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Gender, Egalitarian Societies, and the Writing of Pacific Anthropology

I am honored to participate in the *Pacific Studies* book review forum, and I thank Robert Borofsky and Dale Robertson for inviting me. I also thank Peggy Sanday, Marta Rohatynskyj, and Lisette Josephides for their meticulous readings of my book, their detailed critiques that engage some of the issues I raised in *Fruit of the Motherland,* and their participation in ongoing anthropological conversations about gender, egalitarian societies, and the writing of Pacific anthropology.

Peggy Sanday's essay locates my book in what Marilyn Strathern calls "the feminist debate," and particularly in the search for answers to questions about the origins and the universality of sexual inequality. She traces the origins of this debate in European thought to the writings of Plato and Aristotle, where the questions are embedded in the larger one of the origins of social inequality more generally. Their writings are the earliest source in

Western political philosophy, Sanday argues, of the concepts of domestic versus public domains of social life and of ideological associations of the natural with female and the cultural with male. Universality of such domains and associations in human societies was, of course, suggested by pathbreaking feminist anthropologists Michelle Rosaldo (1974), Sherry Ortner (1974), and others as an explanation of what they also saw as the universality of female subordination.

The anthropology of gender suffered a terrible loss with Rosaldo's death in 1981, and we cannot know how her thinking would have evolved. Sanday suggests a softening of Rosaldo's position on the universality and separateness of a domestic and a public domain of social life in her last published works. (This is arguably visible in Rosaldo 1980.) Indeed, Sanday's own article in the original volume, *Woman, Culture, and Society* (1974)—edited and obviously thoroughly discussed by Rosaldo and her coeditor, Louise Lamphere—suggests that domestic and public spheres tend to overlap in smaller-scale societies organized primarily by kinship systems. For example, then, marriage alliances and affinal competitions are at the same time kin-based and thus domestic, and overtly political and thereby public. (Sanday's current essay review sheds an intriguing light on the history of a subdiscipline when she recalls her own objections, and those of some other contributors, around 1974, to the idea that the *Woman, Culture, and Society* volume should have "theoretical coherence . . . so early in the game in the absence of a solid body of ethnographic data on women's activities.")

Sherry Ortner (1990, 1996), as Sanday notes, has modified her views on the universality of nature and culture as distinctive ideological domains through years of vigorous academic debate, the accumulation of ethnographic evidence, and her own, more recent, inspired readings of classic ethnological reports. I return to the nature/culture issue below.

Anthropologists—not just anthropologists of gender—in the present moment show great caution about addressing the broadest issues of ethnological theory, including questions about the origins and universality of social inequality. For most of us, this is only prudent, and a good anthropological variant of the scientific method of empirical research. I greatly admire the intellectual daring of the exceptions among our contemporaries—scholars such as Marilyn Strathern (1988), Annette Weiner (1992), Raymond Kelly *(Constructing Inequality),* and Maurice Godelier (1998)—who do address these kinds of questions, challenging the rest of us to examine and compare our own ethnographic data in light of their insights, and expanding our thinking about human sociality. These scholars are continuing a tradition: anthropological engagement with Pacific Islands societies has long produced theoretical insights (e.g., Mauss 1923–1924; Mead 1935; Bateson 1936).

Contemporary Pacific theorists generate their ideas from a substrate of long-term, detailed ethnography. I fully agree with Sanday's advocacy of ethnographic particularism, of documenting discourses and individual actions, and of reporting and analyzing "conflict, variability, and contradictions . . . in ethnographic field research" (Sanday 1990:1). My intentions in the research and writing of what became *Fruit of the Motherland* were to carry out and report on my holistic study of an island culture never previously documented, with special attention to what we nowadays call the gendered aspects of social life. Only then could I write about whether the ethnographic data, in their spectacular messiness and inescapable contradictions, seemed to confirm or refute existing anthropological ideas about equality and inequality, and whether they suggested any newer ways of thinking about these phenomena.

Sanday argues in her review that cultural relativism has failed to gain a foothold among feminist anthropologists, who, she says, unquestioningly accept an erroneous "doctrine of universal sexual asymmetry" and thus alter and distort their own ethnographic observations and writings. I do not see my reporting of a less than perfect state of gender equality on Vanatinai as my personal share of some more general failure among feminist scholars to question the universality of male dominance. I think it has been thoroughly questioned. My published findings arise directly from my diverse, and irreducible, ethnographic observations. They derive from my perceptions of the actions and statements of the islanders themselves. They are based, in other words, on ethnographic particularism, the research method championed by Sanday. As I wrote in the preface to *Fruit of the Motherland,* "this book, I think, will fail to satisfy either of two conflicting feminist agendas that I have encountered previously when describing my research to others. The first is the wish to find corroboration of universal male dominance and the universal oppression of women, and the second is the desire to learn that, somewhere in the world, there is a place where sexual equality is real and absolute" (p. xii).

To me, ethnographic particularism has to involve comparing discourses by or about women with those by or about men in the same society. This is true whether the research is explicitly about gender issues or not, in order to avoid gender bias in our analyses and reports of cultural phenomena. In the case of my Vanatinai research, I see the need for cross-sex comparison as especially acute for statements or actions spontaneously explained by islanders to me, or observed directly by me, as opposed to those I elicited through my own labored questions, which are inevitably loaded with Western-derived preconceptions and philosophical categories. We need to know whether ideologies, perceptions, and actions are similar or not—across gen-

dered boundaries and other intracultural divides—before we attempt to generalize about symmetry and disorder in any society.

Sanday explains that she currently eschews the methodological and analytical strategy of cross-sex comparison in her own research, and she believes it obscures her understanding of gender among the Minangkabau. I believe, on the contrary, that intracultural comparisons across culturally marked categories such as gender and rank lead—in part, and as much as any anthropological outsider's approach can—to an ethnography framed not in whatever are the currently fashionable theoretical terms, but in those of indigenous thoughts and actions. Some of the most intriguing ethnological problems only arise when we compare contradictions and disparities across categorical boundaries—those of the people we study and our own—and try to discern the ways in which these boundaries themselves may blur.

I am especially honored by Marta Rohatynskyj's comments on some of the parallels she sees between *Fruit of the Motherland* and *Argonauts of the Western Pacific.* I found that the more time I spent doing fieldwork in the southeastern islands of New Guinea, and then struggling to make sense of my notes and memories, the more I valued the magnitude and subtlety of Bronislaw Malinowski's achievements as field researcher and anthropological writer.

I would like to address particularly the aspects of Rohatynskyj's critique suggesting that some of my theoretical analyses of gender equality and inequality on Vanatinai "are set in a discourse that literally ran its course some time ago." She refers specifically, as examples, to my engagement of "classic works" from the early 1970s, Alice Schlegel's cross-cultural study of women, men, and authority in matrilineal societies (1972), and Sherry Ortner's (1974) essay, "Is Female to Male as Nature Is to Culture?" One of the manuscript reviewers of *Fruit of the Motherland* objected similarly to my extended discussion of Vanatinai philosophy and sexual division of labor in the context of the nature/culture debate—raised in Ortner's article and discussed in a variety of later works—claiming the issue had already been resolved and was no longer relevant. I repeat here and elaborate on some of the arguments I made in rebuttal to my editor and the press committee at Columbia, because they are pertinent both to this book review forum conversation and to the subject of anthropological theorizing and cross-cultural comparison more generally.

I do not believe the fact that a theoretical issue was first raised twenty years ago is sufficient to disqualify it from contemporary discussion. This attitude reminds me irresistibly of the judgment rendered by a fictional resident of my hometown of Los Angeles, Cher Horowitz, in the movie *Clueless,* "Those are *so* last year!"

Cher was talking about a pair of red shoes, as I recall. But anthropological fashions in theory seem to shift just as dizzyingly, as when many North American graduate students regard with suspicion anything written more than three or four years ago (unless its author is a Continental philosopher).

Sherry Ortner's analyses of the cross-cultural ideologies of nature and culture, female and male—inspired in part by writings of Claude Lévi-Strauss translated into English in the 1960s—were amplified and sometimes disputed by later feminist scholars in light of other bodies of ethnographic evidence (e.g., MacCormack and Strathern 1980). They were also discussed at length by Maurice Godelier in his brilliant analysis of social inequality among the Baruya of interior New Guinea, not published in English until 1986. Sherry Ortner herself in 1990 published a longer, revisionist article that detailed her reconsideration of the issues of universal female subordination (she no longer believes in it) and of gender ideologies (she suggests we think of gender hegemonies within each culture, sets of multiple and sometimes contradictory ideologies of gender, certain ones prevalent in each kind of social interactions). I discuss all of these later theoretical developments in *Fruit of the Motherland* and try to show where the ethnographic material I collected on Vanatinai fits in.

Further evidence that the discussion of nature and culture has not "run its course" in anthropological theorizing, with or without reference to Ortner's article, came at or after the time my own book was published. Tanya Luhrmann's (1993) analysis of neopagans in Britain and the United States and their perceptions of spiritual links between nature and the feminine (true of ecofeminists more generally), and Philippe Descola's influential writings on nature and society in the Amazon, which begin from the premise that they are two distinct philosophical domains (1992, 1994; Descola and Palsson 1996), are two further kinds of meditations on the topic.

Sherry Ortner herself gave a paper at the American Anthropological Association meetings in 1995, called "So, *Is* Female to Nature as Male Is to Culture?" Part of a general retrospective on feminist anthropology, by the time Ortner began her talk, every seat in the room was filled, people were sprawling on every available bit of carpeted aisle, the air temperature had risen alarmingly, and crowds of anthropologists pushed against the back doors, emitting an angry buzz as it became clear there was no way they could either get in or hear what Ortner was saying. Ortner published the paper in 1996 in a collection of essays, adding an introduction explaining, " 'Is Female to Male . . . ' [her 1974 article] has continued to have a life of its own, well into the present."

The question, of course, is whether we can generate any interesting insights by comparing our own more recently gathered ethnographic data

with the theories and ethnographies of earlier writers. I believe I did so in *Fruit of the Motherland,* by using various aspects of the nature/culture debate to think about Vanatinai ideologies of gender and their implications.

As a point of comparison, Raymond Kelly's 1993 tour de force study of social inequality among the Etoro, also discussed in this book review forum, takes as its starting point an essay in feminist anthropology from 1981, Jane Collier and Michelle Rosaldo's theoretical analysis of brideservice versus bridewealth societies. (I note in my own book that Vanatinai is simultaneously a brideservice and a bridewealth society, and consider the meanings of that.) Kelly also makes good theoretical use of another 1981 paper in feminist anthropology in developing his own core concepts of prestige, stigma, and cultural hierarchies of virtue: Sherry Ortner and Harriet Whitehead's discussion of prestige systems in gender ideologies and sexual divisions of labor cross-culturally, and their implications for gender inequality. (I also discuss Ortner and Whitehead on gender and prestige in some detail, in relation, for example, to female participation in ceremonial exchange on Vanatinai and to the high ideological value placed on both maternal and paternal nurture.)

Lisette Josephides's review of *Fruit of the Motherland* notes its grounding in "rich ethnographic fieldwork" but calls for more "accounts of specific instances of social practices," individual actions, and "people's own narrative understandings of their situations." This is my own most substantial criticism of the book as published. After losing some skirmishes with editors, manuscript reviewers, and press committee members who found my manuscript too long, too ethnographically detailed, and with too many reported conversations, we reached a compromise. I left the narrative ethnographic introductions to most of the chapters but took out sections of just the kinds of detailed accounts that Josephides calls for, of individuals negotiating and reshaping island customs to their own ends and commenting on their own actions and those of their neighbors. These included, for example, a big-woman who far outshone her husband in interisland renown, a wife who flatly refused her big-man husband's plan of bringing home his mistress as a cowife (and put a stop to the affair), and the middle-aged sorcerer and witch who openly cohabited, even though they were uncle and niece by local matrilineal reckoning.

Josephides is right, I think, that including more individual voices in the text would have made my theoretical arguments more convincing. It would also be more effective in bringing Vanatinai to life for the reader. I am currently completing another book, a narrative account of my experiences on Vanatinai and nearby islands, that tries to do just that (Lepowsky n.d.). Of course, the trade house that has contracted for my next

book does not expect, or want to publish, a conventional anthropological monograph.

Writing Pacific Islands Ethnographies

This seems like an opportune point to do one of the things Robert Borofsky has asked of my reviewers and myself: to comment further on the writing of anthropology and the process of communicating to a wider audience. I would like to discuss frankly some of the conditions of anthropological knowledge production and communication.

To restate some of what I said in my commentaries for the *Anthropology Newsletter,* I do not advocate that all anthropologists, all of the time, write about their research in language and venues that make their findings accessible to the general public. (I certainly do not always want to do so.) The primary purpose of specialization in higher education, and in forming disciplinary communities of scientific or humanistic researchers, is to advance knowledge and insight in a particular field beyond contemporary boundaries. Specialist journals and, traditionally, university presses are vehicles for this kind of communication, allowing us to speak to each other in a shorthand that obviates the need of explaining basic disciplinary histories and terminological meanings to recent high-school graduates and browsers in chain bookstores.

Even so, several recent trends complicate the idea of specialist communication. These include the growing fragmentation of anthropology as a discipline; rejections, among some of our colleagues, of notions of shared history and methodological approaches; and arguments over the content and directions of our major journals, most publicly the *American Anthropologist.* More broadly, it includes recent moves among major, nonprofit university presses to improve their bottom lines and stem their financial losses by capturing a larger segment of the mass audience for quality nonfiction, competing for works that might only a few years ago have been midlist titles at trade publishers. Editors at trade houses, swallowed up by corporate mergers, are under growing pressure to find the next best-seller and to reject books that might sell only ten thousand copies. Farther down the food chain, the anthropological monograph that is expected to sell less than a thousand copies to other specialists is one increasingly endangered species among many in the publishing jungle of the late 1990s.

This only sharpens a paradox I noted in my *Anthropology Newsletter* commentaries. Ever since the 1920s, most established anthropologists have been at best suspicious and at worst disdainful of books by their colleagues that were widely read outside the discipline. Josephides makes the rather

startling charge that by publicly acknowledging, in the pages of the *Anthropology Newsletter,* this disciplinary resistance to writing for nonspecialists, I marginalize anthropological specialists, trivialize their work, and undermine our profession by blaming anthropologists for their current institutional problems. Undeterred, I offer the following observations, which are based on both my research of the history of anthropology—specifically the history of women in American anthropology—and my own ethnographic observations.

Beginning with the years following World War II, junior anthropologists have been explicitly socialized—by advisers, peers, journal editors, and manuscript reviewers—to avoid writing to communicate with anyone other than subdisciplinary specialists if they want their work published in scholarly venues. They have been warned that they need to produce a tightly focused scholarly monograph, preferably with a university press. The sanctions have long included the kinds of rejections that can be fatal to a professional career as an anthropologist: failure to have a dissertation approved as originally written, have articles and books published in scholarly venues, secure an academic job, or get tenure or promotion.

In the 1990s, unfortunately, university presses are far less likely than they were even a decade earlier to accept an esoteric scholarly work, however valuable, written in the style Margaret Mead once identified as being historically derived from the nineteenth-century German university dissertation. This does not imply that we should immediately abandon all attempts to write and publish monographs that advance theory or cross-cultural comparison in one domain of anthropology but that are inaccessible, in their content or rhetoric, to nonspecialists even within the profession, let alone the general reader. It does mean that we and our students need to confront the contemporary realities and contradictions of disciplinary traditions and the academic marketplace, and to reflect on the implications of their cultural and historical transformations, for our own work and for intellectual communication more generally. The current realities challenge us to evaluate both specialist and nonspecialist anthropological writings in terms of their quality, contribution to knowledge, and rhetorical aims. We would be wiser not to regard different genres and rhetorical styles as being in competition, and not to reject more broadly accessible writings as automatically of lesser intellectual value.

Josephides makes a small but revealing error when she writes that I advise anthropologists, in the *Anthropology Newsletter* commentaries, "to have a publicist." I was passing on something I learned myself only after my book was published, which is that anthropologists already have publicists. Jeff Iseminger, assistant director of the University of Wisconsin News and Information Service, was the one who told me that part of his job was to publicize the research and writing of university faculty, including me, for the greater

glory and, indirectly, financial support of the institution. (This is how the world more often hears about the latest cancer research breakthroughs in university laboratories. At my own institution, the research university of the state known as "America's Dairyland," this is also how newspapers around the world recently picked up the story of the world's first calf fetus cloned from a skin cell of an adult cow.) Mr. Iseminger previously held a similar post at a small, religiously affiliated college, and he noted that even smaller North American institutions employ staff members to communicate with the public, including alumni, legislators, and journalists, about the research and general worthiness of their faculty and students. From Susan Skomal, of the American Anthropological Association, I later learned that one function of the AAA is to communicate anthropological findings to the public and emphasize the significance of the discipline for human understanding. She coordinates publicity for all members, academically affiliated or not, editing and distributing press releases and contacting reporters.

In other words, formal means for communicating research findings to a wider public have long existed as part of the institutional cultures of universities and professional associations in North America. But anthropologists have made use of them far less than our colleagues in fields as varied as astronomy, biomedical research, and psychology. This is just one of many reasons why institutional support for anthropology has shrunk in the last couple of decades in comparison to most of the natural and social sciences. It is one, though, that representatives of our profession—those who feel so moved and who have the communication skills—can do something about.

Josephides disparages the quality of my writing. I can only say that *Fruit of the Motherland* continues to be used, several years after its publication, as required reading in classes ranging from large introductory lecture classes to graduate seminars in anthropological theory, and it continues to sell in general-interest bookstores.

In her review essay, Josephides is not calling for the inclusion of individual, indigenous voices in our ethnographies because they make our writing more accessible to nonspecialists—which they do—but because they are theoretically *au courant* in the 1990s, as in Lila Abu-Lughod's call to anthropologists to "write against culture" (1991). Including "individual voices" in our texts has in fact been *de rigueur* in feminist scholarship more generally since at least the 1980s, when writers such as sociologist Ann Oakley (1981) and historian Susan Geiger (1986) took up the charged issue of the power relationships between the feminist researcher and her subject, especially across differences of class or race (see also sociologist Judith Stacey 1988 and Abu-Lughod 1990). Though written with scrupulous ethical concerns, one problem with prescriptions for reducing the power differential in ethnographic research by printing the words of individual women is that

they can sound a bit naive, as with Geiger's extolling of life history as a feminist method, usable across disciplines, that not only gives voice to women previously written out of history but is "non-hierarchical," doing away with the power imbalance between researcher and researched.

Josephides's call in her review for "ethnographies that speak" sounds very much like yet another act of anthropological ventriloquism. In relation to New Guinea Highlanders or islanders of the Coral Sea (or Bedouin Arabs), we anthropologists are the ones who are selectively hearing their voices, remembering and recording them, translating them, and writing them down in our articles and books. We are the ones who not only ask the questions but edit the texts and make the contracts with publishers. And we are writing these texts, and deploying the voices of our anthropological subjects, toward our own philosophical, theoretical, academic, or activist ends, stated or unstated, conscious or unconscious. This is yet another variation on Malinowski's now famous musings about the Trobriand Islanders, in what he thought was his private diary: "Feelings of ownership. It is I who will describe them or create them" (1967:140).

I do not wish to minimize the problem here, either for ethnographic representation in a postmodernist era where everybody's ethnographic authority is under suspicion—by our colleagues and often our subjects—or for the worthy goal of a less exploitative anthropology. I agree with Josephides, and Abu-Lughod, that including the testimonies and commentaries of individual women and men in our ethnographies helps us portray and understand better the diversity of action and speech in a given society and helps lead our readers to their own conclusions. I do not agree with an alternative solution proposed by Johannes Fabian, in a public lecture in 1986, to the dilemma of power imbalances in writing the anthropology of non-Western peoples, which was that we European anthropologists should all just quit writing. (I notice that Fabian recently published another book.)

I do not believe, though, that all monographs in social/cultural anthropology from now on need to weave individual voices into their texts in order to represent ethnographic realities, either to "demonstrate an argument" or to "describe life as locally lived," as Josephides seems to imply. This is just one rhetorical strategy of ethnographic testimony and reportage, a very old one in anthropological writing, but one valorized in the last decade or so by influential theories of text production. In using this rhetorical device, we should not fool ourselves that our ethnographies are speaking, or that we are "giving voice" to somebody else; we are not anthropological gods. We are only trying to represent as best we can—either to anthropological specialists or to a potentially global audience of readers—the richness and complexities of lives lived on the other side of a cultural boundary from ourselves.

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