

Response: GANANATH OBEYESEKERE
PRINCETON UNIVERSITY

**How to Write a Cook Book:
Mythic and Other Realities in Anthropological Writing**

I must express my appreciation to the editors of *Pacific Studies* for providing a forum for the discussion of *The Apotheosis of Captain Cook: European Mythmaking in the Pacific* and to the four reviewers for their reflections on it. I regret that here in Sri Lanka I do not have access to any of the journals of the voyages, not even copies of Valeri's and Sahlins's work. I have to depend on my own book, which I have before me, and my recollections of what is contained in the others. I will also examine the larger methodological, ethical, and political implications of the reviews of *The Apotheosis* in terms of the empirical examples advanced by my critics. My response to Valeri, however, will have an added dimension, I shall show that it is impossible to respond to him on empirical grounds; his and mine are different modes of writing anthropology and I can only criticize his approach and defend mine. Who is right--or ethically and politically sensitive (or correct, or whatever)--is for others to decide. I am totally inept when it comes to prescribing alternative ways of writing ethnography; little recipes for Cook books are more in my line. I shall start off with the two reviews that are most sympathetic, those of Lilikalā Kame'eleihiwa and David Hanlon, and then deal with the highly ambivalent review by Nicholas Thomas and the more straightforward one by Valerio Valeri. Because Thomas's critique is a bit fragmentary I might on occasion also use his earlier review of my book in *Current Anthropology* (vol. 34, no. 3 [1993]: 328-330; hereafter CAR) in my response.

In his review Hanlon asks the following question: "I wonder too how the descendants of **Kalani'ōpu'u** will regard this work; I think they might tell us that their past more than 'barely exists' . . . and that it is not as easily ordered as Obeyesekere believes" (above, p. 111). I shall respond to Hanlon's rather unfair representation of my views of Hawaiian history later, but for the moment let me consider the strongly worded political and ethical critique of anthropology and related disciplines by a daughter of Kalani'opu'u, **Lilikalā** Kame'eleihiwa, who says: "when I teach that part of Hawaiian history I relate to my students that he [Cook] brought venereal disease, violence, and, eventually, an unrelenting wave of foreigners, once his journals had been published in Europe" (above, p. 111). She laughs at the work of *haole* scholars like Sahlins and Valeri, whom she accuses of misrepresenting Hawaiian cultural acts. She castigates our own discipline as the "white man's pseudo-science of anthropology." She has made an important point regarding the manner in which the "native" has been represented in anthropological writings; and I think natives are quite rightly reacting to what they perceive are "condescending attitudes." I can imagine many scholars responding to these ethical and political critiques with snooty hauteur; but that kind of response is only going to alienate us further from those whom we write about and who ought to be co-producers of our ethnographies. I am going to take Kame'eleihiwa's critique seriously in my own political and ethical critique of Valeri's work. For the moment I must defend some kinds of ethnographic writing.

The founding fathers of anthropology in the nineteenth and early twentieth centuries have been part of the colonialist enterprise, as indeed have been historians and other scholars who have represented colonized peoples in their writings. It is futile to deny that that heritage is not alive and well, masquerading under the guise of objectivity, in the work of some of our colleagues. I have pointed out in my Cook book that the prejudices of the past get embedded even in our theoretical work, in such notions as the "savage mind," or the idea that natives don't possess individuality, or that they are incapable of rational discrimination, or that their thought is governed by "stereotypic reproduction," and so forth. Part of the trouble lies with anthropology itself in its self-appointed role as protector of the integrity of native culture, conjoined with the naive belief in cultural relativism and the arrogant notion that the other culture could be understood in terms of the rules devised by the scholar during his short sojourn in an alien culture. On the other hand, in defense of anthropology let it be said that this posture does have a good side. Ethnography did bring into the realm of intellectual discourse the lifeways of peoples who have not been represented in

history before-- ordinary peoples living in small communities or, to use a currently fashionable term, "subalterns"--even if that subaltern consciousness has been falsely arrogated or badly represented by the ethnographer. Moreover, there is I think an emerging self-criticism among anthropologists in respect to the discipline's own past and its styles of representing the other culture, stimulated by recent radical epistemological rethinking in the human sciences in general, especially in post-structuralist thought, contemporary feminism, hermeneutics, and in "cultural studies." The last genre seems to preempt much of the anthropologists' own agendas precisely because of our own conservative stances. I also think that anthropology has the potential to carry out Nietzsche's subversive philosophical game by looking at Western culture through the prism of the thought of other peoples and thereby enriching Western discourse itself. I see this emerging in the work of some of my colleagues and my students; and among ethnographic historians who have looked at the recent pasts of European culture itself, to show their striking affinities to forms of life in the non-West, blurring the binary distinctions that have bedeviled Western social thought. I will take up this critique of anthropology further in my response to Valeri's review; here I must express my disappointment that Thomas's students in Canberra are reading the *The Apotheosis* in terms of pernicious and isolating categories like "Pacific history" or "Polynesian thought," as if such forms of thought do not spill over into those of other humans beings, including anthropologists. Or could it be that Thomas has not understood some of his own students?

I am grateful for Hanlon in providing a clear and succinct summary of my book focusing on its central theme of European mythmaking, something that many reviews of my book have ignored. In several places in the latter portion of his review, Hanlon is critical of my interpretations. I do not want to argue with him on many of these because such differences are integral to the nature of interpretation itself. When it comes to the interpretations of highly opaque writings like the ships' journals, one can only make one's own position unequivocal and clear so that others can in fact adopt different readings. On rereading my book I myself have come across certain kinds of "interpretive license" that Hanlon speaks about, as for example when I use a phrase like "it is clear" (p. 41) when perhaps it is not all that clear to the reader! On the other hand, I do not think my use of suppositional terms like "must have been," "likely that," "quite probable," and "reasonable to assume" indicates interpretive license; it rather underlines my own hesitation and nonauthoritativeness regarding a particular line of interpretation. I

would be hesitant to use categorical imperatives or unqualified language to describe the fluidity of the historical situation and the tentativeness of all forms of interpretation. I do have two serious responses to Hanlon: One pertains to my uses of native history and the other to the term “practical rationality.” I want to clarify both these issues because I think they have been misunderstood by Hanlon, perhaps because I have not expressed them clearly enough. In any case I am grateful to him for giving me a chance to clarify my own position.

I would be a poor scholar of South Asian history and culture if I did not take native histories and “myths” seriously; I have done this in virtually all of my Sri Lankan and Buddhist writing. In *The Apotheosis* I have myself been strongly critical of the Western term “myth” to categorize the multiplicity of texts of different types from the native tradition and have suggested that such texts come close to the Indic notion of *itihasa* that embraces what in Western thought may be labeled as history, myth, and legend. Such histories are central to my work in Hawai‘i also. For example, the critical section entitled “Politics and the Apotheosis: A Hawaiian Perspective” (pp. 74-91) is based on Fornander’s exposition of Kamakau’s history of the Hawai‘i-Maui wars. Yet this does not exempt us from a critical evaluation of these histories and I think it is patronizing to treat them otherwise. I don’t think one can make a claim, as I do, that anthropological theories like Sahlins’s are continuations of prejudicial Western discourses, or that European historical writing must be seen in the context of their times, and then affirm that native histories are exempt from such critical appraisals. They too have to be critically disaggregated in relation to a specific research task (p. 163). I would be surprised if Hanlon objected to statements like the following: “The purpose of our initial look at native histories is not to deny their legitimacy as diverse, even contradictory, Hawaiian visions of their past, but rather to determine whether these histories help illuminate the empirical question of Cook’s apotheosis by Hawaiians” (p. 163). Or: “My contextualizing myth in a particular time, place and tradition does not mean that myth elements do not send tentacles into the past. This is not self-evident, however, and only a critical reading of the myths permits us to make tentative historical inferences” (pp. 170-171). Thomas in his review also attributes to me a “crude historiographic rule” that stipulated that the myths of preliterate peoples are not relevant for history (above, p. 122). If contextualization of a text in a particular time and place and making a claim for a critical reading of histories and myths is a crude historiographical rule, I am happy to subscribe to it. Hanlon is of course right that some of the

native histories are not contaminated by the missionary discourse, but my question to him is this: How does one determine that except through a critical reading of texts? Whether one talks of native or academic histories, it seems to me, does not make the slightest difference to this important and crude (that is, "raw") historiographical rule.

The issue raised by Hanlon is a difficult one for anthropologists to accept since myths and narratives are the stuff of their trade. Let me get back to the Hawaiian example: I state that while the texts recorded by Kamakau may not reveal an empirical history they do reveal a feature of Hawaiian discourse, namely their contentious and argumentative nature. I go on to say that it is possible to consider these texts *as if* they were true, and then argue that, even in terms of this *as if* assumption, there is no way that Cook = Lono can be substantiated (p. 166). Now consider why I take the position that the texts dealing with the events of Cook's time compiled by Kamakau more than eighty years later cannot be treated as literal or empirical history. As Kame'eleihiwa says, Cook's voyages brought about the introduction of new diseases and massive depopulation, followed by serious social and moral dislocation. Soon afterwards there occurred the Kamehameha political reform and unification paralleled by a religious systematization. After Kamehameha's death, Hawaiians gave up their traditional religion in 1819; the following year Calvinist missionaries started preaching and Kamakau, the historian I referred to earlier, became one of the earliest converts. Can one seriously believe that the histories recorded at this time were immune to these momentous events? Why is it that anthropologists have such static and conservative views about the nature of texts? Everywhere in the nineteenth century the colonized world reflected the impact of the colonizer. In Sri Lanka, for instance, I have described the hegemonic power of the missions, in spite of strong resistance to them by Buddhist monks, and have characterized the Buddhism of this period as "Protestant Buddhism." Similar ideological changes occurred in Indian history during the same period; and in both Sri Lanka and India histories were refurbished, reinvented, or created outright under the influence and impact of imperialism, colonization, and missionization. Small islands also had to contend with the dislocations introduced by the violently intruding outside world. If so, is it all that senseless to say that a recent convert like Kamakau tried on occasion to rephrase Hawaiian religion in terms of the new doctrines that he fervently embraced? I would add that if there is a historiographical rule underlying my work it is what Ricoeur calls "the hermeneutics of suspicion," which in my thinking is useful for dealing with texts in general, and especially so with texts that are per-

meated with strong emotions and written in difficult times. How can writing history or anything else (including what we are doing here right now) be immune from power, prejudice, ethical and other presuppositions, and, let us not forget, the not-so-elevating politics underlying academic discourses?

Hanlon's second criticism pertains to my notion of "practical rationality" and it has some merit, particularly his idea that practical rationality might itself end up as a literalizing trope. I agree with this criticism and the dangers of literalizing tropes, particularly since I accuse Sahlins of precisely that! However, I suggest that Hanlon's argument can be turned on its head. I can take the opposite stand and say that one reason for the natives' hostility to anthropology is that it is given to excessive symbolization of ordinary practical rationality and (to take up Thomas's point) common sense! In one manifestation of the anthropological imagination, native worlds are so symbolically closed that there is no space in them for creative improvisation and plain literalism. Does Hanlon believe that natives cannot think literally or commonsensically? Part of *my* political agenda is to bring back what many natives think is obvious: that they are capable of plain literalism in thought and expression and the pursuit of economic interests in terms of simple means-goals nexuses, however culturally defined these may be. I explicitly say that "practical rationality" is simply a way of creating a space for thinking of the Other in human terms in a situation where such spaces are difficult to create (p. 21). It is far from being a "spy glass" to survey the cultural world and should not be taken to mean that I subscribe to a universalistic view of British utilitarianism or of "practical reason" of a Kantian sort. I make explicit my limited view of practical rationality as a foil to symbolic interpretations gone mad, never as a substitute for agency-oriented symbolic analysis.

Hence I link my use of practical rationality with the capacity for "creative improvisation," inventiveness, and change; and beyond that to a common psychobiological nature I share with Hawaiians and others. Surely to attribute insensitivity toward symbolic analysis to the author of *The Work of Culture: Symbolic Transformations in Psychoanalysis and Anthropology* (Chicago: University of Chicago Press, 1990) is a little bit odd, to say the least. And in *The Apotheosis* itself I engage in the symbolic game much more than I do in the practical rationality game, in fact virtually everywhere! But symbolic analysis, as I practice it, gives motivation and agency to the actor. It is not a "closed symbolism" of the sort that Valeri practices that does not permit Hawaiian voices and active consciousnesses to surface in any meaningful way. I

also state that practical rationality is not cross-culturally uniform either but varies with each tradition (p. 19). And beyond that I deal with two kinds of discourses that exemplify a practical, rational one and a more tropic or allusive one, namely that of counsellors and priests--the one more or less literal discourse that can be deciphered by us and the other a thicker, more metaphoric or parabolic one that requires a complicated symbolic exegesis (pp. 170-171). The very fact that Pacific historians and anthropologists can argue whether natives think literally or commonsensically and whether they are capable of making practically rational decisions is part of the political problem that makes anthropology such an alienating discourse for those people whom we study and who are now beginning to talk back to us, sometimes, as Professor Lili-kalā Kame'eleihiwa does, not very politely!

I find Nicholas Thomas's review the most puzzling of the lot because I think he has either not cared to read my work through or read it carelessly. It has disconcerting problems of writing style, gratuitously pejorative references that I think the author is unaware of, and certain departures from what he had written only months ago in CAR. He starts his review with the statement that my book is "frustratingly underdeveloped" and somewhat "underresearched," except for the "reappraisal of Cooks psychology," the very psychology that in CAR he labeled as "implausibly dark," without explaining why (CAR, 329). Let me therefore try to imagine what I think his complaints are all about and respond to them as best I can.

He seems to think the core issue in my book is the death of Cook: a strange inference, since none of the many other reviewers has made that particular point. He adds: "the material concerning European mythmaking is reviewed in critical terms, but is on the whole familiar to readers of Bernard Smith and Greg Dening" and to anyone familiar with eighteenth-century travel literature (above, p. 118). In my introductory chapter I state that my discussion of Cooks psychology is very preliminary and that the core of the book is "European mythmaking," as the subtitle indicates, and as Hanlon also notes in his review. This myth relates to the apotheosis of the white civilizer, understood as myth of imperialism, civilization, and conquest that has had a "long run" in European culture and consciousness. I discussed in detail the refractions of this myth in Cortés and Columbus, its reappearance during Wallis's "discovery" of Tahiti, its manifestations in European poetry and drama, and the turn it took in the missionary discourse of the nineteenth century. If my interpretations are already well known to those in Pacific studies, then my book loses much of its originality. Not having a refer-

ence library here, I can only defer to Thomas's judgment on these matters. However, in my own review of some of Bernard Smith's writing, I pointed out that his reappraisal of Cook is "astute" but, in line with the Enlightenment view of Cook, he fails to see the dark side of his hero and takes for granted his apotheosis (pp. 132-133). Now Thomas tells me that Smith does in fact anticipate my argument; I plead guilty for not having read Smith's relevant work. And as for Greg Denning's book, *Mr Bligh's Bad Language*, that Thomas urges me to read, I can only say that it appeared after I had published mine. I haven't read the book yet but more power to his (Denning's) elbow for having demonstrated the European genesis of the Cook = Lono myth. While Thomas's new information robs my book of much of its originality, it also makes Sahlins's thesis look even more bizarre for having ignored all this European mythmaking data freely available to Pacific historians and Polynesianists.

Thomas advances what he considers two methodological criticisms of *The Apotheosis*, the first pertaining to "unsubstantiated readings" of texts and the second to an excessive reliance on "common sense." He gives examples of each kind of methodological error, and I shall now respond to them in some detail because I really think controversial texts like mine are easily misread and can on occasion become a kind of "projective screen" for the reader/reviewer.

1. Cook and the priest Omiah are in attendance at a boxing match and people acclaim, "O Lono," and chant verses, according to the ship's surgeon, John Law. Law thought that the chanting and acclamation was for Cook; I said that this is not likely and that it was in honor of the priest Omiah who was also called Lono. Thomas responds by saying that the "only evidence" I have for this inference is King's assertion that Omiah is "a personage of great rank and power" and adds patronizingly that "[a]nyone who has worked extensively with early voyage sources ought to be aware that statements of this kind . . . are most impressionistic and . . . misleading." One has to document the priest's "ritual agency," his capacities and so forth, he says (above, p. 119). I must confess my amazement at this critique, aside from the put-down that Obeyesekere does not belong to the class of scholars who have looked extensively at these early voyages. Let me therefore reaffirm what I did say about the priest Omiah, also known as Lono.

In my book I was rather pleased that I had resurrected this important figure from the texts of King, Samwell, and Ledyard and devoted a whole section entirely to him (pp. 92-95). Contrary to Thomas, I show that Lieutenant King had an extended account of Omiah in the official

edition of the voyages (1784). King had much time for reflection and he says very specific things about Omiah-Lono, namely, that he was the head of the priests of the Lono cult; that he “was honored almost to adoration”; that he was the son of the old head priest Kao and the uncle of Keli’ikea (both well known to the journal writers); that, like Omiah, his little boy was also treated with adoration and “was destined to succeed to the high rank of his father.” In a long footnote King adds that he was like the Dalai Lama or the “ecclesiastical emperor of Japan.” Samwell made similar observations, in several places noting that Omiah partook “something of Divinity”; he later added that Omiah’s status was as sacred as that of the king himself (p. 109). How then on earth did Thomas think that King only made generalized statements about Omiah? One can even say that it is in reference to Omiah that King (and Samwell) make their rare concrete statements about specific people. It is on the basis of their statements, and the information supplied by Vancouver much later, that I drew a brief genealogy of the line of important priests to which he belonged (p. 93). I further identify Omiah-alias-Lono with Pailiki, the chief priest who headed King Kala-ni’ōpu’u’s Maui campaign. Thomas complains of the lack of ritual agency given to this priest although I describe his agency in several places, including his role in the Maui campaign (pp. 80-81, 108-109). If my detailed interpretation of Omiah is correct, and if Omiah was present at the boxing ceremonies with Cook, what other interpretive choice have I except to say that the acclamation and chanting of the people was for their own revered high priest and not for Cook (who in my analysis was viewed at best as a chief)? I must also protest at this put-down of Omiah; it is as if scholars like Thomas cannot get away from their idealization of Cook and cannot brook the thought that the Hawaiian chief-priest could possibly be treated as superior to the white civilizer. I can understand if Thomas objected to my identification of Omiah and contested my depiction of his sacred status or his genealogy, as Valeri does. What puzzles me is his ignoring the details of my reconstruction of this important person and flatly attributing to King the one assertion that Omiah was “a personage of great rank and power.” This is simply false.

2. Thomas provides an example of my excessive reliance on common sense in a much more responsible argument, and I can only respond by presenting my own case. Before I do this I want to reaffirm a strategy that I employ in my book: “On occasion [I] repeat an important event or interpretation. This is a heuristic device to restore the reader’s attention and a stylistic device to emphasize a different aspect of that event

or its interpretation" (p. xiv). Thomas's example is that of the canoe of Lono floated at the end of the Makahiki which, according to Sahlins, was one of the reasons why Hawaiians thought Cook's great ships were really Lono's. I suggested that to assume that Hawaiians would identify Cook's ships with the tiny canoe floated at the end of Makahiki would be to attribute to them an error in ordinary perception; however, I added that it is possible that once they had seen Cook's great ships, they could metonymically (or synecdochically) represent them as small canoes (or masts) on the principle that the part represents the whole (p. 61). I still stick to this line of interpretation; and D. Barrère confirms this when she says that the ritual of the canoe did not exist in Cook's time but was the product of the Kamehameha reform. Thomas says that people could easily have seen a connection between Lono's mast and canoe and Cook's ships. I assume he means metaphorically; not iconically (which does require perceptual coordination), nor metonymically. He cites an example from a New Guinea cargo cult where a leader invented a model of an engine-powered ship. All I can say is there is no particular reason why my commonsense argument is better or worse than his noncommonsensical one. I was protesting against the larger issue of attributing to the native the absence of discrimination that you and I have on the basis of ordinary perceptual and cognitive mechanisms. I do not think that this issue could be resolved in this kind of debate except to reaffirm each person's respective position. Thomas's specific Melanesian example leaves me at a loss; if the example is strictly isomorphic with that of the Hawaiian, then the Melanesian innovator must have invented a replica of the ship without having seen a modern ship or heard of one. I just don't believe this. A further metonymic point that both Thomas and I might have ignored: Isn't it just possible that the Hawaiians had seen Spanish ships and then represented Lono's canoe and mast on that model? Cook and his officers certainly thought that Hawaiians had known Spanish contact.

The other examples of Obeyesekere's "commonsense sorcery" are, I think, totally gratuitous. He says that I insist "that because the British were white, they couldn't have been presumed to be Hawaiian deities" (above, p. 120). I never put it in quite this way! On the contrary, I say that whiteness was relevant for the Hawaiians in their relationship to Cook, witness the aid that **Kalani'ōpu'u** sought in the Maui campaign in order to balance the priest of the "white" lineage that joined his enemy's forces (p. 81). I do say, though, that Cook's whiteness, and the fact that he did not speak Hawaiian and was unfamiliar with Hawaiian life-ways, combined with numerous other events that I conceptualized as

“situational overdetermination,” rendered it impossible for Hawaiians to mistake him for their god Lono. The preoccupation with whiteness is Thomas’s hang-up, not mine. So is his idea that Hawaiian and South Asian conceptions of divinity were dissimilar because the latter affirms a radical distance between man and deity. I thought I said the very opposite: that South Asian concepts of deity-naming could range from radical distance to simple familiarity, as for example when the word for “god” is extended to a variety of contexts exactly as in Hawaii. Kame’eleihiwa got me right when she quoted me: “the native can make all sorts of subtle discriminations in his field of beliefs; the outsider-anthropologist practicing a reverse form of discrimination cannot” (pp. 21-22). And finally I must protest at Thomas’s put-down of another native, Te Rangi Hiroa, who, he says, “is not authoritative at the best of times and conspicuously unreliable for Polynesians other than Maori” (above, p. 122). Couldn’t Te Rangi Hiroa as a Polynesian have had some insight into Hawaiian lifeways without being “authoritative” in Thomas’s sense? Does Thomas ever ask himself: What kind of authoritarianism impels him to make statements of this sort? This style of “put-down” is found elsewhere also: in respect to the chief priest Omiah or Lono; in the “Commonsense Sorcery” title of his review of my book; in his reference to my “crude historiographic rule”; in the gratuitous and simplistic misrepresentation of my views; and in the transparent rhetorical ploy of taking a minor issue (Cook’s death) and treating it as the centerpiece of my work. I wonder what went on that affected the tone of the present review as compared to the more balanced and yet critical judgments he made in *Current Anthropology*?

Perhaps the most reasoned scholarly critique of my book is Valerio Valeri’s. I regret that it is not possible for me to respond to the details of his argument because I do not have his or any other Hawaiian texts with me. Let me respond to the more serious criticisms and then deal with what I consider some of the methodological problems in his own position, including the “voiced and unvoiced presuppositions” that any text contains.

The key event as Valeri sees it is the ritual at Hikiau where Cook was installed. He says that both Sahlins and I make partially true and partially false interpretations of this ritual, but that he has it right. There is not a trace of doubt, methodological and substantive, in his argument and, for the most part, the authorities he cites in his bibliography are himself. No wonder when Charlot disagrees with him he comes down harshly on his critic: “In this he [Obeyesekere] appears to follow Charlot’s opinion. But the unconscious Christian projections that are

perhaps understandable in an exseminarian are much less so in a South Asian" (above, p. 133, n. 11). Someone who writes in this vein must surely think that he is exempt from "unconscious projections." I wonder what Valeri thinks of all the seminarians, theologians, Christian believers, and rabbis in classical Western philosophy (from old Descartes down to Gadamer in our own times) and their contributions to knowledge. I find this authoritarianism rather difficult to take; Valeri himself is quite unaware of it as he arbitrates on the truth of Cook's apotheosis. He can make statements like "Sahlins is closer to the truth," of which he (Valeri) is the ultimate knower. Everywhere in this text, as in his major work, he refers to the way Hawaiians think, to Hawaiian ideas, to the rules of their ritual, but the authority is himself. Here then is my fundamental critique of Valeri's review and of his work in general. In *Kingship and Sacrifice* he has invented the rules by which Hawaiians acted and these rules are derived from texts that were available during and after Kamehameha's reform. On the basis of these texts, whose contexts are at best difficult to reconstruct, he has reconstituted kingship and sacrifice in Hawai'i. Now there is nothing wrong with this if he would recognize the invented character of his ethnography and permit spaces for other kinds of interpretations, such as that of the seminarian Charlot's. There are no extant Hawaiian texts that were composed during the period of Cook's arrival and it is, I think, fallacious to assume that he can speak authoritatively about what went on at that time. But then am I not guilty as well of the same lapse? Perhaps, but not as badly. . . . To make this clear let me deal initially with the significance of the Cook = Lono equation for Valeri and myself, and of course for Sahlins, and then deal with another key event, namely Cook's "installation ritual" at Hikiau.

Regarding the Cook = Lono equation, let me briefly summarize our respective hypotheses. For Sahlins Cook is the god Lono who comes in person to the Makahiki festival. I took the position that Cook was called Lono due to the "situational overdetermination" that prevailed at the time of his visit and depicted some of those overdetermined elements (p. 97). I did not say what Valeri imputes to me, namely, that Cook was called Lono simply because he arrived during the Makahiki, though this was a critically important element in the situation. Because of its importance let me contrast my own interpretation of Cook's being called Lono with Valeri's in order to highlight the different strategies of research and writing ethnography.

When Cook arrived in Hawai'i on his *second* visit during the Makahiki festival for the god Lono, the ships' journals everywhere stated that

he was called Lono. But here's a serious problem: When he *first* arrived in Kaua'i and Ni'ihau a year before, not one journal account mentions that he was called Lono. If indeed people like Thomas are correct that Hawaiians saw a consonance between Lono's canoe set afloat at Makahiki and Cooks ships, then Hawaiians must have been peculiarly myopic during this first visit. What then is the crucial difference between the two trips? I have argued that, contrary to Sahlins, there is not the slightest evidence that there was a Makahiki going on during this first visit; but suggested that Cook did arrive at Makahiki time during the second visit (adding that the festival was suspended owing to the difficulty of maintaining the integrity of Hawaiian tabus and other religious beliefs). There seems to me, then, good cause for surmising that it is this crucial event of his arrival during Makahiki that led Hawaiians to call him Lono (in the larger context of "situational overdetermination").

Now take Valeri's argument: For him these inferences from the empirical data of the journals do not matter; what matters is Hawaiian thought that he has elucidated, in this case the ontological significance of why Cook was called Lono. Names are serious business in Hawai'i, he says: "Name, rather than character, was destiny in this society." He then adds that for Obeyesekere the appellation "Lono" given to Cook was "just a name" very much like his own, Gananath (something I never said). To substantiate his argument he uses the famous Hawaiian compendium, *Nana i ke Kumu*, which is the very source that I use to uphold my position that Hawaiians, like many others, name people after important events! It seems to me that to name people after events is not a matter of "just a name," but, as the authors of the above-mentioned compendium state, a serious naming procedure, because events *are* important, even if they are not always destiny. Hawaiians did not practice an exclusive naming procedure either. It is Valeri who posits one kind of naming. The ethnographic evidence suggests that Hawaiians did change names often and even possessed multiple names, and it is likely there were multiple naming procedures also (pp. 92-93). Here then is another problem with Valeri: He can only see a single scenario in Hawaiian beliefs, and that must be one that he has isolated because Hawaiians are rigidly rule-bound like other "islands of history" that ethnographers study.

Now to come to poor Obeyesekere's parents. In general in Sri Lanka, upper status groups are very particular about naming based on astrological computations, such that words and sounds have planetary potency. But they also adopt *other* naming procedures, for example, to commemorate events. Thus, my father studied in Calcutta under a famous

teacher, Gananatha Sen. He gave me his teacher's name not only to remember that personage but also, he told me, to commemorate the crucial educational event in his life. Another example: A famous Sri Lankan public servant is named Bradman Weerakoon because the day he was born was also the day the famous English cricketer, Don Bradman, arrived in Colombo. Anyone familiar with the triumph of British colonialism through cricket can appreciate the significance of the name and the event it commemorates. Further, the surnames of Sri Lankans nowadays simply follow the English naming habit of putting the surname at the end and the personal name in front, a complete reversal of traditional practice, but adopted within a few years of British rule. Thus emerges another invention, not to mention the many more naming procedures based on caste and class that I cannot even begin to enumerate.

I suggest that Hawaiians are the brothers and sisters of the Sri Lankans in this regard. To be fair to Valeri, he does not say that Cook/Lono was strictly the *god* Lono as Sahlins claims. He adopts a safe middle ground: He was not the god Lono arrived in person at the Makahiki, but rather he was hailed as a (second) king and in Hawai'i kings are gods! He has got us back to the European idea of the divinity of kings; consequently Cook is not a real god but "a quasi-image of Lono" (above, p. 130). But this kind of argument inevitably lands him in a terminological morass, for what on earth does he mean by "a quasi-image of Lono," and is there such a Hawaiian conception? I would suggest that in this context the phrase "quasi-quasi-image of Lono" sounds more appropriate.

Now let me make a more detailed critique of Valeri that will further illustrate the methodological issues I raised, this time in relation to the ritual in which Cook was installed as a god, a chief, or a king.

1. I like the part of his argument that says the ritual at Hikiau was created or invented by the Hawaiians to deal with Cook's unexpected arrival, which is my argument also. Yet Valeri undermines his own thesis by saying that while there is precedent for a king to be installed, there "is no mention by the canonical Hawaiian antiquarians [I love this phrase] . . . of a ceremony for installing . . . a person of rank' unless there was navel cutting or circumcision. He then adds, without a trace of humor: "I cannot find any trace [of navel cutting and circumcision] in the ritual undergone by Cook" (above, p. 128). But were Hawaiians as rigid as the scholar who interprets their lifeways? Surely what Valeri does is to deny Hawaiians the capacity for creative improvisation in an unusual historical situation. My puzzlement does not end here. Why

should Hawaiians install Cook as a king simply because that is the only installation ritual they knew, particularly when they already had a divine king, Kalani'opu'u? This objection, incidentally, would not apply to Cooks being installed as a chief or, even as **Lilikalā** Kame'eiehiwa suggests, as a priest, because there were many of them around. Having another king could not possibly bring order into Hawaiian-English relations that Valeri also thinks is important, but would produce its very opposite. Moreover, such a notion of kingship cum divinity must contend with what Beaglehole correctly pointed out-- that even ordinary Hawaiians did not accord Cook ceremonies of prostration in the very realm that Cook held sway, namely his ship (p. 121). A strange divinity indeed!

2. In the ritual at Hikiau an important sequence occurred that is significant to both Valeri and to me but not to Sahlins, namely, at a critical point the priest prostrates himself before the god Kii and urges Cook to do the same. I suggested that Cook is made to acknowledge the superiority of the Hawaiian gods and this act indicates his subservience to them. Valeri thinks this cannot be and that Cook only kissed the deity, an act that does not indicate subservience at all. Here is King's description: "to this [image of **Kū**, Koah the priest] prostrated himself, and afterwards kiss'd, and desired the Captain to do the same, who was quite passive, and suffered Koah to do with him as he chose" (cited in *Apotheosis*, p. 84). Valeri's Cook could not have prostrated himself because "I cannot see Cook, even at his most passive, prostrate himself with his men watching" (above, p. 135, n. 25). Here I am confronted with a stone wall of prejudice; it is impossible for these scholars to shed their idealization of Cook even when the text clearly says that Cook was passive and *suffered Koah to do with him as he chose*. Valeri forgets that in Tonga Cook stripped himself to the waist to participate in the Inasi ritual in the presence of ordinary seamen who were in fact offended at what they thought was indecorous behavior in their captain. Also, to Lieutenant Williamson's disgust, Cook liked to sit on the floor and drink pre-masticated kava with Tongan chiefs. In Tahiti, in his own ship, he got himself stripped naked by several women for a massage to cure his "rheumatism," something that the crew would have known and commented on. Moreover, contrary to Valeri, there were no "men" present at the installation ritual, only two officers, King and Bayly. And surely we know from a variety of instances that even aristocratic European emissaries had to bow, kneel, or prostrate themselves before sacred non-European kings. Not only this: At a critical point in the ritual Cook is

made to raise his hands in an act of worship of the Hawaiian gods (p. 84), something that Valeri's Cook does not do.

3. It seems to me that this ritual is difficult to interpret in every detail owing to King's ignorance of its meanings. I myself resisted any attempt at definitiveness, stating that "the very possibility of a plausible alternate interpretation is at the very least a demonstration of the folly of attempting any rigid interpretation of symbolic forms" (p. 82). I think Valeri's own interpretation of details is subject to the same strictures he makes of mine, but he does not subscribe to my methodological proviso. Consider the following example: The priests offer to the god **Kū** a hog and other items and hail him as "O Lono" and then give Cook and the other officers portions of the hog to eat but only after they had consumed kava. Cook found it difficult to swallow any of the "putrid hog." Valeri's response to my interpretation: "Obeyesekere's imaginative suppositions notwithstanding [this ritual] . . . resembles only one Hawaiian rite" and that is the one performed at Makahiki in which "the image of Lono as god of the Makahiki festival was consecrated by the feeding of his bearer" (above, p. 129). There are serious implications here: The underlying assumption is that of bricolage, where the savage, like the artisan in Western culture, can only put together elements of an existing repertoire of knowledge, rather than reinventing anything. Savage creativity is of a strictly limited kind. It cannot occur to Valeri: (a) that feeding of persons from a sacrificial offering is quite common cross-culturally and perhaps even common in Hawai'i, and (b) that this element could have been given a different creative meaning specific to the ritual at hand rather than as a replication or bricolage of existing elements from another ritual (the knowledge of which is based on the accident that it happened to be the one recorded by canonical antiquarians). Further, if Valeri is right, it does not make sense for Bayly and King also to be given these same foods unless they too were refractions of the god Lono.

For Valeri (as it is for Sahlins and Thomas) even the half-starved Englishmen who appeared in Hawai'i resembled hungry-looking Hawaiian gods! But once again note the empirical issue: The Hawaiians told King that the English came from a land where food supplies had run out and hence their greedy consumption of the food given to them (p. 63). Not so, Valeri tells us; King and the Hawaiians are both wrong because the English emaciation was consonant with the Hawaiian image of gods with greedy appetites and enormous gaping mouths (though this isn't quite how the English looked). Unfortunately Valeri

doesn't give examples of Hawaiian gods who are also dirty and do not bathe but this, I imagine, is simply due to forgetfulness. Once again note Valeri's strategy: All you need to know is Hawaiian thought, narrowed down to mean mythic thought, and you can ignore the Hawaiian voices that King recorded. But look at the other side of the coin: If King's Hawaiians had said that the British looked hale and hearty (or whatever), Valeri could still say, "Sure, they are divinities because there are Hawaiian gods who look hale and hearty (or whatever)"; and indeed there are such deities no doubt. Valeri does not realize that he comes perilously close to making precisely the kind of assumption based on ordinary-sense perception, namely, that the hungry British were correctly perceived on the model of hungry gods.

But in an endnote Valeri moves away from this: Obeyesekere was wrong in saying that Cook, who did not look Hawaiian and spoke no Hawaiian, couldn't possibly be one of their gods, because Hawaiian deities appear with distorted features "and, in one case, with a nose in the shape of a pig [sic]" (above, p. 134, n. 18). He seems to have forgotten two things. First, aren't we now talking about one particular god, Lono, and not Hawaiian gods in general? Second, are there not myths and stories of Lono that say he looked Hawaiian and talked Hawaiian and in fact lived in Hawai'i? What is the relationship between sculpture and varieties of narrative representation? It seems to me that we once again have Valeri's typical strategy of freezing Hawaiian thought into a single scenario. Can you now blame me for insisting that Hawaiians could perceive the external world as you and I do and that they could in fact see that the British were hungry and emaciated, and that they empathized with the Englishmen's plight because hunger and dirt were familiar to them as part of their ordinary human experience? With Valeri we are, I regret to say, in the realm of "anthropologism," a very sophisticated ethnographic counterpart of Orientalism.

4. An important part of my thesis was the strong political motivation for Kalani'opu'u to incorporate Cook and his officers and crew into the Hawaiian social structure as "chiefs" because, among other things, he wanted their aid in his flagging war with Maui. Here is Valeri's response: Obeyesekere's hypothesis, he says, is based on no evidence whatever and to say that proof of the hypothesis is contained in the lost part of Cook's journal is implausible. What do I say though? I suggested that there isn't always enough evidence to determine what went on during that fateful period and therefore whatever evidence available has to be "imaginatively re-ethnographized" (p. xiv). I then adopted the following strategy. First, I showed that wherever Cook went in Polynesia

native peoples solicited his help in their internecine wars, not surprising given the superior firepower he possessed. I then say that, at the time of his arrival in Hawai'i, there was a war being waged between Maui and Hawai'i island in which the latter was being worsted. I based my information on Fornander's account, which is based on a "canonical" authority, Kamakau. And most importantly, after Cook's death, when the ships were on their way home, chiefs of Kaua'i sought Captain Clerke's help in their domestic wars. I suggested that, given these reasonable Polynesian and Hawaiian attitudes, it would have been surprising if Kalani'opu'u, as a good military strategist, did not seek Cook's help in his worsening conflict with Maui. I also showed that iron was mostly sought by Hawaiians to make daggers, once again useful for the same purpose.

For me this is a reasonable way of "re-ethnographizing" the situation, particularly when I explicitly state that in the absence of direct evidence one has to seek "indirect evidence" (p. 78). If one were to discount indirect evidence as "no evidence," then practically all of *Kingship and Sacrifice* would be valueless, depending as it does almost entirely on indirect evidence. I then, somewhat facetiously, recognize that there is no way of "proving" this hypothesis, unless one could recover Cook's journals for this period, which for some reason were the only ones to be "lost" by the admiralty (p. 216, n. 29). Valeri makes a big thing of Cook's lost notes and says that "[i]f such a request [for aid] had been made, the other officers would have recorded it in their own journals, as they did on other occasions" (above, p. 131). I detect here again the very emergence of common sense that he decries in my work. He assumes that Kalani'opu'u, like a good Western commander, would have had a joint conference with Cook and his other officers rather than talking to Cook alone as the only one who could have measured up to his [Kalani'opu'u's] own status; he then commonsensically assumes that Cook would have consulted his officers on these matters. Therefore the evidence should be available in other officers' journals (though I document everywhere in my book that, at this time, and in this voyage in general, Cook rarely consulted his officers owing to his increasing moodiness). In fact, I show that while some opinions are collectively formed, others are not; and that some journals record evidence not found in others. My guess, because here in Sri Lanka I do not have the journals with me, is that references to Polynesians seeking Cook's help is found, for the most part, in the journals of the two captains and not in those of their fellow officers. Commonsense assumptions such as the above are scattered all over Valeri's work. I suggest that it is impossible

to write an ethnography or history without the writer's commonsense (and other) assumptions implicitly affecting his or her writing. This propensity is not just a vice of seminarians.

Let me end this critique on a slightly upbeat note. I think Valeri in effect totally destroys his colleague Sahlins's hypothesis. He says: "Probably, there was nothing preordained about Cook's identity: The performance of rituals was an attempt to orient and fix this identity in a direction favorable to the Hawaiians" (above, p. 130). This is what I say also, though the substance of our interpretations is different. For Sahlins all this is preordained and Cook is the god Lono who comes in person during the Makahiki festival as predicted by Hawaiian prophecies (and their ritual canoes) and this is central to his whole thesis. It does Sahlins no good to say, as Valeri does, that Cook was installed as a king and he was a god by virtue of being a king. Though I agree with Charlot that one must not confuse European ideas of divine kingship with the Hawaiian, I also suggested that the installation ceremony might well have given a sacredness or *mana* to the chiefly officers (p. 86; appendix 2, pp. 197-199). To me it is important to affirm that Cook was *not* the god Lono arriving in person to a savage land because that is a myth of the long run in *European* culture and consciousness, and not a Hawaiian one at all. This Valeri, I think, does not dispute. Instead he disputes my specific tracing of this European myth and suggests, among other things, that it is more accurately reflected in Alexander of Macedon. This is possible; so is Julius Caesar and other civilizers of European myth. Actually, I myself stated that this "cultural structure occurs against a larger background of ancient Indo-European values" (p. 124). Nevertheless, to trace it specifically to Alexander, I think, can only lead us to a sterile scholasticism unless Valeri can demonstrate how the Alexandrian myth affected the lifeways of sailors during the voyages of discovery, unlike the Cortés myth that did affect them, as it did the Enlightenment in general, for example, in Cowper's verses compiled after Cook's second voyage comparing the good Cook to the evil Cortés (see text of Cowper's poem in *Apotheosis*, p. 223). I think I am right: The Cook myth is a manifestation of a more general type found in European thought; more specifically, it is a myth of the Enlightenment, an example of that which is believed to evade mythic thought, namely rationality itself, the credo of the Enlightenment.

Once we move beyond Valeri's Alexandrian reference, there are other areas of agreement also. After all, I do say that Cook was deified after his death in conformity with Hawaiian custom. Thus it is not correct to say that I deny the idea of the "return of Cook"; I only say that there is

no single scenario involved there either and several scenarios are possible. It may be that Valeri is right in his critique that some of the scenarios I have sketched are unconvincing, but that is not proved by his simply asserting so on the basis of his interpretation of Hawaiian thought. That thought is multiple, even contradictory; I think I am right to affirm that it is an error to freeze Hawaiian thought into a single scenario. Valeri protests with some pique that, in a 1990 paper, he also has documented the contentious nature of Hawaiian discourse before I did. But what good is it if Valeri still confines Hawaiian thought to a set of rules that he has devised for it on the basis of shakily contextualized "myths" and "rituals," as if those are the only sources of "thought"? If Valeri had read *my* 1990 book, *The Work of Culture*, that dealt with the theoretical significance of contentious discourses or debates in myth and history in general, he might have developed a more loose or open view of Hawaiian thought. And what about agency, which Valeri ignores in his work and mine? For example, is Cook's Kurtz persona irrelevant to understanding the events that occurred in Hawai'i and in Europe in the eighteenth century? Finally, I think it is not only necessary to unfreeze the world of the native and open up the multiple worlds contained therein, but it is also necessary to perform a parallel act and open up the closed, boxed-in world of ethnographic theorists, particularly those who draw chalk circles around islands of history and thereby unwittingly esotericize those cultures, ignoring human suffering and pain. And also, I might add, bypass those deadly events that occurred in the aftermath of violent contact, colonization, or conquest on which Kame'eleihiwa justly looks back in anger.