BRIEF REVIEW

GROWING UP WITH NEW GUINEA

Pigs for the Ancestors in Retrospect

Glen T. Petersen

As the shadows of 1980 lengthen some Americans celebrate Earth Day's tenth anniversary; others commemorate the tenth anniversary of the killings at Kent State and Jackson State. Those events climaxed, while signifying, what we now refer to as "the sixties." Anthropologists of my generation came of age in that axial decade, and the work we carry forward will always bear the scars of that initiation, just as many of our own teachers were marked by the Depression and World War Two. Roy Rappaport's *Pigs for the Ancestors* [1] characterizes the theoretical milieu in which we matured: the concerns that fired our imaginations and the convictions that informed our commitments.

It is presumptuous of me to write in the first person. Reminiscence is more comfortably the prerogative of those who tell us of the days when the entire American Anthropological Association met in a single hall, rather than of one who represents the time when a single year's production of Ph.D's required more space. But I think that *Pigs for the Ancestors* needs to be considered within the context in which it was received – as well as be evaluated on its own merits – and perhaps I can tell the tale. In 1968, in the paradigmatic 60's, when the book was published, I returned from my last voyage, received my honorable discharge, began college, turned 21, and took my first anthropology course, in that order. By the following spring, in my third anthropology course, I was studying an ecological anthropology which had already taken Rappaport's work as paradigmatic, a term I learned at about the same time. The model Rappaport put forward was applied by my teacher, Lowell J. Bean of California State College, Hayward, to the Cahuilla Indians of Southern California in his book *Mukat's People* [2] and in the papers of almost all the students in his course on cultural ecology. Diffusion from New York City all the way to a small and nondescript California college comes so quickly only in the ripest of conditions. The following year I made the pilgrimage to Columbia University, which I took to be the seat of this new ecology, and began graduate study. Though I came to learn anthropology, I was encouraged to pursue botany, geology, and tropical medicine. Only gradually did I find my way back to Schermerhorn Hall and anthropology courses.

Not all the anthropology students of that period were interested in ecology and econo-

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0304-4092/80/0000–0000/$02.50

mics, of course, but our intensity seemed universal. The postwar baby-boom produced a generation that grew up with singular effects on American society. We might be likened to a large chicken swallowed by a smallish snake. Peristalsis requires that the fowl keep advancing, but it transforms its surroundings as it goes. We watched elementary schools, high schools, and colleges rise before us (and close behind us). The civil rights movement and war resistance gave us even greater delusions about our power to transform, and heightened the antagonism that moved us to demand transformation.

Cultural ecology had progressed slowly in the fifties and sixties. The trial formulation of Vayda, Leeds, and Smith [3], which claimed that the pig feasting of New Guinea's neolithic tribesmen made great eco-logic was furthered by Rappaport and others. With the appearance of *Pigs for the Ancestors,* the old cultural ecology was eschewed and a new vision hailed. Not only did New Guinea highlands ecology and economy make good sense, we students realized, it seemed to make *perfect* sense. Out of the sixties came the ecology movement; out of *Pigs for the Ancestors* came the "new ecology." With the benefit of hindsight, it is now easy to see that the ensuing squabble of claims and counterclaims about this new ecology was the product not of what Rappaport specifically attempted to do in *Pigs for the Ancestors,* but of what we *thought* he was doing. It is also possible that such a band of ardent followers may have had some effect on what Rappaport and others themselves believed that they were doing.

I find the debate that has grown up among Friedman [4], Sahlins [5], Harris [6], and Rappaport [7], over the analysis developed in *Pigs for the Ancestors,* one of the more confusing and least attractive aspects of seventies anthropology. Unattractive because of the vituperation; confusing because much reading and rereading has not made me sure of who is arguing what, and where the differences – if any – lie. I believe that this is so at least partly because of what we brought with us when we began to study the book. This is the way that it is, of course, with any rich and innovative text: we find what we are looking for and are often blind to that which we do not seek.

In my case, and in the experience of many others with whom I have discussed this recently, there was a belief that *Pigs for the Ancestors explained* the existence of the feasts that occupy so much of the lives of the New Guinea highlanders. Vayda, Leeds, and Smith hypothesized that these feasts regulate local environmental relations. It seemed to us that Rappaport had demonstrated the fundamental truth of this proposition. New Guinea highlands pig feasts existed in order to keep local ecosystems in balance. Rituals could be understood as the product of, or determined by a society's ecological systematics. Human societies could be treated as biological populations, *and if we just understood enough biology and ecology, we would understand culture as well.*

The reception of *Pigs for the Ancestors* has to be examined within the context of other writings by Rappaport, and by Vayda and Harris. Those who read one turned to the others. In the 1968 article, "Ecology, Cultural and Noncultural," Vayda and Rappaport [8] suggested that an ecological perspective could contribute to the realization of two of antropology's major goals: (1) to explain why certain traits exist at certain times and places, and (2) to explain how certain traits function. There is, throughout this entire corpus of work, what I perceive, perhaps unkindly, as vacillation. In general, ecological anthropology was hailed as being able to explain origins, or causes, or "why." In specific instances, all that was claimed was a functional description of systematics. Now, as Harris [9] has recently reminded us, a good functional description is not achieved with the wave of a magic wand. It requires much hard work and analysis. The question is, why do that hard work and then be satisfied with no more than a description?

But we students thought that this kind of analysis would explain "why." While one can find multiple disclaimers in *Pigs for the Ancestors* and in other pieces, it seems to me that our teachers also believed that we would be able to use this new ecological analysis to explain "why." Certainly the debates I heard at the Columbia University Seminar on Ecological Systems and Cultural Evolution made me think that this was so.

It did not take long, in those tumultuous times, for countervailing claims to be made. Did *Pigs for the Ancestors* really tell us why the feasts took the form they did, or why they existed at all? A good many of us set off for the field, expecting to demonstrate that the exchange activities of horticulturalists could be explained in terms of local ecosystemic balance. We took with us varying degrees of commitment to that idea, and to the intensive quantification that characterized its genesis. Speaking for my own cohort, I have found Robin Hide's as yet unpublished study of Nimai pig cycles [10] the most careful replication of the Maring study, and the most thorough refutation of any claim that the Maring model might be applicable to the highlands in general, as Vayda, Leeds, and Smith had originally hypothesized and Rappaport tentatively suggested [11]. Hide found that the size of Nimai pig herds is determined by the feasting requirements of their owners, and not vice versa. Brookfield [12], Brown [13], and others have consistently argued this same point.

Rappaport [14] continues to maintain that Tsembaga Maring pig husbandry can be effectively analyzed within the bounds of an ecosystem that is coterminous with Tsembaga territory. Others see the flow of pigs, pork, and people across boundaries as too fundamental to the existence of highlands societies to permit human ecological activity to be analyzed within such narrow limits.

My own fieldwork started with a related problem. Like many other students, I wanted to know something of how change enters the picture, how a nicely equilibriated subsistence agricultural system responded to the demands of commercial farming. I worked on Ponape, in Micronesia's Eastern Caroline Islands, to the northeast of New Guinea. Ponape is the site of an extremely competitive feasting system that includes the production of large numbers of pigs and a wide variety of massive yams. In "Ecology, Cultural and Noncultural," Vayda and Rappaport [15] hypothesized that these feasts have latent ecological functions similar to those described in New Guinea.

On Ponape I encountered two problems that led me away, finally, from explanation of the sort done in *Pigs for the Ancestors.* First, Ponapeans are not only competitive, they are secretive. Feasting is a monthly, even weekly, pastime, and success depends on deception as well as production: If a man's competitors do not know what he is capable of bringing to a feast, their efforts to outdo him are confounded. One does not ask Ponapeans how many pigs they have, or measure their gardens. Not, that is, if one wishes to keep talking with them. I found that just as in my studies I had drifted back from botany, geology, and tropical medicine to anthropology, on Panape I was lured away from quantitative data collection by my desire to live with, learn from, and enjoy my Ponapean hosts. This did not deter me from assiduously attempting to determine, qualitatively, the ecological bases of the Ponapean feasting complex. Here my second problem arose. During my first year on Ponape, I attempted to fit each and everyone of the many hypothesized ecological explanations for feasting to the Ponapean case. In the end I failed. On an island with a stable environment of the sort described for the Maring, with highly productive groves of breadfruit evenly distributed throughout the inhabited territory, and a lagoon full of fish, everyone seems to have more than an ample supply of food all the time. There are historical problems, of course, with nineteenth cen-

tury depopulation, colonialism, the introduction of pigs, and changes in social organization, but I believe that I have taken them all adequately into account [16].

Ponapeans spend a significant portion of their time feasting and producing for feasts. The feasting has important ecological and economic functions, to be sure, but I have been unable to determine that *there is any ecological necessity in the feasting.* I cannot conclude that this very fundamental element of Ponapean social life, which determines so much of their relationship with the land, is adaptive in the biological sense. The feasting cycle integrates Ponapean society, and does it wonderfully well, but I have adduced no evidence that in its absence the population would be threatened by *natural* disorders.

Very well, you might say, but what has Ponapean feasting to do with Rappaport's explanation of Tsembaga Maring pig cycles? Very little, if one judges the book solely as ethnography. However, I believe that the impact of *Pigs for the Ancestors* must be taken in the context of its time and accompanying theory. The notion that neolithic tribesmen could live in small, bounded, neatly equilibrated ecosystems was politically and esthetically attractive. That such a vision would at last provide us with a true science of man was equally seductive. We wanted to believe that *Pigs for the Ancestor* was doing much more than it claimed, and many of us set out to advance this idealized strategy. The subsequent years have taught us differently.

From studies in New Guinea and in places like Ponape, we have learned that systems of exchange permeate every aspect of social life in such societies [17]. When human activity is largely predicated on relations with people miles or mountains away, it may be impossible to know what bounds an ecosystem. When we see, historically, the continual expansion and contraction of social groups, populations, and territories, we cannot comfortably focus on equilibrium. And when growing numbers of biologists, themselves products of the same era as we anthropologists, tell us that equilibrium may not be an attribute of many natural ecosystems at all, we are encouraged to ask what it is about human social and economic relations that is so likely to generate change.

Pigs for the Ancestors taught us a great deal about the Maring, about the New Guinea highlands, and about human ecology. It was, in its time, paradigmatic; an exciting and challenging book. It shaped the anthropological enculturation of my comrades and me. It taught us another lesson as well; one that we all learn at some point. The books we read – like the societies we study – are known not only for what they have to tell us, but through what we have come to them prepared to learn.

NOTES

1 Roy A. Rappaport, *Pigs for the Ancestors* (New Haven: Yale University Press, 1968).
2 Lowell J. Bean, *Mukat's People* (Berkeley: University of California Press, 1972).
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4 Jonathan Friedman, "Marxism, Structuralism, and Vulgar Materialism," *Man*, vol. 9 (1974), pp. 444–469.
5 Marshall Sahlins, "Culture as Protein and Profit," *The New York Review of Books*, (Nov. 23) (1978), pp. 45–53.
6 Marvin Harris, *Cultural Materialism*, (New York: Random House, 1979).
7 Roy A. Rappaport, *Ecology, Meaning, and Religion* (Richmond, CA: North Atlantic Books, 1979).
8 Andrew P. Vayda and Roy A. Rappaport, "Ecology, Cultural and Noncultural," in James A. Clifton (ed.), *Introduction to Cultural Anthropology* (New York: Houghton Mifflin, 1968).
9 Harris, *op. cit.*, p. 244.
10 Robin Hide, "On the Dynamics of Some New Guinea Highland Pig Cycles." Unpublished manuscript.
11 Rappaport, *op. cit.*, 1968, p. 231.
12 Harold C. Brookfield, "Full Circle in Chimbu: A Study of Trends and Cycles," in H.C. Brookfield (ed.), *The Pacific in Transition* (Canberra: Australian National University Press, 1973), pp. 127–160.

13 Paula Brown, "New Guinea: Ecology, Society and Culture," *Annual Review of Anthropology*, vol. 7 (1978), pp. 263–291; "Change in the Boundaries of Systems in Highland New Guinea: The Chimbu," in P.C. Burnham and R.F. Ellen (eds.), *Social and Ecological Systems* (London: Academic, 1979), pp. 235–251.
14 Rappaport, *op. cit.*, 1979.
15 Vayda and Rappaport, *op. cit.*, p. 492.
16 Glen T. Petersen, "Man in Paradise: Implications of Steady-State Agriculture." Presented at the Columbia University Seminar on Ecological Systems and Cultural Evolution, 1977, April 11.
17 Paula Rubel and Abraham Rosman, *Your Own Pigs You Must Not Eat* (Chicago: University of Chicago Press, 1978).

Dialectical Anthropology 5 (1980) 255–259
Elsevier Scientific Publishing Company, Amsterdam – Printed in The Netherlands