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CORRUPTION AND GOVERNANCE IN THE SOUTH PACIFIC

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Talk about corruption has increased in the South Pacific, though it is hard to tell if there is more corruption or just more reporting of it. Defining corruption as "the use of public office for private gain," this article considers some recent reports of corruption in relation to other issues in South Pacific politics: tradition, ethnicity, aid, land tenure, sovereignty, and governance.

"CORRUPTION SEEMS TO BE GAINING GROUND in the highest ranks of our leadership," announced Vanuatu's president in a public address on the 1993 Constitution Day (Timakata 1993). Two years later Papua New Guinea's governor-general warned that "innuendos, manipulations, undercover deals, greed and corruption are becoming deeply rooted in this society (*Australian*, 14 June 1995, 17).

Suspicion of corruption has contributed to the crisis the PNG government recently faced over the use of mercenaries on Bougainville (Regan 1997), with the governor-general reported as referring to the "termites of corruption" (ABC Radio, 24 March 1997). Meanwhile the World Bank has announced a "renewed approach" to preventing corruption, including a revision of its own global-lending policies (*The Independent,* 14 February 1997).

Corruption is hard to pin down, in principle and in practice. Transparency International, the anticorruption nongovernment organization, distinguishes between "grand" corruption, or the use of public office for private gain, and "petty" corruption, in which officials demand "facilitation payments" to carry out perfectly legal tasks, such as clearing a container from a wharf, that they are supposed to perform in any case (Pope 1996). The examples

used in this article refer mainly to grand corruption, which is often linked to election campaigning.

A characteristic of corruption is often the attempt to conceal it, so it is hard to tell if greater visibility means more corruption—or just that more is being found out. The situation in the South Pacific is probably both. The greater visibility of corruption is partly a reflection of the effectiveness of anticorruption agencies, such as ombudsmen enforcing leadership codes in Melanesia, the courts in Kiribati, ad hoc commissions of inquiry in the Solomon Islands, or the auditor general in Western Samoa. Visibility is also related to changes of government, as new governments take power and reveal the excesses of their predecessors. The number of examples quoted here for a particular country does not necessarily imply there is more corruption there, merely that more is being suspected or found out.

Greater visibility can more generally be considered a reflection of the increasing spread, competence, and confidence of the media in the region. Newspapers have an interest in scandal. While they sometimes expose corruption directly, more often they publicize the activities of state agencies that have uncovered corruption, agencies whose reports would otherwise be suppressed or not acted upon. The Barnett Commission of Inquiry into the timber industry in Papua New Guinea, for example, produced a series of seven interim reports and a two-volume final report deeply embarrassing to several politicians. Only two of the interim reports were ever printed and distributed. However, long extracts were published in the *Times of Papua New Guinea,* a church-owned weekly newspaper.

The importance of the media in fueling the campaign against timber-industry corruption was attested to, in a backhand way, when the logging company Rimbunan Hijau set up its own daily newspaper, the *National,* in 1993 (Robie 1995:28–32). By undercutting advertising rates and hiring staff away with offers of better conditions, the *National* caused or contributed to the demise of the *Times* in 1995. The *National,* according to journalist Rowan Callick, now "steers well clear of the huge issue of logging" *(Islands Business,* June 1995, 47).

There certainly seems to be more talk and moralizing about corruption in the region. Politicians are widely suspected of it. The word itself (in English) carries connotations of decline, decay, and falling away from the high ideals of the past. It has religious overtones in the strongly Christian countries of the region. In this essay, I survey some well-publicized examples of corruption in the South Pacific and try to understand them in relation to some other issues in South Pacific politics: tradition, identity, land ownership, privatization, aid, and sovereignty. These are issues in a wider study of governance in the region.

Tradition

Politicians accused of corruption sometimes defend their behavior as being traditional, or they appeal to a disjunction between local traditions and introduced (colonial) law. A distinction between "public" and "private" is hard to sustain in reference to stateless societies like those that preceded colonial rule or in a constitutional monarchy, like Tonga's, where "crown land" is to some extent still the personal property of the monarch and his family.

A PNG prime minister excused himself from criticism over payments made to prevent a minister's defecting to the opposition by arguing that "gift giving" is part of the Melanesian political tradition (*Pacific Islands Monthly,* June 1992, 12). Yet a brisk distinction between gifts and bribes can be made, for example by a former Nigerian head of government, that "the gift is usually a token. It is not demanded. The value is usually in the spirit rather than the material worth. It is usually done in the open, and never in secret. Where it is excessive, it becomes an embarrassment and it is returned. If anything corruption has perverted and destroyed this aspect of our culture" (Obasanjo, quoted in Pope 1996:5). A bribe is thus substantial, insisted upon, and secretive. However, the distinction between traditional gifts and modern bribes creates two other possibilities to explore. What did traditional bribes look like? And what do modern gifts consist of?

The distinction between gifts and bribes has been addressed by a number of South Pacific courts interpreting electoral legislation. The results have been somewhat inconsistent. In Western Samoa the Supreme Court canceled the result of a by-election after it found that money, cigarettes, and food had been distributed before the election, which, though of no "great magnitude," amounted to a "systematic and sustained effort to subvert the electoral process" (*Islands Business,* February 1992, 10).

In Kiribati, a government minister who made gifts of tobacco during his election campaign was found guilty of electoral malpractices and dismissed from office. He was then replaced as a member of parliament by his wife, who made similar gifts and who was also prosecuted and dismissed (Koae 1993). Yet a presidential candidate who gave gifts of tobacco to elders within *maneaba*s (meetinghouses) a few years later was found by the High Court not to have acted corruptly (High Court of Kiribati, 30/94). Among its reasons the court found continuity with an older tradition of *mweaka,* in which tobacco was left at a shrine; it was an offering rather than a gift; it was obligatory for visitors; the amount was not excessive; and the candidate explained, when making the offering, that it was not meant to influence the vote (ibid.:64–73).

The Kiribati court also noted that public-service regulations sanctioned such payments by visiting officials. The court distinguished its decision from the earlier decision against the minister by arguing that, in that case, the gifts of tobacco had been made outside the *maneaba,* or "inside a *maneaba* but [one] to which the respondent had not been invited and to which he was not a visitor, as he lived locally" (ibid.:73). Thus the court was making decisions about context: what was appropriate behavior in one context was corrupt in another.

When leaders try to conceal their behavior it suggests that they are not confident that others will either recognize it as "traditional" or approve of it as such. Traditional habits of deference to leaders may have prevented open criticism but not grumbling and other actions that have been called "weapons of the weak" (Scott 1985). Leaders may be lamenting a loss of deference, rather than any substantive change in values: people are now more willing to complain, and they have agencies to complain to.

In any case, countertraditions may be invoked against corruption. A notion of excess lies behind the traditional Polynesian criticism: "Chiefs that eat the power of government too much" (Sahlins, quoted in Kirch 1989:254). Popular anger at grand corruption, evident in recent demonstrations in Papua New Guinea, for example, also draws on subsistence traditions that make people suspicious of accumulation. These egalitarian traditions may lead to perceptions of more corruption than exists.

Traditions are constructed, and reconstructed, as such over time. People often look back to a period when there was less corruption. Sometimes this golden age is thought to be traditional times, when leaders were felt to be more accountable to their followers, and at others the colonial era, when officials were distant but viewed as incorruptible.

Kinship, Nationalism, and Identity

Mauro found some statistical evidence of correlation between ethnolinguistic fractionalization and corruption, and suggested officials might be favoring their own ethnic groups (1995:692–695). This argument is reflected in the popular theory of *wantok*ism in Melanesia. People readily suspect that others have been appointed or promoted for reasons of kinship or friendship rather than qualifications.

Even policies favoring national entrepreneurs and affirmative action for citizens can also attract suspicion because of the ways they are implemented. In 1986, stock in the mining company Placer Pacific was floated in Australia on the basis of its rights to mine at Porgera and Misima in Papua New Guinea. The PNG cabinet directed the prime minister, who was also minister of

Finance and Planning, to insure that the 10 percent of company shares promised to Papua New Guineans was taken up. To do so, he relaxed foreign-exchange controls and arranged a loan with the Papua New Guinea Banking Corporation so that many members of PNG's elite, including his relatives, could buy shares (*Far Eastern Economic Review,* 9 April 1987, 134–135). Tax concessions to the prime minister's timber company in the Solomon Islands and duty exemptions to ministers and other local businesspeople in Vanuatu have also been defended in terms of encouraging national, rather than foreign, companies (*Solomon Star,* 28 August 1996; Vanuatu 1996b). Policies like Papua New Guinea's or the Solomon Islands' to take "affirmative action" on behalf of indigenous people come uncomfortably close to the use of public office for private gain.[1]

Similar issues have been raised by the recent National Bank of Fiji scandal. The bank had rapidly expanded its lending in the early 1990s, to the point where its lending exceeded its deposits and it had to call on government support. An audit report found many nonperforming loans and loans made outside the bank's own guidelines, including to its own staff members (*The Review,* July 1995, 16–32). The bank had lent to people of different ethnic groups but in a political climate particularly sympathetic to indigenous Fijian enterprise. The 1990 Fiji Constitution specifically provided for affirmative action. Politically well-connected companies held loans. The elements of commercial incaution, procedural breakdown, political pressure, and government policy are hard to disentangle; but they provide a context in which the use of public office for private gain becomes both more possible and widely suspected.

A more fundamental moral process than favoritism may also link ethnicity and development. In subsistence and peasant societies Evers and Schrader identify what they call the "traders' dilemma," which "arises out of their moral obligation to share proceeds with kinsfolk and neighbours, and the necessity to make profits and accumulate trading capital on the other" (1994:5). The dilemma can be resolved in various ways, including accumulation of status honor, the emergence of cash-and-carry petty trade, the depersonalization of economic relations, the immigration of trading minorities, and the formation of ethnic or religious groups outside the "moral community" of the subsistence society (ibid.:10).[2] Trade in many parts of the South Pacific historically has been carried out by immigrant minorities outside the "moral community" of indigenous peasant and subsistence societies (for example, Vietnamese in Vanuatu, Chinese in Papua New Guinea and Fiji, Indians in Fiji, and Europeans throughout the region).

The traders' dilemma suggests that perceptions of corruption may be associated with the formation and maintenance of group identity: "We are

pure, caring, and sharing" goes the story. "They" (Indians, Chinese, Vietnamese, Europeans) are corrupt because they do not distribute their wealth. The PNG crowd's hostility to Chinese businesses and to the Chinese heritage of PNG's prime minister is a recent example. The theory helps to explain the persistent difficulties of indigenous business and the strained alliances between indigenous and minority or immigrant businesspeople (such as the relationship between a former prime minister and a businessman in Vanuatu described in *Islands Business,* July 1994, 24–27). The theory predicts that ethnic tension may increase with development, as a greater moral load is carried by trading outsiders or minorities.

Land Ownership

The timber industry in Melanesia has become a spectacular site for corruption, documented by PNG's Barnett Inquiry and by the Solomon Islands ombudsman (Barnett 1990; Solomon Islands 1989). In both countries, politicians and public servants have been using their public offices for private gain by seeking and accepting bribes to issue licenses, waive regulations, or improperly influence landowners. In Papua New Guinea the Barnett Inquiry found political leaders actively soliciting cash and favors from overseas logging companies. A new forestry minister tried to cover up his share holding in a timber company he was supposed to be regulating and was later found guilty of more than eighty charges of corruption. The secretary for Forests was accepting gifts of golf clubs and cash from a Malaysian timber company, while the premier of New Ireland was writing to another company in code, asking for payments of "cabbages" (one thousand kina) and "apples" (one hundred kina). There were many other instances where "foreign operators misled and bribed local leaders, set up 'puppet' native landowner companies, bribed provincial government, premiers or ministers and gave gifts or bribes to national ministers or members of the national parliament or took such people into some form of partnership with them" (Barnett 1990:100). Timber companies also funded election expenses. In the Solomon Islands, the ombudsman cited evidence that members of area councils deciding on applications from timber companies were given "Negotiation fees, Employment and Hotel stays in Honiara" (Solomon Islands 1989:10–11).

Clearly there are many causes for the comprehensive corruption of the timber industry. These include the business practices of companies, and local and national leaders and officials who are ready to demand and accept bribes. A geographically dispersed industry such as timber is necessarily difficult to supervise centrally, and processes of assessment and categorization of logs that depend on discretion are wide open to abuse. However, the specific institutional framework provided the opportunities.

In both the Solomon Islands and Papua New Guinea, changes in policy and legislation during the 1970s had encouraged customary landowners to harvest forests themselves or to deal directly with foreign companies. These changes in law, like the *Forestry Private Dealings Act* in Papua New Guinea, or in policy, like the New Forest Policy in the Solomon Islands, were reactions to the paternalism and exploitation of colonial policy, in which governments had acquired timber rights from landowners and sold the rights to foreign companies. A new group of local politicians, businesspeople, and lawyers filled the intermediary role previously played by forestry officials. The government was supposed to regulate private dealings by, for example, assessing company proposals, identifying landowners, and insuring negotiations were carried out fairly. In practice, officials were overwhelmed by pressure from two directions. From one direction, budgetary pressures and the personal and political interests of government ministers coincided to encourage the introduction of more logging companies. Ministers intervened directly in licensing. From the other direction, local leaders saw an opportunity for cash and the prospect of development for remote communities. In both countries, decentralization of responsibilities to provincial governments divided forestry departments and officials caved in to political pressure, were bought off, or retreated into cynicism.

Both examples demonstrate the limitations of arguments for a minimal state that merely regulates the private commerce of landowners and foreign companies.[3] On the landowners' side, naiveté and the problems of acting together prevent people from acting in their own best interests:

> Rural people are on their own. Money from logging royalties tempts them and corrupts and divides traditional communities. Their educated leaders may be singled out for special treatment—high court injunctions or more pleasant favours. People are often too disorganised to start a court case, let alone win. I regret to report that those who have been most successful appear to have achieved it with some element of physical force or threat. (Solomon Islands 1989:16–17)

On the investors' side, insecure agreements and the prospect of sudden bans encourage rapid exploitation, with minimal fixed investment. Entrepreneurs bring both sides together, but they are often the same politicians and lawyers as the regulators. The supposedly evenhanded state is also desperate for logging revenues to fund its budget. Yet prospective national and community benefits become privatized in tax evasion or consultancy fees.

In these circumstances it is hard to be good: the incentives all flow the other way.[4] The forestry debacles also suggest a wider view of "public office"

that includes, for example, professions like lawyers and the role of spokespersons for traditional landowners. The institutional breakdowns are not limited to government agencies but include institutions of land tenure, such as "communal ownership," which are easily exploited by unscrupulous middlemen or local leaders. There seems to be a wholesale abuse of "trusteeship"—by government officials, local politicians, lawyers, and other intermediaries.

Privatization

If corruption is the use of public office for private gain, then one way of reducing it is to reduce the scope of "public" activity. This can be done by reducing the number of regulations that officials can use to extort bribes and by limiting the role of government in business. Arguments from first principles suggest privatization may reduce corruption by making enterprises more accountable to consumers and by requiring them to become more efficient. Or privatization may simply transfer public resources into well-connected private hands.

Such a transfer took place in the sale of government housing in the Solomon Islands in the early 1980s. The ombudsman described how

> Government houses have been sold off—invariably without proper tendering procedures, usually at undervalue and to public servants in Honiara who have some kind of influence on the transaction. These sales—made under the guise of "easing the burden of repairs on Works division" or "increasing house ownership among Solomon Islanders" have led to unfairness and corruption, a chronic housing shortage (since neither the Government nor other authorities have managed to build enough new houses); large income and capital losses to the government; dissatisfaction from those who have not benefited and other problems. (Solomon Islands 1988b:9)

The ombudsman's report describes how a 1985 committee appointed to allocate housing allocated themselves houses and how many of the people awarded houses managed to avoid paying anything for them. Some, quite legally, continued to receive housing allowances. A civil service strike in protest led to a Commission of Inquiry, which recommended the allocation be redone. No one was prosecuted—the ombudsman notes that the director of Public Prosecutions had been advising the government at the time and had himself "won" a house, and so was "in no position to act in this matter" (ibid.:10).

The World Bank now recognizes that privatization "presents corruption risks of its own" (*The Independent,* 14 February 1997, 24).

Aid

South Pacific governments receive relatively high levels of foreign aid, which is often delivered by private contractors and their local counterparts. Aid donors are becoming more interested in anticorruption activities, in part because the aid process is itself vulnerable. The bi- or multilateral character of the transactions and difficulties of supervision create opportunities for corruption.

For example, a World Bank primary-school project in the Solomon Islands in the 1980s was intended to provide training, purchase textbooks, and provide building materials for the construction of classrooms and expansion of the Teachers' College. An Australian consultant was appointed as project director. A government Commission of Inquiry subsequently found evidence that the permanent secretary in the Ministry of Education had received payments, above his official salary, from the consultant—though both denied it (Solomon Islands 1988a:30). The commission found that the permanent secretary and the consultant had "a very close business and personal relationship," including both serving as directors of a local consultancy firm. That firm had a contract with the Solomon Islands government to recruit Australian-funded staff, but it was apparently not involved in the primary-education project (the firm's other directors included a former prime minister; ibid.:67–69). Exercise books, stationery, and other educational materials had been ordered at "exorbitant prices" from another local company, without tender (ibid.:15–25). Some of the project textbooks seem to have been ordered through a company partly owned by the consultant's wife (ibid.:43–44). A curriculum-development component was added to the project, then awarded to the Australian consultant outside Solomon Islands government and World Bank guidelines (ibid.:51–54). The commission concluded that the consultant should not have been allowed to play "a dual role as education adviser/project director and as a businessman representing a company in a contractual relationship with the project" (ibid.:62).

Aid funds were also at stake in the IPSECO power plant scandal in Palau. In 1981 the president signed a contract with IPSECO, a British firm, to build Palau a relatively expensive power plant. It was financed by British banks on the basis of optimistic revenue projections and an American government promise that aid funds would be available to pay for it under the Compact of Free Association, which was still being negotiated with Palau. The power plant was built and worked, but in 1985 Palau defaulted on its

first repayment. Palau went on the offensive, accusing IPSECO of fraudulently promising the project would be self-financing. When IPSECO went bankrupt in 1997, it was found to have paid over a million dollars to Palauan officials, including the current president, who had been Palau's ambassador to the United States at the time, and his brother, who had been speaker of the House of Delegates (Aldridge and Myers 1990:97–118; Leibowitz 1996:47–180).

The former ambassador argued, technically, that his job as ambassador had been unpaid and that Palau had no law against conflicts of interest. In any case, there was no substantial conflict: he never acted against the Palau government's wishes, nor did he change his views as a result of the payments. Palau needed a power plant and the United States had not provided one. And if the plant failed to make money, the United States would have to pay (Leibowitz 1996:55). Although he may have used his public office for private gain (the IPSECO payments), he did so in a maneuver that traded on Palau's semisovereign position (negotiating with IPSECO, while relying on U.S. guarantees and the prospect of aid). The ambiguity about sovereignty left Palau with a functioning power plant and IPSECO bankrupt. Clearly certain Palauan officials made private gains from their public or semipublic offices. Yet their activities were clearly in the Palauan public interest, endorsed by the legislature, and it was other taxpayers' money that was at stake. The IPSECO case is one of a wider category of scams that involve trading on a country's sovereignty.

Sovereignty

A common form of corruption in the South Pacific is linked to schemes to trade in tokens of sovereignty. Governments produce stamps, coins, or phone cards that are more valuable to collectors than their face value. Vanuatu and the Marshall Islands license merchant ships (North 1994). There have been official schemes to sell passports in Tonga and more recently in the Marshall Islands and Kiribati. Fiji recently considered a proposal to sell citizenship to seven thousand Hong Kong Chinese "units" (families) through a semiofficial office in Hong Kong (Naidu and Nata 1995).

Some of this trade is legal, at least onshore—though it generates flows of commissions, semiofficial jobs, and consultancies that are hard to track. Countries that provide offshore banking secrecy and low taxes are presumably trading on tax evasion elsewhere, as is being uncovered in the Cook Islands "wine box" inquiry in New Zealand (Wishart 1995). The illegality is sometimes hard to keep offshore. An Australian federal minister resigned after revelations that he had written a reference and phoned the president on

behalf of a businessman accused of misleading Chinese investors in a scheme involving Marshall Islands passports. The Cook Islands Philatelic Bureau became a sort of private bank for a political party and funded the fly-in voters that won them the election of 1978 (Crocombe 1979).

A common scheme is for government ministers to issue letters of guarantee that overseas financiers use to raise loans more cheaply or without the conditions insisted upon by domestic or international banks (North 1996). The corruption here is not necessarily personal, though kickbacks and commissions may be involved. The scams are not new, either: in 1982 the Solomon Islands Monetary Authority stalled a scheme involving several ministers to raise SI$200 million overseas. One had been involved in even earlier schemes to mint commemorative coins (1975) and promote chain letters (1972) (Larmour 1983:270–271).

In 1994 the Cook Islands government experienced a run on its currency, and the Westpac bank was reluctant to extend its overdraft. Instead the government issued a series of letters of guarantee to a company based in the Bahamas, which would use the letters to borrow money for its own dealings and pay the Cook Islands government a fee from the loans. The Cook Islands would in theory, through a special clause, protect itself from having to honor its guarantee (*Cook Islands Press,* special issue, 25 May 1995).

The Vanuatu ombudsman reported on a similar scheme in 1996 (Vanuatu 1996a; *Pacific Islands Monthly,* September 1996, 23–27). The prime minister, the minister of Finance, the Reserve Bank governor, and the first secretary to the minister of Finance signed guarantees worth US$100 million. These guarantees were supposed to be secretly traded, for a commission, by a foreign businessman who had been given a Vanuatu diplomatic passport. The ombudsman noted that the "scam in this case is very similar to that perpetrated in the Cook Islands" (Vanuatu 1996a:8).

The preferred form of sovereignty scam in the U.S. territories is the bond issue, supposedly to fund local infrastructure and housing. In 1988 a U.S. federal grand jury handed down indictments against a Wall Street underwriter involved in a scheme to bribe Guam's governor and others to support a US$300-million bond to fund housing in Guam; US$70,0000 was defrauded from a fund created by the legislature to support local constructors. Other, unsuccessful, bond issues in Saipan and Palau were also involved (*Islands Business,* January 1988, 42).

In these cases the boundary between private and (domestic) public gain is vague. It is possible to imagine a local government official with motives other than personal gain agreeing to a letter of credit proposition, a bond issue that could be considered fraudulent, a sale of passports, or a special issue of exotic postage stamps or commemorative coins. Not all of these

schemes are ridiculous, and some might—and do—benefit the national treasury. Perhaps we need to think about a paradoxical idea of state, rather than individual, corruption, in which a state uses its international sovereignty for domestic revenue.

Governance and Good Government

The World Bank's definition of "governance" includes public sector management, accountability, legal framework, and transparency (1993: vii). Ideas of transparency and the rule of law derive particularly from microeconomic concerns to insure the proper conditions for a freely competitive market. For development to take place, domestic and foreign investors need to know the rules and to be able to rely on their enforcement. The World Bank's "renewed approach" involves economic policy reform, institutional reforms including training for journalists to strengthen scrutiny of public administration, strengthening controls against corruption in its own procedures, and partnerships with other multilateral agencies and nongovernment organizations. Supporters of "good government" are more explicitly concerned with democracy and human rights, but their concerns overlap on issues of accountability and the rule of law.

The link between corruption and public-sector mismanagement is particularly clear in these South Pacific cases. In the PNG and Solomon Islands timber industries, corruption by politicians, officials, and local leaders took place within a much wider context of professional misconduct: lawyers acting for both sides in negotiations, tax evasion by transfer pricing, failures to implement statements of national policy. Barnett found fraud and corruption taking place in Papua New Guinea within a framework of comprehensive mismanagement: an absence of policy, inadequate legislation, lack of information, confusion of responsibilities, and ineffectiveness of government bodies such as the tax office (Barnett 1990: 101–104). The commission investigating the World Bank education project in the Solomon Islands criticized officials for allowing themselves to be "manipulated" by the consultant and for failing to familiarize themselves with, and apply, regulations. The Vanuatu ombudsman found "many—indeed most—of our officials and office-holders have very little idea of 2 things—firstly the realistic demands of the job they have been allocated, and secondly, the moral and ethical standards by which the public is entitled to be served" (Vanuatu 1996b:2)

However, when Western Samoa's chief auditor tried to report on mismanagement as well as corruption, he was suspended.[5] The government then appointed a Commission of Inquiry that confirmed "in the main" the irregularities he identified, but cleared the ministers involved and accused

the chief auditor of exceeding his brief by criticizing government mismanagement (*Pacific Islands Monthly,* November 1995).

The link between "grand" corruption and electoral politics is also strong. The Barnett Commission found PNG politicians seeking funds for their parties or campaigns from foreign timber companies. PNG's Electoral Development Fund was a kind of semiofficial, semicorrupt channel to put political patronage into the hands of MPs in a system where provincial, rather than national, government delivered the most visible services to voters. (A similar Parliamentarians Discretionary Fund has been established—and criticized in the media—in the Solomon Islands; Solomon Islands 1995:4.) Occasionally MPs have been brought before PNG's Leadership Code Tribunal for misuse of these monies, but even if used properly the fund seems to occupy some borderline of the use of public office for private gain and electoral advantage.

Accusations and revelations of corruption typically anticipate, or follow, changes of government. The prodemocracy movement in Tonga gained an early boost by asking where money raised from selling Tongan passports had gone. In the 1987 general election in Fiji, the NFP/Labour coalition accused Alliance Party ministers of corruption—giving those ministers a motive, it has been suggested, for supporting the coup against the coalition when it won. Coup leader Rabuka himself went on to launch an anticorruption drive.

Conclusions

Simply to list examples of corruption, culled from domestic anticorruption agencies, gives little sense of the amount of corruption or its weight. The association of the idea of corruption with the impression of a decline or falling away from an earlier condition may give a false idea of the growth of corruption. How much noncorruption is there?

A recent Solomon Islands commission of inquiry gives pause. Media criticism against its predecessor led the new government elected in 1993 to set up a wide-ranging Commission of Inquiry into Corruption to look at a number of complaints. The commission's report cited a series of newspaper reports criticizing the way decisions had been made to sell the governor-general's residence, award road contracts and work permits, and establish a Parliamentary Discretionary Fund. However, the commission's terms of reference focused on a number of complaints in the media about land allocation; it found evidence of corruption in only one case (the payment of SI$10,000 to secure a land transfer). Yet it found many examples of "impropriety," the absence of proper procedures, and official failure to follow procedures that did exist (Solomon Islands 1995:125–128).

The commission also failed to find any corruption in the aid scheme it

investigated. A provincial assembly member made a series of successful applications for small grants from the Australian High Commission to build classrooms in several parts of the province. In each case he tried to stretch the materials to build more rooms than had been approved, eventually completing none properly. He used funds approved for one project for another and for unapproved projects like repairing a church. But the commission found he had not actually misappropriated money or acted corruptly. It speculated about a political motive: that he had been trying to demonstrate to his constituents that he was doing "a great deal of work throughout his electorate and was not concentrating his efforts in any particular area (ibid.:119).

These rather mild conclusions raise several awkward questions about the role of the media, about commissions of inquiry, and about the incidence of corruption. The Solomon Islands commission just discussed was anxious to dissociate itself from partisan criticism of the previous government. It found less corruption in land administration than had been widely suspected. Did the media get it wrong, or did the commission? Or did the commission's terms of reference, and discretion, turn it away from more embarrassing questions? Were some of the original suspicions of corruption driven (as the "traders' dilemma" would suggest) by traditional values hostile to indigenous business and to partnerships with morally dubious foreigners? And do more-general questions about government secrecy, arbitrariness, lack of accountability, and mismanagement lie behind some of the concern about corruption—as well as providing greater opportunities for the growing number of specific acts of corruption that do take place?

Generally, there may be more corruption in the South Pacific than there used to be: the word itself encourages us to think so. But there may be less than is often claimed, for example by the heads of state quoted at the start of this article or in newspaper editorials. Corruption may be embedded in a wider, but noncorrupt, framework of inept governance (mismanagement, lack of accountability, arbitrary decision making, and so on) that provides the opportunity for specifically corrupt acts and fuels popular suspicion that they may be occurring.

NOTES

Versions of this article were presented to the Pacific Islands Political Studies Association conference in Palau in December 1996, and to the Anthropology Department at the Australian National University, Canberra, in March 1997. I am grateful for the comments made there, and for comments and documents provided by other colleagues who might, in the circumstances, prefer not to be listed.

1. In racially divided New Caledonia, for example, ADRAF, a land and rural development agency set up by the Chirac government in 1986, was found to be buying land at inflated prices and allocating it cheaply to members of its board and their families. The agency also allocated land on ethnic political grounds, favoring applicants with links to the right-wing RPCR, and failed to meet the formal requirement to redistribute land to all ethnic groups in proportion to their population. Thus only 136 of 717 Kanak applications were approved, while Europeans who already owned land, had other jobs, or were not even born in New Caledonia were granted land (*Pacific Islands Monthly,* December 1989, 18). Ron Crocombe describes a conversation at the time in which the high commissioner explained that ADRAF was simply carrying out his instructions to favor Kanaks but also to insure that grants were for economic use of land, rather than simply holding or controlling it (pers. com., 1996).

2. Accumulation of status honor refers to the practice of successful businesspeople "cashing in" their morally dubious wealth in exchange for traditional status, as when a businessman becomes a chief. Cash-and-carry is a pervasive form of indigenous trade in which the absence of profit prevents demands for redistribution. The depersonalization of economic relations is part of the process of development going on throughout the region.

3. Both reports evidence a nostalgia for centralized, bureaucratic forestry departments, which acted to protect landowners from exploitation by foreigners (Barnett 1990:90–91; Solomon Islands 1989:20). At the time, however, and at least in the Solomon Islands, that colonial department had been regarded as too close to foreign multinationals, like Lever's.

4. Economists have the idea of "moral hazard" to describe situations that encourage dishonesty, such as being insured.

5. He had reported that the commissioner for Inland Revenue faced a conflict of interest over his private business activities—some of which he also failed to tax; that a timber company controlled by three ministers made unauthorized use of two government bulldozers for seven months; and that a works director approved payments for false services to another official, who was his brother (*Islands Business,* August 1994, 35).

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THE PLACE OF PERSONS WITH DISABILITIES IN ROTUMAN SOCIETY

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In Rotuma, as in many other small, face-to-face societies, persons with disabilities are treated not as members of categories based on their conditions, but as individuals having whole constellations of characteristics and interpersonal histories. Their roles are not scripted on the basis of disabilities, but patterned by a number of key cultural principles that guide all relationships, including notions of immanent justice for wrongdoing, expectations of generosity and reciprocity, personal autonomy, and concern for family honor. Case studies and analyses of exchange interactions are presented to illustrate these points.

In their paper reviewing the social science literature on physical disabilities, Scheer and Groce, citing Gluckman (1962), suggest that in small-scale societies based on face-to-face contact individuals are related to each other in diffuse social roles and contexts. In such situations, they assert, "a single personal characteristic, such as a physical impairment, does not generalize to define one's total social identity" (Scheer and Groce 1988:31). This situation contrasts with complex societies where "social relationships and contexts are more impersonal and task specific, and individuals are not related to each other in varied contexts. Accordingly, visible characteristics are commonly used to classify and socially notate the individual's identity" (ibid.:31–32).

Rotuma, an island of seventeen square miles and some 2,700 inhabitants, located approximately three hundred miles north of Fiji, conforms to Scheer and Groce's portrayal of a small-scale society. As in most other Pacific Islands communities, personal relationships are based on a combination of kinship,

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co-occupation of villages or hamlets, participation in common activities, and the like. Individuals relate to one another as total persons, based on known personal histories.[1] While physical or mental impairments may influence specific relationships, there are no well-developed roles for disabled persons as such; that is, specific disabilities do not dictate social expectations to any great degree. An impairment or disability is one consideration among many in each of a person's relationships, and much depends on the way in which an individual is situated in a household, community, and network of kin. Thus, no strong socialization pressures are exerted to induce an individual with a disability to conform to specific role expectations for a blind person, a person with a hunched back, a person with a mental disability, and so on. Individual attitudes, abilities, and dispositions play a correspondingly greater part in structuring a person's role within households and communities than do particular impairments or disabilities.

Indeed, disabilities on Rotuma appear to be hypocognized (Levy 1973: 324), as reflected in a comparatively undeveloped lexicon for categorizing impairments (see below). Furthermore, Rotumans do not categorize people according to characteristics like disability. Thus, while someone might mention a disability while referring evaluatively (positively or negatively) to a specific person, we have never heard anyone express attitudes toward *categories* of persons defined by disability. Therefore, instead of seeking to elicit abstract conceptions of disability, we believe it a better strategy to examine the ways in which key cultural principles shape Rotuman thinking about personhood and social relations, and to reflect further on the ways these appear to implicate the treatment of persons with disabilities.

Methods and Theoretical Framework

In this article we present data concerning disability on Rotuma from two periods of fieldwork. Alan Howard conducted research on the island in 1959–1960, then together with Jan Rensel spent a portion of five consecutive years (1987–1991) working there, and returned for a brief visit in 1994. In 1960 Howard conducted a detailed census of the island that included identification of individuals with conditions recognized by Rotumans as disabilities, that is, conditions that, from a Rotuman perspective, interfered with people's ability to function in conformity with their social construction of a fully responsible person. Howard collected case material on several such individuals.

In 1989 Rensel conducted a survey of daily activities of seventeen households in a village on Rotuma (Rensel 1994). Each household recorded labor, meals, gifts, and other items given or received by household members over

the course of thirteen weeks. Households were given cash gifts of five Fijian dollars per week in exchange for their cooperation. The study yielded extensive comparative information on the social interactions of two households headed by persons identified by Rotumans as having disabilities. We have also drawn on less formal observations of interactions involving individuals with disabilities recorded in our field notes.

We wish to make clear at the outset that we did not engage in a study of disability while in the field. Our data therefore are largely incidental; they do not include systematic information on attitudes, self-concepts, and life experiences as these relate to disabilities—an unfortunate omission in any study of this topic. Rather, we have engaged in a historical study, one in which the primary texts are our own field notes. For interpretation of these texts we rely both on our theoretical framework (detailed below) and on our "head notes"—untranscribed understandings we have come to as a result of repeated exposure to Rotuman culture (Sanjek 1990). We hope to demonstrate that such studies are worthwhile, despite their limitations, for what they have to tell us about cultural processes affecting disability.

For analytical purposes we conceive of culture as composed of an array of propositions out of which individuals construct beliefs and attitudes (Goodenough 1963). We purposefully use the term "array," rather than, for example, "system," to underscore the fact that the pool of propositions accessible to individuals inevitably includes inconsistencies and contradictions. This is true not only of complex urban societies, but of small-scale isolated societies as well.[2] Individuals combine and recombine propositions in various ways, depending on context and self-interest, resulting in a variety of perspectives even in the simplest societies. As expressed in discourse about almost any topic, including disability, variable propositions result in an inevitable heteroglossia (Bakhtin 1981, 1986; see also Besnier 1996).

Nevertheless, we can gain insight into the way culture patterns experience by identifying key propositions that have salience for certain conditions. In this spirit we attempt to identify salient Rotuman propositions that have a bearing on conceptions of and responses to disability.

Three levels of proposition may be distinguished.[3] At the level of least complexity are those propositions that underlie conceptual distinctions, that is, that group phenomena as the same or distinguish them as different. Many, if not most, of these propositions are encoded in the lexical and semantic structures of language and can be explored through inquiry into relevant domains. We provide a short account of Rotuman concepts bearing on disability for what it reveals about such basic propositions.

At the second level of complexity, concepts are related to actions, events, thoughts, feelings, and other concepts. The propositions underlying these

perceived relationships are often explicit in statements of association, correlation, causation, and so on, ranging from simple statements such as "x affects y" in some indeterminate way to highly formalized, specific propositions relating multiple variables in precise ways. Folk theories of social action are, in fact, rarely formalized or clearly articulated; they have to be inferred from discourse and observed behavior. Often what is said is indirect, requiring an analysis of underlying presuppositions before reasonable inferences can be made regarding the structure of propositions at this level. The more salient propositions, however, generally reveal themselves through sheer redundancy. From the broad array of culturally salient propositions that both order and provide a basis for interpreting social life on Rotuma, we have selected several we believe to be especially relevant to disabilities for discussion.

At a still greater level of complexity is metatheory—propositions about the formation of propositions and about their acceptability, truthfulness, and the like. Metatheory is an engine of cultural variability and change insofar as it involves the continual assessment of existing propositions, the examination of innovative propositions (whether indigenously generated or imported), and the interpretation of propositions as they apply to new circumstances. We briefly discuss the nature of Rotuman metatheory, particularly with regard to assessments of truthfulness, and examine its implications for disability.

For our current purposes one further distinction—between *person-centered* and *situation-centered* propositions—is required. Person-centered propositions locate causality and/or responsibility inside the person. For example, the proposition "blind people cannot take care of themselves" presumes a quality of personhood irrespective of circumstances, whereas the proposition "it is dangerous for a blind person to cross a busy highway unassisted" is situation specific: it does not preclude the possibility of persons with visual impairments being self-reliant in other circumstances. As we shall see, Rotuman cultural logic concerning disability leans heavily toward situation-centered propositions when contrasted with the more essentialist, person-centered cultural logic characteristic of folk Euro-American cultures.

The view that culture is composed of propositions does not require the ethnographer to reduce it to a list of statements in propositional form. Indeed, to do so would be to trivialize culture and bore the reader. It is the way propositions are employed in action by thinking, feeling people that affords social life its vitality. And just as many propositions are implicit in action or coded in metaphors and metonyms, anthropological accounts premised on this viewpoint can convey appropriate cultural meanings in the form of narrative accounts of behavior or rhetoric, analyses of values and

beliefs, life history analyses, and so on. Whether explicitly or implicitly, culturally grounded propositions form templates for understanding, explaining, and evaluating experience. Our argument in this article is that a certain core set of cultural propositions, particularly those embedded in notions of personhood, are central to the way in which persons with disabilities (culturally defined) are evaluated and treated on the island of Rotuma.

Following a background presentation of Rotuman concepts for disability and data on prevalence, we discuss those principles of Rotuman cultural logic that appear to structure experience of disability on the island. We then furnish data from case studies drawn from Rensel's 1989 study of household exchange and conclude by considering the implications of Rotuman cultural principles for the treatment of persons with disabilities.

Concepts

The Rotuman language has a relatively impoverished vocabulary for talking about disabilities, suggesting they are not a focus for cultural attention. There are no generic terms for "impairment," "disability," or "handicap." The term *pipiki* is used in reference to any distortion or paralysis of limbs. There are four terms for impaired vision—*maf pogi* (night eyes), *maf pala* (blind or nearly so), *maf rahrahu* (dimsighted, purblind), and *tu'u* (cataracts)—but only a single term for deafness *(fuli).* Hunched backs are termed *fomafua to'i* (bent back), and clubfeet *lā fei* (crooked foot). Two words refer to elephantiasis: *pū* is a general term for the condition and usually is used in reference to swelling of the arms and legs; *jua* refers specifically to swelling of the testicles.[4] The vocabulary for mental and behavioral deviations is also restricted. All forms of mental disabilities, from retardation to violent outbursts, are categorized as *filo' raksa'a* (bad head). The terms *jaurarā* (halfwitted; silly, stupid; delirious; Churchward 1940:231) and *koko* (foolish, silly, mad; naughty; ibid.:243) are sometimes used as descriptive adjectives to describe certain individuals or actions, while *huag hapa* (of unbalanced mind, simple, not all there; ibid.:225) refers more directly to a person's mental disability.

Prevalence of Disability on Rotuma

Disabilities in 1960

Howard's survey of disabilities in 1960 identified sixty-one persons with handicaps from a total population of 2,843, yielding a prevalence of twenty-one per thousand. The survey was conducted by two Rotuman research

assistants, who were instructed to identify individuals who "had something wrong with them," that is, who had a condition, considered to be permanent (to distinguish it from illness), that hampered their ability to conform to Rotuman notions of ideal personhood.[5] They were also instructed to inquire whether the conditions were congenital or had been acquired. Conditions were categorized according to type of impairment using English concepts, although they were often described in Rotuman by the assistants. In order of frequency, the following conditions were identified:

visual impairment	18
lame leg	13
mental problems	8
elephantiasis	7
clubfoot	5
unable to hear	4
unable to speak	3
hunchback	3
missing limb or hand	3
speech defect	2
harelip	1

Since some individuals had more than one impairment, the total exceeds the sixty-one persons identified. Visual impairment, mostly as a consequence of cataracts and glaucoma, heads the list. It mainly affected older individuals—fifteen of the eighteen victims were over fifty years of age; in all but one case the condition was acquired, rather than congenital. Somewhat more women were affected than men (F = 10, M = 6, unknown = 2). In eleven instances vision loss was total, in five instances it was partial, and in one instance partial loss had been restored (in one case degree of loss was unspecified). Half of those with visual impairments were listed as either head of household or wife of the household head, suggesting that they maintained an active role in domestic affairs. Six others, without spouses, resided with offspring, as one might expect of elderly individuals on Rotuma.

The second most frequent impairment was a lame leg. It, too, was mainly an acquired condition (nine of thirteen instances), but it occurred among younger individuals (median age = 39.5), as a result of diseases such as polio or accidents. Slightly more men than women were affected (M = 7, F = 5, unknown = 1). Three of the five females and three of the men over twenty years old had never married (one had a hunched back as well), suggesting that this condition may decrease a person's marital prospects.

Of the eight individuals identified as having mental problems, four (one female, three males) had a learning impairment from birth. None of the

adults was married. They all lived in households headed by others, as was the case for those whose mental impairment developed later in life.

Elephantiasis is clearly an acquired condition, affecting men more than women (M = 6, F = 1) and afflicting those over fifty years of age, reflecting the long incubation period of the filarial disease. All who had the disease either were household heads or were married to a household head, indicating their ability to perform such roles successfully.

Four females and one male had clubfeet, a congenital condition. Of the three adult women who were disabled, two had never married and were living as dependents in households headed by others. The three persons with hunched backs and the one woman born with a missing hand were also never married and were all in a dependent position: taken together, these data suggest a definite social disadvantage for individuals with congenital deformities. In contrast, persons with acquired disabilities, including deafness and missing limbs, appear to have the advantage of having established normal life patterns prior to their impairments. Most of them were married or had been married and headed their own households.

Disabilities in 1989

Although we did not collect comparable information and are unable to make precise comparisons,[6] Howard gained the impression of a much higher prevalence of disability on the island in 1989 than in 1960. To the extent this is true, a possible explanation would be that the island has become a haven for Rotumans whose disabilities make it difficult to cope with the demands of an urban environment. In contrast to many other rural settings, Rotuma provides an exceptionally benign environment for persons with disabilities. Food is plentiful and assistance from kin is immediately available. In the years since 1960, the Rotuman population has increased from approximately 4,500 to nearly 10,000, with most now resident in urban centers within Fiji. The population on Rotuma has actually declined in the interim, from approximately 3,000 to about 2,700. It may be that the economic and social costs of caring for individuals with disabilities are much higher in cities, providing an incentive for their remaining on the island or being sent back. In general, Rotuma seems to provide a more supportive environment and presents more opportunities for performing productive tasks than urban areas.

Cultural Propositions and Disability

The most fundamental propositions implicating disability concern Rotuman notions of personhood, since it is against the yardstick of ideal persons that people are judged. Persons with disabilities may be evaluated entirely

benignly if they are able to approximate ideals, while persons without disabilities may be evaluated harshly for deviations. The key issue, then, is to what degree disabilities impede approximations of those ideals.

Personhood

Like their counterparts in other Pacific societies, Rotumans build their sense of personhood primarily (but by no means exclusively) around relationships and contexts. Shore (1982), for example, likens Samoan personhood to a many-faceted gem insofar as persons manifest different sides depending on context. He contrasts this with notions of a unitary, internally consistent self that is the ideal in Euro-American society. Personal differences in Samoa are attributed to types of situations, Shore argues, not to discrete character traits. Morality in Samoa is likewise defined situationally more often than as a matter of personal attribution, and evaluations of behavior are more likely to be ephemeral, related to specific events, than indicative of enduring character. This suggests that Samoans are much less likely than Euro-Americans to associate undesirable personal characteristics, including disabilities, with personal character. As long as we do not overdraw these distinctions, the same could be said for other Polynesians, including Rotumans. (Polynesians certainly do have a sense of themselves as having a "center" and Americans think of themselves and others in terms of relationships and contexts at times, but degrees of emphasis are quite apparent to anyone familiar with the contrasting cultures.)

Despite this emphasis on context rather than on enduring traits, if an individual repeatedly fails to perform in ways expected of a "normal" person of his or her age and gender, expectations are modified accordingly, and he or she is likely to become the object of banter, gossip, and, on occasion, pointed criticism, regardless of reasons for failing to comply with cultural norms. Thus, although judgments focus on behavior, their cumulative effect may nevertheless relegate an individual to a stigmatized position in Rotuman society.

Two aspects of ideal personhood are especially relevant to the treatment of persons with disabilities: notions of the body and social expectations concerning work and reciprocity. The Rotuman word *haharaga* (in good physical condition; stout, plump, youthful) perhaps best describes the ideal state of the human body.[7] It also implies a body that is complete and unblemished. The state of a person's *ata* (life force, soul) is said to be reflected in his or her bodily appearance. Persons who are disfigured from birth or through accident are disadvantaged because they are perceived as deviating from ideal physical form. Certain disabilities—such as a harelip or a clubfoot—may render individuals socially vulnerable, targets for teasing and jest.

More significant, however, are the values Rotumans place on work and reciprocity. Contrary to popular stereotypes of Polynesian laziness, Rotumans regard work as central to assessments of personal worth. During the colonial period (1881–1970) work revolved around the production and preparation of food. Men's work primarily involved planting and tending gardens of taro, yams, bananas, and other crops; in addition, they cut and dried coconut meat for exportation as copra. Women's work centered on weaving mats and keeping the home and its surroundings well groomed. Both men and women fished, tended domestic animals, prepared and cooked food. Nearly equal in importance to domestic work was communal effort—work on behalf of the church or the community. This work generally required efforts similar to those within households, since feasting is a central part of most communal activities. A prestige economy based on the production and distribution of surplus food and of produce of extraordinary size also flourished (Howard 1970:102–103). Today hard work is still valued and remains central to an individual's reputation, although additional opportunities for acquiring resources, through remittances from relatives abroad for instance, now provide alternative paths to social merit.

From a social standpoint, the importance of work is that it allows individuals to engage appropriately in reciprocal exchange. Proper relationships, involving persons of equal status, are expected to balance over time. Exchanges may involve labor, food, political support, money, or other valuables in various combinations, but to be considered a fully competent person requires giving as much as one receives. Giving less than one receives diminishes one's status proportionately vis-à-vis those who give more. To elevate or maintain one's status in the community therefore requires access to resources and/or the capacity to contribute labor to projects (see below regarding 1989 exchange behavior).

The social circumstances of a person with a disability largely depend on how the impairment affects his or her ability to produce food, earn money, and work in productive ways. A clubfoot may prove only a minor handicap, if the individual can maintain a plantation, fish, make an earth oven, or weave a mat, for example. In contrast, disabilities that preclude productivity place individuals at a serious disadvantage and jeopardize their standing as fully competent persons. As much depends on the individual's social circumstances as on the particular disability. Thus a woman who has lost the use of her legs, living in a household with her husband and mature children, may occupy her time weaving fine mats—a highly valued activity—and thus make an important contribution to her family's standing in the community. Another woman with the same physical condition, living in a household without such support, is in a much more vulnerable position if she must occupy most of her time with subsistence tasks.

Another aspect of personhood that impacts judgments of social competence is the management of anger. Like people everywhere, Rotumans expect properly socialized individuals to conform to the rules of decorum appropriate to their age and sex. Of primary importance is the ability to identify persons and situations that require different degrees of restraint and license. With persons, proper socialization means knowing relative kinship status of persons who are related and any special social status that calls for deference or entitles the individual to express intimacy, joke, admonish, and so on. With persons to whom one owes deference, a whole repertoire of behavioral prescriptions apply, including the following examples: (1) one should not talk much in the person's presence and should allow him or her to initiate and control interactions; (2) if eating with the person, one should not commence before he or she does and one should stop eating when he or she does; and (3) when sitting on a mat, proper positions should be assumed (these differ for men and women). In certain formal situations (e.g., community meetings, funerals) the list of rules prescribing behavior is considerably expanded, while in other circumstances (e.g., picnics, informal gatherings) they are relaxed. Persistent violation of social rules arouses suspicion regarding an individual's mental capacity. Some who repeatedly violate norms are recognized as retarded, while others whose alertness is acknowledged but who are seen as prone to frequent, inappropriate outbursts of anger may also be regarded as disabled. We have never known Rotumans to admit to anger, even under circumstances of great provocation and frustration. When asked how they feel, they are likely to use terms that can be translated as "disappointed" or "sad." Our interpretation is that emotions like jealousy, envy, and anger are considered chaotic internal states that would threaten the social order if expressed. This hypocognizing of anger is premised on the cultural proposition that anger results in a loss of self-control, in behavior that will generate pervasive conflict. Wariness of a person who freely expresses anger may therefore lead to his or her exclusion from various activities. (See Levy 1969 and 1973:273–288 for an extensive discussion of the relationship between anger and personhood in Tahiti.)

A further consideration involves a set of metapropositions that implicate judgments of mental soundness. Rotumans evaluate the veracity of statements by relating them to an assessment of the speaker's *mana,* or supernaturally derived potency. Let us say that in the course of a conversation a person says he was told something will happen—for example, that the supply boat will come on such-and-such a date, or that A will marry B. If events unfold in the way stated, the speaker's potency as a social being is affirmed and his credibility enhanced; if they do not, his potency is rendered dubious and his credibility diminished. Whether or not the speaker has

control over the outcome or is reporting information secondhand is not at issue; the statement itself puts his *mana* at risk. Thus individuals who are predisposed to making or reporting problematic predictions, or who often make statements that can be readily falsified, run the risk of losing esteem. To the extent that persons with mental disabilities repeatedly fall into this trap, their social standing is diminished and they become targets for teasing and ridicule.

Immanent Justice

Rotuman explanations for ill fortune often presuppose it to be a punishment for moral transgressions. Individuals commonly dredge up past events or conflicts to account for deaths, serious accidents or illnesses, political and financial setbacks, and so on (Howard 1979b, 1990). In earlier times ancestral or free-roaming spirits were specifically invoked as causal agents of such misfortune, but today agency is usually omitted in explanations (Howard 1996). Thus people attribute the deaths of several young mothers in a new maternity clinic to a failure to perform proper dedication ceremonies when the clinic was built, without directly implicating spirits in their explanation. While short-term illnesses and minor accidents may be accounted for in naturalistic terms, persistent conditions and permanent impairments lend themselves to explanations based on immanent justice. For example, the condition of a woman with a hunchback was attributed to her misappropriation of turtle meat in her youth; her spine was likened in shape to a turtle's shell. Immanent justice is sometimes extended to the entire family and/or descendants of a transgressor, especially if land or chiefly titles are involved.

Belief in immanent justice can affect persons with disabilities in two important ways. On the one hand, it grants license for others to place a person with a disability in a morally inferior position and thus justifies pariah treatment. Such attribution is by no means automatic, however; much depends on the history of previous relationships and current dispositions. Thus some people may resort to claims of immanent justice when it suits them, while others may ignore it altogether. On the other hand, mistreatment of disadvantaged persons is itself a moral transgression and can lead to affliction of unkind individuals. Using this reasoning, people sometimes explain an acquired affliction as a consequence of mistreating someone who had that same condition. Thus, in the early part of this century, medical officers had a difficult time trying to quarantine lepers; Rotumans refused to cease interacting with them, claiming that to do so would bring the disease on themselves. Two case studies from 1960 provide further illustrations:

Jenny was an old woman of around seventy years. She wandered from village to village, staying with whomever she liked, whether or not they were related to her. Despite her bizarre behavior, people said she was not really crazy *(jaurarā),* since she could hold a reasonable conversation if she wanted to. It was mostly in response to the teasing provocations of young people that she would begin to act silly, dancing and singing songs, inciting her audience to laugh at her. She also swore freely, especially to the young boys and girls, and spoke unintelligible words and phrases.

When she came to a house, she might stay for a few days or for several weeks, doing as she pleased and, before leaving, taking whatever she wanted. The owners of the house usually did not object. They said that if they chased her out or complained, others would think them greedy or inhospitable.

One day Jenny came to a house in Itumuta. While there she dug a hole in the middle of the cooking-house and defecated into it. She left it exposed and refused to bury it. Furthermore, she guarded her place and refused to let anyone else bury it, so it remained there until she left. The prevailing explanation for Jenny's behavior was that when she was young, she ill-treated her mother-in-law. In particular, people said that Jenny used to laugh at her mother-in-law when [the latter] was old and afflicted with illness. They said that now that Jenny was old she was being punished for the way she treated her mother-in-law, since everyone now laughed at her. (Howard, field notes, 1960)

Fred [a man in his late twenties] does not do any constructive work and usually just hangs around with his kinsmen. His speech is usually incoherent. He does not get violent and is considered "mad" *(jaurarā)* but harmless. Several years ago Chief Tokaniua's house was burned and he called a meeting to find out who had caused it. No one confessed, so the chief used the *pūʻaki,* curse of immanent justice. The people say that Fred was the guilty one, but he was too afraid to confess. Instead he tried to kill himself by drinking *fuha,* a poison made from the root of a plant [*Derris* spp.]. Arthur [my informant] said that one cup is enough to kill a person, but Fred didn't finish drinking the cup. Instead he got very sick and was taken to the hospital, where the doctor saved him, but since that time he has been in his present condition. I asked Arthur if Fred started the fire at the chief's house because he was angry or if it was an accident. Arthur was unsure, but he said he thought it was inten-

> tional. (Actually I don't know if there is any real evidence that Fred started the fire, but I believe Arthur's assumption of Fred's guilt is based on the fact that the curse presumably worked. If he was not guilty, it would not have.)
>
> Fred stays mostly to himself, although he could not really be described as retiring. The boys don't pay much attention to him because his speech is too garbled. The girls often tease him. Arthur says that when they tease him, he acts drunk; that is, he staggers about and tries to talk but cannot say anything coherent. He goes from household to household among his kinsmen, and everyone accepts him without fuss and feeds him for the time he stays with them. They just let him do as he pleases and excuse him if he does anything silly, because "his head is no good." (Howard, field notes, 1960)

The willingness of households to accommodate both of the above individuals is attributable to the importance placed on generosity in Rotuman culture as much as to fear of supernatural reprisal. Persons who are well off are expected to give amiably to those who are less fortunate. In return they are praised for their kindness, or at least are able to avoid criticism for being stingy and hard-hearted. There are limits, however, which may account for the tendency of the persons described above to move frequently.

Personal Autonomy

A principle of autonomy operates throughout Rotuman society (Howard 1990). Not only do individuals exercise autonomy within their households and communities, but villages are autonomous vis-à-vis one another, and each of the island's seven districts operates as an independent political unit. Even children are granted an astonishing degree of autonomy by Western standards. Respect for others' willfulness means that persons with disabilities are given a strong negotiating voice in determining their social existence. They are generally permitted to participate in whatever activities they feel up to and are not systematically discouraged from involvement. As Marshall observes for Namoluk Atoll in the Caroline Islands, so long as persons with physical disabilities "continue to be actively involved psychologically and morally in relationships with other community members, they are not truly disabled" (1996:259). Alternatively, if a person with a disability (or anyone, for that matter) decides not to participate in communal activities, little effort is made to reintegrate that person until he or she gives clear signs of a willingness to return to the fold. In some respects a low level of participation may

have its advantages, especially if the person is victimized by incessant teasing or banter, although it means forfeiting the many benefits of active involvement. As on Namoluk, however, completely isolating oneself from the community is regarded as the severest form of disability (Marshall 1996; see also Alkire 1982; Howard 1979a).

Family Honor

Some families perceive certain types of disability, especially severe retardation, as shameful and do their best to keep a person with such a disability out of public view. The individual might be given menial tasks within the household but left behind when communal activities take place. Family members may discourage him or her from interacting with anyone outside the household. In the village of Oinafa, where we made our residence during the 1987–1990 field sessions, a young woman we shall call Flora was so treated (see Case 8, below). One day, Howard was walking on the road between villages and Flora was alone, walking slightly behind. She purposefully caught up and started a conversation, talking partly in Rotuman, partly in English. She was animated and engaging, full of quite reasonable questions. As we neared Flora's house, however, she moved away and assumed a vacant, if not sullen, expression. On subsequent occasions, Howard tried to engage her attention, but if other family members were present, Flora kept her head bowed and refused to acknowledge him.

We were told of another family's having sent their retarded son (Case 5, below) away to be brought up on the other side of the island by a distant kinswoman who had leprosy. That son, now thirty-four years old and unmarried, returned to live in a one-room building next to his parents and assists them but is marginal to the activities of the village men.

Teasing

Joking is central to most Pacific Islands cultures; it ranges from light-hearted interpersonal banter to ritual clowning with serious political implications (Hereniko 1995; Mitchell 1992). As several ethnographers have pointed out, persons with disabilities are often singled out as victims of humorous barbs (Barker 1997; Hereniko 1995; Marshall 1983, 1996; Martini and Kirkpatrick 1981; Peter 1992). Laughter is a common response to embarrassment and anxiety, whether it is generated by a near accident, an actual mishap, or the awkwardness of interacting with someone who is noticeably different. Since there are few restrictions on teasing those of equal or lower status, laughing at individuals with disabilities is rarely admonished.

On Rotuma, although awkward movements may be mimicked at times, teasing is more intense for behavioral deviation than for physical disabilities. Much of it is light-hearted and integrative, however, actually drawing otherwise marginal individuals into social activities through verbal play (see Peter 1992 for a particularly poignant, personal account). Again drawing on Howard's 1960 field notes:

> Ted is regarded as learning-impaired by people in the village. One indication is that he cannot tell when he is being teased. If the boys tell him complimentary things in an ironic way, he will not know. The girls also like to tease him about his being their boyfriend, but he doesn't seem to realize they are joking. He works diligently, however, and even is the main provider for his family. He appears calm and happy, and actually seems to enjoy the ironic buildup people give him in jest. Arthur says he is not known to get angry or to show any aggressive tendencies.

> Frank is an unmarried twenty-six-year-old man who is regarded as lacking normal intelligence. He fulfills the role of a normal young man in most ways and is a competent worker. His interpersonal relationships with the other young men are relatively normal, but the girls like to tease him. They tease him indirectly, by getting him to do silly things, then laughing at him. For example, at one of the dances, the girls made him think he was in charge of the dance and encouraged him to the point where he began giving orders as to who was to dance with whom. The boys and girls all complied with his orders, while laughing at him to one another. Arthur says Frank never knows when the people are being serious or fooling him and that he believes whatever they tell him. The girls often pretend that he is their boyfriend in the same way.

An incident in 1988 illustrates how teasing can be used for social control. We had just met Robert (Case 2, below), who lived next door to the house where we were staying. Robert sat with us on the verandah for a long time, telling us about his former exploits, which we found increasingly hard to believe. He had been the captain of a ship that sailed to Australia, he told us, and he had traveled far and wide. Later, we mentioned these stories to our host, Terence. He laughed and said they weren't true. The next day we accompanied Terence to the local cooperative shop. Robert was there, sitting along the wall with some of the other men. Terence pro-

ceeded, in a somewhat mocking manner, to tell everyone there about the stories Robert had told us. Everyone laughed, and Robert hung his head, smiling sheepishly.

Cultural Principles in Action: Case Studies

Reciprocal exchange and sharing of resources is central to social life on Rotuma. On formal occasions people demonstrate kinship ties and loyalty with special gifts of food, mats, money, and assistance. But Rotumans also frequently give informal food gifts, share meals, or help other households on a reciprocal basis, especially within the same village. People are concerned to influence how others think of their family by their generosity in giving, sharing, and helping. They also seek to maintain or strengthen bonds with other households for mutual social, political, and economic support.

Even if it were desirable, households could not maintain equally strong exchange relations with every other household in a village. Household size, the capabilities of its members, and availability of different resources constrain the number and types of exchange in which a household can participate. Closeness of kinship ties, proximity, fluctuating economic circumstances and political alliances, and interpersonal histories are some of the factors influencing how pairs of households interact. The effect of physical or mental disabilities on social interactions must be seen within this wider picture.

Two of the households in Rensel's 1989 thirteen-week survey of daily activities consisted of single adult males living by themselves; in both cases the men were regarded as having disabilities. In this section we provide some background on each man's situation and describe his exchange interactions with other households. We then compare their interaction patterns with those of other households in the study.

Case 1

Condition. Francis, age sixty-four, has crippled legs. He walks with difficulty, with an exaggerated rolling gait. He gets around better on a bicycle, when it is in good repair. Francis is also able to hitch rides on trucks or on the school bus. His upper body is strong, although his hand shakes when he tries to write.

Explanation of Condition. According to the Rotuman medical officer, Francis had polio as a youth. We are unaware of any folk explanations for his condition.

Kinship. Francis has no close family in the village and is said to come from "the other side of the island." However, he was adopted by a second cousin, who has since emigrated to Australia, and is *sigoa* (namesake) of the husband of another second cousin, now deceased. On the strength of these ties he was brought to the village at age fourteen to stay with the extended family, and allowed to live in a neighboring house on family land from the time he was forty-seven.

Education and Experience. Francis attended school through Class 4, which is common for his age group, and has traveled to Australia three times to visit the cousin who adopted him, at her expense. On the third trip he also went to New Zealand with a group from Rotuma who performed dances for a Rotuman gathering there. He speaks English quite well and is intelligent and witty. He has never married.

Activities. Francis is able to garden, although during the survey period he went to work in his garden only eight times, less often than any other adult male. He did not cut copra during the survey, although he does from time to time when he needs cash. Otherwise he receives money from his cousin in Australia. He is fairly skilled at carpentry and other building skills. He attends church services and gatherings regularly, both in the village and elsewhere on the island, and when the village participates in a dance performance, he joins the elders who keep rhythm by singing and beating on a pile of mats.

Exchange Interactions. Since he did not garden very much during the survey, Francis had no produce to share with others. His primary resource was his ability to work. Despite problems with mobility, Francis assisted three other households and the church with construction and repair projects during the survey. One of the households paid him (they were also paying four other workers from outside the village). Francis helped other households prepare *koua* (food cooked in an earth oven) several times, cut grass, cut firewood, and plaited coconut-leaf baskets in which to send food to Suva. Other households thanked him primarily by giving him meals, although he was also invited to eat with some households even when he was not helping them. One woman sewed a *sulu* (wraparound garment) for him. Because he assisted with the grave preparation, a household that hosted a funeral gave Francis a mat. He attended two eightieth birthday parties and gave one of the birthday honorees a gift of five dollars (this may have been the five-dollar gift Rensel had given him for assisting with the activities survey that week).

Francis loaned his bicycle to one man eleven times, gratis. (There were a

total of only fourteen instances of bicycles being loaned by anyone during the entire survey.) Interestingly, this happened after an incident in which Francis had become upset with the other man's son and another boy who were fooling around with his bicycle. Francis struck the children. The first boy's family was furious with him for doing that and avoided him for several days afterwards. It is possible Francis loaned the bicycle in an attempt to make amends for having hit the children.

Intensity of Interactions. During the survey period Francis had no interactions with six other households, a few (one to three) interactions with five other households, and several (sixteen to thirty-four) interactions with five households. His most intensive interactions were with the households of his namesake's daughter and her husband, and of her husband's brother; the household of the catechist (vis-à-vis church activities); the household of the family who hosted the funeral (which was the main focus of their interactions); and the household that paid him to help them with a construction project.

Case 2

Condition. Robert, a fifty-six-year-old man, appears normal, but when trying to engage him in extended conversation, one quickly realizes he is "not right in the head" *(filo' raksa'a).* He tells (in English or Rotuman) of extraordinary experiences he claims to have had, such as having been captain of a ship and going on extensive travels. Some people mock or tease him about this or gently coax him to talk more normally; others avoid speaking to him at all.

We also were told of previous incidents in which he violently attacked his father or his twin brother (both now deceased) with an axe, a crowbar, and then a gun. He also reportedly dreamed that his brother's son was trying to kill him and scuffled with him. People in the village do not consider Robert to be dangerous except perhaps to members of his own family, although some women seemed reluctant to interact with him.

Explanation of Condition. One person, whose family has a history of enmity with Robert's, told us that Robert was this way because Robert's family had asserted that they were of a chiefly line and should have a turn at selecting the district chief; our informant said this claim was nonsense. The family was uncooperative with the chief and thereafter had bad luck, including Robert's condition. The Rotuman medical officer suggested that he had a form of epilepsy that could be controlled by medication.

Kinship. Although he lives on family land, Robert has no close family in the village, and Rensel was unable to get anyone to admit he or she was related to him. He was the eldest of eleven children. All his siblings save one have emigrated from the island. Robert stays in one family house and looks after a second one, which was otherwise empty during the survey period. His siblings contribute to a fund for his support, generally sending food and other goods rather than money. The remaining brother, a catechist, lived on the other side of the island during the survey period, though he and his wife visited and brought Robert produce periodically, and have since moved back to the family home.

Education and Experience. Robert attended school through Class 6. In his twenties he sailed for six or seven years on a copra boat, then returned to the island to stay. He speaks English quite well. He was married and divorced.

Activities. Robert primarily keeps to himself, although others do not actively exclude him when he chooses to participate in community activities. He attends church services only rarely. When there is a feast or other village work, he will sometimes assist the men (see below regarding exchange).

During the survey period Robert looked after about thirty chickens, two cats, and a dog. He gardened on twelve days. On seventeen days he cut copra and used the money to buy cigarettes. Most mornings Robert walks over to the cooperative store to buy two packs of cigarettes. Sometimes he sits quietly with the other men who gather there to talk. Morning and evening, he can be seen walking slowly through the village, between the two houses he looks after, smoking a cigarette.

Exchange Interactions. Robert was on the receiving end for a few food gifts and two rides but gave no gifts other than labor during the survey period. However, in previous years we noted Robert's sharing with an adjacent household some of the food and supplies that his siblings sent him from Fiji (eggs, onions, toilet paper). That household often gave him Sunday lunches, although they did so only once during the survey.

Robert is physically capable of performing most adult male activities in the village and sometimes joins in. When a group of men were tearing down an abandoned building, he assisted. When a family was preparing a feast for an eightieth birthday party, Robert joined in the work. He helped a group of men repairing and painting the church on three consecutive days. Each time Robert, along with the other men, was thanked with meals or refreshments.

Before he died, Robert's brother's wife's father had been exceptionally close to Robert. When Robert would start to talk or act strangely, the old man would come and speak gently to him, bringing him around to normal behavior. In addition, a European son-in-law of the old man was kind to Robert when he visited Rotuma, recognizing Robert's ability to work and involving him in helping the family with a house extension project. Other villagers teased the European man about his helpers, who also included Francis (Case 1) and a man with hearing loss from another village, but the European defended his choices.

When the old man died, Robert helped that household with food preparations for five days and ate with the household during the entire period. At the last dinner on the fifth day, Robert gave a speech, saying how wonderful it was to work together and how he wished it could be like this all the time. Those present were struck silent when he began to talk and made approving noises when he finished. Although other adults frequently give speeches of appreciation at gatherings, we have never heard Robert speak out in public before or since.

Intensity of Interactions. Robert had no interactions with seven households, one to six interactions with eight households, and nineteen interactions with one household. The household with whom Robert had the most intensive interactions was that of his brother's wife's father and sister.

Comparison of Interaction Patterns

Intensity of Interactions with Other Households

The average number of interactions between households was fifteen, but the median was four interactions (see Table 1 below). Clearly some household pairs interacted much more intensively than others. In contrast, both Francis and Robert had fewer interactions with other households than the average. These men differ markedly from the other households in the number of other households with which they had no exchange interactions

TABLE 1. **Number of Interactions between Household Pairs**

	Range	Mean	Median
Seventeen households	0–124	15	4
Francis	0–34	9	2
Robert	0–19	3	2

TABLE 2. **Number of Other Households Interacted With**

Household	Number of Interactions					
	0	1–10	11–20	21–30	31–40	41+
A	2	7	3	3	0	1
B	2	10	2	0	0	2
C	5	9	2	0	0	0
D	0	2	7	3	3	1
E	1	7	3	3	3	1
F*	**6**	**5**	**1**	**2**	**2**	**0**
G	0	12	1	0	2	1
H**	**7**	**8**	**1**	**0**	**0**	**0**
I	0	8	6	0	2	0
J	2	7	4	2	0	1
K	0	4	1	3	3	5
L	1	5	4	5	0	1
M	3	10	2	1	0	0
N	1	7	2	3	1	2
O	1	7	5	1	0	2
P	1	8	2	1	2	2
Q	2	9	3	1	1	0
Mean	1.9	7.5	2.8	1.6	1.1	1.1
Mean excluding F and H	1.4	7.5	3.1	1.7	1.1	1.3

*F = Francis's household; **H = Robert's household.

during the study period (see Table 2, Figure 1). Only one other household (C) had nearly as many households with which they did not interact (five); they were a retired minister and his wife who had just moved to the village temporarily and had only distant relations there. The household of the one Jehovah's Witness family (M) in this village of Methodists interacted with all but three households, despite the limits placed on interaction by their religious differences. (These two households illustrate the influence of social deviation on exchange relations.) All other households maintained some level of exchange interaction with all but one or two households.

Most households in the study maintained a low level of interaction (one to ten) with several other households (average 7.5) while emphasizing exchange relations with a few. The average household had more than twenty interactions with about four other households. Francis followed this pattern, aside from the high number of households with which he did not interact. Robert, however, managed to interact moderately often (nineteen times) with only one other household and intensively with none.

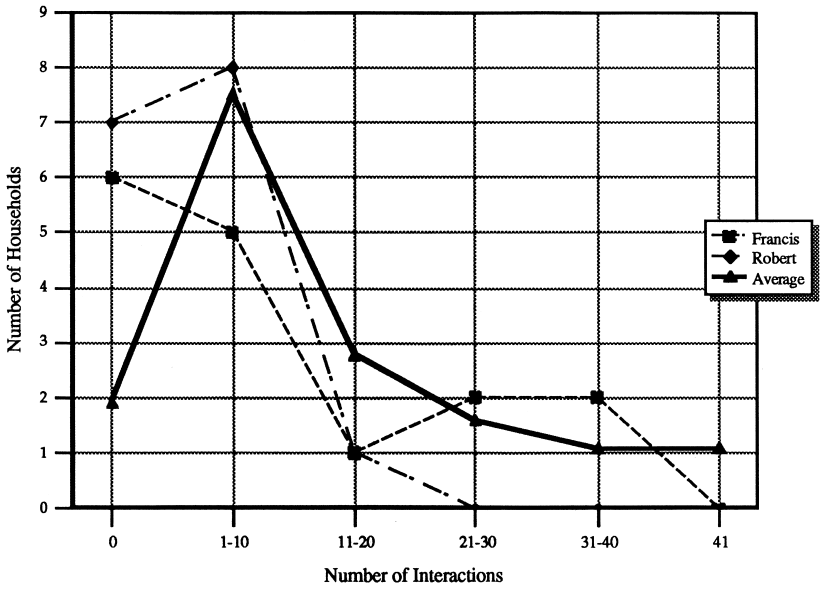

FIGURE 1. **Household interactions: Francis and Robert compared to average.**

Types of Interaction

The primary forms of reciprocal exchange in the study area were (1) food gifts, (2) assistance, and (3) shared meals. The two men with disabilities have different patterns from the average for each of these forms (see Table 3, Figure 2 below).

1. A total of 793 food gifts were recorded in Rensel's survey. For seventeen households an average of forty-seven food gifts were given or received. Francis and Robert each received only eight food gifts and gave none.
2. The households recorded 783 person-days of assistance given to or received from other households in the study area. The average given or received per household was forty-six person-days of assistance. Francis helped other households on forty-two days, but he himself was helped only one day. Robert helped other households fourteen days, and he was helped on four occasions—twice when his brother's wife's family assisted in preparing meals at his house, once when a

TABLE 3. **Numbers of Exchange Interactions by Type**

	Food Gifts		Person-Days of Assistance		Shared Meals	
Household	Given	Rec'd	Given	Rec'd	Given	Rec'd
A	50	48	63	40	18	29
B	40	22	58	19	38	48
C	16	19	25	22	2	3
D	127	59	63	101	83	34
E	48	50	67	39	51	45
F	**0**	**8**	**42**	**1**	**0**	**66**
G	20	47	71	30	12	63
H	**0**	**8**	**14**	**4**	**4**	**21**
I	49	43	16	25	22	28
J	36	50	47	50	42	36
K	55	110	113	127	76	91
L	44	41	45	115	67	20
M	10	18	41	20	2	19
N	22	105	9	102	75	8
O	109	62	58	53	32	25
P	136	59	38	27	32	33
Q	30	44	13	8	30	17
Total	793	793	783	783	586	586
Mean	46.6	46.6	46	46	34.5	34.5
Median	40	47	45	30	32	29

man gave him some melon seeds to plant, and once when a neighbor brought Robert's cargo from the wharf in his truck.

3. The households collectively recorded 586 days when meals were shared with other households. For each of the seventeen households, there were an average of thirty-four days (out of the ninety-one days of the survey) when they shared meals with others, either hosting or being hosted by someone else. Francis ate meals at another household on sixty-six days. This figure is nearly twice the average and represents more than two-thirds of the total survey period. He did not host anyone for meals. Robert ate meals at another household on twenty-one days, and on four days someone from another household ate at his house; on these occasions, his brother and the brother's wife were visiting and actually provided and prepared the meals.

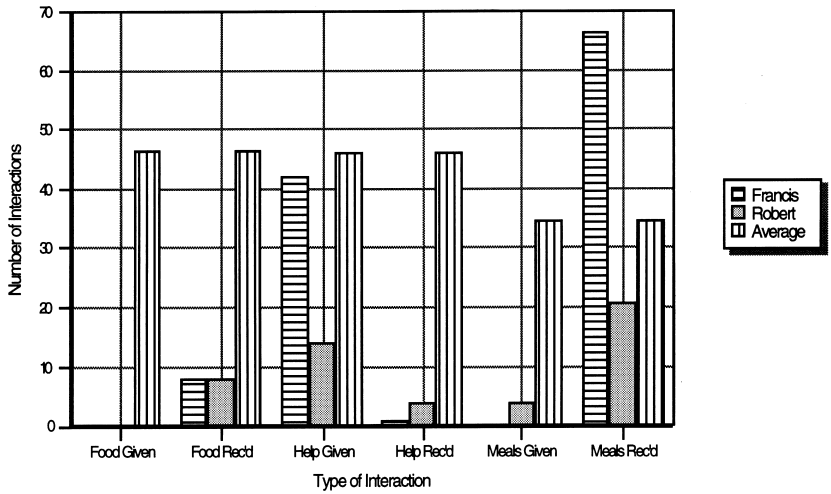

FIGURE 2. **Exchange interactions by type.**

Discussion

Both men interacted with fewer households than the average during the survey period. A number of factors may contribute to this pattern. For instance, age and gender may play a role, insofar as they tend to define appropriate venues and partners for interaction. By the time they reach late adulthood, Rotuman men usually have established networks through decades of involvement with others and continue to strengthen relationships through participation in particularly male activities, such as going inland during the day to garden or cut copra and staying up all night to prepare feasts. Both Robert and Francis were restricted with regard to the extent to which they took part in such activities.

But household size is especially salient in determining levels of interhousehold exchange. In Rensel's 1989 study, larger households generally maintained larger exchange networks, with each household member contributing to the cumulative total of interactions (see Rensel 1994 for a more detailed discussion of interhousehold exchange patterns). Single-person households necessarily face practical constraints on the number of exchange relations they can maintain. Being single, Francis and Robert faced limits on the forms and scope of their interaction with others.

Each man maintained low levels of interaction with several other households, in keeping with the overall village pattern. But whereas Francis, with

his physical impairment, emphasized more intensive interactions with a few other households, Robert, with his mental problem, interacted only moderately often with one other household.

Neither man gave food gifts, and both received few. Francis emphasized assisting other households and was hosted for meals in return. Robert also helped other households but only a third as often; still he was hosted for a moderate number of meals.

The patterns of interaction demonstrated by the survey data are illustrative of general observations we have made during our extended field research on Rotuma. They suggest the following cultural patterns:

1. Individuals with mental impairments find it more difficult to maintain exchange relations with other households than persons with physical disabilities.
2. Persons with disabilities have greater difficulty maintaining exchange relations with more than a few neighboring households.
3. Others are less likely to initiate or maintain exchange relations with households headed by persons with disabilities.
4. Possibilities for exchange are affected by disabilities that limit individuals' productive capabilities. For instance, neither Francis nor Robert gardened very often during the survey, so they had little produce to share with others.

Since exchange relations with other households both constitute an important economic strategy and reinforce social relationships, persons with disabilities who live alone are economically and socially disadvantaged to the extent that their participation in such activities is limited by their disabilities.

Additional Cases

Among the other households participating in the 1989 study were additional people with disabilities, but since they belonged to larger households, their individual contributions to exchange interactions were not tallied separately. However, our observations concerning their activities and the way they were treated by the rest of the village suggest that disabilities are just one factor among many shaping interactions with the rest of the community.

Case 3

John, age twenty-two and unmarried, is missing a finger and has trouble with the muscles of one eye, which flutters. He also suffers from neck trouble,

for which he periodically gets a massage from a neighbor. He lives with his parents, grandfather, and five siblings, of which he is the eldest. John completed Form IV (tenth grade) but was unable to get employment in Fiji. He earns money from occasional labor when opportunities present themselves. John avidly enjoys athletics and despite his slight build played on the island's rugby team in 1989 at the national games. He occasionally attends church and activities organized by the Methodist Youth Fellowship. Whenever young men are needed to help prepare a feast in the village or when they decide to go drinking, John is there. However, when his father needs help, he is likely to be elsewhere. Although his parents consider him unreliable and foolish, most people find John friendly, helpful, and likable. He gets away with defying social convention. For example, the greeting "Come and eat!" when someone passes by one's house is called out as a courtesy (much like "How are you?" in American culture), rather than as a serious invitation. People have come to realize that if it is called out to John, however, he will come and eat! Most just laugh and make him welcome.

Case 4

John's grandfather, Richard, is eighty years old but still active, despite his hearing loss, toothlessness, and periodic bouts of filariasis. Except when he is ill, Richard makes daily trips to the inland gardens, bringing back bananas, yams, or cassava. He frequently climbs the tree next to the house to cut breadfruit. He washes his own clothes and scrubs the pots and pans for the household at an outside standpipe. Richard regularly attends church, although he doesn't talk to anyone. He joins in some social gatherings and sits with other old people, watching what's going on, but he refuses to eat in front of others because he is ashamed of his toothlessness. When guests come to the house, he takes his food outside and eats alone. The house belongs to his deceased wife's family, and he continues to live there only with his son's permission. His daughter-in-law resents his presence, occasionally yelling at him and threatening to kick him out. Although Richard can't hear her, he is aware of her attitude. His eagerness to make daily trips to the garden can be interpreted as an attempt both to contribute materially to the household and to stay out of her way.

Case 5

Samuel is thirty-four and unmarried. He attended school until Class 3 but dropped out because of a learning impairment. The youngest of seven children, Samuel was sent to live with distant kin on the other side of the island when he was small. He has since returned and lives in a small, one-room

building next to his parents' house. He assists with household chores and runs errands for his parents. Samuel is marginal to the young men's activities: that is, he occasionally helps with feast preparations but otherwise his socializing is limited. He attends church services and Methodist Youth Fellowship activities, where he is treated kindly.

Case 6

Edward, age nineteen and unmarried, has a clubfoot. He finished Form IV (tenth grade) and worked for a few years on his father's brother's cattle ranch on the island of Viti Levu in Fiji. After returning to Rotuma, he went to work driving his father's truck to make money by transporting groups to special events. He joins the activities of the other young unmarried men, who have accepted him, and at least one young woman in the village has declared him handsome, implying he would be a good catch.

Case 7

George, age fifty-six, has a bad knee. He walks by swinging one leg around rather than bending it at the knee. George is married and has two children, who live in Fiji. He cuts copra and works for the cooperative association driving a truck for a fortnightly salary. In 1989 he was in the process of building a small house for himself and his wife away from the village, but at the time of the survey he was staying with his wife's mother, brother, and extended family, totaling thirteen people in two dwellings. Along with his wife and brother-in-law, George participates in all the community activities.

Case 8

George's daughter Flora, age twenty-three and unmarried, has never gone to school. Her family explains that she is "sick." She does not participate in community activities and seldom leaves the home compound, where she does some simple chores or just sits, watching others. Occasionally she accompanies her father down the road to cut copra. Her family strictly controls her movements and limits her interactions with others. (Flora is the person who struck up a conversation with Howard, reported above, but refuses to engage when in sight of others.)

Case 9

William is the head of one of the higher-ranking families in the village. Since the death of his wife in 1988, he lives part-time with one son in the village

and part-time with another son on the other side of the island. William suffered a stroke just after the celebration of his eightieth birthday and was subsequently partially paralyzed. His stroke promoted a sudden and intense outpouring of food gifts and assistance from most of the other households in the study area and beyond. One family from a neighboring village came and stayed for a month to help care for him and provide daily *sarao* (therapeutic massage). Neighbors visited often and brought favorite and hard-to-obtain foods. Even households that had long-standing conflicts with William took this opportunity to make amends and show their sympathy. Thus, in his disabled state, William interacted with many more households more intensively than he did prior to his stroke.

Conclusion

We have argued that Rotuman culture shapes the experience of persons with disabilities primarily on the basis of salient propositions about personhood and social relationships. We regard these propositions as part of a broad pool that individuals may choose to employ or disregard, according to circumstances and personal agendas. Indeed, a person with neither physical nor mental impairment may be socially handicapped by other circumstances that call these propositions into question.

Our data suggest that the treatment of persons with disabilities on Rotuma is less the consequence of a prescribed set of norms, based on notions of what such individuals can or cannot do, than the result of negotiated interactions that derive from total social histories.[8] Thus impairments that occur later in life are less likely to require dramatic role shifts (e.g., playing the role of a "blind person") than in societies where disabilities are often primary criteria for defining roles. Persons with visual impairment on Rotuma may drop certain tasks from their repertoire, for example, while continuing to do others that are socially valued. Their interactions with others may show a great deal of continuity. Much depends on their life circumstances and the state of their relationships with people around them. If they have been generous, well-regarded individuals, they are in a strong position to negotiate favorable conditions for themselves, despite impairments. If they have led less than exemplary lives, their positions are correspondingly weakened. Their disabilities under the latter circumstances may be attributed to moral transgressions, lessening the social obligation of others to provide support. Persons with congenital conditions that decrease their opportunities for marriage or impair normal functioning are in a more vulnerable position. They may be deprived of the opportunity of ever becoming, in Rotuman terms, a fully competent adult. But they, too, have negotiating power depending on

circumstances. In the final analysis, the ways in which persons with disabilities are treated on Rotuma depend far more on individual dispositions and agendas than on any set of preconceived cultural premises.

NOTES

Earlier versions of this article were presented at annual meetings of the Association for Social Anthropology in Oceania (ASAO) in New Orleans (1992) and Kailua-Kona, Hawai'i (1993). We wish to thank Maureen Fitzgerald and Jocelyn Armstrong, organizers of the sessions on disability in Pacific societies, for their encouragement and support as well as their constructive criticisms. We also are grateful to several anonymous reviewers whose assessments forced us to clarify our central points.

1. In the spirit of this cultural emphasis, we present our case material intact—a metastatement of sorts—rather than selecting fragments to illustrate specific points.

2. See Clifford (1988) and Abu-Lughod (1990, 1991) for critiques of the tendency for anthropologists to impose unproblematic coherence on cultural models.

3. For a more extensive discussion of this view of culture, see Howard 1985.

4. Churchward lists a third term, *'atuamürsoro,* which he translates as elephantiasis of the buttocks (1940:352). Elisapeti Inia, one of the most knowledgeable Rotumans alive, disputes this translation, insisting that the word should be *'atuamürsolo,* which would translate as a *penetration* of the buttocks by an *'atua,* or ghost. Mrs. Inia says that the condition to which this term refers is polio, not elephantiasis (pers. com., 1993).

5. The notion of "ideal personhood" was not directly invoked at the time of the research, although in retrospect it was the clear implication. In negotiating criteria we settled on a definition of disability that centered on conditions that interfered with a person's being able to meet expectations appropriate to an individual of the same age and sex, without impairments.

6. We nevertheless were able to identify persons with disabilities in the village where Rensel conducted her research on household exchange and can extrapolate. Since categorization of persons as disabled in 1960 was based on Rotuman research assistants' notions of individuals "who had something wrong with them," while in 1989 we made our own assessments on the basis of a combination of observations and comments in unstructured settings, extrapolations are necessarily suspect, but we present them for what they are worth. In 1960 there were thirteen households in this village containing ninety persons, two of whom were identified as having disabilities, yielding a prevalence rate of twenty-two per thousand, nearly identical to the rate for the island as a whole. In 1989 the village included seventeen households and ninety persons, nine of whom had disabilities, providing a prevalence of one hundred per thousand, nearly a fivefold increase.

7. *Haharạgi* also refers to a stage in the Rotuman life cycle, the stage intermediate between childhood and married adult (see Howard n.d.).

8. An anonymous reviewer pointed out that even in complex societies social identity among family and friends is based on many factors, not exclusively on disability. Although we acknowledge this to be a valid observation, we believe that cultural patterns nevertheless shape the experiences of individuals with disabilities, even among intimates. Thus the salience of categorical propositions (beliefs) in Western societies leads even family members to presume limitations based on types of disability. We would argue that while intimacy often results in such expectations being overridden, the very existence of the stereotypes requires a good deal of counterexperience to overcome them. On Rotuma this is not the case.

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CONSUMER DEBT, ALCOHOL USE, AND DOMESTIC VIOLENCE IN GUAM

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Using community-level economic and crime data derived from secondary sources, this brief study explores the relationships between consumer debt, alcohol use, and domestic violence in Guam for the years 1982 through 1993. The results of the analysis indicate that higher levels of consumer debt are strongly related to greater alcohol consumption and domestic violence, thus suggesting that "financial strain" may be an important variable for understanding patterns of substance abuse and family violence in Guam. The implications of these results for theory and future research on substance abuse and domestic violence are also discussed.

THIS RESEARCH NOTE explores the relationships between consumer debt, alcohol use, and the prevalence of domestic violence in Guam. A number of earlier studies have examined similar associations for U.S. mainland populations (see Catalano et al. 1993 and the studies cited therein). The current literature, however, reveals virtually no analyses of this kind for Guam or Micronesia, where domestic violence resulting from alcohol abuse appears to be pervasive (e.g., Hoff 1992; Nero 1990).[1] The results of previous studies indicate that contracting economies contribute to higher rates of unemployment, which in turn relate to greater sales of beverage alcohol, arrests for

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alcohol-related problems (Brenner 1975, 1977; Catalano et al. 1993; Catalano 1991; Crawford et al. 1987; Greenburg and Grunberg 1995; Layne and Lowe 1979), and potentially higher rates of domestic violence (Hoff 1992; Nero 1990; Pinhey 1997).

This brief study examines increases in alcohol use and domestic violence as functions of "financial strain" (Peirce et al. 1994), which is operationalized here as the annual rate of consumer debt for Guam residents.[2] We argue that consumer debt is a relatively recent phenomenon in Guam, and that such debt may have previously been a family-shared obligation rather than an individual burden. Additionally, persons in Guam may now be obligated to formal lending institutions outside of the family for financing, resulting in rigidly scheduled payments and the potential besetment of collection agents upon individuals when payment schedules are not met.[3] Just as unemployment may result in a significant loss of family income, thus producing economic strain, so increasing consumer debt is likely to reduce a family's discretionary spending, resulting in financial strain, greater alcohol use, and increasing domestic violence. We therefore anticipate that greater consumer debt in Guam will correspond to increasing alcohol consumption, and that growth in both of these measures will correlate with increasing reports of domestic violence.

Methods and Measures

Three community-level variables were analyzed to explore the potential effects of financial strain on alcohol consumption and domestic violence in Guam: *consumer debt, beverage alcohol taxes,* and *family violence offenses.* Complete data for these variables were available for twelve years (1982–1993 inclusive). The consumer debt measure reflects annual levels of outstanding installment loans (excluding real property) for Guam residents. Alcohol use is measured using the annual total tax receipts received from the sale of malted beverages on the island. The domestic violence measure reflects annual summaries of family violence offenses reported to the Guam Police Department.[4] To attain comparability for graphing the relationships of these variables, they were standardized so that each has a mean of zero and a standard deviation of 1. Additional calculations were performed on unstandardized measures.

Results

An inspection of the data presented in Table 1 suggests that consumer debt, malted beverage tax receipts, and reports of domestic violence in Guam are

TABLE 1. **Measures of Consumer Debt, Alcohol Taxes, and Reports of Domestic Violence in Guam (1982–1993)**

Year	Consumer Debt (millions of US$)	Alcohol Tax (US$)	Domestic Violence (no. of incidents)
1982	98.0	232,757	28
1983	121.0	369,216	19
1984	143.9	377,394	32
1985	142.0	818,428	38
1986	163.2	804,603	37
1987	176.1	930,766	62
1988	183.3	1,114,120	74
1989	207.8	1,282,748	58
1990	294.5	1,413,714	90
1991	347.2	1,421,469	95
1992	329.7	1,322,426	91
1993	328.4	1,459,070	95
Mean	211.3	962,225.92	55.9
Increase	235%	526%	239%

correlated for the years studied. Indeed, the values for each measure indicate a clear pattern of linear increase. Since the mean scores in Table 1 may be interpreted as the average annual increase for each of the three measures, it can also be seen that consumer debt, alcohol use (as measured by malted beverage tax receipts), and reports of domestic violence are growing rapidly in Guam. To further explore increases for these three measures, we calculated the percentage increase for each variable using the first and last entry in each column as our data points (see Blalock 1972:12–15; Sanders and Pinhey 1982:277). These calculations reveal that reports of domestic violence in Guam have increased by 239 percent over the twelve years studied (95 – 28/28 = 239%), that alcohol sales taxes have increased by 526 percent for the same period, and that consumer debt has increased by 235 percent.

The strong associations between the three variables may be seen more clearly in Figure 1, which presents the graphic results for the three standardized measures. The portion of the graph beginning with 1989 is particularly remarkable. As may be seen, beginning in 1989 there is a clear and sharp increase in both consumer debt and reports of domestic violence in Guam that correspond with increasing alcohol sales. Consumer debt, family violence, and malted beverage tax receipts all decrease slightly in 1992. Between 1992 and 1993, the three measures appear to converge as consumer debt begins to level off and family violence and alcohol use increase slightly.

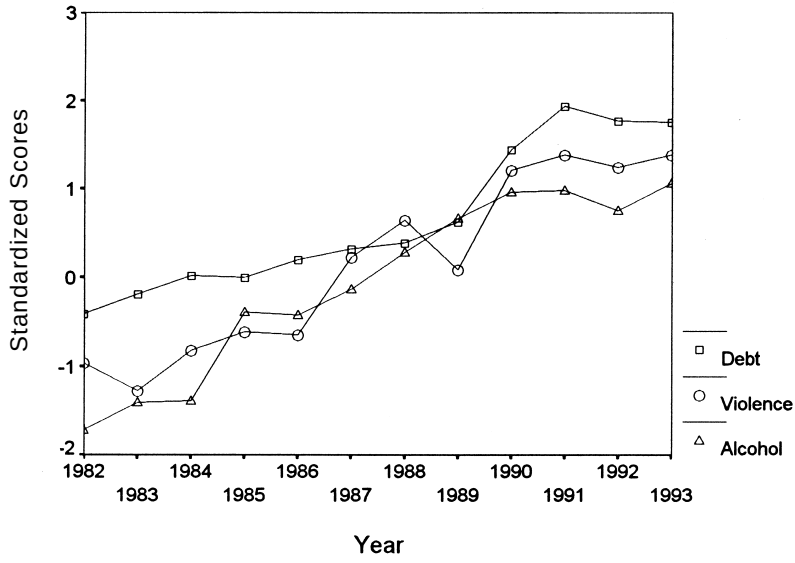

FIGURE 1. **Consumer debt, alcohol taxes, and domestic violence in Guam.**

The strong similarity in patterns of change for these variables beginning in 1989 suggests that family financial conditions may have a significant influence on substance abuse and violent behavior in Guam.

Finally, to further explore these relationships, we conducted an autoregression analysis using the three measures (Table 2). Autoregression techniques are preferred when estimating regression coefficients for time-series data (SPSS 1994). As may be seen in Table 2, consumer debt is related significantly and positively with alcohol sales taxes (equation 1) and reports of domestic violence (equation 2). Alcohol sales taxes are also related significantly and positively with reports of domestic violence (equation 3). When domestic violence is regressed on both variables simultaneously, the coefficients retain their significant and positive associations (equation 4).

Discussion and Conclusions

This research note explored the linkages between economic conditions, alcohol consumption, and family violence in Guam by examining the relationships between consumer debt, alcohol tax levels, and reports of domestic violence. The results of the analysis indicate strong and positive correlations

TABLE 2. **Standardized Prais-Winsten Autoregression Coefficients for Regression of Alcohol Taxes and Reports of Domestic Violence on Consumer Debt and Alcohol Taxes in Guam (1982–1993)**

	Alcohol	Domestic Violence		
	(1)	(2)	(3)	(4)
Debt	.778**	.922***		.498*
Alcohol			.934***	.502*
R^2	.605	.851	.874	.951
R^2 adjusted	.527	.824	.846	.933

* $p < .05$, ** $p < .01$, *** $p < .001$.

between these measures, suggesting that financial strain produces greater alcohol consumption (e.g., Pinhey 1997), which in turn leads to increasing levels of family violence (e.g., Nero 1990). This interpretation presupposes that individuals in Guam are integrated into the economic system such that unemployment or consumer debt would result in their greater distress, and that two outcomes of this distress are greater alcohol consumption and violent behavior.[5] Although empirical documentation supporting these assumptions for Western Pacific populations is limited (e.g., Pinhey 1997), the results of the present study provide evidence suggesting that economic conditions may significantly influence alcohol use and violent behavior in Guam. Thus, future researchers concerned with substance abuse and domestic violence in Micronesia and Guam may wish to systematically explore the effects of contracting economies on mental health, substance abuse, and violent behavior.

An additional implication of our results for theory and future research centers on recent findings showing that women and men respond differently to the same stress-producing circumstances (for reviews, see Horwitz and Davies 1994; Horwitz, White, and Howell-White 1996; Pinhey and Ellison 1997). In summary, gender differences have been shown in response to stress resulting from divorce and bereavement, with greater alcohol consumption among men and greater depression among women. Whether Asian-Pacific women and men respond differently to the stress produced by financial strain is not clear from the present study. Future researchers may wish to systematically examine the influence of financial strain (e.g., job loss, underemployment, consumer debt) on gender differences in response to stress, and the influence of stress resulting from financial conditions on depression and substance abuse in Guam. Moreover, since the current legal drinking

age in Guam is eighteen, certain aspects of our results may also have public policy implications. For example, our findings suggest a significant increase in beverage alcohol use in Guam for the period studied, and several earlier investigations have linked states' enactment of higher minimum legal drinking ages to substantial reductions in morbidity and mortality resulting from traffic crashes (see U.S. General Accounting Office 1987). Researchers may wish to explore the potential impact of similar legislation for Guam.

The potential limitations of this study deserve mention. First, our analysis is limited to three variables, and multivariate analyses using a greater number of measures may produce different patterns and results. Second, although our measures appear appropriate for the study (annual summaries for consumer debt, malted beverage tax receipts, and domestic violence offenses reported to the police), alternate measures may result in different conclusions. For example, annual tax receipts for malted beverages may be inflated since they reflect alcohol consumed by increasing numbers of tourists in Guam, and the use of unemployment rates or other similar measures may prove more enlightening than consumer debt.[6] Moreover, since our analysis relied on community-level measures, we could not identify specific social characteristics (gender, social class, ethnicity, age) that may be important for describing individuals who are at risk for participating in alcohol abuse and family violence. And finally, our study covered only twelve years. It is conceivable that the associations we found for this period may not be representative of the entire history of the relationships of financial strain to alcohol use and domestic violence in Guam. Thus, it is possible that studies examining these or similar variables over longer periods may detect different patterns and relationships.

These potential limitations aside, the results of our analysis support the conjecture that economic conditions contribute to greater alcohol consumption and family violence. Systematic studies focusing on the influence of variations in family economic circumstances for mental health, substance abuse, and violent behavior should therefore be an important priority for future research in Guam and Micronesia.

NOTES

Our appreciation is expressed to Donna Lewis Pinhey, Dirk Ballendorf, Donald H. Rubinstein, Dale B. Robertson, and three anonymous referees for their insightful comments and suggestions on earlier versions of this manuscript.

1. Although no analyses could be found that directly linked alcohol use with domestic violence in Guam, an early study conducted by Marshall clearly linked alcohol consumption with violent behavior in Chuuk (1979). For additional studies of alcohol use in Guam,

see Pinhey, Workman, and Borja 1992 and Pinhey 1997. For additional studies focusing on alcohol use in Micronesia, see Marshall 1987, 1991, 1993. Finally, see Nero's excellent qualitative analysis, which links alcohol use with domestic violence in Palau (1990).

2. Financial strain is conceptualized elsewhere as "economic stress" (Hoyt, O'Donnell, and Mack 1995; Ortega et al. 1994) and "economic pressure" (Lorenz et al. 1993). Each of these conceptualizations refer to economic circumstances—such as increased debt, job loss, or underemployment—that have negative impacts on emotional well-being, alcohol use, and domestic violence (e.g., Gelles and Cornell 1990).

3. See Thompson 1947:151 and Rogers 1995:105, 137 for brief descriptions of the origins and history of banking and "small loans" in Guam.

4. Data for consumer debt were taken from Section X of the annually published *Guam Annual Economic Review* (Economic Research Center 1979–1994). Data for gross receipt taxes received from the sale of malted beverages were derived from the Business Privilege Tax Collection section of the *Guam Annual Economic Review.* Data for family violence offenses reported to the Guam Police Department are summarized under Part II offenses and may be found in the annually published *Guam Police Department Uniform Crime Report* (Guam Police Department 1980–1994).

5. Nero notes that in Palau drinking is an accepted outlet for "frustration" and suggests that "stress" leads to violence (Nero 1990:86, 80). Whether these relationships hold for Guam or for other locations in Micronesia is unclear. Thus, an important research question for the region centers on the identification of sources of stress and the effects of stress for substance abuse and violent behavior. The present study assumes that greater debt produces distress, which in turn contributes to increased alcohol consumption and domestic violence.

6. Regarding employment characteristics and domestic violence, Gelles and Cornell report that unemployed men commit double the rate of wife batterings as employed men, and men working part-time have an even higher rate of assault (1990). Unemployment rates for Guam were unavailable for the present study but may prove particularly useful measures for future researchers wishing to better understand the causes of domestic violence.

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THE MAKING OF *AI MATAI:* A CAUTIONARY TALE IN FIJIAN HISTORIOGRAPHY AND PUBLISHING

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Ai Matai is the official history of the Fiji military's participation in the Malaya Emergency, written by Tevita Nawadra. What ought to be a milestone in Fijian historiography in fact has had little public or academic impact. Publication in the Fijian language and poor distribution have created a small readership that is largely local. Further official military histories are currently being researched in Fiji, and the experience of *Ai Matai* provides useful lessons and warnings about how to and how not to go about such projects. This article attempts to identify the difficulties in the writing of history in Fiji and to serve as a reminder of the basic problems that exist where technical resources are deficient, and attitudes, values, and procedures are different from those in the so-called developed countries. If the lessons of *Ai Matai* are heeded, it should be possible for Fiji to produce official military histories that will correspond with and share the positive features of their counterparts in the First World.

On 19 January 1996, in the spacious grounds of Government House, Suva, the president of Fiji launched *Ai Matai,* the official history of First Battalion, Fiji Infantry Regiment, in the Malaya campaign between 1952 and 1956.[1] Present at the occasion were an assortment of government officials, former servicemen, military officers, and their partners, but apparently there were no academics.

My interest in *Ai Matai* stemmed from difficulty in obtaining a copy. I knew the year before that such a book was in progress,[2] and I first saw a copy in a downtown Suva book shop in July 1996. A week or so later, I asked the University of the South Pacific Book Centre to order copies, but the

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army informed them that these were only available to service personnel. When I returned to the downtown book shop, there were no copies left on the shelves, but one was produced from under the counter—as though it were a pornographic magazine—and I purchased a hardback edition for the very reasonable sum of F$29.95. It was very puzzling and not a little suspicious that a book with obvious and legitimate public interest was under wraps to the extent that I had to obtain a copy by semisurreptitious means. On the principle that forbidden fruits taste sweetest, I decided to look into the matter.

In my follow-up research it became apparent that the print media had done a poor job of reporting *Ai Matai.* The *Fiji Times* reporter said that the "book was compiled by ex-servicemen headed by Colonel Joji Mate."[3] In fact, the book was authored by one Tevita Ratulailai Nawadra—as the book's spine, title page, and dust jacket all make clear—and the text had only been vetted, not written, by a committee of five veterans of the Malaya campaign under the chairmanship of the late Colonel Mate (p. 524). To this day the author has never received proper credit in the print media, and this is symbolized in newspaper photographs of the book launch. The *Fiji Times* photograph showed the president and Adi Lady Lala Mara, who is brandishing a copy of the book. In the Fiji-language newspaper *Volasiga,* the roles were reversed, with the president holding a copy and Adi Lala looking on. In the other Fiji-language paper, *Nai Lalakai,* three prominent Fijians are sitting side by side admiring a copy; again the author goes unmentioned and is nowhere to be seen.[4] The television news coverage was a good deal better: the Fiji One TV report correctly specified the authorship, and the author himself explained how he went about researching the book. But that was the only media acknowledgment of Tevita Nawadra at the book launch, and since then his name has hardly been mentioned in the print media.

But I had a more fundamental concern. *Ai Matai* ought to be a milestone in Fijian historiography. With over 500 pages of text, it is by far the largest history book in the Fijian language, and its impact should have been correspondingly significant. But the distribution of the book has been restricted, and it is still difficult to obtain copies. It is unusual to find it in book shops, meaning that the necessary constant reminder is not before the reading public, and knowledge of its existence overseas must be minimal. I hope, with this essay, to redress the situation—to make the book known to a wider audience, which, in turn, will give its author belated acknowledgment. It is not my purpose, except incidentally, to evaluate *Ai Matai.* Rather, I will relate how the book came into being, discuss the implications of its limited distribution, and comment generally on the Fiji Military Force's efforts to produce an official war history.

The Author and the Making of the Book

In August 1987 Tevita Nawadra was approached by Sitiveni Rabuka (now the prime minister of Fiji) to write *Ai Matai.* It was not the first time that an attempt had been made to write an official history of the Malaya campaign (or the Malaya Emergency, as it was known at the time). Work commenced in the early 1980s, when Warrant Officer I Sam Gilhooly (himself a veteran of the Malaya campaign but retired and working for the Returned Servicemen's Association in New Zealand) took leave and came to Fiji as an honorary captain. He was assisted by another Malaya veteran, Honorary Lieutenant Afalusi Koroi, who died soon afterwards. Gilhooly returned to New Zealand to complete the job, but he too died, within three years. It is reported that his landlady burnt his papers, including his work on the book and the original First Battalion war diary. So Captain Nawadra had to start from scratch (p. xvii). By the late 1980s, moreover, there was an added urgency in that the Malaya campaign veterans were getting on in years, and if work did not resume quickly, many would not likely be around to provide their reminiscences or to enjoy the eventual book.

It would appear that the moving light for the book was the then president, the late Ratu Sir Penaia Ganilau, the former battalion commander in Malaya who had long wanted such a book written.[5] But the credit for choosing Tevita Nawadra surely goes to (then) Colonel Rabuka (p. xvii). Captain Nawadra does not know why he was chosen to write the book: he was never told and he never asked. But it can be surmised that he fitted the bill in most respects, being a well-educated former schoolteacher and public servant who had studied at the University of Leicester. His experience includes attachments to the Curriculum Development Unit (1971), the East-West Center in Honolulu (1972–1974), and the Fijian Dictionary Project (1974–1986). His new assignment would involve extensive interviewing of elderly Malaya campaign veterans, and it is not difficult to see that Nawadra's personality and education made him an appropriate person to approach the task with sympathy and understanding.

For the author the job offer was not only unexpected but gratifying—and the timing, so soon after the first 1987 coup, was auspicious. He was being asked by the man whom most Fijians regarded as their savior to perform an important task, and he could not refuse—even had he wanted to. He answered with approximately these words: "If you have done this [mounted a coup] for the Fijian people, then I am honored to do something for them too, especially those who gave their lives for peace." In this way, a retired civil servant without an armed service background found himself in the army on an indefinite commission, in November 1987, with the rank of

lieutenant (later captain), and thrust into the unfamiliar role of military historian.

Much else was unfamiliar in what he described to me as "a new life." He had to be shown how to wear a beret and how to salute—and the first time he was saluted was an emotional experience. It was very different from "the office," that is, his previous dictionary work, and he had to learn quickly how to present himself and to be "perfect in my turnout." Being a regular soldier, he feels, provided insights into the military mind that enabled him to better envisage the day-to-day life of a combat soldier and helped when interviewing Malaya campaign veterans. It brings to mind the famous dictum of Edward Gibbon that service in the Hampshire Militia was useful to him as the historian of the Roman Empire. Indeed, Captain Nawadra went to Malaysia in 1988, shortly after the commencement of the project. There he visited the areas where the First Battalion operated and heard Malaysian versions of what Fijian troops did there (p. xviii). In this respect he is akin to Thomas Babington Macaulay, who visited every battlefield described in his *History of England.*

Ai Matai makes some use of documentary evidence, but the major source is overwhelmingly the veterans' recollections. The interviewing proceeded fairly rapidly at first, because many of the veterans had been called up for service in the immediate aftermath of the 1987 coups. Many were close at hand in barracks in and around Suva and available for interviewing, often in groups. Some 800 Fijian soldiers served in the Malaya campaign in 1953 alone, and 146 of them are listed as having been interviewed (pp. 522–523). This, however, is a lower-bound figure, because Captain Nawadra was sometimes unable to get the names of everyone in the early group interviews. Once the political situation settled down, the veterans dispersed and had to be interviewed in their villages. Tracking them down was not easy, but the Battalion Commander's Diary was a starting point, as it contained names of many of the soldiers (p. xviii).[6]

The bulk of the interviews and associated transcribing took five years—until 1993—and predictably, with so many different people, there were extremes of reaction. A few, but not many, were downright uncooperative, usually because they genuinely did not want to talk about painful and searing experiences: "They had served their country, come back, and put it behind them" was how Captain Nawadra explained it. Others of taciturn mien needed coaxing. At the other extreme were a few who never stopped talking, and Captain Nawadra would eventually make good his escape "with my ears ringing." One such person talked compulsively for several hours and made the captain very late for his next appointment in the same village. He eventually turned up for his second appointment at five in the afternoon to find

lunch still waiting for him, replete with *yagona.* The grog bowl was replenished and the interview commenced with less than ideal results. The trouble was that Captain Nawadra and the respondent came from rival areas in Fiji (one from Verata, the other from Bau), and neither was prepared to be the first to stop drinking. The man insisted on talking at length, but Captain Nawadra, tired from his previous interview, was having trouble simply staying awake. So the host talked into the tape recorder while the captain drifted into sleep, and every half hour he was woken to turn or change the tape: "I only found out what he was talking about when I got back to base." Such is a day in the life of an oral historian.

The interviews were open-ended with few set questions, although each respondent was given a list of dates to jog the memory and to help prevent the chronology getting skewed or separate events being conflated. At first the captain used an old tape recorder from his dictionary days, a bulky affair resembling a small suitcase. Eventually the army provided more suitable equipment. He initially had a field assistant who also transcribed his own interviews, and trainee secretaries were allocated, but they had difficulty in coping; it was often easier to do things himself. The work was time-consuming, because the tapes were transcribed in full rather than summarized. The captain had to believe in the basic sincerity of his respondents, or writing the book would have been impossible (p. xviii) Sometimes, however, he was given conflicting versions of the same event and had no external means of verification. A case in point is the story of the action leading to a particular soldier's being awarded the Military Medal (p. 304). He recorded several versions of the same story, but details varied between the widely dispersed tellers. Unable to get the various people together or to return to ask follow-up questions, he attempted to reconcile the differing versions and produced a composite account—very much as a historian will often do when the documentary evidence conflicts. At other times, as he explained on television during the book launch, he had to decide which version was the most likely.

Ai Matai is essentially an oral history of the Fijian soldier in the Malaya campaign. It is not based on that blend of oral testimony and documentary evidence typical of many other such histories. The reasons for the stress on the oral record relate to opportunity and the preservation of sources, and the point can be illustrated by the contrasting examples of other war books. When writing his dissertation (in the late 1960s) on Australian soldiers' experiences in the First World War, Bill Gammage corresponded with 269 (of 350 asked) veterans and interviewed some of them. His sample of respondents was necessarily limited by the event having taken place fifty years earlier; and many of the respondents passed away in the period between the acceptance of the dissertation and the publication of the book deriving from

it. Gammage's main source, however, was the diaries and letters of almost 1,000 soldiers in the Australian War Memorial and other repositories.[7] By contrast, the Fijian experience in the Malaya campaign did not generate much of a written record from the ordinary soldiers, and in any case there is no repository in Fiji comparable to the Australian War Memorial. Hank Nelson's book on Australian prisoners-of-war of the Japanese provides another contrast. In addition to interviews with 158 survivors, Nelson drew on a variety of written records including diaries and reminiscences, official war histories, and almost fifty books by and about the POWs.[8] There is simply not the richness of written documentation on the Fijian contribution to the Malaya campaign. A third example is John Barrett's analysis in the 1980s of some 3,700 bulky questionnaires from Australian World War II veterans, many of whom were contacted through the cooperation of the Returned Servicemen's League.[9] It is difficult to imagine that Fiji's veterans would, in any numbers, return completed questionnaires. In all, circumstances in Fiji conspire against writing such finely grained military histories unless the researcher is prepared to go to quite inordinate lenghts.

Historiographic Location

War is a subject of enduring and seemingly universal interest; and the historiography of warfare is as diffuse as its practitioners are varied. Military history is an incredibly diverse subject and home to the whole spectrum of so-called amateur and professional historians. Alongside a huge and heavily pictorial literature for a wider reading public and a sizable corpus of soldiers' reminiscences[10] is a more scholarly literature, enormous in size and varied in scope—the product of "that monstrous modern regiment, the academic strategist."[11] This ranges from studies from the vantage point of high command, military-government relations, and diplomacy through to history-from-below books that focus on the soldiers' experience. The latter trend, indeed, is under way in Pacific Islands historiography as academic researchers —predominantly anthropologists but including historians—have "gone to war," resulting in several publications in recent years focusing specifically on the islanders' experience during World War II.[12] The increasing emphasis on the Pacific Islanders' role and experience is a welcome development.[13]

There is also a well-defined genre of official war histories, into which *Ai Matai* fits; and it is a little-known fact that there is an official history of Fiji in the Second World War, written by an expatriate officer.[14] Genre or not, government-sponsored war histories are also highly differentiated in terms of authorship and scope, qualities that are nicely captured by the *Official His-*

tory of New Zealand in the Second World War, 1939–1945. It contains four series: "Campaign and Service" (nineteen titles), "Unit Histories" (twenty-one titles), "Episodes and Studies" (twenty-four booklets, consolidated into two volumes), and "The New Zealand People at War" (three titles). Like other countries' multivolume official war histories, there are the usual battalion and campaign histories and a smaller number of domestic accounts. The individual authors come from a wide variety of service and nonservice backgrounds, and sometimes a combination of both—although the campaign and unit histories are usually written by people with combat experience. The domestic accounts, typically, were written by career academics with nonservice backgrounds, and in fact the volume *Political and External Affairs* was Professor F. L. W. Wood's *opus magnum.* Typically also, the authors had unrestricted access to official documents and the overall time frame is lengthy; the last volume was completed in 1982 (and even then publication was delayed until 1986).[15]

The diversity of the multivolume official war histories—in New Zealand and elsewhere—makes it difficult to locate *Ai Matai* even within this genre. The mix of theme and author makes it the more so. It is a conventional battalion history but, unconventionally, is written by one with no combat experience. Nor can Captain Nawadra, as an official war historian, compare with careerists such as C. E. W. Bean or a Samuel Eliot Morison, both of whom wrote numerous volumes about a single war.[16] *Ai Matai* is additionally unconventional: being based on oral testimony rather than official documents, it resembles more the history-from-below books than the typical battalion histories, which are equally concerned with strategy and command. *Ai Matai* is also at once a battalion history and an entire official war history in itself, again an unusual situation but explicable because of the relatively small-scale extent of Fiji's involvement in the Malaya Emergency.

In short, *Ai Matai* captures in miniature the nebulous qualities of official war histories, with their overlapping categories. The book is difficult to categorize, because it is the exception to just about every rule: a battalion history that draws lightly on official documents, relying mostly on oral testimony; an author with an unusual background (which implies no criticism); and a relative lack of secretarial and research support. The only point with which the author can be faulted is that insufficent use was made of the documentary record, even taking into account the enormously time-consuming task of almost single-handedly collecting the oral testimonies and then transcribing some 260 cassette tapes. But more use could have been made of the official documentation in what is supposed to be an official history, after all.

Funding and Distribution

The book was produced under fairly rudimentary circumstances. The resources available to Captain Nawadra do not compare, say, with those at the disposal of the New Zealand oral-pictorial project on the 28 Maori Battalion in the Second World War. In addition to gathering oral testimony, the 28 Maori Battalion Project is collecting letters, diaries, songs, poems, photographs, and memorabilia and entering detailed biographical data (including video images and voice) of some 4,000 individuals into a computerized database. Although operating on an amazingly low budget given the amount and variety of work, this Massey University–based project draws on resources (including volunteer helpers) that Captain Nawadra never dreamed of.[17] Another computer-based military history project, involving resources beyond those of the Fiji Army, is the First Australian Infantry Force project at the Australia Defence Force Academy.[18]

Yet the army was beneficent within its means. Captain Nawadra's salary was met from public funds, but extra efforts had to be made to see the book to a conclusion. The army raised F$50,000 toward the book, the proceeds coming from the profit from an insurance premium that would otherwise have been distributed among the troops. The army then decided, in May 1995, to organize a massive fund-raising exercise to meet the balance of the book's production costs. A senior army spokesman declared: "We did not want to depend on Government assistance, and we're not considering Government funds."[19] A F$51,000 target was reported,[20] commencing with a dinner costing F$100 per couple. But the main thrust of fund-raising would be runathons involving every battalion, and when the day came, they all tried to outdo each other. The follow-up newspaper report, while itemizing a number of substantial private and district donations, did not specify the total money raised.[21] In fact, the fund-raising realized a staggering F$72,000, which brought the total to F$122,000. This sum more than covered the eventual printing (F$47,000) and other costs (F$43,000), and Captain Nawadra returned F$32,000 to the army with the recommendation that a revolving fund for future historical projects be established.[22]

The typesetting and printing were undertaken by the Government Printer (who is slow and costly), and the print run was 1,500 hardback copies and an equal number in paperback. Copies sold at the official book launch "like hot cakes," according to the television news coverage; 104 copies changed hands, to be precise. Thereafter, the plot thickens. In a strange decision, instead of the book's being made publicly available, sales of *Ai Matai* were initially restricted to service personnel. No security issues were at stake, the book was likely to have widespread appeal (among Fijians at any rate), and

sales would have helped to recoup the initial outlay and provide funding for future historical projects.

So what should have been a milestone in the historiography of Fiji was largely hidden from sight. A sense of the author's quiet disappointment came across during our discussions. He was presented with the unexpected opportunity and challenge to produce something of lasting worth. As he said to me, "The book had to be from a uniquely Fijian perspective—written by a Fijian from the experiences of Fijian soldiers for a Fijian audience, and for future reference." The book was intended to celebrate and to create a public awareness of the Fijian contribution to the Malaya campaign, but these objectives are largely negated if the book's distribution and availability is so limited, especially among those too young to remember the event—anyone under age fifty. Restricting it to service personnel is akin to preaching to the converted, not creating a wider public awareness. Neither does the limited distribution do anything to foster habits of reading—which are becoming increasingly required—among people who are more comfortable with orality. There seemed no valid explanation for the decision to limit circulation, except perhaps as a function of a restricted literacy setting where the tendency is to have a secretive attitude toward knowledge.[23]

Actually, it was nothing more sinister than a few wires getting crossed within the army. The book was intended for public sale, but the person responsible for distribution had not been made aware of this. Thus, the University of the South Pacific Book Centre placed an order in July 1996, only to be told that copies were not available to the general public. In March 1997, at my prompting, the Book Centre placed another order, and on this occasion the army agreed to make copies available. They would not, however, accept an LPO (local purchasing order) but required that someone from the Book Centre pick up copies at the Nabua base. They further specified a cash on collection arrangement—which is contrary to normal book trade practices where the publisher or distributor arranges dispatch. Even without such mixups, sales would be limited, not least because of the lack of good book outlets in Fiji. Nor is it likely that sales will be augmented by excerpts from *Ai Matai* that were serialized in *Nai Lalakai,* beginning on 19 March 1997, because the book itself is not mentioned.

Then there is the small matter of the F$32,000 that Captain Nawadra returned to the army to establish a revolving fund for other such projects. To this sum can be added whatever monies have been realized through sales—which would have been somewhat more had the book been publicly available from the onset. Because no taxpayers' money is involved, these funds have not been spirited away into consolidated revenue but, instead, placed in a separate bank account for future historical work. A priority is an English

translation of *Ai Matai.* Plans are also afoot for a volume on the Fijian contribution to peacekeeping in the Sinai desert—as Fiji One TV reported in its coverage of the book launching ceremony—but on this occasion the army will seek outside funding. The feeling is that they have done their bit with *Ai Matai,* and now it is the government's or a sponsor's turn to pick up the tab. Indeed, two official histories of Fiji peacekeeping operations are in preparation. The writer of the Sinai history is Captain Stan Brown, a retired naval officer (who has just completed an official history of the Fiji police force). Costs are minimal because, as a retired person, he receives no salary, only out-of-pocket expenses. The Lebanon history is being written by Jim Sanday, a former lieutenant-colonel, with some funding being provided by Taiwan.[24]

Summary

A balance sheet on *Ai Matai* has its debits and credits in about equal measure. The quality of newspaper reporting is regrettable, but in no way is the army accountable for the print media's shortcomings. The army's fund-raising efforts were magnificent but are offset by the inept distribution of the book. The latter can be put down to inexperience only up to a point, considering that the army's original intention was to sell copies to civilians as well as to war veterans. There has also been a tardiness in using the monies raised through *Ai Matai* to get a translation under way. This can only partly be put down to pressure of other work combined with key army personnel being absent on tours of duty.

The lack of an English-language version has prompted strong criticism in some quarters. It was decided to publish a Fiji-language version in the first instance for the sake of the Malaya campaign veterans, meaning that *Ai Matai* is detached from a considerable literature on the Malaya Emergency.[25] Critics have said that greater sales and an international audience would have resulted had the book been written in the English language; and from a commercial as well as an academic standpoint they are correct. The earlier-mentioned official history of Fiji forces in the Second World War was first printed in English and only later in Fijian, and adherence to this precedent would have been better for sales.[26] Sales of *Ai Matai,* as of late March 1997, were limited to the 104 copies sold at the launching ceremony; another 241 copies were distributed but not necessarily sold (and a number of complimentary copies were given out). In keeping with the plan to make *Ai Matai* available to Fijian people and especially the campaign veterans, those 241 copies were distributed for sale by district councils, who are no more experienced at book distribution than the army; in any case, the retail prices of F$20 (hardback) and F$17 (paperback), while very reasonable given the size

of the book, are either beyond the means or more than the average Fijian villager is prepared to pay for a book. A print run of 3,000 was overly ambitious, but at least the book is unlikely to go out of print in the immediate future. Nor have there been attempts to promote *Ai Matai* overtly in its serialization in *Nai Lalakai,* by specifying that the installments are excerpted from the book and indicating where and for how much copies may be purchased.

To continue the economic argument, an English version could have been distributed to a network of overseas service clubs and returned servicemen's associations, not to mention military academies and university libraries. An English-language version is clearly needed, but it is easier said than done. Probably the most difficult aspect of the eventual translation exercise will be to find someone both appropriately qualified *and* available. However, to date the army has made no serious attempt to translate their good intentions into reality.

Why were so many copies printed when likely sales were so limited? The army discussed the print run at some length, and the final decision was based on the combination of assumed sales and printer's quotes. The expectation that veterans and their families would readily purchase copies was overly optimistic. What settled the matter, however, was a quote from the Government Printer, who advised that a larger print run would result in a lower unit cost. This quote pushed the army into agreeing to the untenably large print run. Whereas a reviewer thought that the mere 500 copies of the first volume of *New Zealand and the Korean War* was "amazingly" insufficient, it would have been a realistic figure for *Ai Matai.*[27]

The care and preservation of the tapes containing the oral testimony will also need looking into. As of August 1997, they were securely housed with the transcripts and other research material at the Queen Elizabeth Barracks in Suva. Provision will need to be made for those 260 or so cassette tapes' transfer to air-conditioned premises and for extra copies to be made as a safeguard. It is for the better that the National Archives of Fiji, which has appropriate facilities, has considered laying claim to the tapes on those grounds.

A further concern is the question of the author's independence and rights to his own interpretation. This is one of the most vexatious problems connected with commissioned histories: there is a fine line indeed between "effective supervision" and heavy-handed "meddling or censorship."[28] Although the latter scenario was not the case with *Ai Matai,* the cynic might find tendencies in that direction. Captain Nawadra handed over the completed manuscript to the then president, Ratu Sir Penaia Ganilau, in late 1992. The president kept it for almost a year, which laid to rest the author's hope that the book's publication would coincide with the reburial of the

repatriated remains of twenty-two Fijian veterans of the Malaya campaign, scheduled for November 1993.[29] But the president decided that the book would be vetted, and he formed a committee under Colonel George Mate, who went through the manuscript page by page. The legitimate purpose was to weed out any factual errors that might have resulted from the reliance on oral testimony—a worry compounded by the fact that Captain Nawadra had never been a regular soldier in Malaya, or anywhere else for that matter. Captain Nawadra is happy with the committee's work, but he did lament that it "could have been done earlier."

One can only agree. The requirement that the book be vetted should have been stated at the onset; then the committee could have read each chapter while the next one was being written. Had this been done, the book might have been published in time for the reburial, and there would probably have been the bonus of additional sales.

The vetting procedures—or lack of them—set down for *Ai Matai* might well provide a precedent for overt interference next time round, where the correction of factual error spills over to a more thoroughgoing intervention concerning interpretation and conclusions. This is a distinct possibility and at odds with the conventions of official war histories elsewhere, where authors have unimpeded access to documents and freedom from political interference—whether from politicians, bureaucrats, or the armed forces.[30] These matters should be resolved for the official histories currently under way on the Sinai and Lebanon peacekeeping operations.

But any reservations are outweighed by the achievement of producing *Ai Matai* in good time. Eight years between commencement and actual publication may sound like a long time, but it is reasonable by the standard of official war volumes and especially when so much oral testimony had to be collected, transcribed, and digested.[31] The manuscript, moreover, once out of the clutches of the vetting committee, was promptly printed—unlike some war history volumes, where unconscionable delays are experienced between completion of the manuscript and its eventual publication.

There is bound to be criticism in some quarters about the very subject matter and its implied celebration of war and all its horrors.[32] It must also be admitted that *Ai Matai* will have little, if any, appeal to the Indian community, who view the army askance as an overtly Fijian institution. The army's involvement in the 1987 coups is an emphatic reminder on that point. But the problem goes back further. The Indo-Fijians' near-universal refusal to enlist in 1943, unless given the same pay and conditions as European troops, has been held against them ever since, although "more for modern political emphasis than for any honest attempt to understand the problem."[33] By contrast, army service is an honorable vocation for Fijians, and villagers like a

son to join the army, just as it was once the ambition of French Catholic families to have a son in holy orders. Even if an English translation eventuates, *Ai Matai* and books like it will find little favor with Indo-Fijians. Some might even argue, on the one hand, that *Ai Matai* will only serve to accentuate existing ethnic tensions, but that comment would apply to many books about Fiji where an appeal to one group entails rejection or indifference by the other. On the other hand, *Ai Matai* has obvious uses as a school and university text for Fijian culture and language courses.

Any realistic assessment needs to recognize that Fiji does not have the resources or local expertise to produce official war histories on the scale that the developed countries can.[34] Future activities will be limited by such contingencies, and again a contrast with New Zealand is instructive. With a population of about three million (compared with Fiji's 770,000), New Zealand has some fifty public historians being paid for what they do.[35] The very different financial and educational situation of postindependence Fiji has resulted in a miserly five commissioned historians in the past twenty-seven years. The first was Deryck Scarr, from the Australian National University, who wrote the biography of Ratu Sir Lala Sukuna.[36] More recently, Sir Robert Sanders (who had long service in the Fiji administration) assisted the incumbent president, Ratu Sir Kamasese Mara, in the writing of his memoirs—the project receiving financial backing from the Republic of China.[37] There are also the above-mentioned official histories of the police force and of the peacekeeping operations by Captain Brown and Colonel Sanday. The only ethnic Fijian (as distinct from expatriate, resident European, or part European) in this small group is Tevita Nawadra, the author of *Ai Matai.*

This record points to the dilemma of historical writing in Fiji. On the one hand, no young historians are being produced by the University of the South Pacific, largely because the discipline is seen as irrelevant to one's career prospects.[38] On the other hand, complaints have long been voiced that Fiji history, and Pacific history generally, is overwhelmingly written by outsiders rather than by Pacific Islanders. But the remedy—if this is really such a problem—has to come from within, and the necessary structural, institutional, and financial arrangements must be put into place. At the moment, commissioned histories in Fiji are being written by overseas academics, whose salaries are met by their home institutions, or by retired persons who live off their pensions and only expect out-of-pocket expenses. Only Colonel Sanday is a local person with discipline-specific academic training. It must be stressed that history does not come cheaply, only with different degrees of expense;[39] and historians take a long time to train. But a start needs to be made and the necessary skills nurtured—and it is pointless to complain about

the prevalence of outside researchers when no serious attempts are made to develop home-grown historians. In the meantime, Tevita Nawadra's *Ai Matai* stands as an exemplar.

NOTES

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1. Tevita R. Nawadra, *Ai Matai—Malaya: 1st Battalion, Fiji Infantry Regiment, Far-East Land Forces, 1952–1956* (Suva: History and Archives Unit, Republic of Fiji Military Forces, 1995), xx + 524 pp. Page numbers in parentheses within the text of this article refer to *Ai Matai* (which translates into English as "The First"). The First Battalion has served in other war and peacekeeping zones, including the Solomon Islands and Lebanon. The terms "First Battalion" and "*Ai Matai*" in this article relate only to the Malaya Emergency, during which time the First Battalion was popularly called "the Malaya Battalion." The Malaya campaign lasted some twelve years, but the First Battalion was present only from 1952 to 1956. The consequences for the soldiers and their families are negatively assessed by Brij V. Lal, *Broken Waves: A History of the Fiji Islands in the Twentieth Century* (Honolulu: University of Hawai'i Press, 1992), 149–151. Deryck Scarr mentions the campaign in passing in *Fiji: A Short History* (Sydney/London: George Allen and Unwin, 1984), 164. Brief accounts of Fiji's part in the Malaya campaign are provided in R. J. B. Ackland, "Notes on the Historical Development of the Fiji Military Forces," *Transactions and Proceedings of the Fiji Society* 9, no. 1 (1961–1962): 73–75; [Ian Thorpe and Aquila Savu], *Defending Fiji: Defence White Paper, 1997* (Suva: Parliament of Fiji, Parliamentary Paper No. 3 of 1997), 10–11.

2. My sophomore "Theory and Method of History" course in 1994 and 1995 involved a major oral history project for each student in the course, one of whom interviewed her uncle on his soldiering experiences in Malaya. We contacted the army for further information and were told, quite rightly, that material relating to *Ai Matai* could only be made available after publication.

3. *Fiji Times,* 20 January 1996, 3.

4. Ibid.; *Volasiga,* 22–28 January 1996, 1; *Nai Lalakai,* 20 November 1996, 4. The *Daily Post* (Suva) did not cover the book launch.

5. *Fiji Times,* 9 May 1995, 4; 11 May 1995, 7. Ganilau's role in the Malaya campaign is recounted in Daryl Tarte, *Turaga: The Life and Times and Chiefly Authority of Ratu Sir Penaia Ganilau, G.C.M.G., K.C.V.O., K.B.E., D.S.O., K.St.J., E.D. in Fiji* (Suva: Fiji Times, 1993), 46–50.

6. This diary was the unit "War Diary." These were compiled by all unit commanders on active service and were sent home month by month in chapter form. The Fiji Military Forces headquarters has numerous Middle East "Commander's Diaries" (*not* "War Diaries"). The name was changed because although the later peacekeeping activities can be operationally dangerous, for a "war" one needs an "enemy."

7. Bill Gammage, *The Broken Years: Australian Soldiers in the Great War* (Canberra: Australian National University Press, 1974; Ringwood: Penguin, 1975). The value of oral testimony in reaching the heart of the soldier's experience is promoted by Glynn Harper, "Penetrating the Fog of War: The Use of Oral History in Military Research History," *Oral History Association of Australia Journal* 15 (1993): 38–46; Peter H. Liddle and Matthew J. Richardson, "Voices from the Past: An Evaluation of Oral History as a Source for Research into the Western Front Experience of the British Soldier, 1914–18," *Journal of Contemporary History* 31, no. 4 (1996): 651–674. Another view is presented in Peter Edwards, "Time Distorts Memories," *The Bulletin* (Sydney), 6 July 1993, 46.

8. Hank Nelson, *P.O.W., Prisoners of War: Australians under Nippon* (Sydney: Australian Broadcasting Corporation, 1985). The book is based on an Australian Broadcasting Corporation radio program of the same name, which was produced by Tim Bowden. Reminiscences of Australian POW experiences continue to be published. See the *Weekend Australian Magazine,* 25–26 April 1987, 3–4.

9. John Barrett, *We Were There: Australian Soldiers of World War II Tell Their Stories* (Ringwood: Penguin, 1987), esp. 16–34.

10. For example, the incomparable personal account of the New Guinea campaign by Peter Ryan, *Fear Drive My Feet* (Sydney: Angus and Robertson, 1969; paperback edition issued by Melbourne University Press, 1970); and the reminiscences of a New Zealand soldier whose tours of duty included the Malaya campaign: Frank Rennie, *Regular Soldier* (Auckland: Endeavour Press, 1986).

11. John Keegan, *The Face of Battle* (London: Jonathan Cape, 1976; paperback reprint, Pimlico, 1991), 55. In Australia, to give just one example, the history of war is an appreciable academic industry. Lloyd Robson, "Behold a Pale Horse: Australian War Studies," *Australian Historical Studies* 23, no. 90 (1988): 115–126; Glen St. J. Barclay, "Australian Historians and the Study of War, 1975–88," in *Historical Disciplines in Australasia: Themes, Problems and Debates,* ed. John A. Moses, special issue of the *Australian Journal of Politics and History* 41 (1995): 240–253.

12. Asesela Ravuvu, *Fijians at War, 1939–1945* (Suva: Institute of Pacific Studies, University of the South Pacific, 1974); Neville K. Robinson, *Villagers at War: Some Papua New*

Guinea Experiences in World War II (Canberra: Australian National University, 1981); Hugh Laracy and Geoffrey White, eds., *Taem Blong Faet: World War II in Melanesia,* special issue of *O'o: A Journal of Solomon Islands Studies* 4 (1988); Geoffrey M. White, David W. Gegeo, David Atkin, and Karen Watson-Gegeo, eds., *Bikfala Faet: Olketa Solomon Aelanda Rimembarem Wol Wo Tu/The Big Death: Solomon Islanders Remember World War II* (Honiara/Suva: Solomon Islands College of Higher Education, and Institute of Pacific Studies, University of the South Pacific, 1988); Geoffrey White and Lamont Lindstrom, eds., *The Pacific Theater: Island Representations of World War II* (Honolulu: University of Hawai'i Press, 1989)—see the review article by Hank Nelson, "Turning the Talk of War into History," *Journal of Pacific History* 25, no. 2 (1990): 260–276; Hugh Laracy, "World War II," in *Tides of History: The Pacific Islands in the Twentieth Century,* ed. K. R. Howe, Robert C. Kiste, and Brij V. Lal (Sydney: Allen and Unwin, 1993), 149–169; Peter MacQuarrie, *Strategic Atolls: Tuvalu and the Second World War* (Christchurch/Suva: Macmillan Brown Centre for Pacific Studies, University of Canterbury, and Institute of Pacific Studies, University of the South Pacific, 1994); Leo Scheps, "Chimbu Participation in the Pacific War," *Journal of Pacific History* 30, no. 1 (1995): 76–86. As a by-product of the *Ai Matai* research, Captain Nawadra wrote a pamphlet on the Fijian soldier in the Solomon Islands campaign who was posthumously awarded the Victoria Cross. Tevita R. Nawadra, *Cabobula Nei Kovula Sefanaia Sukanaivalu, V.C., Mawaraka, Bukanivili, 23 June 1944* (Suva: Tabana ni i Tukutuku ni Veigauna Mataivalu ni Viti, 1990). The most recent contributions are Stewart Firth, "The War in the Pacific," in *The Cambridge History of the Pacific Islanders,* ed. Donald Denoon with Stewart Firth, Jocelyn Linnekin, Malama Meleisea, and Karen Nero (Cambridge: Cambridge University Press, 1997), 291–323; and John Garrett, *Where Nets Were Cast: Christianity in Oceania since World War II* (Suva/Geneva: Institute of Pacific Studies, University of the South Pacific in association with World Council of Churches, 1997), 1–147.

13. Some earlier books were more attuned to the Pacific Islanders' experience than others. The superb account of coast-watching activities in the Pacific theater by the man in charge tends to screen out the islanders' role, because the author is writing from the vantage point of the head office in Sydney. Eric A. Feldt, *The Coast Watchers* (Oxford: Oxford University Press, 1946; New York: Bantam Books, 1979). A comparable book was written by a coast watcher himself, and being closer to the action, he is unavoidably more aware of the islanders' contribution. D. C. Norton, *Fire over the Islands: The Coast Watchers of the Solomons* (Wellington: A. H. and A. W. Reed, 1970; London: Lee Cooper, 1975). A New Zealand soldier, who commanded Fijian troops in the Pacific campaign, places similar emphasis on their contribution. See T. E. Dorman, *The Green War* (Christchurch: Claxton Press, 1997). Then there is Richard B. Frank, *Guadalcanal: The Definitive Account of a Landmark Battle* (New York: Random House, 1990; reprinted Penguin Books, 1992), which, despite its subtitle, barely recognizes the islanders' role. It is almost as if the local populations did not exist.

14. R. A. Howlett, *The History of the Royal Military Forces, 1939–1945* (London: Crown Agents for the Colonies, 1948). A follow-up effort was Bruce Adams (photographs) and Robert Howlett (text), *Battlefield South Pacific* (Sydney: A. H. and A. W. Reed, 1970).

15. The volumes specifically referred to are F. L. W. Wood, *The New Zealand People at War: Political and External Affairs* (Wellington: War History Branch of the Department of External Affairs, 1958; reprinted Wellington: A. H. and A. W. Reed, 1971); Nancy M.

Taylor, *The New Zealand People at War: The Home Front*, 2 vols. (Wellington: Historical Publications Branch of the Department of Internal Affairs, 1986).

16. Bean wrote six of the ten volumes of *The Official History of Australia in the War of 1914–1918,* and Morison wrote all fifteen volumes of *History of United States Naval Operations in World War II.* Much admired in his lifetime, Bean was the object of posthumous criticism. John Barrett, "No Straw Man: C. E. W. Bean and His Critics," *Australian Historical Studies* 23, no. 90 (1988): 102–114.

17. Monty Soutar, "28 Maori Battalion Oral History Project," audiovisual presentation at the To Live Is to Learn conference, organized by the National Oral History Association of New Zealand and the University of Waikato, 31 May–1 June 1997. A separate effort on the same subject is Wira Gardiner, *Te Mura o te Ahi: The Story of the Maori Battalion* (Auckland: Reed, 1992).

18. Alan D. Gilbert, John Robertson, and Roslyn Russell, "Computing Military History: A Research Report on the First AIF Project," *War and Society* 7, no. 1 (1989): 106–113.

19. *Fiji Times,* 9 May 1995, 4.

20. This figure is incorrect; and this particular newspaper account (*Fiji Times,* 9 May 1995, 4) contains other inaccuracies. It was reported that 50,000 copies of the book would be printed at a cost of F$61,000 (a unit cost of just over $1!), when in fact the print run was 3,000 and the actual printing costs were F$47,000 (a unit cost of just over $15). I dislike writing about the Fiji print media in this fashion, given the need for an independent press in Third World countries. But its reporting of *Ai Matai* leaves a great deal to be desired.

21. *Fiji Times,* 12 May 1995, 7. The *Daily Post* did not cover the fund-raising, and neither, surprisingly, did the Fiji-language newspapers.

22. Captain Nawadra provided these figures, and the army confirmed their accuracy.

23. See Niko Besnier, *Literacy, Emotion, and Authority: Reading and Writing on a Polynesian Atoll* (Cambridge: Cambridge University Press, 1995), 170–171.

24. Stanley B. Brown, *From Fiji to the Balkans: The History of the Fiji Police Force* (Suva: Fiji Police Force, forthcoming). Taiwan is funding the Lebanon volume to the tune of F$108,000 (*Fiji Times,* 11 October 1996, 3; 18 November 1996, 2).

25. Other official histories of the Malaya Emergency include David Lee, *Eastward: A History of the Royal Navy in the Far East, 1945–1972* (London: HMSO, 1984); Malcolm Postgate, *Operation Firedog: Air Support in the Malayan Emergency, 1948–1960* (London: HMSO, 1992); Colin Bannister, *An Inch of Bravery: 3 RAR in the Malayan Emergency, 1957–59* (Canberra: Directorate of Army Public Affairs, 1994); Peter Dennis and Jeffrey Grey, *Emergency and Confrontation: Australian Military Operations in Malaya and Borneo, 1950–1966* (Sydney: Allen and Unwin in association with the Australian War Memorial, 1996). Chris Pugsley is writing the official New Zealand volume. A soldier's textbook is *The Conduct of Terrorist Operations in Malaya,* 2d ed. (Kuala Lumpur: HQ Malaya

Command, 1954). Nonofficial accounts include Oliver Crawford, *The Door Marked Malaya* (London: Rupert Hart-Davis, 1958); Brigadier Richard Clutterback, *The Long, Long War: The Emergency in Malaya, 1948–60* (London: Cassell, 1966; New York: Praeger, 1966); Richard Stubbs, *Hearts and Minds in Guerilla Warfare: The Malayan Emergency, 1948–1960* (London: Lieutenant-Colonel Osprey, 1982); E. D. Smith, *Counter-Insurgency Operations,* vol. 1: *Malaya and Borneo* (London: Ian Ward, 1985); Brigadier Neil C. Smith, *Mostly Unsung: Australia and the Commonwealth in the Malaya Emergency, 1948–1960* (Melbourne: N. C. Smith, 1989).

26. R. A. Howlett, *Na Kedra Tukutuku Veimataivalu ni Viti, 1939–1945* (Suva: Government of Fiji, 1953).

27. W. David McIntyre, *New Zealand Journal of History* 28, no. 1 (1994): 102. The experience of the New Zealand volumes on the Korea War indicates how difficult it can be to predict accurately the sales potential of a book. Five hundred copies for volume 1, which concerned diplomacy and strategy, seemed reasonable, but as soon as the book was shortlisted in the New Zealand Book Awards, it immediately sold out. The book was out of print before the academic reviews appeared, which meant that potential sales were lost, especially to libraries. Volume 1 was reprinted in 1996, and over half the copies had sold by the end of the year. The print run of volume 2, on combat operations, was 1,250, of which 741 had been presold by the book launch in October 1996. Given that there are about 5,000 veterans, the print run again seems a bit low—but only time will tell. Ian McGibbon, *New Zealand and the Korean War,* 2 vols. (Auckland: Oxford University Press in association with the History Branch, Department of Internal Affairs, 1992 and 1996).

28. Richard Broom, Tony Dingle, and Susan Priestly, "The History of Victoria Project: Looking Back," *Historical Studies* 21, no. 84 (1985): 413–414. See also John Murphy, "The New Official History," *Australian Historical Studies* 26, no. 102 (1994): 119–124.

29. The reburial in the military cemetery at Reservoir Road took place amidst a blaze of publicity. *Fiji Times,* 9, 10, 11, 12 November 1993 (all front-page reports); *Daily Post,* 12 November 1993, 1, 4–5; *Volasiga,* 13 November 1993, 13.

30. As in the case of the Australian official histories of the two world wars. See Robert O'Neill, "Australia in the Second World War," *Historical Studies* 16, no. 64 (1975): 456. Whether this ethic still prevails has recently been disputed. *Weekend Australian,* 2–3 May 1992, 25; 9–10 May 1992, 14. The same liberality applied to Britain's official *History of the Second World War,* but there were restrictions of an editorial nature. In the "Home Series," reference to personalities and the way in which individual decisions led to various solutions were disallowed—to the regret of at least one of the authors. M. J. Tucker, "Joel Hurstfield: Historian for All Seasons," in *Recent Historians of Great Britain: Essays on the Post-1945 Generation,* ed. Walter L. Arnstein (Ames: Iowa State University Press, 1990), 39.

31. The *Fiji Times,* 20 January 1996, 3, reported that the book took eighteen months to complete. What the reporter meant to say was that the vetting committee under Colonel Mate's chairmanship took eighteen months to do its work.

32. For a different view, see generally Michael Howard, *The Causes of Wars and Other Essays* (London: Unwin, 1984), and specifically Stephen Garton, *The Cost of War: Australians Return* (Melbourne: Oxford University Press, 1996), vii–ix.

33. [Thorpe and Savu], *Defending Fiji,* 10. For contrasting views, see Lal, *Broken Waves,* 120–125; Ravuvu, *Fijians at War,* 8–10.

34. In New Zealand, for example, F. L. W. Wood had the benefit of very capable research assistance from five of his former graduate students when researching the *Political and External Affairs* volume during the 1950s (see preface and p. 385). The manuscripts that one of them wrote for Wood's use were later published as books in their own right. M. P. Lissington, *New Zealand and the United States, 1840–1944* and *New Zealand and Japan* (both published Wellington: Government Printer, 1972). Captain Nawadra asked for an assistant of recent-graduate standing, partly to take some of the pressure off himself and partly to develop that person's skills, but without success.

35. Jock Phillips, "Our History, Our Selves: The Historian and National Identity," *New Zealand Journal of History* 30, no. 2 (1996): 107.

36. Deryck Scarr, *Ratu Sukuna: Soldier, Statesman, Man of Two Worlds* (London: Macmillan Educational, 1980); Scarr, ed., *Fiji: The Three Legged Stool: Selected Writings of Ratu Sir Lala Sukuna* (London: Macmillan Educational, 1983). Both volumes were published in association with the Ratu Sir Lala Sukuna Biography Committee and were almost entirely funded by public subscription.

37. Kamasese Mara, *The Pacific Way: A Memoir* (Honolulu: Center for Pacific Islands Studies, University of Hawai'i; Pacific Islands Development Program, East-West Center; University of Hawai'i Press), xv. It is a delightful irony that mainland China provided financial backing for Ratu Mara's memoirs while Taiwan is sponsoring the Lebanon peacekeeping volume. I have always believed in taking the devil's money to do the Lord's work.

38. Doug Munro, "The Isolation of Pacific History," in *Reflections on Pacific Islands Historiography,* special issue of *Journal of Pacific Studies* 20 (1996): 58.

39. The breakdown and discussion of the costs of a major historical project are provided by Broom, Dingle, and Priestly, "The History of Victoria Project," 419–420.

EDITOR'S FORUM

SACRED TEXTS AND INTRODUCTORY TEXTS: THE CASE OF MEAD'S SAMOA

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A survey of 118 introductory anthropology textbooks published in the period 1929–1990 examines the ways in which Margaret Mead's *Coming of Age in Samoa* has been presented to college undergraduates. In contrast to Derek Freeman's claim that her conclusions about Samoan sexuality and adolescence have been reiterated (approvingly) in an "unbroken succession of anthropological textbooks," it appears that this work has been ignored almost as often as it has been cited. Criticisms of Mead, although relatively few and almost entirely methodological, have also been incorporated into textbooks, both before and following Freeman's 1983 book, *Margaret Mead and Samoa.* Whether or not Mead has been a "holy woman" in American cultural anthropology, *Coming of Age in Samoa* does not appear to have been a "sacred text."

Since the publication of Derek Freeman's *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth* (1983b), anthropologists and others have been wrestling with the issue of how Samoa is to be represented to the world—whether in terms of Mead's ethnography, that of Freeman, or some others' accounts. The "Mead-Freeman Controversy" soon extended to broader questions, of course, and the number of interlocutors is by now legion. But clearly we are not finished with the debate. While Paul Shankman has declared that by "1984 almost everything had been said," he had to confess that as of 1987, "almost nothing had been resolved" (1987: 498). As if to demonstrate that in fact not everything *had* been said, in 1988

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a film appeared promising that the "controversy . . . [would] be resolved by startling new evidence presented in [the] program" (Heimans 1988), and much more recently Orans (1996) has gone to Mead's field notes for edification, just as Shankman (1996) has examined critically Freeman's Samoan data.

I should make it clear at the outset that I do not here attempt to resolve the controversy: I am not concerned (at least in this essay) with whose representation of Samoa is "correct." If neither Samoans nor Samoanists can fully agree, it is unlikely that I have any answers.

Instead, I consider here one of the many issues about which contrary views have been expressed during the debate, namely, the degree to which Margaret Mead's representation of Samoan society and culture, especially as it was set forth in *Coming of Age in Samoa* (Mead 1928; hereafter, *COAS*), has in fact been uncritically received and canonized by anthropologists. According to Freeman, for more than sixty years Mead's "conclusions" (and particularly those regarding adolescence and premarital sexual behavior) had never been seriously challenged but were simply repeated endlessly, thus perpetuating the "myth" he has come forth to "unmake." On the other hand, numerous anthropologists have claimed that the flaws in her earlier work have long been recognized and that *COAS* has never been accorded much credibility. About this matter, at least, can we say who is right?

The Making of a Classic

In expressing his distress over the uncritical acceptance of Mead's portrait of Samoa, Freeman attributes the acquiescence initially to "the climate of the times," but subsequently to ignorance or the suppression of contrary evidence, and finally to the sheer weight of Mead's reputation and influence. Thus, "when Mead depicted the Samoans as a people without jealousy, for whom free lovemaking was the pastime par excellence, and who, having developed their emotional lives free from any warping factors, were so amiable as to never hate enough to want to kill anybody, *no anthropological or other critic,* in the fervid intellectual climate of the late 1920s, seriously questioned these extravagant assertions" (Freeman 1983b:95–96; emphasis added). The consequence was that by 1939 "the example of Samoa had become duly incorporated into the literature of the social sciences" (ibid.:101).

In Freeman's view, Samoan fieldwork conducted by others did not result in "corrective" accounts, as he clearly thinks it should have. Rather, during "the 1950s . . . Mead's conclusion about adolescence in Samoa came to be regarded as a proven fact"; such an "uncritical acceptance of Mead's conclusions in centers of higher learning in both Europe and America could occur because none of the anthropologists who had published the results of research

undertaken in Samoa subsequent to Mead's expedition of 1925–1926 had in any way questioned her findings" (ibid.:103).

Continuing with Freeman's scenario, things should have changed after the mid-1950s, but new ethnographic reports only served to support Mead. Lowell Holmes, in 1954, went to Samoa specifically to do a "restudy" of Ta'u, the site of Mead's earlier fieldwork. Holmes's resulting doctoral thesis (1957) and monograph (1958) included "numerous facts widely at variance with the picture Mead had given" and provided "substantial grounds for seriously questioning the validity of Mead's picture of Samoa" (Freeman 1983b: 104). However, again Freeman cites the prevailing *Zeitgeist* as a deciding factor: "given the intellectual climate of the mid 1950s in the department [at Northwestern University]," Holmes did not challenge Mead's "central conclusion" in *COAS*, as Freeman seems to think he should have been obliged to do on the basis of his own evidence, but "gave it as his opinion that the reliability of Mead's account of Samoa was 'remarkably high' " (ibid.:105). Thus, "with Holmes's apparent confirmation of its findings, *Coming of Age in Samoa* came to be regarded more widely than ever before as a classic of American cultural anthropology, and by the 1960s it had become the most widely read of all anthropological books" (ibid.).

But if Holmes was reluctant to draw what to Freeman was an inevitable conclusion, others' work led Mead herself, in an appendix to a new edition of *Social Organization of Manu'a* (1969), "to admit the 'serious problem' of reconciling the contradiction between her own account of Samoa and other records of historical and contemporary Samoan behavior" (Freeman 1983b: 107). Nevertheless, according to Freeman, by the 1970s "such was [Mead's] prodigious reputation that . . . *Coming of Age in Samoa* continued to be accepted by the vast majority of anthropologists as presenting an accurate picture of the Samoan ethos as it had been in the 1920s" (ibid.).

Thus, for Freeman, right up to the time of his "unmaking" of the "myth," *COAS* seemed immune to doubts or criticisms, which were too few and far between and "not substantial enough . . . to affect significantly the general acceptance of Mead's conclusion of 1928" (Freeman 1983a:169 n. 2). Instead, the book "has come to be accepted as a scientific classic, and its conclusions continue to be regarded by anthropologists and others as though they were eternal verities" (Freeman 1983b:108).

But Who Reads Classics?

According to its publishers and numerous journalists, *COAS* has sold millions of copies since its publication in 1928, the book reportedly has been translated into "16 languages, including Urdu and Serbo-Croatian" (McDowell

1983:C21), and it has become a cliché to refer to the "countless" undergraduates to whom the book has been assigned. Citations and quotations from *COAS* are abundant in the general scholarly literature, as Freeman illustrates for works of social criticism and philosophy (1983b:97–102), and its influence is also evident in books dealing with adolescent psychology (e.g., Bandura 1964:8; Conger 1973:14–15; Crow and Crow 1965:41; Fitch 1985:258–259; Muus 1968:68–87), where Samoa is often used as a type case of the purported cultural basis of adolescent turmoil. And in anthropology, the following quote surely makes the book sound like a classic: "Once in a great while, a study of one society profoundly challenges existing conceptions of human development and forces a reevaluation of basic assumptions concerning the range of normality for all humans. One thinks of Margaret Mead's *Coming of Age in Samoa* . . . as [an example] of research conducted in a single cultural setting that compelled revision of generalizations about adolescence and sexual development for the species as a whole" (LeVine 1981:ix).

And yet, when reports of Freeman's book first appeared in the mass media, journalists had no difficulty in finding anthropologists to quote who gave a very different view of Mead's work, and especially of *COAS* (see Begley, Carey, and Robinson 1983; Fields 1983; Leo 1983; Wilford 1983a). Ward Goodenough spoke for many in saying that the "lay public is now learning what professional anthropologists have long known about the quality of her early ethnographic fieldwork, done in her youth" (1983:906–907). Those trained in British traditions have been said to have long denigrated her Samoan fieldwork as an example of how not to conduct proper research and considered *COAS* as "the rustling-of-the-wind-in-the-palm-trees kind of anthropological writing" (Evans-Pritchard 1962:96; see also Douglas 1983: 760; Hsu 1980:349; Jarvie 1983:84; Langness 1985:17; Leach 1983:477). But even American cultural anthropologists, whom Freeman continually accuses of being uncritical of Mead, have casually noted the deficiencies of her early work and Mead's penchant for overstatement and simplification, claiming as well that these flaws have been recognized "for decades" (Bock 1983:336; see also Basham, quoted in Wilford 1983b; Clifford 1983; Glick 1983a:13; Harris, quoted in Wilford 1983b; Kuper 1989:453; Langness 1985:17–18; Rappaport 1986:329; Romanucci-Ross 1983:90–91; Schneider 1983:4; Shore 1983:936–937).

Faced with such statements, Joy Pratt, director of publicity at Harvard University Press (publishers of Freeman's book), responded: "People say it's been known in the field for years that Mead made mistakes in interpreting Samoa, but the general public hasn't known that" (quoted in Fields 1983: 28)—a claim to which one anthropologist replied: "That it is news to the *New York Times, Time, Newsweek,* and *Science* only indicates that these publications do not always report all the news" (Schneider 1983:4).

Had there in fact been such "news" for them to report prior to Freeman's book? Shankman has claimed that "criticism had been circulating since the first academic reviews of *Coming of Age in Samoa* appeared in 1929" (1983: 40). This, too, is not widely known, since upon its publication *COAS* did receive "rave" reviews in the popular press (e.g., Brooks 1928; Kirchwey 1928; *New York Times* 1928) as well as some extremely favorable notices from anthropologists (Benedict 1928; Driberg 1929). In an advertising blurb, none other than Malinowski called it an "absolutely first-rate piece of descriptive anthropology, an excellent sociological comparison . . . an outstanding achievement" (1928).

But there were detractors. Writing for general audiences, Anderson was "undecided whether it has been a contribution to science or art. . . . If it is science, the book is somewhat of a disappointment. It lacks documental base" (1929:514). Another reviewer challenged Mead's credentials and conclusions regarding the reputedly low rates of neuroses among Samoans (Johnson 1929). As for anthropologists, Lowie offered a mixed review, praising "her solid contributions to ethnographic fact and method" but also expressing some "skepticism," finding it "hard to understand how certain conclusions could have been arrived at" (1929:533, 534). And Redfield subtly suggested criticism through rhetorical questions (1929:729), finishing his review with what surely is an unusual complaint, that "Miss Mead is interested, one feels, in problems and cases, not in human nature. There is no warmth in her account" (ibid.:730).

Nor have criticisms been limited to a handful of book reviews. Over the ensuing decades, some of these appeared in the scholarly literature (e.g., Ausubel 1954:254, 403–404; Barnouw 1985:94–100 [and earlier editions]; Campbell 1961:340; Harris 1968:409–412; Raum 1940:42–43, 293–294; Winston 1934), but these were unlikely to have been noticed by the general public. Surely, if anthropologists "have known for decades" that Mead's Samoan work, or at least *COAS*, was "problematic," they seem to have done little to counter publicly her growing popularity and respect in the wider community. One is tempted to agree with Langness that "Whether Mead was right or wrong, . . . the whole world was in collusion with her in one way or another. . . . Margaret Mead did not become famous in a vacuum. She did not become famous merely by her own efforts. She had a lot of help. . . . Everyone was in it together" (1985:17).

And so we have what Langness calls "a most curious situation" (ibid.). Freeman has complained that *COAS*, with its "distorted" representations of Samoa, has been incorporated uncritically for decades into American cultural anthropology, yet numerous of its practitioners claim that during those same decades it was widely discredited, or at least ignored. Thus George Marcus has opined "that outside of introductory courses, her work has not

generally been read in recent years" (quoted in Fields 1983:28), bringing to mind Mark Twain's definition of a classic as a book that everybody wants to have read, but nobody wants to read. Is *COAS* such a "classic"—"the most widely read of all anthropological books" (Freeman 1983c), but not by anthropologists?

Students Read Classics—Don't They?

Perhaps, when considering how peoples and their societies and cultures have been represented, it is more important in any case to focus on non-anthropologists' perceptions—they are, after all, in the demographic majority. To get some sense of what these perceptions are likely to have been, we must first acknowledge that the general public, the presumed purchasers (and readers) of those "millions of copies" of *COAS,* has been heavily exposed to Mead's view of Samoa thanks to aggressive marketing by publishers and, perhaps more significantly, by college professors. Many have noted that the book has been "assigned to countless undergraduates" (e.g., Glick 1983b: 758). Unfortunately, I know of no way to determine the degree to which this has in fact been the case.

However, there is another major way in which representations of other peoples are transmitted, and that is through book excerpts, summaries, or discussions incorporated into introductory textbooks. Indeed, in recounting the "damage" that has been done, Freeman has complained that the "conclusions" of *COAS* "have been given very general credence . . . within the anthropological profession (having been repeated in numerous textbooks)" (1983c). By examining such sources we may gain a new perspective on the contrary claims about anthropologists' acceptance of Mead's Samoan work. No matter what anthropologists may privately believe, or what a few academics might publish in scattered, obscure critiques, such demurrals might count for little or nothing if Freeman is correct in saying that Mead's "unqualified dogma, which gets a fundamental matter fundamentally wrong, was repeated [not only] in millions of copies of *Coming of Age in Samoa* [but] as well . . . in an unbroken succession of anthropological textbooks, and [thus] given very widespread professional and popular credence" (1987:392).

Thus we come to the main concern of this article: How has Mead's representation of Samoa been treated in introductory anthropology textbooks since the publication of *COAS?* To what extent has it been cited at all, and has it been uncritically canonized through such citations? Can changes be seen over time as other Samoanists have published their reports or as critiques have appeared? And, finally, how has the "Mead-Freeman Controversy" itself been remarked on in textbooks?

A Survey of Introductory Textbooks

A total of 118 introductory textbooks in anthropology (readily available to me from colleagues and libraries) with publication dates spanning the time period 1929–1990 were surveyed.[1] Although this opportunistic sample does not represent all such textbooks published during that period, I believe it is both large enough and representative enough to draw meaningful conclusions. Primarily using the textbooks' bibliographies and indexes, but in many cases by skimming the entire book, all passages referring to Margaret Mead, Samoa, or both were located and extracted. The following represents an analysis of those extracts.

As can be seen in Table 1, with respect to overall citations, Margaret Mead's Samoan work is *not mentioned* in 54 (45.8%) of the texts (in 10 of these, Samoa is used in some way for illustrative purposes but Mead's work there is not cited). Thus, Mead's publications on Samoa (which in virtually every case means *COAS*) are cited to some degree in 64 (54.2%) of the texts.[2] In five of these, references are made to her findings regarding the political system or allocation of child-minding tasks, but not to Samoan adolescence or sexuality. Only 60 (50.8%) of the sample texts, then, have cited Mead on the two most-controversial topics—far from the "unbroken succession of anthropological textbooks" that has so exercised Freeman. Nor have all of these citations been mere reiterations of Mead's "unqualified dogma." Of the 60 texts in which her views on Samoan adolescence or sexuality are cited, in 50 this has been done in an uncritical or unqualified manner.[3] While these represent a clear majority (83.3%) of all citing texts, they constitute less than half (42.4%) of the 118 sample textbooks published over a sixty-two-year period.

It should be noted that these figures represent numbers and percentages of textbooks that, given the time span considered, include multiple editions

TABLE 1. **Citations of Mead in Introductory Textbooks, 1929–1990**

	Texts (N = 118)	Authors (N = 77)
No mention of Mead's Samoa work	54 (45.8%)	38 (49.4%)
Citation of Mead on Samoa but not on sexuality or adolescence	4 (3.4%)	3 (3.9%)
Mead cited on Samoan sexuality and/or adolescence	60 (50.8%)	36 (46.8%)
Uncritically	50	30
Critically	10	6

by given authors. When only distinct (senior) authors are counted, we find that the overall statistics do not differ greatly from those for the texts. As is shown in Table 1, 36 (46.8%) of the 77 different textbook (senior) authors cited Mead's Samoan work on adolescence or sexuality, with 30 (83.3%) of those authors doing so uncritically.

Thus, during the decades since the publication of *COAS,* Mead's representation of Samoan adolescence and sexuality has been incorporated into about half (50.8%) of the textbooks published by slightly less than half (46.8%) of textbook authors. It has been presented to students uncritically or in an unqualified way in 50 (42.4%) of the 118 textbooks, by 30 (39.0%) of their 77 (senior) authors. In summary, when *COAS* has been cited, it has been presented more often than not as authoritative, but mentions of it have been absent in virtually as many texts as they have been present, and a slight majority (53.2%) of textbook authors have chosen not to cite it at all.

These figures, however, do not necessarily represent the numbers or percentages of students who have been presented with Mead's view of Samoa, since not all texts have equal readerships. Absent sales figures or other comparative data, we might assume that those texts that have appeared in multiple editions have done so because they were "successful," that is, adopted widely and purchased by large numbers of students. In the sample, multiple-edition (or "successful") textbooks constitute 66 of the total of 118 and represent the work of 25 (senior) authors.

As can be seen in Table 2, the "successful" textbooks have tended to include citations of *COAS* slightly more often than is true for the sample as a whole (56.1% versus 50.8%), and once again the majority of citations (28 of 37, or 75.7%) are uncritical or unqualified. But many of the "successful" texts do not mention Mead's Samoa work at all, and some of the citations are critical or qualified. Thus, so far as the subsample of "successful" texts is con-

TABLE 2. **Citations of Mead in "Successful" Introductory Textbooks**

	Texts (n = 66)	Authors (n = 25)[a]
No mention of Mead's Samoa work	25 (37.9%)	12 (48.0%)
Citation of Mead on Samoa but not on sexuality or adolescence	4 (6.1%)	2 (8.0%)
Mead cited on Samoan sexuality and/or adolescence	37 (56.1%)	14 (56.0%)
Uncritically	28	10
Critically	9	4

[a] Subtotals exceed 25 due to authors' changes over editions.

cerned, the frequency of uncritical or unqualified citations is in fact identical, at 42.4% (28 of 66), to that for the total sample (50 of 118, also 42.4%).

Similarly, while the situation is complicated slightly by the fact that three of the "successful" authors changed their treatment of Mead in subsequent editions,[4] it can also be seen in Table 2 that "successful" authors cited Mead on Samoan sexuality or adolescence in at least one edition of their texts somewhat more than did the sample authors as a whole (56.1% versus 50.8%), but those citing her uncritically on at least one occasion represent only 40.0% (10 of 25) of the "successful" authors, which compares with 39.0% (30 of 77) of all authors.

Just as multiple-edition texts presumably are used by more students than are those that go out of print after a first edition, there are texts that can be considered especially "successful," that is, they have appeared in three or more editions (up to six for the current sample). These texts warrant special attention since, regarding potential impact on students' views of Samoa, these have probably been the most influential. Among the 25 "successful" authors (with their 66 texts in the sample), 12 may be identified thus as "best-selling" authors (accounting for 40 of the 66 multiple-edition texts). The relevant figures are provided in Table 3.

With these "best-sellers," significant differences do appear. A substantially higher percentage of "best-selling" textbooks cited Mead on Samoan sexuality or adolescence (70.0%) than is true for either the "successful" textbooks (56.1%) or the sample as a whole (50.8%). This is also true so far as the distinct authors are concerned: 75.0% of the "best-selling" authors cited her in at least one edition, compared with 56.0% of the "successful" authors and 46.8% of all authors. Moreover, Mead's Samoan work was cited uncritically or in an unqualified way in at least one edition of 55.0% (22 of 40) of the "best-selling" textbooks by 58.3% (7 of 12) of the "best-selling" authors; these

TABLE 3. **Citations of Mead in "Best-Selling" Introductory Textbooks**

	Texts (n = 40)	Authors (n = 12)[a]
No mention of Mead's Samoa work	12 (30.0%)	4 (33.3%)
Citation of Mead on Samoa but not on sexuality or adolescence	0 (0.0%)	0 (0.0%)
Mead cited on Samoan sexuality and/or adolescence	28 (70.0%)	9 (75.0%)
Uncritically	22	7
Critically	6	2

[a] Subtotals exceed 12 due to authors' changes over editions.

figures contrast with 42.4% of the "successful" textbooks by 40.0% of the "successful" authors, and 42.4% of the textbooks by 39.0% of the authors in the sample as a whole.

If these crude indicators of "best-selling" and "successful" authors and textbooks can be considered to correspond to likely proportions of introductory-course student readers, it seems a reasonable inference that over a sixty-two-year period, a majority of introductory anthropology students *have* been presented—at least in their texts—with uncritical or unqualified reiterations of Mead's representation of Samoan adolescence or sexuality. But it is not a large majority and, of course, there is no way to determine what other sources or views such students were exposed to in other reading assignments or class lectures.

The Fate of a "Classic" over Time

The above analysis indicates that Freeman has exaggerated the degree to which *COAS* has been incorporated uncritically into "an unbroken succession of anthropological textbooks" (1987:392), if only because that succession has been broken by Mead's Samoa work having received no attention of any kind in 54 (45.8%) of the 118 texts surveyed, including 25 (37.9%) of the "successful" (multiple-edition) ones and 12 (30.0%) of the "best-sellers" (appearing in three or more editions). Moreover, *COAS* and its conclusions have been presented with qualifications or acknowledging criticisms in 10 (16.7%) of the texts that have cited them at all, including 9 (24.3%) of the 37 citing "successful" textbooks and 6 (21.4%) of the 28 citing "best-sellers." Thus, if my sample is representative, large numbers of introductory students have learned from their textbooks that Mead's representations of Samoa are not necessarily considered accurate, and even larger numbers have not learned of her views at all.

However, when we refine the analysis by examining the fate of *COAS* in textbooks over time, Freeman's complaint (1983a:169 n. 2) that criticisms of it have been few and far between and "not substantial enough . . . to affect significantly the general acceptance of Mead's conclusion of 1928" may be more accurate, whether ultimately justified or not.

The analysis so far has considered the textbook sample in the aggregate, covering a considerable time span of more than sixty years. If, as Freeman contends (1983b:104), Lowell Holmes's doctoral thesis and monograph on Ta'u indeed included "numerous facts widely at variance with the picture Mead had given" and provided "substantial grounds for seriously questioning the validity of Mead's picture of Samoa," then we might expect—if textbooks reflect to any significant degree rethinking that occurs in the profes-

sion, as well as "new findings"—that Holmes's work would have had a significant impact on the treatment received by *COAS* in subsequent years. If that were not enough (for whatever reasons), then surely Freeman's own book, which escaped the notice of hardly anyone within or outside of the profession, must have had a measurable impact in more recent years.

To address these questions, I have broken down the sample of 118 textbooks by publication date into four subsamples: 1928–1957 (the date of Holmes's thesis), 1958–1974 (1958 being the publication date of his more widely available *Samoan Village*), 1975–1983 (the publication date of Freeman's book), and 1984–1990 (following Freeman's book). The results, shown in Table 4, indicate that neither Holmes's nor Freeman's work has had a significant impact on the treatment of Mead and *COAS* in textbooks.

During the period 1928–1957, only 3 of 13 textbooks (23.1%) cited Mead on Samoan sexuality or adolescence, and none did so critically. The next period saw a substantial increase in the publication of textbooks, with 46 (in my sample) during a seventeen-year period; in these, citations of Mead doubled in frequency (22 texts, or 47.8%) and the first critical remarks appeared, but in only 3 texts (6.5% of the total). None of these texts made use of Holmes's work, but criticized Mead with reference to her "subjective" portrayal of Samoa (F. Keesing 1958:163; repeated in R. M. and F. Keesing 1971:395–396) and her heavy reliance "on her own impressions" (Kottak 1974:416). These same criticisms were made during the next period, 1975–1983, with a slight increase in citations of *COAS* (to 23, or 53.5%, of 43 texts) and a slight numerical increase in critical remarks, in 5 texts (11.6% of the total). Three of these cases, however, are repetitions in new editions of previously made points, (Kottak 1978:214–215, 1982:372; R. M. Keesing 1976), although Roger Keesing, now as a sole author, went somewhat further, saying that Mead "can be seen in retrospect to have greatly overemphasized human plasticity and cultural variability" (1976:519). As authority, he cites a paper by Freeman, but nothing of Holmes's work. The other two texts criticize Mead's methods as being "impressionistic" (Benderly, Gallagher, and Young 1977:223) and "highly impressionistic" (Friedl and Pfeiffer 1977:303).

If Holmes's work—whatever its implications might have been for assessing that of Mead—seems to have been ignored by textbook writers, one could hardly expect the same with Freeman's book, given its notoriety if nothing else. Indeed, in the final period under review here, 1984–1990, while the number of texts published seems to have dropped considerably (to 16, for a seven-year period, compared with 43 in the previous nine years), citations of *COAS* increased substantially, appearing in 12 (75.0%) of the textbooks. Of these, 9 discuss the "Mead-Freeman Controversy," but only 2 do so critically, so far as Mead is concerned. Most of the others simply present the

TABLE 4. **Citations of Mead in Introductory Textbooks by Time Period**

	1928–1957	1958–1974	1975–1983	1984–1990	All
	Texts				
Number	13	46	43	16	118
No mention of Mead's Samoa work	9 (69.2%)	21 (45.7%)	20 (46.5%)	4 (25.0%)	54
Citation of Mead on Samoa but not on sexuality or adolescence	1 (7.7%)	3 (6.5%)	0 (0.0%)	0 (0.0%)	4
Mead cited on Samoan sexuality and/or adolescence	3 (23.1%)	22 (47.8%)	23 (53.5%)	12 (75.0%)	60
Uncritically	3 (23.1%)	19 (41.3%)	18 (41.9%)	10 (62.5%)	50
Critically	0 (0.0%)	3 (6.5%)	5 (11.6%)	2 (12.5%)	10
	Authors				
Number	12	35	38	13	98[a]
No mention of Mead's Samoa work	8 (66.7%)	16 (45.7%)	18 (47.4%)	3 (23.1%)	45
Citation of Mead on Samoa but not on sexuality or adolescence	1 (8.3%)	2 (5.7%)	0 (0.0%)	0 (0.0%)	3
Mead cited on Samoan sexuality and/or adolescence	3 (25.0%)	17 (48.6%)	20 (52.6%)	10 (76.9%)	50
Uncritically	3 (25.0%)	14 (40.0%)	16 (42.1%)	8 (61.5%)	41
Critically	0 (0.0%)	3 (8.6%)	4 (10.5%)	2 (15.4%)	9

[a] The total number of authors is higher than in the aggregate tables because some (senior) authors published text editions in more than one time period.

controversy as an example of how difficult it is to compare accounts of the "same" society from different time periods, seen from different theoretical perspectives, and so on. At least one author is critical of Freeman, dismissing his criticisms of Mead as "more a 'media event' than a scientific discourse; among other things, it misrepresents both her work and anthropology itself" (Haviland 1987:119). Of the two remaining texts, one is critical of both Freeman *and* Mead: "In fact, the Samoans appear to be either

gentle or aggressive, depending on the context, and the writings of both observers seem to reflect their own biases" (Oswalt 1986:101). The other does not explicitly discuss Freeman's points but, with reference to Mead's portrayal of Samoa, suggests that "most anthropologists today would probably contend that both physiological changes and sociocultural factors contribute to problems of adolescence" (Peoples and Bailey 1988:361).

For a final look at the fate of *COAS* and the "Mead-Freeman Controversy" in textbooks, I turn briefly to the three texts that have most recently crossed my desk, one a "successful" text (Robbins 1997, in its second edition) and two "best-sellers" (Ember and Ember 1996, in its eighth edition; and Peoples and Bailey 1997, in its fourth). Put simply, the situation seems to be the same as for the period immediately following the publication of Freeman's book.

In Robbins's text, Samoa does not appear in the index, nor does Freeman. Mead's work is discussed (Robbins 1997:140), but for *Sex and Temperament in Three Primitive Societies*—referred to as "her classic" (ibid.:157)—rather than for *COAS.* In the eighth edition of their text, as in their fifth (1988), Ember and Ember discuss the "Mead-Freeman Controversy," this time giving it "box treatment" as a "current issue" (1996:60–61) under the title "Fieldwork: How Can We Know What Is True?" They do so to raise methodological issues and do not clearly come down on either "side," presumably leaving students to decide what is "true." Peoples and Bailey repeat their earlier (1988) point that "some researchers attribute the problems that arise during the period [of adolescence] primarily to physiological changes, but others consider cultural factors to be the major causes," but in this edition they make the point with explicit reference to Freeman's critique (1997: 312). Yet, in a note to "Suggested Readings" they say of *COAS:* "one of anthropology's great classics. Few anthropological studies have been more widely read by the general public" (ibid.:320).

Conclusions

Given the previously discussed claims by many anthropologists that the weaknesses of Mead's Samoan work "have long been recognized," it is tempting to interpret the omissions of *COAS* from substantial numbers of textbooks as indicators of authors' rejection of its validity. However, absences are notoriously difficult to account for in any simple way, and these are not exceptions to the rule.

Some authors who have not cited *COAS*—especially in the early period (e.g., Paul Radin 1932; Clark Wissler 1929)[5]—have overwhelmingly preferred to use North American Indian or other material for illustrative pur-

poses, and numerous texts in the sample do not refer to Samoa in any context. Similarly, some texts (e.g., Elman Service 1958, 1971) have been organized around a small number of cases, not including Samoa. Other textbooks bear the clear stamp of the theoretical orientations of their authors that might preclude, or at least underplay, "culture and personality" approaches, the rubric under which many citations of *COAS* appear. For example, Marvin Harris does not cite the book in any edition of his text (1971, 1980, 1983), and only one of the four textbooks authored by British anthropologists mentions (and that very incidentally) Mead's Samoan work (John Beattie 1964).

On the other hand, *COAS* is, by many accounts, a "classic," and regardless of regional or theoretical leanings of authors, it is an extremely rare introductory textbook that does not include at least a mention of Malinowski's work in the Trobriand Islands and his "classic," *Argonauts of the Western Pacific* (see note 2 above). Moreover, in many cases (such as Harris's), Mead's *Sex and Temperament in Three Primitive Societies* (1935) —another often-claimed "classic" of hers—is cited and discussed (usually uncritically, but that is another paper) while *COAS* is ignored.[6]

Of course, Freeman appears to be concerned mainly with the "enshrinement" of *COAS* in the anthropological profession as a whole, and in the present context it is reasonable to ask to what extent textbook treatments of Samoa, or any other case, are representative of the views of professional anthropologists. Certainly some of the textbook authors have been major figures in the discipline and might be regarded as opinion makers, if not reflectors; for earlier decades, one notes Alexander Goldenweiser (1937), Melville Herskovits (1948), Alfred Kroeber (1923, 1948), Ralph Linton (1936), Robert Lowie (1934, 1940), George P. Murdock (1934), Paul Radin (1932), and Clark Wissler (1929). Of these eight authors, only three—Goldenweiser, Herskovits, and Murdock—cited Mead (all uncritically) on Samoan adolescence or sexuality. Even with these authors, though, it should be noted that Herskovits dropped his citation in the second edition of his book (see note 4 above), and Murdock used *COAS* and *Social Organization of Manu'a* as only two of thirty-three sources drawn upon for his summary description of Samoa.

Whatever may have been the case with these early leaders in the field, it seems fair to say that most textbooks have been written by "everyday" sorts of anthropologists or, at any rate, by those who do not see the introductory textbook as a forum for major theoretical statements or iconoclasm. They have probably tried to present material, and present it in such a way, that it resonates rather than grates. And surely textbook publishers would be more interested in manuscripts that do not differ too much from the interests and

views of potential adopters (that is, teaching anthropologists). Thus one might expect a degree of "leveling" to occur in the textbook business, with most textbooks, or at least the "successful" ones, representing "middle of the road" views and interpretations.

But, for those who have lived through the "Mead-Freeman Controversy" this stance may represent the general views within the profession. Certainly the controversy generated much heated discussion, but for many anthropologists it has seemed "much ado about nothing." Considerable attention has focused on extreme positions, but these have in fact been taken by few. What marks many of the discussions instead is ambivalence, with points scored on "both sides." Thus, so far as textbooks are concerned, we might expect a small number of texts to include *COAS* with high praise and some to do so critically; but for most of the potential audience, perhaps the wisest course would be to present Mead's work in much the same way as others'—as "findings," which are neither extolled nor condemned. Or, to be on "the safe side," one could ignore it altogether. This, I suggest, is precisely the pattern I have found in my survey.

Given the prominence of the controversy in recent years, it might seem that the last option—that of omitting discussion completely—has been increasingly difficult to adopt by a textbook author. Yet, as we have seen, in the period just following the publication of Freeman's book (1984–1990), one-fourth of the textbooks surveyed did precisely that, as did one of the three 1996–1997 texts examined. And, for the period 1929–1990 taken as a whole, *COAS* and its "conclusions" have been incorporated uncritically into a substantial number of textbooks (42.4%), but it also has been ignored (at least with respect to Samoan sexuality and adolescence) by even more (49.2%). So, Freeman was correct in saying that Mead's representation of Samoa in *COAS* has "been repeated in numerous textbooks" (1983c), but not in an "unbroken succession" (1987:392). And he also appears to have been correct in complaining that—in textbooks, at least—criticisms have been few and "not substantial enough . . . to affect significantly the general acceptance of Mead's conclusion of 1928" (Freeman 1983a:169 n. 2). It seems that the publication of Holmes's work, which Freeman says included "numerous facts widely at variance with the picture Mead had given" and provided "substantial grounds for seriously questioning the validity of Mead's picture of Samoa" (1983b:104), had no effect on textbook writers. What criticisms have appeared have been addressed primarily to methodological issues, beginning only in the late 1950s when, perhaps, anthropology as a whole was becoming more concerned with its "scientific" status.

Freeman surely must be disappointed that the publication of his book has not brought about a sea change in the representation of Samoa in textbooks,

with nearly one-half of the 16 texts published in 1984–1990 including no mention of Mead's Samoa (four) or the controversy (another three), as is also the case for one of the three 1996–1997 texts examined. But this in itself can be seen as a continuation of a long-standing pattern: frequent incorporation of Mead's views, coupled with frequent indifference to them—not the behavior one might expect from a profession in the thrall of a "holy woman" (Rappaport 1986). Whatever the divine status of Mead might be, and the reasons for it, *Coming of Age in Samoa* has not been a sacred text.

NOTES

I am grateful to the three anonymous readers of an earlier version of this article for their criticisms, which have been helpful in improving it.

1. A complete listing of these textbooks is available from the author.

2. It may be of interest to compare this citation frequency with those of certain other "classic" Pacific ethnographies from roughly the same time period. Using a slightly different sample (for other purposes) of 112 textbooks published between 1940 and 1990, I found that Spencer and Gillen's *The Arunta* (1927)—the most often cited Australian Aboriginal ethnography—was used in 48 (42.9%) of those texts, somewhat less than *COAS* in this sample but to the same degree as another of Mead's "classics," *Growing Up in New Guinea* (1930), also cited in 48 texts. None of these even approaches, however, the *true* "classic," Malinowski's *Argonauts of the Western Pacific* (1922), cited in 109 (97.3%) of the 112 texts!

3. In my analysis, I consider an "uncritical" or "unqualified" citation of Mead's work one that uses laudatory phrasings or refers to *COAS* as a "classic" study, or one that simply paraphrases her conclusions without evaluative language. An example of the former is the text by Swartz and Jordan (1976:24–25):

> In her justly famous 1928 book, *Coming of Age in Samoa,* [Mead] examines the popular notion that it is part of human nature for adolescence to be a time of emotional tension and conflict. . . . She found . . . that adolescence in Samoa does not entail the upheavals that it does in urban American society and that therefore adolescent stress is a culturally determined phase in individual personality development rather than one rooted in the basic nature of humanity.

The second type is illustrated by Hiebert (1976:435):

> But how far can a culture bend and shape a person? To what extent is a child an untouched plastic form on which culture stamps its image, and to what extent is it a product of genetic codes and biologically based learning processes?
>
> . . . One of the first anthropologists to be concerned with this problem was Margaret Mead. . . . She found (1928) that the adolescent years were not particularly difficult for Samoan girls.

"Critical" or "qualified" citations include those that suggest weaknesses in Mead's work due to its "impressionistic" qualities or otherwise refer to criticisms of it, for example, Kottak (1974:416):

> Mead's hypothesis was that the psychological changes associated with puberty here were not inevitable but were culturally conditioned. She found, as anticipated, that Samoan adolescence was a relatively easy period; sexual frustrations imposed in American society were absent. Other studies conducted subsequently in the Pacific, and employing psychological instruments unavailable to Mead in 1925, suggest that sexual freedom does not always guarantee the absence of frustrations and anxieties in adult life.
>
> Like other early culture and personality researchers, Mead relied heavily on her own impressions of the feelings and emotions of Samoan girls. . . . She eschewed use of statistical data, so that the ratio of normal to deviant behavior cannot be established. . . . Different approaches to culture and personality research, including a more "scientific" one, are discussed later.

Or, providing less detail, Keesing (1976:519):

> Margaret Mead's classic studies of adolescent girls in Samoa and of sex roles in New Guinea can be seen in retrospect to have greatly overemphasized human plasticity and cultural variability.

4. Among the "successful authors" are Friedl, Herskovits, and Howard. In his text coauthored with Pfeiffer, Friedl criticized the "impressionistic" and "subjective" nature of Mead's work (Friedl and Pfeiffer 1977:302–303); later, he noted the Mead-Freeman controversy and concluded that "it appears that Mead's work will successfully withstand Freeman's attack" (Friedl and Whiteford 1988:94). Herskovits, in *Man and His Works,* cited *COAS* as demonstrating that "puberty crises" are "culturally, not biologically determined" (1948:44), but in his later abridgement of the book (1964), there is no citation of Mead's Samoa work. And Howard, writing with McKim (1983), discussed the Samoan political system but did not mention Mead's work there; later, writing alone (1986, 1989), he incorporated a discussion of the Mead-Freeman controversy.

5. When not actual quotations, citations refer to the separate bibliography of textbooks.

6. In this context it is interesting to note the results of a smaller comparative survey of 27 introductory sociology textbooks (complete listing available from the author). Of these, 20 (74.1%) cited or quoted the work of Margaret Mead, but in only one case was *COAS* mentioned. It was cited as "the classic study of adolescence in premodern societies" and the conclusion was simply stated that "the Western model is not universal. Samoan adolescents moved fairly smoothly into adult roles and did not suffer the many self-doubts of adolescents in the West" (Popenoe 1986:138–139, 139). Despite the publication date, no mention is made of Freeman's book or the controversy. Compared to this relative neglect of *COAS,* it is striking that 15 (55.6%) of the 27 sociology texts cite Mead's *Sex and Temperament in Three Primitive Societies* as illustrating the cultural basis of gender roles.

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BOOK REVIEW FORUM

Maria Lepowsky, *Fruit of the Motherland: Gender in an Egalitarian Society.* New York: Columbia University Press, 1993. Pp. 383, illus., maps, bibliography, index. US$18.50 paperback.

Raymond C. Kelly, *Constructing Inequality: The Fabrication of a Hierarchy of Virtue among the Etoro.* Ann Arbor: University of Michigan Press, 1993. Pp. 604, illus., index. US$28.95 paperback.

Review: Peggy Reeves Sanday
University of Pennsylvania

Expanding the Feminist Debate

In *Fruit of the Motherland,* Maria Lepowsky provides a fine-grained description of key social interactions on the island of Vanatinai in the Louisiade Archipelago. Her meticulous documentation of male/female activities in myth, daily life, and ritualized exchange makes the book a must read for anyone interested in questions of female power and authority. In the following, I will discuss Lepowsky's ethnography in the context of what Marilyn Strathern calls "the feminist debate." My aim is not to detract from Lepowsky's contribution but to use it as a platform to point out some of the flaws in the feminist debate as it has developed in anthropology and to suggest ways in which the debate might be expanded in keeping with recent developments in feminist theory.

According to Marilyn Strathern, rather than seeking "constantly fresh

conceptualizations of social life," feminist scholarship "seeks for only one," namely "all the ways in which it would make a difference to the worlds we know to acknowledge women's as well as men's perspectives" (1988:22). Taking the perspective of women means that feminists study women qua women in relation to men qua men. Related to this tendency to totalize gender in terms of male versus female is an analogous move to conflate sex, gender, and social relations. Thus, Strathern refers to "the feminist insight that in dealing with relations between the sexes, one is dealing with social relations at large" (ibid.:35). The social relations of greatest interest to feminists, which Strathern cites, are questions regarding universal female subordination.

Rosaldo and Lamphere began their introduction to the groundbreaking *Women, Culture, and Society* by noting that the first question arising in the anthropological study of women would be "whether there are societies, unlike our own, in which women are publicly recognized as equal to or more powerful than men" (1974:2). Failing to find evidence of such societies, despite Leacock's work on egalitarian societies that they cite, they concluded that while "all agree that there are societies in which women have achieved considerable social recognition and power," everywhere "we find that women are excluded from certain crucial economic or political activities, that their roles as wives and mothers are associated with fewer powers and prerogatives than are the roles of men. It seems fair to say then, that all contemporary societies are to some extent male-dominated, and although the degree and expression of female subordination vary greatly, sexual asymmetry is presently a universal fact of human social life" (ibid.:3).

The rationale for this conclusion is articulated in the first three chapters of the book, written by Rosaldo, Chodorow, and Ortner respectively. Lamphere explained recently that these contributions were placed together "to give the book a theoretical coherence" (1995:97). At the time, some of the other contributors, myself in particular, objected strongly to the idea that there could be any coherence so early in the game in the absence of a solid body of ethnographic data on women's activities. I now understand (and probably understood then, too, which is why I objected so vehemently) that the coherence came not so much from the evidence (which would explain why Leacock's work was discounted) but from the fit between the conceptual framework posited in these opening chapters and a genre of social thinking having considerable historical depth.

Rosaldo, Chodorow, and Ortner accept the traditional anthropological assumption that human social behavior takes place in ethnically bounded social units with certain universal structural properties. Like the political philosophers of the past, they document subordination by focusing on the

natural functions of women in the domestic domain of childbearing and -rearing. This approach is tantamount to reducing gender to social relations structured according to a model of biological sex differences. Thus, Rosaldo aligns gender differences with a preconceived universal domestic/public split in the social arena imposed by biological sex differences; Ortner followed suit with a presumed culture/nature split in the symbolic arena; and Chodorow made growing up male or female a function of the mother-child bond, from which males must separate physically and psychologically in their journey toward the public domain of culture.

Okin's analysis of women's place in Western political thought beginning with Plato and Aristotle helps put into historical perspective some of the underlying assumptions on which such conclusions are based (1979). Plato and Aristotle conceived of the Athenian polity as a bounded social unit separated into domestic and public spheres. The misogyny of Athenian culture and the fact that wives were the property of their husbands, along with uncertainty about paternity, were some of the factors contributing to the rigid distinction of a private, family life separated off from the realm of public life. Okin points out that this distinction led these and other philosophers "to the exaggeration of women's biological differences from men, to the perception of women as primarily suited to fulfill special 'female' functions within the home, and consequently to the justification of the monopoly by men of the whole outside world" (ibid.:275).

According to Okin, "the only place in political philosophy where women are . . . included on the same terms as men is Plato's guardian class in the *Republic*" (1979:274). As outlined by Okin, Plato's schema suggests that the central assumption underlying the Western definition of equality is that for X to be equal to Y, X must be the same as Y. Thus, to put women of the guardian class in a position of equality with men, Plato proposed reducing the natural differences between the sexes by abolishing the private sphere of life and controlling reproduction, the socialization of childrearing, and all domestic functions so that male and female guardians could be both similarly educated and similarly employed.

In other words, Western political philosophy makes natural differences synonymous with unequal. Applying this tenet to today's world, short of conceiving babies in test tubes, gestating them in incubators, and raising them in state-run institutions, women cannot be equal to men. Public men can be fathers but public women cannot be mothers, because they must be like men. Fathers can join other men in the public arena but mothers cannot be their equals. Rather than talking about equal opportunity and granting individuals equal access to valued resources, our definitions of equality stress equal outcomes.

In all fairness, a great deal has changed since the publication of *Women, Culture, and Society.* Rosaldo abandoned the rigid sociological dichotomy of domestic/public. In a more nuanced discussion of "gender hegemonies" that can coexist with contradictory, less pervasive ideologies, Ortner seems to have given up on the nature/culture split (1990). Additionally, since the 1970s the extensive ethnographic research carried out by anthropologists interested in the woman question has yielded a number of intriguing particularistic models. Many of the articles in *Beyond the Second Sex* (Sanday and Goodenough 1990), for example, pose ethnographic and analytic challenges to assumptions that in the past supported categorical judgments of equality/inequality in favor of "addressing the conflict, variability, and contradictions that we all have encountered in ethnographic field research" (ibid.:1).

Nevertheless, the old paradigm persists. Lepowsky is careful to focus on all the roles played by women on the island of Vanatinai—not just on those related to reproduction and childrearing. At the outset, she notes that Vanatinai is "a society in which there is no ideology of male superiority, and one in which women have the same kinds of personal autonomy and control of the means of production as men" (p. xii). To support this contention she provides a convincing description of big-women as well as big-men, female power and authority in exchange, and female control of the very public and culturally salient mortuary ritual.

Lepowsky wades perilously into the old quagmire when she compares male with female activities. For example, after describing big-women and big-men in the system of inter- and intraisland exchange, she mentions the "observable fact that more men than women become prominent in ritualized exchange" (p. 35). Along the same lines, in her discussion of ancestors and spirits she suggests that there is a "slight but perceptible gender asymmetry in the supernatural balance of power" because there are more male supernaturals (despite the fact that the major supernaturals of popular consciousness appear to be female) and males dominate the destructive power of sorcery (p. 166). To give her credit, however, such statements play a minor role in her overall picture and she does not reduce the asymmetry she finds to the reproductive or childrearing functions of women.

Lepowsky's foray into the balancing act inspired by the legacy of structural-functional research reminds me of Martha Macintyre's tantalizing report of female authority and autonomy on the island of Tubetube, not far from Vanatinai. After describing an ethnographic scenario that some might say is truly egalitarian, bordering on female dominance, Macintyre reaches the obligatory conclusion of much of feminist anthropology: "But Tubetube was never a matriarchy and those few women who were exclusive leaders of their lineages achieved the status by default." In an intriguing afterthought,

which I take to be more important by far for those interested in the discourse of gender (see below), Macintyre adds that the imbalance in political parity between senior male and female leaders "is not recognized in Tubetube representations of their lineage political structure and ideally leadership is a co-operative enterprise, undertaken . . . 'by cross-sex siblings, together' " (1988:186).

The problem, as I see it, is that because the doctrine of universal sexual asymmetry has achieved the status of theoretical as well as political hegemony in Western thought, feminist anthropologists feel that their work would be counted as less than scientific did they not pay it some sort of lip service. Relativism seems not to have made much of a dent in feminist anthropology, at least not the kind of relativism that would allow ethnographers to make observations about women's lives without comparing them to a Western-defined male standard, or even to local male standards.

I was among the early feminist anthropologists who began writing on the subject of women in the early 1970s (Sanday 1973, 1974). Although I came to the subject as a cross-cultural researcher and student of George Peter Murdock, I was trained in pretty much the same intellectual tradition that other early feminists brought to their research. Although in *Female Power and Male Dominance* (Sanday 1981), I rejected the idea of universal sexual asymmetry and did not make sameness a condition of equality, I followed Murdock and White (1969) in treating the 156 societies of their standard cross-cultural sample as ethnically bounded social units that could be compared. Within the context of these units, I assumed that able-bodied adult women as a category could be said to have a status vis-à-vis adult men. There have been many justified critiques of this assumption, some of which Lepowsky summarizes (pp. x, xii, 83–84, 282). Nevertheless, the correlations I report show that a significant association of female political and economic power with mythic themes and the relative absence of male violence against women highlight the degree to which these types of gender relations not only vary cross-culturally but co-vary with predictable social conditions. The results kept me from reducing gender to asymmetric social relations imposed by biological sex differences or the reproduction of mothering.

Fifteen years later, with the benefit of eleven field trips to West Sumatra, Indonesia, to study the Minangkabau, I no longer approach the study of women as a cross-cultural researcher but as an ethnographer. Based on long-term field research, I now question the assumption that the traditional societies of anthropological research necessarily conform either to Durkheim's notion of social solidarity or to the organic model posited by Radcliffe-Brown. In West Sumatra, for example, I found that the people who call themselves the Minangkabau are a pastiche of cultural influences, a melting pot of for-

eign immigrants who at the village level identify with a central political core, and that differences from one area to the next are best compared to the radiating spokes of a kaleidoscope.

While I am unwilling to posit a Minangkabau society, I can speak of Minangkabau sociality, by which I mean the local social and ritual networks that bind individuals identifying with Minangkabau ethnicity. Because of the diversity within the Minangkabau ethnic group, one cannot talk about female status, for the powers Minangkabau women exercise in one area may differ dramatically from those exercised in another. It is also useless to make any statements about equality or asymmetry. As informants told me over and over, as if talking to a child, in some activities females have more power and in others males do. Which sex rules (or even whether one does) is a meaningless question, as I found out by asking it too often and getting a confusing array of European-inspired lectures on the Minangkabau "matriarchate" on the one hand and self-serving disquisitions (usually from males) on male dominance on the other.

Recognizing that my feminist-inspired questions obscured my understanding of how gender operated in local Minangkabau culture, after a few years I abandoned the feminist paradigm for a discourse-centered approach to gender. By discourse-centered I mean locating the culture of Minangkabau gender in the concrete. Rather than conceiving gender as an overarching template or an inner identity, I concentrated on the publicly accessible signs of everyday life such as speech, images, myths, and rituals by which individuals perform and mark gender or have gender stamped upon them. My field data suggested that gender is best conceived not as an immutable characteristic of individuals but as a set of signs that are exploited for personal gain or that are thrust upon children to give them a sex. Based on these results I am sympathetic to Judith Butler's argument that gender is not just "the cultural inscription of meaning on a pregiven sex," but "the very apparatus of production whereby the sexes themselves are established" (1990:7). To this I would add, that even after a sex is stamped on the individual, gender as a system of signs can give sexed individuals considerable room to maneuver. In West Sumatra, for example, individuals can and do play games with the signs of gender in pursuit of a variety of interests: ritual, political, economic, or sexual.

In his application of a discourse-centered approach to his fieldwork in a Brazilian Amerindian community, Greg Urban refers to the "circulation" and "fixity" of discourse (1996:10). By circulation he means discourse that achieves a wider currency "because of its effectiveness in helping a community to exist in the world" (ibid.). As discourse circulates it also tends to become fixed and transmitted from one generation to the next. In the field set-

ting, this raises the question of ethnohistorical research. To what extent is the discourse of today inherited from the past and under what conditions? In the case of the Minangkabau this is a very important question because of Kahn's suggestion that Minangkabau matriliny is a response to Dutch colonial agricultural policies (1993).

Turning back to Lepowsky's ethnography, I can give one example of how a discourse-centered approach might be relevant to her data. Lepowsky argues that my finding of a correlation between female-centered creation myths and female power does not hold up in her data, because the Vanatinai creation myth involves a male creator (p. 131). However, if we agree with Urban that culture is anchored in "objectively circulating discourse" (1991: 10), this creation myth would have to be deemed culturally peripheral because, as Lepowsky notes (p. 131), it is unelaborated and rarely told. Of more interest is the female-oriented myth of the origin of exchange, which Lepowsky says is the myth that is the most widely told (pp. 131, 143, 146). The association of females with custom in this myth suggests that my original finding of a significant correlation between the themes of origin myths and female power holds for Vanatinai.

To conclude this all too brief treatment, a discourse-centered approach would lead me to suggest that it is better to speak of egalitarian sociality on Vanatinai rather than of an egalitarian society because, as Lepowsky says (p. 219), "women have equal opportunities of access to the symbolic capital of prestige derived from success in exchange." This access, however, does not guarantee that the same number of women as men will pursue the opportunities open to them, nor should it, any more than the same numbers of men should be expected to accrue the same symbolic capital as women in other arenas of Vanatinai sociality.

Because of her emphasis on women's perspectives and interest in addressing with her data the assertion of universal sexual asymmetry, Lepowsky's ethnography falls within the feminist debate as described by Strathern, while my refusal to weigh male against female in my own fieldwork or to reduce social relations to a putative sex/gender system would count me out. However, the feminist debate in and outside of anthropology is expanding. Gradually, gender is being dethroned as an immutable characteristic of individuals and sexual asymmetry is less persuasive as a conceptual framework for framing social relations (see, for example, articles in di Leonardo 1991; Sanday and Goodenough 1990). This more pliable approach frees women from the totalizing control of gender constructs. The new trend is both feminist and good anthropology: feminist in its attention to women and good anthropology in its attention to variation. Through such an approach we as anthropologists are able to contribute accounts of women negotiating, con-

testing, exercising, and holding power as autonomous agents and individuals rather than as dependents or subordinates of men. Conceived in this fashion, Lepowsky's ethnography helps to expand the feminist debate while remaining faithful to the task of anthropology.

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1996 *Metaphysical Community: The Interplay of the Senses and the Intellect.* Austin: University of Texas Press.

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Fruit of the Motherland is an engaging, well-written, and conscientious monograph adding to the rich corpus of Massim ethnography. Focusing on the question of the possibility of gender equality on the island of Vanatinai, more generally known as Sudest Island in the Louisiade Archipelago of Papua New Guinea, it adds another detailed description of another island society in an ethnographic area that has acted as the crucible for some of the fundamental questions posed by the discipline.

It is not surprising that the work of Bronislaw Malinowski should figure prominently in the more recent ethnographies of the area. What is striking about Lepowsky's book is that unlike Weiner (1976), for example, who uses bits of Malinowskian text as a foil in the presentation of specific aspects of cultural life, this author self-consciously emulates the style of *Argonauts of the Western Pacific.* She does so in the face of the critical questions raised in her preface, having to do with ethnographic authority and positioned subjectivity (p. xiii). Her judicious deployment throughout the text of objectified description, narrative, anecdote, and so on is in many instances as delightful and satisfying as that found in *Argonauts.*

Again self-consciously, she opts for the use of the "arrival narrative" to launch the reader into this documentation of her experience with the people of Vanatinai. Readers of Lepowsky's chapter 1 find themselves in a boat, with the same sense of adventure as in the first chapters of *Argonauts,* setting out into the island system, picking up bits of gossip about sorcery and theft, studying the passing shoreline. The reader feels the same trepidation when the author wonders "whether or not I had made the right decision in choosing to make the island my home for a year or more" (p. 1) as in reading

Malinowski's chilling "Imagine yourself suddenly set down . . ." (1922:4). One wonders though whether this invocation of the authority of style, "an old fashioned, Malinowskian tradition" (p. xiii), is adequate to answer the kinds of fundamental theoretical questions posed in the works of Rosaldo (1989) and Clifford (1983) cited in the preface.

Although I found the ethnographic description of Vanatinai customs and practices valuable in themselves and the author's narratives of interaction with the community insightful, much of the tone of the text seems to harken back to another era and another discourse. Some years ago Marilyn Strathern wondered what conditions would be necessary to allow for the cultural construction of the "gender neutral person" (1987). This question, phrased in the feminist-inspired rhetoric of personhood and agency, was formulated after two decades or more of intense and consistent documentation of gender opposition, complementarity, dichotomy, and hierarchy in Melanesia, much of it drawn from the New Guinea Highlands. To look for the unmarked, to look for the possibility of parity was a welcome relief from the all-pervasive dichotomy and followed upon the exhaustion of the terms of discussion dictated by the concept of sexual antagonism.

According to Lepowsky, the gender-neutral person exists under conditions of low population density and interisland ritual trade. She documents the ability of women to act independently in the area of interisland ritual exchange, to inherit and control land, and to act autonomously in personal relations. She provides evidence of the absence of mythic and ideological denigration of women and the control on the part of women of esoteric and powerful knowledge. At several points Lepowsky discusses the relationship of matrilineal and matrilocal configurations to the relative autonomy of women as social actors. The discussion of coastal matrilineal societies in Melanesia suggests the need for further comparative work, as does the differential configuration of women's ritual exchange activities in the Massim in relation to those of men.

The discussion of social agency of individuals unmarked by gender distinctions and valuations is deployed within a larger argument that attempts to represent Vanatinai society as an egalitarian society. Although initially, in the preface, the goal is to present evidence in support of gender equality, by the end of the book the argument surpasses this restriction, and in the final chapter Lepowsky is arguing that Vanatinai society itself is egalitarian.

There are several threads of argument addressing the gender-equality issue that flow through most of the chapters. These arguments depend on rather transparent devices: the generalization of the discussion from the particular case to the whole of human experience, as in making the link be-

tween low population density and social egalitarianism (p. 38); and the assertion that lack of gender marking in a particular context is an indication of egalitarian gender relations, as in the "fact that old woman as well as old man are terms of respect and not of insult on Vanatinai is another marker of its egalitarian gender relations" (p. 119). These devices are not always convincing, not necessarily because of logical inconsistency. Rather, the points made have a somewhat hollow ring. I suspect this is due to the fact that they are set in a discourse that literally ran its course some time ago. Lepowsky refers repeatedly to classic works like Schlegel's *Male Dominance and Female Autonomy* (1972) and Ortner's "Is Female to Male as Nature Is to Culture?" (1974). I am not suggesting that these works have no further value and have no place in current writing. But there is a sense in which Lepowsky pays lip service to the concerns of the 1990s by the odd reference to writings that have set the tone of current debate (for example, Clifford and Rosaldo in the preface, Appadurai 1990 and 1991 in the concluding chapter). In the preface she assures the reader, "I no longer believe it possible to describe 'the [single and invariant] status of women' in any society" (p. 12), and then proceeds to argue for Vanatinai as an example of a society of gender equality.

Lepowsky's major fieldwork took place from 1977 to 1979. She returned for briefer periods in 1981 and 1987. Conceivably this representation of Vanatinai culture is based on data collected over this ten-year period. If, as we are led to believe, this decade saw little social change on Vanatinai this cannot be said of the concerns of the discipline. The ability to integrate data collected initially under the "status of women" paradigm with a current belief in the impossibility of describing a single and invariant status of women in any society is indeed a challenge.

Malinowski's style of representing Trobriand society in his initial ethnography is indeed worthy of emulation for its range of rhetorical modes, its detail, its thoroughness. What it does lack is self-awareness, especially in terms of the limitations of the concepts deployed, which themselves are cultural artifacts with a life course of their own. Lepowsky shows little appreciation of the life histories of the concepts of gender equality and the gender-neutral person. She uses the terms interchangeably. This equation of the two leads to a further conflation of social equality itself in the final chapter, with value placed on personal autonomy and the ethic of individualism. At one point she describes Vanatinai society as "an equal opportunity society" (p. 287). It is just such confounding of analytic structures and subjective ideology that critics of traditional ethnography write against.

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Review: Lisette Josephides
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Gender Equalities and Ethnographies That Speak

With *The Fruit of the Motherland,* Maria Lepowsky joins an ongoing debate on the nature of gender equality in Melanesian societies. Twenty years ago, Annette Weiner introduced us to aspects of the matrilineal Trobrianders that Malinowski's ethnographies had neglected (Weiner 1976). She acquainted us

with women whose cultural power and social roles far exceeded in prestige, recognition, and influence those of Highlands women in mainland Papua New Guinea whom earlier ethnographies had described. Now again, Lepowsky's account of Vanatinai (Sudest Island) exceeds Weiner's, describing a people whose gender ideologies and practices, though not perfectly egalitarian, "come close." Lepowsky teaches us the salutary lesson that, in other respects, the Trobriands are not to be taken as a paradigm for the whole Milne Bay region: Vanatinai Islanders do not participate in the *kula* ring and they have no chiefs and no ascribed status of any kind. They also have no men's houses, no male cults, and no initiations for either sex.

These cultural conditions facilitate shared gender ideologies, which uphold the value and autonomy of all persons. In particular, these ideologies emphasize the importance, for both sexes, of the qualities of strength, wisdom, and generosity. The same qualities define the *giagia*, big-men and -women who achieve prestige by engaging in ritualized exchange around mortuary feasts. Nevertheless, several conditions prevent Lepowsky from claiming that the Vanatinai are entirely egalitarian. For instance, the position of *asiara* warrior as an avenue for prestige was not open to women, and the equalizing effect of its loss since pacification has been sacrificed to a new disadvantage: women's inability to speak English, and general lack of education as a result of their island-boundedness, disqualifies them from the office of councillor. Lepowsky's account also reveals some contradictory ideologies of gender and discrepancies between belief and reality. Two central contradictions are explained, at least partially, in terms of each other. Though women are considered life giving and men life taking, nurturing mothers are also life-destroying witches; and though men and women have equal ritual obligations, in practice more men than women become prominent in this sphere. Lepowsky suggests that it is precisely women's specialization in childcare and life-giving activities and men's identification with destructive powers that results in men's preponderant activity in ceremonial exchange. In the case of the contradiction between nurturing mother and life-destroying witch, Lepowsky remarks only that images of women "can be multivalent and oppositional" (p. 285).

Lepowsky's strategy is to submit Vanatinai beliefs and practices to several criteria that have been developed to test for sexual egalitarianism. Her use of the criteria is not symmetrical, however; she treats them as significant when they yield positive results but is critical of them when they do not. Three criteria are deemed immediately satisfied: the Vanatinai make valued statuses available to as many individuals as are capable of filling them, they are free of violence towards women, and they lack ideologies of "natural woman" and "cultural man." Another criterion, that of the sexual division of

labor, poses some difficulties. Though in general men and women carry out similar tasks, women do most daily garden maintenance and cooking, and are exclusively responsible for cleaning the hamlet of animal droppings. Only men hunt with spears, and weather magic is their province. They also do most house building. Lepowsky notes that spear killing is high in prestige while hamlet cleaning is low.

Lepowsky has more trouble with the criterion that belief in a male creator spirit is counterindicative of sexual egalitarianism, since the Vanatinai do indeed hold such a belief. So she musters counterevidence to illustrate that "feminine origin myths . . . may . . . symbolize not an egalitarian ideology of gender but male preoccupation with female power" and can, moreover, result in power over women (p. 131). Quite possible, but should not the other criteria have been put through the same mill? Other conditions present among the Vanatinai—women owning and inheriting land, living with their own kin, producing and allocating valued resources—denote, Lepowsky states, societies that "tend to treat women more equally" (p. 46). Her thesis thus links an egalitarian material base with an egalitarian superstructure.

My use of "thesis" rather than "argument" is deliberate. It registers a tendency to state or assert rather than argue or demonstrate. It is not self-evident that low density contributes to an ethic of respect for the individual; that children, old people, and women are more likely to be respected in small-scale societies; or that generally egalitarian cultures are more likely to be gender egalitarian. (Many Highlands societies described as "egalitarian" are far from gender egalitarian. Lepowsky cites Feil's arguments about women's important roles in large-scale pig exchanges in the part of the Highlands where he worked [1978], but ignores counterdemonstrations from other parts of the Highlands, for example, Josephides 1985, which also question Feil's formulations.) One needs concrete accounts that show women attaining considerably more prestige and influence than their husbands, contributing larger quantities of valuables, and creating much of the wealth essential for mortuary ritual exchange. One also wants to learn about the specific instances in which Lepowsky came to know that Vanatinai consider it "morally wrong" to kill people or cause them illness or misfortune through destructive magic, especially in light of descriptions of the sorcerer as simultaneously a *gia,* generous and prestigious giver, and a solitary and antisocial person. One wonders what precisely are some of those concrete decisions made by Vanatinai women and men that reverberate "literally hundreds of miles beyond" (p. 299), and through what channels or networks they do so. And one is not convinced that statements by an unspecified number of men and women in unspecified circumstances, who "sometimes" or "frequently" prefer female

infants, provide "significant evidence of prevailing ideologies of gender equality" (p. 87). (In Highlands societies men and women like to have infants of both sexes for a host of cultural and practical reasons; should their preference be seen as a criterion of gender equality?) Some accounts appear contradictory: gender identity is formed after puberty, yet five-year-old girls do chores while boys play and a seven-year-old girl is seriously reprimanded for joining the boys in a spear-throwing contest; women are not subject to taboos on the basis of sex, yet we are provided with a list of sexual taboos that affect women.

Lepowsky herself evaluates her theoretical contribution: she has indicated some of the conditions in which one egalitarian society exists (p. 298). And indeed, one is aware that rich ethnographic fieldwork informs Lepowsky's conclusions; one merely wishes that she had included accounts of specific instances of social practices that demonstrated those conditions, as well as accounts of people's own narrative understandings of their situations. Instead of submitting to general criteria the degree of equality between the sexes, she might have explored people's creativity and inventiveness, their different ways of acting and exerting their influence within local cultural conditions and constraints. This approach would also have followed the strand in feminist ethnography, developed in the early 1990s, that adopts a different attitude to the reporting of ethnographic data and their relation to theory (e.g., Abu-Lughod 1993; Tsing 1993). It would have required more sustained attention to the fascinating ethnographic observations that Lepowsky throws out here and there: that exchange is sexual encounter, carried out in the idiom of seduction; that *giagia* are "dually gendered" (remarks that induced an anticipation that a narrative of local understandings of gender and sex was about to be developed but instead ended in the *giagia* being desexualized); that knowledge is private; that individual actors are metaphors for other persons, yet unique persons in themselves.

Lepowsky's book attracted considerable media attention. I want to treat the phenomenon of popularity and its implications for anthropology as part of my review. To this end, I offer my remaining comments under three headings: communication and accessibility, ethnographies that speak, and the future of anthropology.

Communication and Accessibility

How should we communicate as anthropologists? Is bad communication our problem? What is good communication? Is there a relationship between the form and content of communication? What is the role of publicity? What impact is exerted on our work by media interest? By the desire to write for

multiple audiences? Should we present our work differently to different audiences? Or write different works for different audiences? Not all of these questions will be tackled, but at least their problematization will have been registered. Since Lepowsky herself has made the question of communication an issue, we must face it head-on.

Following the media interest in Lepowsky's book, *Anthropology Newsletter* invited her to write a series of articles evaluating her success and offering practical advice to others. In her articles Lepowsky dwells on modes of communication, writing styles, and the importance of addressing multiple audiences (*AN* 1994b, 1995a). She advises us to have a publicist (who will prepare "tip sheets" and contact journalists), write accessibly (though it may expose us to charges of insufficient scholarship), and show how our research relates to broader issues of public interest.

How does one write "accessibly"? Apparently by avoiding anthropological "jargon" and "rarefied theory." Lepowsky's own book is copiously written. If this makes it easier for a nonspecialist audience to understand, it also makes it cumbersome and diffuse. Language loses its sharpness and lucidity in pleonastic solecisms (such as "intimidated by fear of . . . revenge" [p. 168], "[o]ne of the only activities" [p. 93], "[o]ne of the only occasions" [p. 30]). As for argument and structure, Lepowsky's writing is often repetitive and at times fragmented and disorienting. Chapter 2 tacks between colonial events and historical and cultural arguments in a way that dissipates the reader's attention, while the final chapter repeats and summarizes what has been said several times before. I would like to replace "accessible writing" with "good writing."

Lepowsky concludes her book by addressing the broader relevance of her work, what others may learn from the Vanatinai case. They (we) learn that the subjugation of women is not a human universal. That small-scale societies, which do not delegate but involve members in direct participation, tend to enjoy an overall ethic of respect that facilitates gender equality. Other societies (the content indicates American society) can move in the right direction by allocating to women and minorities greater control over valued goods. Looking at Papua New Guinea's national politics today, without a single woman in parliament, and the situation on Vanatinai itself, where women may not be councillors, one wonders how this advice is to be taken. This facile location of "relevance," and suggestions about lessons to be learned from the findings of our fieldwork, are part and parcel of the puzzling attitude that if your findings "look like" your own culture, you've been influenced by your own culture, but if they don't, you've risen above it. In Lepowsky's own words, because Susan Carol Rogers found contradictions between ideology and practice in France and Lepowsky found none among

the Vanatinai, "[f]eminist scholars from Europe and America writing in the last few years about contradictions between gender ideology and practice and between levels of ideology have surely been influenced by their own lives in their own countries" (p. 285).

In her *Anthropology Newsletter* articles, Lepowsky chides the profession for discouraging the communication of anthropological ideas to nonspecialists, especially at a time when the teaching of evolutionary theory is threatened and killing is undertaken in the name of cultural preservation. How can anthropologists avert these ills? Surely not by perpetuating the idea that anthropology as a profession is disdainful of nonspecialists and thus ultimately responsible for cuts in funding and the elimination of departments. Lepowsky's references to "jargon" and "rarefied theory" trivialize and marginalize those very findings that may work against the ills she laments. Instead of giving particulars on that jargon and rarefied theory, and naming those departments that brought about their own downfall, she resorts to generalizations that undermine anthropology in more serious ways. Though in current use by anthropologists themselves, these damaging and imprecise expressions derive from others' cynical views of the "facts of life"; which, in this case, are perceptions of what will sell. The old question rears its hoary head: do we accept uncritically what some editors (who may be open to persuasion, or at least conversation) think is marketable, or do we struggle with the sort of scholarship in which we can lose our hearts and souls? Good scholarly books may also sell. We needn't expect to write best-sellers. By some accident we may write one, but this is not a standard by which we should measure our work. Specializations exist in all societies—even, we learn in Lepowsky's ethnography, among the Vanatinai.

Lepowsky herself suggests that newsworthiness depends on the extent to which research claims sensational novelty. One journalist dropped her story following her account of other anthropologists who had studied egalitarian societies. Her story was considered a "rare find," perhaps such as ancient pyramids or the oldest-known hominid remains (*AN* 1994b); when this claim was questioned, its attractiveness declined accordingly.

Ethnographies That Speak

Naturally, as anthropologists, we want the discipline to survive. But what exactly is it that we want to survive, what are our distinctive contributions? What sorts of questions are productively, interestingly, edifyingly, and constructively asked in considering the content and form of ethnographies?

I consider two approaches, or paths, in the writing of ethnographies. One is to demonstrate an argument, another is to describe life as locally lived.

Most ethnographies do both simultaneously, of course, but the emphasis may vary considerably. As publishers desired us to shorten up and smarten up, we focused more on the argument, with the result that "rich ethnographic detail" suffered, even when our focus was a narrative ethnography and the observation of participation (Tedlock 1991). "Theoretically oriented ethnographies" achieve a synthesis of sorts by allowing ethnographic accounts to make the theoretical points, instead of serving as illustrations for a theoretical perspective developed elsewhere. The distinction between "theory-centered" and "ethnography-centered" approaches is also made in philosophy, where theory-centered analysis "poses problems, and seeks solutions, stated in timeless, universal terms . . . whose charms were linked to the quest for certainty" (Toulmin 1992:11). In his argument against "theory-centeredness" (ibid.:27), Toulmin cites Montaigne, who argued, as early as the sixteenth century, that "it was best to suspend judgment about matters of general theory, and to concentrate on accumulating a rich perspective, both on the natural world and on human affairs, as we encounter them in our actual experience." Part of the universalizing intention of theory-centeredness was, of course, to render problems independent of context. Though Lepowsky may not think so, her mode of argumentation decontextualizes by posing its problems in universal terms ("egalitarianism," "equality") and relations ("degree of gender equality," "division of labor") developed elsewhere, yet treated as timeless.

I see both these paths, whether demonstrating an argument or describing social life, as requiring an account of practices as they are actually lived. If an argument, say of gender equality, is the focus of an ethnography, it should unfold through the account of concrete instances of practices that construct that equality as a social reality. By contrast, Lepowsky's discussion of gender and the life course (p. 122) is a general, abstracted account that includes no actual stories of men or women in the process of taking sexual initiative or bringing about marriage or divorce. Thus, it misses the opportunity to develop the local meaning of activities into a rich narrative.

Nevertheless, the theoretical question of the origins of social and gender inequality continues to fascinate and attract research. For those of us who have ingrained in them the paradigm that men and women are equal unless otherwise fabricated, the social fabrication of inequality may seem a more compelling candidate for study—until we realize that equality itself needs to be fabricated, that it is the product rather than the absence of ideology.

Lepowsky wisely plumps for the middle ground in her discussion of whether equality results from material or ideological causes (p. 294). Raymond Kelly does not; in his *Constructing Inequality* (p. 16), he tells us right away that "the grounding of social inequality is essentially ideological"

among the Etoro, who have a "cosmologically derived system of moral evaluation" according to which "gendered division of labor is a scheme of morally evaluated social differentiation" (Kelly, pp. 512, 513). When Kelly argues that social practice, in which everyone participates, does not alter the "metaphysics by which the 'reality' of social action is constituted" (p. 516), his understanding of "practice" excludes those "metaphysical" practices of the elites that reproduce the moral hierarchy. Yet there may be no justification for this exclusion. If this modification is made, Kelly's approach—and I cannot comment on his actual arguments and findings—though "theory-centered" (or "argument-oriented") nevertheless offers an account of practices.

Lepowsky gives us some interesting and lengthy ethnographic accounts, especially in the chapter that describes memorial feasts, "Fruits of the Dead." These accounts, however, are extended ethnographic illustrations of the type "how memorial feasts are held on Vanatinai." Rather than an "ethnography that speaks," they construct an "ethnography that illustrates," in which a summary of the ethnographer's observations and generalized reflections create a "typical" event. An ethnography that speaks would not have pursued typicalities, though it might in the end have had them thrust upon it. It would instead require several accounts, not synthesized, of concrete, idiosyncratic strategies in "similar" cultural contexts. Lepowsky's aim is to demonstrate egalitarianism in gender practices and ideologies. If social inequality "can be minimally defined as social differentiation accompanied by differential moral evaluation" (Kelly, p. 4), formal gender inequality exists on Vanatinai to the extent that gendered labor is morally evaluated to the detriment of women. But even if we are satisfied with Lepowsky's theoretical demonstrations of equality on Vanatinai, we may still wonder what is meant by the claim that women are "treated equally." It is impossible to understand this statement without accounts of concrete practices, those several instances in which being "treated equally" acquires meaning as a social reality.

Thus, the second path for ethnography would require numerous accounts of talk and practices, but also ask different questions. It would not pursue questions of the degree of equality in a given society by applying cross-culturally developed criteria, but rather it would examine the inventiveness of human beings in their local and cultural situations. How do different persons behave differently and exert different influence? What do men and women actually say and do, what can one read in their practices? (As I have indicated, these questions could also be asked in a more conventionally argument-oriented ethnography.) This kind of ethnography would not be about the discovery of pristine cultures that either corroborate the universality of our abhorred traditions or put the lie to them—both forms of uni-

versalizing ethnocentrism, as Todorov has shown (1993). Demonstrations of the second kind feed romantic notions of anthropology and catch the public eye for a brief space, as nostalgic playthings of the "civilized." How can this "ethnography that speaks" influence the future of anthropology in its broadest sense?

The Future of Anthropology: Substantive Issues, Theoretical and Methodological Perspectives

The word "ethnography" is now on everyone's lips. In Marilyn Strathern's ironic formulation (1995:155), our concern to make anthropology relevant to others backfired when they began to borrow its terms and concepts but not "the techniques of understandings" that produced them; in other words, the practices of a specifically anthropological ethnography. The other way of becoming relevant to other disciplines—by reaching out and borrowing *their* concepts and techniques—is also underway, as we shall see below.

The anxiety that the trajectory, techniques, subject matter, and conceptual basis of one's discipline may not be relevant to current realities is not, of course, confined to anthropologists; for accounts of philosophers' angst, see MacIntyre (1984) and Toulmin (1992). Nevertheless, anthropology has a rather large dose of it, as exemplified in the debates about the future of anthropology, sponsored by *Anthropology Newsletter* and reported there during 1994–1996. The debates scraped the depths of moral panic but also explored creative ideas as they tackled the following questions: Will we survive the financial cutbacks and ideological/epistemic/political shifts, including shifts in perceptions of what knowledge is and what it should be for? Can we communicate? Here is Jon W. Anderson, 1995 program chair, describing the American Anthropological Association's ninety-fourth annual meeting, whose theme was "New Forms of Communication and Community":

> Critical scrutiny of anthropology's future both within and beyond the academy, plenary addresses by the chairman of a major federal granting agency and an internationally recognized scholar of social change, a second AAA-sponsored public policy forum . . . have been scheduled. . . . [W]e have sessions on many aspects of the emerging world order. . . . Professional issues of the field itself will be highlighted by two presidential initiatives. A trio of panels will discuss the restructuring academy and the profession beyond the academy. Special plenary sessions include a keynote address . . . on the restructuring of the American Community, by National Endow-

> ment for Humanities Chairman . . . [who] will review why we need such a dialogue and will assess its progress. (*AN* 1995c)

Anthropology too, then, needs its consultants and technicians to know how to proceed. In preparation for this session, *Anthropology Newsletter* fielded the following question to heads of anthropology departments: "What do the next 25 years hold for sociocultural anthropology in your department?" Answers were compiled as follows: traditional ethnographic studies of "exotic societies" will not fare well; anthropology will place a greater emphasis on the contemporary world and processes of global change; it will become more interdisciplinary and integrated into international programs, urban studies, and history; it will become involved in programs such as sustainable development, environmental studies, comparative global perspectives, global interdependence, and internationalization and in medical, urban, and organizational-culture studies; it will shift to U.S. ethnic groups, public planning, and policy; and the education of students about Third and Fourth World cultures and issues will grow in importance (*AN* 1994a).

Social forecasting of "horizons of expectations" is notoriously chancy, as Toulmin reminds us. More to the point, he reminds us of Jouvenel's encouraging observation that "[a]vailable futures are not just those that we can passively forecast, but those that we can actively create . . . if we meanwhile adopt wise attitudes and policies" (Toulmin 1993:2). So let it be with anthropology. As its practitioners, how can we seize the initiative?

To some extent, the initiative was seized in the *Anthropology Newsletter*–sponsored discussion of the place of science in anthropology, whose ramifications implicated the discipline's theoretical and conceptual bases. These questions were asked: Is anthropology seeking new paradigms? Are participant observation, ethnoscience, and empiricism comparable methodologies? How does anthropology understand causation and what "laws" has it generated? Is it possible for anthropological knowledge to yield "objective reality"? Are there alternatives to the scientific method for producing knowledge? Is critical theory compatible with "science"? (*AN* 1995b).

Though not separately tackled, these questions were relevant, given the effects of "the current congressional mood to fund only 'basic science' " on the two primary sources for anthropologists in the United States (the National Endowment for the Humanities and the National Science Foundation), thus severely limiting what we as practitioners can "actively create." Anthropologists responded by interpreting "science" and defining anthropology.

A broad range of perspectives on science was explored. Most relevant to my discussion are the views of Bruno Latour, the upbeat ethnographer of

science. He sees anthropology as having the opportunity, after years of deconstructing itself almost to death, to rise on the crest of vibrant and proliferating hitherto-presumed-vanishing cultures, peoples who have turned the tables and are "reanthropologizing" regions of the earth. Here is an ironic trajectory: in our "mea culpa" deconstructive, guilt-ridden phase we painted images of anthropology as feeding off the backs of vanishing peoples; now again, but in a different way—they have the upper hand, the power of self-definition—they sustain us. "The newly reinvented cultures are much too robust to dwell upon our past misdeeds or present lack of heart," Latour tells us, and one would like to believe him despite evidence to the contrary. "The current situation needs an anthropology willing to embrace its formidable achievements and so further extend its many valuable insights" (*AN* 1996).

Anthropology's other problem is its antiquated theory of science as proceeding by a "methodological rigor," to which we think we fail to measure up. By contrast, Latour defines science as "innovation in the agencies that furnish our world." Anthropologists constantly redefine agencies in the world; we also have "elicited, mobilized, stored, documented, archived, compiled, theorized, assembled and modeled more new facts and agencies than many disciplines purported to be more 'natural,' 'rigorous' or 'scientific' " (ibid.) Moreover, the traits which we think make us too local, too narratively reflexive, or too subjective are also found in the practices of geneticists, neuroscientists, and mathematicians. We are not perfect, however: we fall short in our asymmetrical treatment of belief systems and truth systems, a by-product of a theory of science that sees culture and nature as incommensurable.

Ethnographies that speak can mark an "innovation in the agencies that furnish the world": they redefine local agencies at the same time as they allow them to participate in that definition.

Crisis?

Our problem is not, in general, one of communication. Other elements are more active in the constitution of our self-doubt. The loss of a certain complacency in our work, as a result of methodological, epistemological, political, and ideological shifts, should be welcomed as an opportunity for creative self-questioning, not regretted as a setback. Undoubtedly, many things are not within our reach and escape our control; but still we should proceed with courage and forthrightness in our self-appraisal. Our professional practices, our treatment of fellow anthropologists, our subterfuges, our sometimes defensive attempts to escape accountability are no less constitutive of the

profession than our ideas and publications. Reaching out to other disciplines should not require that we collude in the trivialization of our own practices. The distinctiveness of anthropological knowledge as resulting from understandings of practices in the broadest sense (ethnography-centered more than theory-centered) should not be sold cheap. In our examination and description of those practices, what we render as "inequality" or "equality" may emerge in a different shape, without having to be named in advance. Though this review has moved beyond a consideration of these terms, its intention was to provide a context for their continuing relevance.

What are the uses of "relevance"? Should we view our work from the perspective of its applicability to problem solving? What and whose problems? The theme of the 1996 conference of the European Society for Oceanists was inspired by massive changes in political, economic, and ecological conditions in the world. In this "Pacific Century," Pacific peoples—Latour's robust, re-anthropologizing cultures—are particularly affected. Questions that bring together those local concerns—on environment, epistemologies of nature, creative agency, globalization, media, and material culture—under "knowledge and technologies for the future" are preeminently anthropologists' concerns. They are also the area where all our "relevancies" merge.

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Shamanism and Witchcraft at the Origins of Inequality

Raymond Kelly's *Constructing Inequality* is a remarkable book with new insights on offer for academic readers interested in inequality and stigmatization as general social phenomena, although perhaps only those anthropologists inclined towards quantitative, objectifying studies will find it other than dry going. Potential readers both general and specific would do well to acquaint themselves beforehand with Schieffelin's *The Sorrow of the Lonely and the Burning of the Dancers* (1976) and Knauft's *Good Company and Violence* (1985), both of which provide more interpretative and descriptively vivid accounts of social life in Papua New Guinea's remote and sparsely populated Strickland-Bosavi region as it was in the late colonial period.

The Etoro are one of a number of small tribes occupying the country between the Strickland River and Mount Bosavi in the southwest of Papua New Guinea. In 1968, when Kelly began fieldwork, there were 386 Etoro living in some twenty longhouse groups, although the population had declined by perhaps as much as 50 percent since introduced diseases began to have an impact on the region in the 1930s.[1] The ethnographic present in

which Kelly sets the study refers to the late colonial, pre-mission period, immediately before and during his fieldwork—a fiction retained here for simplicity's sake.

Kelly's aim is to "provide a comprehensive account of social inequality, and of the interplay between egalitarian and inegalitarian features within the Etoro sociocultural system" (p. 3). An economically egalitarian people who greatly value generosity and sharing, the Etoro are nevertheless divided among themselves by differences of moral virtue and prestige. Virtue, in Kelly's analysis, attaches ascriptively to sex-age categories, with senior men at the top and unremarried widows and divorcées at the bottom. Prestige and its reciprocal, stigma, pertain to achievable statuses, of which there are only four: *tafidilo* (secular leader), spirit medium, witch, and "superwitch."[2] *Tafidilo*s are senior men who achieve their positions through communal recognition of their contributions of game and pork to longhouse distributions. Mediums are younger men who serve the community by conducting public séances in which their familiars effect magical cures and discover the identity of sickness-sending witches. Witches can be of either sex and are held to possess "a mutation of the soul" that turns them into spiritual cannibals who prey upon the souls of their neighbors.[3] Superwitches are adolescent males who consume their age-mates' life force by engaging in egalitarian (rather than normatively hierarchical) homosexual relationships.

Kelly frames his study within the larger context of sociological theories of inequality in the tradition from Locke and Rousseau through Marx to such recent anthropological theorists as Meillassoux and Jane Collier. He proposes that understandings of inequality in this tradition have turned on the question, "What is the principal locus for the production of inequality in human society?" Neo-Marxists have sought the source of inequality in modes of production, while Collier's "synthesis of feminist and neo-Marxian perspectives" (p. 3) proposes that "marriage, as the basis of kinship, organizes social inequality" (Collier 1988:vii). Considering the Etoro case, Kelly finds reason to reject both perspectives. His analysis of the factors contributing to inequality among the Etoro leads him repeatedly to their system of metaphysical ideas (about life, death, growth, and the transmission of life force) as the grounds for and cause of the hierarchies of virtue and prestige that constitute inequality in their world.[4]

On the face of it, Kelly's argument is an idealist response to the materialist approaches he opposes. He accepts as valid the search for "a central locus for the social production of inequality, . . . the internal equivalent of a prime mover" (p. 1). But against historical materialism's guiding principle, that "it is not the consciousness of men that determines their existence, but their social existence that determines their consciousness" (Marx 1859), Kelly ad-

vances the thesis that the Etoro cosmological system of ideas is the "prime mover" of the sociocultural system, the "generator" of inequality. There's a trick to the argument, however, turning on the question posed by the book's subjectless title: who or what "constructs" inequality? Throughout most of the book it is the system of cosmological ideas that "constructs," "fabricates," "shapes," "grounds," and is the "source of" inequality. Kelly's explanatory metaphors represent the cosmological system as an autonomous source of social efficacy—a conceptualization further reinforced by his identification of the "locus of the production of inequality" as a "prime mover."[5] The first suggestion of the surprise in store comes a third of the way into the book when, after a discussion leading up to the conclusion that "[b]oth men and women live in a conceptual world shaped by a male-centered cosmology" (p. 191), Kelly raises the question as to "the source, nature, and mechanisms of control over the cosmological formulations that underwrite the hegemonic ideology" (p. 205). In response, he observes that Etoro spirit mediums have "vested authority with respect to doctrine," and he foreshadows a concluding "consideration of this elite's control over the cosmological foundations of social inequality" (ibid.). Nothing more is made of this until the end of the book, where Kelly announces, "While the 'cultural system,' 'meaning system,' and 'value system' constitute comparatively amorphous sources of social inequality . . . the source of Etoro conceptions of the fundamental nature of reality can be precisely specified. Such conceptions are the product of a shamanic elite" (p. 482). Whether the "amorphous" quality of the Etoro cosmological system as a part of the wider Strickland-Bosavi cultural system disqualifies it from being the prime mover of social inequality, as it has hitherto been presented, is not made clear. What is clear is that the cosmological system of ideas as the source of inequality itself has a source—and an entirely material one at that. Kelly comes closest to sorting out the resulting ambiguity in his concluding remarks:

> The cosmological system is the central locus for the production of social inequality as a result of the fact that those who control the production and reproduction of cosmological doctrine formulate that doctrine in a manner that accords moral superiority, prestige, privilege, prerogative, and power to the social positions they occupy and the social categories to which they belong. This is why the concept of a prime mover is indeed viable. . . . Social inequality is . . . produced by an elite that is advantaged by it. (P. 515)

Kelly's ultimate explanation for Etoro inequality is thus the existence of a self-perpetuating elite of shamans who maintain an ideological hegemony in

their own interests. His remark that this is what makes the prime mover concept viable comes as a last-minute revelation (or is it a recognition?) that the cosmological system is not an autonomous source of social efficacy and that the "prime mover" of Etoro inequality is not their system of ideas per se but the tribal elite of shamans who manipulate the system.[6]

* * * * *

Like a Marxist (who would begin with the direct relations of production), Kelly begins his analysis with the social relations of food production, distribution, and consumption. He finds that men put an average of 20.9 hours per week into subsistence activities, of which 6.5 hours are spent in gardening and sago processing. Women put an average of 16.5 hours per week into subsistence, of which 11.8 hours are gardening and sago processing. Women provision their families with sago and garden produce, while men have a dual obligation to provision both their families and the longhouse group with game and pork. Smaller game, caught by either sex, is distributed among family members at garden houses away from the longhouse, while large game and pigs are distributed communally in the longhouse. Normatively, longhouse coresidents should share equally and Kelly finds that this is realized in practice, with an egalitarian nutritional outcome in which women get slightly more protein than men. Thus, as Kelly points out, Etoro production and distribution relationships conform as far as scarce meat resources are concerned to the principle of "from each according to their ability; to each according to their need."[7]

Communal distributions of game and pork provide the mechanism by which the Etoro achieve their protein egalitarianism. This is reinforced by differential social rewards: men who bring game to the longhouse for distribution gain prestige.[8] But since sago, garden produce, and smaller game are not distributed outside the family and since women do not hunt large game or kill and butcher pigs, it follows that only men are in a position to gain prestige and build relationships by distributing meat. Gender-unequal prestige is thus built into the Etoro division of labor and distribution rules, even though "this is not attributable to appropriation of the products of female labor" (p. 8).[9] Thus, although "the division of labor is . . . a source of social inequality, . . . the differential moral evaluation that engenders this is derived from the cosmological system rather than from economic processes" (p. 9). Otherwise put, the Etoro economy is "a domain of relations upon which cosmologically based inequalities have been inscribed" (p. 10).

In chapter 3, "Moral Hierarchy: The Cultural Construction of Social Inequality," Kelly shows how the allocation of prestige and moral virtue is

linked to Etoro ideas about the production and perpetuation of life. Sex-age categories are delimited by metaphysical ideas and valorized on the basis of each category's culturally posited contributions to growth and life. Senior men, for example, are ascribed the highest virtue in the hierarchy of moral standing—primarily because they are considered to be intrinsically generous in provisioning the community with game.[10] Pre-reproductive married women, unremarried widows, and divorcées, on the other hand, categorically occupy the lower range of moral standing, because, so the argument goes, they are seen in Etoro metaphysical terms as contributing least to the group's well-being.[11] And at the bottom of the scale of virtue, as at the bottom of the prestige hierarchy, are the witches: "Spirit mediums seek to protect life, while witches seek to appropriate it in an ongoing contest between the forces of good and evil. The cosmological system thus provides . . . a scheme of social differentiation in which moral evaluation is intrinsically embedded. Social inequality is thus fabricated as a moral hierarchy or hierarchy of virtue" (p. 13).

Chapter 4 considers how stigma is allocated through witchcraft accusations. Whenever a longhouse resident dies from illness or accident, oracles are consulted by next of kin and public séances are conducted to establish the identity of the responsible witch. In two-thirds of all accusations a longhouse coresident is found guilty. And, although accused witches often move to another longhouse, their fate is usually to be accused again and, after two or at most three accusations, killed in a community-sanctioned execution.[12]

Males aged between fourteen and twenty and between thirty-one and forty and females between sixteen and twenty (that is, married but without children) are more than twice as likely to be accused of witchcraft than would be expected if accusations were randomly distributed. Unremarried widows and divorcées are also accused more frequently than their demographic representation in the population would predict. The other side of this distribution is that women over age sixty, males over forty, girls under sixteen, and boys under fourteen are less than half as likely to be accused, although not even infants are entirely immune (large babies of both sexes were supposed to be killed).

Kelly explains this pattern of accusations as conforming "to the contours of the Etoro scheme of morally evaluated social differentiation that delineates age and gender categories" (p. 375), that is, those assigned a low position in the moral hierarchy attract a disproportionate number of accusations. There is an assumption here that the moral hierarchy determines the accusation pattern rather than the other way around. With respect to the latter possibility, Kelly observes that accusations are not aimed at those least able to physically defend themselves, but he does not consider the more plausible hypothesis (proposed by Steadman apropos witchcraft executions among

the Hewa [1971]) that attacks tend to be made against those who become vulnerable through the loss of supportive kin among their coresidents (unremarried widows, for example).

Overlaying the sex-age categorical patterning of accusations are further accusations, amounting to 25 percent of all cases, in which the accused is an aspiring secular leader or medium. Such men are, at the least, disqualified from further advancement. In most of these cases it is significant that the presiding medium has gone beyond his usual role of authenticating others' accusations by contributing directly to the nomination of suspects. Although the "overall system thus fails to inhibit the expression of self-interest on the part of spirit mediums" (p. 282), divergent interests within the longhouse are kept in check by being "melded into a community interest in scapegoating" (p. 283). The person divined to be the witch is, by implicit common consent, the most dispensable member of the group. If the medium's spirit does not indicate that the witch is to be killed, it is left to the putative victim's adult male next-of-kin to choose between demanding compensation or killing the witch and paying compensation to his or her kin. Either course of action brings the incident to a conclusion, since the kin of accused witches will neither protect them from execution nor seek vengeance if they are killed.[13] Formerly, the bodies of executed witches were cannibalized and their hearts impaled outside the longhouse to publicly display their guilt. As elsewhere throughout the region, the eating of witches was regarded as a reciprocal retribution for their own spiritual cannibalism. No other form of cannibalism was practiced.

Chapter 5 examines how men achieve prestigious statuses. Recently initiated bachelors are nominated by established spirit mediums, speaking through their spirit voices, to become protégés and acquire spirit wives. About 40 percent of all young men become novices. After extensive training, a successful novice learns to meet his spirit wife in trances and dreams. At first he can speak only through her voice in séances, and he depends upon her for his abilities to cure and detect witchcraft. Although on the spiritual plane, the relationship is not platonic and in time the spirit wife bears the novice a spirit son. When the son is old enough to speak through his father, he "makes a trail" for the medium's ultimate possession by a male lineage spirit, his *sigisato* spirit. Possession by a *sigisato* is a prerequisite for full status as a medium, since only this spirit can authenticate witchcraft accusations and authorize executions. About 18 percent of all men between ages twenty and forty-five are mediums, with most retiring from practice while in their forties. Illustrious mediums develop multicommunity practices and appoint many protégés before retiring from active practice to take up careers as senior secular leaders.

It is the mediums who, through frequent communal séances, keep the

cosmological system continually alive in people's experiences and thoughts. The system of representations that mediums dramatize as spirit voices constitutes Etoro religion, with the principal imagery of the séance alternating from representations of gender equality (as instantiated by the relationship between the medium and his wife) to representations of brotherly equality among men (the *sigisato* spirits). Since witches are held not to exist in the world of the egalitarian and immortal spirits, Kelly infers that Etoro religion proposes witchcraft as the real obstacle to equality, which is "idealized as the ultimate goal of human aspirations" (p. 336).

Secular leaders (*tafidilo,* from "to be first") organize community enterprises including the construction of longhouses, the coordination of raiding parties, and the hosting of ceremonial dances. Most longhouses have one or two *tafidilo*s, with the two leaders in the latter case working cooperatively as senior and junior. In a sample population of eight longhouses with a total of eighty-nine men aged over twenty, thirteen men were recognized as *tafidilo*s. Eleven of these were between the ages of forty-six and sixty, comprising 58 percent of all the men in that age bracket. To achieve *tafidilo* status a man has to avoid the stigma of witchcraft while accumulating prestige by being a successful and generous hunter. In one documented case, half of the game consumed over a year in a longhouse was provided by a single *tafidilo.* Recognition of a man as a *tafidilo* continues for only as long as he continues to hunt. The *tafidilo* in effect exemplifies behavior that is the opposite of the witch's: by giving meat to his coresidents he increases their life force, whereas witches "eat" the life force of their coresidents by consuming their spiritual bodies.

Relations between a longhouse's senior *tafidilo* and medium are cooperative. Senior *tafidilo*s and mediums together make up an elite that includes only twelve of the eighty-nine men in the eight longhouses surveyed, although taken together with junior *tafidilo*s and mediums, 29 percent of all men have some prestige status. Another 30 percent have been precluded from prestige status by witchcraft accusations, while the remainder will mostly achieve *tafidilo* status if they remain free from accusations and live long enough. Thus, although the prestige/stigma system is stratified, it is also (for men) open to advancement.[14] *Tafidilo*s and mediums derive no material advantage from their positions; neither have more wives on average than other men their age, and both are subject to witchcraft accusations at about the same rate as other men in their age bracket. Prestige and, particularly for the mediums, influence are the main rewards, since "egalitarian values and the levelling effects of witchcraft accusations" permit little else (p. 374). Surveying these findings, Kelly concludes that Etoro social inequality is "predominantly shaped" by the prestige/stigma hierarchy, which in turn is

shaped by the moral hierarchy, which is finally "grounded in" the cosmological system of ideas about life-force transmission (p. 376).

In chapter 6 Kelly analyzes data on the production, circulation, and distribution of wealth. Wealth items, mostly shell valuables, circulate as bridewealth payments and as compensation payments for witchcraft killings. Wealth is obtained by both trade and the direct production of items for trade (pigs, bows and arrows, hornbill beaks, tobacco rolls, sago flour), both activities being largely in the hands of young, unmarried men. Little of women's labor is involved in producing wealth items, and young men are able to raise bridewealth payments without dependence on either the labor of their female kin or contributions from male elders.

Bridewealth and compensation payments are distributed on the basis of generalized reciprocity among those of the distributor's kin who have contributed to or are likely to contribute to his own social payments. The distributor keeps little for himself. (Indeed, Etoro economic egalitarianism is such that no one should keep more than one of anything while others lack.) Kelly's data show that the net effect of these payments is redistributive. Wealth is accumulated from among the compensator's lineage kin on the basis of their ability to contribute and then redistributed among the kin of the compensated on the basis of relationship and need. Males aged twenty-one to forty contribute 81 percent of the total value of all contributions, but they receive back only 64 percent. Women and older men are, correspondingly, the net winners. Although women contribute little to the production of wealth, they receive 10.8 percent of its total distributed value. But, instead of using their valuables to invest in social payments as men do, women keep the shells they receive to wear as ornaments.[15] There is also a "modest transfer of wealth" from those accused of witchcraft to *tafidilo*s, who in turn contribute it to other men's bridewealth payments, thus furthering their own reputations for generosity. By contributing 68 percent more than other men to marriage payments, *tafidilo*s accumulate "the one thing that is readily accumulable, namely prestige" (p. 406). The main conclusions that Kelly draws from Etoro wealth distribution practices, however, are that (1) transactions of both shell wealth and pigs are disconnected from the means by which men gain prestige in the community[16] and (2) the existence of accumulable wealth doesn't necessarily lead to inequality, since the data show that an egalitarian redistribution of wealth (as of meat) is a possibility actually realized by the Etoro.

The larger part of the lengthy concluding chapter, "Theories of Inequality in Simple Societies," is devoted to a critique of Collier's brideservice model of gender inequalities in classless or "simple societies."[17] Kelly shows that Collier's assumptions fail to match Etoro realities and that her model

rests upon fundamental misconceptions. This excursus is justified on the grounds that Collier's is the "most comprehensive and influential current model" for understanding simple societies, but the arguments here are tedious and likely to be of interest only to those for whom Collier's model seemed plausible in the first place.[18]

The conclusion proper returns to the thesis that the cosmological system (and/or the "cosmologically derived system of moral evaluation," the two being largely interchangeable in Kelly's formulations) is (are) "the central locus for the production of inequality." Reviewing his arguments from previous chapters, Kelly concludes that the cosmological system "is central to the organization of production, distribution . . . and consumption" (p. 512) and is capable of "totalistically accounting for" the entire range of inequalities among the Etoro—with the sole exception of the jural prerogatives associated with making and responding to witchcraft accusations. These prerogatives are "organized in terms of a discrete logic, pertaining to the capacity to employ or respond to coercive physical force" (p. 515). The cosmological system is therefore not the sole locus for the production of inequality, although it remains the central one.[19] What is not clear is why the concluding analysis should recognize only this exception when other components of the sociocultural system have also been identified as independently causing inequality, namely the division of labor, the prestige system connecting meat provisioning with leadership, the initiation system, and the system of witchcraft beliefs and practices (pp. 286, 433, 447). Indeed, the "causal factors responsible [for Etoro social inequalities] originate in diverse domains and characteristically produce mutually reinforcing configurations of moral worth, prestige, privilege, stigma, and disability that overdetermine gender [and other] inequalities" (p. 448). If only Kelly had developed this formulation instead of reducing each system of practices to the cosmological system of ideas!

* * * * *

For Marxists, superstructures are never entirely autonomous; cosmologies do not simply descend from the heavens but are connected (often dialectically) to material conditions. Kelly's eventual assignment of a source, the shamanic elite, to the Etoro cosmological system thus answers the principal Marxist objection to what would otherwise be an unashamedly idealist project. But if the interests of the shamanic class finally construct the system, how does this differ from Marxist understandings of ideology in which the dominant ideas in society are understood as promoting class or elite interests?

Godelier's *The Making of Great Men* provided just such an analysis with respect to the Baruya (1986), an Eastern Highlands people with significant social organization and cultural features in common with the Strickland-Bosavi tribes (including male initiation, normative homosexual behavior, shamanism, and an absence of emphasis on ceremonial exchange).[20] Kelly cites this study but overlooks its analysis, thereby missing the chance of clarifying his argument through a comparison. What comparison would show, I think, is that although Kelly and Godelier start from opposed theoretical positions they nevertheless reach similar conclusions. Godelier characterizes Baruya society as a "Great Men system," in contrast to Highlands "Big Man systems." "He shows [to quote from my summary in Modjeska 1991:236] that the distribution of [the] means of producing social relationships (including the 'discovery' of [the next generation's] great men [by the current generation's leaders]) is the outcome, as it must necessarily be while social relations are produced by magico-symbolic rather than gift-economic means, of struggles for the possession of symbolic capital." "Struggles" here refer to the competition that Godelier documents between Baruya lineages for the control of sacred objects embodying the means of self-legitimization.

This is a point of substantive difference between the Baruya and the Etoro, since the Etoro lack a spiritual technology of sacred objects, substituting instead of legitimating rituals a self-legitimizing acting-out of the dictates of the spirit world. Competition between Etoro lineages is thus suspended in favor of the more general battle of all good men against the witches.[21] Although couched in terms of Marxian notions of symbolic capital, Godelier's analytic conclusions are broadly parallel to those of Kelly's cultural idealist analysis: (1) he who controls the means of symbolic legitimation controls the means of reproducing positions of dominance within the system, and (2) this is an entirely different kind of domination/inequality than that found in Highlands societies, where prestige is largely tied to performance in ceremonial gift exchanges that, in turn, depends upon pig production and finance. To the extent that Kelly has independently and from an opposed position arrived at similar conclusions there are, I think, good grounds for accepting them while remaining skeptical about any claims for the superiority of his theoretical orientation.

NOTES

1. Kelly gives a 1981 population of 750, implying an annual rate of increase of almost 5.5 percent!

2. There is apparently no such status as "ordinary person" among the Etoro.

3. Kelly isn't concerned with the Melanesianist literature on witchcraft and sorcery, nor with the terminological and other comparative issues involved. What he terms "witchcraft" is indigenously conceptualized as involving both innate and willful components, cutting across the usual anthropological distinction between "witchcraft" as innate mystical power and "sorcery" as the willful manipulation of paraphernalia and spells. On the terminological question in relation to Strickland-Bosavi concepts of mystical attack, see especially Knauft (1985:339ff.) and Shaw (1990:133 n. 5).

4. Since Kelly begins by defining inequality as "social differentiation accompanied by differential moral evaluation" (p. 4), his analysis necessarily proceeds to a conclusion in similar terms: "The Etoro cosmological system . . . is the central locus for the production of inequality because it constitutes the source of morally evaluated social differentiation" (pp. 511–512). The analysis is a Durkheimian *tour de force,* but Kelly doesn't recognize the extent to which his conclusions are determined by his starting point. A Marxist analysis would, of course, define inequality in different terms (the metaphysic of labor and of "[wo]men working for men"), leading to different conclusions.

5. Readers familiar with classical Marxism will recognize Kelly's "prime mover" as a truncated and rearranged variant of the original historical materialist formulation (e.g., Engels 1892). Rather than directly "producing inequality," the Marxist prime mover drives history, which progresses through changing modes of production upon which class divisions and struggles are consequent. The Marxist prime mover doesn't consist in "the dynamics of a particular mode of production," as Kelly supposes, but in the autonomous development of the forces of production through successive modes of production. In the Marxist motor metaphor, historical change results from the development of the productive forces, alternately facilitated and "fettered" by the social relations of production. Thus the arrival of the sweet potato in the New Guinea Highlands 250–300 years ago initiated a "revolutionary" development of the productive forces, because it made possible a production system in which women's labor could be effectively exploited (Modjeska 1995).

6. From a Marxist viewpoint, Kelly's identification of the "prime mover" of society with its least progressive element might seem ironic, but it is not thereby incorrect. Certainly there is no reason to doubt that the shamans really are the power elite of Etoro society. But it is misleading to say that they control the cosmological system of ideas. As (re)presenters of the spirit world, Etoro shamans strongly influence how community members interpret reality in terms of the cosmological system, but they don't thereby exercise control over the conceptual elements of the system itself. Kelly neglects to point out that Etoro cosmological ideas are but local versions of a system that dominates the spiritual thinking of other peoples far beyond their ken. The idea of the soul as a bodily double is, of course, universal throughout Melanesia, while even specific details in ideas about witchcraft are shared beyond the Strickland-Bosavi region by all the peoples of the Strickland headwaters (Bogaia, Duna, Bimin, Oksapmin, Hewa, and so forth, although not by the contiguous Highlands populations of Ipili and Engan speakers). The interpretation of the social world as a world of witchcraft proceeds from a doxic frame of reference that is beyond the reach of any individual to alter.

7. Kelly holds that the Etoro valuation of meat and their division of labor (which assigns most hunting to men, thereby making gender inequality inevitable) are due to their cosmological system. His culturalism does not extend to claiming that meat's value is inde-

pendent of its objective properties or that the division of labor rests on nothing but culture, but neither does he admit any possibility of material causation.

8. Marxists would note that prestige, unlike meat, is distributed according to the formula "from each according to their ability; to each according to the amount contributed," that is to say, according to the lower of the two forms of communist production relations as defined by the "from . . . ; to . . . " formulas.

9. Etoro pigs are able to forage for most of their food, and pig production makes little use of women's labor, in marked contrast with practices in the nearby Highlands where pig production requires intensive inputs of sweet potato fodder produced by women's labor. In my 1982 analysis of pig production and pork distribution among the Duna (who share common linguistic origins with the Strickland-Bosavi peoples but have a Highlands-style production system rather than a hunting-dominated economy), I arrived at similar conclusions as regards both the egalitarian consumption outcome of pork distributions and the largely ideological basis of men's monopoly over distribution as granted by the division of labor. I also emphasized that "at the level of exchange-values men are clearly the appropriators of the prestige and power created by the exchange of pigs" within male-created exchange structures (Modjeska 1982:85; cf. Kelly, p. 88: "Men, as the creators of this structure of exchange, realize the exchange value that is structurally derived"). Kelly emphasizes what he sees as clear-cut theoretical differences between our perspectives, but he neglects fundamental similarities in our analyses.

10. But also, it seems, by virtue of their past role as donors of seminal life force in homosexual relations with boys and young men.

11. No specific evidence is adduced by Kelly to suggest that sex-age categories are understood by the Etoro to occupy specific positions in a hierarchy of virtue. It emerges elsewhere in the text that a woman must be accused of witchcraft before she can be divorced, so it is also misleading to argue that the low moral standing of divorcées derives from their cosmological status qua divorcées.

12. Kelly points out that reaccusing a previously accused witch is usually the least divisive course of action for a longhouse group.

13. In his study of "sorcery" accusations and killings among the Gebusi (some twenty kilometers to the west of the Etoro), Knauft writes that the "unwillingness of kin groups to protect their members has perpetuated a pattern which has been . . . against each clan's—indeed against the entire society's—self-interest" (1985:355). This seems particularly perceptive when one considers in comparison the importance of clan/lineage solidarity in the face of violence in Highlands societies (see, e.g., Sillitoe 1980). But there's a catch: the Strickland-Bosavi "unwillingness of kin groups to protect their members" goes together with a normative residence pattern of dispersed lineages and coresiding affines. If men were to defend their lineage kin against attack, the solidarity of the longhouse group would be destroyed. Conversely, a lineage-based, more-or-less monocarpellary, Highlands pattern of residence would seem a prerequisite for a brothers-before-affines, Highlands-type social consciousness. Presumably because he is concerned with the analysis of inequality rather than witchcraft per se, Kelly does not mention Knauft's interpretation. Nor, for that matter, is he concerned with the broader consequences of the Strick-

land-Bosavi witchcraft/sorcery complex, which (as Knauft suggests) was apparently accelerating a population "implosion" towards extinction among the smaller tribes of the region immediately prior to colonial intervention.

14. Women can have a degree of prestige too. Senior women freely contribute to longhouse discussions and sometimes take prestigious roles in dancing. But women are "ineligible for the statuses that concretize prestige attainment" (p. 14); that is, they can become neither *tafidilo*s nor shamans.

15. Why don't women use their valuables as men do? Kelly does not provide any insight into what women (or men, for that matter) might say about this, but given his argument that an ideological hegemony prevails, one must suppose that the idea of women using valuables to build social relationships and prestige would be unthinkable for both sexes.

16. The contrast with the Highlands big-man system, where prestige hinges largely on skill in transacting pigs and other wealth, suggests to Kelly that "one of the key features of the evolution of social inequality turns on the development of an engagement between the prestige/stigma system and the economic system in which relations of production are instrumental to the attainment of prestige" (p. 412). Although Kelly does not suggest how this might have come about, my proposal has been that the basis for the engagement lay in the development of pig production based on intensive sweet potato foddering, thereby permitting women's previously underutilized labor to be harnessed for the production of male-controlled wealth.

17. Following Sahlins, Kelly initially defines "simple societies" as those in which age, gender, and personal characteristics provide the bases for social differentiation (p. 3). But elsewhere he contextualizes the category as the equivalent of Collier's "brideservice societies" (e.g., pp. 2, 473). Brideservice societies, however, are contrasted with "bridewealth societies" as subtypes of simple societies in Collier's typology. On the basis of his initial definition, bridewealth societies are also simple societies, yet Kelly implicitly excludes them from this status. This is further complicated by his classification of the Etoro as a "brideservice society," even though (as he points out) they do not follow brideservice practices and do have bridewealth.

Kelly subsequently redefines simple societies as those in which the labor of others cannot be readily turned to the pursuit of prestige and in which "control over metaphysics is the central mechanism" by which elites reproduce their positions (p. 516). In practice this limits the category to those societies of hunter-gatherers and hunter-horticulturalists where individual rather than collective hunting predominates. I would add that identifying a societal type on this basis is entirely consistent with a Marxist approach: technological limitations in the forces of production (the absence of means of concentrating women's labor in the production of game and pigs) necessitate, in the absence of other arrangements, that prestige be the reward of productivity in hunting. Prestige inequalities associated with secular leadership are thus a function of the (lower communist) mode of production: "to each according to his contribution." Control over metaphysics as the ultimate determinant of inequality among the Etoro is, however, only negatively determined by the limiting conditions of the productive forces. That is to say, mediums, séances, spirits, witches, witchcraft accusations, executions, and compensation payments are in large part the products of a semiautonomous ideological structure operating in tandem with, but not functionally necessary to, the continued operation of the subsistence mode. This struc-

ture is basically an imaginary, spiritual economy that, although entirely real from the Etoro point of view, is the basis for "false" relations of production and their consequent inequalities from the Marxist viewpoint.

18. Kelly (p. 521, quoting Modjeska 1982:51) misreads my remark that "the relations of production are relations of kinship" as an assertion that kinship generates relations of production, thereby aligning my position with Collier's. My position was (and is) that the relations of production in these societies are defined by the conjunction of the division of labor with the relations of kinship.

19. It is noticeable in these passages that "centrality" replaces Kelly's earlier formulations in terms of generative causality. From a Marxist perspective, apart from Kelly's initial pretense that the cosmological system has an autonomous existence, there seems little to choose between his notion of "centrality" and a Gramscian understanding of ideology as providing the grounds for action orientations, appropriate attitudes, and social cohesion. Neither conceptualization sits very well with the original base-superstructure spatial metaphor, of course, but even classical Marxism insisted on "reciprocal influence."

20. To clarify, although the Baruya live in the Eastern Highlands, they are not "Highlanders" in terms of their linguistic/cultural affiliations, nor in terms of subsistence and economic regimes.

21. Knauft, however, has argued that the underlying motive for witchcraft accusations among the Gebusi arises from imbalances between lineages in marriage exchanges. Although Kelly affirms that lineages are the real units of Etoro social accounting (p. 467), he doesn't consider whether Knauft's interpretation might also fit the Etoro case.

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The Father's Tears

After an Etoro father has distributed the marriage pig and the many shells given him for his daughter, he weeps profusely for hours in an expression of grief. He laments having accepted the "compensation" and wishes he could recover his daughter instead—but he has transformed her status by his act. As Kelly argues, the shells, like all compensation, are given to assuage grief. Unlike equations often made elsewhere in Papua New Guinea—and he draws contrasts with Highlands societies in particular—such shell valuables are not equated with rights in persons or regarded as substitutes for people or for people's work. Rather, they are given in response to emotions that the donor draws out of the recipient.

This observation about the Etoro comes in the course of Kelly's argument

about the construction of inequality. Collier and Rosaldo's classic paper (1981) was an obvious starting point in that (despite such marriage payments) the Etoro apparently fit many of the criteria of so-called brideservice societies in which transactions at marriage do not refer to the objectification of labor in either persons or valuables but require that labor be directly expended in the service of maintaining affinal relations. Where valuables do embody labor, as in so-called bridewealth societies, their accumulation sets up a very different social dynamic. In the former the groom is largely responsible for such transactions, while in the latter he tends to be dependent on his elders. Dependency has long been one of the factors that anthropologists have taken as a diagnostic for the level of egalitarianism they discern in such societies. Marriage is at the heart of this model: inequality between men and women is Collier and Rosaldo's primary concern, and they argue that the egalitarianism of societies such as Etoro holds only if one excludes marriage from view. The groom has more to gain than the bride, and it is the bride who loses her independence.

Kelly organizes his book as an extended critique of Collier and Rosaldo's position. Several of these propositions are upturned, above all that marriage is not central to the dynamic of sexual inequality and that it is not a gain/constraint for women and men alike. Other of the propositions remain, as we shall see. In the meanwhile, the significance of polemic cannot be overestimated—Collier and Rosaldo's argument animates the substance of this book. If I praise the creativity of Kelly's account, that includes the parent account: I come away with renewed appreciation of the brilliance of their argument at the time, and dazzled by the brilliance of Kelly's rebuttal. The latter has all the meticulousness of conception and attention to factual detail one has come to expect from Kelly; this book fuels a sense of the worth of the anthropological project.

The 750 Etoro (the size of a couple of Highlands clans) are clearly a people to work with. Kelly's book is extensive, its main messages restated in several different ways, its observations rich, and above all its statistics highly informative. My notes cover many pages, and it will be a long time before I have finished with it. Here I raise one small set of issues that seem to me of general interest.

The Father's Tears

The nub of Kelly's argument is an interpretation of the nature of transactions between men and women. He contributes to a debate that continues to reverberate across the Pacific, these days in the context of goods and money that the Etoro had hardly seen when Kelly made his first study—how

inequalities between men and women are tied into larger structures of inequality in society. It is often taken for granted that one basis of these inequalities must be access to the material products of labor, so it is important that ethnographers should remind us of other possibilities. If (as Kelly argues) Etoro inequality is built on a hierarchy of virtue, material equity cannot be a precondition for equality.

One could dismiss "virtue," as others have argued for "reciprocity," as an ideology that conceals the exploitation of labor. But the empirical evidence offered in this account prevents such an easy turn. One would be concealing too many important observations about the quality of social relations and the nature of body and spirit in Etoro thinking, as well as the substitutability of persons for one another. There is now a substantial collection of anthropological material from the Pacific on the contrast between societies where wealth items are (as in the Papua New Guinea Highlands cases I am most familiar with) and are not (as in Etoro, among others) presented as substitutes for person. What Kelly's work does is make strange, raise questions about, related constructs that have to do with growth and substance. These have not received the comparative attention they are due.

Why does the father weep and what are these tears? He weeps after receiving the marriage gifts, not before. While the compensation may have been to assuage his bad feelings at the prospect of his daughter's marriage, the sequence as such nonetheless reverses that in the *kosa* ceremony, which also takes place between affines (including on the occasion of a marriage). Here senior men weep in grief at memories elicited by a dancing display, and compensation follows. In this second context, we may note, older men avoid being recipients and it is the younger generation who demand the shells. Both payments are compensation *(su),* but the one takes place before and the other after weeping. Kelly specifically tells us that the bride's father is weeping in bereavement, because he cannot undo his action in receiving the shells. And on this occasion he cannot avoid being a recipient. Why should being a recipient cause anguish to an older man?

Here we can turn to the larger context of male responsibility that Kelly describes so vividly. At the heart of the prestige system is a moral hierarchy based on the generosity with which senior men deplete their bodies in order to engender the growth of the next generation. This includes the provision of game, the insemination of juniors and wives, the circulation of shell valuables. Men are life-givers. This is a male capacity that is also a virtue, and it is a virtue that transforms itself into certain prestige-giving acts simply not available to women. Male products (meat gained by hunting, semen, shells) have a prestige-producing potential female products lack. He argues that it is the underlying moral evaluation that is at the root of inequality between the sexes.

Younger men are those most anxious to assert to their generosity—both in the active dissemination of shell valuables and in the active insemination of wives and brothers-in-law. Shell valuables are equated with semen. Young men thus keep semen in circulation, directly and indirectly, to a much greater extent than older, middle-aged men who have already depleted themselves. Now, whereas the shells are tied into the growth of younger men—and they try to get rid of them as fast as possible in order to turn themselves from recipients into givers of life force—the shells become almost inactive in older men's hands. Kelly's statistics show that the ways older men have of getting rid of them seem reduced. The father, we might say, is an older man having to face in the opposite direction; from his being a lifelong bestower he is now identified as a recipient. And whereas up till now he has been maturing both son and daughter as recipients of his own life force (a child possesses life force as a consequence of the father's loss of it), both are taken away from him. The daughter's husband will inseminate a designated younger brother as well as her. The father instead receives the shells, a signal of what he has already expended. Perhaps the copious tears reiterate that expenditure, body fluid cascading from his orifices.

To weep after the receipt, then, suggests that the gift is in retrospect for what has been previously expended. If so, it seems possible to accumulate generosity, that is, to be seen to have been generous, which is the state of prestige to which men in their prime aspire. (*Tafilido*s, men of prestige, are inseminating many wives, contributing game to the community and shells to others.) Yet to be only a recipient is to be locked out of what is most worthy according to the moral system.

As Kelly reiterates, women—unable to disseminate (or inseminate)—are permanently locked out. They cannot display the "virtue of generosity." Instead, they are prone to displaying the "vice of accumulation." Or rather, even on those small occasions when they could give away what they accumulate, they often elect not to do so. Indeed, although there are public occasions at which both men and women receive valuables, Kelly points out the transactions are not the "same," for the valuables have different connotations for the two sexes. Women are in any case apt to transform them into body ornaments.

The Mother's Ornaments

Why, when women take valuables, do they wear them as ornaments? I wonder if this is not a stronger statement than Kelly makes of it (he uses it as an example of women's having valuables they do not use). I would doubt very much if Etoro men would want shells from a woman: they want first the receptacle and then the reduplicated receptacle in the child whom they

grow. If the woman offers a vehicle for the man's semen, as the child grows it in turn becomes a vehicle for the father's life-giving powers. Yet the child should not grow too well: the fat are witches. The body is no measure therefore of this kind of generosity, because it cannot be accumulated in the body—it can only show in the corresponding vitality of the young. So, too, the shells a man receives are nothing without receptacles (recipients) to receive them.

Men put themselves into the position of having to disseminate all the time—their theories of vitality, the nervous motility of shells, means that they *cannot* turn valuables into ornaments that stay on their skins. So in a sense the weight and dimension and solidity of bodies cannot serve as a metaphorical resource for male virtue. The ideal male body becomes a register of the state of the life force *(hame).*

In this connection we should consider Kelly's observation that there is no theory of female substance transmission. Like the female-produced starch that only sates hunger, and unlike men's game that causes growth, women produce children unable to grow without men's input. Men "complete" the work of women's childbirth by bringing girls and boys to maturity, through bestowing vital energy *(hame).* We might then ask if there is really a theory of male *substance* transmission—that is, is Etoro semen usefully regarded as a substance? The ornament on the woman's body perhaps invites us to ask what it is that women's bodies, by contrast, contain.

I have never forgotten the service that Collier and Rosaldo did in dislodging the figure of "the mother" from its Euro-American stereotype as universal life-giver. They wrote that "neither men nor women in very simple societies celebrate women as nurturers or women's unique capacity to give life" (1981:276). While they put this down to conjugal dynamics and intergenerational (in)dependence, that figure also holds a key to a wider comparative enterprise. Kelly notes that Collier and Rosaldo were right about the significance of the fact that women are not regarded as life-givers—Etoro men take on that role. However, instead of lingering on the strange figure of the mother, his focus is on understanding the moral hierarchy created by men's efforts (men, signally, do not need women to be generous).[1] Having decided that this hierarchy is a model of Etoro society seen largely from the perspective of the initiated younger men/spirit mediums, he observes that there is no female countermodel and more or less leaves it at that.

The men are modeling a procreative process. Not just about social reproduction, theirs is a model about the building up and depletion of bodies and the creation of persons. I think there is a more interesting set of issues behind the rather dismal-sounding conclusion that Kelly comes to, namely, that women give birth to death, lifeless flesh whose animation comes from men.

Collier and Rosaldo pointed to the expenditure of body energy in "bride-service" societies by contrast with systems where valuables represent the accumulation of labor. That requires a prior analogy (on the part of the actors) between body and output. One medium developed elsewhere in Papua New Guinea is an analogy between the substance of the human body and the substance of the valuables/objects. That is what we find in so-called bridewealth societies: a quite different corporal dynamic. In some, growing large is a sign of wealth and of growth, while in others (through consumption and abstention) ideal states alternate between fatness and thinness. Food production becomes available as a metaphor for maturation, and in that situation the products of work assume different dimensions. Indeed, perhaps *food* then becomes conceptualized as nurturant. Or rather, what goes into producing food as a maturation agent—that is, work—is conceptualized as a value in its own right. The parent's work thus becomes a social problematic, evinced (for instance) in lifetime payments to mother's or father's kin; despite affinal exchanges and initiates' payments to their mother's brother, maternally directed child payments are virtually absent in Etoro. Indeed, there seems no separate relational focus for female parenting that is not derived from male parenting. Kelly's provocative countercase holds much information about how one might reanalyze "bridewealth" regimes.

Labor and Life

The concept of "labor" occupies an interesting place in Kelly's book. As an economic category relating to the organization of work and the control that persons exercise over their activities, Kelly shows that its disposition cannot account for the inequalities evident in Etoro society. On the contrary, he reiterates over and again that the basis for inequality lies in the moral hierarchy of virtue. What is this moral hierarchy based on? It is based, as we have seen, on a notion of vitality, translated as life force *(hame),* which is the cause of bodily growth and energy, and which men transmit to others. Those who attain high evaluations are those who show their generosity through giving away such force. Men in their prime are both the recipients and donors of life force.

Morality displaces labor as the organizing force of this understanding of the Etoro. I see this as part of a much wider move in recent anthropological writings to dislodge morality from social relations. That might sound odd when Kelly's whole account is to show how it is the moral hierarchy that determines social differentiation. But I mean to point to the autonomous role that Kelly's apprehension of Etoro concepts would give to the value of virtue, epitomized at one point in his arguing that we must take semen transactions sui generis. Although semen may be likened to shells, he argues, we

cannot do an economic computation of the exchanges as though one were dealing with the exchange of material items. On the contrary, exchange also fails to fit the case Kelly wants to make. In particular, neither exchange nor labor will do as the pivots from which to think about relations between the sexes. Careful economic analysis of transactions between spouses, for instance, makes it readily apparent that it is not from the transactions themselves that inequalities between men and women arise. This means that any model based on the calculation of control over products of labor will lead to erroneous conclusions. One such model is precisely that based on the difference between brideservice and bridewealth societies, which considered the respective benefits and obligations that arise from conjugal interdependence. Kelly even goes so far as to challenge the concept of obligation as prior to inequality. At least in the Etoro case, he argues, it is not a preexisting structure of obligations that relativizes the contributions of men and women, but the prior determination of the relative value or worth of their activities. That in turn leads to an evaluation of moral worth based on the capacity for generosity.

One effect of Kelly's economic analyses (and he subjects several domains of activity to scrutiny) is that "labor" becomes leached of those very moral qualities that it held in those anthropological writings of the 1970s and 1980s that looked to property relations as the source of sexual inequality. He can find plenty of accounts of economic life that argue along the same variables as he does. But the context twenty years ago was different. As the outer edge of a vortex of interests created by Marxist anthropology, and in many cases as the inner edge of feminist reevaluations of women's work, the abstract concept of "labor" carried significant overtones. I do not think it would have otherwise held quite the analytical power it seemed to offer for the uncovering of total systems.

These overtones included the notion that labor is to be regarded as the expenditure of body effort, a disposition of energies in which a person finds his or her self-expression. The appropriation of labor thus had a particular purchase on the anthropologist's analysis: persons seem to be prevented from enjoying what would otherwise be theirs. In other words, there was an implicit morality to the concept of labor—tied into a sense of self-worth, to the realization of oneself through work, and to the implicit desirability of using work to ends that enhanced the worker. The products of labor were the social token and embodiments of that use of energy. Kelly, so to speak, extrudes morality from labor, rejects the inadequacy of an economic model, and finds Etoro morality embedded instead in a hierarchy based on virtue. This virtue is nothing other than the ability to deploy the effects of one's own energy. Energy is concretized in the notion of life force, its production, distribution, and consumption among persons. In the Etoro case, using this life

force to one's own ends means, for men, being able to give it away in order to attain what Kelly insists is the basis of the moral superiority that comes with generosity. One cannot compute material gains and losses: elevated persons are owed nothing but the acknowledgement of their moral standing.

What is curious is that despite this most persuasive ethnographic vision, the result does not give an unfamiliar picture. It is as though Etoro *hame* were doing in this analysis much of the systemic work that concepts of work and exchange have accomplished in other contexts. We still have men with a dual orientation, towards specific women and towards other men in general, whereas women seem more closely locked into relations with specific men. Social life still seems an alternation between states (cross-sex and same-sex sociality). Men still need wives in one sense, as they do juniors and youths, even though it is as receptacles for their life force rather than for contributions of food and work. They even reinvent in the anthropologist's mind something like the old division between brideservice and bridewealth regimes, even if the focus on marriage and wealth is proved misplaced. An intriguing question, then, is how much of a model has to disappear before it really is laid to rest.

There is a larger question being posed here. How men rather than women came to endow "life" is amply answered by Kelly. What remains to be asked is how, in other Pacific systems not based on such a moral hierarchy of virtue, "life" ever became associated with food/childbirth and parenthood, maternal or paternal, as nurture. The concepts of nurture and growth remain to be explored. For Collier and Rosaldo (1981:275–276), the issue is more than one of food supplies: the mother as nurturer, for instance, is a moral figure in the sense that worth, if not virtue, inheres in the productive and creative efforts that bring others to maturity and keep them in health. Under what circumstances does nurture emerge as a value on its own?

"Life" is a value on its own for some. Kelly's rendering of the Etoro holds great interest for the cultural apprehension of the "life" as in "life-forms." Life-forms have become a Euro-American object of thought in a late twentieth-century context where new genetic and reproductive strategies create new beings, such that the abstract concept of life itself acquires a moral tenor that makes such creations daring. The issue is the appropriate (correct, ethical, worthy) relationship of life to human (pro)creativity. Etoro men have built a whole system of social differentiations based on the access to the ability to transmit life. I wonder if they would recognize the new morality of intellectual property rights, which attribute creativity to those who bring a product to completion through their interventions. But in the Etoro case this is never in perpetuity: Etoro expenditure is directed onto other human beings who act, crucially, as recipients, but whose own subsequent expenditures perpetuate not the donor's original life but their own.

NOTE

1. At various junctures Kelly carefully distinguishes women's from men's viewpoints. Thus his argument about marriage transactions is that men in effect take a same-sex view and women, with their exchange of male and female food products, take a cross-sex view. However, he only rarely uses these terms. By and large in Kelly's account gender is present only when a difference between male and female is relevant. So when men and women do the same things, gender cannot be a variable—hence the denial that gender forms a structuring basis of productive relations! He is quite explicit about the complementarity of husbands and wives (and later, in discussing marriage transactions, he points to the commingled piles of sweet potato and taro in this light). But he implies that only when the activities of men and women are distinct, even "opposed," does gender operate as a principle of organization. Thus he delineates the salient gender configurations between homosexuality and heterosexuality principally in the sense of "sexuality."

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Rethinking Regimes of Social Inequality

I should begin by thanking Robert Borofsky for recruiting such an apt pair of reviewers for my book. I also very much appreciate the depth of thought and contemplation that is so strikingly evident in Marilyn Strathern's and Nicholas Modjeska's commentary on my work. The two reviews (which nicely complement each other) draw attention to a number of lines of inquiry that could usefully be pursued in further research, in the analysis (and reanalysis) of ethnographic data, and in comparative and developmental formulations. Consideration of these potential lines of inquiry is the main focus of this essay.

I am entirely in agreement with Modjeska's suggestion that a comparison of my analysis (in *Constructing Inequality*) with that presented in Godelier's *The Making of Great Men* (1986) would be theoretically productive. The heuristic value of such a comparison is enhanced by the divergent theoretical models engaged by each work. I was concerned with a controlled comparison of the linguistically related Strickland-Bosavi tribes, on one hand, and with Collier's general (and cross-culturally applicable) model of brideservice

societies, on the other hand. Godelier on his part confines his comparative purview to a number of cases that he sees as relevant to the great-man/big-man contrast and to Sahlins's original model of big-man systems (1963; see Godelier 1986:162–188). He does not consider any of the Strickland-Bosavi tribes in this context.

Comparison is invariably motivated, decidedly unpromiscuous, hostile to heterogeneity, and prescriptively endogamous (such that apples are compared to other apples). The emergence of a perception of the mutual relevance of the Etoro and Baruya cases and the invocation of the heuristic desirability of this particular comparison thus raises several interesting sets of issues. One of these concerns the grounds for the construction of a comparative universe: grounds that may be either linguistically or geographically regional, on one hand; or typological in terms of sociocultural characteristics such as form of leadership (big-man) or mode of marriage (brideservice), on the other hand. Both types of comparative frameworks are implicitly underwritten by assumptions that like causes produce like effects and that a convergence of cases thus reveals developmental processes and trajectories. Generally speaking, the selection of an environmentally homogeneous region as the bounds of a comparative universe would be likely to produce substantial convergence if economic and ecological processes drive social transformation, while a typological framework anticipates that core sociocultural features generate constellations of derivative attributes that constitute social formations composed of interrelated elements. Expectations concerning the sources of transformation are thus built into the choice of comparative framework. In calling for a comparison of the Etoro and Baruya, Modjeska invokes "significant social organization and cultural features in common" and specifically eschews a regional litmus test (see his note 20). It is noteworthy that the similarities he lists would all be considered components of the superstructure within the Marxian approach he favors.

The second set of issues concerns the theoretical framework that explicitly provides the conceptual terms of the comparison. Here Modjeska clearly advocates a Marxian framework. He sees Godelier's analysis as "couched in terms of Marxian notions of symbolic capital" that he considers illuminating. However, on rechecking, it seems that Godelier speaks in terms of "overall social logics" that entail modalities of exchange and discrete sets of interrelationships between kinship, wealth, and power that constitute distinctive forms of social hierarchy (1986:175). He does not utilize the concept of symbolic capital per se in this work, and the reframing of his analysis in these terms derives from Modjeska's retrospective perspective (1991).

It should be noted here that Knauft takes up the question of the framing of comparative analysis and the underlying assumptions this entails (1993:

117–128), as well as the applicability of Godelier's great-man/big-man typology to south coast New Guinea (ibid.:78–83). The general question of the utility of Godelier's analytic framework is instructively debated by Andrew Strathern (1995), Pierre Lemonnier (1995), and Bruce Knauft (1995) in a recent *Pacific Studies* book review forum. An earlier article by Andrew Strathern (1993) and a recent contribution by Biersack (1995:247–261) also provide relevant assessments. There is not sufficient space here to review all the perceptive points made in the course of this debate, although my response to Modjeska's suggestion builds upon these in a general way.

Modjeska perceives a broad parallel between my analysis and Godelier's in that both concern a type of reproduction of inequality that is "an entirely different kind of domination/inequality than that found in Highlands societies, where prestige is largely tied to performance in ceremonial gift exchanges that, in turn, depend upon pig production and finance." To my mind this formulation proposes that the character of the prestige-stigma system shapes the components and contours of the systemic generation of inequality. More specifically, the prestige-stigma system prefigures exchange requirements, which then strongly impinge upon production. This being the case, the prestige-stigma system is the logical point of departure for the analysis of social inequality (even though this confounds Marxian analytic protocol). Moreover, comparison grounded in similarities and differences in prestige-stigma systems is potent and productive precisely because of their constitutive character.

Modjeska touches on the contrastive difficulty of employing a conventional or unreconstructed Marxian analysis in his remarks on the "semiautonomous" nature of the superstructure in the Etoro case (see his note 17). It would appear that it is this lack of expectable articulations linking base and superstructure that prompts recourse to the concept of symbolic capital. In view of this it is not unreasonable to closely inquire into how the concept of symbolic capital articulates with an established Marxian framework, and to wonder if it does not constitute an appendage that serves an epicycle-like explanatory function. Thus, while Modjeska sees my analysis of Etoro social inequality as grist for reconstructed Marxian mills, informed by his retrospective perspective on Godelier's analysis of the Baruya, I see it just the other way around. What Modjeska subsumes under the label of symbolic capital is quite readily analyzed in terms of a prestige-stigma system, as *Constructing Inequality* illustrates. Moreover, the concept of symbolic capital does not readily encompass stigma, and the assignment of stigma (in the form of a witchcraft accusation) constitutes a critical arena of power. In short, I contend that the theoretical constructs developed in *Constructing Inequality* provide a better elucidation of the ethnographic data and of the phenomenon of social inequality than those recommended by Modjeska.

In the remark cited above, Modjeska identifies a specific configuration of prestige (tied to ceremonial gift exchange) as the crux of the distinctive forms of inequality found in Highlands societies. Taking this observation to its logical conclusion suggests the formulation of a typology of prestige-stigma systems that would serve as the scaffolding for comparative endeavor. One could readily envision how this might build productively upon prior comparative frameworks, including those that have focused on finance and production (A. J. Strathern 1969, 1978), production intensities (Feil 1987), fertility cults (Whitehead 1986), and overall social logics and/or exchange logics (Rubel and Rosman 1978; Godelier 1982, 1986; Lemonnier 1990, 1991).

Although Marilyn Strathern acknowledges the validity of my critique of Collier and Rosaldo (1981) and Collier (1988), she also endorses the continued utility of drawing a fundamental distinction between two types of societies analogous to Collier and Rosaldo's distinction between "brideservice" and "bridewealth" societies. I am in complete agreement with her on this point and in her assessment of the enduring significance of Collier and Rosaldo's project, namely, the development of contrastive models of social inequality applicable to comparatively unstratified societies (without classes or estates) cross-culturally. However, the question then arises: what theoretical vantage point would most usefully serve as the generative basis for this reconceptualization of contrastive regimes of social inequality?

Strathern proposes that the crux of the contrast between brideservice and bridewealth regimes that invites conceptual reincarnation is a difference in the purposes for which men need wives. Thus, among the Etoro, "Men still need wives in one sense, as they do juniors and youths, even though it is as receptacles for their life force rather than for contributions of food and work. They even reinvent in the anthropologist's mind something like the old division between brideservice and bridewealth regimes, even if the focus on marriage and wealth is proved misplaced." I concur in this view. What I would add is that the purposes for which men need wives are an artifact of the prestige-stigma system and the nature of the engagement between the prestige-stigma system and the economic system, including the articulation grounded in the division of labor. (Modjeska's notes 16, 17, and 18 are also germane to these points.) The theoretical vantage point developed in *Constructing Inequality* will thus serve equally well to forward the comparative projects advocated by both Strathern and Modjeska. More specifically, in *Constructing Inequality* I propose that one may identify a delimited category of societies that manifest comparable forms and dimensions of social inequality:

> [T]he prestige systems of the societies that are grouped together in terms of the employment of principles of categorization based on

> age, gender, and personal characteristics are of a distinctive type. Prestige is accorded on the basis of distinction in culturally valued activities in which personal qualities that betoken virtue are manifested. These are consequently activities in which an individual's own skill and labor contribution are discernible and can be distinguished from the contributions of others. Thus, male prestige is typically derived from activities that are predominantly or exclusively performed by males and that do not require direct female labor inputs. Prestige is often accorded on the basis of hunting success, generosity in distributing game, distinction in raiding and warfare, ritual performance, possession of sacred knowledge, and possession of spiritual powers, while stigma is typically linked to sickness-sending and the violation of taboos. Female access to the performance of culturally valued activities is often limited. However, a substantial number of societies with minimal forms of social differentiation have both male and female shamans, and a few have female ritual leaders (see, for example, Poole 1981a). When prestige-producing activities are open to women, the same principles of allocation apply in that prestige is linked to the individual's own skill and labor. Thus female prestige derived from shamanism is diminished when the efficacy of female practitioners is defined as contingent upon male contributions (see, for example, Godelier 1986: 120–21). This substantiates the centrality of the principle of individual effort. The general applicability of this principle is also evident from the fact that the prestige derived from hunting is significantly reduced or eliminated when hunting is collective. (P. 479)

I should note here that I treated the question of the central locus for the production of inequality as an empirical question, to be answered by an analysis of available data pertaining to all relevant domains of Etoro social life. I would likewise be inclined to pursue the task of reformulating a general typology of contrastive forms of social inequality by consideration of the ethnographic data rather than by generating types directly from theoretical models. My consideration of different forms of the division of labor among the Strickland-Bosavi tribes exemplifies the kind of comparative methodology I consider productive. This application generated contrastive constellations of features pertaining to the Etoro division of labor, on one hand, and that of the Kamula on the other, while at the same time showing that most of the ethnographic cases present a more balanced combination of features. In other words, the distribution of forms of the gendered division of labor among the Strickland-Bosavi tribes is not empirically bimodal, but more nearly ap-

proximates a continuum. Consideration of the contrastive polar cases at either end of this continuum is nevertheless quite instructive, because the two distinctive configurations of the gendered division of labor manifested do co-vary with differences in marital practices and conjugal relations. There are observable constellations of features (consistent with the concept of ideal types). At the same time, the continuum distribution of the bulk of the cases is also theoretically instructive. It is evident that a single sociocultural system may employ one form of the gendered division of labor in one domain of production and the contrastive form in another. The question of whether these are in contradiction (perhaps generating friction in conjugal relations) or represent complementary countervailing tendencies might then be investigated. The central point here is that we need to glean more from such expectable (non-bimodal) configurations of comparative data than merely registering their disconfirmation of an ensemble of structural types generated by a priori theoretical considerations. This might usefully include consideration of the degree to which cases may cluster at certain points along this kind of continuum, as well as consideration of the extent to which every logically derivable permutation is observed (or every point occupied by an ethnographic case). It might be more useful to envision the inquiry as a survey of the naturally occurring isotopes of the phenomena under investigation than as an attempt to locate the empirical manifestation of Platonic archetypes. Likewise, it is unstable intermediate types that may potentially have the most to tell us about processes of change.

In sum, what I am advocating is a systematic, case by case, empirically based ethnographic comparison of the prestige-stigma systems of Melanesia to determine what types emerge, to identify their distinctive features, and to chart the distribution of cases along a continuum of variations. We have good reasons to believe that this would provide a heuristically useful basis for reconceptualizing regimes of social inequality and thereby advancing our understanding of the phenomenon of social inequality, including the developmental aspects of it.

There is a rich tradition of focusing on prestige in Melanesian (and indeed Pacific) ethnography. Malinowski tells us in the introduction to *Argonauts* that what we will see in the ensuing chapters is the Trobriand tribesman "striving to satisfy certain aspirations, to attain his type of value, to follow his line of social ambition" ([1922] 1961:25). He also proposes that it is by focusing on the paramount interest of the Trobriander in the Kula that we will "grasp the native's point of view, his relation to life, to realize *his* vision of *his* world" (ibid.; emphasis in original). The imprint of Malinowski's conception of the task of ethnography carries forward from *Argonauts* up to the present and to some degree transcends the succession of theoretical

epochs witnessed over this period. If the Enga say "Pigs are our hearts!" then virtually any ethnographer of the Enga will likely take note of this particular cultural obsession in some publication, even if said anthropologist is himself or herself obsessed with the theory or issue *de jour.* This is to say that the ethnographic record is quite amenable to the comparative enterprise proposed.

These observations connect to Josephides's discussion of two approaches to the writing of ethnography, and her reference to the parallel philosophical distinction between "theory-centered" and "ethnography-centered" approaches. I agree with her that an account of social practices is essential to both. Sanday's suggestion that ethnographic inquiry focus on the concrete performances and activities of everyday life resonates with this as well. However, I would extend this to encompass Malinowski's emphasis on the obligations, duties, desires, aspirations, and obsessions that animate the diverse social actors engaged in these practices (and carry this further than Malinowski did). I have attempted to do this at many points, including my discussions of (1) the mother's, father's, and initiate's perspectives on the culminating act of anointing the initiate's head with tree oil; (2) male and female perspectives on key myths; (3) male and female responses to the prospect of violent retribution for witchcraft; (4) the meaning of witchcraft compensation to the alleged witch and the deceased's next of kin (respectively); and (5) the father's weeping upon receipt of bridewealth that Marilyn Strathern ponders in her review. Strathern's concern that we examine "labor," "work," "effort," and "nurture" in terms of local meanings (rather than imported assumptions) entails parallel objectives (and she has contributed to this enterprise in *The Gender of the Gift* [1988]).

The questions anthropologists address have often derived from interests shared with a wider audience and resulted in widely read books. (The works of Margaret Mead readily come to mind.) This is certainly consistent with anthropology's role in liberal education and is as it should be. However, the questions that arise in the mind of the nonspecialist (whom we can imagine as a student for the purposes of this discussion) often require reformulation. For example, the student may wish to have his or her predilection that all human beings are motivated by economic self-interest confirmed by Melanesian ethnography. We would then need to turn this into a question of the Melanesian actor's motives and objectives in engaging in exchange. But the difficulty with the nonspecialist's questions about gender equality and inequality is that such queries are more resistant to such reformulation and the reader (or student) may not be so amenable to the disconfirmation of prior beliefs. Thus, although Economic Man—as a modal personality type (Devereux 1978)—can be exorcised and sarcastically debunked, this is not

equally true of Universal Woman, likewise a bundle of assumed motives and desires. Indeed, there is no small irony in the fact that the Trobriand Islands served as the site for both the deconstruction of Economic Man and the fabrication of Universal Woman at successive points in the history of anthropology (Malinowski [1922] 1961; Weiner 1976; M. Strathern 1981).

The potential difficulty posed by a theory-centered approach seems to me to be more readily resolved by recourse to comparative theory. An ethnographically centered theory, built up upon a foundation of systematic comparison of ethnographic cases, provides a receptive ground into which to anchor an ethnographic account. In this respect I do not concur in Montaigne's view (noted by Josephides) that a rich contextual perspective based on case material is necessarily antithetical to general theory. The difficulty tends to arise when exotic models (e.g., African models) are imposed on the ethnographic data, or when the data are forced into a framework associated with a theory developed through the study of other types of data by other disciplines (e.g., economics). Although encounters with exogenous models and theory may be productive, the ethnographic monograph may not be the ideal genre in which to realize this potential. The difficulty in this case is not with a theory-centered approach per se, but rather turns on an incompatibility between genre and theory, and is a difficulty that arises only in the case of certain exogenous theories that the genre of the ethnographic monograph cannot easily be stretched to accommodate without coming apart at the seams. Exploring the exogenous theory or imported model in an article with suitable disclaimers might then solve the problem.

There is not sufficient space to directly engage Lepowsky's stimulating monograph and do justice to the task. However, I hope to have furthered discussion by taking up some of the issues raised by the reviewers of her book, as well as the reviewers of mine.

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Gender, Egalitarian Societies, and the Writing of Pacific Anthropology

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Writing Pacific Islands Ethnographies

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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