PACIFIC STUDIES

A multidisciplinary journal devoted to the study of the peoples of the Pacific Islands

SEPTEMBER/DECEMBER 2004

Anthropology
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PUBLISHED BY
THE PACIFIC INSTITUTE
BRIGHAM YOUNG UNIVERSITY–HAWAI'I
IN ASSOCIATION WITH THE POLYNESIAN CULTURAL CENTER

Articles and reviews in *Pacific Studies* are abstracted or indexed in *Sociological Abstracts, Linguistics and Language Behavior Abstracts, America: History and Life, Historical Abstracts, Abstracts in Anthropology, Anthropological Literature, PAIS Bulletin, International Political Science Abstracts, International Bibliography of Periodical Literature, International Bibliography of Book Reviews, International Bibliography of the Social Sciences, Anthropoligical Index Online, and Referativnyi Zhurnal.*

PACIFIC STUDIES

Editorial Policy: *Pacific Studies* is published quarterly by The Pacific Institute, Brigham Young University–Hawai'i #1979, 55–220 Kulanui Street, Lā'ie, Hawai'i 96762, but responsibility for opinions expressed in the articles rests with the authors alone. Subscription rate is US$40.00 yearly, payable to The Pacific Institute. The Pacific Institute (formerly The Institute for Polynesian Studies) is an organization funded by Brigham Young University–Hawai'i. The Institute assists the University in meeting its cultural and educational goals by undertaking a program of teaching, research, and publication. The Institute cooperates with other scholarly and research institutions in achieving their objectives. It publishes monographs, produces films, underwrites research, and sponsors conferences on the Pacific Islands.

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ISSN 0275-3596
ISBN 0-939154-77-3

PACIFIC STUDIES

BACK IN THE FIELD AGAIN

LONG-TERM FIELDWORK IN OCEANIC ANTHROPOLOGY

Guest Editors

JOHN BARKER
ALAN HOWARD

A *Pacific Studies* Special Issue
VOL. 27, NOS. 3/4 (SEPT./DEC. 2004) • LĀ'IE, HAWAI'I

Volume 27 September/December 2004 Number 3/4

Special Issue

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LONG-TERM FIELDWORK OCEANIC ANTHROPOLOGY

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PUBLISHED 5/2007

PACIFIC STUDIES

SPECIAL ISSUE
BACK IN THE FIELD AGAIN
LONG-TERM FIELDWORK IN OCEANIC ANTHROPOLOGY

Vol. 27, No. 3/4 Sept./Dec. 2004

INTRODUCTION

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ANTHROPOLOGISTS HAVE OFTEN BEEN ACCUSED of exploiting the people they study—of entering a field site, staying for a year or so, then leaving to pursue their own career self-interests armed with knowledge expropriated from the community that had hosted them, never to return. A corollary criticism of one-shot fieldwork is that it too often freezes ethnography in an unrealistic "ethnographic present," depriving the culture studied of meaningful history. None of the contributors to this volume can be accused of such shortcomings; all have returned to their communities multiple times and have documented changes over time.

Long-term fieldwork is not new to the discipline of anthropology. Franz Boas, for example, made repeated visits to the Northwest Coast of North America, and to the Kwakiutl in particular. However, his brand of fieldwork, based mostly on interrogating a few key informants, was generally replaced by a paradigm of intensive participant observation initiated by Bronislaw Malinowski in the Trobriand Islands. Although Malinowski spent nearly three years in the Trobriands, between 1915 and 1918 (albeit as the fortuitous result of his being interned there during World War I), for some reason the rule of thumb for dissertation research in social and cultural anthropology became a one-year expedition. Perhaps this was to suit the academic calendar, on the one hand, allowing students to return to their home institutions at an appropriate time to begin writing a thesis. Longer field trips were often discouraged on the grounds that they would delay the graduate's

professional career. On the other hand, a year was thought to be minimally necessary to observe an annual cycle of events, an idea derivative no doubt from temperate-zone agricultural and transhuman practices in the northern hemisphere.

In any case, many ethnographers have spent a year or so doing dissertation research on a one-time basis, more or less establishing their reputations on publications derived from that time-limited point of view. Some have gone on to conduct research with other people in a similar vein, thus gaining a comparative, though still time-restricted perspective. To be fair, one must acknowledge that, not so long ago, traveling to many of the sites in which ethnographers worked was not easy. Transportation to and from the more remote locations was problematic at best, and communication channels were so restricted that keeping in touch was not practical. Conditions favoring returns to original field sites in Oceania began to improve from the 1970s onwards. The rapid and vast expansion of tenured academic positions in anthropology in Western countries, coupled with the establishment of new regional universities and research centers in the Pacific Islands, made it easier for researchers to justify and fund innovative projects that required return visits. The costs of travel dropped dramatically, even to the most remote areas, not only enabling anthropologists to return to their field sites for regular short visits but also making it possible for members of these communities to visit them. Finally, and not least, communications improved in much of the region, allowing instantaneous contact between researchers and members of host communities by satellite phone or e-mail. These trends are not evenly distributed—there are parts of Melanesia that are today more inaccessible than even a decade ago because of the breakdown of state services and outbreaks of local violence—but in general it has become easier for anthropologists to return to the communities they study on a regular basis, and this has had a significant effect on the practice of the discipline.

Unfortunately, the stereotype of one-shot research persists, perhaps because so little has been written to tell the story of long-term projects. It was this deficiency in the literature that led John Barker and Ann Chowning to organize an informal session on back-to-the-field experiences at the Association for Social Anthropology in Oceania meetings held in Vancouver, British Columbia, in 2000. In truth, we found listening to one another's accounts exhilarating, and decided to continue through the ASAO process of developing and refining our papers at working sessions in 2001 and 2002 and a symposium in 2003. The articles included here were presented at that symposium.

There are two basic modes of longitudinal research. One type involves research conducted by a team of ethnographers, sometimes by several gen-

erations of graduate students. Among the better-known projects of this type are the Harvard Chiapas Project (begun by Evon Vogt in 1957), research among the Ju/'hoansi-!Kung (begun in 1963 by Richard Lee), and work done in Tzintzuntzan, Mexico (begun in 1944 by George Foster); for detailed accounts of these and other long-term projects see Foster et al. (1979) and Kemper and Royce (2002). The other type involves return visits to a field site by the same one or two researchers over a span of years. This is a more common occurrence in the profession and is exemplified by the contributors to this volume. Research in this vein may bring ethnographers back to their field sites only after long absences (for example, Raymond Firth's visits to Tikopia in 1928–1929, 1952, and 1966), or they may make repeated visits after relatively short intervals over a span of years. The length of visits may also vary from a week or two to a few years, but it is clear from all accounts that going back to the field repeatedly results in a very different kind of ethnography.

Raymond Firth, in a paper describing his encounters with Tikopia over sixty years, summed up the situation facing long-term researchers succinctly: "anthropology has changed, I have changed and the Tikopia have changed" (1990:241). His observation suggests a template for thinking about the implications of longitudinal research.

Anthropology over Time

During the mid-twentieth century, the dominant paradigms in social and cultural anthropology were synchronic in nature: functionalism, structuralism, culture and personality, and so forth. This is not to say that history and change were totally ignored. As Firth pointed out in his article, he and some of his colleagues, such as Isaac Schapera and Monica Hunter, were very much aware of the significance of historical factors in the interpretation of the state of a society (1990:244). Likewise, American anthropologists, following in the Boasian tradition, included historical analysis in their accounts and introduced the concept of "acculturation" to deal with change. But for the most part, the history involved was either inferred or derived from earlier writings. As long as anthropologists were basing their accounts on short-term, one-visit field trips, they lacked a sense of the dynamics of historical process. As Thayer Scudder and Elizabeth Colson wrote, reflecting on their long-term study of the Gwembe of Zambia, anthropology's proper subject matter

> is people making decisions through time in contexts which change both because of their own action and because external conditions change in ways which neither they nor we are able to anticipate. Much of anthropology is still tied to system concepts derived from

> biology and the physical sciences, even though we chaff against them and criticize them. A major reason for this problem is that the most common type of fieldwork still centers on a single slice of time. This predisposes the use of terms and concepts that emphasize static as opposed to dynamic relationships and stresses integration as opposed to flux. (2002:214)

One consequence of taking a synchronic view, as Mervyn Meggitt has pointed out, is that "stability is taken to be the norm and change the problem to be explained, whereas, if anything, the assertion should be the other way around" (1979:122).

Repeated visits to a field site change projects by significantly altering the nature of the researcher's database. A time-restricted body of data, no matter how rich, gleaned from a single field trip, tends toward a finalized "ethnographic present," whereas repeated visits render research a work in process. This changes the way ethnographers write and publish; instead of authoritative accounts of how things are, our publications become progress reports. We are well aware that the next time we return things are likely to be quite different, that the notion of a typical year is as much a myth as the notions of stable structures and bounded communities. As a result, we are inevitably cast in the role of historians, whether we conceive of ourselves that way or not. Enhanced attention to context is a natural result of increasing familiarity with a community, Howard and Rensel point out in their article, and propels the ethnographer toward historicism and away from generalizations. At the same time, as vividly illustrated in Kjellgren's discussion of changing styles in Aboriginal art of the East Kimberley, return visits make it possible to separate historical contingencies from persistent cultural patterns.

In many ways, as our understandings deepen, we are better able to anticipate people's behavior, but inevitably anomalies appear, our expectations of how key events will unfold prove incorrect, and people surprise us. Such occurrences tend to produce a more humble fieldworker and more humble ethnography (see, e.g., DeVita 1990). That humility manifests itself in scholarly works that are more nuanced—sensitive to ambiguity and historic conditions. It also manifests itself in works deliberately produced for the use and benefit of the communities researched. Most contributors to this issue have produced such work, ranging from shared copies of rough field notes and photographs, through self-published collections of local narratives and histories, to community Web pages, grant applications, and action plans. Such involvement does little to advance one's academic career, but it is the most important thing an anthropologist can do. And there is nothing more rewarding.

Ethnographers through Time

Anne and Keith Chambers tell in their article of being declared "of the island" as soon as the Nanumea people recognized they were not merely transient visitors but planned to live and work in the community. This is a common, near-unavoidable experience in ethnographic fieldwork. Presented with strangers who intend to reside among them, members of a local community have little choice but to place the newcomers into social categories that will guide appropriate relationships and allow the local people some control. Anthropologists are keenly aware of this and have engaged in much discussion over the years on the research implications of personal factors like gender, age, marital status, and ethnicity upon one's reception in a host community and access to various types of ethnographic information. Time and the opportunity for repeated visits both deepen and widen the fieldworker's social status. As researchers age, marry, and have children, the types of people they can comfortably associate with changes. A young unmarried fieldworker might be classified as a "boy" or "girl" who can interact easily with youths, less comfortably with elders. With time, the possibilities may reverse themselves as the roles of both ethnographer and people in the community change. Indeed, as Gibbs shows in his contribution, given the opportunity, an ethnographer may come to assume a wide range of roles. This provides an opportunity for the anthropologist to gain a more balanced understanding of social life.

Returns, however, typically signal much more profound transformations in the relationship between researchers and host communities than simple shifts in status. In the small Oceanic communities described in the following pages, relationships cannot be taken for granted; they are worked on and validated through a constant process of give and take, an ongoing exchange of advice, labor, and material gifts. Separations threaten social relationships. In the Maisin communities described by Barker in this volume, parents sometimes regard children who have not kept in touch or sent gifts after taking jobs in distant towns as "dead." Given such assumptions, it is not surprising that islanders often treat researchers returning from a great distance and after a long time as relatives feared to have been deceased or as prodigal sons. The return, especially the first return, signals an enduring commitment by the researcher to the community. With repeated visits that commitment grows. People in the community recognize this, which significantly reduces the social distance that characterizes initial fieldwork. Virtually all ethnographers who have engaged in long-term projects report the same thing—that over time they feel more and more "at home" in the community they have been studying. In some cases, their commitment to the study community supersedes allegiance to their original homeland or to the academic community. The transformation

is from (more or less) scientific observer to active participant in community affairs, and in many cases, to ready advocate for community causes.

Involvement in a community over time inevitably deepens and complicates relationships. Certain families are likely to become "our" families as we spend increasing amounts of time with them, share meals, exchange gifts, contribute to the education of children, and foot the bill for medical expenses. As Flinn's account of her fieldwork experience indicates, increasing familiarity with the culture facilitates a comfortable, unself-conscious pattern of interaction, which often has the effect of encouraging people in the community to talk more freely about previously guarded topics. At the same time, people feel freer in making demands of "their" anthropologist and become more open in their criticisms. It becomes much more difficult for anthropologists to avoid being drawn in to local political contests. As relationships deepen and intensify—as one comes to know and be known in ever more personal terms—any sense of the people being studied as "the other" is likely to completely disintegrate. For some fieldworkers, the very core of their values and beliefs may come to mirror those of their adopted community as much as or more than the ones from which they came.

Long-term research poses serious challenges to an ethnographic project. The sheer size of an ever-increasing database, filled with decidedly nuanced, highly contextualized, and often contradictory information, may make writing anything in an anthropological vein difficult. Questions may also arise: about maintaining objectivity as one becomes more actively drawn into social life, about losing the capacity to see the forest for the trees, about developing tunnel vision. A variety of factors can influence the degree to which such problems emerge and affect research outcomes, but in most instances the trade-offs clearly favor deeper involvement. As all of us who have done long-term research can attest, the data we acquire are far richer, deeper, and more representative of life as lived. We have become aware that our original attempts at objectivity were more illusion than actuality, that a limited perspective no matter how objective can lead to serious misunderstandings we only come to recognize later on. Another potential cost can be the loss of a sense of excitement, of the freshness that motivates us during our initial field experience. Perhaps; but explaining the changes we witness with each return continues to challenge intellectually. Our excitement might diminish a bit as we shift from being graduate students to established professionals to professors emeriti, but the ever-changing communities continue to stimulate our intellectual juices.

Deeper involvement also raises ethical issues. How do we deal with inevitable conflicts, with factional disputes involving our close friends and adopted families? As active members of a community we not only take on

additional responsibilities, we are drawn into the web of interpersonal alliances, schisms, and power-wielding dynamics that are part and parcel of every community's social life. There are no simple answers. All we can do is draw on our own ethical principles, informed by an understanding of what is considered right and proper in the community at large.

Does this kind of deep involvement in a community render the ethnographer "one of them," so to speak? Obviously not, if membership qualifications include bloodlines or childhood socialization *in situ*. But if we make the distinction between ethnicity as genealogically derived and ethnic communities as networks of committed individuals who are actively involved in the group, it is quite clear that ethnographers can indeed gain membership (just as spouses from elsewhere can gain membership after marrying in). To be relevant, membership requires a personal history of involvement—a temporal span that frames an extensive record of material exchanges; information sharing; active participation in various activities, including disputes; and a general engagement with the immediate concerns of the community's members.

Communities in Time

The contributors to this special issue deal with much more than the implications of fieldwork over the long term for anthropological understandings and personal relationships; they also detail the ways in which the communities they've studied have changed. Those of us who began fieldwork decades ago have seen phenomenal changes take place. To begin with, the political and economic changes over the past few decades have been dramatic. Many of us have witnessed communities governed by colonial powers become parts of nation-states with entirely different political structures. In some of the new nations, coups have taken place; in others, revised constitutions have resulted in shifting political institutions. As a result, structures that once seemed quite stable now appear much less so; in some instances once-familiar institutions have all but disappeared.

Access to modern medicine has allowed populations to grow; access to education has provided opportunities to enter modern occupations and professions; and access to transportation has facilitated dispersion, with people regularly moving in and out. This increased fluidity has rendered the very concept of "community" problematic, a point that Carucci emphasizes in his reflections on fieldwork with the exiled community from Enewetak Atoll. As Robert Kemper, reflecting on changes in Tzintzuntzan, put it:

> Our continuing long-term fieldwork among the people of Tzintzuntzan suggests that we need to rethink our notion of community.

> . . . I am struck particularly by how hard it has become to define who is and who is not a resident of Tzintzuntzan. . . .
>
> What once was treated—by villagers and anthropologists alike—as if it were a "closed" system has become a spatially and temporally extended community whose changing characteristics cannot be ignored. . . . Through our long-term field research, we have seen how the concept of "community" involves not only the sense of physical place *(pueblo)* but also the commitment to common identity and values *(communidad indígena)* regardless of whether the people of Tzintzuntzan are physically resident in the town or living elsewhere. (2002:303, 306)

In the Pacific we have seen islanders become increasingly cosmopolitan and increasingly aware of themselves and their cultural heritage. They are dispersed around the world, trying to cope with the same problems that confront the ethnographers who study them. Our common humanity is now more in evidence than ever, but this very fact poses challenges to our methods and theories. We need "to develop research designs and methods flexible enough to cope with the fluidity of people who move geographically, seize or reject new opportunities (or try to cope with the nonavailability of opportunities), use and avoid new national and international agencies, rethink and cling to old ideologies, and are becoming something else while trying to remain themselves" (Scudder and Colson 2002:204).

The "field," in other words, is changing and will continue to change. As it has changed, anthropologists have become more conscious that earlier identifications of the field with a specific place were also inadequate and misleading. At the same time we resist the notion that the field has become every place, which, in effect, means that it is no place special. This may well be true for many people, but clearly not for the indigenous groups who are the subject of these essays. The Rotumans and Pollapese may be dispersed—indeed, most members of the Enewetak community can only imagine their home island—yet still they all take their identity from a physical place. Most Gija, Maisin, Nanumeans, and Engla continue to live in their ancestral homes. Modern ethnographic fieldwork may well occur in a variety of locations, but to the extent that the researcher identifies with and, indeed, becomes part of a community, his or her reseach will be oriented to a place. This place will certainly be one the anthropologist wants to experience personally and return to. And on that return, she or he likely will be accompanied by one or more members making their own journey home.

The Future

Rumors of the impending demise of anthropology have been greatly exaggerated. The academic discipline is firmly established in universities, museums, and research institutions around the globe. An ever-growing number of graduates make use of anthropological knowledge and methods in a bewildering variety of occupations, most outside the academy. But there can be no doubt that the discipline is changing rapidly or that one of the casualties of change may be Malinowski-style intensive fieldwork and the opportunity for a continuing relationship between a researcher and a "field." Old-fashioned long-term ethnographic research has come under attack within and without the discipline for many alleged sins—for the "theft" of indigenous culture; for arrogantly "silencing" indigenous voices; for lending scientific credence to notions of "otherness" that serve to hide, even abet, oppression; and so forth. For all of the criticism, however, most professional anthropologists still appear to embrace the ideal of intensive fieldwork. A far-greater threat comes from more mundane causes that have reduced opportunities, especially for new students: declining funding, ratcheting pressure from universities for four-year doctoral degrees, and the increasing dangers from disease and violence in many quarters of the nonindustrialized world.

The criticisms and the obstacles must all be acknowledged. Yet we remain cautiously optimistic. For a twenty-year period at most, anthropologists enjoyed relatively generous financial support and a tolerance for "slow" research. That time has passed. Nonetheless, determined individuals before and after this time have been able to undertake intensive, sustained fieldwork. In one of the most significant and hopeful trends, an ever-increasing number of Pacific Islander scholars are taking and remaking ethnographic practices through sustained research in their home communities and diasporas. Some ninety years after Malinowski first set foot in the Trobriands, we have an extraordinarily rich record that demonstrates the utility of the method for gathering sound ethnographic knowledge. It can also be an effective tool for achieving both practical and political ends in ways that are ethical and responsible to the subjects of anthropology research. Our aim in this collection is to add to the scholarly appreciation of ethnography by providing accounts of its use over the long term, a facet that has not been adequately studied or appreciated.

The authors in this volume describe, in quite personal terms, the ways in which the people they have studied (and the "field") have changed, the ways they themselves have changed, and the ways the nature of their fieldwork has changed. We hope that our stories will encourage others, anthropologists and members of the communities they study and work with, to publish their own. Above all, we hope that the accounts are compelling enough to encourage stu-

dents about to embark on research projects in the Pacific to commit to long-term involvement with the communities they study, no matter how defined.

NOTES

The editors take pleasure in acknowledging the many contributions of scholars who contributed their own stories and participated in the four ASAO sessions leading up to this special issue. The articles here are far better for the ideas and enthusiasm of all of the participants in these sessions. We would like to thank in particular Jane Goodale, Jeanette Dickerson-Putman, Joseph Finney, Judith Huntsman, Suzanne Falgout, Adrienne Kaeppler, Jill Nash, and Mark Mosko. Bob Tonkinson proved a kind and wise discussant during the Auckland meetings. We are especially grateful to Ann Chowning who, along with John Barker, initiated and chaired the sessions.

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CONTEXTUALIZING HISTORIES: OUR ROTUMAN EXPERIENCE

Alan Howard and Jan Rensel
University of Hawai'i at Mānoa

Alan conducted fieldwork on Rotuma in 1959–1960 and did not return until 1987, this time with Jan. Together, we have returned nine times since, for visits ranging from one week to six months. During this period the "field" has changed. About four-fifths of Rotumans now live abroad. We have visited Rotuman communities in Australia, New Zealand, Canada, the U.S. mainland, and Europe, and we keep in touch with Rotumans via the Internet. Prolonged involvement has resulted in a shift away from a scientific generalizing viewpoint to one emphasizing context and personal histories. It has also forced us to reexamine our commitments. We now choose to spend our energies in service of the Rotuman community rather than seeking recognition within academia. As a result, we are actively engaged in assisting knowledgeable Rotumans to publish cultural accounts of their choosing and in operating a Web site that serves the global Rotuman community.

REPEATED FIELD TRIPS to a community make it difficult to maintain the detached observational posture that is the hallmark of the scientific method. Although both of us went to Rotuma for the first time (Alan in 1959, Jan in 1988) with science-oriented projects, we found a scientific stance less satisfying with each return. This was not only because our objectivity tended to fade as we became more deeply involved in community affairs, but also because the scientific requirement of parsimony—the foundation for making generalizations—dropped by the wayside. As we developed a personal history in the community, and coexperienced with Rotuman people changes in physical, social, and cultural aspects of community life, our knowledge of context came to overwhelm any general understandings we had formu-

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lated during our initial visits. The more context we included in our understandings, the less consistency we were able to see. What this has meant for us—and we suspect it is a common experience for others like us—is that we have progressively shifted from a scientific to a historicist perspective. We now find ourselves playing the role of community historians, embracing all the context we can muster.

In this spirit we present a personal, historical account of our multiple visits to Rotuma, describing how Rotuma and the Rotuman community have changed, how we have changed, and how our changed relationship to the now-global Rotuman community has transformed the nature of our work.

Part 1: The Initial Fieldwork Period (1959 to 1961) by Alan Howard

I first went to Rotuma in 1959 as a twenty-five-year-old graduate student to do dissertation research. The choice of Rotuma as a field site was more a happy accident than a well-formulated plan. I had obtained a grant from the National Institute of Mental Health (NIMH) to conduct research on bereavement on the island of Ponape (Pohnpei) in the Caroline Islands. However, the administration of the Trust Territory of the Pacific Islands refused permission on the grounds that a government anthropologist was already resident on the island, and as far as they were concerned one anthropologist per island was enough. I was left with a grant and no place to go. My advisor at Stanford University, Felix Keesing, suggested Samoa (where he had worked) and Fiji as possibilities, so I immediately wrote letters of inquiry. The Fiji colonial government promptly replied, informing me that enough anthropological research had already been done among the Fijians and suggesting I go to New Guinea. If I insisted on doing research in Fiji, however, they offered me the possibilities of working with Fiji Indians or going to the island of Rotuma.[1]

In truth, I had never heard of Rotuma, so I rushed to the Stanford library to look it up. In a 1930s edition of the *Pacific Island Handbook,* a one-page article began with a statement to the effect that "Rotuma is one of the most beautiful and romantic islands in the South Seas." I didn't have to read further to make up my mind. The article provided some basic information about the island's size and population, which suited my research plans. It also mentioned a nine-hole golf course and reported that several of the chiefs were adept at the game! Fortunately I did not bring my golf clubs along, for as I later discovered, the golf course, which had been laid out by a British resident commissioner in the 1930s, had long since been replaced by copra sheds.

Getting There

I made all the necessary arrangements and left California in early October 1959. My first stop was Honolulu, where I spent several days at Bishop Museum trying to find out as much as I could about Rotuma. I also met with Alex Spoehr, the museum's director, who warmly encouraged my research. I arrived in Fiji on October 12 and on Spoehr's advice booked into the Korolevu Beach Hotel, on the road between Nadi and Suva. There I met my first Rotuman, Alex Rae—an incredibly impressive man in his sixties. Mr. Rae was well read in several fields and knew more about the history of boxing than anyone I had ever known. He was gracious and supportive of my intentions, suggesting I contact his sister, Faga Hoeflich, when I got to Suva. Only later did I discover that he was the grandson of Maraf Mamatuki, the fabled paramount chief of Rotuma in the late nineteenth century (see Howard 1990b for a more detailed account of my meeting with Mr. Rae and his sister).

When I arrived in Suva I got a rude shock. After my departure from California the colonial secretary of Fiji had sent a telegram to Stanford rescinding permission for me to do research on Rotuma because of recent problems on the island. A land commission had been charged with surveying and registering Rotuman lands, but because of a provision in the regulation establishing the commission—a provision that effectively converted Rotuman land tenure from one based on bilineal inheritance to one of patrilineality—the Rotuman people had rebelled and forced commission personnel to leave the island. A great deal of anger and hostility had been aroused, and the governor of Fiji, Sir Kenneth Maddocks, had decided that an anthropologist arriving in the aftermath of this fiasco might be identified as a government agent, further aggravating an already tense situation.

The governor was away attending a meeting of the South Pacific Commission in Noumea, New Caledonia, but the assistant colonial secretary, Mr. Hill, was encouraging. He told me that he thought an anthropologist could play a constructive role in resolving Rotuma's land problems, so I did not lose hope. Mr. Hill suggested that I make an appointment with the governor following his return to present my case. I agreed, and feeling buoyed by the rapport I had already established with Rotumans I had met, decided to persevere. I knew that Alex Spoehr was one of the U.S. representatives to the South Pacific Commission and sent him an express letter in Noumea, explaining my situation and requesting that he speak to the governor on my behalf. I felt reassured by his return note, offering encouragement and informing me he had talked with Sir Kenneth.

My interview with Governor Maddocks went well. He asked about my research plans and listened patiently. His only concern, he explained, was that

nothing happen to further unsettle the people there. He said that he would cable the district officer on Rotuma, Fred Ieli, and have him ask the Rotuma Council to decide whether to let me come. This was a wise, practical solution and suited me very well. Rather than being "sent" to the island by the colonial administration, I would be "invited" by representatives of the Rotuman people—if they agreed. When I informed Alex Rae and his sister, Faga, about my meeting with the governor, they immediately offered to contact the district officer, a close relative, to support my request. I was grateful and delighted when permission was finally granted, on November 13, a month after my arrival in Fiji. The governor's only stipulation was that I avoid inquiring into land matters or politics while on the island.

Excited about the prospect of going to Rotuma I tried to make a reservation on the next boat, the *Kurimarau,* but was told that the few cabins aboard had already been booked. I insisted that didn't matter, that I would happily travel aboard deck, like most other passengers. "No," I was told emphatically, "Europeans are not allowed to travel on deck. It would set a bad example." Since I had already become aware of ethnic segregation in its many colonial forms, I was not surprised, but found the situation extremely frustrating nonetheless. The next boat on which I was able to book a cabin was the *Yanawai,* scheduled to leave early in December. I decided to be philosophical about the delay and ended up using it to advantage by immersing myself in government archives, learning what I could of the Rotuman language, and taking part in various activities with my newfound Rotuman friends.

As it turned out, the *Yanawai*'s departure was anything but routine. The day before the boat was scheduled to leave, Fiji's workers staged a strike that grew violent, causing much damage to downtown Suva and shutting down transportation. The city was tense, and getting to the ship meant navigating through dangerous territory. It was with great relief that I boarded the boat on December 11, and watched the troubled city fade into the horizon as we sailed away.

The trip to Rotuma took five days, with stops at Levuka, Savu Savu, Taveuni, and Rabi. We reached Rotuma at midnight, December 16, and anchored outside the reef off Motusa, but did not go ashore by launch until shortly after dawn. I then met for the first time the family who would host me for the following year.

There were no public accommodations on Rotuma. Visitors were required to make arrangements in advance to stay with a host family. While on Viti Levu I had been given the option of having the district officer arrange something for me, which I declined in favor of an offer by Lisi, one of my Rotuman friends. She suggested that I stay with her uncle and aunt, Sakimi and Seferosa, in the district of Itu'muta, at the western end of the island.

They had a large house, she said, and only one daughter, Akeneta. Lisi said she would talk to her uncle, who was visiting Suva at the time, and told me that since I planned to go to Rotuma on the same boat, I would meet him before my arrival on the island. As it turned out, Sakimi went on the *Kurimarau* and assumed, since I was not aboard, that I was not going to Rotuma after all. He had therefore made no arrangements to accommodate me and was not present when I disembarked. I did not know this until many months later, however. What I experienced was an initial sense of confusion in which people asked with whom I would be staying, followed by a period of waiting around while being assured that a lorry was coming to take me and my luggage "home." Akeneta showed up after a little while and without hesitation welcomed me warmly. I cannot say enough about how wonderful this family was to me during my time on Rotuma, and how much they made me feel at home.

Adapting to an Isolated Community

As I imagine to be the case with most fieldwork in isolated communities, I experienced intense highs accompanied by periods of great productivity, and depressing lows when all I wanted to do was get away from everyone. When I watched the *Yanawai* depart, realizing the next boat would not likely come for three or four months, I was apprehensive and full of doubt. But it did not take long to feel at home, which in retrospect is quite a testament to the tolerance and patience of my host family and of Rotumans in general. My adjustment was facilitated by the fact that most Rotumans spoke English, which, on the one hand, made it easier to begin substantive research, and on the other, served as a disincentive to learning the rather difficult Rotuman language. On balance my experience during that year was overwhelmingly positive, almost magical at times, as I gained new insights into cultural values and human relationships.

I tried to abide by the restriction placed on my research by the colonial administration, that I not inquire about land matters, but because the land commission's recent visit was uppermost in people's minds they kept initiating the topic in their conversations with me. To my knowledge, few people thought I was a government agent, perhaps because more exotic rumors circulated—that I was a communist spy, or that I was a descendant of Charles Howard, a renegade English sailor who married and made his home on Rotuma in the mid-nineteenth century. One of Howard's grandsons reputedly had left for America in the 1920s and had not been heard from since. "Is it true that Charlie Howard's grandson is your father?" I was asked several times. "Have you come to Rotuma to find your roots?" It was tempting to

leave the answer ambiguous—Charles Howard had sired many children and being his descendant would make me kin to more than half of the population—but I did my best to squelch both rumors.

People wanted to express their views about the land commission fiasco, and wanted me to know why they were incensed—and in the process taught me a good deal about the nature of land tenure. I was therefore gratified, several months later, when Commissioner Eastern Christopher Legge (under whose jurisdiction Rotuma fell) asked me to include land tenure in my research and to suggest ways to deal with the problems confronted by the commission. I was happy to oblige, and produced a report pointing out the practical benefits of the current tenure system, which was remarkably flexible. My recommendation was that they avoid tampering with the rules of succession and encourage Rotumans to work out disputes informally as much as possible.[2]

My research on Rotuma was aided tremendously by two hardworking research assistants, Amai Sakimi and Rejieli Mejieli. They conducted a comprehensive census of the island, recorded migration and marital histories, wrote down life histories, and collected data for a number of special surveys.[3] In addition, my brother Irwin, who was an undergraduate at Reed College at the time, joined me on Rotuma during his summer vacation; he enjoyed the experience so much that he decided to take a semester off to assist me. My field trip on Rotuma lasted almost exactly one year, but with Irwin's help, I spent another six months collecting survey data among Rotumans in Fiji.

The Evolution of Research In Situ

For my NIMH-funded study of bereavement behavior, I had hypothesized that a variety of cultural variables, and especially the centrality of elaborate funeral rituals, would mitigate the severity of bereavement problems. The issue of grief-related mental health problems had become a hot research topic at the time.[4] My dissertation proposal had a somewhat different focus; it specified that I would conduct an ethnoscientific study of ceremonial structures, ethnoscience being the current fad in anthropology. During this initial visit, though, abstract theory gave way to a topic of compelling practical concern among the Rotuman people, and I ended up doing my dissertation on land tenure (Howard 1962; also see Howard 1963, 1964a).

In fact I ended up collecting data on just about everything that I could imagine might interest my anthropological colleagues. I organized a census of the entire island; copied all the registers of births, deaths, marriages, and divorces back to 1903; recorded genealogies; analyzed court cases concerning land disputes; collected life histories; administered thematic apperception tests and interpersonal relations questionnaires; and spent lots of time

attending ceremonies of all kinds. In the final analysis I think I learned more from just talking and hanging out with people I came to feel comfortable with. In retrospect, however, my relationship with Rotumans at the time seems rather superficial compared to now. They came to recognize that I was a student interested in their history and customs, and for the most part they were appreciative and prepared to help. Still, I was cast in a rather restricted role, not only by them but also by my own obsession with collecting data. From their standpoint I was to be treated like an honored guest, which meant with formality. At ceremonies and feasts I was seated with the chiefs and required to sit cross-legged for hours while being served enormous quantities of food and entertained by well-rehearsed dancers.

I came to understand the underlying rules of social relations in terms of restraint (a product of respect) and license (a product of intimacy) (see Howard 1964b for a formalization of this approach to social relationships). Because I was categorized as an educated "European," people treated me with far more respect than I felt entitled to, which meant their dealings with me tended to be somewhat formal and restrained.

Within my home village of Lopo I became a familiar figure, and people eventually relaxed their guard when I was around. I didn't realize how much I had become a part of the community until shortly before leaving, when an

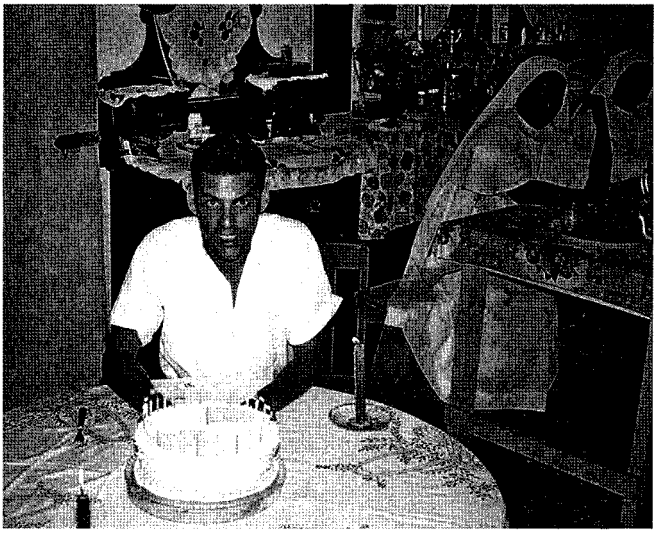

FIGURE 1. **Alan Howard celebrating his twenty-sixth birthday on Rotuma at Upu Catholic Mission Station, in April 1960.**

elderly woman neighbor (with whom I had had little contact aside from the polite greetings one exchanges with casual acquaintances) burst into tears when I said farewell. I must admit that I eventually shed quite a few tears of my own, in part, I suppose, because I never expected to see any of these people again.

During the year I was on Rotuma I went through a series of phases concerning my understanding of the culture. At first everything seemed strange and undecipherable, particularly that part of the culture embedded in the Rotuman language, which I found difficult to learn. Then, after a couple of months, I found everything falling neatly into place—the result of a sweeping theory of the culture, devoid of any significant complexity. It didn't take long for that first set of insights to collapse, to be followed by periods of confusion, revisions involving somewhat more complex understandings, more confusion, more revisions, and so on throughout the year. By the time I left I felt I understood the culture pretty well, but I realize now that the illusion of understanding is a difficult thing for an ethnographer to shed. After all, the quest for understanding is a major part of our motivation for doing fieldwork, and to admit to ignorance or confusion is to risk being seen as a failure, by ourselves as well as by our colleagues.

Superficial as my understandings may have been, by the time I left the field I had collected an enormous quantity of data, most of it extremely well organized. I had made a point of not letting more than two or three days go by without taking time to type and organize my notes, using a modified version of Murdock et al.'s *Outline of Cultural Materials* (1945) as a guide. I also spent time writing down whatever theoretical insights I had. It took a lot of discipline to stick to this routine because it sometimes meant not attending events of considerable interest, but it turned out to be an excellent investment in the long run. My early organizing efforts not only made writing my dissertation a breeze (I finished it over a nine-month stretch while teaching fifteen hours per week of first-time courses in anthropology, sociology, the psychology of adjustment, and marriage and the family at Cabrillo Community College in northern California), but they served me well for years afterwards when opportunities arose to publish papers on a variety of topics I had not anticipated writing about.

The Interim

After a year at Cabrillo I took a temporary teaching position at the University of Auckland in New Zealand (1962–1963), where I was given ample opportunity to write. From there I went to Honolulu to take up a joint appointment at Bishop Museum and the Department of Anthropology at the University

of Hawai'i, eventually moving full-time to the university, where I remained until retiring in 1999. Transportation difficulties and time constraints made returning to Rotuma difficult. Boat schedules remained irregular, and not until 1981 was an airstrip opened on the island. I could never be sure, even if I went during summer vacation, that I could get back to my teaching responsibilities in time for the fall semester. In addition I became involved in other research projects in Hawai'i, which occupied what time I did have available. I didn't get an opportunity to return to Rotuma until a sabbatical in 1987.

My contacts with Rotumans during the interim were sparse. I discovered that Tivaknoa Ieli, a young Rotuman woman, was attending the Church College of Hawai'i (renamed Brigham Young University–Hawai'i in 1974) in Lā'ie, on the windward side of O'ahu. I made a point of visiting her for the purpose of clarifying some of the information I had collected on Rotuma. This was during my cognitive anthropology period (in the mid-1960s), and having a knowledgeable informant helped to extend my fieldwork experience in significant ways. But after Tiva graduated my contacts with Rotumans ended, except for two brief encounters. In the late 1960s I was visited in Hawai'i by Josefa Rigamoto and his eleven-year-old son Walter. Josefa had been awarded an MBE by Queen Elizabeth and was on his way to England by ship to receive the award. When the ship stopped in Honolulu for a day I picked him and Walter up at the dock and spent the day showing them the sights. Then in 1969 I was on the *Oronsay* en route to Australia when the ship stopped for a day in Suva. This gave me an opportunity to visit with a few Rotuman friends, including Josefa. I also discovered that my host family was in Suva attending the funeral of Sakimi, who had died a few days earlier. I was only able to spend a short time with the family, but it was a poignant and tearful reunion that moved me deeply. After this, for the next eighteen years, my contact with Rotuma was essentially confined to fading memories and forays into increasingly lifeless field notes and archival data. Unfortunately the Rotumans I knew were not great letter writers, and attempts to maintain a correspondence proved futile. I continued to publish about Rotuma in the "ethnographic present" until the mid-1980s, completing a book (Howard 1970) and twelve journal articles and book chapters.

My first return to Rotuma after a hiatus of twenty-seven years was a dramatic and emotional experience. I had mixed feelings about going back, on the one hand reluctant to disturb idyllic memories, on the other hand curious about changes. The desire to see old friends and to introduce my new wife, Jan Rensel, to the culture that had so definitively shaped my professional life proved decisive. The changes that had taken place in the Pacific during the interim were profound. Fiji gained its independence from Great Britain in 1970 and economic development had resulted in significant material

changes. Rotuma now had not only an airstrip, with weekly flights to Fiji, but also a new wharf at Oinafa, where ships could dock. It was no longer necessary to load and unload cargo and people onto punts to convey them between the ship and shore. Knowing that I was going back to a society that had been drawn increasingly into the modern world, I was somewhat apprehensive about what I would find. (For an account of my reflections on changes, see Howard 1991.)

Part 2: Many Happy Returns (1987 to Present) by Alan Howard and Jan Rensel

Our visit in 1987 was only for two weeks; it barely gave Alan an opportunity to become reacquainted with people and places, but for Jan it provided a kaleidoscopic introduction to life on the island. Despite changes, Rotuma had retained its charm. The people radiated the same warmth and generosity of spirit they had in 1960, and to Alan's delight, Jan was as taken with the island as he was. She had recently completed a master's degree in anthropology at the University of Hawai'i and was in a quandary about whether to continue toward a doctorate or to seek other outlets for her talents. Her experience on Rotuma settled the issue. We mutually decided to return to the island the following year so that she could begin dissertation research. A return visit would also give Alan an opportunity to study changes that had occurred since his first visit.

We ended up spending three months on Rotuma in 1988 and returned during each of the next three years—for six months in 1989 (when Jan received a Fulbright Grant to support her research), for two months in 1990, and for a week in 1991. We went again for two weeks in 1994. The 1994 visit was part of a three-month excursion to Fiji, Australia, and New Zealand, where we visited Rotuman migrant communities. Jan received her Ph.D. from the University of Hawai'i in May 1994 for a dissertation focusing on the impact of the cash economy on social life (Rensel 1994). In 1996 we attended the 150th anniversary of the Catholic mission on Rotuma and returned again in 1998 when we also revisited Rotuman communities in Australia and New Zealand. Our most recent visits to Rotuma were in 2001 and 2004.

Going back to Rotuma on a regular basis has given us a completely altered perspective. Although previously Alan had been aware of historical changes to the culture, his understanding of those changes was limited to reflecting his 1960 field experience against historical documentation. Our return visits not only afforded a perspective over four decades but also allowed us to witness changes from year to year, greatly enriching our sense of culture as a dynamic process.

1987: Renewing Relationships

Interestingly enough, our 1987 visit also began with a mix-up concerning where we would stay. There were still no hotels or public accommodations on the island, so we had to make arrangements to stay with a family. We had been invited to stay with a woman who had befriended Alan in 1960, Elisapeti Inia, the widow of Wilson Inia, Rotuma's first senator in the post-independence Fiji parliament. Elisapeti's daughter, Betty, informed us that she sent her mother a telegram advising her of our arrival. But it turned out that Elisapeti did not receive the telegram in time and was not at the airport when we arrived. So Josefa Rigamoto's nephew, Tarterani, who had been notified by his uncle of our impending visit, offered to take us to his home in Oinafa, on the northeast side of the island. He and his wife, Fakrau, were gracious hosts and we ended up staying with them for our two weeks on the island.

We spent much of our time wandering around the island seeking to renew old acquaintances. Alan had brought with him eight-by-ten-inch enlargements of black-and-white photographs he had taken in 1960, and showing them evoked warm memories. We had pictures of adults who had been children at the time, and photos of people's parents and grandparents, many of whom were now deceased. It became a game to identify the individuals in the photos and the circumstances in which the pictures were taken. Sharing the pictures proved to be a wonderful icebreaker and stimulant for people to talk about the trajectories of their lives. The photos were treated as treasured gifts by the people to whom we distributed them.

Concordant with the changes that had taken place on the island was an intensified flow of people between Rotuma and Fiji, and a steady growth in the number of Rotumans who had settled in Fiji. Whereas the 1956 census revealed that two-thirds of Rotumans within the colony were resident on Rotuma, by 1986 more than two-thirds were living in Fiji, mostly in Suva and other urban centers on Viti Levu. The population of Rotuma itself had actually declined during that period, from 2,993 to 2,588, despite a high birthrate. (For more on the increased flows of people, as well as material goods and ideas, between Rotuma and Fiji, see Rensel 1993; Howard and Rensel 1994.)

During the brief time we spent in Suva that year (at the home of Ron Crocombe), we had an opportunity to see a number of Rotumans—some of them Alan's old acquaintances, some who had read his book *Learning to Be Rotuman* (Howard 1970) and wanted to meet him, some by chance. These encounters with well-educated, articulate, and accomplished individuals added an important dimension to our experience. For one thing, they brought home to us that Rotuman culture was now firmly embedded in the cosmopolitan world, that one could no longer legitimately think of it as con-

fined to the island. These encounters also presented us with new opportunities for establishing friendships, not simply as a result of experiences shared within a limited time frame, but on the basis of more general understandings of life experiences, historical and political events of worldwide significance, art and literature, music, humor, and so on. What this meant was that the "work" of establishing rapport was considerably reduced, and that "making friends" became an easy, natural, unself-conscious process. Another way to put this is that the cultural barriers that existed in 1960, the gulf that had to be overcome by prodigious effort and constant monitoring, had eased to the point of disappearing, at least with educated and urbanized Rotumans. On the island itself, where traditional rules governing license and restraint still prevailed and many people had limited experience with cosmopolitan urban culture, it was still necessary to maintain vigilance, to monitor one's language and behavior, to remain acutely sensitive to others. However, even on Rotuma one could sense the effects of cosmopolitanism's inroads, making it easier for our hosts to understand us and vice versa.

1988 and 1989: Restarting Fieldwork

Our visits to Rotuma in 1988 and 1989 involved intense fieldwork, paralleling in many respects Alan's research in 1960. We conducted systematic census surveys, in 1988 within the district of Oinafa, in 1989 over the whole island. This allowed us to make comparisons with data from 1960 and to infer a good deal about the processes of change. We also participated in as many events as possible and donated our services in a variety of contexts (e.g., as advisors to a group contemplating business ventures, as photographers at weddings and funerals, as judges at singing competitions, as guest speakers at the high school and teachers' meetings).

Being full-fledged adults and the fact that Alan was now a professor still resulted in our being treated as honored guests, but on a rather different basis than in 1960. On his first field trip Alan had been treated with formal respect despite his youth, primarily because he was a "European," a white person. This kind of behavior was mandated by the colonial government at the time. Since then quite a few Europeans had visited the island, so we were less of a novelty. Also, the colonial regime was gone, and with it the imperative to treat every European with great respect. Indeed, some European visitors had rapidly worn out their welcome and been treated with disdain. The courtesies we were now shown were more a function of the ties we ourselves had established with specific families and an acknowledgement of our commitment to Rotuma and Rotuman culture, manifested by our return visits and Alan's publications.

In preparation for our longer visit in 1989, we arranged to have a separate dwelling refurbished on land belonging to Tarterani's extended family. The building was a concrete-block shell, without windows; we paid for the materials needed to make it livable. Tarterani took pride in his ability as a carpenter and craftsman and did most of the work himself; he expressed delight at the opportunity to rebuild the derelict structure and had finished enough of it by the time we arrived to permit us to occupy it. The semi-independence we enjoyed allowed us to establish a more "normal" routine. We still had to rely on Tarterani for some necessities but now began to engage in reciprocal exchanges with others in the village. Having a gas stove and kerosene refrigerator that we purchased in Suva allowed Jan to prepare cooked foods to share. This made it possible to establish relationships on a basis more like those between other households, who frequently shared labor and resources.

As part of Jan's research on the economics of village exchange, she conducted a thirteen-week survey during which she interviewed a member of each of the seventeen households in the village daily concerning food production, labor expended, and reciprocal exchanges. People seemed to relish the attention and came to anticipate her visits, often providing gifts of food and thanking her for the work she was doing. In addition to giving each family a gift of five dollars per week,[5] Jan reciprocated with banana and pumpkin cakes (we had a prolific pumpkin patch just outside our dwelling and her pumpkin loaves became a village favorite), and pineapple jam and chutney. Her daily visits and increasing familiarity with household members facilitated the cementing of relationships with many people.

The new levels of intimacy we reached with the villagers as a result of Jan's survey did not come without a cost, however. Our increased independence and reciprocal friendships with other households posed a problem for Tarterani and Fakrau. From their point of view we were still members of their household and were expected to order our relationships to mirror theirs. Unfortunately, they had been involved in disputes with a number of families and were unsettled by our friendly relations with them. In addition, Fakrau's father, Kausiriaf, who was the district chief, had become increasingly unpopular in the years prior to our visit. The conflicts he had with families in the village affected Fakrau and Tarterani's relationships as well. Though little was said, we could sense the tension and noticed a distinct cooling of our relationship with the couple (though not with other members of their family, including their sons and Tarterani's elderly father).

Exacerbating our problem was the fact that we were now providing our own food, so we reduced the amount of money we had been giving the family while they had been looking after us. People in the village criticized Tartera-

ni and Fakrau for using the money we had given them for their own purposes (rather than using it for our welfare); this kind of gossipy criticism may have contributed to their alienation from other households and our alienation from Tarterani and Fakrau. In addition, we learned that the money we had been sending them in our absence to make payments on a car had been spent for other purposes, so we arranged to send the car payments directly to the bank. Furthermore, although Tarterani had earlier agreed to help us conduct research in other districts around the island, on our return in 1989 we found that he was too busy renovating his own house, so we had to arrange for several schoolteachers to assist us instead, and we paid them the money that Tarterani might have expected.

Our friendships with other Rotumans were not confined to the village. We made a habit of visiting the schools on a regular basis and spending time chatting with the teachers (all of whom were Rotuman) and got to know many of the people employed at the government station. Since they were all wage earners, this gave Jan a more general perspective on her dissertation topic—the effects of money on social relationships. In addition, we took the time to reinforce friendships Alan had formed in other villages in 1960 through visits and reciprocal exchanges.

By the time we left Rotuma in December 1989 we felt very much at home on the island. We loved our little cottage on the beach, which we had outfitted with a solar panel that ran our lights, a fan, and a newly acquired computer. We had come to cherish the friendships we had formed and looked forward to additional visits in the future, not so much because we were eager to enrich our knowledge of the culture, but because we wanted to nurture the friendships we had come to value so dearly.

1990: The Crisis in Oinafa

When we returned to Rotuma in June 1990 the tensions in Oinafa were palpable. This came as something of a surprise to us, because the year before, despite a history of conflicts and factionalism, a healing process seemed to be taking place in the district, climaxing in a wedding between the offspring of two of the most vocal antagonists in prior disputes (see Howard 1990a). But after that, a conflict had developed between Kausiriaf and members of his kin group that resulted in the kin group's deciding to depose him and select a new chief. Although Kausiriaf refused to accept their decision, the group leaders proceeded with their plans to install a new chief. They recruited from Fiji the brother of one of the key disputants and persuaded him to return to Rotuma to take on the responsibilities of chieftainship. We arrived when the issue was unresolved and feelings were intense on both sides. Kausiriaf was

in Fiji seeking legal advice and the man chosen to replace him had not yet been installed. About 80 percent of the people in the district favored deposing Kausiriaf and were prepared to support his replacement. The main support for the incumbent chief came from members of his immediate family, including his daughter, Fakrau, and her husband, Tarterani.

From the time we arrived it was apparent that we had fallen from grace as far as Tarterani was concerned. Our cottage had not been finished, as he had promised, and many of the materials we had purchased before our departure the previous year to complete the job had disappeared. We discovered later that he had taken some of the items to expand his own house and had donated the paint we had ordered to the church. We took these losses philosophically since the house was still in fact quite livable, but his and Fakrau's coolness to us was disconcerting.

Our fieldwork during this two-month sojourn involved a shift away from systematic data collection toward simple participant observation. We continued to take notes and discussed between us the significance of what we observed and heard, but we made no special effort to collect data on any particular topic. For the most part, we enjoyed visiting with friends, listening to the talk about what was happening in the village, and just being there. Tarterani became increasingly agitated by our visits with people in the village who wanted Kausiriaf to step down. For the first three weeks he avoided talking to us almost entirely, and when he finally did it was to harangue us about the injustice being done by the kin group. He later admonished us for not being loyal to his family and told us explicitly that we were not to speak with anyone who opposed his father-in-law. We tried to explain to him that we were neutral in the dispute and that our work required us to talk with as many people as possible, but he was unmoved.

The last straw for Tarterani was our attendance at the installation ceremony of Poar, the man selected to replace Kausiriaf. We had never before witnessed a chiefly installation and were delighted at the opportunity to document it, but to Tarterani this must have seemed an act of betrayal. Realizing on our way home from the ceremony that Tarterani was going to be incensed, Alan went to visit a Methodist catechist neighbor who had managed to stay neutral in the dispute. While they were discussing our desire to make a formal apology (requiring a roasted pig, ceremonially presented) to Tarterani and Fakrau for any distress we had caused them during our stay, Jan burst in, crying, and said that Tarterani had told her he wanted us out of the house immediately. She had pleaded with him, explaining that we were scheduled to leave in a week anyway, but he was adamant. Alan talked to him, but to no avail. Although it was already late afternoon, Tarterani still demanded that we be gone by nightfall.

What happened next astonished us. In a matter of minutes, as word of our plight spread around the village, people began to show up to console us, to propose alternate accommodations, and to offer assistance in moving. Before long two trucks pulled up at our front door with almost all the men from the village. They moved our furniture, appliances, and personal possessions onto the trucks while Tarterani looked on from his verandah. The men then proceeded to strip the solar power wiring from the house, and offered to pull out the louvered windows and iron roofing since we had paid for them too, but we requested that they not do so. The men expressed considerable anger at the way we were being treated, feeling it reflected badly on the whole village. One of the subchiefs later apologized for not coming to help: he told us he was so angry that if he had seen Tarterani he would not have been able to control himself.

Several people offered to buy things from us, knowing we now had no place of our own to put them. We offered the appliances, including our solar setup and a generator, at nominal prices (in order to "legitimate" the transfer, should it be questioned) to some of our friends, and we gave away the furniture (most of which Tarterani had built out of materials we had purchased). We spent the night at the home of an outspoken critic of Kausiriaf, a man who was instrumental in trying to depose him. The following day, at our request, we were taken to the district of Itu'muta, where we had been invited (prior to these events) to spend time with the family of Harieta and John Bennett (a Rotuman woman and her American husband from Rhode Island). It was a comfortable arrangement since John had built a small apartment with modern conveniences and a delightful verandah overlooking Maka Bay. Many of the Oinafa villagers accompanied us on the truck. For Alan, in a way this was returning home, since he had lived in Itu'muta during his initial field trip.

In the course of fieldwork, anthropologists sometimes experience transformative events, events that significantly shift the way the people they are studying perceive them, the way they perceive their subjects, or both. An event might be as simple as eating food that people think of as uniquely theirs and repugnant to outsiders. It might involve a shared experience of danger, or be the consequence of performing tasks (like planting crops or weaving mats) that define a people's personhood. Sometimes these transformations are incremental, describing a gradual trajectory from being seen as a stranger to being accepted as a "normal" human being. Sometimes events are dramatic, involving a shift from outsider to insider, from someone who is "other" to someone who is "us." It was the latter kind of transformation that we experienced following this episode. A number of people explicitly said to us, "Now you know what it's really like to be a Rotuman," implying that

the facade of polite respect had been penetrated and they could no longer present themselves in a purely positive light. People began to tell us their own stories of fractured relationships within their families, spontaneously revealing intimate details they would never have exposed before. In addition, our stance of neutrality in the chiefly dispute had been shattered. We were now identified with one of the factions, in a strangely heroic-martyred way—and thus became insiders to that faction (fortunately a much larger one) and outcasts to the other. It was as if the frame for our relationships changed from one of ethnicity and nationality to one of political allegiances within Rotuma.

We left Rotuma several days later with mixed emotions. Despite the hurt feelings and a sense of having failed in our roles as neutral anthropologists, we relished the new levels of intimacy that resulted. The somewhat idyllic perspective we had of Rotuma had been shaken, but at the same time we felt more emotionally attached to it. Our experiences had awakened intensified emotions that were previously muted, perhaps because we had been trying to get along with everyone equally well. Following the episode with Tarterani and Fakrau, and the growing intimacy that developed with others during our return visits, we were experiencing emotions much closer to those we experience with our own relatives—complex mixtures of love, joy, irritation, frustration, and so on. And as our relationships grew more complex, we felt our intellectualized understandings of Rotumans (as a category) give way to an affective interplay with specific individuals that depended on circumstances, events, gossip, and other contingencies. This ultimately led us to abandon our analytical pursuit of the kind of systemic understanding that lies at the heart of scientific analysis. Instead, we began a highly contextualized, historically informed quest to comprehend events. The episode also motivated us to become closer to individuals we had come to like or love, without regard for what we might learn about Rotuman culture.

1991: Expanding Horizons

In 1991 we spent the (northern) summer in New Zealand and Australia, with brief revisits to Fiji and Rotuma. When we got to Rotuma we found the split in Oinafa still unresolved. Kausiriaf had won the legal battle; the high court in Fiji decided that under Fiji law only the prime minister could depose a Rotuman chief. Most of the people nonetheless supported Poar and refused to participate in activities presided over by Kausiriaf. Tensions remained high, with people from one faction shunning those on the other side (for a detailed account of the dispute, see Howard and Rensel 1997).

Finding the social atmosphere in Oinafa oppressive, we accepted the invitation of Elisapeti Inia to host us at her home in the village of Savlei, in Itu'ti'u district, on the southwest side of the island. We were invited to stay with other friends and accepted some of their invitations, including an overnight stay with Poar in Oinafa. This further cemented our identification with his faction, and resulted in an exceptionally warm reception by his supporters. We repeated this pattern on subsequent visits to Rotuma. On each occasion we were hosted by Elisapeti and made overnight visits to friends who invited us.

Staying with Elisapeti was especially rewarding for us. Not only had she become our teacher and a dear friend, but Alan had also begun work on a biography of her late husband, Wilson (Howard 1994), and this gave him time to peruse Wilson's papers and to interview Elisapeti at length. Doing a biography seemed a natural outgrowth of the process that had engulfed us. It was an opportunity to focus on the life of a man Alan greatly admired, quite apart from the fact that he was a Rotuman. In a sense the project mirrored the shift in our orientation toward fieldwork: culture was shunted to the background with individuals foregrounded. We were now more interested in coming to grips with how people managed their lives than with the culture that may have patterned their behavior.

After reaffirming relationships with Rotuman friends in Fiji, we flew to New Zealand to enjoy a pleasant holiday, including a week's excursion in a camper van. Although we did not think of this as a field trip, we took the opportunity to visit with the Reverend Jione Langi in Wellington. Langi was a Rotuman who held the position of pastor to Fijian, Fiji-Indian, and Rotuman Methodists in New Zealand. He also had been a student of Wilson Inia's and credited him with having influenced his career path. This presented a double opportunity for us: Alan was able to interview him about his relationship with Wilson, and we were able to find out something about the Rotumans who had immigrated to New Zealand. We learned that there were perhaps a hundred Rotuman or part-Rotuman families in New Zealand and vowed to find out more about them in the future (we were also enchanted with New Zealand and thought this a great excuse to return).

From New Zealand we went to Australia, where we spent most of our time in Sydney at the Mitchell Library scouring their archives for Rotuman historical materials. While in Sydney we encountered several Rotumans, including Saumara Foster, a librarian at the Mitchell who provided considerable assistance. We also received an invitation from a Rotuman woman, Martoa Dickinson, to spend a day at her home. She invited a number of Sydney Rotumans to meet us, providing our first opportunity to get to know Rotumans who had migrated to Australia. We thoroughly enjoyed the experience and decided to return to do fieldwork there in the near future.

1994: Fieldwork in Cities Down Under

The near future turned out to be 1994, when Alan took another sabbatical leave. We spent three months visiting Rotuman communities in Fiji, Australia, and New Zealand, including two weeks on Rotuma. Our time in Suva was momentous since Alan's biography of Wilson Inia was being launched by the Institute of Pacific Studies at the University of the South Pacific. The launching was attended by a cross-section of the Rotuman community, and we were gratified with the book's reception. Whether it was the book or simply the fact that we had come back once again, the warmth of our welcome had noticeably increased. To be sure we had already formed some enduring friendships that transcended time and distance, but Rotumans who had been mere acquaintances before made a point now of seeking our company.

Our reception on Rotuma was also exhilarating. We brought copies of the book with us and they quickly circulated around the island. Elisapeti held a traditional welcoming ceremony in our honor and, as in Suva, we sensed a new level of acceptance within the community at large. Wilson Inia had been a leader of considerable moral authority on Rotuma and our association with him—indirect as it was—gave us enhanced legitimacy. The book was written for Rotumans rather than for academics and documented aspects of their lives they could relate to easily. One consequence of writing for an academic audience is that unfamiliar jargon creates distance between the anthropologist and the people studied. The Inia biography had the opposite effect.

From Rotuma we returned to Fiji for a brief period before going on to Australia via New Zealand. As we were waiting in the transit lounge of Auckland airport we were startled to see the Reverend Langi—we had heard that he was in Fiji at a Methodist conference. He informed us that Martoa Dickinson, our host in Sydney, had invited him to participate in a cultural night she was arranging as well as a joint service of the two Rotuman Methodist congregations. This was the first inkling we had that something major was being planned in our honor. The cultural night was a gala affair held at a posh lawn-bowling club. Approximately 150 Rotumans attended, some from as far away as Melbourne. In addition to providing a feast, members of the Sydney Rotuman community danced a traditional *tautoga* (group dance) and a Tongan band, hired for the occasion, played various kinds of Polynesian music. Alan was eulogized prior to giving a speech concerning his research experience on Rotuma and the importance of maintaining Rotuman identity in migrant communities abroad. Needless to say, this was a wonderful way to be introduced to Rotumans in Sydney, and we spent the next four weeks familiarizing ourselves with the community. We were greatly impressed with the economic and social successes Rotumans enjoyed in Australia. Most people

we met held white-collar, managerial, or professional positions and had comfortably assimilated into the middle and upper-middle classes of Australian society. Although we took notes and kept a diary recording our observations and reflections, it felt less like fieldwork than an extended vacation, visiting people who quickly became extremely dear to us.

From Sydney we drove to Melbourne, where we were hosted by Kapieni Patresio and Torika Sanerive (Torika had attended the cultural night in Sydney), and introduced to the small Rotuman community there, which at the time was struggling to organize. We then went on to Adelaide and spent a few days with Injimo "Oni" Hanfakaga and his Australian wife, Betty. Only a handful of Rotumans lived in Adelaide, giving us a sense of the isolation experienced by migrants who lived apart.

We left Australia for New Zealand, where we were hosted in Auckland by the Reverend Langi and his family for several more weeks. This was a most fortunate happenstance since Langi was at the heart of the Rotuman community in New Zealand. He had formally organized the "New Zealand Rotuman Fellowship," which held semiannual meetings, and he conducted Sunday services in Rotuman for the Auckland community. A constant stream of Rotumans visited his home and he spent a good deal of time on the telephone talking with others. He made every effort to involve us in community activities, going out of his way to introduce us to people he thought would be of interest. We were also fortunate enough to attend one of the fellowship's semiannual meetings, held on a Maori *marae* over a long weekend. It was a delightful experience of communitas. Langi gave us rather prominent roles, including Alan's giving a speech ("Twenty reasons Rotumans are so successful") and Jan's giving the children's sermon ("Three key Rotuman values") during the church service. What perhaps made everything even more rewarding was the fact that by now we knew many of the relatives of the people we were meeting—relatives we had met in Rotuma, Fiji, and Australia. We also had a pretty good command of the news and gossip circulating throughout the broader community and felt more like "insiders" than ever, more so than many of the migrants who were only marginally involved with other Rotumans. Our familiarity with Rotuman history (including recent events) sometimes led people to exaggerate the extent of our cultural knowledge, which on occasion proved embarrassing.

We also visited the small Rotuman community in Christchurch, New Zealand. There, too, we were feted, although in a more informal manner. We were hosted by Sanimeli Gibson, who later married Maraf, chief of Noa'tau district, and moved to Rotuma. Our reputation as people who were knowledgeable about Rotuman genealogies was put to the test there by Munue Tivao and Akanisi Nafrue, a Mormon couple eager to identify their ances-

tors. Fortunately we had with us a laptop computer with a database of births, deaths, and marriages between the years 1903 and 1960—incorporating the information Alan had copied from registers during his initial fieldwork in Fiji and Rotuma, which he had since collated. This made it possible to go back as many as five or six generations for young adults, and sometimes even further if known ancestors articulated with genealogies collected by A. M. Hocart in 1913. The incident with Munue and Akanisi stands out because it was so meaningful to them, as it allowed them to identify ancestors whose souls they could pray for. We had provided this kind of information for interested parties in Fiji and Australia. Some people had offered to pay us for the service—which we refused, of course, happy to have the opportunity to reciprocate the many kindnesses being shown us. We continue to receive periodic requests for such information and more often than not we are able to provide it. It is especially gratifying that data originally collected for anthropological purposes are so highly valued by Rotumans themselves and we are delighted to be able to make the information available to them.

The trips to Australia and New Zealand brought home to us the degree to which the Rotuman community has become global in scope. We became acutely aware of the continuous flows of people, goods, and information that take place between localities with Rotuman populations, and of the futility of drawing boundaries around communities (for more on the transnational Rotuman community, see Howard and Rensel 2001).

One of the by-products of our visits Down Under was a list of names and addresses of Rotumans who were living in Hawai'i, relatives of people we had met in our travels. After returning home we sent out invitations to everyone on that list to a gathering at our place, asking them to inform other Rotumans they knew, and suggesting they bring their favorite Rotuman dish to share. The party was a great success; it brought together people unaware of one another's proximity and stimulated a desire to get together on a regular basis. The result was the formation in 1995 of the "Rotuman Association of Hawai'i," the Tefui Club. (A *tefui* is the distinctive Rotuman garland.) In addition to monthly meetings the club held weekend campouts, celebrated "Rotuma Day" (May 13, the day marking Rotuma's cession to Great Britain in 1881), organized fund-raisers for various purposes, and performed Rotuman dances on several occasions. For us, this extended "the field" into our own backyard, so to speak, and offered the same sorts of pleasures (and disappointments) as fieldwork abroad.

The high point of this newly formed community for us was Alan's retirement party, in September 1999. The event was hosted by the Tefui Club and attended by friends and colleagues from the University of Hawai'i and the broader community. Rotuman friends came from Rotuma, Fiji, Austra-

lia, New Zealand, and Alaska for the party and participated with Tefui Club members in preparing the feast, making *tefui* and other decorations, and entertaining the guests. The club members performed a traditional ceremony honoring Alan and danced a traditional *tautoga* in his honor.

In addition to our subsequent visits to Rotuma and Rotuman communities in Fiji, Australia, and New Zealand (mentioned above), we have also attended Rotuma Day celebrations in the San Francisco Bay area in 1999, 2001, and 2003, and in British Columbia in 2003. We have also been privileged to accept invitations to visit with Rotuman families in Europe—England, Scotland, and Norway—whose close-at-hand Rotuman networks are smaller but who are nonetheless active participants in the global Rotuman community.

Serving the Community

Each visit helps to confirm and consolidate the friendships we have formed. As a result of repeated visits to "the field"—wherever it is—we have experienced a shift in our priorities and allegiances. Whereas we began with a firm commitment to the anthropological community—a commitment that entailed spending most of our energies writing for other academics, and communicating and socializing with them—we have ended up with a strong allegiance to the global Rotuman community and a desire to serve that community in multiple ways. Two of the most central forms of service have been creating and maintaining a Web site dedicated to the Rotuman community and helping knowledgeable Rotumans write and publish their own books.

Toward the end of 1996, enchanted by the possibilities of the Internet for anthropological and, particularly, ethnographic purposes, Alan began construction of a Web site for Rotumans. Our goals were modest at the time. We wanted to create an accessible place in cyberspace where Rotumans could share news and communicate with one another. The site also aimed to provide basic information about the island's history, culture, and language for interested Rotumans and non-Rotumans alike. Toward these ends we created Web pages containing news, maps, information about recent publications, and a set of essays on population, history, economics, politics, myths, and other cultural topics. We also scanned photographs from Rotuma and created a digital photo album. The positive feedback we received from Rotumans encouraged us to invest more of our energies in the Web site and to include new features. We have since added a bulletin board where people can post messages, pages of Rotuman recipes and Rotuman humor, a forum where individuals can post their views on issues of concern to the community, sound clips of Rotuman music, and information on contemporary Rotuman artists.

Concerns over preserving the language led us to incorporate an interactive Rotuman-English dictionary that allows visitors to the Web site to search for glosses from one language to the other. This is an abbreviated version of the *New Rotuman Dictionary* on which we collaborated with German linguist Hans Schmidt, Elisapeti Inia, and Sofie Arntsen, another Rotuman woman with considerable linguistic gifts (Inia et al. 1998). The expressed desire of people to keep in contact with one another across national boundaries resulted in our developing a Rotuman Register, a database where people can fill out a form with personal information allowing searches by various criteria (e.g., name, parents' names, home village on Rotuma, current residence).

We also created an archive of significant nineteenth- and early-twentieth-century writings about Rotuma. This, along with the posting of our own published articles and book chapters about Rotuma, is a way of repatriating materials that languish in obscure volumes and journals, making them unavailable to most Rotumans (see Howard 2002 for more on the issue of repatriation).

The Web site has facilitated fund-raising on a worldwide basis for various causes, including obtaining needed equipment and supplies for the hospital on Rotuma, and funds to support a program of environmental education (LäjeRotuma) organized by a group of well-educated young Rotumans in Suva.

The feedback we have gotten from Rotumans who visit the Web site has been enormously gratifying. We get a constant stream of e-mail messages from grateful Rotumans who have located long-lost friends or relatives, or who simply express delight over being able to keep abreast of happenings in the various communities. The Web site now averages over two hundred visits per day, and many people have told us that they visit daily, especially to get the news (we now have regular correspondents who send reports and photos from their respective communities). The URL for the site is http://www.rotuma.net.

We have also devoted ourselves to assisting Rotumans to publish their own books. In particular we have worked with Elisapeti Inia, who has a vast knowledge of Rotuman culture and a passion for preserving it. We have brought her from Rotuma to our home in Honolulu on several occasions, providing her with an opportunity to focus her energies on recording her knowledge. This has resulted in two books, one a compilation of Rotuman proverbs (Inia 1998), the other a detailed how-to-do-it description of Rotuman ceremonies (Inia 2001). We played the roles of scribes, editors, and typesetters, while constantly asking questions that required her to clarify what she was writing about. Though we make no claims to coauthorship of these volumes, they are among our most gratifying accomplishments.

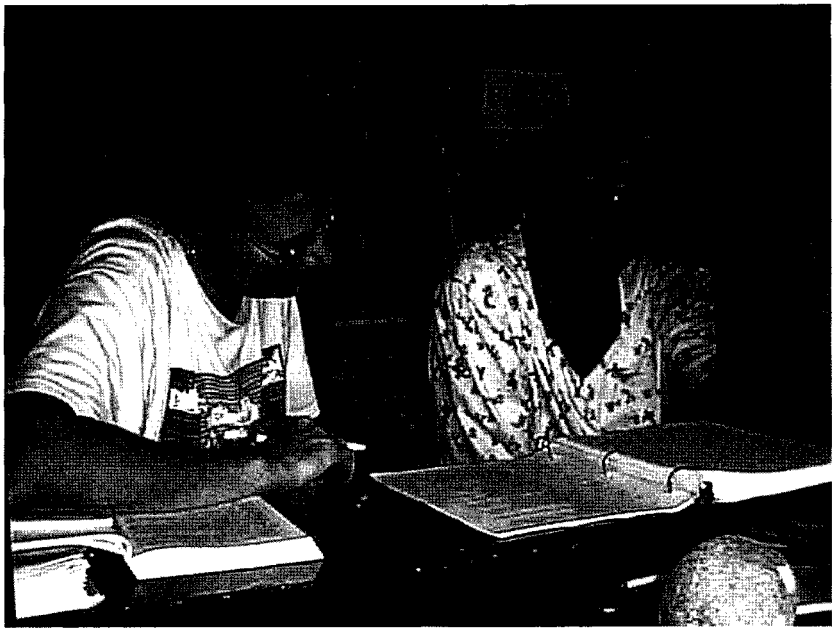

FIGURE 2. **Jan Rensel helping Elisapeti Inia to edit the manuscript of her book, *Kato'aga: Rotuman Ceremonies*, in July 2001.**

In the Final Analysis

Our experience with long-term fieldwork among Rotumans has resulted in several transformations. It has progressively changed our research orientation from science to history, has altered our relationship to Rotumans with whom we interact from one governed primarily by respect and restraint to one characterized by intimate friendship, and has resulted in a shift in commitment from the academic community to serving the now-global Rotuman community.

Although we feel we have gained a richer sense of individuals' histories, we can't claim to have a deeper intellectual understanding of Rotuman culture as a result of our repeated visits. In a sense, we feel we understand it less, in part because our analytical selves have receded into the background while emotional aspects of our relationships with people have become central. The complexity of our relationships has increased many times over, and just as we sometimes have difficulties understanding members of our own families because we know too much about them, we sometimes have trouble understanding friends with whom we have developed profound relationships. Ultimately, articulating understandings requires simplification, and

our immersion in the relationships we have formed with Rotumans is too rewarding to employ the kind of analysis required. When we go to the field nowadays it is only secondarily to increase our understanding of Rotuman culture; our primary reason for returning is simply to be with people who are among our closest, most intimate friends. If our work provides a service to the community, we are doubly rewarded.

NOTES

1. Although Rotuma is nearly three hundred miles north of the Fiji archipelago and is distinctive culturally, linguistically, and in the physical characteristics of its people, the British decided to incorporate Rotuma into the Colony of Fiji when Rotuma was ceded to Great Britain in 1881. After Fiji gained independence in 1970, Rotuma remained a part of the nation of Fiji. However, Rotumans and Fijians alike continue to make a distinction; for example, the Methodist Church is officially "The Methodist Church of Fiji and Rotuma." In this article, whenever a distinction is made between Rotuma and Fiji, we follow local custom in referring to Fiji as an archipelago rather than as a nation.

2. The Rotuma Land Act of 1959 is just now being revised, and a new land commission is expected to go to Rotuma sometime in the near future; it will be interesting to see how things play out this time.

3. An indicator of Amai's abilities and his hardworking commitment to duty was his being awarded an MBE by Queen Elizabeth in 1998 for his long service aboard cable ships.

4. I eventually coauthored a paper on the topic titled "Cultural Values and Attitudes Towards Death" (Howard and Scott 1965).

5. Jan made it clear that the money was a gift and not payment for services, which would have entirely altered the nature of her relationship with them. In fact several people initially refused to accept the money on the grounds that what she was doing was of value to the community and that it was she who should be compensated. Only after Jan convinced them that the cash was a gift to show her appreciation for their assistance did they accept it.

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LIVING A "CONVENIENT FICTION"

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Long-term fieldwork offers a unique perspective on the complex, interactive process of interpretation on which ethnography depends. Through shared experiences, the ethnographer(s) and host community collaborate in constructing reciprocal identities tailored to their respective needs and the local situation. The mutual expectations and assumptions grounding this relationship become more visible as they are defined by subsequent events and encounters over the years. Our article analyzes some significant twists and turns in our thirty-year relationship with the Tuvaluan atoll community of Nanumea. In initially defining us as "of the island," the community established us in a local category resonant with key emic values (community solidarity, equality). As time passed, this fieldwork identity was reinforced, and constrained, by local interactions and decisions. Similarly, research products and opportunities were informed by the expectations the community held regarding our identity. Using a long-term reflexive lens, this case study reveals how complex and interconnected is the process of creating an ethnographic relationship.

THE CREATION OF ETHNOGRAPHIC INFORMATION is a highly complex process, as the last thirty years of anthropological analysis and debate have demonstrated. Recognition that ethnographic writings themselves are interpretations (Geertz 1973), though "fashioned" and "constructed" to be as authentic as the ethnographer can manage, has focused attention on the process of fieldwork immersion and necessitated a more subtle and reflexive description and delineation of key events, historical relationships, and social interactions. Attention has been directed to the internalized "interfering intermediaries" that inevitably "maintain outposts in [the ethnographer's]

Pacific Studies, Vol. 27, Nos. 3/4—Sept./Dec. 2004

mind" (Jackson 1990:32). Theoretical allegiances, personal beliefs, cultural assumptions, and prior ethnographic understandings all constrain (and inspire) the ongoing recording of ethnographic information and its analytic construction. Equally important, fieldwork is a relational process, with the host community determining aspects of ethnographic outcome and increasingly able to influence the interpretive process with feedback about written materials (Jacobs-Huey 2002). As the product of so many internal and external influences, ethnographic writing and fieldwork (like human behavior generally) are now understood to be "overdetermined, . . . reflecting multiple meanings simultaneously" (Johnson and Johnson 1990:163).

This complexity poses an analytical challenge for anthropologists interested in understanding more completely how ethnographic processes steeped in ambiguity (creative but still representative, serendipitous but realistic) produce convincing and representative portraits of the human condition (Sanjek 1990a:395–404). Disciplinary responses to this challenge have taken a variety of paths, including the development of a genre of reflexive fieldwork accounts describing personal and relational aspects of an ethnographer's experience (just a few examples drawn from three decades include Briggs 1970; Rabinow 1977; Rosaldo 1989; Behar 1993; Flinn, Marshall, and Armstrong 1998). In addition, ethnographic interpretation itself has been problematized, dissected, and scrutinized from conceptual, methodological, and ethical perspectives (cf. D'Andrade 1995; Marcus and Fischer 1986; Scheper-Hughes 1995). Analysis of strategies used to claim representational authority (Crapanzano 1986) complement sometimes heated debate about ultimate anthropological goals, science, advocacy, and ownership of ethnographic information (Jaarsma 2002). Assessments of many specific aspects of fieldwork practice, ranging from note taking (Sanjek 1990b) to long-term fieldwork (Foster et al. 1979b; Kemper and Royce 2002), also reveal the assumptions and broader patterns in anthropology's research engagement. Taken together, this considerable body of reflexive and critical analysis has raised disciplinary consciousness about the complexity of ethnographic research. This consciousness sets the stage for our discussion here of one particular component of fieldwork experience, namely, the development and maintenance of fieldwork identity in long-term fieldwork.

While creation of research identity is inevitable in any ethnographic work, analysis of a long-term research situation seems especially conducive to revealing the creative interplay between ethnographer and host community and the effects of a "fabricated" identity on the ethnographic products that result.[1] As Bond notes (drawing on Foster et al. 1979a:330–331 and his own experience), long-term research has predictable effects on fieldworkers' research interests, skills, and social standing:

> The researchers become known quantities in the field and are treated as social persons. They have a place in the past, and their work is understood. They may negotiate their own past; they are of the community and yet beyond it. Their standing within society has changed and so also has their vantage point. There is much assumed common ground, shared knowledge and experience, a situation that does not obtain for beginners in the field. (Bond 1990:281)

Reciprocating interaction over decades results in a highly complex relationship in which potential outcomes may be just as important as actual ones and in which the expectations of researcher and hosts are seldom ever fully articulated.

Both community and ethnographer benefit from having time together: time to create revised understandings of each other's assumptions, goals, and categories. A time frame that can accommodate adaptive changes is crucial to developing a fieldwork identity that is relevant and effective from both parties' points of view. Such a "successful" ethnographic identity must somehow bridge the cultural divide between insiders and outsiders, not contorting too unbearably the reality accepted by either party but facilitating a safe, productive interaction. Because identity-construction processes are as "fabricated" and "overdetermined" as any other ethnographic product, they are seldom fully subject to any one party's conscious control, perhaps especially at first, even though all involved may have goals in this regard. As the relationship unfolds, takes unexpected turns, and transforms in response to implicit cultural categories, diverse hopes and expectations, and strokes of serendipity, conscious decisions or realizations may be made periodically by any or all parties. Much that ultimately proves to be determining may not be consciously marked, of course.

The resulting identity affects, and is affected by, the fieldwork process and its products. Thus, reflexive analysis of factors involved in the construction of fieldwork identity can offer a useful lens for examining the complexity of ethnographic methodology, both generally and in specific historical, social, and political contexts. Of course, as Salzman has noted (2002), "sincere" self-analysis does have inherent limitations that require corrective assessment via disciplinary debate. Incorporation of reflective analysis and feedback from the relevant host community is equally essential.[2]

The following case study, based on our thirty-year-long research relationship with the Tuvaluan community of Nanumea,[3] shows the important role that local categories can play in setting the initial terms for ethnographic engagement. Once an appropriate local category was found and accepted by ourselves and community members, both parties were drawn to use it

as a logical basis for subsequent behaviors and decisions. This grounding in local institutions affected the overall tenor of our fieldwork relationships by subtly rebalancing the power disparity, inevitable given the colonial context framing our early fieldwork. Increasingly, as our involvement with the Nanumea community lengthened to span decades and was accorded "a place in the past," this established identity category became the reference point for further ethnographic engagement as certain key local events offered research opportunities but also confirmed expectations about "who we were supposed to be." Here we describe some of the more informative twists and turns in our "ethnographer's path" (Sanjek 1990a:398–400), focusing on the process through which we came to be associated with a locally relevant category and how this fieldwork identity became confirmed and workable over time.

One personal note is in order. In scripting this account, our analysis takes a unitary perspective. Idiosyncratic particulars have been set aside in favor of shared features, which we believe to be more significant. Even though we are two individuals, and periodically have pursued separate research agendas, our relationship with Nanumea began as a couple and has continued as such into the present. We sense that the community has largely connected us together to form a single social "person," collapsing our identities in a way that effectively complements the local categorization discussed below.[4] Having official married status (and documenting it to the community) did form a legitimizing precondition for our initial acceptance, in fact. In the first month after our arrival, many of the women who visited our house voiced covert concern that we might be just living together, a moral lapse then associated with young expatriates, especially the vague category of "hippies" that no one could clearly describe. Visitors usually noted our lack of wedding rings and listened skeptically to our description of alternative wedding symbolism. Some then asked to see a wedding picture (which we didn't have). Once we realized that serious moral concern lay behind these visits, we began showing visitors a copy of our marriage license. The pastor's wife was the last visitor to be shown this document, having specifically asked to see it. Legitimating our status as an officially married couple in this way may have accentuated our conjoined identity in community eyes. In any case, the fieldwork identity we describe here was intended, by both sides we think, to embrace us equally.

"Of the Island"

Our story begins in June 1973. We had come to Nanumea, one atoll in the Gilbert and Ellice Islands Colony, as part of a research project funded by the British government to provide social, economic, and political data on which planning for the islands' imminent independence could be based. The topics

to be documented were vast, our local-language abilities nonexistent, but we had enthusiastic optimism in abundance. Everyone, administrators and local people alike, must have thought us quite young. As *paalagi* (foreigners, white people), in that British colonial era, our behavior must have seemed unusual too. We sought out local company nearly exclusively, spoke English with an unusual accent (American), dressed far more casually than most other visitors, and worked intensively to learn the language. Perhaps most distinctive—since we felt it imperative to create some measure of "informed consent" within the community for our research to balance its government sponsorship and applied orientation—was that we tried hard to explain our goals to the people we understood to be the community's designated leaders, literally from the day we arrived.

Nonetheless, the identity we initially presented to the community must have been puzzling, even contradictory, to local assumptions and categories. We clearly had government sponsorship and connections because the colony's Agriculture Department, based in the capital, Tarawa, had sent official notification to the island of our impending arrival. Our activities and demeanor, however, bore little resemblance to colonial-government officials who periodically toured outer-island communities in that era. At first this incongruity largely escaped our attention. We were so preoccupied with creating a good impression, adapting to a new lifestyle, learning Tuvaluan, making as few serious mistakes as possible, and justifying ourselves to ourselves as diligent fieldworkers that we were initially oblivious to any possibly dissonant image we might be creating. We did have a disquieting sense, though, that our project seemed barely comprehensible to local leaders despite our efforts to explain it to them.

We also knew that our ultimate goals were sharply different from those of colonial government officials, even those who were knowledgeable about local affairs and tried to further local interests. We needed to carve out a unique, anthropological niche for our relationship with the community, but there seemed little available beyond the broad idea that our work could be used to teach distant others about Nanumean life, sometime in the future. Thus we used small opportunities that presented themselves (such as going barefoot, as virtually all Nanumeans did) to distance ourselves from the two common *palagi* categories of that time—colonial and missionary. Unknown to us, however, the community also held concerns about our identity, based on categories and assumptions that were well beyond our imagination at the time. Our first inkling of these local concerns began after we had been on the island about a week. As we finished coffee one morning, a letter that we had promised to deliver fell out of the book into which it had been carefully tucked, setting in motion a train of helpful conversations, meetings, and

decisions. Within the next hour, the letter led us across the village to a large extended-family household headed by a white-haired man named Samuelu Kolo. Luckily Sunema, our research assistant, had decided to come along to show us the way there. She was a distant relative of Samuelu's, and happily stayed with us to visit and translate.

A deacon in the local church, Samuelu was one of the most respected elders on the island. He was descended from one of two chiefly lineages that had traditionally alternated in ruling the community, and he garnered respectful attention when he spoke at village meetings even though the chiefs no longer played an official role in island government. We knew nothing of this, of course, as we approached Samuelu's house that day. But his warm welcome was encouraging and we were glad when he insisted that we sit down with him inside. Within minutes, however, Samuelu began to question our assistant intently. Slowly, through Sunema's intermittent translations, we began to piece together what he was saying. People were very curious about why we had come, and many were also a little concerned about our treatment. Traditionally, Samuelu explained, the elders would have decided whether to allow our research and then would have organized a suitable reception. The Island Council, instituted by the colony government only a few years before, now ran island affairs and therefore had these responsibilities, but had not acted publicly in regard to our arrival. Samuelu expressed concern that the council's arrangements for us might not reflect well on the community.

As he talked about his concerns, we gradually realized that Samuelu was giving us explicit advice. We should ask the council president to invite the "old men" to a special meeting. If the council refused to allow this, he warned gravely, then we ourselves should call a separate meeting of the village elders. Persuasively, Samuelu insisted that we needed to explain our plans to the whole community as soon as possible since our work was "of the island," not just a concern of the government. Therefore, it involved the community as a whole, rather than simply the Island Council.[5]

Hearing this, we worried whether a struggle for authority was separating council members from the community elders. If so, might we inadvertently have become caught in the middle of a local power struggle? Inwardly concerned, we thanked Samuelu and hesitantly promised to ask the council president to convene a special islandwide meeting, specifically including the elders. If Samuelu was any example, the "old men" of Nanumea clearly were a power to be reckoned with and we hoped that we had not offended them by dealing only with the Island Council thus far.[6] The phrases Samuelu had used, "of the island" and "of the government," stayed in our minds and raised a host of questions. This dichotomy apparently separated the sphere of colony

government, even its local embodiments on Nanumea itself, from that of the local community. Were the old men still making the island's important decisions despite the formal authority of the elected council? Why had our previous conversations with British administrators never raised this issue? Somehow, we had taken the council's existence as confirmation of its actual political authority. Why had we been led so easily to this assumption? We couldn't help but wonder as well about other pragmatic issues. What would it mean to our research to be classified as "of the government"? Should we (or *could* we) try to influence our classification one way or the other? Being "of the island" sounded better to us, but what limitations might that involve? Sunema offered no opinion on these questions. Deciding whether to follow Samuelu's advice, in whole or in part, was clearly going to be up to us despite our relative ignorance about local affairs and power relations.

Fortunately, Samuelu's advice was one of the serendipitous blessings of our fieldwork. It provided clues about key local categories and prompted a definitive Nanumean reaction to our proposed research, though the dichotomy revealed to us still offered only possible identities among which the community (and perhaps vaguely ourselves) would have to choose. The council president proved agreeable to the community meeting we requested and one morning just a few days after our discussion with Samuelu, we walked down the shaded sandy road toward the village meetinghouse, accompanied by Councilor Monise and Sunema. About thirty-five older men (plus the lone woman councilor) watched us approach, waiting for our explanation of why we had come and what work we hoped to accomplish. Mats had been spread for us facing the assembled community leaders and we took our places. Keith spoke at this official gathering, slowly laying out our plans as Sunema translated, sentence by sentence. This was the same overview we had already given to Samuelu and to several Island Council members. How would the community as a whole respond to it?

The assembled group listened impassively and silently as Keith spoke. We found it impossible to guess what their thoughts were, though afterwards there were many questions. Some showed an insightful grasp of our research plans and even made suggestions about beneficial uses our research might eventually have. We felt relieved by these. Other questions utterly surprised us. These are the ones that we still mull over, wondering about the motivations and worldviews that inspired them. How did we want the community to help us with our work? Could we teach better ways to grow *pulaka,* the main root crop? Were we going to study old customs too, or just those of the present day? Had we no children? One old man said ruefully that he had waited all his life for the British government to send someone to help the community, but now he was old and blind and could not see them! When the questions

stopped, we thanked the elders and left the hall, leaving them to decide our fate in private, as Samuelu had earlier advised.

Discussing this as we walked home with Sunema, we all agreed that the meeting had seemed to go well. What we found out later confirmed this initial feeling. The community had approved our research and people were willing to help us insofar as they could. Furthermore, we were to be considered guests "of the island." Because we were *fakaalofa*, "pitiable," without connection to any particular family and thus without rights to use island land, the community as a whole would support us for the next few months.[7] Each Tuesday and Saturday, one family from each island "side" would bring us coconuts and some raw starchy food that we could cook for ourselves.[8] An official welcome feast would now be organized, to be held the following week.

What seemed likely to be a suitable, albeit still somewhat ambiguous, relationship to the community had been confirmed for us. Acknowledged to be "pitiable" because we lacked local kin and membership in an extended-family landholding group, we had been accepted as "of the island." Government sponsorship had been explicitly set aside in favor of a local affiliation, one that confirmed us as "insiders" rather than "outsiders." No one felt it necessary to explain the implications of this category more fully, and we did not pursue any clarification ourselves either. We simply assumed that we would be some sort of marginal insiders at best, and took our "of the island" categorization as a positive fieldwork beginning. Certainly we did not fully appreciate the significance of being connected to the community as a whole, rather than to any particular group or family within it.

In retrospect, the application of this category to us seems somewhat unusual. In traditional legend and the historical era alike, overseas visitors who stayed long enough to develop enduring relationships with the community typically were assimilated through marriage. Marrying-in provided outsiders with a place to live, access to resources, and a network of personal relationships; and there seemed usually to have been no scarcity of spouses. This incorporation strategy was not possible in our case, of course. Adoption was often used as well, either initially or after a person had become established through marriage.

Pastors seemed the only consistent exception to this personalized model for assimilating strangers. With these respected men and their families, the community did structure its relationship through communal support (though on a much more elaborate scale of provisioning than our own) since a pastor's spiritual leadership was viewed as an asset to the community as a whole. Clearly, the community's decision to feed us collectively was an honor, implying concern for our material well-being as well as support for

our research. But once accepted as people "of the island," we would need to fulfill the reciprocal obligations expected of community members. We had little idea what these might be, or whether they would prove onerous or inappropriate. We did feel pleased, however, to have been designated as putative "insiders," especially since the alternative seemed to undermine our ethnographic goals. Our intended fieldwork focus on the community's social, economic, and cultural life did truly seem to have an "of the island" orientation.

Living in the Right Place

When we had first stepped ashore on Nanumea several weeks previously, the local government official (Island Executive Officer or IEO, in the parlance of that time) had come forward, introduced himself, and escorted us to a vacant house on the government station. We could live here, he said, since the radio operator (whose allocated house this was) was a local man who stayed with his family in the village. The house had been carefully prepared for our arrival. New mats were spread over the gravel floor and the local-style walls and roof both showed evidence of fresh repairs. Households of government employees such as the meteorological recorder and the IEO himself, both Tuvaluans but not Nanumeans, were our immediate neighbors. Nearby were the guesthouse where visiting administrators were housed, the radio building, and the Island Council office. As the name implied, the government station was a place intended to serve the needs of the colonial government and bounded off from the rest of the community. Employees native to the island, such as the radio operator whose empty house had been lent to us, usually lived with relatives in the village, a choice that emphasized the "island" dimension of their identities. Since the village was just a few minutes walk away, it did not occur to us at first that living on the government station might conflict with our "of the island" identity.

The British research grant supporting our work included several hundred dollars, quite a large sum in those days, allocated for the construction of a new house. It seemed wasteful not to infuse this money into the local economy, and when we made this known, general enthusiasm developed for the community to work collectively to build us a house. The money would be used to feed the workers, who would be organized into customary work groups based on the two village sides. Building the house would be a communal work project: each family would supply an assigned amount of common building materials but the larger items would be brought in from communal lands. All of the community's buildings were built this way, we were assured. When our research was completed, the island would find a use for the house and look

after it until we returned, as we asserted we hoped to do in the not-too-distant future. This proposal seemed to fit well with an "of the island" identity.

Finding a suitable location for the new house was far from easy, however. Several families offered us sites on their own lands, though others found discreet ways to remind us that communally building a house on privately owned land was incongruous. The pastor graciously offered us long-term use of his guesthouse but we demurred, thinking how difficult it would be for members of one of the island's "new religions" to visit us there and of other constraints that living in the church compound could entail. Except for the central church grounds, the meetinghouse, the school, and the playing-field areas at the village's center, there wasn't much communally owned land within village boundaries to choose from. With our local identity now defined, we realized the importance of living within the actual village boundaries rather than outside them, as some families were now opting to do.

In the end, following subtle local direction, we settled on a lagoon-shore location that was almost precisely on the midline dividing the two village sides. This area had been filled in and built up as a dock by the occupying American forces in World War II, an irony appreciated by both islanders and ourselves. Additionally, since membership in the village sides was defined by residence in the first instance, this location had the tacit advantage of separating us from automatic inclusion in either village side, groupings that in these years were in active rivalry with each other. The elementary school was next to our house site, and our closest neighbors would be the families of the headmaster and teachers, though other village houses were in sight as well. The island gained a new building on communal property, while we could establish a home base conducive to developing relationships widely across the community. Building began immediately and energetically.

In the end, however, our grant funds purchased only token amounts of the vast quantities of food needed for so many workers over the months it took to build the house that the community designed. The house developed into the local equivalent of a palace. Constructed entirely of island materials, including a pandanus-thatched roof, it was much larger and more elaborate than two people could ever need. Admittedly, its two-story design followed our "suggestion": we *had* once commented that with the school and so many curious children close by, second-story privacy would be an advantage for our work. However, we left the house's overall design and construction up to the community. Twelve miles of hand-rolled sennit cord were eventually needed to tie the house together and several palm trees were felled for timber, laboriously hand-sawn into planks, and adzed to a luxurious smoothness. We now wonder whether the villagers saw this house-building project as an opportunity to display their traditional building skills or to showcase the

community's ability to cooperate for communal well-being. Similarly, to what extent did our house's completion revive community enthusiasm for its long-deferred renovation of the pastor's house, including a second-story addition? Certainly, however, the finished house was absolutely beautiful.

Clearly, a living situation for us that confirmed our "of the island" status on several levels had been brought into being. The locale from which our research proceeded was a central area of the village with strong communal associations. Like ourselves, the dock and its adjacent coastal landfill had been added to the village by external forces, though the current use of the area for the school claimed it for the community. With our research beginning to make some local sense as we focused on traditional history and customary practices as well as the socioeconomic questions emphasized in the government project, we were probably conforming comfortably to local expectations. We began a slow census of the village, visiting by prearrangement two or three households each day, a gradual process that allowed us to meet each family individually and discuss our research again personally. The village had come to surround us and to permeate our work. With our new house only a stone's throw away from the meeting hall and playing field, the public spaces where community was symbolically enacted, we seemed to be living out our "of the island" identity relatively unproblematically.

Maintaining "Equality"

However, we were becoming increasingly aware that the community expected us to maintain relationships across the broad spectrum of village households. From comments people made, we realized that a close watch was being kept on our activities. Spending considerable time with traditional experts and established elders made local sense, but notice was taken of where we socialized and to whom we gave small gifts of store items. Reports of where we had recently been sometimes reached back to us through gossip channels. Since "where are you going?" was the standard local greeting, walking anywhere involved us in strategic declarations. Luckily, the census continuously expanded our social networks and probably appeared to move us through the community in a holistic and predictable way. Also, as our separate research foci became better delineated, we each had a chance to interact with a wide range of people. We felt that we were connecting quite broadly with the community, not favoring any particular segment and supporting the "equality" that speakers at meetings so often emphasized as the foundation of community life.

The emic importance of also maintaining an equitable relationship with the two village sides did not occur to us at first. As the work groups of island

life, the two village sides collaborated and competed at every communal event. They marshaled resources and labor for communal projects, provided food for feasts, and invigorated traditional festivities with competition. The village sides seemed to us to bring Nanumea into existence as a community. Thus when we attended island events organized by these groupings, we saw ourselves as interacting unequivocally with the community as a whole.

Ironically, maintaining our "of the island" identity would require us to distance ourselves from either village side, a social fact it took us several months to understand. In the meantime we participated in countless community feasts, meetings, holiday celebrations and dances, as well as wedding and funeral activities sponsored by various extended family groups. At community events, we sat sometimes with one family, sometimes with another, strategically sharing ourselves around. We used no particular plan or sense of pattern though perhaps we should have. One evening during the annual Christmas–New Year celebration of Big Days, the family with whom we were sitting scolded us. Why did we sit with just one village side again and again? Everyone, they said plaintively, was wondering why we favored one side over the other in such a public way. Ignorance never offers a very strong defense, and there wasn't much we could say except to promise to be more careful in the future. And we were, joining alternate sides at events and sometimes splitting up to each join different sides when competitive spirits ran especially high.

As we came to understand local politics and community organization more fully, and gradually developed an understanding of precontact society from the recollections of elders and traditional history accounts, we began to realize that our "of the island" role was fulfilling a convenient, didactic role for the community. Our continual enactment of a relationship with the island as a whole emphasized the value of community unity, a value that was heralded in speeches at virtually every public gathering. This discourse drew its cultural meaning from the key structural tension of Nanumean society: how to balance competing obligations to family interests and community. *Loto fenua* or *loto gatahi*, community loyalty, had long been and still was thoroughly institutionalized in island life. Its importance was apparent in customs ranging from premissionary limitations on family size, to current expectations that fish would be shared with nonrelated neighbors, to the cross-cutting pattern of membership in village groups. Kinship and descent structured social life along many other essential dimensions, however, and people also strategically maintained the well-being of their extended families. When community and family loyalties did come into conflict, as they inevitably must, community responsibility seemed to be given cultural priority, typically enforced by sanctions or heavy persuasion. In this context, we offered a highly visible embodiment of community loyalty.

This was probably a significant cultural message. The 1970s and 1980s were a time of community intensification in Nanumea. The scale of communal building projects, the protracted length of the annual Big Days celebrations, and the relative consensus about local tradition and communal goals all seem remarkable in retrospect, especially given the factionalism of the last decade. Was the "of the island" identity allocated to us a product of this era of community cohesion? Was our acceptance and relatively successful enactment of this role also reinforcing *loto fenua* as an island value? We suspect that the answer is yes to both questions.

Our unconscious reinforcement of community priorities may have been especially apt because the communal celebrations and achievements that took up so much Nanumean time and energy comprised just one dimension of local life in this era. Market-economy influences, increasing pressures for individualized achievement and "development," as well as off-island economic opportunities were cumulatively driving a relentless pace of change. Community members were finding it increasingly difficult to prioritize communal responsibility at the expense of personal and family well-being. By the time Tuvalu became an independent nation in October 1978, Nanumea and other outer-island communities were poised on the edge of worldwide engagement. Even the idea of community itself was being reshaped to accommodate national political interests. By the 1990s Nanumeans living in the capital had formed a subsidiary community, evocatively named Nanu-Futi, which increasingly led decision making for Nanumea as a whole. Nonetheless, the essence of Nanumean culture and community identity was still seen as grounded primarily in the home atoll itself.

As our relationship with Nanumea matured and endured, it necessarily took account of these changes. We developed rapport and relationships with the Nanu-Futi community and with migrants to Suva and Auckland. In all these encounters, our "of the island" identity seemed to have become entrenched as the basis for our ethnographic role. Clearly we were not and could never be true Nanumeans. But community leaders, and probably Nanumeans in general, continued to find it useful to include us metaphorically in that category. Furthermore, certain markers that had earlier carried definitive symbolism became unimportant. "Our house" is still in fine shape (minus its second story) but when we return to Nanumea, the community no longer accommodates us there. The headmaster and his family continue to use it, while a spare house on the government station is refurbished and furnished for us. "Who we are" clearly requires less overt symbolic marking now. The solidification of our identity is also illustrated by two subsequent developments that generated wide community interest and concern during the 1990s.

Coping with "Something That Happened" (Mea Tupu)

We learned about the *mea tupu* within only a few hours of our return to Tuvalu in May 1996, our third fieldwork visit. A conflict with serious sociopolitical implications had apparently taken place on Nanumea, only referred to circuitously as *mea tupu*,"something that happened." No one offered details at first, though it was clear that the issues were both complicated and contentious. We gradually learned that a dispute had occurred in 1994, developing over several weeks, with other problems arising from the original one. To everyone's chagrin, over a year later the *mea tupu* still hovered like a black cloud over community activities. A special delegation of church leaders, government officials, and members of the Nanu-Futi community had been sent to Nanumea some months earlier to mediate a solution. Though the delegation's efforts had seemed successful at the time, no long-term resolution had resulted. Nanumeans whom we talked with in 1996 were worried, and somewhat embarrassed too. The festering of an intractable dispute such as this called into question the value of *loto fenua*, casting a public slur on the community's reputation and making Nanumea the unwelcome target of national gossip.

Several leaders in the capital, the prime minister among them, suggested to us that our current visit and impending return to Nanumea might be fortuitous. It could provide just the impetus the community needed to regain a united front and resolve their problems. These suggestions surprised us, although we can now see that our history of being "of the island," backed up by years of predictable participation in virtually every village event that occurred while we were in residence, perhaps provided a logical basis for these hopes. Whether these hopes would be fulfilled or not by our return remained another issue.

Over the next weeks we pieced together the exceedingly complicated chain of events that had caused such serious social disruption. Many underlying tensions appeared to have surfaced at once, perhaps in cumulative response to rapid cultural change and the global economic pressures the community had been accommodating for decades. Religious unity around the single Congregational church, which dated from the island's initial capitulation to missionary pressures in 1872, was becoming increasingly untenable under the strain of individual claims to religious freedom, which were backed by rights specified in Tuvalu's independence constitution. Locally, the issue involved not just the right to practice a religion of personal choice, but also the right to proselytize for it publicly.

Furthermore, the authority of the traditional chieftainship, reinstituted (after a hiatus of three decades) not long before, was proving more problematic than

anticipated. Consensus about traditional political structures and governing processes had waned to the point that there was insufficient authority to resolve a political impasse. The incumbent chief had first protected the right of a "new religion" to use a traditional metaphor. This had been perceived as slighting the primacy of the majority church and its pastor's authority. The chief had subsequently made a judgment that some community women perceived as favoring one village side over the other in a fund-raising activity. Furthermore, as community dissatisfaction mounted, the chief had refused to relinquish office, even when the lineage that many people asserted had traditional authority to install a replacement asked him to do so.

The dispute also brought into the open smoldering dissension about Nanumean leadership roles in general. The "younger" men (in their forties and fifties) demanded the right to voice their opinions in meetinghouse discussion of the dispute, noting that they had already contributed years of service to the community and that their experiences and Westernized skills were greatly needed. The "older" men, appalled by the strong language and raw emotion in younger men's speeches as well as by their opinions, claimed exclusive traditional speaking rights. These contested interpretations solidified around a church-versus-community split, which increasingly paralleled the division of older-versus-younger men. Because of the cross-cutting membership structure of the community, the village sides were rendered dysfunctional too. Even simple routines such as the Saturday cleaning of the meetinghouse were impossible to organize. Public festivities and decision making alike had ground to a halt. It appeared that communal identity and "unity of heart," traditionally enshrined as core principles of Nanumean society, had been put on hold by the dispute. This impasse had dragged on for over a year by the time of our visit in 1996.

Most community members held firmly to the hope that time would heal the dispute even if mediation had not, but its intractability was worrisome. People expressed concern that Nanumea's communal orientation was directly threatened by the crisis and might not survive it. We wondered whether the dispute, which prevented normal community interaction month after month, would itself lead to overt structural changes in Nanumean culture and social life. Subtle pressures to bring the community's exchange practices and ideology, social patterns, and political system into closer accord with Western assumptions (implicit in national development efforts and individuals' increasing participation in the world market economy) had been underway in Nanumea for a century and had intensified since independence. Perhaps the issues that had surfaced in the dispute were evidence that value shifts had already occurred. On the other hand, a more optimistic interpretation also seemed plausible. Periods of intense community cooperation and periods

of factional struggle had undoubtedly served as necessary counterpoints in the intense process of community life in the past as well. The island had coped successfully with times of confrontation and dispute before.[9] Perhaps in the next few years, Nanumea would regain some of the cohesion that still appeared to be valued.

The hope that our presence might somehow ease the situation pervaded the Nanumean response to our return in 1996. Because we were "of the island," people were hopeful that the various factions might be willing to set aside their differences to create the appropriate, united welcome for us. Everyone knew that we had seen and participated in earlier days of greater harmony and collective purpose. Several people now involved in the dispute had themselves previously explained the importance of communal values to us and helped us understand how the community coped with historic and recent threats. We knew, as did they, that things now were not quite "as they should be." Because the return of community members after a lengthy absence requires a public welcome, our arrival did offer a natural opportunity to regroup and set the dispute aside. This was surely what people in Funafuti were envisioning when they speculated that our return might prove helpful.

Unfortunately, this outcome was not fully realized in the months that followed. In the capital, things went well. The Nanumean community there, Nanu-Futi, warmly welcomed us back and arranged for our housing. They hosted a well-attended traditional welcome feast, followed by an evening of *faatele*, communal singing, drumming, and dancing that captures the very essence of communitas (Turner 1969). We thought it a poignant commentary that this *faatele* session in the capital opened with "A Galiga o Fenua," a locally composed song extolling "unity of heart" as the source of Nanumea's "true beauty" and enduring strength. We had included this song as a frontispiece in an early ethnographic writing in 1975 because it seemed to capture the communal orientation of Nanumea so evocatively. Its choice to open the evening's festivities, and continuing allusion to this *faatele* in speeches that night, gave voice to concern about the dispute and commitment to community loyalty as a core cultural value.

Though we were welcomed back to Nanumea quite genuinely a month later, the official welcome celebration there was both similar and heart-wrenchingly different from the one in the capital. Again, "A Galiga o Fenua" was chosen to begin the *faatele* festivities, but this time it seemed a plaintive reminder of the communitas that everyone attending knew had disappeared from island life since the dispute. The competitive structure needed for the *faatele* had to be drawn from an ad hoc older-younger division (rather than the established village sides), with fifty-year-olds carefully put into the older

category. Turnout was very low, since those in opposition to the organizers did not attend. The meetinghouse, always crowded in the past, echoed forlornly. Our return clearly had not provided the impetus needed by the community to set aside the hard feelings and angry words from months of dispute.

While on Nanumea, we talked with many community members, trying to understand the varying points of dissension and what communal responsibility now entailed. These discussions led us to ponder our ethnographic relationship to the community and especially to consider how our fieldwork identity had drawn us logically into a hoped-for dispute resolution role. Symbiotic effects seemed to link "who we were" to the community and "what we had come to know" of local life. To what extent had we "naturally" come to focus on the community as a whole as we lived out our "of the island" identity? Had we perhaps paid less attention than we might otherwise have done to the strategic machinations of individuals and families? Not living in a Nanumean household had denied us direct experience of interpersonal joys and strains as well as the acknowledged "research advantages of living with a family and observing it on a daily basis" (Foster 2002:257). To what extent did our resolute conforming to the strictures of an "of the island" identity offer methodological compensation? Due to her Hawaiian birthplace, Anne had gained recognition as a fictive member of the Kau Hawaii (the descendents of a nineteenth-century Hawaiian sailor) but this group was diffuse, including perhaps a quarter of island residents. Being "of the island" had theoretically given us access to all families, but how much do understandings derived from a large network of relationships differ from those based on more-intense experiences with a few? How could we pinpoint exactly what understandings had been gained and lost?

Completely recapturing "who one once was" in earlier fieldwork incarnations is an impossibility, as Wolf has noted (1990). Certainly we ourselves were no longer the young ethnographers who had accepted and then tried to live up to an "of the island" identity back in 1973. We had aged as persons, and we sensed a parallel maturation in our identity. Foster suggests that an ethnographer's expectable progress along the "trajectory from whippersnapper to elder" usually confers research benefits, resulting in greater access to people and information (Foster et al. 1979a:331). That seemed generally to have happened for us, though the process had been gradual, lacking any defining indicators.

Envisioning a Cultural Constitution

A second and more positive development also occurred in 1996, which helped to clarify how our "of the island" identity might affect our future relationship

with the community. About four years previously, several Nanu-Futi leaders had decided to compile an overview of Nanumean traditional knowledge. This was conceived as a "cultural constitution," a *fakavae* or "foundation" document, that would specify the distinctive customs that set Nanumea apart from other Tuvalu islands and summarize the island's traditional history and sociopolitical institutions. We first heard of the *fakavae* project in Funafuti at about the same time that the dispute was mentioned. One of the initiators, Tagisia Kilei, asked whether we could find out, when we got to Nanumea, what was holding up the *fakavae* progress. We were mystified at first by his request, having only heard the term *fakavae* used to describe the national constitution, so Tagisia explained how the idea had arisen and what the project involved.

Nanu-Futi's leaders had drafted a long list of questions, which Tagisia showed us, carefully typed up, spaced out over nine pages with room for an answer below each question. The document had been sent several years before to the Nanumean atoll community (as the repository of traditional knowledge) with a request that the questions be discussed by elders there and definitively answered. The Nanu-Futi leaders would write up the *fakavae* once the questionnaire had been completed. But several years had already passed. Tagisia had heard that the elders were not able to agree on certain answers, and that this was probably the reason why the questionnaire had not been returned.

The *fakavae* questions were diverse but some dealt with the same issues that the dispute had raised. What were the rules for succession of a reigning chief? Who, or which chiefly group, had responsibility for determining when a chief should step down so a new one might be chosen? What political structure had the island's founder originally imposed, and what traditional accounts provided justification? Many of the questions concerned traditional social organization and ritual, topics we had also pursued during our work. Especially during our first long visit in the seventies, we had spent many hours conversing with the most knowledgeable elders of that era. Most of these people had since passed on and, while the next generation of elders provided replacements, the Nanu-Futi leaders seemed to fear that real expertise about these topics was waning and time was running out.

Could we check into the status of the questionnaire? Would we be willing to push this along if we could? We agreed, highly curious about this insider effort at cultural documentation. Many of the Nanu-Futi leaders had had careers away from Nanumea, in the phosphate-mining centers of Banaba or Nauru, or on other outer islands in the former colony as civil servants. Educated and cosmopolitan, they nevertheless valued the traditions that supported the community's distinctive identity.

They knew that on Nanumea the "old men" still deliberated in the island meetinghouse, new high chiefs were periodically installed, and traditions ranging from interhousehold sharing to "volunteer mothering" were still practiced (though sometimes with varying degrees of unanimity, as the dispute had shown). Nanumea itself remained the heart of the Nanu-Futi community, and many of the urban elite expected to retire "back home" eventually. But other factors were quietly undermining the strength of traditions during the closing decades of the twentieth century, increasingly shifting the orientation away from Nanumea itself to the "bright lights" and opportunities associated with the capital. High-achieving young people were sent to the national boarding school on Vaitupu or to private schools in Fiji or Samoa. Many young men worked on overseas ships for years at a time. Growing numbers of both men and women found their way to jobs in the metropolitan magnet centers of Suva, Auckland, and Sydney. Each year death skimmed off more elders left on the home atoll, and many of those remaining were drawn away to the capital to be cared for by younger relatives or to seek medical care.

Thus it was perhaps not surprising that the leaders of the Nanu-Futi community in Funafuti envisioned a project that would encapsulate Nanumean customs and the traditional basis of leadership roles, or that they wanted a repository of knowledge that they could access as needed. The elders in Nanumea were several days' sea journey away and that community's coherence had recently appeared to be in decline. The dispute made it seem even less likely that the elders could ever agree on answers to the *fakavae* questions. Precisely this juxtaposition of dispute and *fakavae* was probably what led Tagisia to ask for our help. Since we were "of the island" we could be asked to assist, but unlike other community members we had no particular affiliations that would compound already difficult relationships. Our connection had long been demonstrated to lie primarily with the community as a whole, rather than with any single faction. The leaders of the village sides knew that we had taken pains to remain neutral in the past. We were not beholden to the pastor or deacons or linked to the church faction, any more than we were to the younger leaders who advocated change. We were probably about as close to being "neutral insiders" as it was possible to be.

As it turned out, an abbreviated set of answers was waiting on Nanumea even as Tagisia made his request to us, ready to be sent back to the capital on the next boat. Though the returned information provided little elaboration on the more difficult political questions, the elders' responses did allow the *fakavae* project to move ahead once again. Nanu-Futi leaders subsequently asked us to join them in drafting the *fakavae* itself, providing an appropriate

way for our ethnographic "data" on oral history and traditional customs to make its way back to the community as a whole. We used our next sabbatical opportunity and spent five months in Tuvalu in 2003-04 assisting with the "cultural constitution" project. A preliminary draft manuscript (one version in Tuvaluan, the other in English) resulted. This draft is currently being augmented, with Tagisia Kilei continuing work in Funafuti and with our input from time to time.

"Convenient Fiction"

For us more than thirty years ago, being given an "of the island" identity was reassuring. We interpreted this classification as potentially offering a genuine connection to the community, a basis from which we could develop the much-desired ethnographic role of "outsiders who know something of what it means to be insiders" (Keesing 1991). We felt that we had been offered a mutually convenient starting point for relating with Nanumea's people that would be productive and enduring. The category was indeed convenient in the sense that it did provide us with a workable ethnographic identity for three decades. But, ironically, being "of the island" was a useful identity primarily because it masked our separation from the community so effectively. We were putatively "of" the community, but never really "like" any of its other members. In effect, we became the exception that proved the rule. We were "of the island" in its most generalized and idealized sense. Lacking normal family connections and the constraints these inevitably impose on communal loyalty, we could safely be seen to belong to everyone.

A Nanumean interpretation might place the emphasis differently. Being "of the island" from the islanders' view probably primarily offered us a connection to *community*. Reified and revered, dangerously ephemeral and always contested, *te fenua*, "community," was truly the foundation of Nanumean identity. In our case, though, the connection was a fiction, an identity that never could be fully true because it was idealized, contrasting with the personalized relationships of other community members. Yet extending this category seems a symbolic vote of confidence for us as researchers, but especially for the value of *loto fenua* itself.

In their recent overview of long-term fieldwork, Royce and Kemper emphasize the interconnection between the analytical and personal aspects of research (2002:xxxiii), quoting Geertz: "Everything . . . has both to form the substance of one's personal existence and to be taken as the grist for one's analytical mill. . . . In the field, the anthropologist has to learn to live and to think at the same time" (2000:39). We credit our "of the island" identity with enabling a productive synthesis of ethnographic living and

thinking. Hopefully, it will carry us along a future ethnographic path that both the Nanumean community and we find fulfilling, its fictional nature notwithstanding.

NOTES

1. For reasons both similar and distinctive concerning positionality, researchers working within their own non-Western communities or their own minority ethnic groups seem to have a parallel advantage. See Jacobs-Huey's discussion of "native scholars" and the contribution these researchers have made to the decolonization of anthropology through reflexive analysis of the complexity of insider-outsider roles when "working at home" (2002).

2. At present, the story is essentially ours. Though we use all the cultural understandings available to us, we have not yet had the opportunity to discuss the ideas in this essay with the Nanumean community. We intend to pursue this in the future, however, in belief that emic perspectives will significantly enrich our analysis and that this essay will provide a useful starting point for these discussions.

3. Our fieldwork in Tuvalu and Nanumea to date has included five visits: May 1973–January 1975 for our initial work; December 1983–June 1984; May–July 1996; a trip by Keith alone in May–June 1998; and sabbatical research from September 2003 to February 2004.

4. The most recent example of our being seen as a single social person in Nanumean eyes occurred in August 2003. We sent a draft of the dedication page for a book of women's songs to a Nanumean friend for editing. He revised our names, which we had listed as "Ane mo Kiti" (Anne and Keith) to read simply as "Ane/Kiti," effectively collapsing us into a single identity.

5. Samuelu seemed to assume from the outset that an "of the island" identity would be appropriate for us, which suggests that this category may have been used as a local strategy to accommodate outsiders in the community. Lutz found that the Micronesian atoll community of Ifaluk (Caroline Islands) also had typically managed contacts with outsiders over the last century by making "insiders" of them (1988:38). On Ifaluk this involved encouraging visitors to "dress and eat in the local style and to observe taboos and other local behavior codes." As an Ifaluk "insider," visitors and researchers such as Lutz were seen as needing special "protection," creating a fieldwork relationship that has some parallels (but also certain important differences) with the fieldwork identity we describe below. One significant difference was that our primary protective relationship was with the community as a whole, whereas Lutz was adopted as a "daughter" into one clan leader's household.

6. "Old men" or "elders" are glosses for Nanumean *taumatua* or the pan-Tuvaluan term *toeaina. Taumatua* can refer to female or male elders, while *toeaina* usually implies a male referent.

7. Lutz's description of the Ifaluk view of "pitiable" persons helped us to conceptualize issues relevant to our own situation. In Ifaluk, "people without kin are pitiable, not only because they are lonely or because they have fewer land and labor resources; such

people are also to be pitied or even scorned because they do not take care of others" and thus lose the possibility of exerting control over, and thereby gaining the respect of, other community members (Lutz 1988:142). After ascribing an "of the island" identity to us, the Nanumean community "took care" of us by designating a roster of families to provide local food. This nurturing role also involved an element of control, in that our potential to develop special relationships with specific families became problematic because of our preexisting connection with the community as a whole.

8. Village "sides," *feitu*, are ubiquitous organizing principles throughout Tuvalu. The two sides' strength and involvement in most aspects of Nanumean life at the time of our initial work was something we took for granted as part of Nanumean "reality." The role of the village sides weakened considerably in subsequent decades.

9. Traditional historical accounts suggest that one of these times of difficulty occurred during the imposition-acceptance of Christianity in the late nineteenth century, which resulted in intense disputes between Christians and "traditionalists."

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CHANGING PERCEPTIONS OF A MISSIONARY–RESEARCHER

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The missionary-researcher might well begin fieldwork after an existence as book-bound as any Ph.D. candidate. However, the motives for going to live with people differ from those of most academic anthropologists. Whether one has an academic qualification or not, the primary motive for the missionary-researcher is to come to know and understand people so as to share a faith message with them in a meaningful way. The key term is "dialogue" and just how long that dialogue might last is often not a major issue. For the neophyte academic anthropologist and the novice missionary-researcher alike, culture and sharing people's lives is a means to an end: for one, an academic qualification; for the other, a key to people's minds and hearts.

HOW IMPORTANT is the time factor in field research? Mervyn Meggitt, one of my predecessors in research among the Enga people of Papua New Guinea, wrote that

> I believe that the great danger in reliance on synchronic investigations is that insensibly they may impose on the anthropologist a synchronic analysis, a theoretical set or predisposition that confirms him in an uncritical acceptance of factitious equilibrium or stability in the sociocultural complex he is observing. In consequence, as we see displayed repeatedly in anthropological monographs, stability is taken to be the norm and change the problem to be explained, whereas, if anything, the assertion should be the other way around. (1979:122)

Pacific Studies, Vol. 27, Nos. 3/4—Sept./Dec. 2004

Meggitt is arguing for the importance of observing the same community frequently over a long period. As a missionary-researcher I have had the good fortune to continue frequent contact with a group of Enga people over a period of thirty years. Although I have lived, worked, and studied elsewhere, I have had frequent contact with people in the Par parish in particular and Enga in general, first as a seminary student (1973–1974), later as parish priest for five years (1983–1988), and now as a regular visitor for research or teaching (1995 until today).

In this article I trace the development of my identity as a missionary-researcher/researcher-missionary in the context of my periodic returns to the field in Enga Province. My identity is closely linked to my ever-changing relationships there, and these changes have consequently had an impact on my understanding of "culture." An underlying issue is how I have experienced trying to be both missionary and anthropological researcher.

Missionary and Researcher

My entry into the fields of both missionary and anthropological researcher came about more by chance than by choice. The thought of being a missionary was transformed into conviction after an accident I survived while my two climbing companions drowned. The inspiration to study anthropology came at the end of the novitiate year, after my novice master informed me that my taking of religious vows should be postponed until I had had more "rough edges knocked off" and that in the meantime I could study "something." Having an undergraduate degree in sociology from the University of Canterbury in Christchurch, New Zealand, I cast around and found I could do postgraduate studies in anthropology at the University of Sydney.

The year of courses at Sydney University proved to be something of an ordeal. My study required the equivalent of three "years" of undergraduate courses, plus honors seminars, all in one year. In February 1973, with the religious novitiate and the year of initiation at Sydney University completed, equipped with my Bible and Lawrence and Meggitt's *Gods, Ghosts and Men in Melanesia* (1965), I flew to Papua New Guinea. I was going to the Enga District, ostensibly as a seminarian, but with the possibility of collecting data for my thesis during my "spare time."

Enga has since become a province in the independent nation of Papua New Guinea (PNG). This highlands province now has a population of at least 295,000 people (Census 2000). Most settlements lie between 1,500 and 2,500 meters above sea level. The majority of the population are Enga speakers, though other languages are spoken in certain areas, notably Ipili and Hewa in the province's far west. Traditionally the Enga people lived by hunting

and subsistence gardening. Pigs were raised for food and, more importantly, for ceremonial exchange. Today most people depend on their gardens for food, supplemented by cash from cultivating coffee and other marketable agricultural products, employment in public-service jobs, and in particular from economic developments associated with the Porgera gold mine.

First contact with Westerners occurred in 1934 when the Leahy brothers ventured briefly into central Enga, followed by Taylor and Black during their Hagen-Sepik patrol of 1938–1939. Christian evangelization began in 1947, with missionaries from the Lutheran, Catholic, Apostolic, and Seventh-day Adventist churches. Most Engas today are baptized Christians. Approximately one quarter of the Enga population are adherents of the Catholic Church, forming the Catholic Diocese of Wabag, led by Bishops Hermann Raich and Arnold Orowae, the latter from Enga. The diocese is organized into sixteen parishes, each supposedly headed by a parish priest, though some parishes lack a resident priest because of fewer missionaries and the slow growth of the national diocesan priesthood.

"Red-skin Boy"

In the early 1970s the "standard" Malinowskian research model was the norm in the anthropological circles I frequented. The ideal was considered to be extended stays with isolated groups in order to grasp the indigenous worldview. Enga Province seemed to provide an ideal opportunity to put such an approach into practice. There were occasions when children who had never seen a European might meet me on a track and their eyes would open wide before filling with tears as they ran for comfort.

I was aware of following in the tradition of Wilhelm Schmidt of the Kulterkreis School. Schmidt, of the Society of the Divine Word (SVD), had attempted to show that the monotheism found among "primitive" peoples was traceable to a primeval revelation (*Uroffenbarung*) passed down by tradition to all the descendants of Adam. Admittedly his theory was no longer taken seriously; however, the Anthropos Institute continues and as a member of the Society of the Divine Word myself, I felt almost obliged to study the religious beliefs of the people among whom I was working.

My understanding of culture at that time came straight out of the books we had been studying in Sydney. With my functionalist categories, Enga "culture" seemed like a fascinating collection of exotic data just waiting for me to uncover. People would patiently answer my endless questions about how, why, and when, but I hardly recall being conscious of the link between the people and their culture. With my museum mentality, I failed to appreciate the intimate connection between who people were and the way they lived.

I began my sojourn at the Par mission near Wabag, living with Father Henry Feldkotter. He spoke the Enga language well and knew a lot about the people, especially what he considered their faults and failings. He would spend his evenings looking for errors in the Enga dictionary (Lang 1973).

People invited me to visit and practice speaking Enga. I only discovered later that the different clans were vying to have me on their side. I found that the older people addressed me either as *kone* (red-skin) or as *wane* (boy), or a combination of both. Even though I was twenty-five years old and had a full beard I did not qualify to be called *akali* (man). The primary requirement to be called a man was that one be married and have children. Since I was trying to be celibate, did this mean that I would be forever addressed as "boy"? The prospect did not appeal to me but I felt there was little I could do about it at the time.

The term *kone* is not reserved for pale-skinned Europeans. A light-skinned Enga person might be called *kone*, and pigs with light reddish hair are also called *kone*. When applied to persons, the opposite would be *pumbuti* (dark, black). Sometimes, if children called me *kone* in a cheeky way, I would respond *pumbuti*, and people would laugh, with little concern about racial overtones.

From hindsight I realize that being addressed as *wane* had more serious implications: it meant that I was unlikely to be taken seriously by the leaders in the community. Father Henry, through his mastering of the language and his position as parish priest, had established his leadership credentials and was not called *wane*. Yet being *wane* allowed me to relate easily with the young people. The women were happy to have a young man eager to accept invitations to courting parties and marriage feasts. No doubt some considered me a prospective partner. (In my first week at Par I was introduced to a young woman feeding a light-skinned child, the son, I was informed, of a European missionary.) The young men convinced me that I should help them build a dance hall. Wanting to be accepted and eager to please, I did my best to fit in with the other "boys" in the community. I remained only six months at Par, but many of the relationships begun then still endure.

After a number of false starts the "red-skin boy" began collecting material on traditional Enga and Ipili religion. Looking back now, I realize that not having quality time with the older men or leaders in the community seriously limited my access to information about sacred matters. However, with the data collected I wrote what I thought was a reasonable draft of my thesis. I will always remember the feeling of dismay, after my return to Sydney in 1974, hearing Professor Peter Lawrence's judgment on my work: "These are excellent field notes, now you must write them up as a thesis."

Spiritual Rambo

Ten years later, in 1983, I returned to the Par mission in a different role. I had studied four years in the United States, been ordained a priest, and had worked happily for four years in the Porgera and Paiela valleys at the western end of Enga Province. By then the priest at the Par mission was Father Leo Defland. He spoke Enga fluently and reputedly knew more Enga swear words than any of the local lads. Eventually I took his place. Being parish priest in a large Catholic community provides a certain institutional status. It also allowed me a privileged opening into people's lives. However, it cuts both ways. On the one hand, hearing hours of confessions each week certainly gave me insights into people's trials and difficulties that I might not have otherwise heard. On the other hand, I simply could not be an impartial observer. At times, as when young church members would enter into polygamous marriages, I felt obliged to confront them though their behavior was sanctioned by traditional cultural norms.

My thesis at Chicago was an attempt to apply both theological and anthropological insights to an Ipili myth. I had read widely in the symbolic anthropology of Geertz and Turner and was fascinated by the structuralism of Lévi-Strauss. Somewhat skeptically, I tried a structural analysis of the myth and it worked! In that American academic setting, in the transformations and oppositions of that Papua New Guinea myth, I felt that I had uncovered many new insights into the underlying grammar of the culture.

My academic insights proved to be short-lived following my return to Enga. In the 1980s the situation was changing rapidly. Papua New Guinea was already an independent nation. There was a new confidence and urgency, particularly in the world of politics. Also, we experienced a resurgence in tribal fighting. I'm sure that spears and arrows may have deeply symbolic meanings, but when the shouting gets louder and the arrows start flying, there is little time to think about symbols or underlying structures.

If I were simply a researcher I imagine that I would have observed the fighting and asked questions after. However, as missionary, I felt I must intervene in the fighting before someone died. In those days—the early 1980s—fighting was with bows and arrows, and with spears and axes at close quarters. I had a gun but sent it back to New Zealand, lest I be seen as a *kiap* (colonial government figure), or lest someone break into my house to steal the weapon. Instead I found a sheet of flat iron and fashioned a rough suit of armor that I wore beneath my motorcycle jacket, and on several occasions went into the middle of fights before anyone was mortally wounded. The sight of a missionary, dressed in a red motorcycle jacket, wildly gesticulating in the middle of the fighting ground was much too distracting for many fighters,

most of whom stopped fighting to figure out how to deal with me. Looking back it appears foolhardy in the extreme. Other times I would come with my camera and threaten to photograph the fighters and hand over the photos to the police. Miraculously the interventions paid off. I would go to both sides of the conflict and use the parish car to take any seriously wounded to different hospitals. Two leading warriors were seriously injured in the fighting at Yampu, near Par. My taking both to hospital and their subsequent survival opened up new relationships with the two men (as well as other clan leaders).

Perhaps it was more good luck than good management, but no one died in a tribal fight during the five years I was at Par. My apparently successful intrusions into the militaristic dimension of life were enough to counter perceptions of my single status or any lack of maturity. I no longer heard people calling me *wane*. It took political turmoil to bring about a change in people's perception of me. A fellow missionary asked facetiously if I was trying to be a "Spiritual Rambo," referring to a character whose name, borrowed from American films, is now a commonly used term in Enga for a fearless warrior. The modifier "spiritual" would no doubt sound odd to Enga ears; however, perhaps my fellow missionary was making a valid point, and Spiritual Rambo sits well with the term the local people used: *nee kapae singi* (one concerned enough to intervene in time of trouble). My protest, and refusal to respond to traditional cultural strategies in established ways, resulted in my initiation into a new social identity.

Guardian

By the end of my first year as missionary-priest at Par I was feeling bad about not having learned the local Enga language well. I decided to take six months of local leave for that purpose. So, at the beginning of 1985 I went into the mountains and lived with a family, making a point to have no dealings at all with outsiders and not to speak or read any English during my time there. I even had a tape recorder playing Enga language tapes at low volume the whole night while I slept.

It was a strange existence up in the bleak muddy cold heights of Kaiputesa (3,000 meters). I was delighted to have the opportunity to learn the language —something I had wanted to do for ages. However, I found it very trying as the learning went slower than I had anticipated, and I feared that six months would be too short to gain control of the language. The time there was a learning experience in many ways as people's lives and feelings were revealed. Living, eating, and sleeping with a family, I started to realize many things that I didn't even know enough to ask questions about before. The

youngest child in the family died. Another close family member was killed in an accident. I wasn't just observing the funerals. I was part of them.

The experience left a lasting impression on my values and spirituality. Sitting with the old men, we talked about the initiation rites that were no longer practiced. They lamented the fact that young men were no longer interested in such rites. I could see that that the whole worldview of the young people was changing, but volunteered to learn some of the traditional myths, songs, and spells used during the initiation. An elderly man who had been an *isingi akali* (guardian) during the initiation willingly obliged. An *isingi akali* is a man who has a special ritual status in Enga society. His role is to act as "guardian" of the sacred *lepe* plants, thought to bring health and well being to young men (Gibbs 1988). Because the *lepe* plants are ritually associated with a mythical "skywoman," the *isingi akali* had to remain celibate and could have no sexual contact with earthly women. If he wanted to take a wife he had to pass on his knowledge and his powers to a celibate successor before he could marry. My association with the former *isingi akali* at Kaiputesa helped open up to me a religious experience outside of the familiar traditional Christian beliefs and practices. I got a feel for the Enga worldview, which made sense in that bleak forest environment. Here, the academy was the forest. It was not a matter of "raw and cooked" but life and death. Who said that bush demons did not exist? I learned the *titi pingi* – the song recalling the heroic acts of men who had brought with them the sacred *lepe* plant. I was given a *lepe* plant, which I treated with great respect. The experience was both "off the verandah" and "out of the sacristy," leading to understanding through shared life experience.

At the end of that six months of total immersion, I felt as if I was standing in a doorway ready to step out into a whole new world, one I had previously been looking at through a window. The parishioners welcomed me back, obviously pleased with my attempts to speak their language. But it wasn't long before I encountered a bitter consequence of the immersion experience. On returning to mission life I felt alienated from most of my fellow missionaries. "Alien" would be a good term for how I felt at the diocesan meetings where expatriate priests would refer to "them"—meaning Enga people who I had begun to identify with at a "gut" feeling level.

The pastoral church work was rewarding. We tried all sorts of experiments in inculturation of worship and prayer. The people considered me some new form of *isingi akali*—a guardian set apart for dealing with sacred symbols. The gap between the indigenous Enga symbolism and imported traditional Christian symbolism was too great for most of my fellow expatriate missionaries, some of whom thought I was showing the first signs of having gone "troppo."

The experience provoked in me a change in focus from symbols to values. Symbols are fascinating, and at times have the power to influence our ideas and our behavior. I felt that values were a more fundamental element in culture, allowing me to see things as desirable or undesirable, thus enabling or restricting my choices in life. My value system had been altered by my experience living with the Enga family. Thus I found myself at odds with many missionary attitudes. I did not see the Christian message as necessarily promoting individuality, prosperity, or even moral progress. I was more interested in promoting human, and particularly communal, values in the face of modern secular influences. However, there were two significant areas where I felt I could not accept what I perceived as traditional Enga values. First, I was opposed to fear as a motivation for action, and second, I did not accept fully the traditional position of women in Enga society. I might intervene, for example, if I felt a young woman was being forced into a marriage against her will. In supporting young women rather than young men, my role as *isingi akali* was somewhat compromised. I can now see in hindsight that I was not cognizant of the contingent nature of culture—trying where possible to be a guardian of traditional values at a time when the society and its values were in a state of rapid change (Gibbs 2003). In romanticizing the past, I often failed to acknowledge that traditional, Christian, and secular value systems all impact on people's lives today.

Friend

The conversion in my approach to the culture and its values was reinforced by another incident near the end of my time as parish priest at Par. In 1986 I attended the twenty-fifth anniversary of the Catholic church in Porgera-Paiela parish of the same Wabag diocese. This was my first visit after the tearful farewell three years previously. While in Porgera I had grown close to a young girl called Sandewan. It was like a father-daughter relationship, which I treasured. During the celebration, Sandewan's mother presented me with Kiwi, a small fat pig. It was Sandewan's way of reciprocating (in Western terms one would say to "thank" me) for the ways I had helped her. I proudly took Kiwi back to Par where it soon became a pet, following me around wherever I went.

Shortly afterwards I went to Keman, an isolated station deep in the bush, for ten days of private prayer and study. It turned out to be more a time of soul-searching. Why had the gift of a pig affected me so much? Was I not past the stage of cultural romanticism? From a modern Western perspective a gift of a small pig is hardly significant. I began to realize that it was symbolic of a change in my own values. When given a choice I preferred the company

of these people rather than that of expatriate missionaries or others from America or Europe. The local people were symbolized by Sandewan and her pig. This Enga society, so foreign and strange initially, was now "home." What was the path of greater involvement? Should I take out PNG citizenship and commit myself irrevocably to stay? I wanted to stay, yet the leaders in my religious order were putting pressure on me to go for doctoral studies.

In Papua New Guinea, Enga people have a reputation as being irascible, violent, and "big-headed." I would not say that the image is false. However, in the field, after many years of sharing life experiences I have caught glimpses of another side. Despite experiencing the frustration of seeing a whole clan's property destroyed after a foolish drunken brawl involving only a few, of sharing the anger of extortion and threats, of feeling the pain of children dying, I found my impression of Enga culture changing from the stereotyped image of the violent, irascible Enga to one based on relationships with individuals and groups. Research and culture faded into the background as relationships came to the fore. Relationships, of course, take time, and have their ups and downs. It is rewarding when someone gives you a netbag or a pig, but what about when one's friend appears wanting urgent monetary assistance?

I was becoming more aware that culture, whether viewed as a "system" of beliefs, values, and behavior or simply as a people's way of life, was constructed and reconstructed through human action and interaction. I hadn't read anything about action theory then, but my thinking was moving in that direction. It is all very well to investigate structures and values and to try to find out what goes on inside people's heads, but in the end what emerges as important are people and their lives. In order to build on existing societal structures, we organized the parish into eighteen communities divided according to clan groupings. Thus my life revolved around the ever-changing relationships within and between these groups.

My religious superiors finally won out. At the end of 1988 I left for doctoral studies in Europe. It meant leaving behind people who were now close friends. The Enga have a term for friend, *mona singi*, "where your heart is." It might sound ridiculous, but as I prepared to leave I became acutely aware of my sense of being a *mona singi akali*, that my relationship with the people was the equivalent of a marriage, and that my going was not a divorce but a forced separation. I left my *mona* (heart) there and vowed to return.

Storyteller

While in Europe, I attended the funeral of Father John Schwab, one of the early missionaries to the Enga. In the early 1950s, only a few years after the area was derestricted and Europeans were allowed to enter, Schwab had

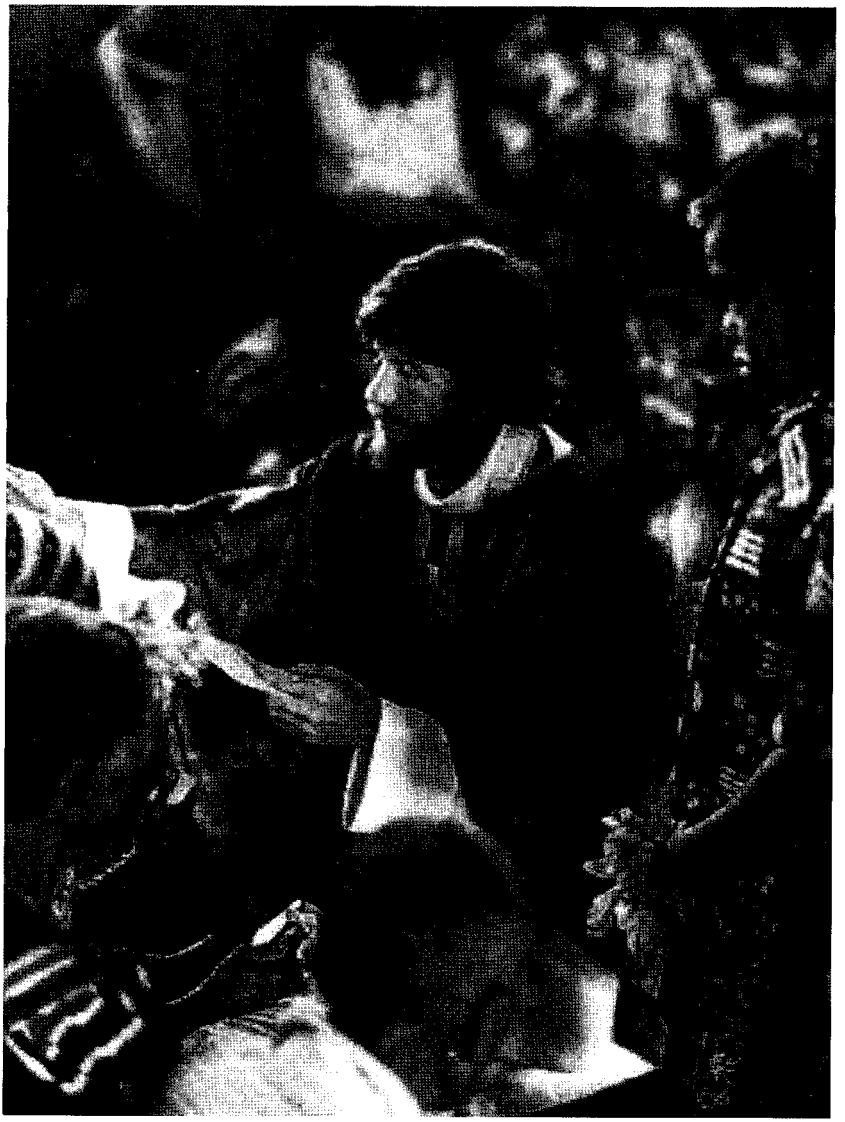

FIGURE 1. **Healing Service at Yampu, 1986.**

participated several times in the *sandalu* initiation rites. He had wanted to write up the experience but had not completed the work before his death. At his graveside in Austria I prayed aloud in the Enga language, promising my deceased confrere that I would finish the job.

I returned to Enga for a few months in 1993 and again in 1996. Work on Schwab's paper meant meeting the "boys" he had been initiated with forty-five years previously. It was fascinating trying to decipher tape recordings made four decades before and to have them interpreted by men who were now mature leaders in the community. Schwab was for me the epitome of the Malinowskian researcher. I was trying to take it a step further. I was not content to study the rites as historical events. With the initiation rites no longer practiced, and the original participants old and nearing the end of their days, I wanted to delve into how such rites could be reinterpreted so as to have meaning for young men today.

At one stage, in 1996, the Catholic people of Enga had carried what they called a *bokis kontrak* (Ark of the Covenant) around the province. The pilgrimage culminated in a gathering at Par, my former parish. I was asked to give a talk on the history of fifty years of missionary presence in Enga. Rather than present a regular speech, in a moment of folly I decided to try singing the history in the same form as the men had sung about the coming of the sacred *lepe* plant for their initiation. It seemed like anthropological heresy to tamper with such a sacred rite, but the more I thought about it the more the idea appealed. With the help of some men I prepared a fifteen-minute song, using the traditional tune and symbolic word-forms but in fact telling about the seed of God's Word being brought to Enga. The song took me several hours a day over a period of two months to prepare and learn by heart. On the day of the gathering more than 10,000 people converged on the old Par airstrip. When it came to my turn and it was announced that I would tell about the history of the mission I nearly passed out with anxiety. "I must be crazy to even contemplate singing something like this in the Enga language in front of 10,000 people. I'll probably make a total fool of myself!" In the end I took a deep breath and started, and a minute into the song there was an amazing sensation as 10,000 people breathed in together when I paused at the end of each line. The audience sound got louder as the song progressed, and I realized the people were with me. Afterwards some young people said that they were aware of that song but had never actually heard it. There were tears in the eyes of old people as they passed by. Looking back I see the anthropological heresy as a missiological triumph. Even now, nine years later, I am often introduced as a *tindi lenge*—"the one who chanted the *tindi pii*" (myth-story) at Par.

Healthy cultures change in order to live. In their book on traditional Enga culture, *Historical Vines*, Wiessner and Tumu argue convincingly how between 250 and 450 years ago the introduction of sweet potato precipitated rapid changes among the Enga people (1998). The authors describe in great detail how in precolonial times warfare, ceremonial exchange, and large ancestral cults were orchestrated by powerful big-men who were brilliant

orators, flamboyant performers, and skilled economists. These men may have lived with what is termed a stone-age technology in that they used stone tools, but the culture was not stonelike in the sense of being static or inanimate. On the contrary, the precolonial culture was dynamic, and it continues to be so today. Despite the changes, continuity is maintained through stories told and reinterpreted for changing circumstances. Mine was an attempt to continue that tradition while introducing the novelty of the Christian story.

Chuck Norris's Brother

My doctorate in theology from the Gregorian University focused on liberation theology in various parts of the so-called third world. I was fortunate to visit parts of Africa and have extended periods of fieldwork in Bolivia, Peru, and Sri Lanka. So when I returned to Papua New Guinea in 1996 I was more conscious of the effects of globalization and of political factors impinging on the lives of indigenous peoples. I was much more aware of the struggle of the haves and have-nots, and resolved to look at the effects of the hegemony of the missions in Papua New Guinea.

It is an unwritten law that one does not return to one's old parish, so as to allow the new parish priest the freedom to work in peace without people being reminded of the good old days. However, Par is the SVD center for Enga, and besides, my relationships with people there go far beyond those resulting from my being parish priest. In fact, if I should die prematurely in Papua New Guinea I have ask to be buried in the mission cemetery beside the church at Par.

Since 1996 I have been returning there for a week at a time several times a year. I find it a bittersweet experience. I witness what appears to me very patronizing views about the Enga people and their culture both by some missionaries and by some of the educated Enga elite. My offers to help in some ministerial capacity are sometimes ignored by both the missionary and national priests.

With such infrequent visits, my facility in the language is not what it used to be, and I don't know the young people as well as I would like. Recently, after teaching a class in the Par primary school, one student raised the question, "Are you really Chuck Norris's Brother?" I do bear some resemblance to the American actor, I suppose, but had never paid much attention to the young people calling out "Chuck Norris!" as I went past them on the motorcycle. I responded to the question saying that it was a secret that I wasn't free to reveal at the time. Having reconstructed their role models in terms of actors like Bruce Lee and Rambo, I wonder just who the young people think they are meeting on the road.

My being reconstructed as the brother of an American film star illustrates the younger generation's exposure to globalization and its effects. Divine Word missionaries used to have a maxim: "The world is our parish." But, as Michael Rynkiewich points out, it might be more correct to say: "The world is *in* our parish" (2002:303). In a country that produces excellent coffee we had got used to advertisements for imported Nescafe. Now the most common commercials on PNG television are for condoms: "*Sapos yu tingting long koap, tingim Karamap*" (If you're thinking about sex, think about Karamap condoms). Karamap condoms are imported, and at one kina (about thirty U.S. cents for a packet of three) young people are complaining that now they have to pay for sex!

Mentor

During recent visits I have been encouraged by many requests from Enga people for courses and workshops on Enga language and culture. I get more requests than I can ever hope to fulfill. As fundamentalist Christian influence has grown in recent years, many Enga Catholics are tempted to adopt a negative view of their culture, which amounts to rejecting everything traditional as evil and sinful. This is a common revival phenomenon documented by many researchers in Papua New Guinea (Robbins 1997; Schmid 1999).

Several young Engans and I have developed a weeklong workshop to try to counter this tendency. It has been conducted successfully with women in six parishes of the diocese and with men in two parishes. Groups of between 50 and 150 mature people gather in a predetermined place, and I come accompanied by a small team of three or four younger educated Engans. For the women, our basic text is Polly Wiessner and Alome Kyakas's excellent book, *From Inside the Women's House* (1992), supplemented by participants' own stories. Over the course of a week, for eight hours a day, the group reflects on topics like childhood, growing up, courtship, marriage, childbirth, sickness, domestic life, religious life, conflicts and reconciliation, aging, and death. For many, particularly the older people, it is an opportunity to relive and share experiences from early days.

The workshop is conducted in the local language throughout, and we use group work and drama, finishing with a special worship service, usually a Catholic mass, in which people celebrate their identities as Enga Christian men or women. For me, the evenings and nights are particularly valuable times to sit around the fire and share with the male participants, or to compare notes with the female assistants in the case of workshops for women.

I find that the workshops are appreciated by the local people, who gain new confidence and interest in their cultural life and its values. It is also a special

FIGURE 2. **On the road in Enga, 2000.**

opportunity for me to share with them and to document their experiences from childhood until death. I now have thousands of pages of transcribed taped interviews and over a thousand proverbs and sayings collected during such workshops. There is no law of diminishing returns here. The data from personal experiences are pouring in at an ever-increasing rate and my store of traditional wisdom (*mana pii*) is continually being enriched. At this stage we want to experiment with ways to share this wisdom and experience with young people.

Thirty years ago it was me looking for ways to ask people about their culture. Now I find Engas coming to me looking for answers to questions about traditional culture and how to discern what is of value for their lives today. The "feeling for" developed as a result of many powerful experiences must now be translated back into "thinking about" as I prepare the workshops. Seen as a *mana lenge* (teacher, mentor) I find it embarrassing because often people presume I know more than I do. However, it is also gratifying to have people putting that much trust in me in this new form of mission as dialogue. A cutting edge for me is how to engage in dialogue between the traditional Enga wisdom and the wisdom of the Christian tradition that I represent as a Christian priest and teacher. I feel that there are many points of contact if one treats humanity at its deeper levels. There are different forms of dialogue: intellectual, dialogue of life, and of common action. My aim is not so much

intellectual, but more in terms of a dialogue of common action encountered at the more profound levels of life and death.

Mediator

The 1990s have brought major changes to lives in Papua New Guinea. In the 1980s the currency (kina) was valued above the U.S. dollar. Now it is worth around thirty cents. Windfall incomes from mineral resources have mostly vanished in waste and corruption. The economy has stagnated. Less than 10 percent of the population is employed in the formal sector. Government health and education facilities have been drastically reduced in rural areas. The Bougainville crisis drained the country economically and morally. There have been several military revolts. There have also been natural disasters, including the volcanic eruption that destroyed Rabaul, the drought of 1997, and the tsunami in Aitape in 1998. Cases of HIV/AIDS are increasing at an alarming rate. Papua New Guinea ranks 133rd on the Human Development Index—one of the lowest rates in the Pacific region (UNDP 2002).

Economic and political instability have wrought a social toll. The retreat of government services has witnessed a corresponding increase in social disorder and lawlessness. Criminal activity previously confined mostly to the towns now reaches into all areas of society. If coffee growers find a road open to export their crop, they risk losing it to thieves while it is being transported to outlets in town. It is no secret that high-powered firearms and ammunition are coming into the country in large quantities, paid for by locally produced "New Guinea Gold" (marijuana).

How does one dialogue in a situation of anarchy where murder is commonplace? At a conference in Canberra, Australia, in 2002 I described electoral politics in the PNG Highlands as a quasi-religious cult in opposition to the state (Gibbs 2002). This political cult leads to the institution of the state becoming redundant and brings government and many church services to a standstill. Politics has the power to take over the minds and emotions of the people and to sustain that influence for long periods of time. In Enga Province, campaign houses around Wabag were commonly called "animal houses." Why? "Because animals don't think," I was told. "People can do anything they like in such houses and the normal rules do not apply. They don't think or act like people with intelligence."

In the face of power struggles, and individual self-interest the state appears powerless to control an increasingly volatile situation. As people become more educated and have improved access to modern means of communication, there is a greater demand for democratic processes that cater to individual freedom of choice. Should one be polite like Alphonse Gelu and call the roughhouse

politics of the emerging political culture, "non-liberal democracy" (2000), or like Standish, simply name it for what is: "Gun-point democracy" (1996)?

On polling day in 2002 at Par, people were shot or cut down during a fight over the ballot boxes. The researcher part of me wants to find out what happened and to write about it. The missionary wants to go and sit with people and comfort the bereaved. The motives are complementary. I went into the fight—with more caution than I used to twenty years before. Then it was bows and arrows. Now warriors use high-powered military firearms. They recognized me as a *kainakali* (mediator) and paused to allow me to go back and forth to speak with both sides in the conflict. However, once I had left the fighting resumed and continued for several more months, leaving sixteen people dead and many wounded.

Standing in the mission cemetery, which might one day be my last resting place, I glanced around to view the Par village burned and in ruins. For the moment the mission station remains, though scarred with broken windows and bullet holes, perhaps an appropriate symbol for my own feelings. The local Sambeoko clan is divided and dispersed. Intraclan warfare is notoriously hard to solve. The enemy knows one so intimately. They recognize your footprints. They know your favorite spots. Typically you might meet your end simply in going to the toilet. That is the modern situation in a political culture of rivalry and violence. That is the Enga culture that both researcher and missionary are dealing with today.

Grandfather

In the midst of the election-related violence I visited an Enga nurse who I had helped through nursing school. She now has six children and named the last boy after me. I was struck by the term she used to introduce me to the child—*kauwane* (grandfather). The term *kauwane* has the connotation of wisdom and respect. People will go to their *kauwane* for advice or consolation. What does it mean to have progressed from *wane* (boy) to *akali* (man) to *kauwane* (grandfather)? I am touched by the term of endearment, yet taken aback by the implication that the saga of my relationship with people in Enga is entering another chapter. That chapter has yet to come, but it is shaping up to be a challenging one.

Now is not the time to retire and dote over one's grandchildren. Nor is it an occasion to imagine that one can conduct apolitical research independent of the interests of different groups in society. There is an urgent task for people of integrity—Christian or otherwise—to enter into the "missionary" tasks of outreach to civil leadership and to engage in a prophetic role that offers alternatives to what, in some parts of Papua New Guinea, has become

a power-hungry, dysfunctional cult. This task is a far cry from the standard Malinowskian model that I began with. For example, today we need fieldwork that will provide an understanding of the youth culture so dominated by the effects of the daily use of home brew and marijuana.

A Common Task

Roger Keesing notes the contrasting stereotypes of the straight-laced, narrow-minded Bible thumping missionary with the bearded, degenerate anthropologist (1981:402). I agree with Keesing that "things are more complicated than that." Admittedly there are examples of indigenous people being exploited and subjugated, and being promised second-class citizenship in a white man's heaven. The postmodern critique of missions promotes the idea that missionaries have pushed Western customs onto indigenous cultures, thereby destroying them (Hiebert 1997). From my experience, though, I also see the converse, that is the danger of missionaries pushing for the inculturation of the church whether the indigenous people want it or not. Both approaches suffer from a similar flaw of presuming or allowing little initiative or choice on the part of the local people. There can be both fundamentalist missionaries and fundamentalist anthropologists—both behaving as though they had a monopoly on the truth. Rienkiewich notes (1980:174), "The key question is not whether options are good or bad, not whether change should or should not take place, but rather what kinds of power and influence missionaries have, now and in the past over the decision-making processes." The same question might be put to anthropological researchers because, consciously or not, the participant-observer always affects the people he or she is with.

At the same time, besides the natural processes of growth and maturity, my long-term contact with people in Papua New Guinea has surely left its mark on my beliefs, values, and behavior. Meggitt refers to the "ambiguities of advocacy" in his dealings with the Enga people (1991). I think of my thirty years of relating to Enga communities more in terms of changing perceptions on the part of the people whose lives I have shared and of my own perceptions of myself and the world around me. There are aspects of life in Enga that conflict with my values, such as tribal fighting and domestic violence. I still feel uncomfortable with what I see as brutality in bludgeoning pigs to death and the maltreatment of captured birds or animals.

In other respects, Enga people have influenced me in at least three important ways: spiritually, in ways of relating, and heightening my political awareness. I arrived thirty years ago brimming with individualistic piety. Over the years in Enga I have come to appreciate the importance of communal spirituality. Church community is not something sentimental but includes

tensions and even oppositions. I have come to see how people who call themselves Christian are called to live in communities in which their ordinary human relationships are healed and enriched by a common commitment to Christ and the gospel. I don't see any conflict in a person's being both a true Christian and a genuine Enga. Nor do I see a dilemma in my being a researcher and a priest. To me the roles are complementary, though as I have noted, not all my colleagues would agree.

Secondly, Engas have taught me the priority of relationships. Surely there are striking analogies between ethnographic fieldwork and the missionary enterprise. As Bronwen Douglas notes (2001:39), to be successful both pursuits require long-term intimacy with local people. At first I came with a romanticized view of village life, which did not last long and certainly changed radically during the time I spent living with a family while learning the language. Initially the differences between my attitudes and values were very obvious to the people and to myself. However, gradually as I have become *ae masepae palenge* (literally: local thinking sleeps), differences seem trivial compared to perceived similarities and shared concerns. The main concern that I share with Enga people today is how to move from fear and death to freedom and life.

Thirdly, I think that the Enga community has helped me develop a heightened political awareness. A researcher cannot ignore the fact that Enga is now divided between what the people call the *nenge* and the *nanenge*—literally: the eaters and the non-eaters (the haves and have-nots) (Lakane and Gibbs 2003). I think that the solution to an abused system is not necessarily another one but rather the transformation of the present system in a way to ensure freedom and dignity for a majority of the frustrated and disenfranchised people. Surely research can in some way provide insights that will help bring relief to the disempowered casualties of the present political culture. Are there communal models of shared responsibility that can be salvaged from traditional Melanesian values? Can we identify agents who can deal in a constructive way with the unintended consequences of globalization? Are there ways to relate to the younger generation that will enable them to create a meaningful future out of their present alienation? Enga people must be free to choose their own future. The magnitude of the challenges ahead will surely benefit from a multidisciplinary approach, and that includes both missionary zeal and academic insight.

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FILMS AND OTHER TRIALS: REFLECTIONS ON LONG-TERM FIELDWORK AMONG THE MAISIN, PAPUA NEW GUINEA

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This essay is a personal reflection on my developing relationship with the Maisin of Papua New Guinea from December 1981 to the present. I discuss my initial research focus and how my early fieldwork helped me to better understand the ways the Maisin have responded to changes in their community over the past twenty years, particularly in dealing with outsiders. I contrast my understandings of Maisin society with portrayals in two documentary films made in the community, *Anthropology on Trial* (1983) and *Changing Ground* (2001). I argue that the relationship between anthropologists and host communities fuses intellectual engagement with a powerful ethical commitment, both of which strengthen with time and experience.

When I decided to do ethnographic fieldwork in Papua New Guinea, I hoped that it would be the beginning of a lifelong relationship. Most of my mentors and many of the anthropologists whose work had absorbed me over the years had made returns to their field sites, sometimes repeatedly. Although I could not know whether I would also have that opportunity, the possibility shaped the way I thought about my fieldwork. I tried to undertake as comprehensive a study as I could manage, while keenly aware that I could have only a snapshot impression of the Maisin people at a particular historical time. To achieve a deeper understanding and a closer relationship—something I wanted very much—I would need to somehow maintain contact despite the distances.

Two decades and five visits later, I now find myself in the fortunate position of those anthropologists I so admired when I started out. Given how common returns really are, I find it curious that few scholars have written about them,

Pacific Studies, Vol. 27, Nos. 3/4—Sept./Dec. 2004

especially in Melanesia. The subject has not been entirely neglected, but it is interesting that the four most prominent accounts, including Bruce Knauft's recent *Exchanging the Past* (2002), all marry a personal narrative of return to a remote locale with a vibrant description of its utter transformation in the face of advancing "modernity" (Mead 1966; Read 1986; Tuzin 1997). I do not question either the accuracy or the value of these accounts. They are powerful and important contributions. However, they do not describe *my* experience or that of most anthropologists I know. The Maisin, like most indigenous groups in Melanesia and elsewhere, have been dealing with "outside" agents and forces for rather a long time. While the changes that have come to their communities are profound, they have had time to adjust and adapt over the course of a century of interactions. I have not myself witnessed an abrupt transition of a "traditional" society into "modernity." At the same time, the circumstances of my employment as a tenured academic and the circumstances of Maisin lives, which I describe below, have made revisiting the community relatively easy for me and created opportunities for some Maisin to visit me at my home and for others to keep in touch by letters and e-mail. My perceptions of the Maisin as a culture with a distinct location, in the usual anthropological sense, have been powerfully reshaped by my lengthening experience with an increasingly far-flung network of which I and several others who are not ethnically Maisin are active members.

This article presents a narrative of my interactions over the past 25 years with the Maisin. I make some observations on how the community has changed and discuss my evolving and intensifying involvement in Maisin social networks, at the village level and beyond. Extended observation over time and diversifying sorts of interactions with members of the community have worked to greatly complicate my understanding of the Maisin. My experiences have been unique in detail, but I strongly suspect they reflect the experiences of many other anthropologists working in Melanesia and elsewhere. I hope that this account will encourage others to write about their experiences of long-term fieldwork while providing beginning ethnographers with an appreciation of the value of planning for a sustained commitment to a community.

Why is it that so few anthropologists who have sustained relations with a particular community over time have written about their experiences? Anthropologists have hardly been shy, often writing extremely intimate accounts of fieldwork (DeVita 1990). Indeed, autobiographical ethnographies form an important subgenre in anthropological literature. Such accounts tend to dramatize the adventure of doing ethnographic research in exotic locations that are often presented as perched on the very edge of momentous change. In other words, they conform to the conventions of prevailing metanarratives

of modernity. Accounts of sustained interactions with indigenous groups are necessarily more complicated and messy and hence perhaps less satisfying to write or read about than tales structured around or implying scenarios in which pristine indigenous societies are threatened by "Western" forces, whether they appear in the guise of agents like missionaries or the juggernaut of "globalization."

Anthropologists have been among the vocal critics of the long-standing tendency of many observers to reduce the historical experiences of indigenous societies to simplistic oppositions, whether as in economic programs meant to "modernize traditional" societies, in writings that suppose a vast gulf between Orient and Occident, or in the politics of denying indigenous First Peoples inherent rights to ancestral territories because they now live "modern" lifestyles. We need to make a more concerted effort to carry such sensitivities into our stories about fieldwork. The contributions to this special issue all attempt to do this. This article adds an additional wrinkle by contrasting my experiences and developing understandings of the Maisin with those of two filmmakers who produced documentaries on the community. The contrast reveals the intellectual value of sustained ethnographic research as well as its ethical basis. It also suggests, however, that finding compelling stories that challenge the stereotypes perpetuated in popular writings and films concerning indigenous peoples will likely not come about merely or even mostly through ethnographic research. The value of long-term fieldwork lies elsewhere.

Arrivals and Departures: The Initial Fieldwork

I first developed an interest in the South Pacific as an undergraduate at the University of Western Ontario in the late 1970s. Determined to work on a graduate research project with practical relevance, I first thought to study the impact of advertising campaigns by multinational corporations on island societies, particularly on their sense of identity. After arriving in Wellington, New Zealand, in 1978 to begin study for a master's degree under Ann Chowning, I quickly found this idea unworkable. In those pre-Internet days, the necessary information just wasn't readily available. While searching the collection of the wonderful Alexander Turnbull Library and finding scant information on advertising, however, I did come across fascinating early missionary accounts of exploration. I ended up writing a thesis on the social history of three missions to Papua from their arrivals, beginning in 1871, to the point where the paper record ended in the early 1930s (Barker 1979). I was surprised to learn that the vast majority of Papua New Guineans were now members of Christian churches. My reading of ethnographies had led

me to believe that the missions had made little inroad into "traditional" belief systems (Barker 1992). Intrigued, I decided that I wanted to study the contemporary religion of Melanesians who were at least second-generation Christians. I moved on to the University of British Columbia to work on my Ph.D. under the supervision of Kenelm Burridge. I had already decided to work in an Anglican area along the north coast of either Oro or Milne Bay provinces. The decision to work in Uiaku village came about mostly because it was conveniently close to an airstrip, had been recommended by the Anglican archbishop of Papua New Guinea, and had a large population. The size factor was especially important to my wife, Anne Marie Tietjen, who was pursuing a separate research project on the development of prosocial reasoning among Melanesian children (Tietjen 1986).

We knew little about the Maisin prior to our arrival. I had written to the local priest, a Papua New Guinean, but had received no reply. Anne and I thus initiated our projects without any kind of local consultation. We could only hope that villagers would be interested and supportive. As it turned out, people were remarkably receptive and patient with us, even if most had difficulty understanding why we had come. I had hoped from the onset that our relationship with the Maisin would last a lifetime. Five trips later, extending over two decades and twenty-seven months in the villages and a number of visits from Maisin visiting North America, my connections to the community have intensified and deepened. I feel tremendously privileged.

Anthropologists love to relate personal tales from the field, and I'm no exception. However, for the purposes of this article I wish to focus primarily on the evolution of my research interests and perceptions of Maisin society from my initial extended fieldwork in the early 1980s to shorter visits in the late 1990s and 2000. This will make the comparison with the ways Maisin have been portrayed by documentary filmmakers clearer. First, however, I need to provide some background on the people.

The Maisin are a small sociolinguistic group, numbering around three thousand people in all. Most live in a series of beach villages along the southern coast of Collingwood Bay in Oro Province. Maisin villages have a very "traditional" appearance. Outrigger canoes of various sizes line the beaches; attractive thatched houses front packed-earth plazas, shaded by coconut groves and fruit trees. Like their ancestors, villagers today survive mostly through swidden horticulture, hunting and gathering from the surrounding bush and forests, and fishing. Cash cropping is limited by the lack of shipping along the northeastern coast, but the Maisin have long earned a small income from sales of their beautifully designed tapa cloth (pounded bark cloth). Despite appearances, local people have become increasingly depen-

dent on cash and commodities. These are supplied mostly through remittances sent home by the quarter or so of the population that has migrated to distant urban areas since the early 1960s.

From the start, my perspective on the Maisin was resolutely historical. Having already written a thesis in which I examined the history of early missions in Papua, and having consulted archives in Australia and Port Moresby before arriving in the field, I came prepared, intellectually at least, to see contemporary Maisin society as the outcome of almost a century of interaction with European outsiders. I was interested in examining the obvious imports—the village schools, churches, trade stores, and so forth. But I was also keenly aware that indigenous institutions would bear the marks of colonial interventions, no matter how "traditional" they might appear. In a patrol report from the early 1930s, for instance, I had discovered that the Maisin at that time were radically changing their house styles to conform to a standard type that the colonial administration considered to be healthier. While village houses would be made of bush materials and look traditional, I already knew that they had been modified. And from studies I had read from other coastal areas, I expected that the same would be true of nonmaterial aspects of life like kinship and sorcery beliefs (Abbi 1975; Zelenietz and Lindenbaum 1981). I was determined not to represent the Maisin as a "traditional" society that had somehow held off change.

If one were to take a prominent version of the current folk history of anthropology seriously, I suppose I could claim I was doing something quite radical. Alas, this would be misleading. Anthropologists did not suddenly awaken to the reality of history with the publication in the early 1980s of seminal volumes on *kastom* and the "invention of tradition" (Hobsbawm and Ranger 1983; Keesing and Tonkinson 1982). I was fortunate enough to study under two anthropologists—Ann Chowning and Kenelm Burridge—much attuned to the formative influences of colonial agencies on indigenous societies, but they were hardly the only ones at the time. Studying historical developments in economic and religious activities, especially cargo cults, was already a well-established tradition in Melanesian anthropology. By the late 1970s and early 1980s there was a growing consensus to extend historical perspectives to the study of other facets of Melanesian culture, evident in the appearance of many such studies in the following years. I encountered little surprise, let alone resistance, when I came up with the topic of Melanesian Christianity. In fact, most anthropologists I encountered, while usually showing minimal interest in the subject, declared that it was about time someone tackled it!

All the same, the trend towards a historically informed anthropology is much clearer to see in retrospect. For all of the encouragement I received,

FIGURE 1. **Anne Marie Tietjen and John Barker with their "fathers," Claude Daima and Adelbert Sevaru, Uiaku, July 1983.** (Photo by A.M. Tietjen)

I worried that I had made a huge mistake by not studying a more traditional topic in a more remote community. Like Juliana Flinn (writing elsewhere in this special issue)—and I imagine most graduate students doing their first fieldwork—I agonized over my own competency and whether anyone would be interested in the slightest in what I had to report about the Maisin. I kept to my original research project, but I confess that I did allow my anxieties to dictate several decisions. Like Flinn, I spent much of my effort conducting highly organized research through interviews, surveys, and questionnaires. I spent a great deal of time passively recording oral narratives that I later translated with the help of assistants. I did not "hang out" much and I did not get out of the villages to the gardens and the forest nearly enough. I managed to amass a huge amount of data and make some close friends, but often did not feel comfortable in the community.

After I returned home and systematically reviewed my notes, I noticed that my concern that neither the Maisin nor my topic was "traditional enough" had shaped my inquiry in subtle but important ways. Although my research focused on local Christianity, I actually attended more to indigenous-

appearing religious beliefs and practices than those that struck me as Western imports. I had dutifully attended church services, recorded prayers and sermons, and taken copious notes on the minutiae of village-style worship. I had gone to parish meetings and conducted semiformal interviews about the church and Christianity with a wide range of people. Yet my extensive notes on Christianity were dwarfed by those detailing sorcery beliefs and accusations, magic, and the ubiquitous presence of spirits. This is not to say that Maisin themselves drew a sharp distinction between indigenous and Christian beliefs. Clearly, though, I was drawn more to the exotic aspects of their religious lives than to familiar ones, and thus my appreciation for the latter was less developed than it might have been.

While I was trying with mixed success to deal with a topic then neglected by most anthropologists, I nevertheless enthusiastically embraced a classic approach to research little different from that advocated by Malinowski (1922). My techniques were very low tech, with the bulk of information going into handwritten notebooks and journals. I tried to be as comprehensive as my skills, tools, and imagination would allow, both in methodology and subject matter. Along with recording oral traditions, attending endless village meetings, sharing food at funerals and so forth, I measured gardens, conducted two censuses of the entire village and a neighboring one for comparative purposes, surveyed household economies, and polled samples of villagers on a variety of subjects. I take pleasure here in crediting Ken Burridge for one of my most useful tools. While I was working on my fieldwork proposal, he insisted that I write up a short appendix of "protocols"—sets of research questions and procedures to guide my fieldwork. At first I considered this a nuisance but then, after consulting *Notes and Queries on Anthropology* (Royal Anthropological Institute 1951) as well as Murdock's *Outline of Cultural Materials* (1975), I got into the swing of it, quickly producing a sixty-page-long list, which I carried with me to Uiaku. Whenever I found myself stranded for something to do, I consulted my "protocols." As a result my files filled up on topics that I'm sure I would not have otherwise addressed.

Despite this industry, I was uncertain whether I understood much of anything, and worried that most of the information I was collecting was of little interest or use for the Maisin or anyone else. I was also often very lonely. Anne was back in Canada from July 1982 to May 1983. I made a point of socializing during down times, but most evenings, after finishing my notes, curling up under the mosquito net with a cheap mystery proved far more inviting. Fieldwork sometimes felt like a trial. Ironically enough, in early 1983 an American filmmaker appeared in Uiaku village hoping to use it as a backdrop for a documentary entitled *Anthropology on Trial.*

Anthropology on Trial

The initial omens were not good. Barbara Gullahorn-Holecek arrived on the beach one afternoon unannounced, having caught a dinghy from the regional airstrip, located about twelve miles up the coast from Uiaku. Once villagers determined that she had not come to purchase tapa cloth, they brought her to my house. She was clearly discomforted by this, asking only if I could direct her to a certain village man. He happened to be attending a soccer match up the coast and I arranged for a dinghy to take her there. Gullahorn-Holecek returned disheveled and exhausted the next day, after a sleepless night without a mosquito net. I put her up in my house and, after she had a rest and some food, arranged for her to meet with village leaders on her own. The following day she described the film project to me in some detail, explaining that she wanted to document Papua New Guinean criticisms concerning anthropologists. I was astonished to learn that an American couple living in Port Moresby, whom I had hosted in the village over a weekend several months earlier, had described me to her as a Kurtz-like figure, living a high life at the expense of the villagers. An exposé would have made a great segment for her film, but fortunately for me the Maisin she spoke to were mostly content with my presence and made it clear they would welcome any other Europeans who might want to settle amongst them. After Gullahorn-Holecek dropped her bombshell, she asked if I would have any objections to being in the film. I told her that it was up to the Maisin, knowing full well that the village leaders would be delighted. And so they were. Village leaders announced that the film would show Americans just how poor the Maisin were. Given the fond memories elders had of American soldiers they had met during the Second World War, people were hopeful that the viewers of the film would take pity and send "development." About six weeks later, a small film crew arrived with an impressive load of equipment. They shot their film in a bit less than three days, packed up, and left.

The following November, about five months after returning to Canada, I nervously sat down with friends to watch the premier of *Anthropology on Trial* on the PBS television network in the United States. The filmmakers had interviewed me on camera for about two hours and I was keenly aware of how easily they could edit the footage to produce whatever image they wanted of me and my work. As it was, I more or less came out on the side of the angels. I cringed at the sight and sound of myself, but mostly felt relief. And then I started thinking about the way the filmmakers had constructed Maisin life and my research. I agreed with the general message of the film but was struck by inaccuracies and exaggerations in my segment and elsewhere.

The film pictures the Maisin as a traditional people perched on the edge of modernity. The segment opens with the camera following me as I walk

through the village. As I pass the community school, the camera pulls away and enters a classroom where a teacher is giving a lesson in geometrical forms. The narrator tells viewers that with the arrival of the school, children are no longer learning the old traditions from their elders. Instead, I have become the last student of Maisin culture, hanging out with the old people and learning their stories. The work is not without its difficulties. The most important of the Maisin stories concern their history. While "we" possess one history, the narrator somberly intones, each Maisin clan jealously possesses its own version of the people's origins. Maisin appreciate what I'm doing, but they (and I) are also concerned that I have stirred things up by forcing the issue of who is telling the truth.

It was a nice clean storyline that bore a dim resemblance to reality. Many Maisin were concerned that they were forgetting their traditions; I did spend a fair amount of my time recording narratives; and my study of clans histories had caused some friction as different big men tried to get me to endorse their versions (and associated land claims) and reject those offered by rivals. The claim that the younger generation was no longer listening to the elders following the recent arrival of the school, on the other hand, was a stretch. A school and church had operated continuously in Uiaku since 1902. It was hardly new. Children in the 1980s certainly spent more years in school than their elders and had other distractions, but the young adults I knew were clearly interested in Maisin customs and very concerned that too much customary knowledge was being forgotten.

Given the subject of the documentary, however, the most serious distortion concerned my motivation for fieldwork. I had not come to record Maisin traditions, but to study Christianity. I studied the clan histories at the insistence of Maisin elders who were eager to bolster their political claims by having the stories written down and, they hoped, validated. While I certainly did not resist, I was always conscious that the clan histories were a side issue to my own research project about which, sad to say, people were far less interested. The film, however, depicted me as one of a new generation of young anthropologists, culturally sensitive and aware that the elders are the true teachers (a theme carried even further in a later segment focused upon Andrew Strathern and the Melpa bigman, Ongka). In contrast, the film cruelly misrepresents Margaret Mead as arrogantly ignoring cultural restrictions and promoting insulting stereotypes in her work on Manus Islanders. Conveniently dead and unable to defend herself, Mead got a bum rap while I escaped censure.

For all its heavy-handedness, *Anthropology on Trial* raises important questions about anthropologists' responsibilities to the people they study and write about. It is historically significant in showing visually that "citizens and

natives in Papua New Guinea ... and across the Pacific are reading the work of anthropologists and demanding a political accountability that was difficult to imagine a generation or so ago" (Rohatynskyj and Jaarsma 2000:1). This is a valuable service that the film performs well. Watching it as one of the subjects, however, I find a second lesson—a confirmation of the special nature of ethnography as opposed to alternative strategies of research and representation. From the perspective of a working anthropologist, Gullahorn-Holecek's approach to her subject was badly flawed. She assumed that the truth of the complex relationship between ethnographic fieldworkers and their hosts lay close to the surface and could be accessed by making brief visits to various fieldsites and conducting short interviews. Lacking much knowledge of either Papua New Guinea or anthropologists, the resulting film projects commonsense assumptions about its subjects that, at best are superficial and, at worst, flat out wrong. The documentary thus anachronistically represents anthropologists as students solely of indigenous traditions who have, at least in the past, engaged in a kind of theft by taking away records of those traditions and giving nothing in return. Good anthropologists, the film seems to suggest, should be scribes (like me), recording the words of our informant-teachers without inserting our own interpretations or opinions and leaving full copies of our fieldnotes behind us. Even better, they should be like Strathern who has, the film implies, settled down permanently with his Melpa hosts and pretty much gone native. The complaints of the Papua New Guineans who appear in the film are handled better, but here too the film resolutely sticks to the surface of things. Ignorant of the centrality of reciprocity in Melanesian moral reasoning, the film reduces the criticisms to a matter of simple fairness, of tit for tat. If anthropologists take away information, they should give something back; if North American anthropologists get to study Papua New Guineans, Papua New Guinean anthropologists should get to study North Americans. Problem solved. As an ethnographer, my criticism of the film is not that it is raises awkward questions about my profession but that it doesn't take those questions nearly seriously enough.

I returned to Uiaku for two months of fieldwork in late 1986. In most respects, this felt like an extension of my first stint of fieldwork. I had not been away long and changes in the village were few. However, this time I lived with a family instead of in my own house, which had been torn down after we departed in July 1983. I was more focused this visit, concentrating on a study of tapa cloth, but I also felt more relaxed and comfortable. There was, however, one dark cloud over this period. My first application for a research visa had been refused. I was informed by the Institute of Papua New Guinea Studies that the Maisin had held a meeting and decided against allowing me to return because, among other things, I had made a huge amount of

money by writing books about them. Fortunately for me, the Oro Province administrator overturned the ban, as he was delighted to have someone study the economic potential of tapa cloth . When I arrived in Uiaku, I found no one who knew anything about the supposed meeting. People seemed genuinely pleased that I had returned. About a month after I arrived, a man whom I suspected had started the rumor visited me late one night to admit to it and to apologize. He told me that he remained friends of the American couple in Port Moresby who had identified me as the "evil" anthropologist three years earlier. He had created the story of the meeting and passed it on to them as well as to a provincial politician I did not know; they did the rest. I never did uncover the details of this petty intrigue—the others involved did not answer my letters—but the apology lifted a considerable weight from my shoulders. I did rather feel I had been put on trial and was now acquitted.

Returns: New Missions for Old

I did not get back to Uiaku for another ten years. I had a new job and a child, which limited my ability to be away from home for long. I developed a new research project to study Christianity in First Nations (indigenous) communities in coastal British Columbia and carried out limited fieldwork on the subject among the Nisga'a First Nation near the Alaska border (Barker 1998). The Maisin seemed a long way away until events conspired in the mid-1990s that brought my old fieldsite much closer to home. Late in 1994 I received a letter from Franklin Seri, the village councilor for Uiaku, letting me know that he would be attending an exhibition of Maisin tapa cloth at the Berkeley Art Museum the following April. He wondered if Berkeley was close to my home in Bellingham, Washington, as he wanted to visit. Larry Rinder, a curator at the museum, and Lafcadio Cortesi, a Greenpeace activist who lived in Berkeley, were organizing the event. I promptly got in touch with both. My initial phone conversations were rather eerie as both Rinder and Cortesi had heard stories about me and Anne from the Maisin, but I knew nothing of them. The following spring, we joined the four Maisin men who had been brought over to open the Berkeley exhibit, "Jumping Lines: Maisin Art and Rainforest Conservation." Seri then came north to Bellingham where, among other things, we compiled a collection of stories in Maisin and English translation that I had recorded during the 1980s for use in the community schools. I began to plot my return to Papua New Guinea in earnest.

Two years later I landed once again on the beach at Uiaku for a stay of six weeks. The Maisin villages looked much the same and it was wonderful to see old friends and acquaintances. The people's circumstances within the larger

regional context had changed profoundly, however, and I soon became aware of transformations in the society. For one, there were many more people than before, especially young people. Up to the early 1980s, almost all Maisin youths had left the villages to attend high schools in larger towns and most went on to jobs in various parts of the country. Two decades of out-migration had left few younger adults remaining in the villages at that time. By 1997, however, only a third or so of community-school graduates were gaining acceptance into high school and many of those who graduated returned after failing to find jobs. In addition, members of the first generation of Maisin migrants were now returning to the villages with their families to enjoy their retirement. Uiaku in particular looked wealthier. People had newer clothes, more motorized dinghies, and generally a greater abundance of commodities than ever before. The custom of eating food by hand off of banana leaves during feasts had lapsed; everyone now used plates and cutlery. Many of those returning to the villages had spent a good part of their lives in relatively well-paid professional positions in the government, spoke good English, and brought with them a somewhat more upscale lifestyle. Several had abandoned Anglicanism for the Seventh-day Adventist Church and a Pentecostal sect.

Uiaku was thus more diversified and cosmopolitan than a decade earlier. It had also become better known to the outside world. During the six weeks I spent in the area in 1997, I was astonished by the constant comings and goings of representatives of a variety of environmental nongovernmental organizations (NGOs) who were carrying out projects in partnership with the Maisin. Their presence had come in response to a shift in Maisin thinking about the rain forest lying inland from the villages. In the early 1980s, local leaders had eagerly sought out logging companies, thinking that this represented the only option for bringing "development" to their communities. Ten years later, they had come to the opposite conclusion. One of the several causes precipitating the change was a growing awareness that villagers would receive few benefits from industrial logging and could well lose control of their land. This suspicion seem confirmed when, in 1994, word leaked of a secret deal between politicians and a group of Collingwood Bay migrants living in town to allow a logging concession. Local landowners were enraged and, with the help of relatives and environmentalists in Port Moresby, managed to quash the project, declaring in prominent newspaper advertisements that Collingwood Bay was "not for sale." The Maisin rejection of a logging project before it started was unusual for Papua New Guinea. The more common pattern is for a community to demand compensation after a project has begun (Filer 1998a). Their stance attracted immediate and widespread attention from conservation organizations, which were expanding rapidly throughout the country at the time.

By 1997, with support from the World Wildlife Fund and other international bodies, the Maisin had become the single focus of a new national NGO, Conservation Melanesia (CM). CM staff undertook surveys of the lands, flora, and fauna in preparation for the eventual declaration of a conservation area. CM also coordinated workshops given by affiliated NGOs on landowners' rights, insect farming, and leadership. Meanwhile, Lafacadio Cortesi was using the Greenpeace network to develop an international market for Maisin tapa cloth—an initiative that led, among other things, to exhibitions by the Berkeley Art Museum in 1995 and the Fabric Workshop of Philadelphia in 1998, as well as the placement of a Peace Corps couple in Uiaku to help with the local organization of tapa production and sales. As word spread about the hospitality of the Maisin and their embrace of the conservation ethic, more environmentalists came to see for themselves. Meanwhile, accomplishing a long-held dream, church leaders managed to convince the Summer Institute of Linguistics to place two European volunteers in Uiaku to begin the fifteen-year task of translating the Bible into Maisin. In response to these developments the leaders from the various Maisin villages gathered to form a new political entity, MICAD (for "Maisin Integrated Conservation and Development"), to represent their collective rights over the land, to promote locally-controlled development, and to coordinate projects with their new external partners.

I had come this time specifically to study the impact of the environmental activists on Maisin society. My previous work on the community provided me with a rough baseline to assess change. The degree of change was indeed impressive; but my earlier fieldwork also allowed me to identify key points of cultural continuity. In the early 1980s, the Maisin villages had been fiercely egalitarian in outlook. This was reflected in innumerable ways: a stress on sharing food and labor, the etiquette of not standing on a canoe's gunwales when passing a village so as not to be above people on shore, notions of sorcery as retaliation for non-reciprocal and selfish acts, and so forth. A decade later, the villagers seemed to me less fearful of sorcery and less involved in exchanges. Still, the cultural stress on egalitarianism played itself out to the full in the never-ceasing squabbling over the leadership and decisions of MICAD. I was impressed by eloquent statements praising the values of conservation and stewardship of resources, especially from younger people and women. Yet it was also abundantly clear the rain forest campaign was motivated by suspicion that commercial logging would benefit a few at the expense of the many. It was a view that I happened to agree with, but I could also appreciate that it resonated with deeply held moral values. The same values that brought Maisin together to keep outsiders away from their resources led villagers to suspect their own leaders of lining their pockets through MICAD activities

and associations with environmentalists. And such suspicions, in turn, seriously challenged unity and efforts to develop economic alternatives.

My earlier research also illuminated my understanding of the environmentalists and other outsiders now working with the Maisin. While most of them seemed to think of the Maisin as living in a more or less pristine "traditional" society, I was keenly conscious that the activists were only the latest in a long series of outsiders who had interacted with the Maisin over the course of a century. During the colonial period, the Maisin had learned to distinguish between government officers, who came to give orders and take censuses, and missionaries, who brought valued spiritual and practical knowledge (through the schools). By 1982, Papua New Guinea had been independent for seven years and the Anglican Church was almost entirely localized. The priests serving the Maisin villages and their bishop had long been Papua New Guineans. Still, the categories persisted. Maisin categorized their own leaders and activities in terms of "government," "mission," and "village." And they applied these labels to visitors. Anne and I had been viewed by many, for instance, as a rather peculiar and perhaps disappointing species of missionary. As I watched the Maisin interact with the newest outsiders and listened to the chatter about them, it dawned on me that they were placing the newcomers into the broad "missionary" category. That is to say, they saw the activists as people deserving respect and support primarily because they had come selflessly to "help the people" by bringing them knowledge of the outside world and connections to its powerful forces.

I did not like being likened to missionaries very much in the 1980s and I expect the activists would like it less. None of us came to Uiaku to change the religion of the people. Indeed, we all in our different ways praise "traditional" Maisin ways and seek to support their survival. All the same, if Maisin perceive missionaries, anthropologists, and environmentalists as belonging to a single class of people, the anthropologist in me wants to understand exactly what *they* understand the nature of that class to be. The answer turns out to be quite complex, turning on indigenous notions of reciprocal morality and long-term experience of various types of outsiders, both foreign and Papua New Guinean. Much of my earlier work had been taken up with the question of how the Maisin perceived and interacted with missionaries and how this, in turn, had reshaped their society. This was necessarily a historical question, for missionaries in the conventional sense had long departed the area by the time I arrived. My knowledge of the historical encounter between the Maisin and the colonial agencies of church and state, however, provided me with a framework for understanding the people's present interactions with environmentalists. And, in turn, the present-day encounters opened up new insights on the past.

My most recent writings on the Maisin have focused on their interactions with the new secular missionaries of environmentalism (Barker 2003, 2007, n.d.). Without a close familiarity gained from my earlier fieldwork, I doubt I would never have appreciated the degree to which Maisin history and culture have informed the people's reception of the newcomers. Above and beyond this important point, I think the Maisin may be onto something. There are uncanny if ironic resonances in the motivations, perceptions, and unintended consequences of the Anglican missionary interventions of the past and those of environmentalists in the present.

Critics tend not to pay much attention to the ways that missionaries often conflate their causes with the people they come to convert. They instead look at missionaries as intruders, ethnocentrically imposing their notions of Western morality and religious beliefs on non-Western "others." The Maisin have had little experience of this kind of missionary. The Anglican missionaries of old were led by university-educated High Churchmen, who rejected what they saw as the corruption of modernism while embracing the simplicity of traditional village life (Wetherell 1977). They wanted to create in Papua New Guinea a vibrant indigenous Christianity that preserved and protected village ways from exploitation by outsiders seeking a fast buck. When shorn of its Christian trappings, the Anglicans' romantic rhetoric celebrating the communal values of village societies resembles that uttered by secular activists today. Activists now, like the missionaries before them, come with knowledge of global truths they presume harmonize and strengthened the best features of native culture. They hope to offer peoples like the Maisin tools and advice to improve their lives while respecting and strengthening their distinctive traditions. Echoing another older missionary theme, activists perceive themselves as invited guests working to protect vulnerable indigenous groups against exploitation by outside profiteers while shielding them from the globalizing culture of mass consumerism or large-scale logging and mining interests.

There are further parallels. While white missionaries are commonly credited or blamed for the spread of Christianity, most of the actual work of proselytizing in the indigenous world was and continues to be done by local evangelists. For all of their sympathy for village societies, most Anglican missionaries had limited direct experience and a very superficial understanding of the lives of ordinary Papuans. The Maisin experience was fairly typical. Except for a brief period around 1920 when a white priest resided in Uiaku, most people learned about Christianity from Melanesian teacher-evangelists (Barker 2005). Activist understandings of the Maisin today also tend to be superficial, based mostly on brief visits, a smattering of knowledge picked up in conversations with individual villagers, and general

preconceptions about Melanesian culture shared in the NGO networks. They visit to give workshops and encouragement, but it is middle-level organizations like MICAD and Conservation Melanesia, and not least local Maisin themselves, who act on or ignore initiatives.

It is hard to imagine how things could be otherwise. The environmental activists who have worked with the Maisin are by and large selfless individuals who give expert advice on protecting the environment and on small-scale local economic development. They are not there to study the culture and, besides, it is arguable whether they need to. The Maisin must be the ones to make decisions about their own lives, and that includes what help to accept from outsiders. I like and admire many of the activists I have met. All the same, I have felt jarred at times by the tendency of some to make definitive statements about the Maisin combined with a general lack of curiosity about the actualities of people's lives. I have heard much naïve talk, for instance, of "chiefs," of the people's "spiritual connection" to the land, women's "servility," and so forth. Although NGOs are far from wealthy, their representatives bring money and goods in the villages and offer coveted opportunities for trips overseas. With little knowledge of the local community, activists rely heavily on those Maisin who speak the best English and have had the greatest experience living in the towns (and thus away from the villages). Other villagers resent what they perceive as favoritism. Activists are generally not aware of how great this resentment is until it bursts into the open in arguments, accusations, and, sometimes, withdrawal from projects. Squabbles over the spoils of NGO visits and interventions can be quite disillusioning to those who have constructed fantasies of brave tribal peoples fending off the juggernaut of globalization.

If ignorance in such cases does not lead to bliss, neither is it folly. A positive conception of the Maisin, even if naïve, is certainly preferable to a negative one. It is also worth repeating that a deep familiarity with the community is not a requirement for a positive contribution, particularly as the local people themselves are in a position to pick, choose, and modify those contributions that make the best sense to themselves. Perhaps most important, I suspect that a deep familiarity with a place would too often impede the sorts of contributions the environmentalists have been making. Knowing something of the complexities, combined with a sharpened appreciation of my ignorance of many facets of the society, I would find it daunting to undertake the types of economic and political projects that the Maisin's new partners have introduced over the past decade. Much as I wish that the activists were a little more curious about Maisin culture and history, I envy the clarity they possess that allows them to act.

Changing Ground

The anthropologist Hortense Powdermaker titled her wonderful autobiography, *Stranger and Friend* (1966). My own relationship with the Maisin has evolved into a state that might be described, using the local senses of these words, as "missionary" and "kinsman." During our early fieldwork, both Anne and I were claimed as their "children" by senior men belonging to different clans. We thought this a bit comical at the time, but by the 1990s we found that people took our status as family very seriously. Our close "kin" refer to us by kinship terms rather than our names, and they expect us to either stay with them or visit them frequently while in the village. Our relationships have been confirmed by gifts of tapa cloth bearing our clan insignia, which we are forbidden to give away. In June 2000, Anne's "brothers" took our son Jake through the early stages of the initiation ritual for firstborn children. I have planted betel-nut palms in "my" clan hamlet and have been offered a new house should I decide to retire to Uiaku.

This all sounds wonderful and largely is. Yet kinship in Melanesian societies also carries heavy obligations that detract from any romanticism about "going native." An ethic of reciprocity underlies most aspects of life, not least in relations between close kin. My kin have been wonderfully generous over the years with advice, information, food, and companionship. And, like any son or brother, they expect me to reciprocate. Moreover, like any relative fortunate enough to have a job and vast resources by village standards, I am expected to give back more than I receive. For the most part this expectation is fine. My kin are rarely unreasonable in their requests and in any case are accepting when I say I can't meet them. All the same, the near constancy of requests can feel onerous, even overwhelming at times. And the expectation that I will automatically side with my kin in village disputes sometimes requires delicate diplomacy.

I don't think that most other Maisin regard me as kin—as one of them—but rather as a missionary, albeit one who has had a long relationship with the community. As I suggested above, the Maisin have long incorporated the category of missionary into local notions of leadership and responsibility. As with other relationships, that between villagers and missionaries is governed by reciprocity. The people listen respectfully to the "advice" of missionaries and help them in their efforts, and in return the missionary (or God) should do something good for them. Almost from the start of my work among them, the leading members of the Maisin community have periodically reminded me, usually graciously but occasionally forcefully, of their expectations.

Having a long-term relationship with the community has allowed me to give back to the community in a wide variety of ways. During our early stint of fieldwork, Anne and I organized a small library in Uiaku using books donated

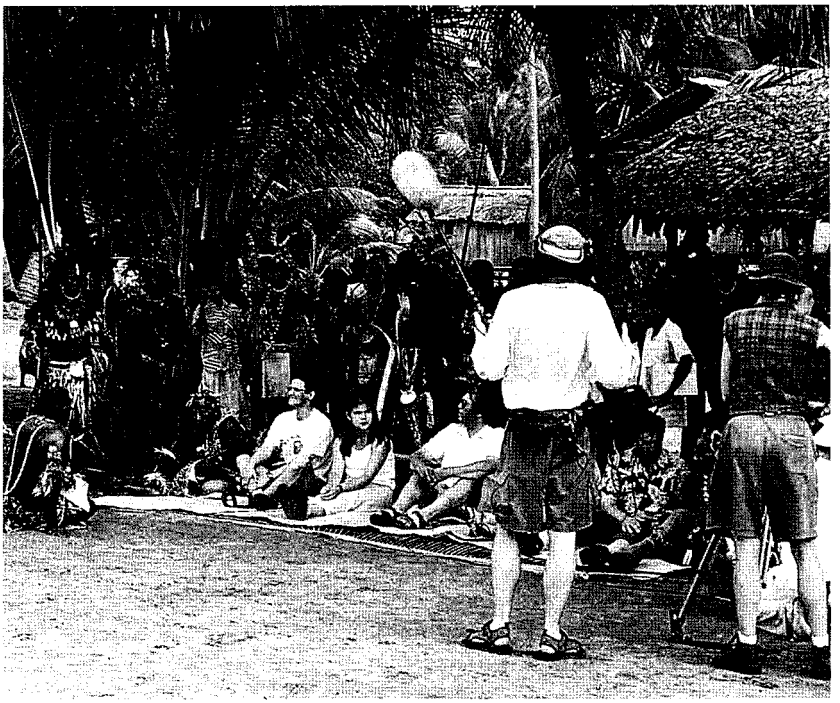

FIGURE 2. **Crew filming members of Stó:lo delegation, Uiaku, June 2000.** (Photo by J. Barker)

from a nearby plantation and from friends in the United States. Over the years, I have helped to write grant proposals for various community projects, raised money and publicity in support of Maisin resistance to commercial logging on their ancestral lands, written readers for the community schools, and hosted Maisin visiting North America. I undertook, along with Franklin Seri, a much larger project in the late 1990s by seeking a filmmaker to document the Maisin's struggle to retain control over their land. This project morphed into another when, in June 2000, I organized and led a delegation from the Stó:lo First Nation in British Columbia to the Maisin villages in an effort to foster mutually beneficial ties between the two communities. The visit was filmed for a documentary, *Changing Ground*, shown early the next year on the Canadian science program, *The Nature of Things*.

Arranging for the film and the visit involved a great deal of consultation with Maisin communities as well as fund-raising. The results of these labors, unfortunately, were crushingly disappointing. The presence of a film crew turned out to be enormously disruptive. The troubles began on the

director's arrival, when to my astonishment he vetoed a carefully worked out itinerary that would have had the delegation and film crew visit all of the Maisin villages. The decision flamed well-founded resentment against Uiaku, which has gained most benefit from recent outside interest in the Maisin. The director was also far more interested in capturing the Stó:lo delegates' reactions to village life than documenting discussions between the two groups. As a result of this tack, and a decision to shoot on film rather than video, most of the delegates' days were taken up being filmed repeatedly in small set pieces in which they observed villagers at work or attempted tasks themselves like beating sago, leaving little time for meetings with village leaders. A year later, the Stó:lo government hosted a delegation of Maisin to their lands, but here again the main purpose of the visit was turned over to the making of a documentary rather than discussions for future cooperation. The Maisin did seem to enjoy meeting the delegation and some benefited from the cash the visitors brought in or as members of the delegation to Canada. But, sadly, the experience seems to have widened a festering rift between the largest village of Uiaku and other Maisin communities, while a continuing relationship with the Stó:lo has so far failed to develop.

Changing Ground displays many of the same problems as *Anthropology on Trial* in its projection of popular but ethnographically dubious assumptions. The story line, conveyed through narration and interview clips, suggests that the Stó:lo delegates are encountering their ancestral past in the Maisin. The delegates express admiration for the Maisin way of life and regret for culture they themselves have lost. The telling exception is an adolescent member of the delegation, who misses the modern conveniences and amusements of home. As for the Maisin, they have tasted the world of money and decided to return to defend the lands and ways of life of their ancestors. The narration is quite spare but still includes annoying errors, beginning with the mispronunciation of "Maisin." But the silences are more telling of the film's intent. We are told in the opening scene that this meeting between two indigenous people was "remarkable" but never why it happened or what makes it notable. With little information to work from, viewers are made into voyeurs watching scene after scene of people performing unexplained exotic activities (dancing, waving spears, beating sago, and so forth) in a gorgeous tropical setting. The silences work to reinforce the general narrative of the film, one that most of the audience would have no trouble recognizing: the grand narrative of modernization, the destruction of an indigenous arcadia through the corruption of Western consumerism and greed. The only real novelty is the appearance of two indigenous groups on either side of the divide, both reduced to stereotypes: the Stó:lo who have "lost" their culture and the Maisin who are defiantly "holding on" to theirs.

My experience with this film touches on an ethical dilemma that many anthropologists face. For all of its simplicities, *Changing Ground* conveys a sympathetic image of the Maisin to its intended audience that might, in the future, help win valuable support when outside interests attempt to exploit local resources without the consent of the people. Younger Maisin in particular rather like the image of themselves as indigenous guardians of the rain forest as conveyed by environmentalists and this film. As well, many villagers hope that the film and similar projects will bring more money by promoting the sale of tapa cloth and encouraging tourism. On the other hand, I balk both professionally and personally at representations of the Maisin that venture into propaganda. Such portrayals set up expectations in an audience that are easily dashed by revelations that the subjects are not so "traditional" or pure in their motives as the film suggests. Beyond the politics, though, there is something deeply troubling about the conceit that indigenous cultures, while to be honored and respected, are nevertheless very easy to understand. In the 1980s, anthropologists engaged in an often-acrimonious discussion concerning the alleged tendency of the discipline to "appropriate" other cultures and to assume an unwarranted voice of "ethnographic authority" in accounts of them. My experience suggests that we need to turn this critique outwards as well as inwards, for I have seen little in the anthropological literature, past or present, to compare with the heavy-handed authority assumed in films like *Changing Ground*.

The creators of *Changing Ground* are not likely to ever work with the Maisin again to update or modify their film. It is the Maisin for whom the ground is changing, not the filmmakers. My experience is different. While I have observed and to a limited extent participated in major changes in the community, I am keenly aware that the grounds of my research and my relationships with the people have both changed enormously. I am continually rethinking and modifying my understanding of Maisin experience, both in my writing and teaching, and I am deeply grateful for the expanded opportunities I have enjoyed to give back to the community in a variety of ways. Most of my efforts have been devoted to written work. I still consider this kind of work—while unlikely to be seen, let alone appeal, to the wide audiences enjoyed by the films made about the Maisin—to be extremely important and worth defending. By way of conclusion, I now turn to that defense.

Conclusion

Critics of anthropological research conducted in places like Melanesia have often portrayed the endeavor as a kind of exchange: anthropologists "take" cultural information from which they build a career and, in return, they are

obliged to give something back. As we have seen, Melanesians themselves often talk this way. Yet that characterization needs to be explored rather than simply stated. The Maisin conception of reciprocity is complex and highly contextual. As in other Melanesian societies, exchanges occur constantly, working on several registers: economic, social, and moral. Any anthropologist working in Melanesia inevitably finds him or herself positioned within exchange networks. And, to the extent that one remains in touch, one continues to be subject to the push and pulls of the network even after leaving the field. Indeed, for many anthropologists, the network rather than the place has become the "field."

I have found that the longer I work with the Maisin, the more useful I have become to them. My main use has been as an archivist. Over the years, I have provided hundreds of copies of rough field notes concerning clan emblems and genealogies, audio tapes of stories and interviews, and photographs from my own collection and from those I've located in mission and museum archives. Like other anthropologists, I have found that few Maisin are much interested in my academic writing (although they appreciate copies), preferring texts that highlight the words of their own people. This has led me to spend increasing time on editing folktales, histories, and World War II narratives into self-published collections. Each time I return to the area I carry a carton full of duplicated notes, tapes, and collections to share as widely as I can.

I suppose that I could be accused of appropriating Maisin knowledge to benefit myself in that my scholarly writing is not addressed to that community and, indeed, interprets their experiences in terms that are foreign to most of them. Like all anthropologists, I hope that my interpretations both respect and make useful sense of the community and await, with some anxiety to be sure, the day that Maisin begin to comment directly on them. All the same, I do not view my academic publications as independent of my relationship with the community. Instead, they are an outer face, as is my teaching, of the Maisin network of exchanges that I have so long been part of. In my work, I have increasingly tried to convey the fact of my own location as an observer and interpreter, on the one hand, and an appreciation for the complexity of the society, on the other. I continue to search for patterns, cultural and historical, but have gained an increasing tolerance for ambiguities and contradictions. This makes, perhaps, for less-elegant portraits of Maisin experience but, I hope, better shows the people as fellow human beings often struggling to construct and make sense of their lives, and not as exemplars of a non-Western other.

When I first began graduate studies in anthropology, it was already becoming difficult to undertake long-term fieldwork in places like

Melanesia. Since then the obstacles have only increased. Students entering the discipline today have trouble securing funds for an eighteen- to twenty-four-month stint of fieldwork and, in many places, come under strong pressure to complete their degrees quickly. The focus of the discipline has also shifted over the years—for a variety of reasons—from research in "exotic" locations to projects close to home. In Papua New Guinea and, more recently, the Solomon Islands, the breakdown of infrastructures, prevalence of deadlier forms of malaria, and ratcheting levels of violence have also worked to dissuade younger fieldworkers. Finally, few new graduates can count on securing tenured academic positions that would enhance the opportunity for long-term research based on regular return visits. Obviously these obstacles will be far less acute for Melanesian scholars working in their own region, but in general the trend would seem to be away from long-term research.

Yet changing circumstances have at the same time opened new possibilities. Over the past twenty years, travel and communication across the globe have become easier and more economical. I do not think that the "field" will vanish from the anthropological vocabulary, but the word clearly has come to mean something more than a place. Opportunities for interactions, in some ways more intense and frequent, have increased for many places. There are also more opportunities for anthropologists to maintain their relationship with a community in occupations outside the academy, by working as journalists, for instance, or as environmental educators and activists.

The need for anthropological research based on a long-term personal commitment to indigenous communities has never been greater. Advocacy in the forms of superficial journalism or films like *Changing Ground* can never be of more than temporary value. Well-formed advocacy, based upon a deep appreciation of the lives of real people, may be less popular in the West but more important where it counts—in courtrooms and in classrooms. Anthropologists perform their most important function by resisting an implicit ethnocentric conceit in the modernization metanarrative: that indigenous peoples are of interest and respect only to the degree they succeed in remaining "traditional." The anthropological "gift" of ethnographic records, imperfect and inadequate as they may be, is perhaps our most crucial contribution to the communities we work with. I feel immensely privileged to have had my dream of establishing a long-term relationship with a community in Papua New Guinea. As long as they want me, I intend to continue working with the Maisin. I hope that future generations of anthropologists will be willing and able to make the same commitment to the communities they choose and are called upon to study.

NOTES

Anne Marie Tietjen has been my constant companion both in the field and in my thoughts when we have been apart. I gratefully acknowledge our shared experiences and her reflections on the Maisin people, which have helped to shape much of what I write here. I take the greatest pleasure in thanking our Maisin family, friends, and neighbors for their willingness to accept us into their community for all of these years. Au roisesinamme, tenkiu bejji aifa.

1. The first known contact occurred in 1890, when the newly appointed administrator of (then) British New Guinea, William Macgregor, toured the northeastern coast. The Maisin were "pacified," at the cost of a number of lives, in 1900, shortly after the establishment of a government post at Tufi. Two years later, the Anglican Mission began a school and church at Uiaku (Barker 1987).

2. The exhibit has been preserved in "virtual" form at http://www.bampfa.berkeley.edu/exhibits/jumpline/jlhome.html.

3. And just in time for the climax of the Sandline crisis, but that's another story (see Ivarature 1998).

4. According to Anna-Karina Hermkens, who has been researching Maisin women and tapa cloth, the older form of feasting persists in eastern Maisin villages, perhaps because fewer people in these poorer communities can afford plates and cutlery.

5. Commercial logging began growing exponentially in Papua New Guinea in the early 1980s, accompanied more often than not by broken promises of local development, political corruption, and environmental devastation. One response was an equally rapid growth in NGOs dedicated to conservation. These developments have been tracked by Colin Filer in several publications (e.g., 1997; 1998b).

6. One can, of course, push the analogy too far. The Anglican missionaries were primarily concerned with establishing Christianity; they settled in or near indigenous villages for long periods of time and established permanent churches and schools. Just as importantly, they worked in the context of colonial overrule, seeing themselves not as guests but as teachers and managers of the indigenous population. Such aspects, however, may not have been all that visible to most Maisin who had little direct contact with European missionaries (see below).

7. In 1998, the film producer, director and myself visited and held meetings in the major Maisin villages to explain the film project and seek permission. One major stipulation made by village leaders was that the film would incorporate all of the Maisin villages. It is quite possible that the film director, who did not attend all of the meetings, did not understand the reasons for this. In any case, I made a major mistake at the time by not insisting that a contract be drawn up between the Maisin leaders and the filmmakers.

8. In 2002, a second documentary based on the visit of seven Maisin to the Stó:lo in British Columbia aired on Canadian television. Entitled, *Years from Here*, the film continues the theme of culture loss. Interestingly, however, it is subverted by the Stó:lo who,

unlike the Maisin, have had a great deal of experience with the media and obviously exercised a strong control over the film's message. Unlike *Changing Ground*, the second film projects a positive spin on the Stó:lo as a dynamic nation that has, through years of struggle, renewed itself. In contrast to the eloquent and sophisticated Stó:lo, the Maisin appear rather lost. While I had the opportunity to meet the delegation briefly, I had no involvement with this project.

9. As Sarah Pink observes in a cogent discussion of the ethics of exchange in the field, the same is true for other foreigners seeking to "help" local communities, including salaried development-aid workers, volunteers, and, one might add, filmmakers and journalists, "each of whom has his/her own personal agenda and meeting points with local culture" (2002:110).

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LIFE IN DIS-PLACE: RE-SEARCHING PROCESSES OF IMAGINING WITH ENEWETAK-UJELANG PEOPLE

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The current article attempts to outline the way in which ongoing research with the Enewetak-Ujelang community and with other Marshall Islanders over a period of nearly three decades has required the author to reformulate taken-for granted ideas about "culture," "the field," and "field research" as well as presuppositions about self/other and about ethnographic writing. Not only has the shape of "the field" shifted substantially during this time period, becoming much more multifaceted and multilocational, so too, the identity of the author has been recontoured many times over by members of Enewetak-Ujelang communities. Therefore, neither "field" nor "fieldworker" are ever the same since members of the community and the field researcher are continuously reformulating their ideas about each other. I argue that shared experiences—in this case, ongoing and extended periods of living life with members of the Enewetak-Ujelang community, engaging in their daily activities and pursuing their varied agendas—allow an anthropologist of the long term to speak and write with some legitimacy about the lives of others because their lives and the ethnographer's life are of a piece. If the aim of anthropology is "to grasp the native's point of view" (Malinowski 1922), this enigmatic quest can only be realized to the degree that the ethnographer has shared in the processes of mutual self-fashioning that make the practices of others "experience-near" fragments of one's self.

IN A RECENT WORK, James Clifford writes about the way in which "The Field" (the location, both physical and motivational, for field research and writing) has become an ambivalent, multifaceted, multilocational space (Clifford 1997). He argues that while this has always been the case, it is now more true than ever. In part, of course, this has to do with the fact that the communities

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that anthropologists work with often have become increasingly mobile, but equally, it relates to anthropologists' increased interest in the many semiotic residues and inspirational threads of material that come to be embedded in anthropological texts. Indeed, Clifford's own path leads him from a more general interest in the way in which anthropology has constructed its natives, its "objects" (Clifford 1983), to cognitive-spatial scenarios that are insinuated in this process.

The current article attempts to trace significant contextual changes that have contributed to shifts in my own research career with members of the Enewetak community and with other Marshall Islanders. Enewetak, in particular, presents an ideal landscape to investigate the way in which the field must be far more than a discrete physical location to be visited, a site separate from one's "real" place of residence. Years of displacement and exile resulting from World War II and the subsequent era of United States nuclear tests forced community members to construct senses of identity that transcend space. Over the past twenty-eight years, I have had the good fortune to be part of these refashionings, joining the community many times as they have sought to create dis-place-based identities on Ujelang, Enewetak, Majuro, Honolulu, the Big Island (Hawai'i), and elsewhere in the United States. These engagements suggest that the "field" and "fieldwork" are neither here nor there: they are elusive ideas, multiply situated and far more complex than typically described. Equally, both the community and their constructions of me have been refashioned many times over. While the field as a fixed location, the unchanging community of native residents, and the stable researcher with his research products are each comforting ideas, our comfort with them obscures their complexity. In my many years working with Marshall Islanders, each of these constructs might be understood more accurately as continuously negotiated symbolic arrangements shaped by the multitude of interactions and experiences I have shared with the people of Enewetak and those from other Marshall Islands locales.

In a related piece, I have elaborated on the way in which significant alterations in the Enewetak community's positioning of me as a fellow human had substantial effects on my understanding of the community. This included their shifting understandings of me as a young researcher—one of a tribe of anthropologists with whom they had interacted—yet, in their view, more easily classifiable as a Peace Corps, with whom they had frequent and continuous interaction from the mid-1960s until 1980. These understandings were further altered as I became an adopted member of the community and, eventually, a returning researcher accompanied by a family (Carucci 1997b).

These "era-depictions" of me by members of the Ujelang-Enewetak community, at some gross level, do typify people's shifting understandings of my general relationship to the community. Nevertheless, they are an inadequate means of expressing the multifaceted and extremely varied ways in which particular residents, in specific interactions, talk about and classify me as a fellow human in meaningful ways generated by the circumstances of those particular interactions. Each of these shifting understandings is critical in relation to this article since I wish to argue that there is something uniquely valuable about the long-term, intensely involving, research method that many anthropologists use. Of course, many others have elaborated on the value of such research, from the contributors to *Long-Term Field Research in Social Anthropology* (Foster et al. 1979) to the authors represented in *Chronicling Cultures: Long-Term Field Research in Anthropology* (Kemper and Royce 2002).

As Royce and Kemper note, extended research allows a researcher to move away from the "fallacy of misplaced concreteness" (2002:xv). Extended research engenders experiences that encourage a broader view, one that allows the researcher to accentuate central events and separate them from transient, peripheral ones. Political motivations are commonly clarified the moment a researcher submits those contested motivations to the magnifying lens of multiple layers of shared experiences. Indeed, the very boundaries of community and culture are transformed as a long-term investigator is able to demonstrate how any social group re-invents itself through time (Royce and Kemper 2002:xvii–xxvii). Yet, just as critical as these advantages of long-term research, I suggest that an equally high value should be placed on becoming experientially inundated in the daily pursuits of local people. The "experience near," "participant part" of "participant observation" is far more valuable than the "observation" part [Wikan 1991] precisely because it allows the researcher-as subject to come into view. As Marshall Islanders often say, only through *bed wōt*, "remaining with" (for an extended period of time), can one participate in a way that leads to shared meaningful interactions and understandings.[1]

Combining multiple research encounters with experience near motivations lead one inevitably toward meta-contextualization. Indeed, it could be argued that this is one of the central productive processes of cultural understanding: by layering one contextual frame upon another, anthropologists as participants learn to form their own culturally sensitive, experience-near discriminations. As a correlate of such multi-layered, repetitive, experiences, returning to "the field" multiple times can only lead to much more reflexive and historically situated senses of local consciousnesses. Finally, by making moral commitments to the causes of local people, the entire observer/observed

dichotomy ruptures in ways that simultaneously complicate and enrich the research experience. These complicating enrichments, if allowed to play out their course, require researchers to give up their feigned objectivity (see note 1). But, inasmuch as objectivity is fashioned by researchers for professional audiences in a foreign world, its "loss" can only result in greater mutual understanding (cf., Schepler-Hughes 1993).

Of course these propositions rest on certain understandings of the anthropological project that are not shared by all anthropologists. Such understandings are, however, largely shared by those who take Malinowski's dictum to heart: viz. that the anthropological aim is to "grasp the native's point of view, his relation to life, to realize *his* vision of *his* world"(1922:25). Equally embedded in this perspective is Geertz' more recent understanding of the anthropological endeavor as a semiotic project, an endeavor at once about and implicated in the production of intersubjective meanings (Geertz 1973). And should we be tempted (as anthropologists often are) to jump incautiously from the intersubjective to the apparently objective, Lacan suggests that the alienation of the subject from his/her desire to act in the world in an unmediated experiential manner is, itself, prevented by the enabling possibilities of language and culture. Therefore, anthropology must move well beyond its complicity in objectifying the objects of its own production as "the language of" or "the culture of." Meanings are only separated from "feelings" by European dichotomies of an entirely cultural order (Wikan 1991). And one does not think-feel in an abstract, objective, way.[2]

The entire ethnoscience experiment in anthropology clearly demonstrated that, in an important sense, Malinowski's project, while laudable, was also unattainable since the bridge between the ethnographer's interpretations and the native's interpretations was uncrossable. Nevertheless, this far from invalidates the potentials of the anthropological attempt to "get at" the native point of view. Since meaning and practice are inextricably intertwined, Wikan (rightly, I believe) takes us down the path of the *experience near*; the same path, I would argue, that each of us traverses in our own enculturation. In following Wikan's lead, anthropologists are led (as was Malinowski, in some degree) "off the verandah" and directly into the experiential realities of life in another culture. For this reason, long term and return research, is not only desirable: it is directly correlated with anthropologists' ability to align their own experiences as closely as possible with that of "the natives" and, therefore, to empathize and understand through "feel-thinking." Any other form of understanding takes anthropology away from Malinowski's notion of what the discipline was all about.

Peggy Trawick, in interpreting Lacan, notes that "meaning, the seeming goal and source of language, has no stability. It . . . is a matter of relations,

arbitrary and contingent, always shifting. Meaning . . . this "other of the other" . . . can never be captured or appropriated" (1990:145). And yet, if one takes Malinowski's dictum to heart too literally, is this not the aim of anthropology, to capture and appropriate the "other"? For Lacan, this search for the (re)integration of self and other is pan-human, doubly embedded in the separation from one's own mother at birth and in the psyche of each of us as symbolic creatures (Lemaire 1977). Thus, much of the blaming discourse that has "othered" anthropologists and anthropology for creating "others" only to fulfill a (slightly) repressed desire to appropriate and control "them" is hardly unique to anthropology (cf., Rosaldo 1993, and O'Rourke's *Cannibal Tours* 1987). If we follow Lacan's lead, all academic endeavors, indeed, all human representational activities, endlessly chase their own other/self images in pursuit of unfulfilled desire (see Lindstrom 1993 for a Lacanian take on Cargo Cult).

If meaning can only be posited, never captured or appropriated, however, I believe there are distinctions in the modes of knowing and modes of depiction that can differentiate the levels of satisfaction/fulfillment in a person's attempts to comprehend. And, for ethnographic research, that which separates the near total self projection that many associate with nineteenth-century armchair anthropology from the most insightful forms of ethnographic inquiry, relate to the levels of (inter)personal experience and to "experience-nearness." Such experiential insight cannot be obtained in lieu of long-term, repetitive, inundation in the lives of others (though not guaranteed by these factors). Such co-self fashioning Schutz calls "growing older together" (Schutz 1967:103). I can speak/write only with my own voice. Only through extensively sharing experience with others, can my voice be made to resonate/reverberate in tones that, in frequency and contour, evoke the dilemmas and desires of those of who, through shared time, talk, labor, and love, have become a part of me. For this same reason, the *way* in which the field experience is authored is critical, since texts are primary artifacts that link representations in multifaceted ways with authors, and inscribed meanings with derivational contexts. Certainly, the ultimate aim of the anthropological project should not be thought of as a form of "ventriloquy." Nevertheless, for many genres of anthropological discourse and writing, I believe it is important for an anthropologist to be very explicit about the precise ways in which the representations that s/he tosses about in conversation, or inscribes in a text, are related back to specific interactive research contexts in which those representations are rooted. Every good ethnographer must triangulate statements about meaning in this manner, since so much of the hocus pocus of ethnography occurs in the space between what was experienced and what comes to be inscribed (often multiply, and in different ways for

different audiences).[3] Even in these ideal contextually sensitized conditions, texts are authored and meanings fashioned. The text never "captures" an experience. Nevertheless, if ethnographers were to adhere to the dictum of triangulation, the logic of why a particular set of representations came to be inscribed and, thereby, overdetermined in a text would be apparent to readers of that text. If the worlds of others necessarily escape captivity, an experienced ethnographic author need not be left solely with the detritus of desire. The reflexive accounting of an ethnographer saturated with years of the experience-near, can tell a story that, while reflecting his/her particular methods and intents, has the possibility of demonstrating mutual empathic understanding in writing of the intertwined character of thoughts and feelings and the experiential processes of "coming to know" (however little one ever may know).

Situating Selves and Others

To return to Ujelang and Enewetak people's shifting understandings of me over the years, it is generically legitimate to say that, early on, many people asked about or introduced me using a Peace Corps designation. This was particularly true of those who had not come to know me well. With Peace Corps of roughly my age moving on and off island every few months, this classification made sense. Later on, many would say "Oh, he is the offspring of Biola" (my mother by adoption), when introducing me to others who were not long-standing residents. When others would say "He is really an Ujelang (or Enewetak) person," that classification was, most often, an alternate way of noting that I was adopted by a member of the community. But each time someone attributed a local identity to me, the meaning depended on specific elements of the context of use. The speech situations in which such comments were made share the fact that the local speaker always addressed someone far less familiar with the situation, but the "he is one of us" message also meant very different things when, as in one case, the speaker was addressing a local student returning from college and, as in others, when the speaker was addressing a foreign, white, government official. Equally, there are other very different understandings of my identity that coexist with the above. Yet, even though they are less common, they are classifications just as critical as the above. For example, on one occasion, a huge argument erupted with the husband of one of my sister's daughter's when he wanted to use my Swiss Army knife to work on a model canoe. While I indicated he could use the knife at my house, I refused to let him take it home, having recently sacrificed three such knives when they had been "borrowed" and, at the time, having no backup pocket knives.[4] In the argument, he depicted me as "very

haughty" and "the worst of white people (Americans)." (Being stuck up, or haughty, is a common Marshallese classification of Americans' nonsharing demeanor.) *In this context*, haughtiness and being the worst white person served as effective contestations of the otherwise common public situations in which Ujelang people claimed me as one of their own based, in part, on my generosity. As a pedagogical statement, it pointed out that for Ujelang people there was *never* a condition when selfishness and hoarding was acceptable among close relatives (even though it did not mean that such activities never took place among close relatives).

These varied meanings and the practical activities in which they are embedded are virtually unlimited. Nevertheless, the more time one spends in the field, the broader the array of interactions one experiences, and the greater the likelihood that the anthropologist as author can construct accounts that depict experiences in ways that reflect the contexts in which they occur as well as their frequency.

If this is the advantage of lengthy research encounters, the superficially recurrent character of multiple encounters may have a down side as well. With a huge array of interactive contexts to use as points of reference, there also may be a greater tendency to fashion new accounts out of the normative. In other words, often the common aspects of varied interactions become the taken-for-granted theme of an ethnography, thereby obliterating the context-specific conditions that made a particular interaction meaningful. The ethnographic account, always a meta-expressive document, becomes further divorced from events in the field and moves ever more toward an ethnographer's assessment of the superficially similar character of many different interactions. It represents the ethnographer in the act of creating culture by inscribing that which inevitably (always, typically) happens. Normative statements of this sort may be unavoidable. Even an experimental ethnography like *N!isa* (Shostak 1981), typified by lengthy transcriptions of N!isa's talk, is far from straightforward. Initially, the normative sections seem to be cordoned off in separate introductory sections.[5] Nevertheless, the transcriptions are also interpretive work. In these sections Shostak translates into the language of the consumer of the text and selectively decontextualizes quotations, moving them from their contexts of elicitation to newly contextualized settings that reflect Shostak's own biases about what she takes to be generic similarities in women's lives and in the supposedly universal course of the human life cycle. Once the unquestioned brokers of knowledge about other cultures, in recent years anthropologists (like Shostak, above) have begun to work much harder at defining a viable social location, a positioned stance (less presupposing, less patriarchal) where their voices still make sense. Given the shifting grounds of this pursuit, I believe

it is critical to be explicit about the distinctive types of meanings that are sandwiched together in any ethnographic account. The anthropological "tool kit" of literary devices is not often used to maximum advantage in order to discriminate "whose meanings," or "meanings in relation to what."

Re-turns to the Field(s) (Never the Same Me, Nor the Same Field)

If returns to the field are of extraordinary value, part of their value lies in the immediate disproof of early anthropological thinking about the unchanging nature of so-called primitive societies. Changes in culture and changes in social selves are continuous and while the specific social sites where change occurs are far from universal, both for societies and for social persona, the radical divide between Lévi-Strauss' hot and cold societies (1962), as Sahlins was perhaps the first to point out (1976), was little more than ethnocentric projection. Elsewhere (Carucci 1997b), I have noted how substantially my own position within the Ujelang-Enewetak shifted through time. Even within the first two years, people came to see me differently, and interact differently with me as they re-situated me from Peace Corps like visitor/outsider, through numerous intermediate moments to adopted-by-Biola long-term resident, and eventually to potential future spouse of Jinet (different generation, different family, opposite half of the village). Five, fifteen, and twenty years later, these re-situated understandings of me by community members shifted even more, and as Ujelang and Enewetak people repositioned me, so my understandings of different elements of local experiences changed (Carucci 1997b). Now most Ujelang-Enewetak people interact with me as *jimma* (grandfather), *wūllepa* (mother's older brother), or *rūkora* (mother's younger brother), whereas twenty-five years ago I had several people who could call me "child," and a plethora of older siblings. And, of course, expectations of how I should act and how others act in relation to me have shifted. But I have not been the sole persona to change. The entire fabric of Ujelang society was in equal flux, as was each social persona therein. In short, the whole idea of a "return," like that of "the field," becomes problematic. Even when returns are rapid, the "society" has reinvented itself. Each time I return to Enewetak, new social actors, new events, new social arrangements have appeared. While they bear definite historical connections with formerly encountered actors, events and alignments, they never take the concise forms one might have supposed in advance. Therefore, the social fields are every bit as new and transformed as the turns I must negotiate to place me in face-to-face encounters with my Ujelang-Enewetak family and friends. Social persona, constructed out of a nexus of interpersonal relationships and dependent on shifts of situation and life circumstance as well as social life,

are dynamic, all-too-often taking on the dimensions of cohesive individuals solely to fit projections of Eurocentric individuals.

The recent Enewetak magistrate and chief, Naptali, is a perfect example. Much as I have been repositioned within the community, so Naptali has shed the skins of multiply situated selves, and is now a radically different social persona than the Naptali I knew as a young field researcher in 1976. At that time Naptali's father, Ioanej was "the chief," the last of a long line of chiefs with substantial clout and power. Naptali's position as the future chief was far from secure. It was clear that if Ioanej' younger brother, Tom, outlived him, he would be the next chief, but the two elders were close in age and Tom's rule would not be long. Beyond that, however, chiefly inheritance paths were cloudy. An older sibling line of Ioanej father, at that time represented by the brother pair Apinar and Aduwo, were eager to claim chiefly rights. And Naptali's own older brother, while adopted by the chief of the Enjebi half of the community, Ebream, still had some chiefly claim. There were even those who contended that the community should go back to an earlier time in the nineteenth century when, by their self-empowering accounts, the chieftainship has passed along matriclan pathways, a route that would make Joseph the Enewetak successor. Demeanor was also a consideration. And Naptali, one of the *di nana* (ill-behaved ones) in 1976-1978—a smoking, drinking, nonchurch member—was not the epitome of a future community leader. His older brother-by-birth, however, was a far heavier drinker and an equally questionable future chief. Further clouding the lens, the older of the older line sibling pair, Apinar, was not a skilled orator. Aduwo, however, was a famed storyteller and outspoken purveyor of communal knowledge. Yet, unlike his brother Apinar, he smoked, drank and, in other ways contravened the teachings of the church.

Five years later, only one element of the scenario looked familiar: Ioanej was aging rapidly and Tom was in a position to succeed his older brother as chief. In other ways, the social scene was quite different. Aduwo had disappeared on a winter fishing trip with two of his sons (Americans would call them nephews), and the community had, after some time, held a memorial ceremony. And Naptali, now a member of the Enewetak-Ujelang Local Government Council, was attempting to reshape his persona by giving up smoking and drinking. He was talking about becoming a member of the church. Such patterns of social identity reformulation were not uncommon among men in early middle-age and, knowing that Tom's tenure as Enewetak chief would be short, Naptali had substantial reason to give up his rebellious ways.

By 1988, Tom, had also died and with Aduwo now out of consideration, Naptali had become the Enewetak chief. His elder brother was attempting

FIGURE 1: **Naptali: prior to his years as magistrate/mayor, and chief.**
(Photo by L. M. Carucci 1976)

to reformulate his own identity as well but was struggling to sever his long-standing relationship with alcoholic beverages. Naptali had succeeded in this endeavor and was now a deacon in the church. In the 1990s another quirk of fate took the life of the former magistrate, Hertej—once undefeatable on account of his vast network of relatives. The next election therefore returned the community to a circumstance familiar from some thirty-years earlier: the civil affairs of the community, now governed by a mayor (rather than magistrate), would be aligned with the once-sacred care of the atoll by a chief. The Enewetak chief, like his father and grandfather, was now also the mayor. From begging a fragment of cigarette at my doorstep when supplies ran low, from his criticism of council doctrine, from his joking with the daughters of his older sisters, Naptali had become a central figure on the atoll. As mayor and Enewetak chief his power was manifest (if not unquestioned). His humor now constrained, his voice having sacrificed critique for pronouncement, the alcohol and nicotine now winnowed from his veins by the repetitive incantation of biblical verse, he was a being of very different social contours.[6]

If this was the height of Naptali's power, it was not the last chapter in his career. Like many Marshall Islands chiefs and empowered officials, new sources of money from outside the Marshall Islands proved irresistible to Naptali (see Carucci 1997a). Failing to distribute these funds among commoners, Naptali began to lose favor in the community. In 2003 Naptali had definitely fallen from grace.[7] Apinar, the guileless elder who earlier had been overlooked in the selection process, was now the chief of choice to represent the Enewetak half of the community in the legislature. While Naptali was still the magistrate, younger candidates planned to challenge the mayor in the coming election, claiming that they would not engage in the same diversion of funds that had plagued Naptali's reign. In private conversations on the Big Island, Naptali was criticized for not representing people's interests, indeed, for being so haughty that he would not even speak to Enewetak residents when he encountered them in a store.

While Naptali's discourses are still contextually contoured, the degree to which his performances are now much more uniform and constrained is striking. Social scrutiny of his demeanor is now strict and the importance of his position in the community is marked by the expectation of fixity, almost impotence. The result has been an increased incapacitation and, having interacted with him over some decades, in recent years a disappointment is evident in his demeanor. In looking back on his radically altered identity, I believe that, in spite of his days of substantial empowerment, he also has a certain nostalgia for the social freedoms of his own experiential past. Indeed, speaking with Naptali in 2003 it seemed that, at some subliminal

level, he would welcome not being in the spotlight. Ultimately, if this was his unspoken wish, it was a wish that was fulfilled. Naptali lost the mayoral election in November 2003 creating other contradictions, but moving him slightly off center stage.

None of these nuances of shifting self-fashioning would be evident without the multiple glimpses of Naptali's activities over a lengthy period of time. Such glimpses are enabled by multiple returns to the field.

Turns within the Field (The Field Turning into New Fields, into New Instantiations of Itself)

If multiple field encounters have led to a shift in the way Enewetak people have come to describe me and interact with me, the reasons lie as much in changes within the community as in the fact that I have returned multiple times to the Marshall Islands, or the fact that life cycle shifts have created inevitable changes in every person's ability to manipulate their social identities within the community. Substantial alterations in social organization have occurred as a result of the community's involuntary involvement in nuclear testing. Indeed, an extraordinarily complex politics has been created as local people have attempted to obtain a just settlement for the damages to their atoll as well as for the suffering they endured during their years in exile. These social organizational shifts clearly have engendered correlative changes in the way Enewetak people conceive of themselves. As the group's physical form has changed, as well as its members' desires and conceptions, so have my own interests and research foci. It is the course of these alterations that I would like to trace in the remainder of this article, a set of concerns that is not separate from the issues I have described previously, but certainly one that has been the direct result of having continued to do research with the same community, and with closely related Marshallese communities, for more than twenty-five years.[8] While I use the term "research" perhaps too much in the classical scientific tradition set in the late 19th century by anthropological expeditions, or in the Malinowskiian remodeling of this tradition into extended stays with isolated groups of so-called primitive peoples, the kind of continual work I do in and with the Enewetak community, and with neighboring groups, is really a lifestyle. It is continuous and unbounded, not clearly separated out into "the field" as something opposed to the day-to-day. Part of my yearly routine includes time in Hawai'i and the Marshall Islands. It often includes time in Washington, D. C., Los Angeles, or Arkansas where I work with Enewetak people on issues of concern to them. At other times they ask me to work for them to help solve nutritional problems that have resulted from the era of nuclear testing.

In a multitude of circumstances, the lines between fieldwork and other work seem meaningless. To learn more about Marshallese is often indiscernible from learning more about myself. Today, as I check my e-mail in Montana, there is a note from an Enewetak woman with additional information about her ancestors. She is not responding to a survey of my design on kinship, but rather is using me as her primary consultant to find out more about her father's family. A few months ago, in 2003, I awoke to face my portable computer on the Big Island, to expand on some research notes from the previous day, and to complete a paper for publication. Beginning in the 1970s, technological advances began to allow for a blending together of the anthropologist as author and the anthropologist as field researcher. These technological changes enabled me to engage in interactions with my Marshallese relatives, note-taking on those events, writing about elements of that work, and e-mailing back and forth with editors about related publications all in the same day. Six months before that, with my family also in residence on the Big Island in 2002, I woke to take my daughter to a school function, after which my son and I provided help for our Marshallese relatives. We loaded our gifts of food into the car, transported ourselves and other (Marshallese) family members to church, attended a lengthy church service, and participated in an intensive songfest performance of the sort that occupies many Sunday evenings in the early part of the "Christmas" season. Perhaps that was a true "field day," but the next day I expanded on notes, practiced new songs in preparation for my own group's songfest rehearsal and searched for additional automobile parts that an Enewetak relative wanted me to pursue at the salvage yards in Hilo. By the end of the day, I had reported back to him about the parts, but our telephone conversation was dominated by a discussion of the latest argument within the community. A few days previously, this same man, accompanied by two others, were in our living room sharing fried rice after a day of stripping automobile parts from rusting cars in the salvage yards. I learned far more about important community events on Enewetak and in Majuro than I did about used car parts on that particular day. In circumstances like these, "in the field" and "out of the field" fail to have any meaning. I find myself both "at home" and "in the field" in multiple locations with very different referential parameters. With multiple extended turns to the field, and even more momentary ones, "the field" is neither a distinct place nor consistently differentiable practice; at most, it is perhaps distinguishable as a domain of consciousness that blends, nearly indistinguishably at times, into other parameters of my being.

This blurring between life and work, in part, may be due to shifting tools and conceptions of work in the United States. Equally, however, it has to do with the long term component of my work, with the fact that I have

FIGURES 2, 3, AND 4: **While change has significant effects in all domains of life, Kūrijmōj —"Marshallese Christmas"— continues to provide a sense of meaningful identity for Ujelang/Enewetak people. (above: 1977: Ujelang Atoll; below: 2002: Hawai'i, Hawai'i; opposite page: Enewetak Atoll: 1982).**

chosen to work with Enewetak people in particular, and other Marshallese with similar histories more generally, for a good part of my life. In so doing, the idea of the anthropologist as outsider, as a visiting alien, becomes more than slightly problematic. Indeed, the whole idea of my understanding of Marshallese culture as a thing that is "out there" somewhere, analyzable and documentable, rather than "in here," an experiential part of my own consciousness, is equally problematic. As most Marshallese realize, the designata "*di palle*," "white person," "person with clothes," "American," and so on is simply not adequate to capture their classification of me, since our relationships are more convoluted and complex. At some level, as they say, "you are just a piece of Marshallese people." And, in many senses, that is precisely how I experience Marshallese life, as a feeling, not just as a "thinking about." Certainly, this does not mean that extended work in a community allows one to morph one's own persona into that of another. It does, however, mean that the totality of shared experiences is extensive and grand. It does mean that interpretative frames and praxis routines that I use in everyday life have embedded in them elements of Enewetak and Ujelang frames and routines. If these are not separate worlds, distinct cultures, but intercultural activities typified by bricolage and pidgin, they are enacted everywhere as complex vectors of situated experience, not as degenerate admixtures of pure, rarified culture.

Of course, when I am physically on Enewetak, the Enewetak component of my identity is placed in vivid relief. I commonly speak in the Enewetak dialect of Marshallese from the break of dawn until I go to bed. Even then, I frequently dream in Marshallese. When traveling without my Montana family, all of my daily interactions on Enewetak are with local people, in local dialect, discussing issues of local concern. This is far different from my first interactions with the community when my language abilities and knowledge of local practices were rudimentary, and when primary attention to my own research interests created a product and experience that contrasts sharply with my work today.

Nowadays, my students and my family in Montana often suffer from the embeddedness of Enewetak-ness in me. In lectures, I commonly confuse "he" and "she" (or "his" and "her") in a way that never occurred prior to Marshallese having become embedded in my subconscious (there are no comparable pronouns in Marshallese). And at home, my children are often trapped in hodgepodge socialization strategies with Marshallese exhortations interwoven into Mountain West expectations. Trawick (1990) uses the riverine metaphor of confluence to describe the continuous construction of culture and identity, a gradual intermixing of vital fluids of varied source. Often, however, I fear my interactions with my children will seem no more than

muddled moralizing trapped in the noncommittal space between American valuations of self-sufficient individuality and outer Marshall Islands stress on negotiated communal solidarities.

Blurred Boundaries

As noted, movements on the reverse side of this dialectic—changes in the contours of the Enewetak-Ujelang community—make "the field" an extremely problematic designation. At moments, such changes are monumental, but even when they are incremental, "the field" is always dynamic. Dis-*place*-ment was a critical feature of the Ujelang community at the time of my first extended stay, a period of living in exile that transformed the community from di Enewetak (the people of Enewetak) into Ujelang folks (Carucci 1992 and in press). This, of course, was a result of United States nuclear testing that destroyed and re-contoured much of the physical fabric of the place called Enewetak, remaking it into Eniwetok (Enni-wee-tak [and various other mispronounced variants]), a location occupied by the U.S. military and Department of Energy. For Enewetak people, life in exile on Ujelang began in 1947 and lasted for thirty-three years. The physical relocation of the community had numerous effects on the group's structural fabric and human contours including long periods of isolation, famine, and additional hardships, but the consolation prize for having suffered isolation and impoverishment was increased cohesion and solidarity. As the most isolated community in the Marshall Islands, both geographically and culturally, visits by supply ships to Ujelang were infrequent and, inasmuch as these ships also provided the sole source of transport on and off of the atoll, local people had to become far more local than they might have desired. Mobility during the Japanese era that preceded the war appears to have been substantially greater. Certainly, movement of Enewetak people has increased exponentially since their return to Enewetak in 1980.

After World War II, however, U.S. colonial strategies re-created Micronesians in the American image of Pacific primitives: docile natives, kept at a distance from the world's worries (though not far enough from nuclear tests that turned Northern Marshall Islander's lives upside down),[9] living simple lives by fishing, gathering, and animal husbandry on their isolated islands and atolls. Under these social conditions the boundaries of "Ujelang people" were, in most senses, easily defined. Long-term, local residents, were diUjelang ("people of Ujelang"). Former distinctions between northern islet dwellers on Enewetak, diEnjebi, and residents of the southern islets, diEnewetak, were reconfigured on Ujelang. Interactions occurred daily on this tiny atoll of exile, and mutual interaction created new cohesion. Those

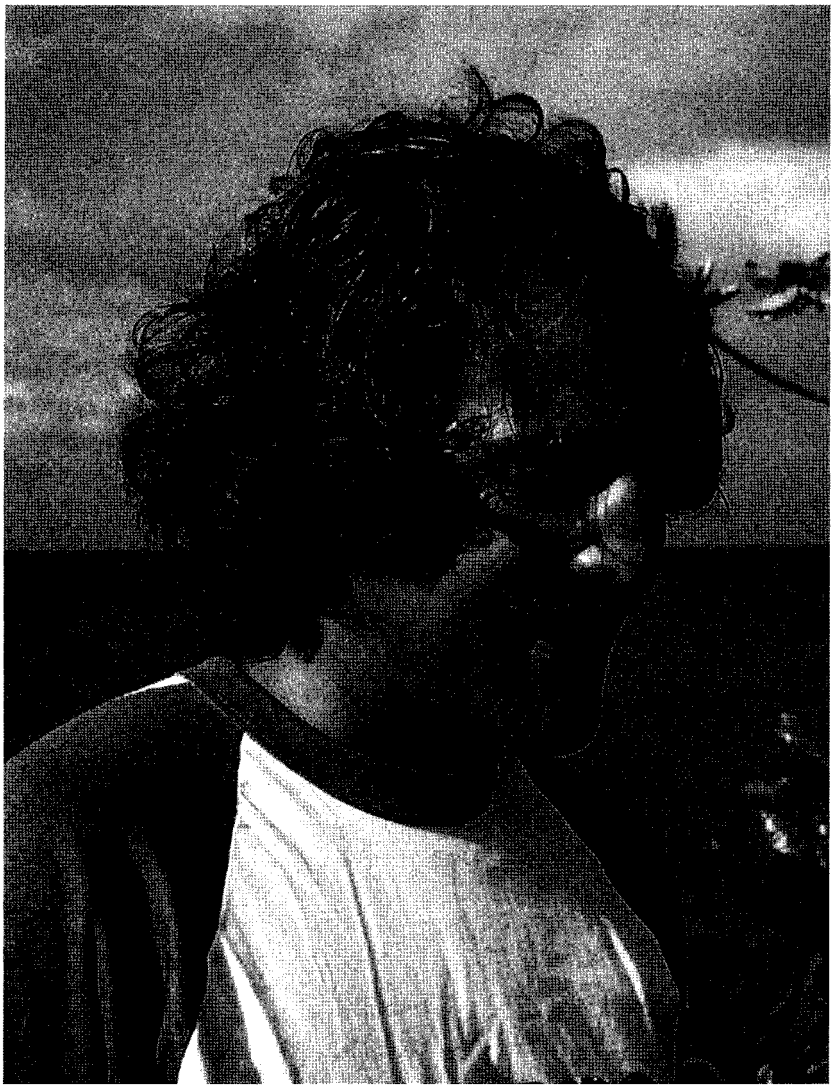

FIGURE 5: **"Lehri" (LMC) speaking with children on the lagoon side of Enewetak.** (Photo by L. M. Carucci 1983)

from Pohnpei, Pingelap, and N̄atik who married in to the community and "stayed put" (*bed wōt*) on Ujelang during the years of hardship and isolation, came to be "Ujelang people" just like expatriate Enewetak people. Ujelang people who had married into the Enewetak community in prior years also

became part of the new cohesive group of diUjelang (though, they could never stop feeling that they were a little "more diUjelang" than other Ujelang residents of the post–World War II era).

If the isolationist and antidevelopment policies of the United States created the conditions for an inclusionary solidarity on Ujelang, it is far more difficult to say just where "the community" lies in the current day due to ever-changing, multidimensional residence choices and an equally diverse group of lifestyle choices. In short, the contours of the group are becoming more problematic for Enewetak people themselves than they once were. Some Enewetak people have grown up in Majuro, having visited Ujelang or Enewetak once or twice, if ever. The heads of these families were among the earliest Ujelang residents on Majuro, and performed a critical community function from the 1950s until the 1970s. Many were founding or early residents of "Ujelang Town," a Small Islands land parcel given to Ujelang people to ease their suffering by a respected Majuro chief. For years, many of these expatriate Ujelang people maintained strong links to Ujelang by hosting an ever-more-voluminous stream of visitor relatives who came to the government center from Ujelang. When Ujelang people were repatriated to Enewetak in 1980, however, the value of Ujelang Town shifted. With outer island air service at least twice a month, short term visits in both directions became commonplace. Equally, however, an increasing number of marriages with Majuro people as well as rental arrangements with Majuro land heads has blurred the identity claims of the out-of-residence Ujelang-Enewetak group. Even though Ujelang Town remains overcrowded, many Enewetak people on Majuro do not even visit Ujelang Town during their time in the government center. For those who reside on Enewetak, living on the land, transforming it through work, and becoming one with the soil after death, are critical identity markers. In most cases, these criteria are not fulfilled by Enewetak people on Majuro.

In addition to the Majuro "Enewetak" group, a subcommunity of Enewetak people has established itself on the Big Island in Hawai'i and as many as one-fourth of all Enewetak live there. As has long been the case for Enewetak people (Carucci 1993, 1999), a substantial component of local identity is interwoven with residence and with caring for the land. Given these indexical ties that posit a primary identity link between a people and a place ("the people of Ujelang") many Enewetak residents in the Marshall Islands refer to Enewetak people on Hawai'i as diKona "people of Kona," "Hawai'i people," or "people of the Big Island." The first Big Island residents departed from Enewetak in 1990. All of the current leaders of the Big Island community were born on Ujelang or Enewetak and most have spent at least a substantial part of their lives in the Marshall Islands. As yet there is little

question about their Enewetak-ness, though discourses are changing. Many children have been born on the Big Island, giving them U.S. citizenship. Others have died and been buried in Ka'u, embedding their substance in this new land. A large number also own homes in Hawai'i. The currently diversified discourses and altered practices will inevitably lead to a reconsideration of what it means to be an Enewetak person.

There are others with ties to Enewetak people who are much further on the fringe of community membership than the Enewetak residents of Majuro or the Big Island. Their only true measure of being an Enewetak-Ujelang person is that they receive a share of nuclear compensation funds. Not only are these members not resident on the atoll, they speak neither Marshallese nor an Enewetak dialect, have never participated in the day-to-day life of the community, and either never have visited the Marshall Islands or have visited for a few days two or three times in their lifetimes. In essence, they are nonmembers who receive compensation payments only out of the kindheartedness of core members of the community and out of their own lack of shame. Indeed, in my own estimate, Enewetak people need to give serious consideration to the underlying reasons for including these folks as community members, since the primary criteria for inclusion is American (blood quanta) rather than Marshallese.[10] Through their inclusion, the community faces the same risk as Native American and Native Hawaiian groups: membership is defined not by performance (those who demonstrate community commitment through lived activity), but rather by an arbitrary Euro-American criteria unrelated to cultural integrity (one-sixteenth "native blood"). Most critically, in the Native American case, blood quanta has often become a mechanism through which cultures are legally recognized or declared comatose, even though measures of blood may be of no significance to local people as they define their own cultural viability.

My own membership as an adopted member of the community is often asserted by long-standing Enewetak-Ujelang people by drawing contrasts with this non-Marshallese-speaking, nonresident, group. Indeed, further extending their virtually unlimited generosity, many have suggested that I should clearly receive a "a bite" (share) of compensation funds given the current mode of division. While I have declined, their suggestion of my inclusion supports my contention that it is shared, lived activity, not blood quanta, that is the measure of community membership. My own membership lies somewhere at the fringe of ordinary community members primarily because I spend a major part of each year as a professor in Montana. Not surprisingly, they suggest I should receive compensation benefits befitting a community member with increasing frequency during the times when I live in the community for the most extended periods of time. In their discourses,

living with, and in the same manner as, Enewetak-Ujelang residents, makes me as Enewetak-like as possible.[11] And, of course, this is but their telling me precisely what I am attempting to tell in this paper. While I speak only with my own voice, sharing experientially in the community for long periods of time over two and one half decades allows my voice to resonate with the increasingly varied voices of Enewetak people living very different lives.

Under Construction: Ongoing Experiments in Identity Formation

Life in the Big Island Marshallese community in Hawai'i provides important clues to coming shifts in the construction of identity. Even though current Enewetak residents of the Big Island unquestioningly refer to themselves as diEnewetak, their daily activities and relations to this land are considerably different than residents of Enewetak or Ujelang. In Hawai'i, people often work in hotels, small businesses, or for young Marshallese, at McDonald's. Others make a living performing day labor on coffee and macadamia nut farms or work clearing overgrown parcels of land. Following an original settler's lead, Enewetak-Ujelang people began to purchase land in Ocean View, Ka'u, in 1995, but they have not yet begun to speak of their fee-simple purchases with the same representations of attachment that they reserve for Ujelang and Enewetak. On the other hand, they say they are here for the long term, and they are working to embed elements of their identity in this new land (see Carucci 2002). Clearly, as the Big Island community continues to increase in size, it will develop a dynamic that will recontour people's thinking about themselves, both on Enewetak, and in other locations where "Enewetak people" now live. For now, however, expatriate Marshallese work far harder at manufacturing continuities with the homeland than they do at stressing their differences from those who have chosen to remain on the lands of their ancestors. Equally, while complaints may be heard about those who do not reside on Enewetak but still receive the benefits of local residents, those who reside in the homeland continue to maintain rules of community membership that are near their limits of maximal inclusion. These flexible forms of constructing community allow ample opportunities for people to argue for their own place as a "person of Enewetak." While shifting discourses and altered practices will inevitably lead to a reconsideration of what it means to be an Enewetak person, it is my hope that community members will recognize the limitations of basing such claims solely on formal criteria like residence or blood. When they lived on Ujelang, people dreamed of how desirable life once was during their youth on Enewetak. Nearly thirty years later, on Enewetak and the Big Island alike, many adults speak in nostalgic terms about life on Ujelang (in spite of its hardships). If these sentiments are

renewed in years to come, in order to fulfill their most "heartfelt" desire[12], Ujelang-Enewetak people must fashion their own identities not out of measures of blood quanta, but rather out of the constituents of lifestyle that, in their own images of the past, make life on Ujelang (and a yet earlier life on Enewetak) so desirable. These are grounds familiar to anthropologists of the long term who can only through the perpetual sharing of lived existence, come to feel, and hence to know, the experience near.

Conclusion

In my attempt to situate accounts of Enewetak-Ujelang people and the shifting contours of what their varied classifications of me as an Enewetak person may mean, I have attempted to show that several interdependent, taken-for-granted, concepts of anthropological understanding are inadequate descriptive devices for ethnographic research that spatially spans continents and temporally spans decades. "Culture" itself suggests a bounded, monolithic thing and "the field" is, all too commonly, used to refer to that place where an exotic culture may be discovered. The ethnographer, often depicted as a space/time traveler engaged in field research, becomes the hero who reveals the true way of life of unknown and exoticized others. Yet, as much recent work has demonstrated, culture is dynamic and ever-emergent in lived social relationships, not a thing to be captured as the momentary fulfillment of unrequited desire. "It" shifts temporally, is internally multifaceted, and has multidimensional forms that are reinvented in new locations according to historically emergent conditions.

As manifest in experientially embedded relationships as well as in discourses/feelings and lived practices, cultural meanings cannot be detached from their constitutional settings. Capturing the ethos or world view of the other can never move beyond wish fulfillment. Experience-nearness, however, attained through sharing the life-conditions of others for years and decades under many differing circumstances, empowers ethnographers' own voices by allowing them to speak of their own lives, their own views, and their own feelings from the depths of their respective beings. Commonalities of practice, shared life's activities, and ways of doing and being, are precisely what Enewetak people capture in their inclusion of me, upon the many occasions they choose to do so, as a piece of their own collectivizing identities.

I wish to reiterate that in no sense do I begin with the naive assumption that I have, in fact, become the native and therefore, that I speak for all Enewetak people. I speak only for the transcultural person that I am, a persona that allows me to speak "from experience(s)" about what it is like to live an Ujelang-Enewetak existence for many years and, at the same time,

allows me to weave my authorial voice(s) into the documents I produce as an ethnographer. The dyadic (racialized) "othering" that an author like Trask uses to posit her own indigenity in opposition to anthropological authors—in her case, Keesing and Linnekin (Trask 1991), seriously oversimplifies and obscures these intersubjective connections and authorial conventions. Posing as the metonymous voice of Hawaiian people in opposition to the colonialist, anthropological other, Trask conflates the differences that link her to specific Hawaiian settings and persons and separate her from a wide variety of others. While Trask's comments serve their own political purposes, and their own identity-fashioning aims, her rhetorical strategies are simply inadequate to account for the diversity of lived experiences or intertextual propositions and interpretations that typify human encounters in the current day. But, if dyadic opposition fails to allow Trask to capture the complex admixtures of identity that are typical of Pacific residents and researchers today, anthropologists need to listen closely to her advice about differential relations of power. All too often anthropologists rely upon the guise of objectivity as a false rationale to avoid personal involvement in the political struggles of local people. Nevertheless, as Rensel and Howard, like myself, have discovered, living with local people over the long-term necessarily requires political engagement. Ongoing, experience-near, work with other people provides neither a space to avoid conflicts within a local setting nor a position to avoid commitment to and advocacy of moral issues of concern to one's fellow community members. Each interpersonal encounter must now be negotiated with special acumen in order to seek resolutions and solutions to dilemmas in the border regions of varied cultural spaces that, out of long historical practice, have been kept separate and unequal by declarations of difference that have proven unjust, and have certainly outlived justification.

If long-term research requires us to rethink a whole set of anthropological conventions and research practices, as suggested above, it also implies concomitant shifts in representational methods. As selves social to the core (as Bourdieu contends) authors never write as disembodied beings. Whatever messages an ethnographer may wish to convey, each author's rhetoric and inscriptions are, inevitably, representations of themselves (also see Peirce 1931). Experience-nearness, thoroughly embedded in one's being over the long term, provides a method, indeed the *only* method, to approach Malinowski's visionary and enigmatic quest: to capture the other person's view of his/her world. In lieu of the possibility of being able to overcome the inexorable symbolic gap between self and other, representation and object, type and token, long-term research with its multiple re-"turns" to multifaceted "fields," provides the surest way of "standing" in a position from which context-bound, intersubjective, (under)stand-ings can be conveyed.

NOTES

1. On this point, I differ substantially from the perspective of Royce and Kemper. In trying to bridge between anthropologists who have returned to the field many times and multi-generational field projects the editors of *Chronicling Cultures* seek a stance in the artificial space between the "objective observer" and the subjective participant and participant/advocate. They note:

> The ethnographers represented in part I are clear examples of a shift from observer to active partner and, in some instances and in some cases, to advocate. It is important here to remind ourselves that we never abandon the stance of observer; doing so robs our interpretations and recommendations of validity because they are then based on opinion and reaction. As Geertz (2000: 39) suggests, we are always seeing society as an object and experiencing it as a subject. (2002:xxii)

Royce and Kemper wish never to abandon the "objective" stance of the observer (which, I would argue is *different* from "seeing society as an object"). I begin from very different assumptions (though they can also be read into Geertz): that all knowledge and experience is intersubjective, and objectivity is a self-empowering myth that obscures the specific details of intersubjectively grounded knowledge. Given these assumptions, many returns to the field inexorably lead to the possibility of greater understanding, which itself requires positioned stances (see Howard and Rensel, this volume) and enables active partnerships.

2. Bourdieu has another way of addressing a slightly different perimeter of the same mind:body dichotomy in his contention that social practices cannot be divorced from symbolic forms (1977).

3. Silverstein and Urban remind us that texts are always "metadiscursive notion(s), useful to participants in a culture as a way of creating an image of a durable, shared culture immanent in or even undifferentiated from its ensemble of realized or even potential texts." For this reason, they refer to transcriptions as "text-artifacts"—reminiscent "of museum specimens that can be transported back from the field and evaluated for their authenticity and cultural-aesthetic authoritativeness" (Silverstein and Urban 1996: 2–3).

4. Some said these knives had "disappeared"; others said I had "given them away." No one contended they had been filched, an important point in relation to their interpretations of my actions.

5. A typical deep-interpretation section begins: "The !Kung have little privacy, either in the village or within the family dwelling. Parents and children sleep together, sharing their blankets, in small one-room huts that have no dividers or private sections. Adults try to keep children from noticing their sexual activity" (Shostak 1981:105). Yet each generalization, the quintessence of standard ethnographic interpretation, leaves the reader with other questions: "When do !Kung have privacy?" "Under what conditions?" "Would it be in the village or in the dwelling?" "Do !Kung ever meet in the bush?" And, on closer inspection, is the very idea of "privacy" a salient Ju//hoan category, something !Kung might try to seek

out, or is it Shostak's feeling about something that she desired to have in the field, a desire she sought to fill with limited success? My guess is that privacy is a category directed at her readers, aimed at causing them to question the cultural disjunctions Shostak felt, even though it is phrased as a "thing" that !Kung do not have but, themselves, desire.

6. This is not to say that traces of his former selves do not remain inscribed in his current demeanor, nor that, in various hidden ways, his earlier persona did not manifest elements of the leader he would become.

7. In a parallel move at the national level the high-ranked Rālik chief, Imata Kabua, had lost the election that would have allowed him to continue as president of the Republic of the Marshall Islands. Many voters lost confidence in the president for reasons similar to Enewetak voters' distrust of Naptali.

8. My research visits during this lengthy period include twenty-five months on Ujelang Atoll with the exiled Enewetak-Ujelang people from 1976 to 1978, thirteen months on Enewetak in 1982–1983. Several months on Majuro, Kwajalein, and Enewetak in 1990–1991, an extended stay on Kwajalein in 1995, and numerous shorter visits (typically one to two months) to Enewetak, Ujelang, Rongelap, Bikini, Utdik, Majuro, and Kwajalein between 1990 and 2001. In 2002–2003, I spent approximately seven months living and working with Ujelang-Enewetak people as well as other Marshallese on the "Big Island" of Hawai'i. I returned to the Big Island in 2006 for another 2 months of research. My thanks to the National Science Foundation, the National Endowment for the Humanities, the Host Nations Program on Kwajalein, the Pacific Health Research Institute, Montana State University Scholarship and Creativity Grants Committee, and the Enewetak-Ujelang Local Government Council for funding these research opportunities.

9. Enewetak people were psychologically traumatized as they watched the 1952 "Mike" test vaporize segments of their homeland. Rongelap and Utedik people both suffered devastating medical effects when the 1954 "Bravo" test on Bikini showered them with fallout. Bikini people, like their Enewetak cousins, also endured substantial suffering during their years in exile from their homeland.

10. Marshallese criteria include such things as: spending time with, helping, living with, working with/on, sharing, investing labor in, consuming products of, embedding one's own substance in, etc. Being "born to" is a qualifying criteria only if it is supported by other identity-solidifying symbols, such as those mentioned above.

11. While such statements are fairly frequent, nowadays, at a time when I typically reside on the atoll for about a month or two every couple of years, they were quite infrequent in the past, when I was in residence for a year or two. These are contextual issues, of course. In the past, after months of coresidence, "who" and "why" questions about my position in the community were pragmatically quite evident, and statements about my being a "true Ujelang-Enewetak person" typically were made by Enewetak people speaking with other Marshallese. The same is true today, though I have also heard established members of the community make such statements when speaking with young children. In part, the children do not share the long life's experience of mature youth and adults but, equally, changes have brought a much broader array of "others" into the community, most with

only fleeting associations. Essentially, young children are asking "why is this guy different from other 'white, clothed beings' "?

12. I translate very roughly since, for Marshallese, desire and other feeling states rest in the throat rather than the heart.

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RETURNING TO THE FIELD: I'M OLDER AND THEY'RE WISER

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Several processes of change and of human development affect the process of fieldwork over time. The concerns, issues, interests, relationships, coping strategies, motivations, and even personality of a mature adult are not the same as those of a younger adult. Consequently, fieldwork conducted on Pollap Atoll in Micronesia in 1998–1999 differed from dissertation research conducted in the same place eighteen years earlier. Changes since 1980 in the Pollapese way of life, their relationships with outsiders, and their understandings of anthropology contributed to a different experience, as did my heightened sense of confidence, decreased anxiety, and increased faith in participant observation.

RETURNING TO THE FIELD occurs in a particular context involving the interweaving of several processes of change and human development. The discipline of anthropology itself changes over time, as theories wax and wane, and as new approaches to the conduct of field research surface; these influence how research questions are formulated and subsequently investigated. Obviously our field sites undergo social and cultural change over time, which also affects the fieldwork experience. Yet it's not just culture and anthropology that evolve over time: our friends and consultants in the field change and mature, as do we, and as do our relationships with those friends and consultants. Furthermore, studies of long-term fieldwork also indicate that our stance toward those friends and their way of life shifts over time: it is not uncommon for ethnographers to begin their careers as observers in the field and then move toward becoming more active participants, and in many cases outright advocates (Royce and Kemper 2002).

Pacific Studies, Vol. 27, Nos. 3/4—Sept./Dec. 2004

Many studies in the past twenty years have analyzed the influence on fieldwork of ethnographers, personal aspects such as gender (Bell, Caplan, and Karim 1993; Golde 1986; Whitehead and Conaway 1986) and family situation (Butler and Turner 1987; Cassell 1987; Flinn, Marshall, and Armstrong 1998). We also need to recognize that a fieldworker's package of personal characteristics shifts, develops, and matures with time. The concerns, issues, interests, relationships, and coping strategies of a mature adult are not the same as those of the younger adult. We move through different stages of our careers and our family life, we develop new intellectual interests, we shift social roles, we adjust our sense of identity. Even our motivations for fieldwork may evolve with time. All contribute to shaping the fieldwork experience.

Mature adults are in a different developmental stage of the life cycle than youths,[1] and a number of studies indicate that adults at this later stage are typically at the peak of their careers and have often acquired more self-confidence, a heightened sense of control, more mature coping styles, more stability, and a stronger sense of well-being (Clark-Plaskie and Lachman 1999; Diehl, Coyle, and Labouvie-Vief 1996; Helson and Wink 1992; Mroczek and Kolarz 1998; Whitbourne and Connolly 1999). Research suggests that for many Americans "midlife is a time when people function particularly well relative to those who are younger or older" (Keyes and Ryff 1999:169). Thus it should have been no surprise to me—though it was—that fieldwork I conducted at midlife would contrast so dramatically with fieldwork I conducted as a young graduate student. Even in the absence of other changes, human developmental processes contribute to changes in how a mature adult experiences fieldwork compared with a young adult. An analysis of how these various processes of change and human development affect fieldwork over time can help illuminate the process of field research in general, providing us with a richer framework for understanding the various factors shaping our research and its products. And perhaps novice ethnographers may be able to avoid a few of the pitfalls of their predecessors and not have to wait twenty years to acquire at least some of the benefits of that experience.

I first went to Pollap Atoll in Micronesia in 1980 for dissertation research and then returned for another eleven months in 1998–1999. During my initial period of fieldwork in 1980, I had little thought about future field research on Pollap and certainly no long-term strategy for an ongoing or even intermittent commitment. That process simply evolved over time as I found myself looking for ways to continue my research with Pollapese. My experiences on Pollap in 1980 and 1998–1999 and decisions surrounding them were colored by other visits to Micronesia, both before 1980 and between the two trips to Pollap. My choice of research sites, questions, and even to a certain extent my methods have been affected over time by personal con-

cerns, previous experiences, and the state of the discipline of anthropology. Two years of Peace Corps experience in Micronesia before beginning graduate school gave rise to my choice of Pollap as a research site and shaped the questions I later wanted to ask; each subsequent decision was in turn influenced by earlier experiences. Once back on Pollap in 1998, I found that changes in me over time (changes I like to think of as growth), changes that had come to Pollap in the intervening years, and probably changes in anthropology as well resulted in a year far different from what I had anticipated or even hoped for. In some ways it was more demanding, yet in other ways more rewarding and less stressful.

Dissertation Fieldwork

I chose Micronesia as a field site when pursuing my dissertation research because of earlier experiences as a Peace Corps volunteer from 1974 to 1976, though I wasn't on Pollap at the time. Instead, I lived on Namonuito Atoll, which lies north of the Western Islands, where Pollap is located. My husband and I taught English as a second language at a secondary school for students from Namonuito, the Westerns, and a few other islands. As a result I became interested in issues of education, its impact on change, the role of kinship in the midst of change, and how the more traditional islands were coping with those changes. I became particularly interested in learning more about the Western Islands as I listened to people speak of them as the most "traditional" in the area (which was Truk District at the time and later Chuuk State, part of the Federated States of Micronesia). The acting principal was from Pollap (which was then spelled Pulap), and he readily and consistently captivated me with stories of his home island. I should also admit that another factor influencing my decision was that some of my favorite students were from Pollap. I was further intrigued when I had a chance to briefly visit some of the Western Islands, though not the atoll of Pollap itself. By the time I left the Peace Corps, I had decided that if at all possible, Pollap was the site I wanted for my dissertation research in order to pursue some of the questions I'd become interested in while teaching on Namonuito.

In a graduate school seminar on preparing for fieldwork, I felt as though I were approaching my research backwards, however. I already had a site in mind and some issues that intrigued me related to that site. But this contrasted with the model presented in the seminar of beginning with a research question and then selecting the site most suited to answering that question. Moreover, at the time I did not recognize my choice as the beginning of a long-term relationship with Micronesia; I only knew that I wanted to return to Micronesia for my research and that I hoped it would be Pollap in particular.

I was successful in doing so. Accompanied by my husband and my son, who was about a year and a half old when we arrived, I spent most of 1980 conducting dissertation research. I lived about nine months on Pollap with both husband and son, and then with just my son for a few more months with Pollap migrants on Weno (then known as Moen), the main island of Chuuk.[2] Some aspects of our arrival on Pollap were akin to going "back to the field" even then, since we were returning to a general area we were familiar with, and since many of our former students had become young adults on Pollap by 1980. We'd been gone less than four years, so their memories of us were still fresh. In addition, the island community as a whole seemed pleased that I had chosen Pollap rather than our Peace Corps site of Namonuito. They also appeared impressed with the fact that we'd brought our son to their island, which they interpreted as a sign that we trusted them and their way of life.

Otherwise, most of my memories as well as my journal entries dwell on stress and anxiety. I was shy and insecure, and at some level, I realized my whole future was at stake with the project I was undertaking.[3] Until I returned to Pollap in 1998, I had never recognized how much the pressure to produce a good dissertation had contributed to my anxiety. Nor did I realize how little faith I'd had in eventually getting valuable data through "hanging out" or just participating in activities as opportunities presented themselves. I certainly participated in community life, but whenever possible in1980, I seemed to focus on interviews instead, a method that probably contributed to stress because I felt so timid. None of this did I fully realize until I was able to return.

Returning

It was eighteen years before I returned, in part because Pollap seemed to be so inaccessible. The only access was via ships that followed no reliable schedule, which meant that it could be several months from one trip to the next. I could pursue questions of migration and other topics, however, among Pollapese living on Weno, and that's the strategy I pursued in the summer of 1986 and again in 1989. Depending on who was in town at the time, I was able to rekindle some relationships and at least hear news of other people and learn about changes and developments back on the island. Several summer sessions of fieldwork among Carolinians on Saipan also brought me into contact with Pollapese. Like Howard and Rensel (this volume) and unlike Barker (this volume), I had not originally anticipated developing long-term relationships with certain Pollapese. It was not until I had my first chance to return to Chuuk in 1986 that I even realized how much I valued the friendships I had

made and how much I yearned to develop them further. I had secured a tenure-track teaching position, with ongoing research as one of the expectations. Returning at least intermittently to Micronesia clearly seemed feasible.

Eventually I was able to indulge the longing to return to Pollap itself and to have more than a few summer months for research. I decided to return to Pollap for a year's sabbatical leave during the 1998–1999 academic year. Accessibility was then just a minor problem, since I had a year rather than only a few weeks or a summer. I very much wanted the chance to get to know people on Pollap again, learn more about women's lives, deepen my understandings and my relationships if possible, and hone my language skills. Perhaps even more important, however, I had remarried, and I hoped my husband could experience Pollap firsthand. Like Alan Howard (this volume), my reasons for returning had personal considerations that came into play. Yes, I had a research agenda, but what mattered perhaps even more was that I be able to return to Pollap, reconnect with friends I'd made, and introduce my husband to a place and way of life that had played such a major role in my personal and professional life.

Furthermore, my experiences back in 1980 shaped some of the questions I wanted to pursue and the methods I planned to use. I had vivid memories of anxieties about many of the interviews I pursued, especially those with men. Therefore, I wanted a project that involved women (an interest that had grown over the years) and one that would allow me to focus more of my energies on participating in activities rather than primarily conducting interviews. I also wanted to use some structured methods I'd learned at a National Science Foundation summer seminar.[4] Not only was I intrigued with their methodological and theoretical value, but I also saw these approaches as somehow less stressful and anxiety-creating than what I had done before. Perhaps I just had more confidence and experience, but soliciting free-listings and asking women to sort the results into piles appeared both useful and nonthreatening.

Even though I regularly had had news of the island and of people I had developed relationships with during my dissertation research, I had not been able to continue nurturing most of those relationships because the population of Pollapese migrants in the port town and on Saipan varied so much. Returning to Pollap after eighteen years would mean experiencing a number of changes. My "back to the field" experience doesn't fit neatly into either an "intermittent" return or a "regular" return category but has aspects of both. I had not been to the atoll for eighteen years, which puts my experience in the "intermittent" category, but I had maintained some connection with the community and had received at least some regular news. I had returned from time to time to "pieces" of the Pollap community, but not to the home atoll.

Change on Pollap

Changes since 1980 with the Pollapese way of life, including their relations with outsiders, certainly contributed to a different fieldwork experience and had an impact on my research and on local attitudes people held about me.[5] I was prepared for a certain degree of change, especially in material conditions, and I knew that many more young people were pursuing school and work off-island, especially on Guam and Saipan. Yet I have to admit that some of the changes saddened me. For example, the sailing canoes that had seemed so central to the Pollapese way of life and sense of identity in 1980 had practically disappeared. A single one remained, and the only time I saw it used was in connection with tourism. In the same vein, most of the dancing that was performed during my return visit was also connected with tourism. Social problems that Pollapese used to see as being confined to the port town were also looming larger on the island. These were problems such as young men fighting when they drink, which had not been a concern eighteen years earlier. Pollapese used to feel insulated from such problems; in the past these social ills had plagued other peoples in Chuuk but not them.

In general, Pollapese have a pragmatic attitude about much of their material culture. To the extent that they find something beneficial about outside items, they adopt them, while they often continue to use, adapt, or even readopt older ones. Most homesites in 1998–1999, for example, included what were viewed as "modern" as well as "traditional" structures. Most of the modern ones were concrete houses, which provide better shelter than thatch houses in the event of storms. They were hot, however, with the sun beating down on metal roofs, whereas the thatch houses were cool. The thatch ones tended to be close to the shore, where they could catch the breeze, and the modern ones were a bit inland, where they could provide more shelter from storms. Families with the money to buy and maintain motorboats (and keep them supplied with gas and oil) were also likely to have a small paddling canoe or two especially for when they ran out of gas or the motorboat broke down.

Although Pollapese were still proud of certain older practices, especially those connected with respect shown to senior siblings, they were more ambivalent about "tradition" than in earlier years. Many more islanders had been away at school or working off Pollap and even completely outside Chuuk on Guam, Saipan, Hawai'i, and the mainland United States. From the very beginning, I noticed far more concerns about outsiders looking down on them and perhaps seeing them as "primitive" and "dirty." In addition, community leaders were pushing development projects such as a desalinization pump. Islanders were in general much more aware of outsiders, other possibilities, and other attitudes about "tradition."

Pollapese had also had more experience with outsiders living on their island over the past eighteen years. Other Peace Corps volunteers had lived with them during that time, including several women. What had the most impact for me, however, was the fact that a Japanese anthropologist had been on Pollap several times since I had been there in 1980, and he worked with what sounded like a fair amount of grant money and a different research style. According to Pollapese, he paid people for old stories and medicinal recipes, he hired assistants to collect information such as names of plants, and he gave money to others for household assistance such as washing dishes. I had done none of that in 1980 and had no plans to do so in 1998–1999, at least not on a major scale.

The work of the Japanese anthropologist and the increased awareness of outsiders had an impact on local attitudes about my research, including more detailed interest in what I was doing and a more knowledgeable understanding of the work of anthropologists. In 1980, I organized a community meeting to explain why I was on Pollap and what I was planning on doing, but I don't remember people having any questions or even much interest in the particular details. In 1998, however, there was a vast difference. Almost immediately I was asked to attend a council meeting, and its members asked me questions for hours. They were much more aware of what anthropologists do and of the interests of foreigners. Outside the council meeting, I also had more islanders asking me similar questions when I went visiting. A number of the questions were quite sophisticated, not just a vague "what are you doing here?" They were genuinely interested in the details of what I wanted to learn and why, and how I intended to proceed. In my journal from the day of the council meeting, I wrote that "I was particularly taken with questions about keeping their customs and contending with change and what the consequences could be—the questions of an anthropologist."

One particular question I hadn't anticipated had to do with why I was there *again*. Hadn't I learned everything the first time? Perhaps the Japanese anthropologist had made it clear that he'd come for different purposes each time. Islanders spoke of one visit of his to film an initiation ceremony for navigators, and they spoke of his bringing students another time. So, unlike my first visit in 1980, when I detected little interest in the details of my work, I spent considerable effort the second time explaining what I wanted to do, how I planned to do it, and how it was different from—though related to—what I had done before.

The stickiest issue, however, was about money. For quite some time on my return I was regularly asked if I would be paying people. I hadn't planned to, since I had only a small grant that paid for transportation and supplies such as paper, notebooks, and film. Nor had I brought much money with me.

I explained all of this the best that I could. In 1980 I had some grant money for paying people, which I had done in the form of food, coffee, shampoo, and the like. It seemed to be a culturally appropriate way of interacting, since giving and sharing such items were already a part of their way of life. At the time I was concerned that paying money for information would commercialize and distance relationships. In addition, I never explicitly spoke of giving money or a gift in exchange for information.

For my 1998–1999 return trip, I had already planned on volunteering to teach English classes at the elementary school (since teaching English as a second language had been my job in the Peace Corps) as at least a partial way of repaying the Pollapese for their hospitality, time, and data. My husband decided to teach as well. We also gave gifts, and as often as possible we provided other items when asked. In the end these all turned out to be good choices. Through my activities at school, I became well acquainted with the teachers, and many turned into wonderful informants. In fact, it was a relationship with one of them that led to an invitation to sit in on meetings of the traditional council, which was otherwise normally restricted only to men. Parents seemed to appreciate our efforts, and that helped to establish rapport with many of the adults on the island. But what mattered most to me is that Pollapese seemed to see value in what we were doing and interpreted our teaching efforts as a sign that we cared about them. I found this to be one of many activities that did not explicitly involve "collecting data" which nonetheless proved valuable in countless ways, not the least of which was *increased* access to data. In 1980, in contrast, I had been a bit leery of taking time with activities that did not appear to be directly related to my research.

Many more outsiders had visited Pollap over the years, and during 1998–1999 a fair number arrived while I was there. For example, a cruise ship and dive boat both called at the island. Some Koreans visited for short periods of time in connection with installing a desalinization pump and building a gym. A canoe from Hawai'i with a Micronesian navigator, Hawaiian students, and journalists arrived for a couple of days. Pollapese seemed much freer about discussing outsiders and making explicit comments. It had seemed to me back in 1980 that I had to probe to get information about former Peace Corps volunteers and the few other outsiders who had been there earlier. This second time, people freely told stories and made comparisons. One woman volunteered differences she'd noticed in me between my two visits, and by the end of the year, I even managed to feel comfortable with their comparisons of me to the Japanese anthropologist. We heard that people had enjoyed having us on the island and (as recorded in my field notes) "people realize that we don't have money the way [the Japanese anthropologist] does—and that the work at the school is equivalent to a lot of money."

Unexpected Differences

I arrived on Pollap in 1998 prepared for some changes in the way of life, in the look of the island, and in many of the people, and I had consciously prepared a different project and methods. Nonetheless, I was unprepared for *how* different the experience would be. Some changes that I had never really expected—though should have—turned out to be changes in me. I was eighteen years older, more confident, less shy, less worried about looking foolish—and my career was not at stake as it had been in 1980. These changes had an impact on my relationships with people and how I conducted my research, even beyond what I had already explicitly planned.

Being eighteen years older, I did assume that I would be seen as more mature and therefore classified as an "adult." In my 1980 journal I discussed worries about being perceived as a mere "girl" instead of as a "woman" and not taken seriously. I wrote, for example, about an event during which I deliberately avoided sitting with some younger women for fear I'd be associated with the "girls." What I hadn't taken into account on my return was the fact that I *had* matured and grown in a number of ways, and those changes would affect the conduct of my research. I was far more willing to risk doing something that would make me look incompetent or foolish. One consequence was that I was much more comfortable participating in more activities. I'd been somewhat passive the first time, except in pursuing questions and interviews. Back then I watched, listened, and "hung out," but waited to be asked before engaging in most activities. On my return, however, I explicitly asked to help garden, hunt for octopus, assist with cooking, learn how to weave mats, make roofing panels and baskets, attend massage school. I wasn't very good at any of it, which had bothered me in 1980 but no longer seemed to matter. The islanders themselves didn't seem concerned with my incompetence either, except for worrying that I might cut myself or get sunburned.

My journals from 1980 are sprinkled with comments about feeling "overwhelmed," "nervous," or "shy," and keenly wanting "courage." Other comments reveal insecurities that, along with some of my shyness, had faded over the years. I worried in 1980, for example, that "I won't get decent information," and I wrote about being frustrated: "I want to be taken seriously. . . . I still felt [today] as though I were playacting . . . at being an anthropologist." Those feelings eased somewhat over time during my first fieldwork, but they still were a stark contrast with my return experience. I often wrote in my 1998–1999 journals about "rich," "full," and "exciting" days. Even toward the end of 1980, when I had become at least a little more comfortable, I wrote, "You have to learn how to poke your nose into other people's business in

order to be an anthropologist, and that may be what gives me my qualms" and "I think it's the formal interviews and planning for them that make me nervous." I hadn't changed so much in the intervening years that I never felt any uneasiness on my return, but eighteen years nonetheless made a significant difference. It seems odd now that I didn't anticipate the change, but my 1998–1999 journals are full of remarks about the unexpected differences between the two experiences and how much more I seemed to enjoy my return and its relative absence of stress.

It's not that I never felt uncomfortable. In a very early 1998 entry I wrote, "I feel overwhelmed—like I have to do this all over again! . . . The place has sure changed—nothing looks like it did before. . . . I need to give myself time. I have months and months." Yet except for intermittent complaints about bugs everywhere and children constantly asking me questions, such negative comments are rare and confined to the early days. Whereas in 1980 I seemed constantly nervous about going out to visit people and almost preferred working on my field notes, the opposite was the case on my return. One day I wrote, "When I finally couldn't stand typing and not getting out, I returned a *hepi* to P., visited with women resting from . . . work, talked with M., visited T. for some dance practice."

Another reason, however, that I hadn't often actively sought to participate in activities the first time was because I was obsessed with collecting data for my dissertation. To do that, I believed I had to be out asking focused questions that specifically related to my research question. I shouldn't waste time with other activities that seemed irrelevant. I even wrote in my journal that "I think I get discouraged when I don't spend a lot of time actively collecting data." When I was hanging out with people, just visiting without specific questions to ask, or participated in activities just because they were going on, I fretted that I wasn't taking notes on answers to particular questions clearly related to my research. Looking over my journals, I found I at least contemplated other strategies: "I'm wondering if it wouldn't be better—and easier—to make things lots less formal and much less like an interview—and just talking. I feel they are uncomfortable with me taking notes and formally asking if I can interview." Nonetheless, I still focused primarily on interviews. I didn't truly believe in the value of engaging in activities with others on the island simply to get to know them better or to keep myself open to unanticipated findings that could prove to be relevant. Although I participated in plenty of activities the first time, I was far more open to them during the 1998–1999 visit, in part because I was more comfortable seeking out such activities but also in part because I wasn't so obsessed with collecting data.

In addition, I found during my second visit that women specifically asked me to participate much more than they had the first time, and we received

many more visitors in our house. One factor could be that the islanders had had more contact with outsiders over the intervening years, including the Peace Corps volunteers and the other anthropologist, and more and more Pollapese had also been in contact with a wide range of outsiders by living on Guam and Saipan. In general, Pollapese had become familiar with American culture through movies and television videos, and through stories of those living in the United States, where some had even made successful lives for themselves. In sum, foreigners and Americans in particular had become far less intimidating, more ordinary, more comprehensible, and certainly more approachable.

Island residents also perceived me as someone used to their food, their ways, and their language, which probably played a role as well. What particularly struck me is that fairly early on women asked if I was planning to dance with them when a party was scheduled that was to include dances as part of the entertainment. A couple of Peace Corps women had danced with them during the intervening years, which probably contributed to their belief that I, too, would be interested. At the time, I wrote in my journal, "So many women have been asking if I'm going to dance. I don't remember that the first time. Did I act differently this time—from the beginning—that set a different . . . type of pattern of relationships? . . . What about the role of my expectations? Different stakes? Decreased shyness? Increased age and all that comes with that?"

In the middle of my return visit, I was also asked if I wanted to accompany a group of women to a nearby island for a workshop. Much as I dread ocean travel because of seasickness, I nonetheless took advantage of the opportunity and attended. I had no specific questions I was seeking answers to and no interviews planned. I simply assumed that I would learn about women's lives and that I would hear talk about women and their role in the church, issues I had become interested in. And I was right. I don't believe I would have been asked to accompany such a group in 1980, and I know I would not have been assertive enough to ask permission to join them.

I also seem to have been inordinately serious the first time, whereas the second time I joked and teased quite frequently. I found not a single reference in my 1980 journals to joking (either doing so myself or being teased by someone else), and I have no memory of it. My 1998–1999 journals, however, are full of comments about joking, teasing, or being teased. I also wrote, "I don't remember joking or teasing people before. Was I really serious all the time? Does this mean my language is better? Or that I'm less shy?" This was probably both a cause and a consequence of closer relationships.

I also pursued opportunities as they arose more than I did the first time, when I was so worried about collecting data. I found in my 1980 journals

that I complained about how church activities occupied much of people's time, making it harder to arrange for interviews. On my return, however, I participated in those activities precisely because they were ones so many people took part in—and something I could do with them. I wanted to be busy, whether or not it yielded hard data. Without the pressure to produce a dissertation, I felt I had the luxury, in a sense, of taking the time to enjoy my year on the island. And in the end those activities pointed me in very valuable directions. I haven't compared pages of notes from the two trips, but I certainly had the sense during the second visit that I was constantly learning and indeed getting data. Granted, I had much more confidence that I would be successful and be able to publish results. Nonetheless, taking advantage of activities, events, and opportunities as they presented themselves—even when they didn't appear to be directly relevant to my research questions—eventually proved to be enormously productive. I wrote something in my journal unlike anything I'd ever written the first time: "Sometimes stuff just comes pouring in that I hadn't expected." This was a far cry from feeling that I was constantly hunting for data.

This casual attitude was possible for me in part because I knew my career was not at stake the way it had been in 1980. One of my goals in returning to Pollap was to have a pleasant experience, without the high degree of anxiety and insecurities of the first visit. And I wanted to feel more connected, deepen my earlier relationships, make new friends, participate in community activities. I had confidence after my years of experience that I would come away with "enough" data and that the data would be useful. I just didn't anticipate it would make such a difference. I found myself amazed as the first weeks and months went by that I wasn't experiencing the same anxieties as before. More confidence didn't account for it all. It truly made a difference that I had a secure job unthreatened by what happened while I was on Pollap—as opposed to my entire future depending on what happened with my dissertation research.

Ironically, I probably ended up with more useful data as a result of not so obsessively pursuing it. It was a far more enjoyable experience as a result. I only wished I'd known that the first time. My 1998–1999 journals are sprinkled with comments about having another "unexpected" day, or in other words, a "good" day. For example, "[This was] an unexpected day. I guess that's one of the fun things about being out here—and about not having structure—seeing what happens, taking things as they come." In 1980 I remember worrying about creating structure for my work and keeping up with interviews, but this time I took advantage of events as they happened. I was unexpectedly invited to a meeting of the traditional council, for example. I attended workshops local women attended, I went to church and to rosary,

and I went to Parish Council meetings. Indeed, I went to anything that came up, secure in the belief that I would learn something useful.

On the other hand, I often went visiting women or sat around with them after rosary or meetings just because I appreciated their company. Even though such behavior meant I could return home with more notes to record, I was nonetheless experiencing some of what Alan Howard and Jan Rensel elsewhere in this volume so eloquently describe about Rotuma: people and relationships coming into the foreground while culture and research questions recede to the background.

I did pursue the planned structured interviews but no longer fretted about daily life getting in the way: "More 'unexpecteds' today. I'm getting to like just seeing what happens, what materializes, even though it means *not* getting to interviews. I just assume there will be time." When possible, I visited women with a set of systematic questions. In addition to a census and collecting details about schooling and work histories for each household, I conducted three types of structured interviews. First, I asked a series of women for free-listings of "women's work." Using the most salient items, I then asked women to sort them into piles according to how similar or different they were and finally to rate the relative importance of the items. Aside from the methodological and theoretical value, I thought such structured interviews with their clear focus and specific activities would also be easier for me to conduct.

I was also more comfortable than the first time behaving more like Pollapese and acting as a member of the community in making both requests and contributions. Except for seeking eggs for my son's birthday cake in 1980, I don't remember requesting food, other goods, or even help with anything except for my research. When I was back on the island the second time, however, I noted in my journal a number of requests we made of our host family. I also made an explicit commitment to both a clan and father's clan, attended their meetings, and made the appropriate donations. When I had physical problems with my back, I asked for help finding someone for traditional massage treatment. In church I also made the requisite offerings—as an oldest daughter, for example, or as a person with a paying job.

At the same time, I didn't mind so much looking different, especially when it would enable my husband or me to play an active role in the community. For example, he and I both agreed to requests that each of us perform a musical act as part of the Christmas events. It meant that I would play a mountain dulcimer and my husband a hammered dulcimer—not exactly local instruments or local music. We would certainly stand out as non-Pollapese. But we were pleased at an opportunity to make a contribution. During Lent, when local people were visiting the sick, elderly, and housebound,

we, too, visited and performed small concerts of traditional American music. Not surprisingly, all those activities helped build relationships with people that made them far more comfortable with me. We visited and played music as a contribution to the community and for our personal satisfaction, not as part of a research strategy, but at the same time the strengthened, nurtured relationships were methodologically useful as well.

Listening to the discussion of others at the sessions of the Association for Social Anthropology in Oceania devoted to returning to the field, I began to consider the possible impact of changes in anthropology over the years, although I hadn't consciously considered the issue while in the field. With the neutral, objective observer revealed as a myth, was I perhaps more comfortable being "positioned" in 1998 than I had been in 1980? I was certainly more open to and interested in sharing what talents or abilities I had. I spent time helping with English classes in the elementary school, and I agreed to serve as liaison and translator when a cruise ship and tourists visited the island. This tendency — or urge — to contribute and to help appears to be a common experience among the contributors to this volume.

I also noticed I spent more time with elderly women. When I visited older women in 1980, I almost invariably asked a friend and key informant of my own age to accompany me. Having more confidence that comes with experience and age made me feel comfortable with older women during the second visit, and I visited two of them in particular quite regularly. Even when I visited for no particular reason, I usually left with new information or some new insight.

In general I was just plain happier. From early on in my stay, I had a sense of good relationships with people: "I feel a sense of connectedness." On a later date I wrote, "There were a couple of moments today when I had the sensation 'This is where I want to be right now.'" About halfway through, I even started having nightmares that I had already left Pollap and returned home. When I received mail, I felt uncomfortably jolted out of my Pollap life and frame of mind, whereas my 1980 journals reflect on how homesick letters made me feel. At one point during the second fieldwork session, I realized that this surprised me, because in many respects I'd left much more at home the second time than I had the first time. In 1998 I had left a job, anthropology program, house, and son back home (i.e., to worry about), none of which had been the case the first time. When I could have taken a boat off the island at about the time I'd originally hoped to do so toward the end of the academic year in 1999, I didn't—even though there was no definite alternative in the future before my airplane tickets expired. Quite simply, I wanted a little more time.

During my one difficult period on the island, I discovered that I had a local social-support network in place to help me cope. My college-age son

had planned to visit at Christmas, and I was close to panic when I learned he had not arrived in town on the airplane as scheduled. A number of women provided emotional support through that time, and several of them also provided material support such as a motorboat to transport me to a neighboring island and reach a ship going into town. Soon enough I discovered my son was fine, but I was nonetheless intensely disappointed that he wasn't going to be able to visit, and again women of the island rallied around me. I don't have a comparison with the first time, but that experience gave me the sense that I had developed more personal relationships than before, and with more women.

I'm not quite sure of all the reasons, but my pattern of relationships, especially close relationships, was quite different from the first time. I assumed that since I had spent time on the island before, I could more or less slip back into the old relationships. In 1980 I felt to a certain extent that I was treated as someone "returning" since I had been in the Peace Corps on a nearby island. I expected something similar in 1998, but it wasn't that simple. Two of the women that I had expected to reconnect with I managed to do so quite readily, perhaps because I sought them out when I heard they had returned to the island. A third woman, however, I initially thought wasn't on the island because I assumed that if she were, she would have approached me. As it turned out, she had been waiting for me to take the initiative. As soon as I did, our relationship picked up more or less where we had left it. Even though eighteen years had elapsed and we'd had only a few brief visits when one or the other had been in the port town and on Saipan, we easily fell into our previous relationships. All three spent a good part of the year off-island, though, and a number of other women whom I had barely known the first time became new friends. In the end, my network of relationships looked quite different from what it had been in 1980—and from what I had expected.

I was intrigued with the memories that some islanders had of me. I had expected them to remember my son, and some of the younger people in particular talked of playing with him or helping to care for him. That certainly helped my rapport. But some who had been children in 1980 and had since grown into young adults had memories of me as well, and they were memories that I didn't share, that I couldn't remember myself. In certain respects, I felt almost as though I was starting over, especially since it had been so long since my first visit. Those feelings were probably more on my part than theirs, since they kept talking of how I was "used to" Pollap and Pollapese (as opposed to my husband, for instance) and how I knew their language.

What I should have expected but didn't was the way relatives of people I'd had close relationships with treated me as a returning relative, even when I

hadn't known them well, if at all, the first time. The mother and the daughter of one friend treated me that way, as did the sister of the man I'd known since my Peace Corps days. In addition, a couple of young adults who had been elementary school children had strong memories of my son, had treated him as a younger brother, and thus considered me a relative as well.

Part of the explanation for a different quality of relationships—but only part of it—probably lies in changes I saw in women of about my age and a little younger. Years before, many of them had been very shy and quiet themselves. The women I became closest to the first time were either in the family I lived with or were among the more outgoing women. In 1998, however, the formerly quiet women were in their forties, not their twenties, and in full adulthood—and they had changed. One whom I had first known in the Peace Corps, I specifically commented on in my 1980 journal: "She still seems shy." By 1998 she had become a lively, boisterous, strong-willed mother of eight children and not the slightest bit shy. Her development perhaps paralleled mine, and her behavior seemed more culturally appropriate for older women on Pollap than for younger women. I wrote in my journal about three women in particular, "The seemingly so quiet and shy but bright students [from my Peace Corps days] . . . now seem self-confident—like they've bloomed." They were reaching the age at which older women tend to direct the younger women, and they are expected to play a larger role in planning and making decisions. These changes in women around my age probably also contributed to a very different experience for me.

Conclusion

This second lengthy field experience on Pollap differed from the first one through the confluence of several developmental processes, some of which go beyond historical changes in anthropology and in the Pacific. Certainly Pollapese have had considerably more experience with foreigners, especially Americans, and they have a better understanding of the work of anthropologists. Yet the changes that seemed to have had the most dramatic impact were the developmental processes that my friends and I had both undergone over the years, and the ways we differed at midlife compared with young adulthood.

For almost all young adults conducting dissertation fieldwork, it is true that much of their future is indeed at stake, and probably little can be done to relieve stress associated with those anxieties. Yet perhaps merely understanding that such anxiety may be inevitable can be a relief, and at least knowing one is not alone in such worries should be a comfort. That's certainly one reason I've written here of my own insecurities.

Unfortunately, though, I cannot magically package heightened self-confidence or years of maturation. I can only point out the value of old-fashioned participant observation. With all the emphasis on methods for collecting data (many of which I admittedly used to advantage both times), we need at the same time to remind researchers first venturing out into the field of the real value of "hanging out," pursuing activities as opportunities arise, and trusting that time invested in building rapport and making friends will indeed eventually result in valuable data.

NOTES

1. See, for example, classic works in this area by Erikson (1950) and Jung (1933).

2. For details on my fieldwork with and without family members, including the dissertation research, see Flinn 1998.

3. Ways I attempted to cope with shyness and some advantages of it are discussed in Flinn 2000.

4. For more details about these methods, see works by Weller and Romney (1988) and Bernard (1994:237–255).

5. Details about Pollap in the 1980s can be found in Flinn 1992.

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"WE DIDN'T THINK YOU WAS COMIN' BACK": ART, FIELDWORK, AND HISTORY IN THE EAST KIMBERLEY, WESTERN AUSTRALIA

Eric Kjellgren
The Metropolitan Museum of Art

Heracleitus observed that it is impossible to step twice into the same river, yet the river remains over time. This paradox summarizes the relationship between anthropological fieldwork and ethnography. Entering the field is to experience a culture at one point in time. To write ethnography is to create a general analysis of that culture. Just as one can better understand a river's nature by observing it over time, one gains a more nuanced understanding of a culture through repeated episodes of fieldwork. Using examples from my research in the East Kimberley, Western Australia, I explore how the ongoing history of cultural phenomena affects the information collected about them. I argue that *when* one is in the field is as important as *what* one is investigating and that, to gain a fuller understanding of any cultural phenomenon, long-term fieldwork is essential.

THE GREEK PHILOSOPHER HERACLEITUS of Ephesus (ca. 540–480 B.C.) once observed that, because the waters are continually flowing, it is impossible to step twice into the same river. And yet, despite Heracleitus's observation, the river remains over time. Though recognizable as the same phenomenon, the river's course and level are variable, its meanderings slow but perceptible. A better paradox would be difficult to find to summarize the relationship between anthropological fieldwork and ethnography or describe the potential pitfalls of writing ethnography based on a single episode of fieldwork. To enter the field is to see a culture at one point in time—to step into the waters as they flow by. To write ethnography is, at least in classical anthropological conceptions, to create a deeper and broader analysis of that

Pacific Studies, Vol. 27, Nos. 3/4—Sept./Dec. 2004

culture—to describe the river itself. Just as one can gain a fuller recognition of the river's changing nature by stepping into it several times, so too can one gain a better and more nuanced understanding of a culture through repeated periods of fieldwork.

Writing ethnography using material gathered at a single point in time is not without risks, both for anthropologists and, increasingly, for the people they study. As the recent history of interactions among ethnographic texts, governments, and indigenous peoples has often demonstrated, an ethnography has a strong tendency to become the de facto "official" record of a culture and its practices. *When* one is in the field has a profound influence on *what* one observes and, therefore, on how a culture or cultural phenomenon is ultimately understood and represented in anthropological discourses. As Fabian notes, "No experience can simply be 'used' as naked data. All personal experience [such as fieldwork] is produced under historical conditions, in historical contexts; it must be used with critical awareness and with constant attention to authoritative claims" (1983:89). An ethnography based on a single episode of fieldwork tends to become reified as the fixed and incontestable account of the institutions and beliefs of the culture throughout its history, but actually records the viewpoints, narratives, and agendas of individuals living at a specific point in time. It is as if a single observation of a river has been used to describe its level and course for all time: any alternative perceptions of the river or future changes to it are to be regarded as somehow invalid.

To understand the complex and historically contingent relationships among fieldwork, analysis, and ethnography—and the potential pitfalls of basing conclusions on a single period of fieldwork—does not, as some might assume, require generations or even decades. Subtle, even profound, changes that significantly affect cultural practices and expressions, and, consequently, how they are perceived and interpreted by an anthropological observer, can occur over a period as short as two or three years.

Both the rapidly changing nature of culture and the dangers of basing any interpretation (much less a complete ethnography) on fieldwork undertaken at a single point in time have become clear to me even during what has thus far been a relatively short period of "long-term" fieldwork among contemporary Aboriginal painters in the East Kimberley region of Western Australia. During each of my four fieldwork visits in the area between 1994 and 2000,[1] a subtly, but appreciably, different picture of Aboriginal art as a cultural phenomenon has emerged. The passage of a year or two can, and often does, have a significant effect on the information collected, the cultural concerns expressed by the painters, and even the visual characteristics of the paintings themselves as the works and their creators become increasingly

entangled with the global art market. Using the continuing development of contemporary Aboriginal art in the East Kimberley as a case study, I examine how the ongoing history of cultural phenomena can affect the nature of information collected at any given point in time, even over a period as brief as six years. I argue that when one is in the field is as important as the topic one is investigating, and thus why, if anthropologists wish to claim a deeper understanding of any cultural phenomenon, it is essential to observe such phenomena not once, but over time.

Fieldwork in the East Kimberley

Located at the extreme northeastern corner of Western Australia, the East Kimberley region is home to a unique and largely self-contained movement within contemporary Aboriginal painting. Culturally, the East Kimberley includes the peoples of the eastern portion of the Kimberley and adjacent sections of the Northern Territory.[2] At its core lie the homelands of the Gija and Miriwoong peoples. Other local Aboriginal groups include the Gajirrawoong, Ngaliwurru, Jaminjung, and Jaru. Individuals from the more distant desert peoples of the interior live and paint in the region as well.

The peoples of the East Kimberley were originally hunter-gatherers. They lived in small, seminomadic bands of related individuals who moved from place to place within well-defined home territories. Today the region's Aboriginals live in permanent settlements. These consist of exclusively Aboriginal communities, such as Warmun (also known as Turkey Creek), as well as enclaves within larger towns, such as the Mirima Aborginal Community in Kununurra; and, increasingly, "outstations," small settlements established by one or more families who wish to live at particular locations within their traditional lands.

At present the contemporary painting movement in the East Kimberley encompasses roughly fifty painters living primarily in the Aboriginal communities at Warmun and Kununurra and their associated outstations. Distinct from the brightly colored Western Desert acrylics (or "dot" paintings) of the peoples to the south and the bark-painting traditions of Arnhem Land to the east, East Kimberley painting is characterized by the use of large areas of solid color set off from each other by dotted (or, more rarely, solid) borders of a lighter color, usually white (see Figure 1).[3]

Contemporary Aboriginal painting in the East Kimberley centers on the representation of *country* and *dreamings*, that is, the landscape and its mythic history. Every individual at birth inherits ownership rights to, and ceremonial responsibility for, particular portions of the territory owned by his or her language and/or kin group. These tracts are known in Kriol (Aboriginal

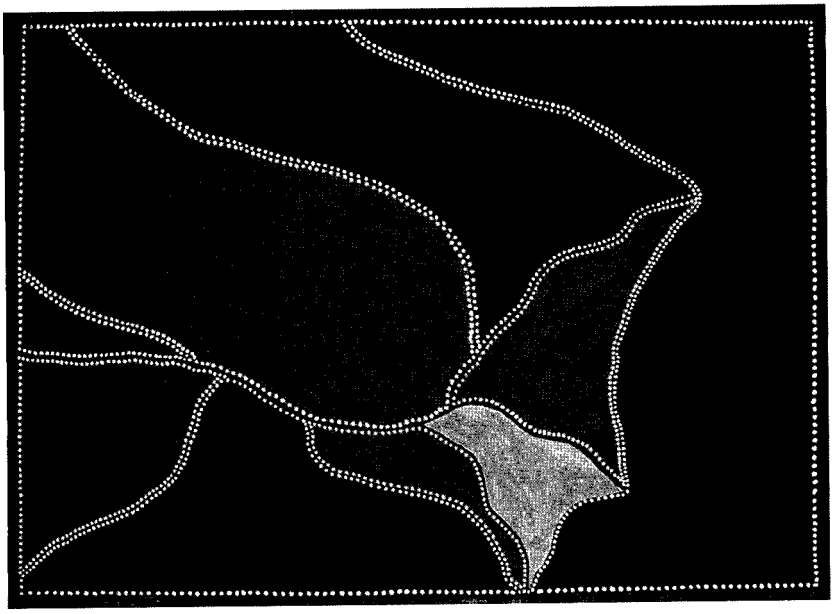

FIGURE 1. **Freddy Timms, ca. 1995, "Bow River Country," ochre on canvas.**

English) as *country*. Although Aboriginals in the region today often live in permanent settlements distant from their traditional homelands, *country* remains central to Aboriginal identity and religion.

Each individual *country* is associated with one or more *dreamings*, the ancestral beings and events that created or shaped its physical features during the *dreaming*, or creation period,[4] and whose creative (and destructive) power is still present at specific sites within in the landscape. In Aboriginal cosmologies *dreaming* beings, *dreaming* sites, and the narratives that describe them are all aspects of a single phenomenon. Thus, the word *dreaming* can be used to refer equally and interchangeably to the initial creation period, a supernatural being from that period, a feature of the landscape created by that being, and the songs or narratives that describe its creation. A lightning *dreaming*, for instance, might be manifest as an outcrop of rocks, an anthropomorphic being, a song or story describing that being or its associated sites, an actual lightning bolt, or all four simultaneously. Both *country* and *dreamings* also have different layers of meanings expressed in songs, stories, and ceremonies. Many of these meanings are public and can be told to anyone. Others, particularly those that deal with male and female ritu-

als, are considered sacred and secret, and knowledge of them is restricted to the initiated. To reveal secret-sacred knowledge contained within a story or ritual to a general audience, either through speech or visually through a painting, is a serious violation of Aboriginal *law* that warrants potentially severe punishment.[5]

As described by many painters, the money they receive for their work is perceived as payment for imparting the (partial) knowledge of the *dreamings* and *country* they portray rather than for the image itself. While the intended non-Aborginal consumers of the work think they are buying the image, the painter frequently believes he or she is selling the knowledge of myth and landscape on which that image is based.

The historically-contingent nature of contemporary East Kimberley painting as a cultural phenomenon is profoundly demonstrated by the fact that thirty years ago it did not exist. Though some community elders participated in the creation of rock paintings in their youth and individuals continued to adorn their bodies for ceremonies, by the early 1970s few if any Aboriginals in the area painted on a regular basis. In the mid 1970s, however, the situation began to change, due not to a revival of earlier rock-art traditions but, surprisingly, as the result of an auto accident.

In late 1974 a local Aboriginal woman was critically injured in a truck rollover near Warmun and died as she was being airlifted to a hospital in the Western Australian capital of Perth. About a month later her spirit "visited" Rover Thomas, a desert-born Aboriginal man who had settled among the Gija people of Warmun, in a series of dreams. On her visits she gave him the songs associated with a *corroboree* (song and dance performance) known as the Goorirr Goorirr. During performances of the Goorirr Goorirr, dancers carry painted boards depicting places and events described in the songs. These boards, along with a series of paintings illustrating local *country* and *dreamings* created by community elders for the newly established school at Warmun, became the sources of the contemporary painting movement.

When East Kimberley paintings came to the attention of the art world in the early 1980s, painters were encouraged by art dealers to produce works on canvas for sale to non-Aboriginals. A commercial market for the painting gradually developed. In the years that followed, an increasing number of East Kimberley Aboriginals began to paint, not solely as a source of additional income but also, according to their own statements, as a means of perpetuating knowledge of the *country* and *dreamings* their works portray. By the mid 1990s East Kimberley painting had grown to become an established and widely recognized regional school within contemporary Aboriginal art.[6]

My own association with Aboriginal painters in the East Kimberley began in 1994, when I made a brief trip to Kununurra to investigate the nature

and extent of contemporary painting in the region and discuss the prospects of returning for a more extended period of research. In 1995 I returned to spend a year in the region working closely with Waringarri Arts, a cooperative owned by the local Aboriginal community. To make me useful to the cooperative, I was assigned the task of recording the mythology or "story" associated with each painting as the artist brought it in to sell. This entailed listening to and tape-recording the story in Kriol, then writing a brief summary in standard English. The summary of the painting's story subsequently became part of the documentation, which accompanied each painting as it was sold and served, to an art world audience, as a marker of the painting's authorship and "authenticity" (see Kjellgren 1999:273–280). My role as "story man" proved critical in developing relationships with the Aboriginal painters in Kununurra and Warmun, virtually all of whom at that time sold their work through Waringarri Arts.

During the course of 1995–1996 I worked closely with about a dozen painters, primarily through informal interviews, most of which were tape-recorded. Through my conversations with them I sought to understand the social, political, and religious dynamics of contemporary painting within local Aboriginal communities as well as the painters' perspectives on the wider (and almost entirely non-Aboriginal) art world for which their works were ultimately destined.

In mid 1996 a second, privately owned "cooperative," Warmun Traditional Arts (later renamed Narangunny Arts), was established at Warmun. A number of the most prominent artists in the movement, including Rover Thomas, Queenie McKenzie, Freddy Timms, Jack Britten, and Hector Jandalu (all of whom lived in or near Warmun), subsequently began to paint for this second cooperative, where I was able to conduct additional interviews.[7]

I left the field in 1996 with a number of preliminary conclusions about the nature and importance of contemporary painting in the local Aboriginal communities. First, that by far the most important aspects of Aboriginal paintings to their original creators are the content and ownership of subject matter (their *country* and *dreamings*) rather than visual characteristics. Additionally, though the art world was perceived as an alien phenomenon of little relevance to their lives, East Kimberley painters nonetheless remained concerned with the ultimate destination of their work and the ways in which it was displayed (see below).

Despite increasing competition between the two local businesses that marketed the art, the contemporary painting movement itself appeared to be thriving at the end of 1996, although the fact that nearly all the painters were in their seventies raised concerns about its long-term viability. Within the movement each artist had developed a highly recognizable individual

style, which appeared to be relatively stable. There was, however, already beginning to be an observable movement among artists who had initially painted in a "representational" style, depicting human and animal figures in their work, toward creating more "abstract" images, in large measure attributable to the higher prices "abstract" paintings commanded (and continue to command) on the art market (discussed below).

When I next visited the East Kimberley in 1998, a number of painters expressed genuine (and I hoped pleasant) surprise that I had made good on my promise to return. One artist, meeting me by chance in Warmun, even told me explicitly, "We didn't think you was coming back." I came back, however, to discover both the production and marketing of art in the region increasingly fragmented and acrimonious. From essentially one centralized art wholesaler in 1995, by 1998 no fewer than five had emerged (or were emerging): two private and three Aboriginal-owned. These five separate entities all actively competed to control the marketing of works by the dozen or so best-known artists (whose paintings can sell wholesale for several thousand Australian dollars apiece). Competition and demand for East Kimberley painting had increased dramatically over the previous two years, but the number of painters had not. Apart from a few "new" painters (most in their sixties or older), the movement continued to depend primarily on the efforts of a small core group drawn from the original painters, most of whom were now in their mid to late seventies. That same year saw the deaths of Rover Thomas, cofounder of the contemporary painting movement, and Queenie McKenzie, the East Kimberley's most renowned woman artist, substantially reducing both the number as well as the national and international profile of East Kimberley painters. I left the field in 1998 discouraged, with the impression that the movement was slowly dying out as an ever-growing number of dealers squabbled over an ever-diminishing number of painters, at times dividing communities and creating bad feelings in the process.

I returned in 2000, however, to find the contemporary painting movement rejuvenated. The number of painters and paintings in the area was on the rise. Though the five competing wholesalers remained, there now appeared to be an uneasy truce among them. At Warmun, the staff of the Warmun Art Centre, a community-owned cooperative, had been actively seeking out and encouraging younger artists and at least a dozen new painters, ranging in age from their late twenties to their early sixties, were now creating works on a regular basis. In Kununurra, a group of painters from the nearby outstation community of Emu Creek, encouraged by the privately owned wholesaler Red Rock Arts, had begun to create a great variety of works, which together constituted a distinctive new subtradition within East Kimberley painting. Where two years before, the contemporary painting movement appeared

moribund, it was now expanding so fast that simply recording the names and representative works of all the emerging artists required a significant investment of time and effort.

Throughout my fieldwork in the East Kimberley an important factor affecting the quantity, depth, and type of information I obtained has been the continually developing nature of my relationships with the painters themselves. These sorts of ongoing relationships are another factor made possible only through repeated periods of fieldwork. As I continued to return to the East Kimberley, I built up a greater degree of trust with certain individuals. This resulted in my being told more (and, in some instances, different kinds of) information about the religious and mythological background of contemporary paintings than the painters had been willing to reveal during my initial fieldwork. Some of this information was secret-sacred in nature and therefore cannot be published. However, the great majority of it consisted of fuller and more nuanced accounts of diverse nonsecret aspects of Aboriginal culture that proved invaluable to me in attempting to understand the subtle and complex social dynamics of Aboriginal painting.

Against this shifting backdrop of social, economic, and interpersonal factors, as I continued to return to the field I observed that both the painters and the visual characteristics of their works were also continually changing. Both the imagery in East Kimberley painting and its creators' perspectives on the non-Aboriginal art world for which their works are destined have changed markedly within the six-year period in which I have been documenting them. As examples of the historically contingent nature of the relationships between observation and the ethnographic project, specific instances of historical change in the East Kimberley can be useful in examining how they have affected my own observations (and interpretations) of East Kimberley painting as a cultural phenomenon.

Same *Country*, Different Way: The Movement to "Abstraction"

One of the most obvious changes in the nature of East Kimberley painting over the course of my research (or at least so I initially thought) has been the movement by many artists from "representational" to "abstract" imagery. Both the earlier rock-art traditions and many of the first contemporary paintings in the East Kimberley appear (to non-Aboriginal eyes) primarily "representational" in nature. They contain recognizable, if stylized, images of humans, animals, and supernatural beings. Shortly after I began my fieldwork in 1995, however, many painters began to slowly shift to creating works that contain no immediately recognizable figures or motifs, and thus are perceived as "abstract" by non-Aboriginal observers.[8] This profound shift oc-

curred largely as a response by Aboriginal painters to the preferences of the non-Aboriginal consumers for whom the works are ultimately destined.

At least one scholar, in discussing Western Desert acrylic paintings, has implied that contemporary Aboriginal painting likely owes a great measure of its acceptance and popularity in the art world to its (superficial) resemblance to Western abstraction (see Michaels 1994:154–157). Through the perception and representation of Aboriginal paintings as "abstract," dealers, critics, and collectors re-present the unfamiliar (Aboriginal paintings) in terms of the familiar (abstract art), linking them with a specific set of discourses through which they can be understood by a Western audience.

The high cultural and commercial value accorded to abstract art by the contemporary art world translates directly to the values accorded by dealers, critics, and collectors to Aboriginal paintings (and painters) in the East Kimberley. With rare exceptions, works by individuals from the Warmun community, such as Rover Thomas, Freddy Timms, and Queenie McKenzie,[9] which appear abstract to the Western gaze (see Figure 1), attract more critical attention (and command higher prices) than those of Kununurra painters, who often include representational imagery. Abstract works by any artist are also widely regarded as more marketable than examples that incorporate human or animal figures. For this reason, the inclusion of representational images in paintings is, in some instances, discouraged by art coordinators.

During my initial fieldwork in 1995–1996, a number of painters in Kununurra began to respond to these market preferences and were in the process of changing the imagery in their paintings from works that included human and animal figures to more abstract compositions resembling those of successful Warmun painters. This trend is readily apparent in the work of Paddy Carlton. Comparing Carlton's 1993 canvas "Man Drowned by Two Rainbow Snakes in the Dreamtime" (Figure 2), for example, with his late 1995 work "Possum, Lightning and Dingo Dreaming," (Figure 3), one is immediately struck with the fact that the first consists wholly of naturalistic images (a man, a crocodile, two snakes) while the second appears entirely abstract and strongly (if superficially) resembles the work of Warmun painter Queenie McKenzie.

Perhaps the most dramatic example of the shift from representational to abstract images by painters in Kununurra can be seen in the work of Alan and Peggy Griffiths. While most works by these artists are attributed to one or the other of them individually, the Griffithses, a married couple, often collaborate in the production of paintings, which is common practice among Aboriginal painters in the East Kimberley and elsewhere. When I first met the Griffithses in 1995, their strongly figural work depicted landforms such as caves and waterholes, surrounded by images of the plants, animals, and

supernatural beings associated with them (Figure 4). When I returned in 1998, I found their work had undergone a complete visual transformation. The two continued to depict the same subjects, but all trace of anthropomorphic and zoomorphic imagery had disappeared. The landforms themselves were so highly stylized that they appear totally abstract (Figure 5), occasionally resembling the work of Warmun's most celebrated abstract painter, Rover Thomas (a personal friend of Alan Griffiths).

By the end of my field season in 1998, the trend toward abstraction seemed firm and virtually universal. Had I returned permanently to the United States and written up the results of my research based solely on the information from these two episodes of fieldwork, an account of historical change might have resulted that, while significant, followed a predictable course. The imagery of East Kimberley painting appeared to be changing from representational to abstract, and the change appeared to be unidirectional. Thus when I returned in 2000, I expected to find the Griffithses and other Kununurra painters continuing to create the abstract images so appealing to non-Aboriginals. The Griffithses, however, had other ideas.

As soon as I saw their latest works in 2000, I realized that the Griffithses had once again radically transformed their style. In contrast to the stark, minimal landscapes of 1998, their paintings now abounded with tiny human figures, in some instances dozens on a single canvas, dressed in ceremonial finery and engaged in elaborate *corroboree* performances. Furthermore, the figures did not represent a reversion to the earlier style of 1995–1996 but were a new type, reminiscent of the work of the innovative Western Desert painter Mary McLean.[10] Within the space of two years—at least in the case of Alan and Peggy Griffiths—the trend toward greater abstraction had been completely reversed. What had first appeared to be a straightforward aspect of contemporary East Kimberley art proved, with the passing of more time, far more complex.

The examination of what was, in retrospect, a somewhat illusory "movement toward abstraction" among East Kimberley painters makes clear the historically contingent nature of Aboriginal art as a cultural phenomenon, as well as the dangers of attempting to produce a general account from a single episode (or even two) of fieldwork conducted within a relatively brief period of time. An ethnography drawing solely on information from 1995–1996, for example, might describe an inherent contrast between the representational works of Kununurra and the abstract works of Warmun, perhaps interpreted as an effort by artists in each community to assert a distinctive identity. An account based on 1995–1996 and 1998, conversely, would describe a widespread trend toward abstraction and, thus, a growing homogeneity and unity between the art of the two communities. An observer visiting the region only

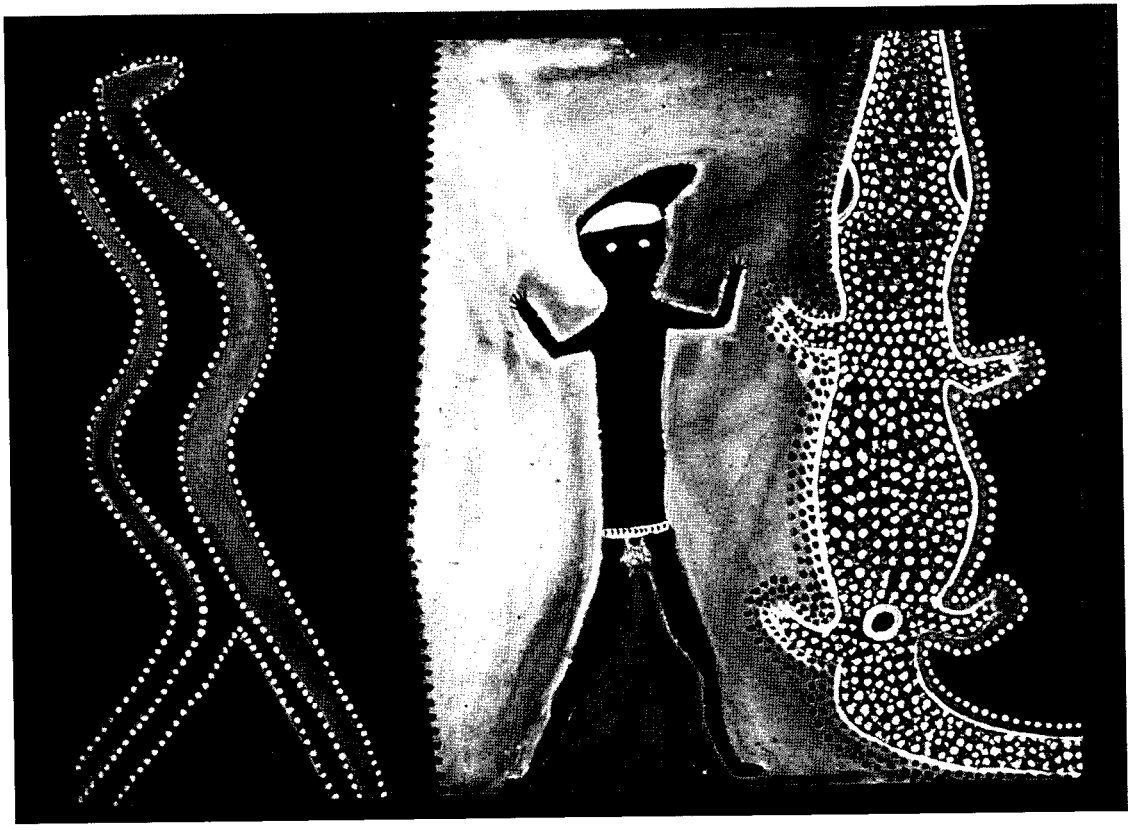

FIGURE 2. **Paddy Carlton, ca. 1994, "Man Drowned by Two Rainbow Snakes in the Dreamtime," ochre on canvas.**

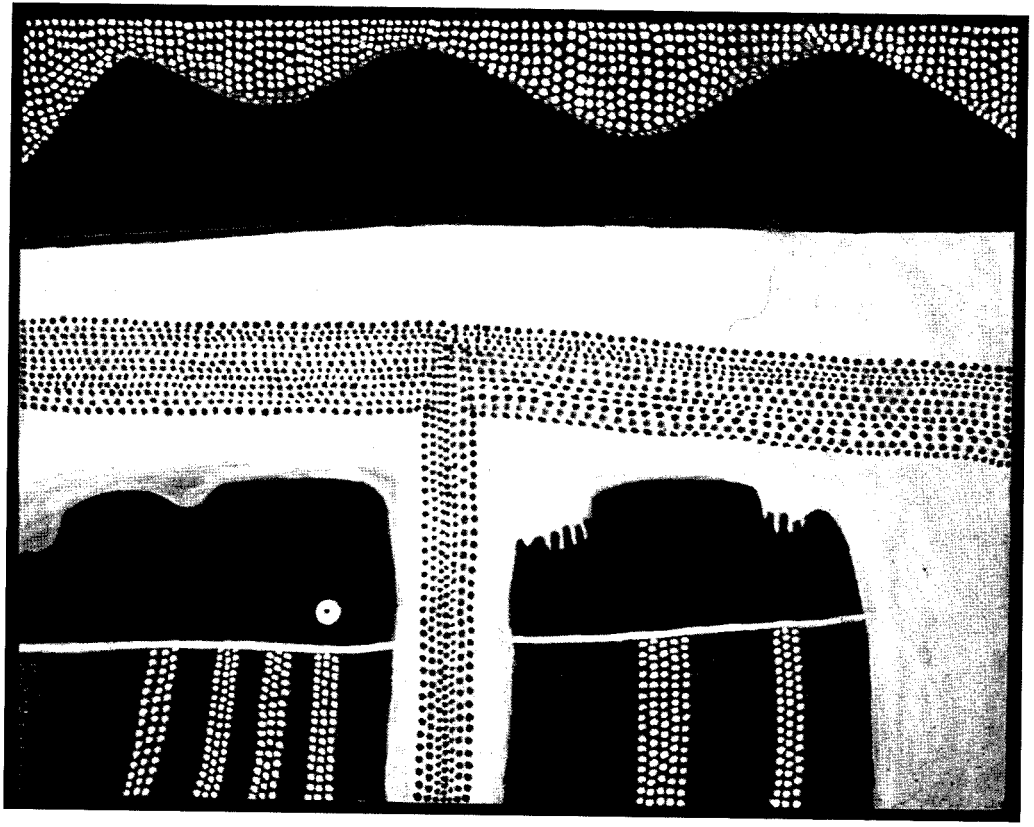

FIGURE 3. **Paddy Carlton, 1995, "Possum, Lightning and Dingo Dreaming—Bool-oobiloobi Country," ochre on canvas.**

FIGURE 4. **Alan Griffiths, ca. 1995, "Jasper Gorge Country," ochre on canvas.**

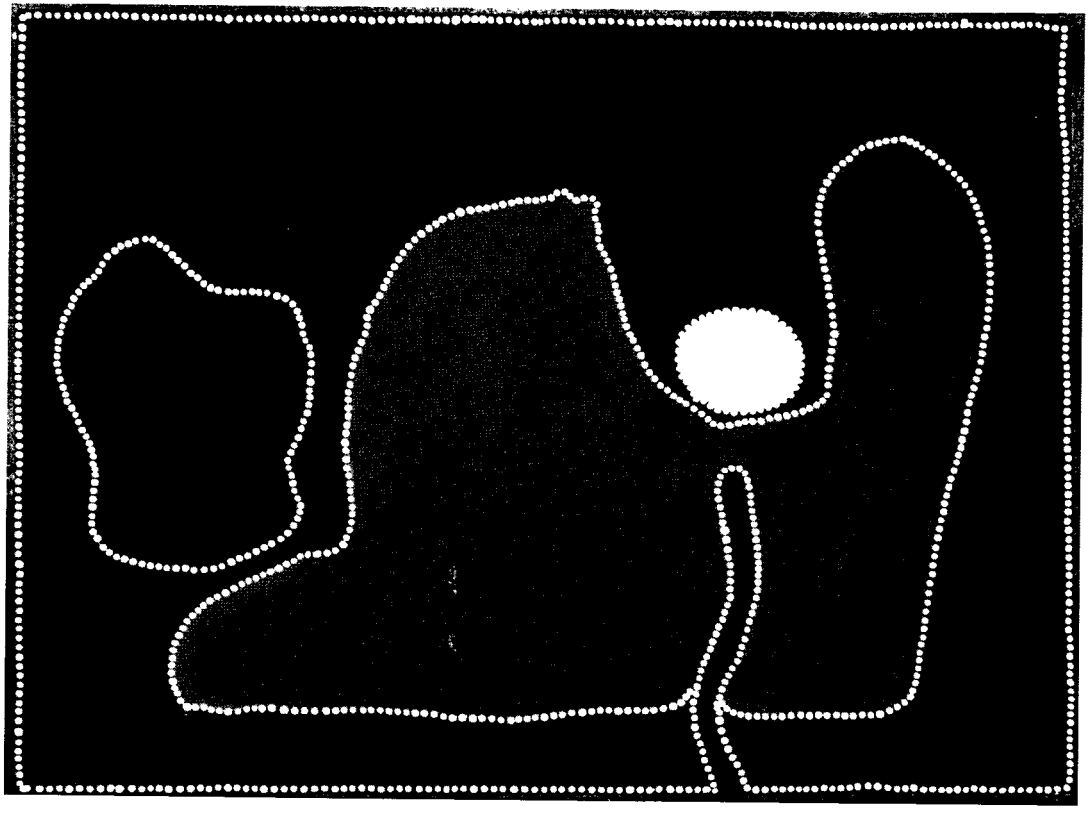

FIGURE 5. **Alan Griffiths, 1998, "Full Moon," ochre on canvas.**

in 1998 could, with some justification, characterize all East Kimberley painting as abstract; while a second, visiting Kununurra in 1995–1996 and 2000 (but not 1998) could just as rationally characterize much of its painting as representational. The same phenomenon, sampled during one or more discrete episodes during a six-year period, has the potential to produce radically different results, which in turn would likely lead to significantly different interpretations (and representations) of local Aboriginal culture on the part of a non-Aboriginal observer.

Display and the Dead

Just as the imagery of East Kimberley painting changes through time, so too do the perspectives and, to some extent, the cultural practices of the painters. As East Kimberley painting has gained increasing recognition in the global art world over the past two decades, a growing number of painters have had the opportunity to visit museums and private collections exhibiting their work. Observing how their works are displayed by their non-Aboriginal owners, painters subsequently express a range of concerns, many of which reflect cultural priorities that clash directly with the conventions and practices of the art world. Painters frequently regret in particular that representations of local *dreamings* now reside far from their home *country*. But, as is also the case with Aboriginal painters from Arnhem Land and the Western Desert, a more prominent issue among East Kimberley painters concerns the appropriateness (or lack thereof) of displaying paintings by deceased individuals.

In the mainstream art world, discourse typically emphasizes a painter's legacy, in which an artist's work serves as a lasting monument to his or her artistic achievement and form a permanent part of museum displays and collections. But such Western notions of artistic immortality directly contradict conventions of respect for the dead in many Aboriginal societies. Aboriginal *law* in the East Kimberley customarily calls for the destruction of all images of a deceased person and all his or her belongings, and forbids the living to utter his or her name. In recent years this situation has been gradually changing, at least in regards to photographs and possessions. In 1996, for example, Warmun painter Shirley Purdie, although explicitly noting that the *law* requires that the possessions of the dead be destroyed, described telling her children that it was permissible to keep photographs of her as well as her paintings after her death: "Like all my kids now. I tell them, you know. Before might be anything happen to me....They can keep my photo, they can keep my painting, I tell them now. [In former times] they used to burnim up, you know....You can't keep anything. But now, you know [it's different], because kids now growin' up more or less like a *kar-*

tiya[11] now...and they like to keep things for remembering" (Shirley Purdie, interview, 26 June 1996).

During this interview, nonetheless, Purdie did express concern about the (in)appropriateness of displaying paintings by deceased artists in museums and galleries. Asked her thoughts on seeing a group of paintings on exhibit at the National Gallery of Victoria in Melbourne, she specifically mentioned being "worried" about the inclusion of works by Warmun painter Paddy Jaminji, who had recently died (note that she does not mention him by name): "That old fella one really. That we lost here [Jaminji]....Yeah, we saw his one [painting] there. Yeah, bit worried what, you know....You do your painting and painting he'll be still there. Yeah, really we don't know. Can't help really." To judge from my 1996 interview with Purdie, the practice of displaying paintings by deceased artists in museums and art galleries was at best unsettling and at worst offensive to those Aboriginals who continue to practice the customary proscriptions associated with the dead.

Over the next four years Purdie's opinion on the issue underwent a noticeable change. When I interviewed her about the same topic in 2000, she began by expressing the notion that keeping and displaying paintings by deceased people in museums was appropriate because it preserves the memory of the dead for their descendants. Significantly, she prefaced the statement by noting that this was her own idea:

> Well,...my idea, I reckon he's alright [to display works by deceased individuals], you know, long as they there for when people want to [see them]....Like the old people now, when they [are] gone....Well, if they [their] painting there, you know, we can still see it. The ones they keepim there [in the museum], you know. That's for remember the old people. Well, that's same as us [living painters], you know. If we finish [die], well, might be our children or...some family want to seeim [the paintings]. They can now. Long as he's there for all the time. (Shirley Purdie, interview, 12 July 2000)

More interestingly, she subsequently went on to give a substantially different account of the customary Aboriginal prohibitions regarding the works of the dead than she had in 1996—one that can accommodate both Western and Aboriginal customs. According to her 2000 description of Aboriginal *law*, works by deceased individuals, rather than being permanently withdrawn (and, ideally, destroyed), can be displayed following an appropriate period of mourning after the heavy rains of the Wet Season[12] have cleansed the land, as seen in this excerpt:

E.K: So it's okay [now] to have paintings up there [in museums] from people who bin pass away, or what?

S.P.: Yeah, well some family, you know, they don't worry [about it]. Long as they reckon that's good memory, you know.

E.K: But *early days*[13] different, eh?

S.P.: Different *early days*, yeah. They bin always puttim [paintings] away.... Till might be two year then they puttim out [again]....They say when rain season come, you know, rain time. He wash all the thing away from them old people bin passed away. Alright, the next new year time he'll be right. That's what the old people used to say, you know. After the rain, well, he'll be alright....They think that rain he's wash [away] everything, you know, like [a human or animal] track.... (Ibid.)

The notable differences not only in Purdie's personal perspective on the subject but also in her account of relevant Aboriginal *law* again highlight the historically contingent nature of Aboriginal painting as a cultural phenomenon in the East Kimberley. Had I written an account of painters' attitudes toward the display of works by deceased artists based solely on my 1995–1996 fieldwork (as I did when I wrote my doctoral dissertation), I would have concluded (as, indeed, I then did) that the display of such works was problematic, even unacceptable, to members of the local Aboriginal communities (see Kjellgren 1999:327).

Reexamining the same topic with the same person in 2000 lead to a very different conclusion: that, although stated by Aboriginals in the East Kimberley to be permanent and unchanging, *law* exhibits considerable flexibility, allowing it to accommodate, on Aboriginal cultural terms, the growing interaction between local peoples and the global art world. While it is uncertain if every member of the Gija community would agree that the display of works by (or images of) deceased artists is appropriate, the consensus among the artists themselves now appears to be that it is. This point was made explicitly clear to me again when I was with a group of Gija painters in Sydney at the opening of the exhibition "True Stories: Art of the East Kimberley" in January 2003. When I asked them if I could show images of deceased artists and their paintings in a slide lecture at the opening symposium, they unanimously agreed that it was permissible to do so. The nature of *law* and other cultural practices as dynamic rather than static phenomena has become increasingly apparent through examining such issues historically, made possible by repeated episodes of fieldwork. Conclusions that seemed firm after my initial period of fieldwork must now be approached with caution.

Conclusion

This brief examination of the relationship between history, culture, and Aboriginal painting in the East Kimberley readily reveals the complex and problematic entanglement of history with the ethnographic project. Although the changes I have described are among the more significant I have observed in my four trips to the region between 1994 and 2000, numerous subtler changes are continually transforming the social (and physical) manifestations of this unique tradition within contemporary Aboriginal art. Had I (or another researcher) attempted to create an account of the contemporary painting movement based on the classical anthropological method of making a single trip to the area, the *when* of that trip would clearly have had a major effect on the nature of information recorded and, therefore, on any subsequent interpretations and texts concerning Aboriginal painting as a cultural phenomenon.

Even in a period as brief as six years, the risks of creating a general account of East Kimberley art from information gathered at any given moment—to describe the river after having stepped into it only once—are immediately evident. Many factors are constantly, at times radically, changing: the participants in the movement, its relative vigor, relationships to and perspectives of the painters on the art world, even the visual characteristics of the paintings. These changes become visible only through multiple fieldwork visits separated, at least to some degree, by time.

In this essay I have deliberately stressed instances of change and historicity, providing case studies of the differences I have observed over time among the painters and painting of the East Kimberley. Long-term fieldwork reveals the changing nature of many cultural phenomena; equally important, it also reveals aspects of culture that are constant, or at least changing at far slower rates. In the East Kimberley these include factors such as the central importance of *country* and *dreamings*, not simply to contemporary painting but also to individual identity and, increasingly, to contemporary land politics. While the ways that *country* and *dreamings* are portrayed in East Kimberley painting have clearly changed over time, their centrality as its subject matter remains, and is likely to remain, constant. Multiple periods of fieldwork over time have thus been invaluable to me in distinguishing what are essentially passing fashions from truly enduring cultural institutions.

NOTES

In preparing this article I want to thank, first and foremost, the painters and other Aboriginal people in the East Kimberley who have continued to allow me to share their

lives and cultures. For sponsoring my research I want to thank the Fulbright Foundation as well as The Metropolitan Museum of Art and the North Australia Research Unit of The Australian National University. At Warmun I wish to thank the Balingarri Aboriginal Council, Warmun Art Centre, and Warmun Pensioner Centre. For interviews with artists working for their art cooperatives thanks are also due to Tony Oliver of Jirrawun Aboriginal Arts and Maxine Taylor of Narangunny Arts. In Kununurra I wish to thank the Mirima Aboriginal Council and Waringarri Aboriginal Arts and especially Kevin Kelly (now of Red Rock Arts) and Rashida Bin Omar. Finally, I want to thank John Barker for his close reading of and helpful comments on an earlier draft of this article. All figures, here reproduced in black and white, were originally photographed in color by the author and are used with permission of the artists or copyright holders.

1. Ever since first undertaking intensive fieldwork in the East Kimberley in 1995–1996, I have intended to make a long-term study of the continuing history, evolution, and cultural significance of contemporary Aboriginal art in the region. I have had the opportunity to return to the region in 1998 and 2000, and plan to continue to do so in the future.

2. While the literature on the region and its peoples remains relatively scant, important sources on the cultures of the East Kimberley include Kaberry 1939; Kimberley Language Resource Centre 1996; Kjellgren 1999, 2002; Ryan 2001; Shaw 1981, 1986, 1992; and Stewart 1999.

3. For surveys and discussion of contemporary Aboriginal painting and painters in the East Kimberley, see Ian Potter Museum of Art 2002; Kjellgren 1999, 2002; Ryan and Akerman 1992; Stanton 1989; Thomas et al. 1994; and Watson 2002.

4. In literature, this era of initial creation is frequently called the *dreamtime;* the term is occasionally used by Aboriginal Kriol speakers as well. Most recent authors and Aboriginal Australians prefer *dreaming,* which better reflects belief that its supernatural power is an ongoing phenomenon that is not confined to a finite period in the distant past.

5. The Kriol term *law* denotes the sacred corpus of rules and customs established during the *dreaming* by which Aboriginal people are expected to live their lives and which governs their relations to other members of the group and the natural and supernatural worlds.

6. See works by Kjellgren for a detailed account of the origins of the contemporary painting movement in the East Kimberley (1999:168–202; 2002).

7. The decision of these and other painters to paint for this second cooperative was motivated in part by convenience (to visit Waringarri Arts in Kununurra requires traveling more than two hundred kilometers from Warmun). It may also reflect the desire of Warmun residents, most of whom are from the Gija language group, to sell their work from a source located in a Gija community rather than through Waringarri Arts, which is owned by the Miriwoong language group.

8. The apparent abstraction of these paintings is purely superficial. Both abstract and representational canvases depict the same subject matter, differing only in how the artist chooses to represent the underlying *dreaming* or *country* in a particular work.

9. The works of Thomas and Timms are often described as abstract or minimalist, but a number of their paintings, particularly Thomas's earlier works, contain representational

images of birds, *dreaming* beings, and other figures (see, e.g., Thomas et al. 1994:10). McKenzie also occasionally incorporated small human figures into her landscapes throughout her career.

10. This resemblance may not have been entirely coincidental, as I remember seeing a poster of one of McLean's paintings on the wall at Waringarri Arts.

11. The Kriol noun *kartiya* refers to a person of European ancestry or, when used as an adjective, to any object, plant, animal, concept, or practice derived from Western as opposed to Aboriginal culture.

12. The annual rainy season, also simply "The Wet," which occurs between November and March.

13. The Kriol expression *early days* here refers to the period before European contact when Aboriginal people, according to their contemporary descendants, lived entirely according to the precepts of customary law.

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