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Since Howe's, Hecht's, and Ralston's thoughtful comments provide much to consider, I appreciate the opportunity *Pacific Studies* has provided for reply. I will organize my comments around certain general themes and then turn to specific points raised by the reviewers.

### **Themes**

*Making History* compares Pukapukan and anthropological ways for constructing a Polynesian atoll's traditions. Chapters 3 and 4 explore the dynamic nature of Pukapukan traditions—how Pukapukans, in the process of learning and validating their cultural traditions, often alter them. Chapter 2 focuses on two sets of anthropologists—the Beagleholes and Hecht—and their tendency to overstructure the traditional social organization in their analyses, emphasizing stasis, for example, at the expense of change. Comparisons between Pukapukan and anthropolog-

ical ways of constructing the atoll's past are developed through anecdotes and case studies. Most prominent among these is an exploration of the *Akatawa*, a reputed form of traditional social organization revived between 1976 and 1980, but about which little anthropological information existed prior to that date. On the one hand, the *Akatawa* represents an example of the fluid, dynamic character of Pukapukan social organization, involving changing social alignments through time. On the other, it constitutes an anthropological conundrum. Was it or was it not traditional? Had it or had it not occurred in the past despite limited anthropological reports on it?

Pervading the book are three implicit tensions. The first involves my attempt to escape the overstructuring tendencies in earlier anthropological accounts of the atoll. The second centers on the degree to which we can accurately know the Pukapukan past. And the third concerns how to best address certain issues regarding the construction of ethnographic knowledge.

The first tension focuses on the questions: If anthropological accounts overstructure indigenous perspectives and forms of social organization, how can I accurately describe them myself? How can I overcome the biases I attribute to others? Various techniques are used for coping with the problem in *Making Histoy*. I put considerable emphasis on the ethnographic data, indicating not only what informants told me (or what I observed), but also what particular informants were like as individuals. To allow readers to follow particular informants through various contexts, the index contains their names. Readers can thus relate one anecdote to another, building up a picture of informants as individual personalities (and the degree to which they adhere to generalizations I make regarding Pukapukans). Rather than supporting my analysis about the fluidity and diversity of Pukapukan knowledge with scattered examples, I focus on one particular issue – the *Akatawa* – and, through an in-depth accounting, try to provide a sense of the subtleties and complications involved in describing it. In addition, I use statistical presentations to indicate patterns of diversity. The statistical format is somewhat stilted. But we need to ask, How else – besides statistical presentations combined with anecdotes and case studies – can one provide a sense of diversity? Most anthropologists recognize the importance of diversity. But its range and its depth seem to be repeatedly downplayed. What is needed is a better understanding of the forms diversity takes under particular circumstances and how best to represent them.

I also attempt to handle this tension through what might be called negative description. In describing the *koputangata* (or cognatic de-

scent groups), I focus on the cultural constructs people use in their discussions and how actual experience tends to diverge from these. While this creates an ambiguity regarding what the *koputangata* are (in contrast to what they are not), I feel the style of presentation provides a more reasonable picture of the situation than a straightforward, structured account. The latter would be appropriate for the village organization, not the *koputangata*. The *koputangata* are too ambiguous to define with precision.

Various readers of *Making History* have stressed the value of the anecdotal material in conveying a sense of individual informants. And the negative description also seems to be well-received. But I do not say I have by any means resolved the issue. Anthropologists must continually grapple with this problem.

The second tension revolves around the questions: How can anthropologists formulate constructions of the Pukapukan past in regard to what “really” happened in earlier times given the problems with oral transmission noted in the text? How can one know the past independent of the present that preserves it and gives it meaning? This is an issue of much concern to Pacific historians of the “island-centered” approach (see Borofsky and Howard 1989). In considering the problem, I emphasize the importance of understanding the subtle dynamics involved in indigenous constructions of the past and how these constructions change over time.

Pukapukan traditions, in being preserved, are being altered. But in being altered, they are also being preserved. The past is being made meaningful to those upholding it in the present. Perhaps Pukapukans and anthropologists preserve a past that never was, but they preserve it in a way that is meaningful to present-day audiences. (P. 144)

A central theme of *Making History* is that indigenous and Western constructions of the past may diverge because they are formed in different contexts and are intended for different audiences. One should not blithely combine indigenous with Western accounts of an island's past. Carter makes this point well in a discussion of Reynolds's book on Australian Aboriginal history:

Bringing together a host of scattered oral and written data, in a manner wholly foreign to an oral culture, ordering them under the aegis of a culture-specific discourse known as history, [does

it not] merely continue by other means two hundred years of white [Australian] history, a history founded on the successful appropriation (and suppression) of neighboring cultures? This is not Reynolds's intention, of course, but it is an unfortunate consequence of his assumption that what goes on on the other side of the [cultural] fence is strictly comparable with what goes on here. (Carter 1987:160-161)

Discounting indigenous histories would be unwise. But beyond a certain level of generality, it is uncertain to what degree they represent accurate recountings of the past. Some accounts clearly are accurate (see Price 1983 for an example). But it is equally clear others are not. The question is how to separate accurate from inaccurate (or only partially accurate) accounts. That is not easily determined, especially since a number of variables extraneous to the accounts are involved, such as who related the material, in what contexts, how was it learned, and so forth. A real need exists for understanding why some individuals and groups preserve aspects of the past more effectively than others. Our understanding of these processes is only beginning.

For the above reasons – plus those listed by Ralston in her review – I prefer to err on the side of caution in presenting reconstructions of (1) how various revival movements started, (2) past forms of the *Akatawa*, and (3) traditional patterns of Pukapukan social organization. In each case I attempt to make certain points. But I do not propose a full-scale reconstruction for any of these. To do so would be to disregard my own cautions. One way ethnohistory is practiced today is to note various problems that exist in presenting accounts and then to subtly ignore them in one's own presentation. I raise problems and note issues that must be considered in ethnohistorical accounts. But I have chosen not to go beyond the limits of my knowledge to a discussion of what might (or might not) have been in times past.

The third tension concerns how best to address certain issues regarding the construction of ethnographic knowledge. These issues raise critical – and, to some degree, threatening – questions about how anthropologists construct their understandings of other cultures. The problem is how to present the issues in a positive way that draws people into a dialogue about them.

One issue involves the inevitable overstructuring of ethnographic materials that comes with the asking of questions. As I point out (pp. 150-152), asking certain questions not usually raised by Pukapukans stretches the material in artificial ways. Knowledgeable informants, in

trying to uphold their reputations, may formulate answers that are not part of everyday discourse. The difficulty is that Pukapukans do not necessarily ask each other a host of questions that interest anthropologists. Pukapukans do not go about trying to construct ethnographies of themselves. In writing accounts meaningful to Western audience, anthropologists are often drawn into asking inappropriate questions. But limiting oneself to questions Pukapukans find “culturally meaningful” does not necessarily solve the problem – it may only lead at times to asking no questions at all. The anthropologist thus becomes caught in a bind in writing a culturally sensitive account. Will it be incomplete or distorted?

Related to this is the question of “to what degree . . . can ethnographic accounts properly represent indigenous perspectives – and still be read by others” (p. 153). Indigenous knowledge – as expressed in everyday life – does not always possess a coherent order. It may be open, ambiguous, fluid, or contradictory. But anthropologists must give this material a certain structure in conveying its meaning to others, such as Western readers who do not use the knowledge in the same contexts as the anthropologist’s informants. In writing a dictionary, for example, anthropologists must repeatedly sort through disagreements and ambiguities in defining a word for outsiders. The fact that various people interpret a word differently – and at times have a social investment in keeping a word’s meaning ambiguous – must be set aside in the need for closure, in the need for coherency, in writing a dictionary.

There is also the issue of the informant-anthropologist dialogue. Ethnographic knowledge is not generally produced by isolated anthropologists. It is produced by anthropologists interacting with informants. And this dialogue not only is shaped by the context in which it occurs but also reshapes the context itself. Hecht’s questions regarding the atoll’s traditional social organization within a particular context, for instance, apparently helped reshape that context. Her questions encouraged a set of traditional revivals that, in turn, altered Pukapukan perceptions of their former matrilineal organization (see pp. 69, 132; Borofsky 1988).

Taken together these points raise important questions and offer much food for thought. Given their complexity, my goal was to stimulate others to reflect on them with me and to rethink their own data in light of them. In developing my points, I tend to focus on ethnography more than theory. Given the abundance of theoretical perspectives that have come into prominence during the past two decades and the ambiguous results achieved through them (see Salzman 1988), I have tried to stick

close to the data themselves. I often focus on a particular ethnographic context or interaction in dealing with an issue. I do not stress in chapter 1, for example, the ambiguities surrounding Pukapukan assessments of knowledgeable elders in relation to anthropological assessments. But I do describe Molingi's knowledge of the *Akatawa* (pp. 7-11). From Molingi's answers, one would be hard pressed to view her as one of the foremost experts on the subject. Yet that is the role she took in a group meeting. Similarly, rather than wax at length about the problems inherent in capturing indigenous perspectives, I focus on the issue of closure in writing a Pukapukan-English dictionary (pp. 147-149).

This does not mean I do not have opinions. I do, and I try to make them clear. Following on the central theme of comparison, I stress the need for a "dialogue of perspectives," especially in overcoming the limitations of ethnographic constructions. When anthropological accounts open themselves to differing perspectives that raise critical questions regarding the ethnographic record, they have an ability to overcome some of these difficulties. The refined discourse, the thicker description, that develops about a culture comes not from one account but from a comparison of various accounts over time, in which a set of perspectives can be seen in relation to one another. This is what I seek to do in regard to the Beagleholes', Hecht's, and my accounts of Pukapukan social organization and in regard to Pukapukan and anthropological perceptions of the atoll's past.

Rather than lecturing readers, then, I seek to intrigue them with problems – hoping they will reflect, with me, on the complexities involved. This way important issues are raised without oversimplifying the problems or their possible solutions.

### Responses

In responding directly to each reviewer's remarks, I will frame my comments within the context of the preceding statements. I begin with Hecht since she raises a set of specific ethnographic questions.

I was surprised by Hecht's representation of our interaction as minimal. My perception is that we had several interactions between 1982 and 1986. For example, she wrote detailed comments – first in September 1985 and then in January 1986 – on a nearly final draft of *Making History*; her remarks on the *Akatawa* are included in a note (p. 166 n. 3). Our conversations during this period implied a fairly close reading of the material. She suggested, for instance, that I rephrase my description of her role in stimulating the revival of the matrimoiety organization in

1974. (The change can be seen by comparing p. 132 with Borofsky 1982:209.) And I discussed with Hecht the specific passage in my book (p. 13) that she cites in her review, making sure that it was indeed accurate. Thus, while Hecht is right to note the value of the first conversation we had on Pukapukan matters in 1982, it was only one of several in which I sought and obtained valuable feedback on my analysis of the *Akatawa* and her work.

Hecht raises an important set of questions about the *Akatawa*: Did it (or did it not) constitute a fully fledged alternative to the village organization? Was it (or was it not) always conceived of as a temporary change? And did affiliation through the moieties ever become organizationally or culturally expressed between 1976 and 1980? Let me take each question in turn.

Different people might well draw different conclusions regarding whether the *Akatawa* constituted a fully fledged alternative to the village. From the Pukapukan perspective, it clearly was a distinct alternative in 1979-1980. The problems centering on its demise – discussed in my dissertation (Borofsky 1982:225-229) and briefly in *Making History* (pp.149-150) – emphasize that. Perhaps in the *Akatawa's* early stage, especially in 1976, it was seen as only a brief respite from the village pattern. But by 1980, it had clearly become an organizational alternative. There would not have been so much tension in 1980 about the decision to return to the village pattern if the *Akatawa* had not been perceived as an alternative form of organization. It should be stressed that there were distinct advantages and disadvantages for various groups imparted by the *Akatawa*. People who previously belonged to Yato and Ngake villages, for example, now had greater access to taro in Motu Uta (Loto village's reserve under the village system). The new alignments also drew people together who normally did not participate in the same activities or share the same resources. For the former members of Loto village, which was split in two under the *Akatawa*, there were real disadvantages. They were at times overshadowed in meetings involving Tawa Lalo and Tawa Ngake (the two "sides" or groups of the *Akatawa*) by people formerly affiliated with Yato or Ngake. *Koputangata* descent groups previously belonging to these other villages, moreover, increasingly made claims on land in Motu Uta. Also, there were obvious tensions during certain sports competitions with the whole island involved in two fairly evenly matched teams. (The 1980 Kavekave fishing competition, for example, ended in disarray and dispute.) In its own manner, the *Akatawa* was reshaping resource allocation and social relations in ways that some saw as beneficial and others as detri-

mental. That is why, from my understanding of Pukapukan perspectives, it clearly constituted an alternative form of organization to the village in 1980.

Whether other anthropologists would view the *Akatawa* as a full-fledged alternative is another matter. On the one hand, some of the changes the *Akatawa* introduced seem fairly major. A considerable portion of the islands resources and all of its people were reorganized into a bipartite rather than tripartite pattern. These changes, over time, were bringing about additional alterations in Pukapukan social organization. On the other hand, the changes surrounding the *Akatawa* could be viewed as relatively minor. The transformation between the two patterns was achieved with relative ease because such organizing structures as the food-sharing units (*tuanga kai*) remained intact. With the exception of the obvious tripartite to bipartite transformation, the patterns of allocation and organization essentially remained the same. And while various other changes did develop, they were gradual, apparent only with time. The problem is, then, that the *Akatawa* essentially retained the same underlying structure as the village organization. But the structure was manifested in different ways with different implications. The question for anthropologists is at what point an "alternative organization" is alternative enough to be seen as such. If one focuses on the underlying structure, one might make a case in either direction regarding the *Akatawa*. It would depend on one's perspective and how one defined certain structural elements in relation to resource allocation. If one focuses on surface manifestations, it clearly was an alternative.

As readers can see, Hecht splits the *Akatawa* into parts in her discussion. She separates the land division and moiety organization (of Tawa Lalo and Tawa Ngake) from the term *Akatawa*. She is able to thereby state that the moiety organization is seemingly old while wondering if the term *Akatawa* is perhaps new. This leads back to the atoll's flexible social organization and the anthropological analysis of it. When is the *Akatawa* really the *Akatawa*? Is it when a moiety social division exists with a certain land division and a particular name? Or can it occur with some but not all of these properties – for example, the moiety and land division without the name? I can only note that for Pukapukans between 1977 and 1981, the period of my fieldwork, it was when all of these elements were combined.

What I sought to do in *Making History* was to give the *Akatawa* equal billing with the village organization – no more, no less. Both involve essentially the same resources and people. If one downplays the village organization – emphasizing instead an underlying set of cross-cutting

ties centered by certain corporate groupings—it would certainly be appropriate to deemphasize the *Akatawa's* significance. But if one gives credence to the village organization, its *Akatawa* transformation is significant as well. Since anthropologists studying the atoll have repeatedly emphasized the village organization, I feel it appropriate to also emphasize the *Akatawa*.

The question of whether the *Akatawa* was “never regarded as more than” temporary is an ambiguous matter. First, how long must a form of social organization last before it can be regarded as enduring? For some, four years’ duration would classify it as more than a temporary change. Pukapukans took various attitudes toward the permanency of the *Akatawa*. Some, who wanted to return to the village system, viewed the *Akatawa* as a temporary alternative and only that. Others, who favored it, viewed it as an experiment that, once it had a proven track record, should permanently replace the village organization. Clearly the *Akatawa* was initially seen in 1976 as a temporary alternative to the village organization. But as it became established, it took on a momentum of its own. In 1980 it came reasonably close to replacing the village system (p. 149; Borofsky 1982:225-229). Its permanency, in other words, was something negotiated by Pukapukans over time. In hindsight, from afar, it may now seem only a temporary alteration. Its permanency, though, was an open question in 1979-1980.

This raises a related question: the demarcation of change. Does change have to disrupt the whole established order at one fell swoop? Or can it come gradually, subtly working its effects through time? I believe the *Akatawa* developed in the latter way. One wonders, in this respect, how the village system came about, probably sometime near the turn of the century (note Hecht 1987:196-199). Might a “temporary” alteration have gradually overturned an earlier system—with the support of missionaries (p. 45; Hecht 1987:196), government agents (Hecht 1987:196), and population changes (see p. 40)? In retrospect, it might seem a simple decision was made to organize the atoll by villages. In fact, organizing by villages might well have been a gradual process, negotiated over time—readily delineated only in hindsight.

I am a little puzzled by Hecht’s remark that “apparently affiliation through the *moieties* was never organizationally or culturally expressed’ (her emphasis). She seems to differentiate between moiety organization and the *Akatawa* here. But generally, I would say the distinction between Tawa Lalo and Tawa Ngake was well expressed: organizationally in the division of land, food, and people during the 1976-1980 period and culturally (if Hecht makes a clear distinction

between the two aspects) in its competitions, religious observances, and celebrations.

Howe's comment regarding the "grip of typology" is particularly relevant here. The atoll's social organization, for good ecological and social reasons, is not a neat structure that can be fit into a little box. (I wonder if this is not the case for any social organization.) Ultimately, it seems to me that my differences with Hecht, regarding the *Akatawa's* character, relate to how to bound (and describe) some very complex processes.

My view is that the 1976-1980 *Akatawa* expressed certain underlying Pukapukan structural elements that were quite old but that probably had not been combined in precisely that form prior to 1976. The specific organization that arose in 1976, I suspect, was the result of a momentum developing out of earlier, less successful, efforts to revive traditional forms of social organization in 1974 and 1975. The connection of the *Akatawa* to the past derived partly from an accurate perception that certain elements (such as the land division in Motu Uta) were quite old and partly from the ways Pukapukans acquire and validate their traditions. Personally, I doubt the *Akatawa* existed around the time of the 1914 hurricane. From my examination of the Beagleholes' field notes and from the types of questions they apparently asked, I believe they would have gathered information on it had it occurred. But I also well understand that placing the *Akatawa* at that time makes considerable sense to modern Pukapukans (see p. 11).

While valuing all three reviews of *Making History*, I find Howe's especially thought provoking. He elaborates on subtleties in the material in ways that further my understanding of several issues. Like Howe, I hope *Making History* will stimulate additional interest in styles of learning and how these shape peoples' understandings of the past. It is an important topic that deserves greater attention.

Generally I concur with many of Howe's comments. I agree that more attention could have been paid to the wider currents within which the Beagleholes, Hecht, and I operated and how these shaped our constructions of texts. Howe's comment on cross-cutting ties is intriguing and, I suspect, essentially correct. Having focused on literature related to Polynesia, I missed the Hallpike, Kang, and Dillon references. And it is true that theory, as much as fieldwork, led me to perceive the integrative functions involved. Nor will I deny that the argument relating to cross-cutting ties is essentially functionalist. Whether it is teleological requires explication on Howe's part of what he is specifically referring to. And whether functionalism is a bad thing, a label to be rejected and a perspective to have stakes driven through its heart, depends on how he

defines the term. But it is certainly true that I perceive positive aspects in the cross-cutting ties and a reanalysis of the situation might well also indicate the theme alluded to by Howe (note in this respect, p. 164 n. 5). Concerning the overstructuring of typologies, I thought I had made that point relative to the Beagleholes' account of Pukapukan residence rules (pp. 51-53). I state: the Beagleholes "tended to fit the data into somewhat arbitrary, somewhat inaccurate categories that overstated the degree of cultural uniformity" (p. 53). Howard comments on this theme in his foreword.

Howe is correct that I do not fully contextualize myself in the ethnographic continuum. Howard's foreword does do this to some extent, and the preface (p. xvii) and the notes indicate individuals and texts that influenced the construction of my analysis. But I draw back from an elaboration of my biases in the text. There is, it seems to me, something essentially incongruous and self-serving about an anthropologist's explaining his biases to others. How do we know that these are the essential ones? And if they are important, why did he or she not try to overcome them? More revealing, I think, are other scholars' comments on one's work—such as Howe, Hecht, and Ralston have presented here. I suspect this is intellectually more productive in the long run.

Howe is also correct about the value of examining interconnecting texts. But I would not substitute examining texts for examining anthropologists' backgrounds. Both have a role to play. A study of interconnected texts illuminates the traditions shaping ethnographers' agendas. A study of individual backgrounds suggests the perspectives ethnographers bring to the texts.

By way of introduction to Ralston's review, let me note I appreciate her historical perspective and concern with the "growing dialogue and interpenetration" of history and anthropology. There is much to be gained on both sides by this dialogue—as the works of Denig, Oliver, Sahlins, and Ralston herself indicate. My contribution to this dialogue focuses on the processes by which Western and Polynesian groups construct cultural traditions. If we are going to include indigenous formulations of the past in historical and anthropological accounts, then we should understand the nature of these constructions and the processes that went into shaping them. In this respect, I hope historians will see *Making History* as relevant to a number of cultures, not just those involving atolls.

I concur with Ralston that historians seem more willing to recognize the validity of differing interpretations than anthropologists. One might ponder why. Perhaps it is because historians often work on the same

topic as others (for example, the French Revolution). Anthropologists tend to seek out their “own” society in fieldwork. With few others to contradict him or her, each anthropologist has been seen as the “expert” on the society – though this is clearly changing today.

While certainly open to a feminist perspective in the matters Ralston discusses, I would be more cautious than she is in definitely assuming “gender politics” were involved – at least without further clarification. Pearl Beaglehole was a better linguist than Ernest, but she lacked his interest in theory according to Jane and Jim Ritchie (her daughter and son-in-law). The acknowledgments in *Ethnology of Pukapuka* indicate that “Ernest Beaglehole was especially responsible for collecting material on social and economic organization, religion, traditional history, and material culture” and “the bulk of the manuscript was written” by him (Beaglehole and Beaglehole 1938:3). This would be appropriate for Jane Ritchie, Pearl’s daughter, to comment on, but my impression is that Pearl Beaglehole held her own intellectually with Ernest. (If one is to believe such books as *The Feminine Mystique*, woman could be quite independent and assertive during the 1930s.) I would also like to know more regarding how Ralston believes the trend toward feminist critiques in anthropology affected Hecht’s work. I would not assume a direct relation simply because Hecht is a woman and because she wrote during a time when these critiques were prominent in the literature. I believe David Schneider and his perspective on cultural analysis, for example, had a greater impact on Hecht. But I would defer to Hecht’s opinion in this matter.

Though I wish to avoid quibbling over details, I would not lump Sahlins, Clifford, Marcus, and Denning into the same category. Sahlins and Denning yes, Clifford and Marcus yes, but not the two groups together. The former are involved in the anthropological interpretation of history; the latter are more concerned with the construction of ethnographic texts. My not stressing theoretical themes more explicitly in *Making History* stems partly from a sense that the issues raised by Marcus and Clifford require less abstract discussion and more concrete ethnographic case studies, especially those involving more than one anthropologist at a field site (see Borofsky 1988). It is all too easy in interpretive discussions to lose sight of the issues involved unless they are tied to specific ethnographic analyses.

Finally, I basically concur with the premise of Ralston’s remark that a variety of groups – both Polynesian and Western – probably influence Pukapukan constructions of their past. But I do not see that my analysis denies such a possibility. To focus on two groups about which I have a

reasonable amount of data in order to develop a comparison—and the book is about a comparison—is not to preclude other influences. Still I have no problem with Ralston's more general point that one needs a broad perspective in such matters. What I ask for is a dialogue with her and others. Despite the real interpenetration that has occurred between history and anthropology, much remains to be done. I appreciate Ralston's seeing *Making History* as contributing to the developing dialogue.

In summary, *Making History* is a comparison of the ways two different groups, Pukapukans and anthropologists, construct knowledge of the atoll's traditions. It attempts through concrete ethnographic comparisons to reflect on a number of critical issues in anthropology and history. But it is only an initial effort. Its themes need to be developed further. In this respect, I am in the process of finishing a number of articles that elaborate on points raised here and in the book. My desire is that they will stimulate additional dialogue paralleling the valuable comments by Howe, Hecht, and Ralston here. I perceive *Making History* as part of an ongoing discussion concerning our and other people's constructions of the past—how we, individually and collectively, make history.

#### NOTE

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