Chapter 3

On Not Knowing One's Place

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The Association for Social Anthropology in Oceania (ASAO), while nominally restricted to discussions of research on a geographical area, has perfected a tradition in its meetings and publications that reveals a great deal about ethnography, its authority, and its audiences on a wider scale. This tradition, which is only an extreme form of a practice implicit in much areal ethnographic comparison, consists of the following: one or more scholars suggest a theme or topic that they and others then illustrate and analyze with material from the society or group in which each has carried out fieldwork. It is as though the conveners "own" the theme or topic, while fieldworkers (among whom may be counted the organizers wearing their ethnographic hats) "own" the society, village, or community about which they write. Some conference sessions now no longer fit this particular bill, but its effects linger and are seen to constitute normal practice. This continuity was confirmed with startling literalness at the 1995 ASAO meeting in Clearwater, Florida, when I observed a session in which I had an interest but no direct involvement. A senior member of the association saw that the symposium convener was wondering how to develop the topic at the next conference after a confusing welter of suggestions from the floor and proffered the following advice: "Don't be too dialogic—you *own* it" (emphasis added).

Such an arrangement is grounded in complementary positions of intellectual authority; some participants are authorized to speak on theoretical or comparative matters, while others are authorized to speak on what those topics mean for the subjects whom they represent. However one defines the roles, a clear division of labor and responsibility exists. Indeed, from my observations, the session organizer's authority is generally welcomed by the other contributors. Taken to extremes, this division can lead to an untheorized kind of perspectivalism that defines and legitimizes knowledge by virtue of the (concrete) place from which the ethnographer speaks. Again, the same senior association member expressed this view with stark clarity during the session referred to earlier: "ASAO is not just about speaking to a common theme but also about comparable data." Despite the initial importance of the theory in topic selection, ethnography has a sovereign authority within its own jurisdiction, being called on to shed the judicious light of empirical truth on questions raised under other auspices. A topic is just an idea; fieldwork, on the other hand, provides material substance by speaking through substitutable observers. The field data have to be "comparable," which reintroduces by the back door the authority of the topic theorist as the one who selects the framework through which such data are to be viewed.

It is true that at ASAO meetings, as in any other fora, these conventions of ethnographic staging have not been settled once and for all. Indeed at various times they have been negotiated, contested, or subverted by those involved—but they have never been ignored. This is despite the fact that most of the ethnographers involved know that they are by no means neutral collectors of facts to be analyzed by others, and despite the fact that their representations in other contexts explicitly recognize that they gather knowledge in social fields populated by competing interests. Hence I think the characterization sketched above has held true for most ASAO sessions and probably still does so. Where resistance to this kind of staging has occurred, it generally arises from *within* the dominant paradigm of ethnographic knowledge.[1] This does not necessarily mean that such notions of ownership and authority consciously reflect or influence the ways that ethnographers treat the people whom they represent. Rather, the problem is inherent in the logic of an idiographic division of labor and affects all ethnographic work to a greater or lesser extent. Anyone who takes part in a formal paper presentation at an ASAO symposium, knows how hard it is to break out of the mold. As with any other formation of knowledge, ethnography inhabits a domain of disciplinary ties from which it cannot entirely escape. Does this mean, however, that it should supinely welcome the restraints?

The ASAO system embraces a taken-for-granted but always elusive division between ideas and facts that disguises ethnography's role as a system of representation in both the political and semiotic senses of that term. Ethnography shares this duality with all other forms of social theory and research. Gayatri Spivak has drawn attention to an early example, Karl Marx's famous discussion (1970, 170-171) of whether the French peasantry constituted a class, in which he pondered the relationship between the political dimension of representation *(Vertretung)* and the symbolic *(Darstellung).* More recently, political theorists have developed the distinction and its potential for cross-fertilization (Pitkin 1967; Shapiro 1988). Spivak's own use of the alliterative tropes of "proxy and portrait" to convey the same doubled meaning (Spivak 1988, 276) has exerted an influence in the field of cultural studies (e.g., Probyn 1993, 79). Increasing numbers of anthropologists, too, work under the banner of "the politics of representation," a rubric of special intensity within so-called postcolonial states (e.g., Beckett 1985; John 1989). I argue that anthropology can no longer avoid this entanglement of politics and meaning, if it ever could. Representation of the "Other" has become an increasingly problematic business, while description of the world is an ever more theory-drenched activity.

To be fair, defenders of the ASAO approach can point to some extenuating circumstances and strengths of their system. Many, perhaps most, of the papers collected under this aegis at the annual gatherings have been useful and interesting. They have rung creative changes on a number of important topics, or at least on topics that the discipline currently acknowledges as fashionable. Furthermore, there are diseconomies of ethnographic scale in carrying out fieldwork among the small, dispersed communities and diverse cultures in this region of the world. These encourage a territorial division of labor. Arguably, no single fieldworker has, or could have, the detailed knowledge and firsthand experience to command the cultural variation required to carry out detailed intraregional comparison. Under the circumstances, an "additive" strategy of data accumulation that holds each ethnographer responsible for relating a "whole" society to the chosen dimension of analysis is a logical enough strategy.

As a further incentive to stick to one's field site, the few scholars brave enough to attempt Polynesia- or Pacific-wide comparisons (most famously, Sahlins 1958, 1963; and Goldman 1970) have had to weather storms of criticism. Much of that adverse reaction, however, confirms my thesis on a larger scale. Dissent on theoretical grounds has been relatively rare. Rather, each ethnographic critic exercises a gatekeeping function by showing that the comparativist got the facts wrong on a certain point in a particular society at such and such a time.

On a more positive note, the ASAO tradition has guarded against overly sweeping generalization and provided constant reminders of cultural variation. Behind this concern for empirical particularity, however (and perhaps even underpinning it), there are still strong echoes of the old-fashioned comparativists' fascination with Oceania as a "laboratory" of naturally controlled experiments. At the same time, the tradition has opened itself up to a converse kind of criticism by perpetuating a view of the inviolable separateness of societies and of ethnographers' experiences in them. The approach thereby reinforces the irreducibility of self-contained societies and assumes the equivalence of individual fieldworkers.[2] This can lead to the trap of confusing the delimited authority that stems from actual presence in localized settings with the presumption that an ethnographer can mirror a whole society or even a whole village in direct proprietary fashion. For a generalist to represent through a comparative frame a whole culture area (Polynesia or the Pacific) is only an extension of this same logic, just as *refusal* to let one's ethnographic field site be the subject of comparison is an inversion of it. The "social whole" deemed suitable for comparison is simply framed within wider or narrower boundaries. ASAO's "third way" of limited comparison, in which fieldworkers control the right to represent but topic owners control the resulting theoretical and areal generalizations, merely masks the same epistemological problems behind an agreed division of labor.

A collection of essays explicitly addressing the ethnography of ethnography goes some way toward questioning these conventions. To confront the issues of different interpretations of the same society and of the effect those interpretations have on the subjects of anthropological research raises difficulties for the model I have outlined. These difficulties cannot be resolved by the strategies of trying to heighten the rigor of comparative ethnography or increasing the amount of information available on which to make such comparisons. This impasse brings me to my title, which may require brief explanation. If the original ideal behind the inspiration for ASAO meetings was that ethnographic contributors should "know their place," I probably offend this disciplinary cartography in at least three senses: I disavow the notion of a field site to which I can claim some exclusive insight; I wish to question the basis on which such exclusive knowledge is generated; and I am rudely critical of an ancestral tradition.

My discomfort with overly reified assumptions of fit between researcher and researched stems in part from an intermittent and fractured involvement in fieldwork and a sense that the stereotyped ethnographic division of labor does not reflect that kind of experience or the experience of those living in the societies at stake.' Not everyone suffers from my "handicap," if it is one. Nevertheless, I am not the only ethnographer of the Pacific discomfited by the basic problem (qv. Marcus 1995). The fiction that societies are bounded cultural wholes and accessible as totalities to ethnographic representation is exposed by the crosscutting pressures of travel, migration, education, trade, regional elite formation, nationalism, and globalism. Of all the linkages and leakages between these, literacy seems crucial. It creates a focus that highlights the creation of old and new kinds of texts, the embeddedness of literacy in forms of life, the transmission of "traditional" knowledge in written form, the diffusion of such knowledge across cultural boundaries, and the construction of readerships.

I will illustrate these issues with some vignettes from my own ethnographic career. I do so in the recognition that knowledge of one's place or of the gate-keeping that places you there is rarely as straightforward as academic discourse suggests. The following stories pose puzzles that go well beyond my limited attempts at interpretation; but they may serve the purpose of unsettling a few preconceptions.

Three Literary Episodes

In March 1971 I arrived to do research in the largest Tokelau community in western Samoa, situated at Lotopa on the outskirts of Apia.[4] For most of the next six months, apart from a brief stay in American Samoa, I lived in the home of Penaia Kitiona and confronted the mysteries of fieldwork as an acolyte ethnographer. Penaia and his wife, Salani, were the recognized leaders of a community comprising a large household in their own two-storied residence plus several households in smaller dwellings on the same patch of leasehold land. Both had been born on the northernmost Tokelau atoll of Atafu but were long-term and well-regarded residents of western Samoa. At the time, Penaia, who was nearing fifty years of age, worked in Nelson's store in downtown Apia. To supplement their income, he and Salani also provided accommodation and support for people from the atolls who came to town for education, medical treatment, or kinship visits, as well as for people in transit between Tokelau and New Zealand.5

About two weeks into my stay, Penaia came into my room and nonchalantly placed a book on my table. My diary entry for 29 March records the incident as follows:

> Penaia "just happened" to have copy of McGregor's *[sic] Ethnology* in his room; it will be useful as a reference. I wonder how much (if at all) it has influenced his ideas on TK culture. Gave him my copy of WHO report on Tokelau and Dakar studies (by Al Wessen) & he seemed v. int[ereste]d; took it away to read.

The book that Penaia showed me was Gordon Macgregor's Bishop Museum Bulletin, *Ethnology of Tokelau Islands* (1937), published on the strength of a two-month stay at Atafu in 1932. Like most of the Bishop Museum Bulletins of that era, notably those authored by the museum's roving ethnologist-at-large, Peter Buck (Te Rangi Hiroa), it concentrated primarily on history, traditions, and material culture. Whatever its intrinsic merits, I felt then that it bore few direct links to my own study—although I had read it before embarking on fieldwork and was to dip into it again in Apia in lieu of other reading matter. It is only in retrospect that it has acquired a more interesting set of meanings as a token of an ethnographic relationship.

What my brief diary entry does not convey was a contradictory mixture of surprise and déjà vu on my part. Surprise, it has to be admitted, at finding a moderately rare book in that setting, though I think Penaia expected me to show more interest in learning of the book's very existence than of his ownership of a copy; but also déja vu because I already had a sense of the incident's banality from student folklore about the resigned bemusement of informants who read up about their own culture from monographs, practice their impression management beforehand, and perform for anthropologists' benefit. "Of course!" I rationalized at the time, "it makes sense for a self-respecting Tokelau community leader to have a copy of such a book."

I treated the incident too nonchalantly. To begin with, the question posed in my diary is unanswerable. I really have no idea how much Macgregor's monograph influenced Penaia's "understanding of Tokelau culture," or even if the question makes sense. Though I do not generally believe that cultures are static, if there were resonances between what Macgregor wrote and what I or any other ethnographer observed later, would it even be possible to distinguish the authority of his influence from the accuracy of his account of Tokelau and the continuity of its culture?[6] Moreover, the question was not only idle (I never followed it up because the book seemed to bear so little connection to the lives of Tokelau people I knew) but also naive. It assumed a kind of reified entity called "Tokelau culture," even though there could *be* no such "thing" without practitioners like Penaia to perform it. More pointedly, there could be no Tokelau culture for me to study without "gatekeepers" like Penaia to manage my access to it.

To prefigure another part of my argument, I am inclined to think that an ethnographic monograph's chances of influencing the culture it describes are enhanced by two conditions: first, the receptivity of the local audience (a matter to be addressed later), and second, its reception in the outside world. These conditions also apply, it has to be said, to foreign academics' commentaries on larger societies, though the effect is magnified in smaller Pacific nations by disparities of scale. While a few monographs have had enormous impacts, such fame has clearly not accrued to Macgregor's effort. Few people outside the arcane fields of Tokelau studies or pan-Polynesian comparison are likely to have read it. The fact that Penaia owned a copy, however, indicates at least the possibility of some converse fetishization of the text in his world. The book was clearly a possession of some value, having been kept in a private room and protected from the depredations of climate and insects. It probably accompanied Penaia and Salani years later when they moved to New Zealand.

The next vignette is a footnote, *avant la lettre,* to a controversy surrounding one of the classic texts of Pacific anthropology. It concerns an encounter a few months after the one just described. The setting was Manu`a, the easternmost island group of American Samoa, which I visited in June 1971 with the aim of making contact with the small Tokelau community in that neighboring territory. While there, I was urged to make at least a brief visit to the island of Ta`ti where Margaret Mead had carried out her famous fieldwork in the 1920s. I was actually more familiar with Lowell Holmes' later research in the same village, however, and even quickly reread his monograph (Holmes 1958) in the Pago Pago public library before my trip. Apart from the anthropological and historical interest of the setting, my justification for the trip was an opportunity to interview Panapa, a Tokelau pastor of the Christian Congregational Church of Samoa at the main village on the island.

Unfortunately, things did not work out quite as intended. I caught the ferry to Manu`a, but, upon landing there, my inquiries produced a flurry of people pointing seaward over my shoulder. The good pastor and his family had stepped into the whaleboat I had just vacated and were being rowed out to the *Lady Lata,* the ferry that had brought me from Pago Pago. They soon vanished over the horizon. The purpose of their departure, I found later, was Panapa's mandatory six-yearly furlough of several months, during which time the village congregation would decide whether or not to invite him to resume his appointment.

Consequently, I spent the two or so days before my return voyage to Pago Pago being passed around as a shipwrecked stranger. In that capacity, I stayed one night in the small neighboring village of Faleasao at the home of Fagamanu Unutoa, a schoolteacher who left the next day and handed me over to his wife's cousin in Ta'ü. The household in which I now found myself was headed by an elderly *tülafale* (orator chief), holder of the Lauofo title.

My diary entry for 18 June includes the following notes:

> [Lauofo] knew Holmes; didn't have v. much to say, tho he was given a copy of *Ta`u [i.e.,* Holmes' monograph] wh[ich] someone took. But v. critical of Margaret Mead—"the first girl." Predictably his crit[icism] concerned what she wrote about sex: "girls and young men sleeping together and having carnal ways"—her book v. bad and v. wrong. . . . Lauofo also met J D Freeman [in] 1966—[he had] visited Tali.

I recall, but I did not record, Lauofo telling me that Derek Freeman had advised the chiefs and orators of Tali to sue Mead for spreading such calumnies about them."' We can safely assume that the "v[ery] bad" and "v[ery] wrong" book was *Coming of Age in Samoa* (1928), though the uncertainty or, more likely, the taken-for-granted nature of the reference is revealing.

Compare the fame of Mead's monograph with the comparative obscurity of Macgregor's. Lauofo almost certainly expected me to have heard of the former and to have at least a passing acquaintance with its contents (or, what is virtually the same thing, with the contents as diffused through myth and oral tradition).[8] As opposed to an exclusive or perhaps complicitous knowledge of the Tokelau *Ethnology,* which Penaia carefully regulated, *Coming of Age* had entered freely and spectacularly into the public imagination, a fame reinforced rather than diminished by Freeman's later attempt at a demolition job (1984). For Samoans to have two such strongly contrasting characterizations of themselves purveyed in anthropological literature simultaneously fosters a rather bemused pride in the amount of outside attention they have received and encourages the expression of an ironic attitude to those accounts (e.g., Rampell 1995, 36).

Samoa, of course, is a larger society than Tokelau, has had a longer history in the European imaginary, and has been written and published about much more extensively. It has also produced more scribes who direct their attention to local concerns, including well-published novelists, poets, journalists, and academics. This does not mean that Samoans as a whole are necessarily more literate than Tokelauans (both societies have extremely high literacy rates by world, especially developing world, standards), but they do seem to accept their appearance in print and any resulting controversies with notable aplomb. More importantly, the books written about them have provided important material for their own self-description. Apart from Mead's work, a case in point is the interest shown in Samoa and by members of overseas Samoan communities in the recent retranslation into English and republication of the first of Augustin Kramer's volumes on traditions and genealogies and material culture (Kramer 1994, 1996; qv. Meleisea 1994, 1996). And for an anthropologist in Samoa, ever since Robert Louis Stevenson put the place on the literary map, there has been a default cultural slot available as a *tusitala* (writer, storyteller), a term that was even applied to me as I scribbled fieldnotes while among people some of whose identities were caught between Tokelauan and Samoan.

Years later—and this is my third vignette—when I went to do doctoral fieldwork in Tuvalu there was a similar ready-made label for me, that of *fai-lautusi* (secretary). This designation mirrored the role of the man who acted as my mentor, the Reverend Alovaka Maui, general secretary of the Tuvalu Church, the dominant Protestant denomination.' He and I spent many evenings together in his office working on our respective writing and occasionally collaborating on letters and reports for the church. In line with what I suspect is a fairly standard ethnographic transaction, I also produced a statistical summary of congregation membership figures for administrative use in exchange for access to church records. On those occasions, in effect, I was Alovaka's secretary just as he was the church's.

But such an assumption of responsibility came after the transaction I am about to recount. In 1978, on the occasion of my first Christmas in Tuvalu, shortly after my arrival, I found myself short of gifts. It was a season when, because of the vagaries of shipping, the cooperative store on Funafuti was woefully understocked. I had foolishly omitted to cover my options by buying extra trade goods in Fiji on my way from New Zealand. Despairingly, I decided that a book I had brought with me, a hardback edition of Raymond Firth's *The Work of the Gods in Tikopia* (1967), might make an appropriate present for Alovaka. This hope rested on the fact that his postgraduate thesis on Bible translation (Maui 1977) had included a reference to D. G. Kennedy's classic study *Field Notes on the Culture of Vaitupu* (1931), which had recorded a "traditional" pagan ritual.

Penaia handing me his copy of Macgregor may have intended his action to highlight his role as a cultural broker. Conversely, I could be seen as engaging in the same game vis-a-vis Alovaka, in the sense of acting as a conduit to another world of Polynesia (which I may be quite mistaken in assuming was new to him). That was not strictly my intention. I had a copy of Firth's work with me in the same way that one may take a copy of Tolstoi's *War and Peace* on a long trip where reading materials are likely to be in short supply—that is, in order to be forced to read something worthy that one ought to have read before. But I did genuinely wonder how the book would be received.

As with Macgregor's *Ethnology,* however, I cannot say whether the book had any effect on its intended audience or what happened to that particular copy. In fact, I do not recall Alovaka ever mentioning it afterward. My own prestatory etiquette inhibited me from inquiring too deeply into the matter. Besides, he was usually busy on practical matters of church administration. I like to think, though, that eventually he would have found *Work of the Gods'* rich descriptions stimulating, just as he had clearly dipped into Kennedy's book and been influenced by it. He contemplated writing a doctoral thesis on religion and politics in Tuvalu and, as a brilliant concocter of fictions, he might have found in Firth a fruitful source for the (re)invention of local theological tradition.[10] These outcomes remain speculative because he died tragically young in 1982. The copy of *Work of the Gods,* if it survived the hazards of tropical pests, mold, and children, probably ended up in the church archives, where it may still be consulted by a curious pastor from time to time.

Reputations

Whether consciously or not, ethnography is always an ethnography of the self who happens to be an Other to the Others with whom s/he engages in mutual scrutiny. My experience in both Samoa and Tuvalu tells me that fieldworkers are routinely compared with one another by the subjects of their research. It would be an interesting exercise to collect these comparisons as well as to monitor the strategies by which ethnographers present themselves to local audiences and paper over their deficiencies in front of their peer groups. If the myth of scientific progress is to be believed, later researchers should be able to supersede earlier efforts; but, historiographically, "earlier" has the connotation of being closer to the primary sources. Does this explain the curious anthropological obsession with ethnographic precursors? There are always hidden questions as to "my" abilities in the field compared with those of my precursors, or whether I will be remembered as they were.

Put cynically, the vignettes suggest that there are at least three ways of making an ethnographic reputation. The first is by *being* first or, since this is virtually impossible, by being acknowledged as such. Bronislaw Malinowski's lines in his *Diary* about making the Trobrianders "his" are an example of this sort of ambition: "Feeling of ownership: It is I who will describe them or create them" (1967, 140). This strategy worked for him with the publication of *Argonauts of the Western Pacific* in 1922, just as it did with Mead's youthful work on Samoa. Secondly, there is the strategy of *destroying* the reputation of those who came before.[11] Malinowski was not above using this strategy as well (viz., his debates with A. R. Radcliffe-Brown and the diffusionists), but in recent times it is probably best exemplified by Freeman's critique of Mead. Straddling the other two is the *mediating* model of apprenticeship and patronage, of receiving the mantle from the early explorer and shielding his or her reputation against later critics (a strategy also well represented in the Samoan controversy by Mead's defenders).

The Samoa dispute became even more scandalous in the wider culture that had assimilated the myths created by Mead. It undermined the trust attributed to those supposedly interchangeable ethnographers whose task is to represent Others to academic and nonacademic audiences. However, among the discipline's practitioners, much of the resulting discomfort, I suspect, had to do with factors pertaining to the assumptions behind ASAO's way of arranging its symposia, as discussed earlier. Freeman's attempt to undermine Mead is the analogue in time of another ethnographer's invasion of a fieldworker's territory in space. Knowing one's own place means refraining from intruding on another's space, a violation that all ethnographic restudies imply. Yet to defend against it fails to recognize that such invasions have taken place constantly before, since, and during the heroic era of anthropology. In consequence, no reputation is secure.

The options for making a reputation are only ideal types, of course, and people's motivations are inevitably more mixed or muted in practice. These days, the sheer weight of prior anthropologizing makes such strategies increasingly pointless, unless there are other simplifying factors at work: the size of the reputation one wishes to destroy (e.g., Mead); the degree of difficulty of access to the field site (e.g., Tikopia); the putative absence of previous ethnographers (nowadays a rare phenomenon indeed). Even though Tuvalu, the society where I wound up doing doctoral research, is farther off the beaten track in ethnographic and historiographic terms than Samoa, the list of social science researchers who have studied there is still impressive.[12] It includes Gerd Koch, Arne Koskinen, Ivan Brady, Barrie Macdonald, Doug Munro, Niko Besnier, Anne Chambers, Keith Chambers, Peter McQuarrie, Jay Noricks, and Barbara Liiem. That is not counting, in reverse chronological order, the scattered writings of administrator-ethnographers like Robbie Roberts and D. G. Kennedy; Cara David's well-known sideline account of life on Funafuti as companion to her husband's geological expedition; Charles Hedley's ethnological studies from the same fin de siècle era; the occasional scholarly work of missionary observers like Archibald Murray, Stuart J. Whitmee (himself a contributor to the *Journal of the Anthropological Institute of Great Britain and Ireland),* the Georges Turner (father and son), and—delving indeed a very respectable distance back into the history of cross-cultural research in the Pacific—Horatio Hale, the word-list-collecting prodigy of the 1838-1842 United States Exploring Expedition.[13] In short, no contemporary ethnographer can claim to be first, and the "firstness" claimed by previous generations was always in part a socially constructed phenomenon, dependent on the suppression of others (travelers, missionaries, administrators, proto-ethnographers of all stripes).

Penaia Kitiona made me aware not only of an earlier fieldworker but also of *his* awareness of my precursor and, by implication, of the likelihood of repetitiveness and unoriginality in my own work on Tokelau. In the second vignette, my position was that of bemused bystander to a dispute between another earlier fieldworker in Samoa and the subjects of her research, a dispute fomented by but probably not originating with a researcher who pursued her across the decades. The third story portrayed me as a smalltime cultural *bricoleur* in the domain of Tuvaluan church discourse and, potentially at least, a coinventor of tradition.

The first two examples (Lotopä and Manu`a), in particular, are clichés, a failing for which I make no apologies. Banality *is* precisely one of the issues I am trying to highlight. The notion of ethnographers being confronted and supplemented by the existence of other ethnographers has become, as I mentioned earlier, almost a standard trope of anthropology. The cases I have sketched are not special. They could be multiplied endlessly from my experience and that of others. The phenomenon highlights the evolution of ethnography as a third-, fourth-, and fifth-generation enterprise, one in which we latecomers inevitably live in the shade of ancestors. This generational pressure is intensified by other developments: the complicity of indigenous subjects in the process of circulation; the sheer proliferation of ethnographers and their varying degrees of willingness to pass manuscripts around for commentary, to offer copies of their publications in reciprocity for fieldwork help, and to donate books or theses to local archives and libraries; and our growing willingness to retrospectively widen our inclusion of certain historical figures (explorers, governors, traders, missionaries) within the genealogies of anthropology. Why, indeed, should we single out previous ethnographers as especially influential? Were not all sorts of "others" relevant to later discourses, especially—given the importance of literacy—teachers and missionaries?

A subtext of all three episodes that concerns me here, then, is the contingency of the collection and diffusion of knowledge through writing. This variability is, of course, linked to educational structures and power/knowledge relations as well as to the time depth of colonialization and missionization. The Pacific is a sea of literacy, but, like all, seas, some parts of it are deeper than others. And not only is literacy structured by depth (especially historical) but also by societally differentiated access to knowledge. How important has the secondary and tertiary education of Pacific elites been in the diffusion of "Western" ethnographic knowledge to their region of the world? I suspect its influence is considerable. *Coming of Age in Samoa* is a classic example of the spread of anthropological views into school and college syllabi throughout the world, including the classrooms and lecture theaters where Pacific Islanders have tended to congregate.

Along the way, the subjects of culture have become textualized on a broad scale. Pacific ethnographies are on reading lists at regional universities in Port Moresby, Suva, and Mangilao. At the University of Waikato, the tertiary institution in New Zealand where I teach, a substantial minority of the two hundred or so Pacific Island students on campus (not counting those with New Zealand residency or citizenship) takes courses in social science subjects. While this includes social anthropology, it has to be said that more people enroll in political science, history, and geography, and there is a general preference for law and management degrees. Essay questions for my undergraduate course in Pacific politics are discussed in late-night cheap-rate telephone calls to Fiji, Tokelau, and Solomon Islands where cabinet ministers and island council members deal patiently with the queries of their children and other relatives.

None of this is meant to imply that the self-reflection of Pacific societies is a straightforward function of cultural transmission. For a start, it is generally children of educated elites who go to university, but barriers to tertiary education are not the sole means of exclusion. Access to knowledge in both its "traditional" and "modern" guises has routinely been monitored and restricted in

Pacific societies. I suspect, for example, that Penaia would not have shown his copy of *Ethnology of Tokelau Islands* to all members of his own community.[14] Nor would everyone's English have been up to the task of reading it. Access is therefore affected by linguistic competence as well as by regulation and academic privilege. Lauofo's reaction to *Coming of Age in Samoa* may or may not have been based on direct acquaintance with the text, but if he had read Mead's book he undoubtedly did so in the most widespread elite Pacific lingua franca, i.e., English, a language that now also makes Kramer's work accessible to a new generation of young Samoans. It seems ironic that one of the most important issues surrounding the comparison and self-representation of Pacific cultures is the choice of which "international" language to carry out these activities in.[15]

Reforming Boundaries

Alberto Melucci writes: "The particular form of action which we call research introduces new cognitive inputs into the field of social relations, derived from the action itself and from the observation of its processes and effects" (1992, 50), Melucci, Alain Touraine, and other observers of contemporary social movements have been struck by the need to see researchers as part of the social field they describe and to consider the subjects of the research as engaged in a reflective process of societal steering that may be influenced by the information that researchers can provide. The study of Pacific societies demands a like awareness of the reflexivity of research. Reflexivity, like ethnography itself in the ASAO tradition, however, can be interpreted in an unduly concrete way. Its demands are not exhausted by adding a personal or confessional dimension to ethnography or by ritually claiming a particular standpoint or sociopolitical identity, with all the subsequent advantages or disadvantages such positioning provides. Academic writing *always* reveals its auspices, which may or may not be those claimed for it. In this chapter, I have been trying to read my own words and those of others for what they show rather than what they say.

My objective is not to provide definitive answers to the various questions raised by the editors of this volume but, rather, to address the issue of what might be adequate questions, theirs and mine, to bring to the study of the ethnographic study of ethnography in the first place. In dealing with this issue, we should beware of certain conceptual traps. In particular, I view with skepticism a tacit model of anthropological tradition, in which "descent" encompasses the sense not only of kinship with founding ancestors but also the notion of decline from an original pristine purity of ethnographic intention. By contrast, I want to ask questions that suggest different boundaries to anthropology and the ethnographic enterprise. In short, I am intrigued by the limits imposed on the topic and by the interests or fantasies expressed and suppressed in its formulation. It should be apparent from my argument that I have a more relaxed view than many about those boundaries, if only because (1) I find it difficult to impose a foundational definition of ethnography that would neatly exclude other contenders for authoritative cultural knowledge, and because (2) in my view no ethnographer is able to trace with certainty the flows of knowledge in any cultural domain.

A postcolonial ethnography that incorporates the flow, permeability, and contingency of cultural traditions may or may not be possible, but it would be more faithful to my own experience of research. By contrast, in Judith Macdonald's case study for this volume (chapter 6), the problem is to explain the Tikopia preference for an ethnographic image dating back to the colonial period. Perhaps their attitude allows them to "be" Tikopia, renders them static and part of a stable classificatory system. It also clearly gives them status within a colonial jurisdiction, a status that might be threatened by majority rule in a postcolonial democracy. It seems that the tendency to atavism within anthropology is sometimes mirrored by its subjects. Among the many lessons to be drawn from this is that anthropology cannot legislate its own reception.

Ethnographers have described many cultural worlds of the Pacific with subtlety and energy, but those worlds were and are always more complex than most standard forms of ethnography have recognized. The ASAO model for the presentation of expertise, while an impressive vehicle for demonstrating ethnographic skills and thoroughness, has yet to reform the accepted boundaries of the discipline or the tradition of Pacific societies seen as "social wholes." It has depended on a division of labor that allocates theory and field-work to different roles, it has recognized ethnographic authority as accruing to those with a concretely territorial claim to represent others, and it has encouraged a static, monocultural sense of its audiences. I hasten to add that ASAO is not unique in this regard; these strictures apply to academic anthropology in general. Moreover, change is always possible as ethnographers strive to reinvent their discipline beyond the boundaries of the possible. But the historically closed and compartmentalized nature of academic knowledge means that challenges to its perceptual boundaries tend to result from the serendipitous recognition of moments where one does not "know one's place."

Notes

Besides the people whose hospitality and kindness I mention in the text, I would like to thank Antony Hooper and Judith Huntsman of the Tokelau Islands Migrant Study project and its medical director, Ian Prior, for my initial invitation to ethnography; the Spalding Foundation and the University of Waikato for Tuvalu fieldwork support; Cherie Flintoff for research assistance; Dorothy McCormick for providing access to student enrollment data at the University of Waikato; and Judith Macdonald, Marta Rohatynskyj, Sjoerd Jaarsma, and the anonymous readers of the University of Hawai`i Press for their critical acumen and encouragement.

1. An interesting e-mail discussion on ASAO process and the so-called three-year rule (which glosses the preferred trajectory of a topic from an informal development of ideas to the final presentation of formal papers over a period of three consecutive annual conferences) ran on ASAOnet in March and April 1995. While it never explicitly addressed the matters raised here, it indicated an ongoing concern for the fonnat of ASAO sessions and for the conventions that have developed to justify that format. More to the point, it also showed the commitment of many members to practices that set the smaller and friendlier ASAO above the impersonality, hierarchy, and superficiality of American Anthropological Association meetings. An unwillingness to tinker with tradition is even more understandable in this light.

2. On the one hand, it seems to me that the (in)comparability of scale and the often radical incommensurability of the societies and case studies offered for ASAO scrutiny should be a matter for analysis; on the other hand, this incongruity may have been one of the few factors to destabilize the model I am criticizing.

3. This kind of personal history can be explored fruitfully in relation to the concept of "decenterering," which I examined recently in a paper on the subject of Pacific biographies (Goldsmith 1995).

4. I carried out this research as the most junior member of the Tokelau Islands Migrant Study team, the senior anthropologists being Tony Hooper and Judith Huntsman of the University of Auckland, where I had recently begun graduate studies.

5. There is insufficient space here to give full details of the situation of the Tokelau population in Western Samoa at that time. Suffice it to say that there were other smaller groups near Apia, as well as (part-) Tokelau families and individuals who had married into *'aiga* throughout the Samoan archipelago. In fact, there had been considerable contact between the two island groups for decades, if not centuries. This interaction intensified under the aegis of London Missionary Society (Protestant) and Roman Catholic missionary activities in the second half of the nineteenth century. While New Zealand was the administering power for both societies (1914-1962 in the case of Western Samoa, 1925—present in the case of Tokelau), Tokelau

people had visited and found new lives among their high-island neighbors with comparative freedom. After Western Samoa gained independence, however, many Tokelau people began to be faced with a choice of remaining New Zealand citizens or more fully assimilating into Samoan society. At the time of my fieldwork, the Tokelau Administration was still based in Apia, though it was under the direct jurisdiction of Wellington.

6. Judith Macdonald highlights precisely this difficulty in her chapter on Tikopia in this collection (chapter 6).

7. I may not have recorded this striking detail simply because of a filtering assumption that legal aspects of publication lay outside the "normal" realm of ethnography. A quarter of a century later, most fieldworkers would probably be far more conscious of the implications. See, for example, an interesting recent case study exploring the risks and implications of litigation by the subjects of one's research (Lee and Ackerman 1994). The authors attribute an increasing tendency for previously underprivileged groups to seek legal redress for past wrongs to a recent upsurge in "global embourgeoisement." I see it more as the globalization of certain kinds of cultures, including those tied to writing and other forms of media, which have been building to a political climax for centuries.

8. No putdown or great cultural contrast is intended by this assertion. Most ideas, in literate as well as nonliterate societies, are transmitted through oral tradition, in forms ranging from talkback radio to academic lectures.

9. For a fuller description of the background to my research on church and society in Tuvalu and of my relationship to Alovaka, see my Ph.D. dissertation and a recent paper (Goldsmith 1989, 1996).

10. Such ideas were clearly in the air at the time Alovaka was undergoing his theological education. See, for example, Garrett and Mayor (1973).

11. I owe this barbaric formulation to Judith Macdonald.

12. Marta Rohatynskyj (chapter 9) notes that difficulty of access is not always as straightforwardly linked to the absence of previous researchers as one might imagine. Indeed, when the Tolai and the Baining are compared, the relation may even be inverse: the farther off the beaten track, the more researchers.

13. My list of references would stretch to unreasonable proportions if I were to cite even just the main works of these academics and commentators. Interested readers may consult the bibliography compiled for my dissertation (1989).

14. Penaia also gave me access to records of the Tokelau Association in Western Samoa. The information these contained would have had some political sensitivity in the occasional disputes between different factions of the Tokelau community, but the association *(Fakalapotopotoga)* was generally moribund at the time so the issue did not arise.

15. This raises the question of how much anthropological, historical, and social

science literature published in "international" languages has been subsequently or simultaneously translated into local vernaculars. As far as the Pacific is concerned, there appears to be very little: a booklet on the Vaitupu Company (Isala and Munro 1987), translated into Tuvaluan from an earlier article (Munro and Munro 1985), and the bilingual editions of *Kiribati: Aspects of History* (Talu 1979) and *Matagi Tokelau* (Hooper and Huntsman 1991).