicable symbolic operations, of the sort that can make logical and intelligible what is otherwise empirically unbelievable. "This bread is the body of Christ." "The sweet potato is the body of Lono." "I am descended from an eagle." The issue is not perception merely but judgment: what is what there is, and how is it related to what else? We can follow not only the distinctions that other peoples may be selectively perceiving in things but also what these distinctions signify and, above all, the relevant relationships of signification: of similitude, contrast, identity, negation, classification, causation, proportionality, synecdoche, temporality, existence, and the many other analytic (or synthetic a priori) operations people use to construct a form of life. The issue is not sensory perception but meaningful predication.

Something like cannibalism or the eucharist can thus become anthropologically intelligible even if it is not to everyone's taste. But then, cultural relativism has never meant for anthropology the vulgar moral relativity for which it is criticized by the defenders of Western-cum-universal virtues. It does not mean that any peoples' values are as good as any others', if not better. Relativism is the simple prescription that, in order to be intelligible, other peoples' practices and ideals must be placed in their own context, thus understood as positional values in a field of their own cultural relationships, rather than appropriated in the intellectual and moral judgments of our own categories. Relativism is the provisional suspension of one's own judgments in order to situate the practices at issue in the historical and cultural order that has made them possible. Then something like cannibalism achieves an anthropological status: it appears as humanly *logical*—if not universally likable. This symbolic possibility may be a reason some people are tempted to believe that even the worst human behavior is biologically natural.

Logic, in this instance a Fijian logic by origin, is *something going on inside us*. We come back to the singular character of anthropological knowledge—that it involves a substantial unity of the knowing subject with that which is known. One need not speak of a union of subject and *object*. This not simply to avoid the accusation of objectifying other people but because, to recall Lévi-Strauss's observation, the successful an-

thropological project is marked by the always decreasing distance between the thought of the knower and the nature of the known. By virtue of the shared humanity of anthropologists and their interlocutors, which is also to say their common symbolic capacity, the former replicate in mind, as the meaningful significance of custom, what the latter express in practice. By virtue of their common ability to grasp, analyze, and recombine meaning, the necessities of custom practiced by other peoples reappear as the logical sequiturs of an anthropological understanding. In a certain way, more or less imperfect of course, the anthropologist recapitulates as his or her own mind—as logical operations—the process by which the phenomena of custom were produced. Hence the seemingly bizarre possibility that “les mythes se pensent en moi.” Method approaches truth ontologically—not in the manner of the natural sciences, as the meaningful translation of a foreign-material process. Indeed, the more we know about physical objects the less familiar they become, the more remote they stand from any human experience. The molecular structure of the table on which I write is far removed from my sense of it—let alone, to speak of what is humanly communicable, my use of it or my purchase of it. Nor will I ever appreciate tableteness, rockness, or the like, in the way I might know cannibalism. On the contrary, by the time one gets to the deeper nature of material things as discovered by quantum physics, it can be described only in the form of mathematical equations, so much does this understanding depart from our ordinary ways of perceiving and thinking objects.

The differences are Vichian. In a golden passage of *The New Science*, Vico spelled out the conditions of the possibility of a new anthropological science by a foundational contrast between the epistemologies of culture and nature. Even societies of antiquity “so remote from ourselves” can become uniquely accessible to us because they were made by operations of the human mind, whereas the things of nature we must know externally, as it were, since we did not create them. Of human doings we have understanding “through causes,” why they are made as they are, but of nonhuman things only through attributes, what they are. The true and the made are interchangeable (*verum et factum convertuntur*). As the famous text runs, we can acquire a unique truth of civil society since it “has certainly been made by men, and . . . its principles are therefore to be found within the modifications of the human mind.” Hence it is rather marvelous “that the philosophers should have lent all their energies to the study of the world of nature, which since God made it, He alone knows; and that they should have neglected the world of nations, or civil world, which, since men made it, men could come to know” (*New Science* ¶ 331; see also ¶ 349).

Still, Vico had no easy time of it. It took Herculean efforts, as he endlessly complained (and boasted), to think this way into the strange concepts of the poetic and heroic ages past—the same problems of *empfinden* that continued to attend the parallel distinction be-

tween *Geisteswissenschaft* and *Naturwissenschaft* into the 20th century. The problem was (and is) that so far as any other human life form is concerned, we did *not* make it, even though it was made by minds like our own. There remains the problem of conjugating Self and Other.

But then, is not the synthesis of Self and Other a fundamental necessity of symbolic communication—and a fortiori a unique capacity of the human mind? What puts language—the whole world—at the disposition of the individual person and at the same time defines his or her subjectivity is the pronoun “I,” a term whose only reference entails the act of speaking itself. “I” is the person who utters the sentence containing “I.” Yet the “you” to whom I am speaking becomes “I” when speaking to me, an interchange of subject positions that is complemented by the reversible subjectification of space, time, and ostensive reference in grammatical shifters such as “here” and “there,” “then” and “now,” “this” and “that.” At one and the same time, language constitutes a unique consciousness of self and the commonality of oneself and another as fellows (persons), since in speaking one must also become aware of the other as a self and the self as an other. Rimbaud is not the only Frenchman we can call upon here for clarification. Benveniste (1971:224–25) explains it eloquently and at length:

Consciousness of self is only possible if it is experienced by contrast. I use *I* only when I am speaking to someone who will be a *you* in my address. It is this condition of dialogue that is constitutive of *person*, for it implies that reciprocally *I* becomes *you* in the address of the one who in his turn designates himself as *I*. . . . Language is possible only because each speaker sets himself up as a *subject* by referring to himself as *I* in his discourse. . . . This polarity does not mean either equality or symmetry: “ego” always has a position of transcendence with regard to *you*. Nevertheless, neither of these terms can be conceived of without the other; they are complementary, although according to an “interior/exterior” opposition, and, at the same time, they are reversible. If we seek a parallel to this, we will not find it. The condition of man in language is unique.

Again the problem that arises for anthropology is that we are not dealing with one language only but precisely with “the condition of man in language.” Here, in the human universe of language, not even the discourse definitions of the first-person pronoun are self-evident or sufficient. If “I” refers to the person speaking the sentence containing “I,” still we do not know a priori what is intended in the concept of “the person.” A Fijian or Maori chief could quite grammatically use “I” in reference to the exploits of his ancestors long dead or of his lineage long before he was born. “I fought at such-and-such a place, and there I died; then I moved to this village,” he could say. “It was here that I ate you long ago, before the white man came, so you have no claims

here." The chief instantiates the ancestor, which is a way of figuring that his people's lives are owing to, calculated on, his. The chief is a contemporary ancestor. As such, by descent, he stands as the proper name of a general class. The phenomenon—which has well-known analogues of "positional succession" in East Africa—is once again intelligible even though it was initially inscrutable. It proves that some of our basic sensibilities of temporality, individuality, and identity can indeed be suspended in favor of other peoples' intuitions of reality. By the same move, our own categories are relativized. And by this dialogue of exotopy and endotopy is created a cosmopolitan anthropological consciousness of the species being. When one seeks to study men, said Rousseau, it is sufficient to look around oneself, "but to study *man* one must first look afar; one must first perceive the differences in order to discover the characteristics."

Reflecting on how it was possible for the Rousseau of the *Second Discourse* to conceive a science of ethnology that did not yet exist, Lévi-Strauss argued that the decisive move was the transcendence of the *Cogito* as the beginning of knowledge. The received Cartesian philosophy, "imprisoned by the hypothetical evidences of the self," could then aspire to the creation of a physics, but "only at the expense of founding a sociology or even a biology" (Lévi-Strauss 1976:36). Where Descartes failed to see that one cannot move directly from the interiority of the thinking subject to the exterior world without passing through the human worlds (the societies) mediating these extremes, Rousseau was able to reflect upon himself from such an outside vantage, as another: a necessary complement, ever repeated in the experience of the ethnographer, of the determination of the other as an I. But now the union of method and truth that distinguishes anthropology becomes even more complex, since it synthesizes at once the unique symbolic capacity of humanity, the nature of the ethnographic project, and the construction of human society. The reciprocity that Rousseau and Lévi-Strauss in their different ways determined as the formative act of society is no more or less than the realization in practical action of the same reversible intelligibility that Benveniste described for human language. For Rousseau, the human beginning was in compassion (*pitié*), the disposition to identify with any suffering being, especially those like ourselves. Hence Lévi-Strauss, by putting exchange at the origin of culture, properly laid claim to the Genevan *philosophe* as anthropological ancestor. Exogamy and the gift, as opposed to incest and egoism, are structural counterparts of Rousseauian *pitié*, with the same effect of mediating "the threefold passage from nature to culture, from feelings to knowledge, from animality to humanity." And anthropology, as the ethnographic practice of the same sort of symbolic interchange, finds an intellectual sympathy with the cultural relationships it seeks to understand.

Yet the ultimate impossibility, many will say, issues from the unequal power relationships between anthropologists and their interlocutors. The arrogance of eth-

nographer authority is that only the ethnographer, the one from the dominating society, writes. (Moreover, he writes inscrutable things like this commentary.) What is to prevent him—should we not just leave the pronoun in the masculine?—from organizing the others in his own terms and interests? And the situation seems even worse when it comes to writing history. The inequalities of something like the colonizer and the colonized are here compounded by the differences between the quick and the dead. At the same time, history is an interesting test, for who is seriously prepared to argue that we have learned nothing from history except about ourselves? And how exactly have we been able to understand the meaningful values and relations, indeed, the distinct ontologies and reasons of action, of other times, places, and peoples? To say that such history cannot be done, that a priori we can only succeed in constructing others in our own image, would, however, be an ultimate assumption of power. It would take divine omniscience thus to know in advance the limits of what we can understand about humanity.

True, in any intersubjective dialogue—including now ethnography and history—there is always a kind of Nietzschean moment, or a built-in will to power, insofar as the world is egocentrically constituted by the "I" of the speaker. But this hubris is necessarily reversible if there is to be communication. Indeed, one might say that fundamental relationships of society are present in the microcosm of symbolic interchange. If the assertion of an "I" is a claim to *power*, the reversibility of "I" and "you" is alternatively *competition* or *reciprocity*, even as the mutual recognition of personhood is the germ of *sociability*. Elementary principles of human society are intrinsic to the structures of symbolic discourse. Anthropology is an attempt to transcend the customary parochial limits of such discourse. So criticize the anthropologists and their culture concepts as you will for ethnocentrism and imperialism, there have always been contradictory human relationships in their project.

## Reply

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Let me begin with two shared points of reference. All of us acknowledge that uncertainty exists about our interpretations of British-Hawaiian contact in 1778–79. Obeyesekere is clearest about this in his writings, but both Kane (with his reference to "a Rorschach test") and Sahlins (with his reference to "a higher order of impossibility") acknowledge it as well. How could we not, given the limitations and diversity of the data we have from Western and Hawaiian sources? "Knowing the past," Kubler (1962:19) observes, "is as astonishing a performance as knowing the stars."

Still, despite such uncertainty, we all try to make



sense of the events surrounding Cook's visits to Hawaii. The uncertainty involved in understanding the past does not prevent our analyzing it; it only frames how we proceed. While sensitive to the problems involved, all of us still weigh the existing evidence, make judgments regarding its credibility, and draw conclusions. I suspect that Kane and Sahlins—from what they have written here and elsewhere—would concur with the standard of evidence Obeyesekere offers for evaluating these conclusions: that they should involve “reasoned, evidentially supported arguments.”

*Arguments judged.* Given the above-noted uncertainty, we cannot talk of absolutes or of “truths” in these evaluations. We can only compare one account with another. But using Obeyesekere's standard as a reference point, we can still perceive key weaknesses in Obeyesekere's account vis-à-vis Sahlins's.

First, to call part of his text—which I perceive as “selectively ignoring or misrepresenting . . . primary documents” (Sahlins 1995:117)—*jouissance* is to raise the question of what is not *jouissance* in Obeyesekere's work. How do we interpret, for instance, the examples of imprecise scholarship cited in n. 4? What do we make of Obeyesekere's response to my criticism that he uses Kamakau in contradictory ways—that Kamakau “like all of us . . . could be sometimes right and sometimes wrong”? Are these also cases of *jouissance*?

Second, in not responding to my specific criticisms, Obeyesekere allows that various factual and bibliographical errors “are normal, mostly innocuous, and eventually correctable.” But my point is that such errors are pervasive in *Apotheosis*. (N. 4 provides only a sampling.) In accepting them, Obeyesekere is, ultimately, accepting that he does not have an “evidentially supported argument.” In replying to other reviews of *Apotheosis*—in *Social Analysis* (1993), *Pacific Studies* (1994), and *Oceania* (1995)—Obeyesekere takes the same tack as he does here. He avoids going over a host of documentary details point by point. The result is, as here, a set of critical comments that skirts important specifics. If, as Obeyesekere asserts, “ethnography is an empirical discipline that cannot afford to turn its back on evidence” (1992:xv), why, in his comment here, does he seem to do just that?

Third, there are critical problems with the reasoning behind Obeyesekere's argument, for example, in the case of *kapu moe*, the Hawaiian ritual of prostration. What he suggests here is that the British myth of apotheosis occurred only in Hawaii because “the rituals of prostration that the Europeans interpreted as ‘worship’ occurred only in Hawaii.” However, there are cases of *kapu moe* without any reference to European apotheosis. Cook, for example, observed rituals similar to *kapu moe* (called *moemoe*) in Tonga during his second and third voyages (see Beaglehole 1961:lxxv, 269; 1967:100, 117), but they were not addressed to the British, only to Tongans of the highest rank. Hawaiians at Kauai performed the *kapu moe* to Cook in early 1778 (see Beaglehole 1967:269). Yet, as Obeyesekere himself acknowledges (1995:270; 1993:79), Cook was not called

Lono in the British texts, nor did the British depict Cook as being apotheosized. Furthermore, the one clear case of *kapu moe* coexisting with the apotheosis of a European is also the case in which European observers and Pacific Islanders alike claimed that a European was, in fact, apotheosized—at Kealakekua Bay, when Cook was perceived as a manifestation of the *akua* Lono. It is not clear here in what sense we are dealing with a myth. And if we call it a “myth,” then it is a myth that some Europeans and Hawaiians both held to. One might well ask why Obeyesekere focuses solely on Europeans. Even if we accept that the *kapu moe* at Kealakekua Bay might have inspired a European myth, why could various Hawaiians not have been acting out their *own* beliefs about Cook's relation to Lono in performing this ritual? In clarifying his argument, Obeyesekere allows us to see some of the problems he has in supporting it.<sup>1</sup>

Finally, Obeyesekere agrees that his “whole argument is doomed if it can be shown that the apotheosis is not a Western myth model.” At issue is (a) what constitutes “proof” (if I may temporarily use such an ambiguous word) of the existence of Obeyesekere's myth model during a certain period of time in a certain locale and (b) what impact such a myth model, if it existed, had on explorer accounts. Having read the Hamlin (1994, 1996) references Obeyesekere cites, examined the actual explorer narratives, found confirming evidence in Greenblatt (1988) and Clendinnen (1993), and talked to Hamlin himself, I would concur with Obeyesekere that a reasonable case might be made for some sort of “myth model” in Renaissance explorer narratives (though perhaps not as all-pervasive as Obeyesekere's “virtually every” implies). I cannot, however, accept Obeyesekere's extension of this myth model to the Pacific in the mid- to late 1700s because, aside from the Hawaiian case, there are no convincing examples to support it. In respect to Tahiti, the reference Obeyesekere cites is from Robertson's journal: “Some of my mess-mates thought they [the Tahitians] would now look upon us as Demi-Gods” (1973:43). But there is nothing in Robertson's journal or in Hawkesworth's (1773) published account of the voyage that states that the Tahitians actually then proceeded to treat Wallis or other members of the *Dolphin* as “gods.” Nothing parallels the statements made by the British regarding Cook at Kealakekua Bay, despite the fact that the British stayed more than a month at Tahiti. What makes the Tahitian case particularly interesting is that there are hints that the Tahitians may indeed have equated some members of the *Dolphin* with the Tahitian *atua* 'Oro (see Denning 1986) but that these hints were generally misperceived by the British as tokens “of Peace and

1. I also fail to understand how the following statement from Samuel's *journal* makes “no reference to Cook's apotheosis”: “These People pay the greatest attention to Captain Cook. . . . To day a Ceremony was performed by the Priests in which he was invested by them with the Title and Dignity of Orono, which is the highest Rank among these Indians and is a Character that is looked upon by them as partaking something of divinity” (Beaglehole 1967: 1161–62).

friendship" (1973:46; see also Hawkesworth 1773, vol. 1:213).<sup>2</sup> I am puzzled over how Zimmermann's account—including the statement "The inhabitants of the island of O-waihi raised Captain Cook to the dignity of a god" (1926:36)—supports Obeyesekere's thesis. Obeyesekere seems involved here in verbal gymnastics; calling Zimmermann's statements opaque, he reinterprets them to suit his purpose. He never really comes to terms with a basic problem: that according to his argument there should logically be additional confirming evidence that European explorers—either Cook at other times or other explorers—depicted Pacific islanders as apotheosizing Europeans in the mid- to late 1700s. Evidentially, he never makes a convincing case for one. That is why I would question the existence of Obeyesekere's myth model among Pacific explorers during this period.

*Questions answered.* I appreciate Obeyesekere's responses to my questions. He clarifies the context that frames certain of his polemical remarks, and he is honest about wearing his "own peculiar kinds of blinders."

Sahlins, instead of responding to specific ethnographic issues in my review, turns to broader concerns that add to the discussion. Readers should know, however, that while he was framing his reply, faxed questions and counterquestions traveled between us for roughly a month regarding the differences between his and Obeyesekere's positions, my interpretations of and phrasings for particular events, and the questions posed to him. Detail was piled upon detail, citation upon citation in the exchange, and I spent much time rere-searching issues and resupporting statements. The process, I would note, added to my respect for Sahlins's scholarship. In examining documentary evidence, Sahlins seems concerned with the cultural contexts that framed and to a certain degree helped produce it. To quote his response to one of my questions: "There is a structure of discourse and its efficaciousness that is not ours; it lies in the society concerned and may be determined from the generality and effects of various voices in action and the course of history" (personal communication, July 14, 1996).

It is critical in following this controversy to realize that both Obeyesekere and Sahlins have refined and continue to refine their positions. For example, Obeyesekere has softened his opposition to the possibility of chiefs' being divine, moving closer to the position well argued by Kane. And he clarifies earlier phrasings in *Apotheosis* regarding his placement "as a native ethnographer and former colonized subject" and the privileged insights this possibly offered; "I neither assert nor imply that [the insights gained from this place-

ment] provide privileged access into Hawaiian culture." Sahlins clarifies and develops the determinants of action in *Islands of History* and refines the notion of *kino lau* (or myriad embodiments of Hawaiian deities) in *How "Natives" Think*. To catch the full import of Obeyesekere's and Sahlins's ideas, we need to trace them through time, following their progress as they clarify and refine them. I think it inappropriate, therefore, to continue accusing Sahlins of being insensitive to the fluidities of structure, misperceiving what he means for the sake of argument. However one interprets—or misinterprets—Sahlins's earlier writings, it is only fair and sensible to take note of how he responds to readers' perceptions/*misperceptions* of his work. Otherwise, one is debating phantoms.

*Speaking for "Others."* One of the most striking points arising from the comments is that both Obeyesekere and Sahlins deny any interest in or intention of speaking for Hawaiians. Yet Kane points out that "the popular view of anthropologists . . . is one which lumps them with missionaries, fortune hunters, and snake-oil salesmen." I noted Kane's suggestive renaming of Sahlins's book *How Anthropologists Think: About Polyne-sians, For Example*. Kane's comments go a long way toward explaining why Sahlins is repeatedly attacked by certain Hawaiian activists despite their concurrence with details of his analysis and why Obeyesekere is accepted as long as he attacks Sahlins. We all, I believe, agree on one point: We need shared conversations across borderlands of difference in which scholars on both sides feel empowered to question, to challenge, to assert their positions within a set of common reference points. It is unclear whether that is happening today. Most anthropologists renounce anthropology's past ties to colonial regimes. But is not a more subtle form of intellectual hegemony occurring within the discipline as anthropologists talk to each other in ways that those they talk about find either confusing or excluding because of the jargon? At times anthropologists appear to be self-absorbed communicators who reach out, through "current fad theory" (in Kane's terms), only to each other.

Clearly, there is no one authoritative voice among Hawaiians or among anthropologists. There is a plethora of voices questioning each other's authority. This is what I suggested occurred among Hawaiians at Kealakekua Bay. Particular priests and particular chiefs, I suspect, had divergent interpretations of Cook's presence. What is central to understanding those events, then, is who had what power to enforce what interpretations on others during what periods of time. I have cited a host of references regarding the tensions perceived by the British between Lono priests and Kalaniōpu'u's chiefs. That is why I question that the priests of Lono were simply following Kalaniōpu'u's directions at Kealakekua Bay. I doubt that they were so compliant. I would also question whether present-day scholars should dismiss various 19th-century Hawaiian assertions that Cook constituted a manifestation of the *akua* Lono simply because Europeans recorded these statements or

2. There is some indication that certain British perceived some Tahitians as viewing the red pendant that the British planted on the beach to claim possession of the island (though not the British themselves) in supernatural terms (see Robertson 1973:50; Hawkesworth 1773, vol. 1:227–28). Tahitians proceeded to take this pendant and incorporate it into their prestigious *malo ula* (see Denning 1986:105).

because certain missionaries may have influenced them. I would prefer examining the specific contexts in which each statement was made before providing a blanket assessment of their overall validity. Kane and I cannot, in these pages, extend our conversation regarding other critical points he raises. (My reply is required to be short.) But we can, and will, continue our discussions through phone calls and faxes.

Ultimately, each of us can only offer our perspective in a conversation around common points of reference as we have done here. To be effective, anthropology must speak *with* others—in this case, *with* Hawaiians—rather than *to* (or *at*) them and certainly not, as we all agree, *for* them. *Anthropology must enlarge its sense of relevant audience.* It must converse more effectively with those who lie beyond the borderlands of the discipline. There must be, to use Sahlins's phrase, a "mutual recognition of personhood."

*Knowing "Others."* One of the more interesting questions that surely comes out of this controversy is how Obeyesekere got so far with so little evidence. The process, I suspect, is common in anthropology. Rarely do anthropologists check each other's ethnographic accounts. Appearance counts for much in the discipline. If the ethnographic argument fits with research done by others in neighboring (or related) groups, if it supports and/or develops current theoretical perspectives, if it includes extensive documentation (e.g., citation upon citation), it tends to gain credibility. But why? All these epistemological criteria involve matters outside of the documenting experience itself. They are used by people who have not examined—or in the case of certain fieldwork situations cannot examine—the original documentation (see Pratt 1986). Obeyesekere is correct that "virtually everyone who wrote about this debate [in reviewing his book] was a Pacific historian or anthropologist," but that does not mean that most of them knew the specifics of British-Hawaiian contact. Nor did most, apparently, check many of Obeyesekere's references.<sup>3</sup>

This style of evaluation has had some interesting consequences. Howe uses conflicting British accounts concerning details of Cook's death—a rather narrow definition of the controversy and the documentary data relevant to it—to conclude that "there is no final judgement for History" (1996:118). Rather than take a comparative perspective in dealing with the past's uncertainties, he bases his assessment mostly on questions of style and personal preference. One is left to wonder: Does acknowledging the past's uncertainty allow one to

ignore key questions and data? To make one's preferences central to assessing the evidence? Clifford Geertz has taken on the status of an "expert" in a recent Honolulu magazine exchange regarding the controversy not on the basis of what he knows—he readily admits to being unfamiliar with the specifics of Cook's visit—but on his *New York Review of Books* article and his being a distinguished anthropologist (see Rees 1996a, b; Stannard 1996). And finally, several scholars have told me in private that they prefer Obeyesekere's argument to Sahlins's because it fits better with present-day postcolonial concerns. Even if Obeyesekere lacks the evidence, they suggest, he grasps the big picture; he understands the politics of oppression. How does one respond to such statements?

We might, as we have done here, make *comparative* assessments based on shared points of reference. What anthropology is all about, ultimately, is comparison. From a comparative perspective, issues of representation—so problematic to anthropology today—constitute an important intellectual tool for developing the discipline. Clearly, we represent others in different ways than they represent themselves and vice versa. (Kane lumps anthropologists with snake-oil salesmen, remember?) We are surprised by Hawaiian representations of anthropologists, and Hawaiians are surprised by anthropological representations of Hawaiians. Each of us gives voice to the other's silences. Each perceives things that the other, at times, obscures. (This is where Kirch and Sahlins [1992] ran afoul of Kame'ele'ihiwa in respect to Hawaiian chiefs.) The result of our comparisons is surprise (how could "they" assert that?). This leads to perceiving our framings for what they are—our constructions. And we gain some idea of the reasons behind various perceptions and misperceptions of ourselves vis-à-vis others (and others vis-à-vis ourselves). Differences of representation, from this perspective, are less part of the problem than part of the solution. In conversing across our differences, we enhance the opportunity to learn from them. We also enhance the discipline of anthropology.

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3. I have focused on published works here so that readers can check the documentary material on which my assertions are based. I have copies of all the unpublished journals cited in Beaglehole (1967: clxxvi-clcvii) for Hawaii and of most of the published materials cited in this debate, but I prefer, when possible, to forgo the status game of citing references others generally cannot check and allow readers to participate in the evaluative process. I would add that many of the published references cited in this controversy are reasonably accessible, as an examination of the OCLC World Cat will indicate.



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