Response: Douglas Oliver
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I must commence this response with an admission: I found the Tahitians I lived among most engaging as individuals, but their institutions very dull (one of the several reasons why Robert Levy's *Tahitians* is so much more interesting than *Two Tahitian Villages*). As individuals, most of them--especially the older ones--stood out sharply, like large, roughly-sculpted, granite figures on a flat landscape. (In their own rural settings, that is; in the European-Chinese ambience of Papeete they seemed to me to shrink and lose shape--a poignant reminder of their actual and self-conscious marginality in the colonial scene.) However, the division of labor that Levy and I agreed upon--for good and obvious reasons--led him to focus on and write about individuals, and me about institutions. And in my case, since most of those institutions were about as exotic as Coca-Cola, I spent much of the time doing things like listing choir-practice attendance and noting who got soused at weddings--which of course is a necessary part of *controlled comparison* but was for me a tedious routine. (Doubtless, a deplorable attitude in an ethnographer, but the reflection of a life-long preference for ethnology over sociology.)

However, before indulging in any more self-revelations let me address some of the criticisms aimed by the reviewers at my "large" ("overwhelming," "old-fashioned," etc.) book. I begin with Allan Hanson's, the most explicit of the three.

First is his commonsensical reminder that some differences between the two villages (in this case the size and composition of households) involved numbers too small to worry about, Having been schooled during the pre-statistical era of ethnology, I did indeed "worry" about all differences, however small--but not to the point of tearing my hair (of which there is little left to tear).

There is also truth in his charge that I was not "expansive" enough in my analysis at certain points, specifically regarding the different ways in which the villages reacted to the 1958 referendum concerned with relations with metropolitan France. In fact, the Postscript in which that event was mentioned was intended not as a source of new information, and hence requiring more explanation, but as a caveat regarding premature explanation based on short-term observation. An analysis of that particular event and its consequences would have required far more pages than I was prepared to add to an already over-long manuscript. My mistake was in offering any explanation at all--especially one as cryptic as that which Dr. Hanson justifiably objects to.

Next, in his discussion about the relations between household size, inter-household exchange, and involvement in the money economy, he is, I believe, correct in proposing a direct correlation between the first two (i.e., the larger the household the less its *need* for and practice of inter-household exchange, regardless of the money factor). Also, there is good logic in his point that households with more money have less *need* to engage in inter-household exchange. Nonetheless, I continue to believe that a *mental attitude* was also an important factor in the equation--that one village's longer and deeper involvement in the money economy of French Polynesia helped to foster a generally negative attitude (i.e., toward extra-household gemeinschaft) that included, for example, disdain for cooperative, mutual-aid work groups ("too bumpkinly") and disinclination for inter-household exchange ("unbalanced reciprocity makes for bad blood"). I would not characterize the latter as "selfishness," as Dr. Hanson does, but rather as a higher value placed on autonomy and privacy.

Dr. Hanson's fourth criticism concerns what he calls my "piecemeal" presentation of the villages' institutions--my failure to treat them as "organized systems" or "structural wholes," I am somewhat puzzled by this charge. He is, of course, correct in stating that I described and compared one institution at a time; but I cannot for the life of me see how one can discuss the *connections* between parts before describing the parts themselves. Moreover, I did in fact compare the villages in terms of the size and pervasiveness of their kin networks, and in terms of the connections between the personnel and organization of their church parishes, their governmental administration, and their political parties (499-502). And while those networks and connections may not add up to "organized systems" or "structured wholes," they are about as far as I could have gone, analytically, without resorting to (esthetically pleasing but semantically empty) metaphor.

Dr. Hanson's fifth criticism, leveled at the book's lack of an index, is well deserved. All I can offer in extenuation is that the organization of the book is so relentlessly and systematically "piecemeal" that any reader seeking information on *anything* should soon know where to look. Also, I must confess, by the time the thing was written and rewritten, typed and retyped, proofread and re-proofread, etc., etc., I had neither the will nor the energy to prepare an index for it--and am of the opinion that an index for an ethnography prepared by anyone but the author has limited usefulness. (Apropos which, I have been intrigued--amused, bemused, etc.--by the fact that for several of the reviewers of my book, its most noteworthy feature has been its lack of an index. Perhaps my next one should consist of an *index* to which can be added a text.)

Turning to Paul Shankman's more indulgent remarks, they seem to boil down to a mild reproach--that I did not do *more* with the data than I did, that I did not combine them with *other* data, from other places and other times, to construct hypotheses of wider application (and of greater interest to readers like himself!). All I can say, in answer to this amicable complaint, is that I share his judgment about the theoretical aridity of the book; it is not one I would recommend to someone searching for anthropology's Great Ideas. On the other hand, I would not, out of modesty, be reluctant to prescribe it as an antidote to the bold claims still being made about "the comparative method" in particular, and about anthropological "science" in general.

Which reminds me, none of these three reviews (nor any others about the book that I have seen) has made more than passing reference to my application of "controlled comparison," which, after all, was the raison d'etre of the whole exercise. Was it applied correctly or not? Did its findings add anything to what is already known about cultural process? Is it worth pursuing in other places? And so on. Dr. Shankman suggests --one feels almost consolingly--that extension of the comparisons to the six other Tahitian communities included in the larger project *might* generate more hunger-satisfying (i.e., less Chinese cuisine-type) theory, Perhaps so; but extension of "comparison" to a wider and more varied universe inevitably reduces "control"-- so where is the line to be drawn?

Greg Dening's friendly remarks prompt me to wear my glasses when I next look into a mirror. They seem to combine an appreciation for what I am (i.e., what I have written), with good-natured vexation that I am not something else.

I am of course grateful for the appreciation, and flattered to be linked with the likes of A. Smith, J. Bentham, and W. James. Also, I am deeply moved, really, by his "confidence" in my "wisdom," but am uncertain what that wisdom is--except that "it," as represented in my writings, is not something he himself wishes to emulate! I, on the other hand, do wish I possessed the ability to compose a book like his *Islands and Beaches.*

Along with my gratitude, I feel some sorrow that I vex him--"madden" him, in his words--by what he calls my "refusal to say [what I think about my exercise in comparison] in relationship to wider issues." Dear me; I thought I had done so, namely:

> It has been claimed by some of its proponents that, because of anthropology's inability to conduct sufficiently controlled *experiments,* controlled *comparison* is the sole means at its disposal to arrive at "scientific," universally valid generalizations about cultural process. I am not convinced that this is so--or, for that matter, that *any* research method heretofore proposed or practiced is capable of producing such generalizations--but controlled comparison appears to be a method worth devoting more effort to. (xi)

As for my attempt to formulate a scheme for describing the *economics* of a whole community in general (Appendix), and of an individual's life cycle in particular (391-400), I am left to conclude that in his judgment it either does not touch on "wider issues," or that, out of kindness to myself, the least said about it the better.

But perhaps our notions differ about what those "wider issues" are. To me, as a matter of priority, they have to do with ethnographies themselves: first, with making them fuller, more faithful representations of various distinctive ways of life; and secondly, doing so objectively and in language that will permit them to be compared one with another. The first goal springs from a conviction about the paramount importance--the necessity--of recording as fully as possible as many distinctive cultures as possible. This I consider to be a sufficient goal in itself, one that does not require any other justification. (When voiced by some anthropologists, the judgment "just another ethnography" connotes disparagement; in fact, the production of any honest ethnography is a commendable act,) The second, largely instrumental, goal I refer to involves an ambiguity more easily stated than resolved. While I acknowledge the difficulty, perhaps in some cases the impossibility, of describing the institutions of cultures in a language (e.g., English, French, etc.) other than their own, I nevertheless believe that the attempt must be made, in the hope--not very sanguine-- that someone, someday may be able to construct wider generalizations about mankind's institutions by means of comparison, controlled or otherwise.

I infer from Dening's statement that he also is in search of those "wider generalizations" (and is more sanguine about discovering them). In addition--to his greater credit--his attempts to discover them are more *direct* in that he employs each of his own ethnographies as a straight path to the goal. There is no way of foretelling which of the two approaches will reach the goal first--or whether the goal will ever be reached! But even if it is not, the products of both approaches will prove to be worthwhile. The *Islands and Beaches* approach will continue to provide engrossing and thought-provoking documents for anyone interested in the human condition. And the Just-Another-Ethnography approach will provide irreplaceable information about the human past.