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Historical Anthropology and the Politics of Critique

> Cavilling here and there at some Expression, or little incident of my Discourse, is not an answer to my Book
>
> --Locke

In the several years since I wrote *Out of Time* I have been concerned with rather different projects, and thus have a certain distance from the work that is not only temporal but conceptual. I am conscious of various deficiencies, and now feel, for example, that rather than confining my discussion to Polynesia, it would have been useful to have considered the ways in which anthropological dealings with histories and with historical sources emerged in the scholarly traditions of other regions. I wonder also whether I should have been more explicit about the way in which the book sought to cut across genre distinctions in the social sciences, by trying to force questions of methodology, substantive interpretation, disciplinary history, and theory to react against one another.

Certainly, it would seem from these reviews that I failed to make my intentions clear; but what is puzzling is that many other readers and reviewers found the book readable, straightforward, and informative.[1] In other words, while I might have expected *Out of Time* to be most accessible to scholars familiar with the Pacific, it emerges that some of them find it much harder to deal with than nonspecialist readers. Confusion and intelligibility are, however, not so much properties of either texts or persons as states that emerge in the relation between them, in particular readings; and while there are no doubt passages in my text that should have been more orderly or coherent, there may be good reasons why Oceanic specialists such as Richard Parmentier and Bradd Shore need to discover confusion in the book, which also account for their rather singular constructions of its project and argument. As we all know, it is no longer appropriate to claim that there is a single true rendering of any event or a single authoritative interpretation of a text. But this fact does not preclude assessments of competing readings; I would insist that some interpretations may be not only less adequate than others, but simply wrong, and it appears that because of their commitments to the disciplinary procedures and protocols that *Out of Time* interrogates, Parmentier and Shore are obliged to distort the text they anathematize in a distinctly extravagant way.

I begin here by briefly summarizing the books argument. It was concerned with the relation between overt theoretical interests, particularly with the historicization of anthropology, and paradigmatic features of the discipline that might be at odds with those interests. I suggested that the problem arose in the first place because the professionalization of anthropology, in both its British and American forms, was premised on radical exclusions that enabled specialists to establish a monopoly of competence, particularly by sealing off their subject matter from historical contingency, and secondly by dismissing or marginalizing nonprofessional ethnographic sources. This provided a background to more specific arguments about the ways in which history had been neglected or suppressed in Polynesian anthropology, in part for these general reasons and partly because of certain features of the interpretation of Polynesian hierarchy and transformation. The critique at this point worked in two directions, establishing that ahistorical ethnography led to particular misinterpretations, but also more generally that theory-laden sources carried a burden of interpretation that was often not appreciated or disarticulated as the sources were incorporated into another theoretical edifice.

Another strand of the argument was that the repudiation of evolutionary theories, or of generalized notions of unilinear evolution, did not prevent arguments from being saturated with metaphors and connotations that implied progressivist development, archaic/advanced juxtapositions, and equations between remoteness in time and difference in space. I argued that despite the interest of Sahlins's structural history, and the extent to which it did help build a more historical anthropology, it remained inflected by evolutionary discriminations of this kind; the extent to which it actually historicized indigenous culture was also rather limited. These critiques led to certain recommendations, for example, that in the Oceanic case any interest in precontact social forms had to consider archaeological evidence more seriously, though without succumbing to the crude materialism and positivism that is still surprisingly widely encountered in that discipline.

More generally, I suggested that ethnographic fieldwork needed to be decentered--meaning not that it should not be done, but that it should no longer occupy a sovereign place as the source of anthropological evidence. Since these wider conclusions are very much at issue in this debate, I discuss them further below.

Histories

A book that dealt with such a range of themes obviously needed to delimit its subject matter in various ways. Hence I made it quite clear that I did not venture into debates about historical representation, about what history is, in the abstract. David Hanlon considers that any inquiry of the kind I engage in "requires meticulous definition of the key concepts involved." He notes that, far from doing this, I intentionally and deliberately avoided defining "history" and "evolution" on the grounds that meanings subsist in the uses of ideas in texts, thus regrettably leaving "the reader to manage the intellectual void and confusion that result." Setting aside the interesting question of how one can produce *both* void and confusion, this strikes me as rather like an ethnographic situation in which an inquisitive stranger might ask about the meaning of a word such as "politics" or "democracy," and you respond by suggesting that you can take the visitor to political meetings and watch television news programs in which these words are used frequently; exposure to the discourse, you suggest, will give the inquirer a sense of the sorts of things that "politics" can signal or allude to. No, says the visitor, I want *you* to tell me what it means, to give me a definition While it is entirely understandable that glosses and cognitive maps are demanded (cf. Bourdieu 1977:2), it is evident that definitions, whether of "politics," "history," or "evolution," are spurious and unhelpful. The terms have been used in eclectic and contested ways, and the important exercise entails tracing those uncertainties, rather than attempting to legislate them. To expect that a certain definition of a word--advanced, say, in a preface-- actually encloses or determines its meanings in the remainder of any particular text, or its meanings as registered by readers, seems to entail a peculiarly rationalistic view of the ways in which written arguments are ordered. Even if a concept is a neologism or theoretical novelty, and thus might seem less susceptible to "misreading," one cannot presume that its meanings are coherent, that it has the same value or function wherever it is deployed.[2] I suggest that if one is situating certain problems in the prior meanings of concepts, it becomes important not to declare redefinitions in a flag-waving fashion, but to implement a different usage in analytical practice. My stance on this owed something to later Wittgenstein, and I would go back to his work if it seemed worth debating the abstract issue at greater length.

I am puzzled by the suggestion that I conflate history with mere temporality, since at several points I argued that some social theorists have effectively temporalized systemic models (generally by privileging some conception of reproduction) but did not effectively historicize them; that is, they did not create terms that would enable us to understand culture or social relations as historically constituted. What was pivotal to this argument, obviously, is the assumption that time and history are entirely different. Hanlon proceeds to raise the question of whether my references to "real history" and "actual history" imply regression to a naive idea of "what really happened" that would seem oblivious of all the reflection upon historical method and interpretation since Carr. But what these words meant is quite clear from the context:[3] in one case I was juxtaposing historical social transformations with the kind of transitions imagined in evolutionary narratives, and in the other I was differentiating a historical anthropology from one that had merely been installed with a certain temporality. Though my adjectives were evidently injudicious, I cannot apologize for suggesting an important difference between the transformations imputed in a general model of the progression from chiefdoms to states and those postulated or reconstructed in an inquiry into the social transformation of, say, Tongan or Marquesan society over the last thousand years. In this case, "real history" means particular history as opposed to a generalized or conjectural scheme. This is not to say that I take my reconstructions of such histories to be less theorized, or to somehow reflect developments in a less-mediated fashion, than an explicit or implicit evolutionary model; but it does say that one has an object of quite a different kind to the other.

In fact, I think that both my general meanings of history and evolution and the direction of argument were made quite clear in the opening discussion, where I noted that there was "a tension between former characterizations--in which history is an empirical succession of events and evolution refers to progressive development--and a merging of meanings in a more satisfactory analysis of change which is processual and systemic but neither directed nor abstracted" (pp. 3-4). In other words, what I was proposing was that neither conventional narrative history nor evolutionary anthropology could constitute an adequate historical account, if one's interest was in the short- and long-term dynamics of social forms. These juxtaposed discourses could, however, be superseded by a kind of history that was systemic (and hence interested in structures of meaning, political dynamics, and the expression of structural change in immediate events and representations), that thus appropriated the deterministic character of evolutionary argument, and its interest in larger dynamics and transformations, while repudiating its teleologies and directionality--features that were unsatisfactory both on analytic grounds and because of their ideological implications.

I am also perplexed by the claim that my use of archaeology in reinterpreting eastern Polynesian social transformations leads to an account that is chronological and heavily dependent "upon simple linear developments" rather than historical in any admissible sense. I am fully aware of the reductive and theoretically unsatisfactory character of much of the archaeological literature (and had written earlier on this topic myself), but its limitations do not make it irrelevant or wholly unusable from the perspective of social history or anthropology. Of course, there is no general question about whether archaeological data are useful or not that can be posed in the absence of a particular agenda or theoretical problem. My questions concerned how indigenous societies and political hierarchies on Rapanui, in the Marquesas, and elsewhere had developed, with the distinctive results that were apparent in the early contact period. What I extracted from archaeology was not merely chronology, but implications of social processes and various correlates of political and ritual transformations. For example, the elaboration and expansion of Marquesan *tohua* grounds suggested that competitive feasting might have expanded dramatically at the same time as various other developments, such as accelerated ecological degradation, intensified warfare, and so on; with respect to Rapanui, it was important to establish that indigenous society had not always been the battlefield apparent in the nineteenth century, that the particular features of that disorder owed something to the more stable hierarchy that preceded it. In other words, these discussions were concerned with transformations of social orders, which did impinge very substantially on the conditions of life that Hanlon suggests I neglect. I was, for instance, trying to account for the different configurations of ritual authority in the Marquesas and elsewhere, and the extent to which food supplies and lived hierarchical relationships were far less secure on Rapanui and Niue than elsewhere in Polynesia.

The underlying problem, I sense, is not that there is a coherent complaint about what this investigation produces, but that Hanlon believes I ought to be doing something different. My text "evidences no appreciation for the ways in which other societies might construe, express, and utilize a very different sense of time"; having my own culturally determined sense of time, I moreover effectively suggest "that others' pasts can be discerned, charted, and understood through Western notions of change over and in time." So, far from breaking from an evolutionary argument, I inadvertently promote it by "limiting anthropological understanding to a *very specific* cultural understanding of chronology and sequence" (emphasis added). In other words, the suggestion here is that *because* I am "oblivious to or unconcerned with local conceptions of time," my analysis cannot transcend the evolutionary problematic that it criticizes; Hanlon thus stipulates that a real theoretical break can only derive from an appreciation of non-Western temporalities. What is unacceptable here is the homogenization of "Western" notions of change and time and their conflation with "the existing evolutionary paradigm"; obviously, ideas of contingency, time, and historical change in European thought have been enormously diverse (see, e.g., Pocock 1975), and it is not *in principle* necessary to step outside that tradition in order to criticize or reconstruct particular ways of representing histories and social transformations (however desirable it may be from the stance of particular arguments). Of course, I nowhere claim that my conceptions of change and history are not culturally informed, and insofar as they are, they are obviously peculiarly constrained and inflected to the same extent as any other cultural representation. So what? Is this not also true of accounts that do take an interest in others' constructions of their histories, that after all have often been accused of ventriloquism, of purporting to present others' voices while structuring and enframing what they say?

It is beside the point to claim that *Out of Time* "gives little attention or credibility to indigenous sources and modes of historical expression" since this was a task that the book explicitly bracketed off. Hanlon makes the point that in challenging Handy's account of Marquesan society, I attempted to "discredit" his informants' memories, but neglects to consider the context of my argument. I was considering specifically whether the salvage ethnography Handy engaged in during 1920-1921 could provide an adequate account of precontact (late eighteenth-century) social relations, that is, the "native culture" prior to transformations attendant upon European contact and dispossession. I was discrediting the method, not the indigenous knowledge; Handy himself accorded no importance to indigenous constructions of the history, but was merely using memories to answer ethnological questions. If it is these ethnological questions--concerning, say, the nature and significance of chieftainship-- that are at issue, I would suggest that what islanders were reported to have said in 1800 is more useful than what their descendants were reported to have said 120 years later, which I wouldn't expect to be more reliable than an account I could give of social circumstances during my grandfather's infancy. I would suggest, moreover, that if we are concerned with such questions as the character of chiefly or shamanic agency, it is most important that practices and events--the circumstances under which cultural precepts are put at risk and contested--are examined. This can only be done by working through early contact descriptions, obviously in a fashion that reads them critically, taking account of the interests of observers and authors. Indigenous recollections are in principle an equally important source for practical contests and other events, and are likely to be more important to the extent that the outsider often has a poor grasp of the cultural dimensions. However, the type of information obtained by Handy mostly took the form of generalized statements rather than particular narratives that might have enabled a more nuanced understanding (as is apparent not only from his publication but also from his field notes). More importantly, these informants were simply too distant in time to be in a position to give an account of the practices and relations of the precontact or early contact period. This is, moreover, a context in which it is problematic to treat the accounts of modern islanders as "internal" and Europeans as "external." For contemporary islanders, who are mostly Christians, pagan ancestors may be stereotyped in a variety of ways that are almost equivalent to the "othering" imposed by early European observers. Without suggesting that their accounts are therefore uninteresting, I do contend only an ethnic or cultural essentialism can presume some organic continuity between modern views and those of indigenous people in the early contact period or before.

All of this, however, was an argument that specifically related to the reconstruction of indigenous social relations in places such as the Marquesas, where contact histories were comparatively long and also highly disruptive. It was not argued that this was the only appropriate historical project or that indigenous perceptions of the past were generally unimportant. Nor did I suggest that the wider issues that Hanlon alludes to concerning the problematic and contested character of historical representation could be passed over. The extent to which I have taken these issues seriously is to be measured from extensive discussions in separate publications (Thomas 1990a, 1990b, 1991a), not from *Out of Time,* which focused on the anthropological occlusion of history and stated that it did not deal with associated issues such as these.

The major charge--that I fail to tackle the problem of what history is--thus seems equivalent to a complaint that Hobsbawm's *Age of Empire, 1875-1914* does not tell us what we need to know about the age of capital, 1848-1875. Hanlon is an ethnohistorian whose book on Pohnpei I very much respect. But I think that in this context he is failing to acknowledge that one publication of mine had a particular agenda; its writing was not haunted by the question of Carr's that haunted his reading, but by a variety of other problems; instead of questioning whether the book effectively addressed the issues it did raise, he objects in effect that it does not deal with his preoccupations--though these, ironically, have also been preoccupations of mine in other contexts.

The Marquesas and Polynesian Social Transformations

Richard Parmentier makes a very basic mistake right at the beginning of his review, in assuming that *Out of Time* was motivated by some personal "anger" toward other Oceanic researchers and to the discipline of anthropology (an interpretation that no other reviewer has advanced). This leads him to read the book as a sort of anthology of personal attacks and to ignore or misconstrue its key arguments. He thus pays no attention to the central points of my critique of Sahlins--that the structural history model could account only for externally prompted, not endogenous, change; and that the argument could not provide an adequate account of transformed living conditions over the longer colonial period, being restricted in its effectiveness to the cultural dynamics associated with the relatively brief phase of early contact. These were obviously comments on Sahlins's *texts,* and *Historical Metaphors* and *Islands of History* in particular, yet Parmentier reads them as if I was imputing some reproachable lack of interest in Hawaiian living conditions to Sahlins personally. If critique of theory is only legible as critique of individuals, the preconditions for an adequate reading of my book, or any other work of contemporary analysis, are lacking.

This reduction is not peculiar to one moment of Parmentier's comment, but is also manifest in his attempt to defend Handy's work on the Marquesas. He states that I argue that Handy "was fooled into reconstructing a picture of early Marquesan society on the basis of informants' recollections." It is quite crucial that I nowhere suggest that Handy was "fooled" into producing an ahistorical distillation: my argument was that he produced precisely the sort of synchronic construct of "the native culture" that Bishop Museum modes of investigation were designed to produce (a construct only partly derived from informants' memories, though I did suggest that Handy attributed what was remembered to a generalized traditional culture rather than to the singular circumstances of the 1850s and 1860s). I pointed out that within the spectrum of Bishop Museum reports, Handy's effort would have to be regarded as a competent and unusually extensive description; for this reason, in fact, it stood as a good example for assessment.

Parmentier subsequently raises the issue of how I can be critical of those who use voyage writers and missionary sources carelessly, while claiming to overcome difficulties with such sources myself; for him, this manifests "Archimedean hubris," but the example of Handy's work makes it obvious that there is a simple and concrete difference of method. In suggesting that Marquesan society "was always of the very simplest order," that chiefs did not have elaborate powers over their people, Handy made no effort to investigate what individual chiefs actually had done; he did not refer to incidents in which their capacities were at risk or at issue. Instead he quoted the generalized impressions of certain early visitors, such as Krusenstern and Stewart, to the effect, for instance, that the government was "anything but monarchical" (Handy 1923:35). Now, it should be obvious that statements of that kind often neither refer to nor draw upon any especially elaborate knowledge of the manifold rights and capacities that constitute power; in the case of the missionary source, I suggested good reasons why the missionaries would understate the degree of chiefly control: they attributed the failure of their own project to a lack of centralized chiefly support. In contrast to this, my argument about the alternate character of chiefly power was based on a reading of events, in some cases on the basis of day-to-day accounts, that manifested the actual capacities of chiefs such as Keatonui and Iotete, and the ways in which these were represented by themselves and other islanders at the time. While Shore suggests that Krusenstern's statement that if a chief hit anyone "he would infallibly meet with a like return" requires more serious consideration than I accord it, he fails to note that this is merely an impression that was not based on observation or on definite information of any kind; to the contrary, it was no more routine for commoners to physically assault chiefs in the Marquesas than anywhere else in Polynesia.[4]

The central point of this discussion was not one that Parmentier alluded to, such as my reference to Handy's diffusionism (which I did not claim his book relied "heavily" on; such an argument would have undermined my emphasis on the museum's preoccupation with synchronic native culture). Rather, I was concerned that Handy fundamentally misrecognized Marquesan forms of property,[5] and was thus able to misrepresent Marquesan society primarily as a less centralized or stratified version of other eastern Polynesian systems rather than as a distinct and divergent development. This, in turn, is what makes it possible for Goldman to imagine "Open" Marquesan society as a possible precursor to "Stratified" societies, whereas if its specificities are recognized, it is apparent that Hawaiian- or Tahitian-type hierarchies cannot develop out of this form. Both Parmentier and Shore take exception to my suggestion that Handy saw the Marquesas as "relatively egalitarian" in relation to Tahiti and Hawaii, on the grounds that he did not use that word. Handy, however, alludes to "the communism and simple democratic nature of the tribe" (1923:35), which, if anything, carries rather stronger implications.[6] I do not accept that it is unscholarly to paraphrase a writer's usage, especially given that "communism" in this sense has dropped out of the anthropological vocabulary and is potentially misleading.

Parmentier's attempt to salvage Handy's account of the Marquesas as an "excellent ethnography" comes as something of a surprise. There are major misinterpretations concerning hierarchical forms: contrary to what Parmentier asserts,[7] Handy had no understanding that a system of *tapu* grades associated with particular forms of tattooing and exclusive eating fraternities existed, though this was the closest approximation to an encompassing hierarchy in the Marquesan polity. Among Handy's many careless assertions is the claim that unqualified individuals could rise to positions such as chief on the basis of achievement, which is not documented at all prior to the 1840s and not even common thereafter, even though chiefly positions were subject to direct interference from the French and had suffered from a loss of influence and prestige for a variety of reasons. A more specific example of the shortcomings of Handy's information is the question of the political unity of the island of 'Ua Pou. Though he noted correctly that the island was unified under one chief, that this was an indigenous development, and that the situation was unique in the group, he thought the unity was effected only at a late date: "About 1860, before European influence was really felt in Ua Pou, Te-iki-tai-uao . . . secured control of the whole island" (Handy 1923:31). In fact, it is clear, in part from documents that Handy himself used or had access to, that there was a line of paramount chiefs earlier, of whom three individuals can be identified by name (see Thomas 1990c:215-216); and, although it is not clear what European influence being "really felt" means, there was actually a good deal of missionary intervention before 1860 that was partly responsible for social instability by that date. In other words, even when Handy happens to be correct about a general point, he had not located the most relevant evidence and attached dubious significance to that which he did use.

Turning to my alleged misreadings of Goldman, Shore similarly misunderstands the exercise by interpreting *Out of Time* exclusively as a critique of Goldman's explicit argument, rather than also as a discussion of the theories and evolutionary adjudications implied in the text. Far from forcing Goldman's account into a straitjacket, I acknowledge and discuss the varying formulations of difference and change (pp. 35-36, 127-128), which I suggest are ambiguous and not fully coherent. It is clear, however, that Goldman sets up some societies as antecedents of others (the Maori of the eighteenth and nineteenth centuries being posited as a "Traditional" antecedent to other types) and, moreover, that he postulates an overall developmental categorization and sequence. Shore says that "Goldman does not claim that such transformations are inevitable in Polynesia, only that when internal transformations *did* occur, they were structurally constrained to occur in the predicted direction." I never suggested that Goldman assumed that Polynesian societies *had* to evolve toward the Stratified form. My argument relates mainly to the second general claim; I suggest that the Open/Traditional/Stratified categories are inadequate both for mapping Polynesian social variation and as a basis for theorizing transformations. The western and eastern Polynesian "Stratified" societies include Hawaii and Tonga, which can only suppress the significance of exchange and regional integration that gives the latter much of its distinctiveness as a form of hierarchical reproduction.

Secondly, Shore asserts that I argue that Marquesan society is simply different, as though I was merely pointing to some empirical exception to Goldman's scheme. It is clear, though, even from what he acknowledges himself (since he notes that I show that Easter Island accorded with a similar pattern), that I was suggesting Marquesan society exemplified a divergent transformational path, one in which shamanism, warrior dimensions of chiefship, other forms of *tapu* hierarchy, and nonchiefly based property relations became consequential and overshadowed the structures from which the eastern Polynesian type of "Stratified" society could develop. Contrary to the inadmissible suggestion that I attributed this pattern merely to postcontact developments,[8] Thomas 1990c contained a detailed argument that attempted to link these developments to Suggs's phases of Marquesan prehistory; this and the analogous argument for Rapanui was summarized in *Out of Time* (pp. 59-65). If either Shore or Parmentier really wanted to explore further the differences between my construction of the Marquesas and those of Handy and Goldman, they should have read *Marquesan Societies,* which presents much fuller ethnohistorical documentation and analysis.

Jonathan Friedman's comments are the only ones here that I find consequential or informative. Regarding his response to my criticism of the world-systems approach, I would concede that I imposed something of an empirical reduction onto a structural model; although I would also suggest that if empirical illustrations are being used to evoke structural processes, it is important whether the proposed model can, in fact, generate the range of variants that are adduced. Friedman's suggestions in the article I discussed (1981) and in this context concerning the distinctive character of the eastern Polynesian societies and the nature of western Polynesian contact history remain stimulating; rather than commenting further here I would prefer to revise my arguments more extensively elsewhere. Part of the problem is that much detailed ethnohistorical research remains to be done; and many accepted views--concerning, for example, the nature of relations within the Fiji-Samoa-Tonga triangle--might need to be reformulated in the light of closer analyses of the eighteenth- and nineteenth-century sources.

The Politics of Critique

The reader will, of course, have noted the generalized hostility that underlies Shore's and especially Parmentier's critiques. This leads to innumerable minor misrepresentations that add up to a travesty of *Out of Time.* I can only draw attention to a few of these by way of example. Shore follows Hanlon by complaining that I do not problematize history but neglects my explicit statement (p. 4) that that is a separate task (one I have in fact addressed in several other publications). He also suggests that my argument complains that ethnological discourse reinforces Western postulates of a historical self and a timeless other, though I allude to generalized ideological dimensions of evolutionary thought only in passing and never in that particular form. What Shore is doing is homogenizing certain critiques he evidently considers undesirable (myself, Fabian, "the current round of scholarly self-abuse," etc.) and casting them as crudely political. A bewildering aspect of this is Shore's claim that I seek "to cast doubt on the widely shared belief" of Polynesia's unity. What I actually say, referring to early perceptions of the shared background of Polynesian populations, is that "the basic insights remain far more credible than those of any other diffusionist scheme" (p. 31). Additionally, I am explicit that my arguments depend on an elaboration of Kirch's construct of "ancestral Polynesian society" (1984), but Shore is eager to find me "pronouncing the very notion of a culture area in Polynesia to be politically unacceptable"--a pronouncement that, needless to say, appears nowhere in *Out of Time.*

This attitude is even more extreme in Parmentier's comparisons between the book and Marquesan-warrior aggressiveness and cannibalism. This is so extraordinary that I can hardly find it insulting, but am disturbed to find that such hackneyed colonialist stereotypes of Marquesan behavior (for cannibalism was restricted to very specific ritual contexts indeed) remain current among Oceanic anthropologists, of all people. This predictably leads into a series of complaints that have no basis whatsoever in a competent reading of my text. For example, Parmentier complains that a number of authors are criticized in brief discussions, but (apart from attempting to salvage Handy) he makes no attempt to argue that these are *too* brief, that is, that their points are insufficiently substantiated. Beattie's work was, for instance, discussed briefly to illustrate the simple point that methods of investigation could encode theoretical and explanatory models. Why should twenty pages be devoted to this matter instead of two? Where Parmentier does comment on the interpretation of specific writers, what he says makes no reference to my main arguments (with respect to Sahlins, for instance); he merely asserts that those familiar with Sahlins's work will be "shocked' by what is claimed.[9] As if the arts of misreading have not been fully displayed, he proceeds to assert that for *Out of Time* "history" "does not include cultural categories, discursive forms, or semiotic records." In fact, what I set aside at the beginning of the book was the definition of history that was *limited* to the representation of the past; this did not mean that my view of the past *excluded* representations. Even the most cursory reading of the work in question, of *Marquesan Societies,* or of any of my other books could not sustain the view that the history I sought to construct was not both cultural and political.[10]

Parmentier obviously has a stake in the legitimacy of the whole edifice of Polynesian studies that leads to his hysterical characterizations of "cannibalistic" critique. Shore also revealingly notes that his response to *Out of Time* stems from his own high regard for Goldman's work, though he makes no effort to argue on the basis of his Samoan expertise that Goldman's construction of that society is informative or defensible. Had demolishing Goldman been my main concern, many factual or interpretative errors unconnected with the evolutionary issue might have been mentioned. As Judith Huntsman has recently noted, *Ancient Polynesian Society* "has been celebrated far beyond its merits as a basic source and major contribution-- even by scholars who should know better. It is seriously flawed in both conception and substance and far too many scholars through naïveté or laziness have allowed themselves to be misled by it" (Huntsman 1991:331). Responses of the Parmentier-Shore variety do not amount to "an answer to my Book" but merely express a subdiscipline's insecurity: my sort of critical discussion is unacceptable because it fails to defer before the profession's hall of fame (Handy is a "distinguished Polynesianist"). To take offense, to personalize the issue, to refrain from any critical engagement with currently authorized and established texts, are all part of a problem that can be explicated by the sociology of the academy, hardly peculiar to Pacific studies. Shore wittily suggests that my book is "out of tune," but in scholarly milieux of this kind--in which senses are dulled by the guild members' narcotic--discordant notes seem called for, indeed. Had the book been widely misread in the manner of these reviewers, I would be disturbed and disappointed; within the spectrum of responses that I have received, I can only situate Shore's and Parmentier's difficulties in their own defensive professional agendas; and, conscious as I am of the many ways in which the book might have been improved, I cannot say that their comments would prompt me to revise a word of the text. I am amused, however, that another reviewer found fault with the book because it had no radical content--most anthropologists would agree with everything it said!

NOTES

1. See, for example, Coronil 1991, Leaf 1991, and Roseberry 1991.

2. Consider, for instance, the rather various uses of the notion of "the structure of the conjuncture" in Sahlins's *Historical Metaphors* (1981).

3. In fact, the allusion to "the orderly march of people and their thoughts and doings" (p. 118) was not a gloss on history at all, but rather an ironic reference to functionalist-anthropological views of society, as is quite apparent from the context.

4. In the same way, Shore's defense of Goldman on the grounds that he did draw on what is called "naive ethnography" is not to the point. While true that Goldman cites many works other than the Bishop Museum bulletins, in most instances the latter are his key sources (see *Out of Time,* pp. 41-49 and 128 for detailed discussion). Goldman's account would have been more adequate if he had had access to a wider range of material, but adequate appreciation of that material would have required him to exercise greater contextual sensitivity, which would have separated out accounts relating to say 1800 and 1850 in various cases. In its homogenization of "native cultures" in each case, Goldman's approach to ahistorical distillation is much the same as that of the museum bulletins--though I do take the point that certain transformations such as the consolidation of the Pomare's authority in Tahiti lead to more specific and historically staged characterizations. But this is merely to point out that what Goldman should have attempted for all the Polynesian societies was gestured toward in two or three cases.

5. Parmentier correctly notes that Handy makes a few references to nonchiefly landowners, but these are exceptions at odds with his overall characterization. The general view Handy advances, that there was encompassing titular ownership on the part of the chief (1923:57), must be categorically rejected (for full discussion, see Thomas 1990c: ch. 3).

6. Parmentier also dismisses my comment on Handy's quotation of the missionary Stewart's reference to a "republic *en sauvage*": "after citing the passage from Stewart, Handy says *absolutely nothing*" (Parmentier's emphasis). What this ignores is the whole section in which Handy quotes a sequence of texts to establish the "communism" and simplicity of Marquesan society, to reject the view that Marquesan chiefs were in any way like kings (1923:35-36). I made it clear that Handy could not be taken to be making a simple identification between American and Marquesan egalitarianism, but that something of the sort was necessarily implied by his claim that American visitors could understand the society better than those from Europe: as distinct from European sailors "imbued with the European conception of kings and nobles and commoners," Porter "from republican America . . . speaks always of chiefs, never of kings" (1923:37). My point here was not, of course, that Marquesan chiefs actually were like kings, but that this axis of characterization was blind to distinctive forms of inequality in Marquesan society, which needed to be examined in the context of domesticity and property relations rather than with reference to political centralization.

7. If this is what he means when he refers to my view that "the operation of *tapu* was localized rather than regionalized."

8. This is linked with an argument that I overemphasize colonial "penetration," even though such emphasis is rejected in the book (p. 113), as it is in a more extensive account of Pacific colonial histories (Thomas 1991b).

9. "The fact that Sahlins triangulates among Hawaiian, Maori, and Fijian ethnographic cases is evidence enough for Thomas to label his research program 'implicit evolutionism' (p. 109)." What is actually being referred to there is my comment on Sahlins's contrast between Maori and Hawaiian ritual regimes, in which "from . . . to" idioms are used, implying that the Hawaiian polity emerges from a Maori beginning point. As I made clear, that form of argument, which makes contemporary society A the ancestor of society B, can only be seen to carry evolutionary implications. Although Valeri has suggested in a constructive and critical review (1991) that Sahlins's implication is not evolutionary but is situated in the "purely logical space" of Levi-Straussian method that does "not prejudge a historical account of the relationships between Maori and Hawaiian cultures," it hardly seems accidental that the transformations are from Maori to Hawaiian rather than vice versa.

10. With respect to other points, Parmentier complains that because *Out of Time* complements *Marquesan Societies* "readers will need to switch back and forth between the two volumes" (though the review conveys nothing to indicate that he is familiar with the second book) and suggests that for some bewildering reason I "decided not to publish a single work that would express a methodological and empirical synthesis." This ignores the different ways both works were at once empirical and theoretical, and overlooks the different horizons and audiences of each project. Second, Parmentier's assumption that I do not believe in fieldwork is incorrect: my critique concerned the place of this form of research activity in the construction of disciplinary authority. Third, I take it that the juxtaposition of a passage of mine concerning ideas, representations, and practices and a quote from Sahlins's article in *Critique of Anthropology* is supposed to indicate that far from being new, my ideas are anticipated by one of the writers I criticize. The passage quoted is not actually a summary or "prospectus" (on p. 68?) of the larger argument, but relates to more limited issues about the interpretation of the different properties of expository and event-oriented description. And at a number of points I do agree with Sahlins's general objectives and formulations; the debate was about the extent to which certain of his concepts and interpretative methods effected a historicization of anthropology. Fourth, while the "exchange of husbands" notion is supposed to attest to my ignorance of alliance matters and what real anthropology is all about, this was part of a description of the matrilineal, uxorilocal prestige-goods system; what was intended to be a lighthearted inversion of androcentric terminology is not, in this case, inaccurate anyway. Finally, I would not accept being labeled "a historian" if that is supposed to make me external to the discipline of anthropology; as it happens, in both Cambridge and Canberra my affiliations have been with anthropology departments.

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