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GRAUN BILONG MIPELA: LOCAL LAND COURTS AND THE CHANGING CUSTOMARY LAW OF PAPUA NEW GUINEA

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Despite repeated attempts by the colonial administration to change the nature of traditional land tenure, Papua New Guinea attained independence with approximately 97 percent of its land still held by indigenous Papua New Guineans, and the rights to use and transfer that land still governed primarily by customary law.¹ The customary land-tenure rules of each of Papua New Guinea's more than seven hundred language groups are woven from a complex web of traditional norms, kinship relations, and social obligations. Customary norms about the acquisition and use of land have their roots in precolonial Papua New Guinea, but they have changed over time, in part to meet changing economic and social conditions.

The economy of Papua New Guinea was, and is, predominantly agricultural. More than 85 percent of the nation's adult population lives in rural areas. The precolonial subsistence economy was characterized by slash-and-burn cultivation in which plots of arable land were developed as food gardens for a few years and then left fallow for as much as a generation. Even today, those Papua New Guineans who obtain all or most of their subsistence from traditional gardens significantly outnumber those who depend primarily on cash cropping or urban employment. Although cash crops account for 35 percent of gross domestic product and are the primary focus of the government's agricultural research and support services, subsistence gardening on customary land continues to be a mainstay of the economy.² The myth that every worker has custom-

ary land to which he or she can at any time return permits the formal sector to pay low wages and the government to offer few social services.

Given the economic dominance of agriculture, it is not surprising that land is, and has always been, centrally important to most Papua New Guinean cultures. But, with changing economic conditions and changing uses of land the cultural meanings ascribed to land and the relations of persons to their land change as well. In traditional Papua New Guinea, the clan and the land are one. *Graun bilong mipela* ("the land is ours") transposes to *mipela bilong graun* ("we are the land's"). The land feeds the people who name it and its features. Many Papua New Guineans believe that land cannot be alienated from the clan. It belongs not only to the living but to their ancestors and descendants as well, and they belong to it. Land is not a commodity that can be bought and sold but a source of clan identity. It is also a source of shelter and subsistence, and, as such, the rights of individuals and households to use clan land can be, and frequently are, redistributed to take account of changing needs for land and changing social or political relationships. In a market economy, land takes on different connotations. It becomes a source of wealth for individuals and households who use it to plant cash crops or for logging, mining, or industry. Like any other product, it can be sold or leased for immediate gain. It loses permanent identification with the clan but gains another kind of permanence in the finality of freehold titles and sales.

Although subsistence gardening continues its central role, everyone in Papua New Guinea has been touched by the market economy. There are few areas of the country where some cash cropping is not going on. Mining and timber companies compete for access to customary land. The meaning of land for the people it supports, and the relationships of individuals to one another and to the land, are growing more complex. Customary law, which was predicated on the premise that land provides subsistence, must deal with situations in which land provides wealth. Customary law was developed to be flexible, to take account of shifting gardens, changing household needs, and shifting sociopolitical alliances; now it must deal with individuals and groups who want the law's decisions to be final so that they can assert permanent control over areas of land. In precolonial times, customary law seldom had to deal with population pressures, absentee landowners, landless workers, the use of prime gardening land for cash crops or other industries, or ecologically disastrous agricultural, mining, or logging practices.

Because customary law is, and always has been, constantly changing to meet new needs and conditions, it probably can change again to resolve the conflicts (both those between disputing claimants to land and those between differing views of what land is and how its uses should be managed) that have arisen as a result of these new pressures on land. However, not everyone agrees that these issues should be left to custom to decide. During the colonial period, a parade of administrators and experts attempted to transform Papua New Guinea into a market economy. They announced that land should be converted into a commodity, that market notions of individualized ownership and control should replace communitarian values, and that interests in land should therefore be removed from the aegis of customary law and governed instead by the statutes and common law that the colonial authorities had imported into Papua New Guinea.³ Although this view has not been put forward so starkly since 1971, when the colonial authorities' last attempt at wholesale conversion of customary land into individualized freehold ownership was defeated by Papua New Guinea's first elected legislature,⁴ many of the people influential in today's government are among those who would benefit if conversion were implemented, and recommendations for the registration of customary land are still being advanced.⁵

Even if customary land is not formally converted to freehold, there are many pressures to mold custom in the direction of individual ownership and a market approach to land, and few powerful voices in opposition. Papua New Guinea's is a pluralist legal culture:⁶ customary law, the common law enunciated by the formal courts, the statutes and regulations of national and provincial authorities are all sources of state law (the law formally recognized and enforced by the courts and other state institutions). Customary law itself is not unitary; there are as many different customary law regimes, with different rules and different legal processes, as there are clans and cultures within Papua New Guinea. And, if the law is defined to include all the norms that govern behavior rather than just those applied by the state's formal institutions, then there are additional sources of law in Papua New Guinea. Kinship affiliations, church groups, women's groups, workers' and growers' organizations and other social groupings, formal and informal, long-term or fleeting, also serve as sources of the norms that govern the lives of their members or adherents. Legal pluralist theorists posit that each of these sources of law will influence and change the others.⁷ But the impact is likely to be greater when a more powerful source of law attempts to

influence a weaker source, as, for example, when the common law courts, backed by the power of the state, attempt to change customary law.

During the colonial period, the Anglo-Australian legal system was introduced into Papua New Guinea and became, with few exceptions, the law applied in all formal courts. Although the colonial authorities intended that state law replace customary law, custom continued to operate informally in the villages.⁸ In fact, since few Papua New Guineans were parties to formal court cases (other than as criminal defendants), customary law continued throughout the colonial period to govern the lives of Papua New Guineans more directly and to a greater extent than did the imported laws. But state law influenced custom and continues to do so.

Often, state law's influence is indirect and unplanned. Sometimes, however, the attempts of the courts to influence custom is overt, although the results of that influence may not be precisely what the courts intended. This article charts the attempts of Papua New Guinea's National Court to change customary land law and the policies underlying that law. The judges of the National Court say, in the written opinions or judgments that accompany and justify their orders, that they are concerned merely that the land courts, which apply customary law, do so correctly. But the law inheres as much in process as it does in substance, and the attempts of the National Court to alter the procedures of the land courts will result in substantive changes to customary law. Moreover, because the procedures recommended are those of state law, acceptance of these procedural rules will move the land courts in the direction of state law. The substantive rules of state law, as well as the procedures of state courts, were developed to support a market economy, so, to the extent that customary land courts adopt state law processes, they are contributing to the redefinition of customary land as a marketable commodity.

However, customary law is not without its own ability to influence Papua New Guinea's legal values and processes. Customary law may not have behind it the unalloyed power of the state apparatus, but it has nevertheless had an impact in shaping the decisions of state courts.⁹ And, even where state law has influenced customary rules or procedures, that influence has been refracted through a customary lens, so that state law principles emerge in a customary setting with different meanings and different consequences than they have when applied in state courts.¹⁰

Customary Land Law in Papua New Guinea

In precolonial times, each of Papua New Guinea's many cultures was an autonomous social and political unit that developed its own norms and customs, so that customary law is as varied as the variegated landscape of this island nation. It is possible to make some generalizations about land law in traditional Papua New Guinean societies, but none that I will make in this brief overview of customary land law is true for every clan or village.¹¹ Moreover, customary law is by its nature adaptable. In response to changing circumstances, it has changed considerably in the last century, and I make little attempt here to distinguish between immemorial and newer customs.

Custom and state law treat the relation of people to land very differently. Under Anglo-Australian common law, land is property. Land can be owned in freehold, which means that an individual, corporation, or group can have a virtually unfettered right unilaterally to determine who uses the land and for what purposes, together with the right to all profits and products obtained from it, as well as the right to sell or otherwise dispose of any or all of these interests in the land. The notion of land as property did not exist under customary law. It is truer to say that the clan belongs to the land than that the land belongs to the clan. If the right of a clan to its land can be equated to any state law concept, it is more accurate to say that a clan has sovereignty over its territory than to say that it merely owns the land. In most Papua New Guinean societies, the claim of the clan, subclan, lineage, or village to its territory is based upon original settlement, though conquest is another of the recognized means of obtaining territory.

The rights of clan members to use clan land does not derive from purchase but from their putative membership in the clan.¹² The nature and duration of use rights varies according to the uses to which the land can be put. The land for village meetings and feasts is open to everyone in the group, as usually are hunting or foraging lands and thoroughfares. Gardening land and house sites tend to be under the control of the households or individuals who cleared and planted them. Because gardens must lie fallow and land productivity varies from place to place, households tend to have rights to a number of small garden plots scattered throughout the clan's territory. Usually, the household that planted a garden has the exclusive right to its products, but, just as often, someone else may have a claim to some of the coconut palms or other plants in the garden.

Rules of succession to a household's plots vary widely and are capable of much flexibility. In some societies (particularly those on the New Guinea islands), interests in land pass matrilineally; in others (particularly those in the Highlands), patrilineally. But a household is generally free, within the bounds of acceptable behavior, to make different arrangements for its members. Even in patrilineal societies, a son-in-law may choose to live with his wife's family and will probably be given gardening land. If the concept of inheritance connotes (as it does under the common law) that heirs receive their shares in the land upon the death of the testator, then inheritance is not an important concept under customary law. The right of children to a household's plots accrue when children are born. In most societies, a household head will assign garden plots to sons or daughters as they come of age or marry.

As a general rule, an individual's rights to land arise from membership in a kinship group. But rights to land also depend upon being an active and participating member of the group, and thus can be lost by clan members if they move away for too long, or acquired by outsiders if they move in and make themselves useful. A person may move to another clan's territory to take care of an aging relative who has no children, to live near a friend, or to live far away from an enemy. Eventually, if the person contributes to the life of the adopted clan, the gardening land once loaned to him will become his, or his children's. Sometimes a clan will permit the members of a neighboring group, which is land poor or which has been driven from its land in war, to settle on clan land. The land may (or may not) eventually come to be viewed as belonging to the neighboring group.

Customary law processes permit much flexibility in the choice and application of these substantive rules, so that rights to land can change to meet changing conditions and changing needs. The processes by which substantive rules are recognized and applied can best be seen when disputes call the rules into play.¹³ In many clans or villages, if the parties cannot settle the dispute themselves, a meeting may be called and big-men or elders may attempt to mediate. The parties to a dispute will call upon the substantive rules to support their claims to the land. Perhaps one is the son or daughter of a deceased landholder and the other cared for the landholder in her old age. There will be much discussion by everyone present of which rule should apply, of the customary practice in cases of this sort, and of the equities of each party's situation. Eventually a solution may be reached. The solution may well be a compromise because, in a society where acquiescence is the only means of rule enforcement, there is no solution without the agreement of the

parties. In this process, substantive rules serve a dual function. The rules demonstrate that the parties have a claim to the land, but the existence of mutually contradictory rules permits decisions to be made that serve the needs at the time of the disputants and of the group as a whole.

Of course, customary disputes are not always resolved by mediation, and mediation does not always produce a compromise. Custom varies from place to place, changes from time to time, and is more complex and variegated than the compromise (or any one-dimensional) model suggests. Mediation occurs in many villages, but in other villages a leader may, after listening to the parties, declare a decision. Sometimes one party simply has the better of the argument, either because of superior strength or because customary norms favor that party's position.¹⁴ And, often, customary disputes are not settled at all or are seemingly settled only to arise again. The immediate result of many disputes, particularly those between clans, may not be compromise but heightened conflict, even war.¹⁵

Land disputes between members of a clan or lineage are relatively amenable to mediated settlement. Disputes between clans are less susceptible to mediation until war or the threat of it has occurred. Fewer crosscutting ties and fewer ongoing relationships exist to impel the disputants (or their supporters) towards resolution. Disputes between clans escalate into warfare more regularly than do intraclan conflicts. Clans resolve their territorial disputes by political, rather than legal, means. Negotiation may take the place of warfare, or it may take place as a result of war.

The resolution of a dispute, whether within or between clans, is seldom a permanent determination of the status of disputed land. It decides merely which party will have which interests in the land for the time being. The dispute may be reopened at any time the circumstances of the parties change or either comes to regret the solution. Moreover, others may develop claims to, or needs for, the same piece of land and may ask for its status to be determined anew. In a Papua New Guinean village, the need for land is constantly changing: old gardens need to lie fallow and new ones planted; family members are born, die, or move away; a daughter, once thought married and no longer needing clan land, returns with her children; members of a neighboring clan, fleeing from a war perhaps, request shelter and use of the clan's lands; a son-in-law decides that he would rather live with his wife's clan than with his own. The choice of one party's claim over that of another is not a decision that the rule invoked by one party is valid and the other not; both rules continue to be available to parties in future disputes.

It has been suggested that customary law cannot meet the challenges posed by Papua New Guinea's developing market economy, that land disputes were more amenable to a negotiated resolution so long as land was plentiful and its uses confined to subsistence. With population increases, the expansion of a market economy that turns land into a money-making commodity, and the loss of gardens to cash crops, conflicts over land may become more difficult to resolve through negotiation, and once settled conflicts may be reopened. When land takes on market value, clans that had long ago permitted another clan to settle on their land revive old claims to it, and individuals remember debts that distant cousins had promised to repay in gardening land. In addition, new circumstances have brought new kinds of transactions in land. Papua New Guinean villagers who live near urban centers have "leased" customary land to migrants from other parts of the country; in the Highlands, some Papua New Guinean coffee growers pay compensation for the use of gardening land.¹⁶ Traditional rules of customary law must be reworked if they are to accommodate new kinds of land dealings.

However, neither the difficulties of achieving a resolution when land has begun to take on market values nor the existence of new uses of land requires that customary laws and processes be supplanted. If anything, customary law may be better able to solve the problems caused by the emerging market economy than is state law. Customary law's insistence that land should be available to those who need it is a necessary counter to the market's tendency to foster divisions between those who become land rich and those who become land poor. And, customary law's recognition that no decision about land is ever final permits the status of land to change as people's needs and circumstances change, a valuable flexibility not available under state law.

Papua New Guinea's Land Courts

Papua New Guinea's special courts to hear disputes over customary land were established in 1975, the year independence was achieved. Three factors contributed to Parliament's decision to create customary land courts and shaped the form those courts would take. First, the move towards independence increased interest in replacing as far as possible the imported common law with customary law, which was seen as home-grown and therefore better suited to the values, needs, and conditions of the new nation.¹⁷ In the exhilaration of independence, the proponents of the land courts did not stop to consider how customary law,

which had been developed when Papua New Guineans lived in small, technologically simple, and essentially egalitarian societies, might be adapted to solve questions of land ownership in a nation-state with a rapidly developing market economy and the beginnings of unequal distribution of resources. Nor did they consider the extent to which the vast socioeconomic changes of the colonial period, as well as the pressures of colonial rule, might already have changed customary law into something very different from its precolonial manifestation. These issues were left for time and the land courts to work out. Custom (whatever it might be) had taken on symbolic importance as an exemplar of independence and self-rule.

A second factor leading to the establishment of the land courts was the perception that tribal fighting seemed to be on the increase. Conflicts over rights to customary land are often cited as among the major causes of tribal warfare, and these conflicts were growing in number as population increases produced land scarcity at the same time that gardening land was in demand for cash cropping.¹⁸ The concern over tribal fighting led to the creation of a Committee of Inquiry into Tribal Fighting in the Highlands, which in its 1973 report agreed that land disputes were a frequent cause of tribal wars. The report recommended new procedures, including mediation, for resolving these disputes, rather than merely “hand[ing] down decisions in a purely judicial manner. People charged with settling land disputes should make a point of actually visiting the land in dispute and then attempt to mediate on the spot and arrive at a decision acceptable to the disputing groups.”¹⁹ In effect, the committee was recommending that customary processes replace state law processes in the resolution of land disputes.

Mediation is one of the dispute-management methods associated with custom, whereas adjudication is a hallmark of the common law method. However, mediation is only one among the processes available to customary law and there is no evidence that it leads to lasting resolutions. In presuming that mediation was the principal customary dispute-settlement method even between different clans, and that it usually led to permanent resolution of disputes over land, the committee was partaking of fallacies common at the time. As part of the fervor of independence, customary law had been mythicized. Customary law was portrayed, in contrast to the imported common law, as invariably community-centered, compromise-oriented, and leading inevitably to the restoration of social harmony.²⁰ The differences in the processes used to resolve intraclan and interclan disputes were ignored. Although mediation and compromise were significant among the methods, goals,

and values of customary law, there were many instances in which mediation was not used, in which compromise was not a goal, in which amicable relations were not restored, and in which fighting was a necessary prelude to or substitute for negotiation.²¹

The third factor leading to the creation of customary land courts was the recognition that no governmental agency existing at the time was able to bring customary land disputes to an end. Over the years the colonial administration had tried various institutional measures, all unsuccessful. Through 1952, responsibility for settling disputes over customary land lay with the courts for native affairs. Most disputes unresolved by the villagers themselves, however, tended to be heard by patrol officers (called, in Papua New Guinea, *kiaps*) on visits to the villages in their far-flung districts.²² In 1952, the Native Lands Commission was set up to investigate and record customary rights in land, with the ancillary purpose of determining which land was "waste and vacant" (or ownerless).²³ Colonial authorities believed that much land in Papua New Guinea was ownerless, and that, once it had been identified, it could be taken over by the state and sold or leased to expatriates for development as plantations. The commission was also supposed to create a register of all occupied land, a preliminary to making land available for market agriculture, lease, or sale. In its twelve years of operation, though, the commission did not identify any unowned land. Nor did it determine the ownership of much occupied land. It decided only 176 cases and registered no titles to customary land.²⁴

In 1963, the Native Lands Commission was replaced by the Land Titles Commission, which was given exclusive jurisdiction to decide rights to customary land. The colonial administration had reluctantly realized that Papua New Guinea's economic future did not lie in plantation agriculture and had decided instead to promote development by convincing indigenous people to grow cash crops. It was a basic tenet of the prevailing ideologies of the time that customary land tenures based on communal rights to land were a barrier to economic progress. So, the decisions of the Land Titles Commission on ownership were intended to permit clans either to record their title to the land in a register of communal titles or to divide up the clan land and convert to individual freehold titles. But, although the Land Titles Commission did resolve a number of land disputes, very few titles, either communal or individual, were ever registered.²⁵

Neither of the colonial land commissions had succeeded in settling many land disputes or in stemming the rising tide of conflicts over customary land. There were a number of reasons for their failure. Perhaps

the most important was that the colonial authorities' major purpose in creating them had not been to settle disputes between Papua New Guineans but to establish and register title to customary land so that it could be converted to market uses. Even after the failure of the first of the commissions, colonial authorities continued to believe that rights to most customary land were undisputed. The commissions were therefore structured more to serve the administrative function of ascertaining and recording ownership than to fulfill the adjudicatory function of resolving disputes over ownership.

The failures of these colonial institutions led to the formation shortly after the introduction of self-government in 1973 of a Commission of Inquiry into Land Matters, chaired by a Papua New Guinean. A significant focus of this commission's recommendations was on resolving land disputes. Its report identified certain shortcomings of the Land Titles Commission's dispute-resolution process and suggested, as had the Committee of Inquiry into Tribal Fighting, that top-down adjudication be replaced by a party-centered, mediatory approach, such as was believed to exist in customary law:

We think that certain principles should be used in developing a dispute settlement structure suitable for Papua New Guinea. People should be involved in the settlement of their own disputes and not be able to avoid this responsibility by referring the matter to the *kiaps*. . . . No dispute settlement process, no matter how wisely conceived and appropriate, can succeed until the disputants themselves are prepared to take some responsibility in the settling of the matter, and, if they cannot settle it, are prepared to abide by a decision of a tribunal set up by the Government.²⁶

The Commission of Inquiry into Land Matters was correct in suggesting that land disputes would not be resolved by autocratic or disinterested decision-making in which the parties could take no meaningful part. But, like all the commissions and committees that had preceded it, it was incorrect in presuming that correction of this problem would automatically lead to the final resolution of disputes. In presuming finality, it misunderstood the workings, and the goals, of the customary legal process. Customary dispute management provides for changing circumstances by presuming that any decision is temporary at best, capable of being reopened. A return to customary dispute-management processes provides a number of benefits for Papua New Guinea, but sure and cer-

tain outcomes that the parties will not try to overturn as soon as they grow dissatisfied are not among them.

The reports of the two commissions were influential in the enactment in 1975 of the Land Disputes Settlement Act, which established the land courts and provided that they would apply substantive customary law using customary dispute-settlement methods. The land matters commission had recommended "a three-stage system of mediation, arbitration and appeal."²⁷ The act provides for local people with knowledge of land matters and customary land law to be appointed as full-time or *ad hoc* mediators, and requires that all disputes be mediated. A dispute may be brought to a local land court for adjudication only if the parties have, in the opinion of the mediator, "made reasonable efforts to reach agreement but have been unable to do so." Each local land court consists of a magistrate of a local court (the local courts are the lowest-level trial courts in Papua New Guinea's hierarchy of common law courts) or a district officer, sitting with up to four mediators. Decisions of the local land courts are by majority vote of the magistrate and mediators. The choice of magistrates from the local courts to adjudicate customary disputes over land runs the risk of removing the land courts from custom, since these magistrates are not necessarily native to the areas where they sit as judges. However, this risk is offset by the presence on the panel of mediators who are from the area; additionally, because by 1975 local courts were already operating throughout Papua New Guinea, the use of their magistrates had the advantage of permitting land courts to be operational quickly throughout the country. The act permits appeals from decisions of the local land courts to provincial land courts, which are constituted by district court magistrates (the district courts are the level immediately above local courts in Papua New Guinea's common law court hierarchy). Provincial land court magistrates may sit with land mediators if they wish, but at this appellate level the mediators act only as advisors to the magistrate and do not have a vote in the decision. The appeal is usually a complete rehearing of the case.²⁸

The act highlights in many ways its intent that disputes over customary land should be resolved not only according to the substantive norms of customary law, but in light of the values and beliefs that were believed to underlie customary law as well. Thus, section 1 provides:

The purpose of this Act is to provide a just, efficient and effective machinery for the settlement of disputes in relation to interests in customary land by--

- (a) encouraging self-reliance through the involvement of the people in the settlement of their own disputes; and
- (b) the use of the principles underlying traditional dispute settlement processes.

Mediators are reminded that their primary function is “to assist in the attainment of peace and harmony . . . by mediating in, and endeavouring to obtain the just and amicable settlement of, disputes” (section 15). Recognizing that in customary forums the discussion is never limited to the issue that is the immediate cause of the dispute but is allowed to range over all the matters on which the parties disagree, the act provides that the local land court may hear and decide other issues that are “inextricably involved” with the land dispute (section 29). The local land courts are not bound by any of the common law rules of evidence, practice, or procedure; are free to call and examine any witnesses they think appropriate; and may inform themselves on any question before the court in whatever manner they think appropriate.²⁹

The act recognizes that customary law favors outcomes that are fair to the parties--taking into account their needs, interests, and relative social positions--over decisions based on the rigid application of rules, and permits the allocation of land to be changed when appropriate. Land courts are directed to apply the customs of the area, in regard both to interests in land that are recognized by custom and to the processes by which these interests are allocated or reallocated. Thus, a land court may order a party with an abundant supply to return land to another party that is short of land, if at some time within the past hundred years the land-short party had an interest in that land. Land court orders may also include provisions dividing the land, ordering it held in common, or requiring the payment of compensation or the giving of a feast. After twelve years from the date of a land court’s order, a party may apply for a variation of the order if the party can show that “circumstances have changed so that the enforcement of the order is causing hardship.” The act requires magistrates and mediators to visit the land, both before making their decision and, after the decision has been made, to mark the boundaries and “satisfy [themselves] that the parties and the witnesses understand’ the scope and nature of the court’s decision.”³⁰

The act differs from customary dispute-settlement processes in that mediation is the only one of the various customary responses to disputes recognized. Moreover, the act presumes that local land courts should

mediate not only disputes involving members of the same clan but also disputes between clans. Although negotiation of disputes between clans was probably little tried, and seldom successful, in precolonial Papua New Guinea, there is no reason to presume that it would not succeed today. In precolonial times, each clan was a separate and sovereign polity. Clans did not share the same leaders, were not subsumed under a common political structure, and were not constrained institutionally from interclan war. Today, however, Papua New Guinea's myriad clan polities have all been subsumed under a single state network, which can provide a common political and legal structure, as well as common leaders, and, thus, holds out the possibility that mediation can occur between clans. To date, a significant proportion of the land courts' cases have involved disputes between clans.

If public acceptance of the land court process and of the decisions of land court mediators and magistrates is an indication, they are working relatively well. By 1979, land courts were operating in every province, 105 permanent and more than a hundred part-time mediators had been appointed, and four regional (supervisory land magistrates) and thirty local land magistrates had been named.³¹ There have been occasional problems. In late 1978, the land courts in Enga Province in the Highlands were closed for some months after violence interrupted a number of attempts by land court magistrates to mark boundaries.³² And there have been some criticisms of the operation of the land courts. Richard James Giddings, one of the best of the provincial land court magistrates, has pointed out that there are too many appeals from local land court decisions--in part because the provincial land courts too often overturn the decisions of the local land courts.³³ In 1979, at a seminar for land court magistrates, some participants expressed the opinion that mediators do not receive sufficient training and that, as a result, many mediators issue orders rather than mediating.³⁴ However, the courts seem for the most part to be applying customary law, as they know it. But in their attempt to apply customary law in a customary way they have come into conflict with the common law courts.

Customary Law in a Common Law Setting

The land courts were to be separated, as far as possible, from Papua New Guinea's common law courts, largely to prevent the common law courts from imposing their notions of law and legal procedure on the land dispute-settlement process. The act bars lawyers from appearing in

most cases, and decisions may be appealed only to the provincial land courts (sections 60, 72). The act was intended to prohibit all appeals to the Papua New Guinea National Court (the common law court that serves as a trial court for major cases from throughout the country and as an appellate tribunal for the local and district courts), but section 155 of the Papua New Guinea Constitution provides that the National Court has a right to review (by writ of *certiorari*) all lower court decisions. When the National Court wanted to change land courts to make them more like itself, it used this review power to circumvent the act's prohibition on appeals.³⁵

Customary law and common law differ not only in the substantive rules that each would apply to determine rights to land but, of equal importance, in the processes that each uses for determining rights, managing disputes, and maintaining order. The typical common law process is an adversary trial of a carefully delineated set of issues between two sets of contestants. The trial is presided over by an unrelated third party, and the intended end is an adjudicated decision under which one contestant wins and the other loses. The paradigmatic customary law process is an informal village moot, in which everyone connected to the contestants and the dispute may have a say and in which all the grievances between them may be aired. The dispute may involve a mediator, but he or she has a relationship to the parties, and the intended end is a mutually agreeable resolution.³⁶

Substantive rules are invoked for different purposes and function very differently in these disparate legal processes. In the customary law process the rules may be mutually contradictory and their application to disputes is flexible, whereas the common law demands consistency, predictability, and efficiency in the application of rules. Because the meaning and effect of a substantive rule depends upon the procedural framework in which the rule operates, the integration of a customary rule into a common law framework significantly alters the meaning and effect of the rule.³⁷

A survey of the cases in which the Papua New Guinea National Court has reviewed the decisions of customary land courts demonstrates the changes that occur to customary law when a common law court imposes its own standards onto customary courts. In these cases, the National Court disagreed with the land courts about which rules of substantive customary law should apply and disapproved of the land courts' willingness to countenance the simultaneous existence of multiple, sometimes contradictory rules and the informality of land court

procedures. If the land courts were to adopt all the changes ordered by the National Court, they would operate much more like common law courts than in the customary fashion envisioned by the act.

Changing the Substantive Rules

All the land court cases reviewed by the National Court have concerned disputes between clans in which one clan claimed the land on the basis of original occupancy and the other on the grounds of conquest, gift, or undisputed occupation and use. At first, the National Court refused to recognize original occupancy as a basis for clan land claims, preferring instead to support the claims either of clans in possession of the land when the colonial administration first encountered the area or of clans currently inhabiting the territory. But, in more recent cases, the National Court has given some recognition to land claims based upon original occupancy. Its preference for certain customary rules over others and the change in its preferences over time does not arise from a careful study of substantive customary law and an attempt to apply that law, but is instead predicated on the National Court's desire that the land courts adopt and apply a single rule--any single rule, so long as it can be easily applied and will lead to a quick and final end to disputes. If these goals were possible of achievement, then the function of the land courts would be not only, as customary law prescribes, to resolve disputes over land but also, as state law intends, to make permanent determinations about land ownership. Once the ownership of customary land has been determined and once it has been made clear that the determination is not open to change, then an end very like land registration will have been achieved. Customary land will have become property. With title to it clear, it will be amenable to sale or long-term lease.

Acquisition by Conquest. The first National Court decision intended to have an impact on the land courts was actually an appeal not from a land court but from one of the last cases heard by the Land Titles Commission before its authority to adjudicate most customary land disputes was transferred to the land courts. *Kaigo v. Kurondo*, decided in 1976, shortly after the land courts became operational, involved a dispute between the Siku and the Gena, two clans from Chimbu Province. Both claimed the same tract of land, the Siku because their ancestors had been the original settlers and the Gena because their clan had taken the land by conquest and maintained effective occupation thereafter. The National Court did not completely accept the principle of ownership by

conquest, even though “[t]here is ample evidence that the Chimbu custom of recognizing acquisition of land by conquest and effective occupation exists.” The court argued that the conquest principle is “repugnant to the general principles of humanity.” However, it was willing to recognize claims based on conquest in certain situations. It would, for example, uphold the claim of the Gena even though they had gained their occupation through conquest because they were in effective occupation of the disputed land at the time when the colonial administration established its hegemony over the area. But the court would not recognize the claims of clans who won land through conquest after the onset of colonial control:

To recognize as owners of native land persons who had acquired that land by conquest after Government control had been established would undoubtedly be repugnant to the general principles of humanity but to recognize as owners of land those who had acquired it by conquest and who were in effective occupation of the land at the time when Government control was established is not repugnant to the general principles of humanity. It is the only practical and sensible basis upon which ownership of land can be recognized. . . . Before the advent of the Administration native customary law had reigned supreme, and it was not only expedient but also right and proper that when it imposed its own control the Administration should have recognized rights of ownership of land acquired by native custom even if native custom meant brute force.³⁸

Holding that a change in government can make morally repugnant that which was not repugnant before may be illogical; and it is, of course, morally repugnant to hold that Chimbu land conquests are immoral while ignoring Australia’s conquest of Papua New Guinea by brute force. But the ruling supports the aims of the common law, in particular the common law’s interest in providing the government, the courts, and the parties to land disputes with a single, easily ascertainable rule for determining interests in customary land. Once it has been established that rights accrue to those who were in occupation when the colonial administration took control of the area, then parties can govern their relations by the rule, and courts can apply it mechanistically.

Adoption of this rule supports other goals of state law as well, in particular the interest of the courts in fostering state authority. When private parties settle their disagreements by forceful means (as in wars over

land) rather than by recourse to the courts, their perceived need for the government's dispute-settlement and enforcement institutions is lessened, and the authority of the state is to that extent undermined. In holding that conquest after colonization should not be recognized as a means for acquiring rights in land, the court is upholding the state's monopoly over the settlement of disputes and the use of force.

Denying the Rule of Original Occupation. Despite its preference for a single, generally applicable rule of land ownership, the National Court did not uphold the principle advanced by the Siku clan that interests in customary land should be allotted on the basis of original occupation, even though that is a recognized principle of customary law. The court refused to base its decision on original occupation because application of that rule would be inefficient and uncertain: "a tribunal would be faced with the impossible task of going back to the mists of time in order to ascertain who are the rightful owners of disputed land."³⁹ The formulation of clear rules that can be easily applied is a goal of the common law courts, and this court believed that the difficulties of assessing the validity of competing oral histories of precolonial events made the principle of original occupation difficult to apply.

But customary legal practice did not bow immediately to the demands of the common law courts. In 1981, five years after *Kaigo v. Kurondo*, the National Court heard *State v. Giddings*, a case in which the land courts had again attempted to settle a dispute between two clans, one of which claimed the land as original occupants and the other on the grounds of long-term settlement on and improvement of the disputed land. The parties to *State v. Giddings* were two clans from Enga Province, the Pialin, who claimed to be the original occupants of the land, and the Ambai, who had settled on the land after another clan had driven the Pialin from it in a long ago war. The land court had awarded the larger share of the disputed land to the Pialin, primarily because the Pialin were the land's original occupants (in Papua New Guinean pidgin, the land was their *as graun*).

It was evident that, if the Enga land mediators had heard about *Kaigo v. Kurondo*, they had not been swayed by it. In upholding the local land court's decision, the provincial land magistrate, Richard James Giddings, remarked laconically, "mediation policy in the . . . [Enga Province] is to find in favour of the 'as graun' (original owners) of land under dispute." The National Court responded with horror:

If I may say so, that is a surprising policy; if it has been applied efficiently, it could be responsible for a great deal of the High-

lands tribal fighting in recent years. Indeed, I believe it is a matter which would bear urgent investigation; for five years, or so, this system has been operating for good or ill, and this application is the first time the system has been opened up to examination by the ordinary courts.⁴⁰

The National Court revealed its prejudices in that remark--not least the assumption that the (imported) common law courts are Papua New Guinea's "ordinary courts," implying that the land court system, founded upon indigenous legal principles, is not ordinary.

The National Court's arguments in opposition to the *as graun* principle were based primarily on a technical and narrow reading of the Land Disputes Settlement Act. Thus, the court argued that it "is highly doubtful whether the adoption of an 'as graun' policy to determine ownership is consistent with carrying out the statutory duty" of sections 36(d) and 67 of the act, which require that land courts "endeavour to do substantial justice." But the court did not explain why the return of land to its original occupants might be inconsistent with doing justice. Instead, the court went on to list even more sections of the act with which "the 'as graun' policy does not appear to be consistent," though the court did not explain the relevance or even describe the content of the other sections it cited.

Almost as an afterthought, the National Court concluded that the policy couldn't possibly be consistent with customary law either: "It may be, of course, that in Enga the Local Land Courts have consistently found, in carrying out their duty under . . . [the act] that the relevant custom is an 'as graun' principle. I doubt that."⁴¹ The court preferred its view of customary law even though the local land courts consist of mediators and magistrates from and working in Enga Province, whereas the National Court sits in Papua New Guinea's capital city, far away from Enga.

The major reason for the National Court's dislike of the *as graun* principle probably lies in the interest of common law courts in bringing disputes to closure and preventing once-settled cases from being reopened. To achieve this aim, the National Court was willing to change substantive customary law, to substitute government-made rules for the rules of customary law. For years, colonial *kiaps* (patrol officers) had settled disputes over customary land by giving the land to whoever was on it when the administration first appeared.⁴² The courts feared that if they overturned that *kiap*-made rule in favor of the actual rules of customary law, such as original occupation, they would be flooded with litigation from all those who had once accepted a *kiap*'s determination.

Returning to the Rule of Original Occupation. At common law, once a court decides that rights to land do not arise from original occupation, then courts in later cases are not supposed to grant rights on that basis.⁴³ But in customary law all rules retain their power. A rule that received no support in one dispute can nevertheless be recognized in later disputes. The continuing viability of customary norms is one of the ways in which customary law withstands the attempts of state law to change it.

Thus, in *Application of Nango Pinzi*, a case heard in the National Court in 1986, five years after *State v. Giddings*, and decided in 1989, one of the disputing clans claimed land on the basis of original occupation. And, this time, the National Court was willing to countenance the claim.⁴⁴ The case involved two clans from Morobe Province on Papua New Guinea's north coast. The Sio claimed the contested territory as original occupants but the Kulavi had been in sole possession of the land for at least ten years and had planted coconuts and other cash crops on it. The local land court had found that the land belonged to the Sio ("It is [our] unanimous decision that the land known as Kulavi . . . is owned by Sio Clan and [they] can use and do anything with it as they see fit and [it can] later be used by their children and children's children"),⁴⁵ had ordered the Sio to pay K 20,000 (approximately US\$20,000) in compensation for the trees the Kulavi had planted, permitted the Kulavi to continue to harvest the trees until the first payment was made, and allowed the Kulavi to "continue to harvest and live on the produce of their existing gardens [but] no new gardens [are] to be made."⁴⁶ The provincial land court reversed that decision, granting permanent possession to the Kulavi. The Kulavi had been in possession for more than twelve years, the provincial land court found, and therefore came under section 67 of the Land Disputes Settlement Act, which establishes a presumption in favor of the possessory claim of any party that "has exercised an interest over the land . . . for not less than 12 years without the permission, agreement or approval of any other person."

The National Court quashed the reversal, returning the case for a rehearing because the provincial land court had determined only the question of possession under section 67 and "did not deal with the question of ownership of the land."⁴⁷ In suggesting that the original occupants might have a claim to the land, the National Court reversed the rule developed in earlier cases (in fact, the court does not even mention the discussion of original occupancy in those cases). By 1989, when *Application of Nango Pinzi* was decided, the court had come to realize that its continuing refusal to accept the original-occupancy rule created problems greater than the evidentiary difficulties of choosing between conflicting oral histories. The 1980s had seen continuing land disputes

between clans, many of which had erupted into violence. Papua New Guineans were not about to agree with a legal regime that denied them access to the land of their ancestors. And, as the court notes in *Application of Nango Pinzi*, economic changes had made people less willing to give up claims. Papua New Guineans were developing new uses for customary land, and new relationships as landholders and lessees, making land more valuable. The court believed that, if land disputes were to be settled, original occupation could not be ignored.

The decision in *Application of Nango Pinzi* seemed to be a victory for customary law. First, the National Court reversed earlier attempts to change the substantive rules of customary law. The court also recognized the necessity, as customary law long since had recognized, of uncovering all the issues, of settling all the areas of conflict between the disputants, and of attending to the needs of both parties if a dispute is to be resolved. The provincial land court magistrate (sitting without assessors versed in local custom) had acted like a common law court, mechanically applying a statutory provision to produce a narrow determination of possessory rights.⁴⁸ And the National Court felt bound to remind the provincial land court that customary law (and the customary land courts) aim “more towards solving the ‘dispute’ for the future and not just towards disposing of the present ‘appeal.’ ”⁴⁹

The decision, however, also reflects the bias of the common law. First, it presumes (as did the local land court) that, at customary law, there is such a thing as ownership of land--and that, if there is, ownership under customary law would convey the same meaning, the same rights and powers, as ownership under the common law. This misinterpretation has percolated through all the recent discussions of customary land tenure. It may be that common law concepts of ownership have so thoroughly infiltrated customary law that ownership must now be accepted as a customary law concept, as well.⁵⁰ But a court’s decision about ownership will not necessarily settle a dispute. That is the second common law fallacy in the National Court’s decision. The court presumed that a permanent settlement of this dispute was possible, if only the right rules and procedures could be applied, if only “ownership” could be determined. But it is the nature of customary relations to land, and of the customary dispute-settlement process, that no resolution is permanent.

Process, Procedure, and the Resolution of Disputes

In addition to wanting the land courts to change the substantive rules of customary law, the National Court also wanted to change their pro-

cesses and procedures in ways that would make the land courts operate more like the National Court. First, the National Court wanted the land courts to choose a single substantive rule and apply it uniformly to all disputes. Second, it wanted the land courts to obey the technical requirements of the Land Disputes Settlement Act and other state laws precisely and mechanistically, rigidly adhering to the letter of the statutes even at the expense of their spirit. The result of these changes, were the land courts to accede to them, would be to turn a mediatory process, aimed at obtaining the parties' mutual agreement to a resolution of their dispute, into an adjudicatory process, aimed at a determination by a court of interests in land. Once the land courts were in the habit of clarifying title to customary land, registration and sale would be only a step away.

Many Rules or One Rule? The National Court has criticized the land courts not only for applying what it believes to be the wrong rules of customary law, but also for the land courts' willingness to recognize that numerous rules, some of which are in conflict, may simultaneously apply in a case. The National Court wants the land courts to recognize either original occupation or conquest, but not both. The National Court would reduce the rules applicable in any case to one. In *State v. Giddings*, for example, the local land court attempted to end the long-festering controversy between the Ambai and the Pialin by dividing the land and giving the greater share to the Pialin, who were poorer in land but stronger both in fighting power and in their ability to manipulate the political system.⁵¹ The National Court reacted as angrily to these bases for the decision as it had to the land court's acceptance of original occupation, accusing the mediators and the advisers whom the land court had consulted of corruption.

But in allowing its decision to be guided by these aims and interests, the land court was following the dictates both of customary law and of the Land Disputes Settlement Act. In the typical customary dispute-settlement meeting (an informal village moot, for example), rules influence the outcome but do not determine it. The rules sometimes operate as bargaining chips, used by the various parties to support their arguments. Sometimes mediators will refer to one rule or another in an effort to attain the parties' acquiescence in a proposed solution. The rules also set boundaries, separating out those claims to land that might be accepted from those that would not be. As such, the rules are also guides to behavior, maintaining order and preventing further disputes from arising. Just as consensus solutions can involve a compromise

between the parties, such as dividing the property that is the subject of the conflict, they can also involve a compromise among the potentially applicable rules, in effect dividing the rules so that each one is honored a little. For rules to operate in this way, for the process to remain open to different solutions that take into account the relative strength and needs of the contending parties, there must be a multiplicity of rules potentially available to each dispute, and there usually are. In land matters, for example, one clan might argue that the land is theirs because of original occupancy and another that they developed and worked it. Both arguments are predicated on recognized principles of customary law. No outcome arises solely from the operation of a rule, for to do that would be to limit the multiplicity of available rules and consequently to restrict the flexibility of the dispute-management process. The Land Disputes Settlement Act follows customary law in permitting the land courts to take a number of rules, needs, interests, and goals into account in each decision. A land court is permitted to divide disputed land (section 39[5]) or order its return from one party to another if “one of the parties to the dispute is short of land and another party has an abundant supply” (section 40).

At common law, unlike customary law, rules are seen as directly determining the outcome of a dispute. The adjudication process is presumed to consist of the neutral application of rules to facts by a disinterested arbiter, and the winner is the party that the rule favors. The process requires consistency in the choice and application of rules. From the multiplicity of potentially applicable rules, a single rule is chosen and, once chosen, will be applied not only in the present case, but thereafter in all cases in which the facts are similar. For example, were it once decided that rights to land accrue from original occupancy rather than from conquest, need, or power, then the general practice of the courts would be to follow that rule in all future cases. To ensure consistency, judicial opinions are written, becoming available for citation in later cases. The written opinion comes to be regarded as a rule in itself, disguising the choices that were involved in its selection of rules to be applied. The *Application of Nango Pinzi* case is unusual in its inattention to the settled rule.

These different approaches to rules reflect the different purposes of state law and customary law. The purpose of a common law court is to apply substantive law (the rules about how interests in land are acquired) in order to determine which of two competing claimants owns the disputed land. The purpose of a customary land court, however, is to settle the dispute between the claimants--a purpose that may

or may not be best accomplished by deciding that, under the applicable rules, one of them owns the land and the other does not.

Sometimes, common law courts do apply more than one rule to a case, but they organize the rules differently than customary courts do. For example, in *Application of Nango Pinzi*, the National Court criticized the land courts for failing to consider a number of rules in reaching their decisions:

There should have been an inquiry into the custom regarding an agreement by the land-owning clan to let outsiders "use" the land and the customary terms of such agreements (if any). . . . In order to do justice and to apply this Act correctly, the Magistrate should also have inquired into any possible shortage of land amongst the two parties. . . .⁵²

However, the National Court did not expect the land courts to accord equal weight to all these rules. It presumed that the rules could be interpreted so as not to be in conflict. One way to do so would be to decide that certain rules are bases for claims to ownership of land, others are bases for claims to possession, yet others give a clan a right to the products of the land. The court presumed, as common law courts do, that earlier failures to resolve the dispute arose primarily because of an error as to which rules should be applied, or in how the rules had been applied, and that the parties to this dispute (and all potential claimants to the land in the future) would cease their conflict once they perceived that correct rules had now been applied to the question.

The transformation by the National Court of customary rules into rules of the common law reached its height in *Application of Ambra Nii*, a case from the Western Highlands Province. The Gupamp claimed land on the basis of original occupation and the Toisap claimed it on the grounds that it had been given to the Toisap at least twenty-five years earlier by the Wakiam clan, which was in possession of it at the time and also claimed to be its original owners.⁵³ The Gupamp did not dispute the Toisap's possession until the 1980s, when the Toisap began to earn a substantial income from coffee they had planted. The local land court had divided the land between the Toisap and Gupamp, giving the cultivated portions to the Toisap and the undeveloped portions to the Gupamp. The provincial land court reversed that decision, holding that all the land belonged to the Gupamp. The National Court reinstated the order of the local land court. But, in reinstating that order, the National Court actually changed the order.

The National Court cited with approval the comments of one of the local land mediators, who had noted that “both sides have lived there side by side for too long and have worked and owned the land thereabouts. It would be against natural justice if one party is removed. Neither party has shown it had an exclusive right and ownership of the disputed land.”⁵⁴ Despite its praise for these sentiments, the National Court would not, as had the land mediators, simply hold that both clans had a continuing right to occupancy, without determining precisely what kinds of ownership and possessory rights each clan had. The National Court misread the order of the local land court to hold that, through their failure over many years to contest the occupation of the land by the Toisap, the Gupamp had lost their ownership rights to the portion of the land that the Toisap had occupied and developed. The local land court had, in effect, left the question of ownership undecided; the National Court interpreted the local land court’s writings as if it had decided the matter.

The National Court approved of the land court’s decision (or, more precisely, its own restatement of the land court’s decision) as based upon “the appropriate principles to consider in any investigation of customary land ownership.” The National Court found these principles in “an Institute of National Affairs publication being a report by Professor D. Cooter titled *Issues in Customary Land Law*.” (The Institute of National Affairs is a research and lobbying organization, set up by companies and businesses in Papua New Guinea.) The court did not say how Professor Cooter conducted his study of customary land law--whether he consulted ethnographies, interviewed informants, attended land court hearings or read National Court cases. The court did report that the study yielded Professor Cooter a set of eight principles or rules--ranging from “adverse possession” (“A group who resides upon or improves land for a sufficient time without the permission or active opposition from others thereby owns it”) to “preponderance of the evidence” (“In customary land disputes, the party shall prevail whose case is supported by the preponderance of the evidence”) to “right to resist attempt to return” (“The extent to which people attempting to return to the land of their ancestors are opposed is largely dependent upon the extent to which their land has been taken over and used by others and the extent to which they have been able to forge friendly relationships with those now in control of it”).⁵⁵

There are a number of problems with Professor Cooter’s principles, aside from their doubtful provenance. First, as the three cited above demonstrate, they are an odd and illogical mixture of substantive law

(adverse possession), procedure (preponderance of the evidence), and statements that are not about law but about expected behavior (resisting attempts to return). Second, some are so foreign to customary law as to call all the rest into question. The concept of preponderance of the evidence, for example, is a procedural guideline for common law courts; it does not exist in custom. Third, all the rules favor the retention of title by current users and occupiers of the land, particularly by those who are putting the land to economic uses, in opposition to clans who base their claims on original occupation. For example, Professor Cooter's principles, in addition to adverse possession, include "last is first" ("If land is not used for successive generations, the claim of those furthest removed from those who vacated it becomes, as the years pass, of diminishing importance"), "maintenance of interest in land" ("An interest in land is maintained by building houses and settling on it and by gardening, grazing or burning it off, collecting from it or forbidding others to occupy and use it"), and "no unqualified right of return" ("Once a group has abandoned its ancestral [land] . . . they cannot return and claim it at a much later date without the agreement of those who prior to that date have assumed controlling rights to it").⁵⁶ These may well be operative principles of customary law. However, the list ignores the conflicting principles, all of which are also operative. And, the list is so remarkably a reflection of common law principles, so patently in the interests of the business community, as to raise suspicions about its authenticity.

Finally, the very notion that customary law can be codified, can be reduced to a set of internally consistent principles, has long been derided by legal anthropologists. In legal anthropology's earliest days, anthropologists in Africa did attempt to make collections of the rules of customary law. But, the wrongheadedness of this task was quickly recognized. The rules of customary law are too various, too flexible, too capable of infinite change and variety, to be captured in a code. The very act of codification relieves them of their capacity for constant change. By removing customary rules from the customary process, codification subverts their meaning and purpose. Professor Cooter's list does not reflect custom--in creating a set of rules that are consistent and that will be applied uniformly, it does reflect state law.

Court Procedure and the Aims of the Legal Process. Although there are major differences in the substantive rules of customary and common law, the more significant difference is in the processes by which these rules are applied to disputes and the very different results that these pro-

cesses are intended to achieve. Customary dispute-management processes often result in a division of the land between the disputants, or some other compromise, and it is a common, though not always accurate, assumption that in criticizing customary processes the National Court is criticizing compromise. Yet, in cases involving land disputes between clans the National Court has sometimes approved and sometimes disapproved of land court decisions that divide the land.

For example, although in *State v. Giddings* the National Court criticized the land court for dividing the disputed land,⁵⁷ in *State v. District Land Court, Ex Parte Caspar Nuli*, which was decided in the same year, it praised the local land court's decision as "a compromise giving some rights to each side."⁵⁸ But, in the *Caspar Nuli* case, the National Court overturned a provincial land court decision that was itself as much a compromise as had been the local land court decision that the National Court reinstated. The case involved the Wasikuru and the Ruka, two Tolai clans from East New Britain Province. The Wasikuru, original occupants of the land, had permitted some Ruka to settle on their land but had not expected them to move in permanently or to plant coconuts and other long-term cash crops. The local land court had upheld the Wasikuru's claim to ownership but ordered that the Ruka could continue to harvest their crops for five years, so long as they did not plant new trees and so long as they paid an annual rental to the Wasikuru. At the end of the five-year lease, the Wasikuru were to compensate the Ruka for trees that were still in existence. The provincial land court, on the other hand, had ordered a permanent division of the land between the disputing clans, giving some to each clan.

In its seemingly inconsistent reactions to attempts by the customary land courts to effect a compromise, the National Court is pursuing a consistent principle. It is less concerned with stamping out compromise decisions than with convincing the land courts to forgo the flexibility of the customary law process in favor of technically formal procedures, similar to those used by the National Court. In the *Capar Nuli* case, for example, the grounds for the National Court's dislike of the appellate decision did not lie in its perception that the provincial land court had failed to order a compromise, but in that court's failure to follow the technical rules of appellate procedure as laid down in the Land Disputes Settlement Act. The act provides that a provincial land court may affirm an order of the local land court, may quash the order and make a different order, or may quash the order and remit the case to the local land court (section 60). In the *Caspar Nuli* case, the provincial land court magistrate affirmed the order with, he said, "slight variations."

The National Court overturned his decision: "He has no power to do that and has erred in law. If he affirms the order he must simply affirm it; he cannot add variations." Besides, the National Court pointed out, "the 'slight alterations' or 'slight variations' which the magistrate has purported to add to the Local Land Court's decision are by no means slight." Nor would the National Court interpret the provincial land court magistrate's decision as quashing the order and replacing it with another; to do that, the provincial land court would have had to state expressly "that one of the grounds of appeal of s[ection] 59 [of the act] had succeeded," and it had not mouthed these magic words.⁵⁹ Technically, the National Court might have been correct; the magistrate's order did not precisely track the statutory requirements. However, was this minor procedural inefficacy adequate grounds for overturning the judgment? Perhaps the magistrate had not wanted to offend the local land court by stating outright that he disagreed with its order. Perhaps he had not yet learned the talismanic importance to the common law process of magic words and phrases that replicate the statutory language. Perhaps he had been seduced by the Land Disputes Settlement Act, which seems to reject procedural niceties in favor of resolutions that will solve disputes.

Similarly, in *State v. Giddings* the National Court's disagreement with the local land court was not directed primarily at the attempt to fashion a compromise but at the failure of the land court, in the course of fashioning that compromise, to act like a common law court. The National Court had a number of criticisms of the local land court's procedure: it failed to limit itself to admissible evidence, the court was incorrectly constituted, members of the court allowed themselves to be swayed by their relationships to the parties, the court did not hear all the witnesses or allow them to confront one another, the marking of the land boundaries did not proceed in the presence of the parties as mandated by the act.⁶⁰ In common law jurisprudence, these criticisms go to weightier considerations than did the criticisms made in the *Caspar Nuli* case. The National Court has, in effect, accused the land court of ignoring the major procedural requirements of the adjudicatory process. If adjudication is to be effective, it is necessary that judges appear to be acting fairly, that they give both sides a full opportunity to be heard, both by the court and by one another, and that the judge appear to have no interest in the outcome. These requirements are necessary to the common law because it is the function of common law courts to make rulings. Parties are expected to abide by a court's ruling whether or not they agree with it, whether or not it is in their favor. Parties,

especially losing parties, will be more likely to accept a court's ruling if they believe that the court acted rightly and made its ruling fairly.

But the land courts do not need to emphasize these indicia of procedural fairness in their proceedings, because the outcome of those proceedings is not supposed to be the issuance of a ruling. Instead, the land courts are supposed to use mediation to reach a result to which both parties can accede. The land courts therefore do not need to find in procedural fairness a justification for why the parties should accept their decisions. In the customary legal process, which land courts are supposed to follow, the perception of justice inheres in the outcomes, not in the process.⁶¹

Procedural rigidity, the close attention by a court to the mechanics of its decision-making process, is intended to produce results that will not be reopened, and the National Court probably hopes that, if the land courts adopt these procedures, they will be able to bring land disputes to a close. But, for years, Papua New Guineans have refused to close land disputes or to accept the determination of any tribunal as final, no matter what its procedure. The continual resurrection of disputes has been an ongoing source of considerable grievance to, and misunderstanding by, state officials. In the colonial era *kiaps* complained that village people often asked them to settle land disputes that had been decided on previous patrols; they took to writing their decisions in village record books so the next *kiap* would not innocently be drawn into rehearing the same dispute.⁶² Few of the disputes that now reach the land courts are new. The land dispute that was the subject of *Kaigo v. Kurondo* had been going on for years, with the clans trying every tribunal then available--*kiaps*, district officers, the Native Lands Commission, the Supreme Court, the Land Titles Commission, the Supreme Court again--and, in the interim, fighting with one another. The dispute that underlay *State v. Giddings* was also long-standing, having involved two local land court hearings, two appeals to the provincial land court, and numerous tribal battles during which people had lost their lives.

The customary process, which is often called "dispute settlement," might be better termed "dispute management." In customary proceedings, such as mediation, the parties reach a settlement for the time being, but it is not intended to be a decision that can never be reopened. Such a decision would entail the grant to one or the other of the parties of permanent rights to the land--a determination that would likely not be acceptable to both parties and thus would not obtain their mutual agreement. The customary process presumes that no dispute is ever per-

manently resolved and that rights to land are never permanently determined. It therefore leaves room for reopening decisions as changing circumstances require.

If the National Court believes that procedural rigidity can guarantee that parties will not attempt to reopen the decisions of the land courts, the National Court is mistaken. The very National Court decisions that were intended to stem the flow of litigation and to convince litigants that a fair and therefore final disposition of their case has been reached have had the opposite effect. Litigants, supported by the customary law notion that no case need ever be final until there is no one left with an unfilled need or a grievance, view the common law court's procedural wrangling as evidence that nothing is final in the common law courts either--and pursue their cases endlessly.⁶³

Customary law permits land disputes to be reopened whereas the common law courts expect to achieve finality because the purposes served by the two legal systems and the environments in which they operate differ. Land, in a customary environment, is primarily a source of shelter and subsistence, and one function of the customary dispute-settlement system is to ensure access to land of all who need it, a function best performed if decisions can be changed as circumstances change. The common law operates in a market environment in which land is a commodity; a primary function of the common law is to make sure that determinations about interests in land are final so that purchasers can be confident that what they have paid for will not be taken from them whenever a disgruntled claimant wishes to reopen the case.

Occasionally, however, the imposition by the National Court of procedural requirements on a lower court, even on a customary law court, can have a salutary effect. For example, in *Application of Nango Pinzi*, the National Court used procedural errors committed by the provincial land court as the grounds for overturning the lesser court's allocation of the land. The National Court believed that the provincial land court had been mistaken in granting the land to the Kulavi when the Sio seemed able to prove original ownership, and mistaken as well in not recognizing that these customary landholders could make arrangements with each other about the use of the land. But common law courts cannot overturn the decisions of lesser courts just because they disagree with them. So, the National Court phrased its disagreement with the provincial land court in procedural terms: it held that the provincial land court had made "sufficient [procedural] errors to amount to a substantial miscarriage of justice." First, the magistrate had allowed an appeal from the local land court's decision "merely because he disagreed

with the lower court's decision" and not, as the Land Disputes Settlement Act requires (section 59), because he was not satisfied "in the circumstances of the case that no court doing justice could have reached the decision appealed against." The act's standard for review, which is similar to the common law standard, is intended to discourage a pattern of constant appeals by permitting a decision of a local land court to stand unless it is significantly wrong. Second, the magistrate based his decision partly upon letters written by *kiaps* to one another during the colonial period, letters that were derogatory towards Papua New Guineans and that he did not discuss with the parties. The act permits a land court to "inform itself on any question before it in such manner as it thinks proper," but requires the court, when it does so, to make the information available to the parties (section 50[3]). And, by the way, the "magistrate failed to determine questions of custom regarding ownership, usage and possession."⁶⁴

The National Court's decision in *Application of Nango Pinzi* crystallizes the ambiguities in common law procedural requirements. As this case demonstrates, the existence of procedural requirements gives a higher court an excuse for overturning the ruling of a lesser court even though the higher court might not be able directly to attack the lesser court's holding on substantive grounds. In the *Nango Pinzi* case, the National Court criticized the lower court for failing to follow proper procedures, not because procedure is an end in itself but because the provincial land court's decision violated the goals of the Land Disputes Settlement Act. Because the National Court is a common law court, to overturn the lesser court's decision it had to argue that the lesser court had used procedures improper under common and statutory law.

Custom, the Common Law, and Economic Development

It briefly seemed, as Papua New Guinea was nearing independence, that the new nation would opt for planned development and economic equality.⁶⁵ More than fifteen years after independence, however, the country's urban economy is essentially a market system. The National Court's uncritical acceptance of common law principles has been a contributing factor in the gradual erosion of the social and economic ideals of the independence period, because the substantive rules and, even more, the procedural requirements of the common law have as their primary goal the support and maintenance of a market economy.

The role of government and its courts in a market economy is severely circumscribed. According to market economy theorists, economic de-

velopment occurs naturally, without government planning or interference, because individuals and companies in search of expanding profits develop new industries, which, by producing more marketable goods and employing more workers, create the opportunity for yet more industries to develop.⁶⁶ Government in a market economy is not supposed to take a central role in planning and promoting development but merely provide infrastructure, enforce market rules and agreements, and occasionally alleviate the harsher effects of the system. The major role of the courts is to support marketplace dealings by providing a set of rules upon whose predictability entrepreneurs can rely, by applying and enforcing the rules consistently and by providing a forum in which disputes about the rules can be quickly and permanently resolved. When a common law court decides a dispute by applying substantive laws to the facts of the dispute, it accomplishes two goals. It ends that dispute between those parties, and it lets future marketplace actors know what rules will be applied should a similar dispute arise between them, thus shaping their behavior.

But the market economy does not produce greater wealth for everyone. Access to goods, services, and the means of production is unevenly distributed. Even with the continuing availability of the rural village as a place where workers and their families may be housed and fed, poverty occurs and increases. So a secondary role of the common law is remedial. Common law courts support the continuation of the market economy by correcting some of its excesses. Where courts are not alert to the social and economic implications of their decisions, their tendency is to utilize common law rules in most cases and to turn to customary rules only in those circumstances when they wish to counter the undue harshness of the market. Generally, the only purpose of the introduction of a substantive rule of customary law into a common law court's decision is to alleviate what would otherwise be the harsh results of a common law rule.

In the land court cases, the tendency of the National Court has been, with few exceptions, to presume the superiority of the common law procedural model and to treat substantive rules of customary law as if they were common law rules. Thus, the National Court requires rules about customary land rights to be applied as consistently, as predictably, and as efficiently as if they were common law rules. The land courts may produce compromise decisions, but they must do so in a way that is procedurally correct and that results in closure, both of the dispute and of the issues that gave rise to the dispute. Moreover, the land courts must settle on certain substantive rules, and apply them, to the exclusion of

all others. If the National Court has its way--if the land courts become like the common law courts, if a system of rules is developed for adjudicating land claims and determining ownership--then the stage will again be set for the registration of titles to customary land. Once land ownership is clearer and unchallengeable, land can be bought and sold.

The common law courts wish to bring land disputes to a close, to turn land into a marketable commodity. But the imposition of alien procedural forms is not the way to do it. Nor is it clear that closure should be a goal of the courts. In Papua New Guinea today, the meaning and uses of land are rapidly changing. Clans once rich in land find themselves, as a result of population increases or changing land uses, land poor. Papua New Guineans are experimenting with new kinds of transactions in customary land. Perhaps, in this fluid situation, claims to land should be permitted to remain fluid as well.

If Papua New Guineans want their land to escape commodification they should support customary law. Customary law was developed for economic systems that are more egalitarian than those maintained by the common law, probably smaller in scale as well, in which the production and distribution of goods are accomplished by reciprocity or redistribution rather than by buying and selling. By permitting contradictory rules to exist simultaneously, by eschewing finality, by focusing on interests in and needs for land rather than on ownership and other rights, customary law permits land cases to be reopened whenever the need arises. By keeping to customary law, Papua New Guineans stand a chance of keeping their land from being totally in the thrall of the market.

NOTES

Thanks are owed to many people who have helped me to refine the ideas in this article, but I will leave these friends and colleagues unnamed for now in order to express my special gratitude to R. J. Giddings for his contributions to Papua New Guinea's land courts (and to this article).

1. R. W. James, " 'Unalienated' Land Policies," *Melanesian Law Journal* 11 (1983): 34; Michael J. Trebilcock, "Customary Land Law Reform in Papua New Guinea: Law, Economics, and Property Rights in a Traditional Culture," *Adelaide Law Review* 9 (1983): 194.

2. Donald Denoon, "Introduction," in *A Time to Plant and a Time to Uproot*, ed. Donald Denoon and Catherine Snowden (Port Moresby: Institute of Papua New Guinea Studies, undated), 1-2, 10; Trebilcock, "Customary Land Law Reform," 191.

3. Peter Fitzpatrick, "The Knowledge and Politics of Land Law," *Melanesian Law Journal* 11 (1983): 17; James, " 'Unalienated' Land Policies," 34-38; Robin Hide, *The Land Titles Commission in Chimbu*, New Guinea Research Bulletin no. 50 (Port Moresby and Canberra: New Guinea Research Unit, 1973), 13-14; Alan Ward, "Customary Land, Land Registration, and Social Equality," in Denoon and Snowden, *A Time to Plant*, 249-250.

4. Alan Ward, "Agrarian Revolution: Handle with Care," *New Guinea* 6 (1972); Ward, "Customary Land," 249-250; S. Rowton Simpson, R. L. Hide, A. M. Healy, and J. K. Kinyanjui, *Land Tenure and Economic Development: Problems and Policies in New Guinea and Kenya*, New Guinea Research Bulletin no. 40 (Port Moresby and Canberra: Australian National University, 1973).

5. Those who advocate customary land registration as a vehicle for transforming the nature of land tenure and thus the ownership of Papua New Guinea's natural resources include Trebilcock, "Customary Land Law Reform." Those who believe that through registration, customary tenure, clan ownership and Papua New Guinean land ownership can be maintained include James, " 'Unalienated' Land Policies"; J. Fingleton, "Customary Land Registration as an Instrument of Socio-Economic Change," paper presented at the 1981 Waigani Seminar (University of Papua New Guinea, Port Moresby, mimeo, 1981); and Papua New Guinea Government, Commission of Inquiry into Land Tenure (Port Moresby: Papua New Guinea Government Printer, 1983).

6. For definitions of legal pluralism, see John Griffiths, "What Is Legal Pluralism?" *Journal of Legal Pluralism* 24 (1986); Sally Merry, "Legal Pluralism" *Law & Society Review* 22 (1988); Sally Falk Moore, *Law as Process: An Anthropological Approach* (London: Routledge & Kegan Paul, 1978).

7. Sally Falk Moore, *Social Facts and Fabrications: "Customary" Law on Kilimanjaro* (Cambridge: Cambridge University Press, 1986).

8. Marilyn Strathern, *Official and Unofficial Courts: Legal Assumptions and Expectations in a Highlands Community*, New Guinea Research Bulletin no. 47 (Canberra: Australian National University, 1972).

9. For a discussion of the interplay of customary law and common law in the Papua New Guinea common law courts, see Jean G. Zorn, "Making Law in Papua New Guinea," *Pacific Studies* 14, no. 4 (1991): 1-34.

10. Robert Gordon and Mervyn Meggitt, *Law and Order in the New Guinea Highlands: Encounters with Enga* (Hanover, N.H.: University Press of New England, 1985).

11. The discussions of customary land law in this and other sections of this article are drawn from a number of sources, including Ron Crocombe and Robin Hide, "New Guinea: Unity in Diversity," in *Land Tenure in the Pacific*, ed. Ron Crocombe, 3d ed. (Suva: University of the South Pacific, 1987), 324-354; D. J. Colquhoun-Kerr and Andrew A. L. Lakau, "Land Tenure and Land Dispute Settlement in Enga," *Melanesian Law Journal* 11 (1983): 62-67; Andrew Strathern, "Melpa Land Tenure: Rules and Processes," in *Land Tenure in Oceania*, ed. Henry P. Lundsgaarde (Honolulu: University Press of Hawaii, 1974), 18-38; Peter Eaton, "Customary Land Dispute Settlement:

Should Lawyers Be Kept Out?" *Melanesian Law Journal* 11 (1983): 47-50; Ian Hogbin, "Land Tenure in Wogeo," in Ian Hogbin and Peter Lawrence, *Studies in New Guinea Land Tenure* (Sydney: Sydney University Press, 1967), 3-44.

12. In some Papua New Guinean societies the corporate landholding body is the clan, in others the subclan, lineage, village, or extended family. For brevity's sake, I talk here about "clan land" but mean by that "clan, subclan, lineage, village, or extended family land" as appropriate.

13. K. N. Llewellyn and E. A. Hoebel, *The Cheyenne Way: Conflict and Case Law in Primitive Jurisprudence* (Norman: University of Oklahoma Press, 1941).

14. For example, Andrew Strathern describes five land disputes among individual Melpa. One was settled (so to speak) when a disputant stole the produce from the garden in question; another was won by the party with more "personal assertiveness"; in a third, clansmen determined new boundaries; in the fourth, the parties' arguments caused so much trouble that clansmen forbade either of them to use the disputed garden; finally, in one case, the disputed land was divided. Strathern, "Melpa Land Tenure," 36-37.

15. Mervyn Meggitt, *Blood Is Their Argument: Warfare among the Mae Enga Tribesmen of the New Guinea Highlands* (Palo Alto, Calif.: Mayfield, 1977).

16. Ward, "Customary Land," 255-258.

17. See, for example, the articles collected in *Problem of Choice: Land in Papua New Guinea's Future*, ed. Peter G. Sack (Canberra: Australian National University Press, 1974); and in *Lo Bilong Ol Manmeri: Crime, Compensation, and Village Courts*, ed. Jean Zorn and Peter Bayne (Port Moresby: University of Papua New Guinea Press, 1975). For a provocative discussion of the ways in which new nations recreate the story of their past to support the ideologies of the present, see Roger M. Keesing, "Creating the Past: Custom and Identity in the Contemporary Pacific," *The Contemporary Pacific* 1, nos. 1 & 2 (1989).

18. Colquhoun-Kerr and Lakau, "Land Tenure," 59; Meggitt, *Blood Is Their Argument*.

19. Government of Papua New Guinea, Committee of Inquiry into Tribal Fighting in the Highlands, *Report* (Port Moresby: Papua New Guinea Government Printer, 1973), 14.

20. For the anthropological bases of this approach, see Max Gluckman, *The Judicial Process among the Barotse of Northern Rhodesia* (Glencoe, Ill.: The Free Press, 1955); Philip H. Gulliver, *Social Control in an African Society* (London: Routledge & Kegan Paul, 1963); Paul Bohannan, ed., *Law and Warfare* (Austin: University of Texas Press, 1967); Lucy Mair, *Primitive Government*, 2d ed. (Bloomington: Indiana University Press, 1978); A. L. Epstein, ed., *Contention and Dispute: Aspects of Law and Social Control in Melanesia* (Canberra: Australian National University Press, 1974).

21. The Land Disputes Settlement Act (ch. 45) sections 17 to 20 supports the assumption that mediation and compromise are central to customary law, in providing that local land courts should attempt to settle disputes using mediation; a number of the commentators on the act have presumed that mediation inevitably entails compromise. Colquhoun-Kerr and Lakau, "Land Tenure," 72-76, criticize the local land courts in Enga Province because they decided, rather than mediated, disputes. But land court magistrates and others have pointed out that mediation was never part of the traditional dispute manage-

ment process of every group. R. Giddings, "Beyond the Land Courts: Dispute Settlement or Land Distribution," in Institute of National Affairs Public Seminar, *Land Policy and Economic Development in Papua New Guinea* (Port Moresby: Institute of National Affairs, 1981), 37-42; Steven Zuckerman, "Vengeance and Mediation: The Incorporation of Village Courts into Kamano Society," *Journal of Anthropology* 6, no. 2 (1987): 1-33.

22. Hide, *The Land Titles Commission*, 13-14.

23. Native Lands Registration Ordinance 1952.

24. Crocombe and Hide, "New Guinea," 348-349; Colquhoun-Kerr and Lakau, "Land Tenure," 70.

25. Government of Papua New Guinea, Commission of Inquiry into Land Matters, *Report* (Port Moresby: Papua New Guinea Government Printer, 1973), ch. 8; Colquhoun-Kerr and Lakau, "Land Tenure," 70-71; James, "'Unalienated' Land Policies," 36-37; Crocombe and Hide, "New Guinea," 349-350; Hide, *The Land Titles Commission*, 13-14; Jean Zorn, "The Land Titles Commission and Customary Land Law: Settling Disputes between Papua New Guineans," *Melanesian Law Journal* 2 (1974).

26. Commission of Inquiry into Land Matters, *Report*, 113.

27. *Ibid.*, 114.

28. Land Disputes Settlement Act (ch. 45) sections 11, 15, and 27; 27(1)(c) (quotation); 22 and 23; 46, 47, 50, and 51.

29. *Ibid.*, section 35.

30. *Ibid.*, sections 39(1), 40, 39(5), 44 (quotation), 42 (quotation).

31. Government of Papua New Guinea, Land Court Secretariat, "First Regional Land Magistrates Seminar 1979: Report" (Port Moresby, mimeo, July 1980), 25-35.

32. Colquhoun-Kerr and Lakau, "Land Tenure," 79-81.

33. R. Giddings, "Land Dispute Settlement in Enga: Where to from Here?" (Enga Integrated Rural Development Project, Wabag, mimeo, 1980).

34. Government of Papua New Guinea, Land Court Secretariat, "Annual Report 7/79 to 7/80" (Port Moresby, mimeo, October 1980); Eaton, "Customary Land Dispute Settlement," 53.

35. *Augustine Olei v. The Provincial Land Court at Port Moresby, Kwalimu Lofena, Edward Iorive and Bue Goroga* [1984] PNGLR 295, 298; *The State v. Giddings, Ex Parte Tiangan Koam* [1981] PNGLR 423, 424; *The State v. District Land Court Ex Parte Caspar Nuli* [1981] PNGLR 192, 193. The opinions of the Papua New Guinea National Court are collected annually in *Papua New Guinea Law Reports* (PNGLR) and are available in many major law libraries in Australia and the United States. Citations to National Court cases discussed in this article are to the name, date, PNGLR volume, and page number of the case. Local and provincial land court opinions are not published and are available only from the land court involved or, occasionally, from the Papua New Guinea government's Land Court Secretariat.

36. For analyses of the customary dispute-settlement process, see Philip H. Gulliver, *Social Control in an African Society* (London: Routledge & Kegan Paul, 1963); Philip H. Gul-

liver, *Disputes and Negotiations* (New York: Academic Press, 1979); Laura Nader, "Styles of Court Procedure: To Make the Balance," in *Law in Culture and Society*, ed. Laura Nader (Chicago: Aldine, 1969), 69-91.

37. Zorn, "Making Law," 23-25.

38. *Wena Kaigo v. Siwi Kurondo and Others* [1976] PNGLR 34, 37-38.

39. *Ibid.*, 38.

40. *State v. Giddings* [1981] PNGLR 423, 429.

41. *Ibid.*, 429-430.

42. Hide, *The Land Titles Commission*, 14.

43. And usually won't, citing the principle of *stare decisis*, which binds the court to apply the currently effective rule. Courts can, and occasionally do, decide to forgo precedent, to adopt a different rule, but seldom before changing conditions have made the accepted rule totally inoperative. More often, rules change subtly and gradually, through changing court interpretations and applications.

44. *The Application of Nango Pinzi on Behalf of Certain Sio People*, unreported National Court judgment no. N770 (mimeo, 1989). But, interestingly, though the court in *Application of Nango Pinzi* discusses *State v. Giddings* and even praises its logic, the court does not mention that its view of the viability of the rule of original occupancy and that of *State v. Giddings* are opposite.

45. *The Application of Nango Pinzi on Behalf of Certain Sio People*, unreported National Court judgment no. N770 (1989), 4. Although the local land court states that the land is "owned" by the Sio, the decision's definition of "ownership" is closer to a customary law view of the relation of a clan to land (the current generation can do with the land as it likes but may not transfer it away from future generations of the clan) than it is to a common law definition of freehold ownership (under which current owners have the right to sell or otherwise transfer ownership of the land).

46. *Ibid.*, 5.

47. *Ibid.*, 8.

48. And, by the way, applying that provision wrongly. Under section 67 of the Land Disputes Settlement Act, a party establishes rights to land only if it has held the land without the permission of the party claiming to own the land. Here, the provincial land court seems to have stopped when it found that the Kulavi had been in possession for more than twelve years, and did not inquire whether its possession was adverse to the Sio or arose from a grant by the Sio of possessory rights.

49. *The Application of Nango Pinzi on Behalf of Certain Sio People*, unreported National Court judgment no. N770 (1989), 19.

50. This new interpretation of customary law has already produced problems. Customary "owners" of land demand large payments before development projects can proceed (and renew their demands at intervals). This not only slows development (or brings it to a halt, as in the case of the landholders revolt in Bougainville Province) but also means that, through the accident of being on the land where a project occurs, some Papua New Guineans will get much richer than others.

51. *State v. Giddings* [1981] PNGLR 423, 426-427.
52. *The Application of Nango Pinzi on Behalf of Certain Sio People*, unreported National Court judgment no. N770 (1989), 12.
53. *The Application of Ambra Nii on Behalf of Himself and Other Members of the Toisap Clan*, unreported National Court decision no. N1007 (1991).
54. *Ibid.*, 5.
55. *Ibid.*, 8-9.
56. *Ibid.*
57. *The State v. Giddings* [1981] PNGLR 423, 429-431.
58. *State v. District Lund Court, Ex Parte Caspar Nuli* [1981] PNGLR 192, 196.
59. *Ibid.*, 196-197.
60. *State v. Giddings* [1981] PNGLR 423, 428-429, 431.
61. In *State v. Giddings*, it is also probable that the land court's unusual procedure (which included meeting in secret, hearing few witnesses, not permitting witnesses from opposing clans to confront one another, and marking the boundaries with neither clan present) were occasioned by the violence that had been attending Enga land court hearings and attempts to mark the boundaries of disputed land, violence that had led to the closing of the land courts for several months in 1978 to 1979.
62. Hide, *The Land Titles Commission*, 13-14.
63. Gordon and Meggitt, *Law and Order*.
64. *The Application of Nango Pinzi on Behalf of Certain Sio People*, unpublished National Court opinion no. N770 (1989), 19.
65. See, for example, the articles collected in Jean Zorn and Peter Bayne, eds., *Foreign Investment, International Law, and National Development* (Sydney: Butterworths, 1975). The National Goals and Directive Principles reflect these ideas, as well. *Constitution*, Preamble: National Goals and Directive Principles 1-5.
66. W. W. Rostow, *The Process of Economic Growth* (New York: Norton, 1962); W. W. Rostow, *The Stages of Economic Growth* (Cambridge: Cambridge University Press, 1971); Mark Galanter, "The Modernization of Law," in *Modernization*, ed. M. Weiner (New York: Hill and Wang, 1976), 153. For critiques of market theory, see James D. Cockcroft, Andre Gunder Frank, and Dale L. Johnson, *Dependence and Underdevelopment: Latin America's Political Economy* (Garden City, N.Y.: Doubleday, 1972); Francis Snyder, "Law and Development in the Light of Dependency Theory," *Law & Society Review* 14 (1980).

**WHEN UNITY IS TORN ASUNDER:
THE DISTRESSING CASE OF THOMAS AND LUCIA HOLMAN**

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Missionaries pose problems from a historical, social, and even religious perspective. They are the ground troops in a multifaceted war for souls. They carry the banner of a particular type of European or American culture, a conviction that their interpretation of God and the millennium is the only correct one, and a fanatical opposition to other points of view, be they religious, economic, military, or political. At first glance historians and anthropologists have found them to be cardboard figures with facades that reflect the bias of the viewer. Missionaries are imperialists of varying hues; they are the destroyers of indigenous culture; they are the best of a bad lot during the early contact years; they are stiff-necked, corrupt, dedicated; adventurers in sheep's clothing. One thing they are not is representative of human foibles in the nineteenth century.

On second glance we often are able to construct a theory to justify our respective views.¹ I suggest we take yet a third glance to determine who the missionaries were as human beings, their goals and misgivings, their fears and their faith. Such a look will not alter the result of their activities; it will not justify the disruption of indigenous cultures, but it may provide the historian, the anthropologist, the religious apologist with the reality of what the missionaries thought they were doing at the time they were doing it.

An event in the first year of missionary activity in Hawai'i provides such a portrait. An account of it is presented here with the goal of illuminating the aspirations of the pioneer company (1820) to those islands

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through the records of an excommunication trial. This account presupposes the reader's familiarity with Calvinist theology as it discusses the expectations placed on members of the mission and the failure of two individuals to meet those requirements.² The trial and the issues leading to it demonstrate the insecurities, spirit of conformity, and religious zeal that afflicted early Protestant missionaries to Hawai'i. The verdict of excommunication and the expulsion of the offenders from the missionary family reinforced these traits into a spirit of orthodoxy that restricted the admission of Hawaiians into the church until the revival of 1837-1838.

I

The year was 1821 and already the small, pioneer band of missionaries struggling to establish itself in the Sandwich Islands was in disarray. The members were shattered. "If it were the enemy," Hiram Bingham observed, "we could have set ourselves to the battle & *in the name of our God* defy his assaults. But from within, the door is opened, & great does he deem his advantage!" Indeed, it was not the devil, nor the intransigence of unbelieving Hawaiians that left the mission family in such a defenseless position. It was, rather, the seemingly purposeful defection of Dr. and Mrs. Thomas Holman, "who after the most unwearying and faithful efforts to reclaim them still manifest[ed] a determination to pursue a course obviously wrong."³

Thomas and Lucia Holman were members of the pioneer missionary company sponsored by the American Board of Commissioners for Foreign Missions for service in Hawai'i. The company's departure from Boston in 1819 was in danger of indeterminate delay because the evangelists lacked a physician. Samuel Ruggles thought of his sister, Lucia, and her suitor, a physician practicing in Cooperstown, New York. If the doctor could be persuaded to join the missionary cause, events could proceed on schedule; Lucia could marry, and the Ruggles would have the company of kin on this awesome endeavor.

Lucia Ruggles at twenty-six years of age was an independent and strong-minded woman. She was not indifferent to religion or the cause of foreign missions. Her brother, Samuel, was a teacher at the Foreign Mission School at Cornwall, Connecticut, and she had been active in the Society of Butternuts, a fund-raising organization for the Cornwall school, prior to opening a girls' school in Cooperstown, New York. There Miss Ruggles met Dr. Thomas Holman, a recent graduate of Cherry Valley Medical School in New York. The couple fell in love but

could not marry due to the debts incurred by the doctor's unsuccessful practice. Then a solution appeared in the guise of becoming missionaries. Reportedly refusing his father's offer of three thousand dollars to clear his debts, Dr. Holman signed on with the American Board.⁴ The Prudential Committee acting on behalf of the American Board assumed the debts, purchased the necessary medical books, instruments, drugs, and supplies, and sent Holman to Cornwall for training.

Upon initial acquaintance, all seemed to be well between the doctor and his new associates. Hiram Bingham stated that Holman presented "very solid testimonials as a discreet, solid, & pious young man, devoted to the cause of Missions and qualified to be useful both as a Christian and a physician among the heathen." Holman's missionary training commenced in May 1819. By August, however, Herman Daggett, the school's master, observed to Samuel Worcester, secretary of the Prudential Committee, that the doctor had a disposition given to complaints and that he needed to learn humility. Even Samuel Ruggles told Lucia's fiance that if he could not bring himself to live in harmony with the mission family and the rules under which the group lived, he had better stay at home.⁵ This would prove to be an important consideration. Members of the pioneer missionary company to Hawai'i expected to emulate the early Christian church, holding all things in common and creating a family community based on mutual consideration and trust. If Holman did not lose his abrasive manner, the harmony of the mission would be disrupted.

Of course, if Holman had taken Ruggles's advice, he would have lost his bride and regained his debts. The couple married in September and departed aboard the *Thaddeus* in November 1819. Bingham later placed much of the blame-for the disruptions caused by the Holmans on the staff of the Foreign Mission School. Bingham asserted they should have withheld their approval of Holman's candidacy until they knew he could resist temptation and walk the true path. But the staff had had no real choice. If they had rejected Holman the entire enterprise would have been held back for want of a doctor. Besides, they must have reasoned, the doctor's brother-in-law was Samuel Ruggles, a man of proven integrity and religious zeal. Surely that would mean something.⁶

The expectations of both the Christian life and the missionary calling were quite clear. The Calvinist tradition, in particular the legacy of Jonathan Edwards and the New Divinity, stressed the unity of will between the regenerate Christian and God.⁷ The converted soul ceased to want his or her own--selfish--desires but sought only the more perfect purpose of God's glory. So great would be the love of God that the

converted would willingly suffer damnation if it would further God's plan, but he or she would not choose damnation or any other path. It was truly a case of the right hand not knowing what the left was doing. Within this context, an individual might receive a call to foreign mission. Such a call required selfless devotion and certain martyrdom in foreign climes.

Theologically, then, the missionary did not look for personal happiness or gain, but for the furtherance of God's kingdom. This point was institutionalized by the American Board in 1815 when the sixth annual meeting declared that every missionary employed by the American Board was to be solely dependent on the board for support and that any earnings by a missionary or his wife became the board's property for the greater object of the missionary cause. Further, the American Board stated that at any missionary station with more than one missionary, all salaries, presents, and possessions would be part of a common stock. There would be no individual ownership of property or supplies and no individual wealth. All was subsumed within the greater cause of mission.⁸

The instructions issued by the Prudential Committee to those missionaries departing for the Sandwich Islands in 1819 bound the participants even as they admonished the small band. The instructions emphasized that "if a Christian is devoted to Christ, the minister is especially devoted & the missionary even more so." The first point stressed to each individual, "if you have renounced the world, be sure it is without reserve. It is hard enough to live the divine life here. What will you do there if you aren't devoted heart, soul & body to Christ?" The contract between the evangelists and the American Board was based on this renunciation and was considered valid only so long as the individuals conformed to the instructions, which reiterated the 1815 ruling. The Prudential Committee realized that "living so close to one another and so far from the world, there will be disaffections. Brotherly love may only continue via much vigilance, much prayer, crucifixion of self & sanctifying grace." It was expected that the missionaries would do all within their power to strengthen the ties of their fledgling church and mission family even as they fulfilled the tasks of bringing literacy, "civilization," and "Eternal Life" to the unbelieving Hawaiians.⁹

The mission to Hawai'i, however, did not involve just the American Board, the missionaries, and the Hawaiians. It encompassed the millennial hopes of the entire New England Christian community. "Beloved Members of the Mission, Male and Female, this Christian Community

is moved for you, and for your enterprise. The offerings, and prayers, and tears, and benedictions, and vows of the Churches are before the throne of Everlasting Mercy. They must not be violated; --they must not --cannot be lost.”¹⁰

The mission family was bound together in a great enterprise, but the binding was not as strong as the majority of members had hoped. Thomas and Lucia Holman allegedly began almost immediately to disrupt the unity other members of the young mission family found so vitally important. During the voyage the Holmans began to express their intention of acquiring property in Hawai'i and then returning to America. The doctor stated that he had not understood the instructions at the time of embarkation and that he “did not now nor did he ever feel it to be his duty to engage to hold his *earnings* or his *art*, at the disposal of the Board or of the mission, in such a sense that he could not if he pleased acquire personal property & return at pleasure to his native land.”¹¹

Such sentiments shocked Bingham and the others. Holman now clearly stated that he had never intended to spend his life in the field, and that while his services to the mission would be free, others must pay. “But,” sputtered the Reverend Mr. Bingham, “the plan of this mission, & the unequivocal instruction of our Patrons do not allow us to set up private wealth as an object.” The doctor responded, “You know very well the situation we were in, when those instructions were given. --I did not understand them; & I question whether you did yourself at the time.” Bingham protested that he “did understand them--they were such as I was looking for, --such as I had long desired, & such as I was glad to hear.” “But I,” asserted Thomas Holman, “do not feel myself bound by them any further than they accord with my original plan. I felt willing to spend a few years in the practice of physic among the heathen, --& if my services would aid the mission, & promote the civilization of the natives, I should be glad of it. But why should you feel concerned about my earnings unless you think I can earn more than the rest.” There was little more to be said in the face of such blatant fractiousness. The mission family could only hope that if it behaved with kindness and forbearance and received the assistance of divine blessing, the young man might be “reclaimed, reformed & saved.”¹²

The doctor's bride also gave cause for concern. The couple's absorption in each other and the groom's extreme attentiveness to his wife's every desire were offensive to the others. Public displays of affection seemed an affront to the greater purpose of the journey. Despite such

secular interests, however, Lucia Holman was not unaware of the great work, and if she did not completely relinquish her past life, neither did she cling to it.

"This year," confided Mrs. Holman on 31 December 1819, "has witnessed the most trying yet interesting scenes of my life. A new course is marked out for me to pursue: new hopes, new joys, and new sorrows are before me. I often review with pleasure the past scenes of my life, tho this pleasure is mingled with regret that they are never more to be realized." As to her new tasks among the Hawaiians, the young woman exclaimed, "Yes, with the eye of faith I can look forward to the day when the sons & daughters of [Hawai'i] . . . shall become the true & humble followers of the Prince of Peace." Later, Mrs. Holman decided that she was not the one to effect such a change, but aboard the *Thaddeus* her sentiments and apprehensions were little different from those of her missionary sisters.¹³

Mrs. Holman did deviate, however, in her shipboard activities. She did not attend the sessions led by Mrs. Bingham and Mrs. Thurston to instruct the ladies in grammar, rhetoric, and geography. She, too, had been a teacher and felt no need of the lessons, especially when she suffered from seasickness. But the absence was noted and served to isolate Mrs. Holman from the other ladies.

Mrs. Holman also had poor relations with Hiram Bingham. Bingham accused Mrs. Holman of hoarding fruit that her brother, Isaac Ruggles, had given her in Boston. She defended her position, but the seeds of suspicion were planted. Dr. Holman later accused Bingham of telling his wife that she was "an improper person for a missionary" during one of their conversations regarding the fruit.¹⁴

Whether or not the Holmans had any aptitude for mission, they seemed to have had none at all for diplomacy. By the time the *Thaddeus* arrived at Kailua-Kona on the "Big Island" of Hawai'i in 1820, the couple had alienated most of the company. Only the Ruggleses could still be counted as friends. The others hoped for the best. Ironically, it would be the technology of medicine that first attracted the attention of Kamehameha II (Liholiho) and enabled the missionaries to establish their first toehold in the Sandwich Islands.

II

Liholiho was cautious when the New Englanders asked permission to establish a mission station at Honolulu. There were rumors, encouraged by the king's English advisor, that the Americans would interfere with

Hawaiian politics and disrupt amicable relations with Great Britain, Perhaps their intention was to incite a rebellion or monopolize Hawaiian commerce. The evangelists explained that they merely wanted to teach a new religion. But, having only abolished the state religion in the fall of 1819, the king had no desire for a replacement. The missionaries offered literacy, which elicited some interest, and the services of a doctor. The last caught Liholiho's attention. The doctor, he decided, would remain with the court. The others were given permission to stay in the islands for one year.¹⁵

Not wishing to leave the Holmans alone in their new situation, the Reverend Asa Thurston and his wife, Lucy, also remained at Kailua-Kona. In retrospect, Bingham and Thurston testified that Holman selected the Thurstons and Thomas Hopu as companions. The doctor, however, recalled that ballots were cast and that Thurston was not pleased when the assignment came to him. The remainder of the mission family went on to O'ahu, but "it was expected, it was said & the Dr. understood it so . . . that with respect to the family proceeding to [O'ahu] in case of fever, or in other cases of urgency, it would be his duty to visit them." The cases of "urgency" were primarily the confinements of several wives. It was the expectation of such events that had made the inclusion of a physician so important to the company.¹⁶

Meanwhile, Dr. Holman attended one of the king's wives and several of his servants, all of whom recovered. Had the doctor failed, his life could have been forfeit. Inasmuch as he succeeded, the Holmans received gifts and provisions that they only occasionally shared with the Thurstons. The doctor also enjoyed some influence with the king but did not utilize this advantage in extending the Gospel. "Never . . . had a medical man a better opportunity to make a good impression as a pioneer of science, civilization, and Christianity, than he enjoyed," Bingham wrote, but the doctor threw away such opportunities, Relations between the Holmans and the mission family remained strained. Thurston and Bingham corresponded on the doctor's growing alienation and spirit of divisiveness.¹⁷

The Holmans and the Thurstons often shared only the barest civility. Lucy Thurston wrote Sybil Bingham of an altercation over the issue of sharing water, at which time Dr. Holman clearly stated his intentions to live in his own dwelling, "that it was not his intention to remain a member of the mission--that at a future period he intended to return to his native country--that the medicines in his possession he considered *his own*." Holman also spoke of "the dignity of his profession--the superiority it bore contrasted with Mr. T's--& his being made instrumental of

this mission's being received & so comfortably situated." It was all very upsetting to people believing in the unity of Christ.¹⁸

While Dr. Holman concerned himself with his secular profession, his wife fluctuated between her Christian commitment and human frailties. Even aboard the *Thaddeus*, "at a time when each of the family needed the support of the others, [Mrs. Holman] allowed herself to express a seeming regret that she had embarked." The others expressed no verbal doubt of their calling and kept their fears to themselves. Hiram Bingham remonstrated with Mrs. Holman on this topic with some good effect, but when the doctor learned of the conversation, he found the reproof to be an "ungentlemanlike abuse of a delicate female."¹⁹

Lucia Holman's second thoughts became stronger as she encountered her first mission station. The culture shock was intense for the entire mission party, but, once again, Mrs. Holman was the only one to express a desire to leave the mission family. She verbalized her reactions to Lucy Thurston. "I do not find things here as I expected," Lucia confided to her colleague, "I do not feel for the heathen in being among them as I formerly did--reading or hearing of their miseries-- If there are any that do feel for [the Hawaiians]," she went on, "& possess that self-denying spirit which is necessary to live among them & do them good, I am glad of it-- It is for *them* to do the work-- But, as for myself I do *not* possess these feelings & consequently cannot be useful among them--& I intend to embrace the first opportunity to return to my native land." Mrs. Holman had gone beyond a mere apprehension that she did not possess the proper spirit. She now declared "she would never be willing to exercise that degree of self-denial, which was called for" by the situation. Such thoughts might be honest, and the conclusions logical, but they were not appreciated.²⁰

Lucia Holman's ambiguous commitment was further taxed by physical privations. She complained of her discomfort while the others endured their disappointments in silence. Kailua-Kona is on the dry side of Hawai'i island. Water was five miles away from the mission station. There was no arable garden land and few fresh provisions. The stress of environmental change, physical labor, and mental tension took its toll on an already tenuous commitment. Lucia's position became increasingly ambiguous as she began to complain of her health and the desire for plentiful water, good food, and agreeable company. At the end of the summer she wrote that in all her "trials of sickness and privations by sea & land, I have never regretted my undertaking," and, yet, unlike others in the company, she would be pleased to return home again.

“Could any female,” Lucia further reflected, “have known before she left home, *all* the trials and afflictions through which she must inevitably pass, she would not *of herself* have strength or grace to enlist in so great an enterprise. I think,” she went on, “I may say the same of men.” Two months later Mrs. Holman elaborated on that theme, saying that “it was pride & selfishness and the desire of a great name that influenced every one of the mission family to come out here.”²¹

The doctor had an overabundance of pride and a fractious nature, but his wife possessed a tongue both loose and sharp. It is difficult to say which behavioral traits were the most distressing to the mission family, Both represented the enactment of private doubts and fears that the family sought to deny. They were a small band in a strange place. They could not go back; such an act would deny their calling and perhaps their conversion. It was not a question of making a mistake, but of eternal salvation. The charge of seeking fame was easily denied, but the greater issue of coming out for personal gain--that is, the assurance of salvation--raised the question of motive. If the motivation was even slightly self-interested, then all sacrifice was for nought and damnation more than probable. It was, indeed, a psychic disaster to listen to the Holmans. Such a situation could not continue.²²

III

The tension between the Holmans and the mission family reached crisis proportions when the Holmans moved to Lahaina, Maui, without the approval of the church. The move touched on four issues, but the major point was authority. The ostensible reason for the move was Lucia Holman's health. The couple went to Lahaina rather than Honolulu because that was where the king granted them permission to travel. In the uproar of moving, Dr. Holman failed to appear for Maria Loomis's confinement. For flouting authority, placing the missionary enterprise in jeopardy, and endangering the life of Mrs. Loomis, the Holmans were placed under censure.

If there is blame to be fixed, it would seem to revolve around Lucia Holman and the high regard her husband had for her. Unlike the resourceful Lucy Thurston, Lucia felt increasingly overwhelmed by her calling and was quickly disillusioned. She did not have any particular affinity for the Thurstons and later wrote Samuel Ruggles that isolation among the Hawaiians was preferable to “the society of [those] who feel and conduct towards me as if a stranger.” In short, Mrs. Holman was unhappy and unsuited to her new environment. In fact, she had

expressed her desire to depart almost upon arrival, causing Bingham to respond "in his usual taunting way get away if you can!" (Bingham denied this alleged conversation.) Dr. Holman testified the move was necessary to protect his wife's health. Bingham and Thurston said Mrs. Holman never mentioned her health problems to her Christian sisters and even admitted her conduct had nothing to do with her health. In a letter to her brother, Lucia insisted that the removal was her desire, "and at first quite contrary to the will of my husband."²³

From the church's point of view, if the Holmans insisted on leaving Kailua-Kona, they should have appealed for permission from the church and the king, and moved to the main station at Honolulu. The family believed the doctor had made his plans for Lahaina without even informing the Thurstons. This would leave the Thurstons in isolation and raise Liholiho's suspicions, because the mission family at Honolulu believed the king was jealous of Kalanimoku and Ka'ahumanu, who were at Lahaina.²⁴ By angering the king the Holmans placed the entire missionary enterprise in jeopardy. Later, the charge was also made that Dr. Holman was becoming increasingly discontented "as he saw the good name of the leaders rising, & gaining influence."²⁵

Dr. Holman denied leaving Kailua-Kona by devious means. "Did I not," he queried, "more than once or twice ask the counsel of brother Thurston on the subject?" But Thurston replied, "in an unfriendly, unbrotherly manner, Thus, 'I don't know' & would not converse with me at that time any more on the subject." The doctor also stated that he had made a written request to Honolulu regarding the move to Lahaina and received no reply. The choice of Lahaina was made by the king. The doctor admitted he had moved without mission family permission, but if he had waited, Holman feared the king might have changed his mind.²⁶

The charge that Holman had placed his own interests above those of Mrs. Loomis's health was a harsh one. The doctor declared, however, that he did not attend her confinement because he was told he was not needed, an assertion both Bingham and Thurston denied. Mrs. Loomis delivered without complications, fortunately.²⁷

On 13 July 1820 the brethren issued a united remonstrance signed by Hiram Bingham, Daniel Chamberlain, Samuel Ruggles, and Elisha Loomis. They wrote of the fine work Holman was doing but stressed his duty to the mission and his acceptance of the general instructions before the company's departure from Boston. After arriving at Lahaina, Holman responded that he was pleased to be on Maui "and should you see fit to withdraw me from your fellowship & support yet I am *confi-*

dent that God . . . will continue to provide all things necessary for my *usefulness, wants & happiness.*” Lucia Holman’s entreaty that her husband should not be censured “for the faults that I have committed” did not soften the tone of Thomas’s defiance. The church suspended Dr. Holman from all privileges on 8 August 1820 and placed him under censure pending a fuller investigation. In the interim, the doctor was expected to continue to fulfill his medical obligations.²⁸

Lucia Holman continued to waffle. Shortly after the move to Lahaina, she tried to mend fences with Lucy Thurston. “I verily believe that great good can be affected among [the Hawaiians] with proper means--but I need not tell you never to expect that from me. I only ask your charity to believe that I do not intend to do any harm.” Indeed, this contrite woman was “willing to live forgotten among mankind if I can live in peace--enjoy the pleasures of a quiet conscience--void of offense towards God & man.” Not surprisingly, her plea fell on deaf ears. Thomas Holman wrote that his wife did not receive the interest or sympathy she had expected, causing a decline in both her health and spirits. “Should she continue in this frame of mind, with no more prospect of relief, I shall feel it my absolute duty to return [to the United States] with her.”²⁹

The evidence seemed to be mounting that the Holmans were looking for a way out of their obligation. Surely the removal to Maui was indicative of this intent. Lucia Holman disagreed. “We never thought nor spoke of separating from the Mission,” she wrote prior to her husband’s suspension. “No! Far be it from me or my dear husband to wish to separate from this family.” But should the brethren decide to separate, “I will feel myself happy to be alone.”³⁰

Alone the Holmans would soon be. The mission family tried to sort out why the Holmans had moved--whether from maliciousness or misunderstanding. Daniel Chamberlain concluded by November that the Holmans’ version was not to be trusted since it changed almost daily. Bingham blamed Mrs. Holman for urging her husband to measures that could only result in censure. Dr. Holman continued to assert that he was doing his duty and was, therefore, neglected and abused.³¹

The entire issue was crucial to the identity of the mission. The mission family as a whole accepted fully the philosophy of the American Board as expressed by Dr. Leonard Woods of Andover Theological Seminary on the occasion of the departure of missionaries to Asia in 1812. A “Christian presents himself a living sacrifice unto God; and counts it a privilege to do and to suffer any thing for the advancement of his cause,” a point that had been rearticulated in the Prudential Commit-

tee's instructions issued in 1819 for the Sandwich Islands mission.³² Missionaries did not belong to themselves, but to God. Their work among nonbelievers was dedicated to saving souls and, by so doing, providing an example of the godly life for Christians at home.

Initial contact with the Hawaiians had been a traumatic experience requiring renewed dedication to the object of the mission in order to cope with culture shock and a new environment. The Holmans threatened the psychic unity of the group. If the doctor who had been equally charged with this duty could so openly flout church authority and even suggest abandoning the mission, then every member was at risk. And, in accordance with their theological and cultural beliefs, that risk of failure would mean not only public humiliation, but also eternal damnation. The Holmans could not be permitted to continue their disruptive behavior, but at the same time the missionaries felt it their duty to make every effort to bring Thomas and Lucia to repentance and a renewal of their commitment.³³

At length, the family made a decision. Chamberlain questioned Holman closely to demonstrate that the doctor's position was without merit. During the questioning Dr. Holman stated: "Mr. Chamberlain I'd have you know that the blood that runs in my veins was born free, & I'm determined, it never shall be bound by any man." To which the Reverend Mr. Bingham responded, "We do not wish to change the current of your blood, we only wish you to behave decently." But the time of reconciliation was past. "Your brethren having suspended you from the fellowship without excluding you from the pale of the church, have long waited for you to wipe away the stain & heal the wound, which you have brought upon this little branch of Zion, upon the cause of missions, & on the cause of Christ in general; --but they have waited in vain-- They have sat down by the turbid waters of Babylon, & waited & wept in vain." Only Samuel Ruggles argued that Dr. Holman be given more time to repent. With sadness and determination, Bingham and Thurston drew up the letter of excommunication, charging Thomas Holman with "walketh disorderly" (2 Thess. 3:6), "slander & railing" (1 Cor. 5: 11), and "covetousness" (1 Cor. 5: 11). The motion to "publicly, & solemnly, deliver [Thomas Holman] over into the visible kingdom of Satan & declare you and to the world, *that you are, & of right ought to be excommunicated from the church of Christ, & no more entitled to the fellowship or the privileges of his kingdom on earth*" passed by unanimous vote on 31 January 1821. That same date Lucia Holman was placed under suspension.³⁴

Mrs. Holman had received her first admonition on January 16 and

made no effort to repent. The church charged Mrs. Holman with persuading her husband to move to Lahaina and declaring that if such was grounds for dismissal she would rejoice. Such actions were unbecoming in a church member, and in a female missionary they constituted "walking disorderly." Lucia was also charged with possessing an improper spirit and manner that manifested itself as "evil speaking." Her greatest crime, however, was the same as the doctor's. "Any feelings, conduct or expressions, inconsistent with the full exercise of holy benevolence, are contrary to the duties which we owe to God & to each other as subjects of this kingdom. Every particular branch of it must be governed by the same laws that regulate the whole." When a member departed from the path of duty, every effort must be made to reclaim him or her. But neither Lucia nor Thomas Holman wanted to be reclaimed.³⁵

The decision of excommunication and suspension was a difficult one. Maria Loomis wrote that the "subject is too painful to dwell on. It is deeply felt by every member of the family." Bingham lamented "the defection of Dr. Holman. --Lord what is man!" Samuel Whitney was less forgiving. He found Dr. Holman's continued residence with the family an inconvenience since he did not care to share a meal with one under excommunication. Whitney was also Ruggles's associate on Kaua'i, and could scarcely speak with him on the subject.³⁶

The church submitted a report of the charges and proceedings to the Prudential Committee. Dr. Holman submitted his version of the dispute, and the family included its response. In a letter to Bingham, Thomas Holman struck a conciliatory tone, but stated his belief that "I have not been properly treated, as a brother, a friend, or a stranger, or even a menial servant of a commonly good character." In his response to the charges submitted to the Prudential Committee, he continued this approach rather than dealing directly with the charges. An unsigned letter from a mission family partisan insisted that Holman's "paper is altogether offensive in its aspect & character. He seems to think his own case will appear fair, if he can attach disgrace to Mr. Bingham." The writer concluded that Thomas Holman did not exhibit "a single expression of genuine grief on account of the unhappy spectacle presented to the heathen--or of sorrow that he was compelled to leave a mission, to which he had publically devoted himself, & to which he was bound by the most solemn ties."³⁷

The Prudential Committee considered the Holman case at their meeting of 7 June 1821. The committee found that Dr. Holman's reasons for leaving Kailua-Kona were not satisfactory; that he be required to turn over all medicines, medical books, surgical supplies, and other Ameri-

can Board property; and that "no person can be considered as belonging to the mission at the Sandwich Islands unless upon the principles expressed in the Public Instructions of the Committee delivered in Boston, Oct. 15, 1819." It remained only to find a suitable ship for the Holmans' departure and to attempt to reclaim Lucia Holman before she, too, was irretrievably lost.³⁸

Mrs. Holman, however, remained out of the fold. "We laboured, but in vain, to make her sensible of the dishonor she had brought upon the Church," Elisha Loomis reported. "She maintains that she has conducted herself aright--that she is unconscious of having slandered the members of the church--that she has been wronged--and esteems it her highest happiness that she will one day be able to make known her sufferings to the Christians of America." The latter plan was one the American Board had hoped to avoid; such publicity would be harmful to the missionary cause.³⁹

In fact, Lucia Holman was enjoying her return to the secular world. Maria Loomis noted that the doctor's wife was receiving presents of every description from the antimissionary faction and left in excellent spirits, seemingly "quite insensitive to the injury she has done & is still doing to the cause of Christ." Hiram Bingham expressed his distress at Lucia's "pleasure in *going home*; --the complacency shown in the multiple attentions of the sea-captains towards her--the confidence expressed in God--the joy also at leaving the mission family." The reverend had hoped the family could bear the dispute with none the wiser. It would seem, then, that the sorrow was not just for the lost sheep and the disruption of family unity, but for the more secular concern of bad publicity.⁴⁰

The Holmans departed for Canton aboard the *Mentor* and ultimately made their way back to Boston via England, arriving in May 1822. Lucia Holman became the first American woman to circumnavigate the globe. The brethren in Hawai'i were "not sorry [the Holmans] have left this place. The extent of the injury they have done this Mission, and the cause of Christ can never be fully known till the great day when all men must give an account to God."⁴¹

IV

"The conduct of Dr. Holman gives great pain in this country," observed Jeremiah Evarts, "so far as it is known. The distressing issue is indeed known universally; but the particulars not at all by the public at large." Indeed, Mr. Evarts hoped some good might come out of the Holman

case. It demonstrated what can happen when one “departs from a mission and shows himself before the whole Christian world, destitute of common integrity.”⁴²

Destitute became an operative word in Thomas Holman’s life. The doctor tried to open a practice in Bridgeport, Connecticut, but the onus of excommunication denied him both patients and society. He appealed to the American Board to vindicate him. When that body reiterated their position and pointed out that under the circumstances Holman was obligated to reimburse his expenses, he became angry and demanding. “I do not feel under any obligation whatever ‘to reimburse the treasury of the Board’ any *expenses* of my *outfit*, passage etc.--but on the other hand, the Board is morally and legally bound to remunerate me for the time spent in their employ, and for my services to the Mission family, from the time of my leaving America to the time of my quitting the Sandwich Islands.”⁴³

Holman’s arguments fell on deaf ears insofar as the American Board was concerned, but several “good men” including Pastor Waterman of Bridgeport supported the doctor’s case. The American Board chose not to respond to Holman’s accusations, because any attention given to the case not only deflected from the greater cause of foreign mission, but also provided ammunition to the enterprise’s enemies. The board sadly concluded that Thomas Holman’s hindrance to the cause was far greater than his service had been, for he had caused “incalculable trouble, shame, confusion, distress, & wasting of spirits.” He died 20 March 1826 at the age of thirty-three, a failure as a doctor, as a missionary, and as an aspiring man of property.⁴⁴

The widow Holman, however, achieved worldly fame and property. Lucia returned to her birthplace of Brookfield, where she met and married Daniel Tomlinson, a prominent, propertied man. After that gentleman’s death in 1863, Lucia Ruggles Holman Tomlinson moved to New Milford, Connecticut, where she died twenty-three years later. In an ironic twist, Lucia outlived the entire pioneer missionary company and it was she who represented them for a nostalgic public.

V

There is no question that Thomas Holman was in clear violation of the Public Instructions issued in 1819, and it is highly improbable that he misunderstood them. The basic concept that a regenerate Christian must eschew worldly gain was well known. The expectations for a foreign missionary were even higher. The description of the missionary call

was also well publicized via the *Panoplist*, the *Memoirs of David Brainerd* (c. 1747), David Bogue's *Defense of the Cause of Mission* (1811), and other reading material. Dr. Holman knew the requirements; so did his wife.

It is also clear that the doctor had never possessed a missionary spirit. His dislike for authority was immediately apparent, as was his argumentative nature. It was his profession and family connections that secured his place in the company, and his need for money that made him accept it. It is evident that the doctor wanted to do more than pay his debts. Otherwise, he would have accepted his father's offer and stayed home. Thomas Holman was being honest when he said he wanted only to acquire property in Hawai'i and then return to the United States.

Indeed, Thomas Holman was generally honest about his intentions, but his wife was not. To be fair, Lucia Holman seems not to have been entirely sure what her intentions were. She liked the romantic notion of uplifting islanders; she also liked the thought of returning to America with wealth. Most of all, she liked the idea of being married and receiving her husband's attentions. All of Lucia's aspirations were tied to the mission. The altruistic notions were encouraged by brother Samuel; the avaricious ones by husband Thomas.

Lucia Holman might have been more enthusiastic about the mission and thereby influenced her husband in a more service-oriented view if the Ruggleses had shared the station at Kailua-Kona. It had never really occurred to Lucia that Samuel and Nancy Ruggles would not be her companions. The desire to be closer to her brother suggests one reason why Lucia wanted to leave Kailua-Kona and why she made no effort to bridge the gap with the Thurstons or dissuade her husband from his acquisitive proclivities. It was undoubtedly a bitter pill for Lucia Ruggles Holman when her brother signed the documents against her and her husband.

If the case had been simply a legal matter, it would have been quickly settled. But the situation can be likened only to a divorce. The others had taken their vows as a church and a mission family with the utmost seriousness and truly believed they were fulfilling God's will. The disruption of that unity, the breaching of those sacred vows, cast everyone's position in doubt. Excommunication became the only way of restoring psychic unity and redirecting the company's focus to the task at hand.

The scars of the Holman apostasy remained, appearing each time a candidate applied for the position of missionary, each time an unbe-

liever applied for baptism. They made the mission family cling more closely to each other and anguish over the repatriation of members for reasons of health or unsuitability. Thomas Holman also carried those scars, as he spent his last years seeking vindication. Only Lucia Holman seemed to remain untouched in the furor, glad for the service foreign mission had done her and just as glad to leave it.

VI

The Holman case is more than a family squabble in which the black sheep are ejected from the manse to make their way in the world. It is a mirror that reflects the fears, convictions, and tenuous unity of the first missionary company to Hawai'i. It is a crystal ball that projects the difficulty these pious men and women had in accepting the sincerity of indigenous conversions to Calvinism. If they had been fooled by members of their own culture, how could they trust themselves to correctly assess the religious conviction of a people whom they did not begin to understand? The instructions they had received on their departure to be cautious in accepting professions of faith became an article of law in the work of the Sandwich Islands mission.⁴⁵

The repercussions of the Holman apostasy were equally felt by subsequent missionaries. Works became an important test to determine whether a candidate's religious conviction was genuine. The decision of Ka'ahumanu and other prominent chiefs to patronize missionary instruction, particularly in the schools, and to suppress vice in June of 1825 paved the way for their baptism in December. The first laws of the Hawaiian kingdom were issued shortly thereafter.⁴⁶

Yet, even changes in behavior could not reveal the true faith of the candidate. In 1826 William Richards, a member of the second company who later became the first minister to join the government (1838), wrote the American Board that he "had been growing particularly anxious lest the people should settle down satisfied with the more outward performance of the duties of Christianity, to the neglect of that which alone can save the soul."⁴⁷

Missionaries are in the business of saving souls. Their methods are determined by their theology and their personal interpretation of that theology. The early missionaries to Hawai'i were bound by a rigid Calvinism that did not allow flexibility in its implementation. The Holman affair increased the propensity of these missionaries toward a literal interpretation of the signs of conviction and conversion among the Hawaiians. On 24 April 1828 Bingham reported there were fifty

Hawaiians in the church, a small return for eight years of work.⁴⁸ We will never know what the results might have been if the fear of apostasy had not dominated the collective psyche of the mission family. Neither will we know the extent to which the low number of Hawaiian church members affected missionary attitudes towards the Hawaiian people, an important point when one considers that during this same period missionaries became trusted advisors to Hawaiian chiefs.

NOTES

1. This article has been developed from a paper read at the Rocky Mountain Regional Conference of the World History Association in July 1990 entitled "The Holman Window to Enigmatic Martyrs." A discussion of the incident within the broader context of the structural and personal motivations that resulted in the formation of the first two companies of missionaries to the Sandwich Islands may be found in Sandra Wagner-Wright, *The Structure of the Missionary Call to the Sandwich Islands 1790-1830: Sojourners among Strangers* (San Francisco: Mellen Research University Press, 1990). The author would like to thank David C. Purcell, Jr., for his comments. For general accounts of missionary activity in Hawai'i, see Harold W. Bradley, *The American Frontier in Hawaii* (1942; reprint, Gloucester, Mass.: Peter Smith, 1968); Gavan Daws, *Shoal of Time* (Honolulu: University Press of Hawaii, 1982); Ralph S. Kuykendall, *The Hawaiian Kingdom*, vol. 1. (Honolulu: University Press of Hawaii, 1978). Apologists for the cause of missions include Samuel C. Bartlett, *Historical Sketch of the Missions of the American Board in the Sandwich Islands, Micronesia, and Marquesas* (1876; reprint, New York: Arno Press, 1972); Oliver W. Elsbree, *The Rise of the Missionary Spirit in America* (1928; reprint, Philadelphia: Porcupine Press, 1980); Joseph Tracy, "History of the American Board of Commissioners for Foreign Missions," in *History of American Missions to the Heathen from Their Commencement to the Present Time* (Worcester, Mass.: Spooner & Howland, 1840). For a focus on the political and economic motivations of missionaries, see Aarne A. Koskinen, *Missionary Influence as a Political Factor in the Pacific Islands* (Helsinki, 1953); Clifton J. Phillips, *Protestant America and the Pagan World* (Boston: Harvard East Asian Monographs, 1969). Institutional motivations for the entire missionary enterprise to Hawai'i are discussed by John A. Andrew, *Rebuilding the Christian Commonwealth* (Lexington: University Press of Kentucky, 1976). For a discussion of the particular experience of women involved in the Sandwich Islands mission, see Patricia Grimshaw, *Paths of Duty: American Missionary Wives in Nineteenth-Century Hawaii* (Honolulu: University of Hawaii Press, 1989).

2. The pioneer company of missionaries to Hawai'i subscribed to Samuel Hopkins's theology of disinterested benevolence, in which the believer's will is so subsumed in God's will that he or she would cheerfully accept a descent to Hell if it would further God's work. One of the most difficult aspects of this theology is that if a candidate chooses salvation to avoid damnation, rather than from true conviction, he or she is worse off than if conversion had not been experienced, because conversion should only occur when the individual's will has merged with God's, not for personal gain. For a full discussion of the particular Calvinist theology of the American Board of Commissioners for Foreign Missions (ABCFM) from 1810 to 1825, see Wagner-Wright, *Structure of the Missionary Call*.

3. A note on sources: This article is based primarily on two reports sent from the church at Honolulu, O'ahu, to the American Board at Boston. Hiram Bingham and Asa Thurston to Samuel Worcester, 15 Feb. 1821, TS, Hawaiian Mission Children's Society Library, Honolulu (hereafter cited as HMCSL). Hiram Bingham and Asa Thurston, "History of the Defection of Dr. Tho. Holman," 11 May 1822, Missionary Letters (hereafter cited as ML), HMCSL. Aside from statements by the Holmans, there is no evidence refuting the charges.

Hiram Bingham, "Journal of Mr. Bingham's Tour to Atooi [Kaua'i]," 1 Oct. 1821, ABCFM-Hawaii Papers, Houghton Library, Harvard University (hereafter cited as ABCFM-H), HMCSL. Elisha and Maria Loomis, Journal, 28 Nov. 1820, Journal Collection (hereafter cited as JC), HMCSL.

4. Bingham and Thurston, "History of the Defection," p. 16.

5. H. Bingham to Samuel Worcester, 11 May 1819, ML, HMCSL. H. Bingham to Jeremiah Evarts, 2 Nov. 1820, ML, HMCSL. Andrew, *Rebuilding the Christian Commonwealth*, 113. Albertine Loomis, *Grapes of Canaan* (New York: Dodd, Mead, 1951), 72. Char Miller, *Fathers and Sons* (Philadelphia: Temple University Press, 1982), 35.

6. Bingham to Evarts, 2 Nov. 1820.

7. Jonathan Edwards first described the phenomenon of the First Great Awakening in 1740. He sought a return from Puritan declension and contractual relationship with God to a recognition of God's absolute sovereignty. He introduced the concept that the truly converted will demonstrate their change of heart by their fruits, or deeds. Edwards's followers became known as the New Divinity Men and included Joseph Bellamy and Samuel Hopkins, who attempted to build a complete and consistent system of practical, evangelical Calvinism. The impetus for the Second Great Awakening (1790 to 1840) was initially based on their work.

8. *Panoplist*, November 1815, p. 341.

9. Heman Humphrey, *The Promised Land . . . Sept. 29, 1819* (Boston: Samuel T. Armstrong, 1819), ii-iv, viii-ix.

10. *Ibid.*, xv.

11. Bingham and Thurston, "History of the Defection," p. 26.

12. *Ibid.*, pp. 27-29.

13. Lucia Holman, *Journal*, Special Publication, no. 17 (Honolulu: Bishop Museum, 1931), 10. For a discussion of the activities of missionary wives while in transit to Hawai'i, see Grimshaw, *Paths of Duty*; Wagner-Wright, *Structure of the Missionary Call*.

14. Thomas Holman to Prudential Committee, 14 May 1822, ABCFM-H, HMCSL.

15. Bradley, *American Frontier*, 126. Kuykendall, *Hawaiian Kingdom*, 1:103.

16. Bingham and Thurston, "History of the Defection," pp. 29-30. Thomas Holman to Prudential Committee, 21 Nov. 1820, ML, HMCSL.

17. T. Holman to Prudential Committee, 21 Nov. 1820. H. Bingham, *A Residence of Twenty-One Years in the Sandwich Islands* (1849; reprint, Rutland, Vt.: Charles E. Tuttle Company, 1981), 104.

18. Bingham and Thurston, "History of the Defection," pp. 40-41.

19. *Ibid.*, pp. 7-8. Bingham went on to say that such a response "has been the fate of every brother or sister who has had the kindness to remind [the Holmans] of their faults." Lucy Thurston, *Journal* (Printer's Copy), p. 48, ML, HMCSL. Bingham and Thurston to Worcester, 15 Feb. 1821, p. 41.

20. Bingham and Thurston to Worcester, 15 Feb. 1821, p. 41.

21. L. Holman, *Journal*, 37-38. Daniel Chamberlain to S. Worcester, 14 Nov. 1820, ML, HMCSL.

22. See Henry N. Wieman and Regina Wescott-Wieman, *Normative Psychology of Religion* (New York: Thomas Y. Crowell, 1935). It is suggested that the conversion process brought the regenerate Christian into a cultus that provided a sense of permanence and continuity. Such proclivities would be especially strong among those who both accept a missionary calling and represent a cultural enclave surrounded by nonmembers. Unity among the members was of primary importance to maintaining religious and cultural identity. Thus, the Holmans' behavior threatened to upset the delicate balance upon which the members of the mission family based their existence.

Samuel Ruggles reflected the general feeling of the missionary company two months after their arrival in Hawai'i: "What would the missionary of the cross do if it were not for the consolation contained in the promises of God. These to him are sweet and refreshing. Without *them* before him, he would soon faint and die; but with them he may go to his work and cheerfully labor and toil." Samuel and Nancy Ruggles, *Journal*, 8 May 1820, JC, HMCSL. The missionaries had to keep their focus on their purpose and ignore their environment in order to succeed. The Holmans made it difficult to overcome the reality of the mission family's situation on isolated islands among strange people.

23. Bingham and Thurston to Worcester, 15 Feb. 1821, pp. 111-112. L. Holman to Samuel Ruggles, 14 Nov. 1820, ML, HMCSL.

24. Ka'ahumanu and Liholiho were co-rulers, she with the title of *kuhina nui*. With the support of the five Kona chiefs and her genealogical connections, Ka'ahumanu was able to exercise almost complete political authority, easily dominating the less aggressive king. At this time she and her primary supporters were residing at Lahaina. Since the king had granted the missionaries permission to stay, they were concerned that if they seemed to ally themselves with those who were possibly his political opponents, the king would deport the mission family. For a description of the political situation, see Kuykendall, *Hawaiian Kingdom*, vol. 1.

25. Bingham and Thurston, "History of the Defection," pp. 46-48. Bingham and Thurston to Worcester, 15 Feb. 1821, p. 76.

26. Bingham and Thurston to Worcester, 15 Feb. 1821, pp. 68-69. T. Holman to Prudential Committee, 14 May 1822.

27. H. Bingham to S. Worcester, 11 Oct. 1820, ML, HMCSL. Bingham and Thurston to Worcester, 15 Feb. 1821, p. 70. Bingham and Thurston, "History of the Defection," p. 67.

28. Bingham and Thurston to Worcester, 15 Feb. 1821, pp. 61-62, 73, 84. L. Holman to Ruggles, 1 Aug. 1820. Lucia Holman further insisted, "You may be assured that my *dear* husband was not unfaithful to me on this subject when I *entreated* him to come [to Lahaina]." Bingham to Worcester, 11 Oct. 1820.

29. Bingham and Thurston, "History of the Defection," p. 54. T. Holman to Prudential Committee, 21 Nov. 1820.
30. L. Holman to Ruggles, 1 Aug. 1820.
31. Bingham to Evarts, 2 Nov. 1820. D. Chamberlain to S. Ruggles, 14 Nov. 1820, ML, HMCSL. T. Holman to S. Worcester, 21 Nov. 1820, ML, HMCSL.
32. Woods, the Abbott Professor of Christian Theology at Andover, clearly presented the public's expectations of missionaries in his 1812 charge. "The fervent, devoted Christian presents himself a living sacrifice unto God; and counts it a privilege to do and to suffer any thing for the advancement of his cause. . . . The sacrifice of property and pleasure; stripes, imprisonment, and death lose their terrors, and become more attractive than any earthly good. . . . This is the principle which governs and animates the church of Christ." Leonard Woods, "A Sermon Deliver[e]d at the Tabernacle in Salem, February 6, 1812 . . . ," in *Pioneers in Mission*, ed. R. Pierce Beaver (Grand Rapids, Mich.: William B. Eerdmans Publishing, 1966), 257-258.
33. Initial contact between the Hawaiians and the New England missionaries was traumatic for the evangelists. Nakedness and noise seemed to be the biggest shock. Sybil Bingham wrote her sisters, "You cannot tell how the sight of these poor degraded creatures, both literally and spiritually naked, would affect you." Nancy Ruggles exclaimed that although she had been aware of the plight of the heathen, "half of their real wretchedness was never told me." Mercy Whitney commented on the "incessant noise. . . . We hear continual yelling and screaming of natives all day as they swim or run around." And Lucia Holman complained, "I have got so tired with the noise and sight of these naked creatures that I could almost wish myself as far from them as you are." S. Bingham, Journal, 31 Mar. 1820, JC, HMCSL. Samuel and Nancy Ruggles, Journal, 1 Apr. 1820, JC, HMCSL. M. Whitney, Journal, 30 Apr. 1820, JC, HMCSL. L. Holman, Journal, 3 Apr. 1820, JC, HMCSL. Noise, nakedness, and the reality of another culture made the missionaries cling *more* closely to each other and to their purpose. See Wieman and Wescott-Wieman, *Normative Psychology; Wagner-Wright, Structure of the Missionary Call*.
34. Bingham and Thurston to Worcester, 15 Feb. 1821, pp. 102, 125-132. Bingham and Thurston, "History of the Defection," pp. 68, 100, 111. H. Bingham to J. Evarts, 31 Jan. 1821, ML, HMCSL.
35. Bingham and Thurston to Worcester, 15 Feb. 1821, pp. 113-114.
36. E. and M. Loomis, Journal, 16 Jan. 1821. Samuel Whitney, Journal, 13 Jan. 1820, JC, HMCSL. H. Bingham to Rev. William Jackson, February 1821, ML, HMCSL.
37. Bingham and Thurston, "History of the Defection," p. 22. Unsigned to the Prudential Committee, n.d., ML, HMCSL.
38. J. Evarts for the Prudential Committee, Minutes re Holman, 7 June 1821, ML, HMCSL.
39. E. and M. Loomis, Journal, 14 Aug. 1821.
40. Ibid., 2 Oct. 1821. Bingham, "Journal of Mr. Bingham's Tour," 1 Oct. 1821.
41. E. and M. Loomis, Journal, 2 Oct. 1821.
42. J. Evarts to Sandwich Islands Mission, 5 Jan. 1822, ABCFM-HEA, HMCSL.

43. T. Holman to Committee for the ABCFM, 12 Sept. 1823, ABCFM-H, HMCSL.
44. H. Bingham, L. Chamberlain, and E. Loomis to J. Evarts, 9 Mar. 1824, ML, HMCSL. J. Evarts to Sandwich Island Mission, 11 Oct. 1823, ABCFM-HEA, HMCSL.
45. Samuel Worcester had issued the instructions that applied to the pioneer company in 1812. Those pertaining to the standards for baptism that brought admission to the church were clear. "You will allow sufficient time for trial, and for the reality of conversion to be attested by its fruits." The Reverend David L. Perry reinforced this direction in his 1819 charge to those departing with the pioneer company to the Sandwich Islands. "Be not hasty in forming your opinion of the spiritual attainments of the heathen; and do not suddenly receive them into communion with the church. One apostate may do more injury than hundred who are without." Samuel Worcester, "Instructions Given by the Prudential Committee . . . February 7, 1812," in *First Ten Annual Meetings of the American Board of Commissioners for Foreign Missions* (Boston: Crocker and Brewster, 1834), 41. David L. Perry, "The Charge," in Humphrey, *The Promised Land*, 34-35.
46. For further information, see Kuykendall, *Hawaiian Kingdom*, 1:122.
47. Richards, arriving with the second company in 1823, was strict in the admission standards at his station at Lahaina. In 1828 he wrote the Prudential Committee that by New England membership standards, the church would have 150 members, but he took pride in the fact that none of his church's fifty members had ever been disciplined. Asa Thurston, a member of the pioneer company stationed at Kailua-Kona, insisted that none should be baptized until he or she had been well instructed in grace and the fruits of their repentance were well established. William Richards to Rufus Anderson, 20 May 1828, ML, HMCSL. Asa Thurston to unknown, 10 Dec. 1828, ABCFM-H, HMCSL.
48. Membership figure from Bradley, *American Frontier*, 145.

**REVISING THE REVISIONISTS:
THE HISTORIOGRAPHY OF IMMIGRANT MELANESIANS
IN AUSTRALIA**

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The Queensland Labor Trade

Between 1863 and 1904 in Queensland, Australia, 62,000 first-indenture agreements were entered into by Pacific Islanders, mainly from what are now Vanuatu, New Caledonia (the Loyalty Islands), the Solomon Islands, and Papua New Guinea; and by a few Micronesians from Kiribati, and Polynesians from Tuvalu and outliers in Melanesia. In 1901 the new Commonwealth government of Australia banned further recruiting and attempted to deport all immigrant Melanesians. A royal commission further considered the matter in 1906 and allowed various categories of exemption. By 1908 there were around 2,500 immigrant Melanesians left; today they have approximately 15,000 descendants in Australia.

Since 1908 there have been more than 240 books, chapters, articles, documents, and contemporary accounts published about the Queensland labor trade and descendants of the Kanakas.¹ Academic writing on the Queensland labor trade dates back to B. H. Molesworth's 1917 master's thesis from the University of Queensland. Earlier historians viewed the labor trade as an aspect of British imperial history and concentrated on the legislation and regulations that controlled the sugar industry and the labor trade, seldom doubting that recruiting was a euphemism for kidnapping. They knew little about the working or private lives of the Melanesians who provided the labor to establish Queensland's sugar industry.

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The Revisionists

A change in interpretation is evident in the 1960s and 1970s, when Canberra-based historians such as Dorothy Shineberg (1967), Deryck Scarr (1967; Giles 1968), and Peter Corris (1973) argued that the mobilization of labor in Melanesia last century was characterized not so much by violence and illegality as by a substantial degree of active cooperation by Islanders, who had considerable control over their lives and labor in the sandalwood trade and on sugar and coconut plantations. More recent academic studies of the Queensland labor trade have modified and extended, though not changed, the essence of this conclusion. However, there never was a coherent revisionist school of thought relating to the Queensland labor trade. Nonacademic writers such as Holthouse (1969, 1988) continued to produce popular histories, oblivious of any revision in thought, and the media largely ignored the revision. Academic historians have continued to approach the Queensland labor trade from different perspectives: geographically, as part of the history of both Australia and the Pacific; and ideologically, taking on J. W. Davidson's brief to write island-centered rather than empire-centered history, or in terms of race relations in colonial Queensland, or from a Marxist perspective. Research techniques have also varied considerably, from sampling of information available, to detailed examination of total runs of archival and documentary sources, to quantification and large-scale use of oral testimony.

Kay Saunders has produced the most thorough documentary study of the Queensland labor trade and made a significant theoretical contribution through her analysis of indenture and race relations. But her study grew out of an interest in colonial Queensland and the plantation as an institution. In seeming contradiction to Scarr and Corris, she argued for greater recognition of the exploitation and oppression of the laborers. Adrian Graves's argument is similar, but he also challenged most previous analyses of the Pacific labor market, which he saw as based on neoclassical economic assumptions. Charles Price produced statistical tables on Melanesian immigration. Ralph Shlomowitz provided detailed analysis of wages, work categories, and mortality. Patricia Mercer, Mark Finnane, and I have added to this quantification. Tom Dutton and Peter Mühlhäusler have researched Kanaka pidgin English. And Mercer and I, with two South Sea Islander authors, Faith Bandler and Noel Fatnowna, have used oral tradition to provide an Islander perspective that was missing from previous writing.

Much recent research has been a natural progression from early

studies, which sampled evidence. The more comprehensive studies inevitably found some shortcomings in the pioneering works but also explored new areas. But the changes go farther than this. The historiography of the Queensland labor trade sits uniquely midway between that of the Pacific and Australia. Study has progressed from an initial focus on the actual labor trade (1863-1908) to longer-term investigation of the descendants of those involved, in Australia and, to a lesser extent, in the islands. This article begins by surveying the twist and turns in recent writing on the Queensland labor trade, argues the case for a wider methodology, places the labor trade within the historiography of indigenous and non-European Australia, and ends with some suggestions on directions for future research.

Economics and Class Analysis

Adrian Graves attributes neoclassical economic assumptions to the revisionists, who (he says) do not provide a satisfactory explanation for the operation of the labor market. He is correct in doubting the earlier emphasis on individual motivations and in claiming that there has not been enough serious consideration of changes that may have occurred in the laborers' own societies and economies (Graves 1984: 113-115). But his reason for doubting the emphasis on individual motivations relates to capitalist exploitation, not, as I have argued, the communal obligations of the recruits. He exaggerates the extent of capitalist intrusion and proletarianization of the Melanesian subsistence economy during the nineteenth century, and its increasing dependence on the sale of labor power to secure the subsistence of its members (Shlomowitz 1985c, 1987, 1989:590-591; Scarr 1984; Moore 1987). Over the last 130 years the traditional Pacific mode of production has been restructured toward a capitalist norm and accompanying class formation (Naidu and Leckie 1990), but the major changes occurred this century, not in the last. The arguments supporting capitalist intrusion are often based more on theory than on hard evidence.

Graves sees the Melanesians as the exploited labor component in the Pacific capitalist mode of production, but his revisionism fails to appreciate their incorporation of capitalism within their own cultural schemes. The importance of exchange varied considerably in precapitalist societies, and labor was not considered a commodity to be bought and sold. Both neoclassical and Marxist economics fail to grapple with the dilemma caused when capitalist labor and commodities are indigenized in other cultural logics. From the point of view of the indige-

nous people, their exploitation by the world system may well be a welcome enrichment of their local system.

The argument over proletarianization and the development of class has recently been extended by Mark Finnane and me. Hard evidence is provided in our statistical analysis of Islanders within the Queensland criminal justice system in the 1890s. We argue that proletarianization of the long-staying members of the Islander community in Queensland had begun by the 1890s but was truncated by the mass deportation of 1906-1908. Melanesians' changing experiences with the criminal justice system in the 1890s confirms their place in colonial society as transitional (Finnane and Moore 1990). More extensive quantification of the data available in government and mission records in Queensland will allow the debate to continue, but it seems likely that the data from the islands are too fragmentary to go beyond conjecture.

Revising the Revisionists

Recent refinement of the scholarship of the 1960s and 1970s goes well beyond disputes over motivations and the extent of capitalist development to the cultural accommodation and health of the immigrants. For instance, Corris's and Scarr's work on the labor trade is highly regarded, but they lack understanding of the various subcategories and wages rates among the indentured laborers, now provided by Ralph Shlomowitz (1981a, 1982a, 1985a). Corris totally underestimated the degree of cultural retention by immigrant Melanesians in Queensland, particularly in relation to magic and sorcery (Corris 1970b:63; Corris 1973:96-97; see also Mercer and Moore 1976), which has repercussions for our understanding of the mechanism by which individuals coped with the traumatic relocation from their small-scale societies (Moore 1985:68-69, 263-273). Saunders and Graves erred in their assessment of the Islanders' diet and the relationship between diet and health. Saunders concluded that the official dietary scale was not nutritionally balanced and not sufficient to sustain men and youths engaged in hard manual labor (1974:293; 1982:82-86). She uses this argument to explain the high mortality rate, but in doing so overlooks the fact that the majority survived. Graves (1979:96) supports Saunders's assessment. Although the diet was different from that to which the Melanesians were accustomed in the islands, even the official diet was adequate. Furthermore, analysis of the official dietary scale is fairly pointless, firstly because it is doubtful if it was ever more than a rough guide to what was provided by employers, and secondly because the Islanders

supplemented employer-provided food supplies with produce from their own gardens and they foraged and hunted in the bush (Moore 1985:218-235). Starving Melanesians into submission when they were surrounded by the bountiful bush of tropical Australia would have been nigh impossible. The earlier historians have misread the documentary evidence and been unfamiliar with Islander oral testimony.

The high mortality rates among immigrant laborers related to their lack of immunity to diseases, not to lack of food or to substantial physical mistreatment (Shlomowitz 1987, 1989). Further, many of the early studies were of the labor trade itself, within limited time boundaries (usually 1863-1908), rather than analyses of the communities in the islands that the laborers came from and returned to or of the substantial Islander settlements that remained in Queensland. These refinements have substantially extended the scholarship of the 1960s and 1970s.

Accounts by Descendants of the Kanakas

The revisionist historiography has been augmented by another source, one difficult to argue against but nevertheless flawed. There are now several accounts written by the descendants of the Melanesian laborers, notably Faith Bandler and Noel Fatnowna, a large collection of taped oral testimony, and continuing media interest in the "Forgotten People," the descendants of the original Kanaka laborers who remained in Australia after the mass deportation of 1906-1908. The Islanders' view was first presented on television on the Australian Broadcasting Commission's (ABC) most popular series for 1975 (Peach 1976), and then on "The Big Country" in 1976. In 1978 Matthew Peacock prepared three one-hour ABC radio programs, consisting of oral histories of the Islander community in Australia. These were edited and published the following year (Moore 1979). The next substantial television programs on the Islanders were screened by SBS (Special Broadcasting Service) in 1989 and the ABC in 1992. Faith Bandler has been the most consistent spokesperson for the Islanders, in the print media and on numerous radio and television programs over many years.

In 1977 Bandler published *Wacvie*, the story of her father, Peter Wacvie Mussington, from Ambrim island in the New Hebrides (Vanuatu), written in semifictional form. *Wacvie* and later books by Bandler (1984; Bandler and Fox 1980) are significant as a Melanesian perception of the Kanaka days, but unfortunately they perpetuate the legend of the Islanders as a kidnapped and unhappy people forced to work for callous white masters. No mention is made of the complex intransigent society

that developed in Australia: the traditional feasts and dances, reciprocal relationships, sorcery, leadership, and kinship patterns that helped the Islander community maintain a distinct identity in an alien environment. Noel Fatnowna's *Fragments of a Lost Heritage* manages to capture the Melanesians' spirit, strength, and survival strategies (1989). It comes much closer to agreeing with the views of most Pacific historians and has been well received by scholars (Quanchi 1989).² Fatnowna tells the story of his family on Malaita and in Australia. The book deals with the twentieth century as much as the nineteenth, strengthening my conclusion that the worst injustices to the Melanesians were perpetrated not during the years of the labor trade but after 1906. At that time they and their children were pushed out of the sugar industry, which (because of the occupational restrictions placed on them in the 1880s and 1890s) had been their only means of gaining a livelihood.

Australian Islander families are also beginning to research their own family histories, to hold centennial gatherings, and to publish booklets. Carol Gistitin's short history of St. John's Anglican church at Rockhampton (1989), a predominantly South Sea Islander congregation, provides the first information on the Islander community in that district. The Togo, Corowa, Dodds, and Moss families have published reminiscences of the lives led by Islanders in northern New South Wales (Belleair et al. 1990). At Mackay the Mooney and Andrews families held reunions in the late 1980s, and the Eggmolesse, Bikwai, and Bowda families of Nambour are researching their history. At Rockhampton, Mabel Edmund has completed a short biography and is negotiating publication. Other Islander families are taking similar interest in their heritage. *The Forgotten People*, Bandler's and Fatnowna's books, and recent Islander family histories provide biographical details on individuals, families, and communities that are missing from histories written from conventional documentary sources. Where they are at odds with the academic historians is over their interpretation of the recruiting process.

The general public and Australian South Sea Islanders steadfastly believe that the original Melanesian immigrants were kidnapped, a view reinforced by the writings of Bandler. This is contradicted by the historical revision of the 1960s and 1970s, and by the view from the islands, where oral testimony confirms that many willingly participated in the labor trade. My introduction to *The Forgotten People* (Moore 1979:5-8) and other publications (Moore 1978-1979, 1981, 1985:337-343) provided explanations of how the kidnapping myths arose and the effect they have had on the outlook of Australia's immigrant Melanesians. Similar arguments appeared in reviews by P. M. Mercer (1980)

and Bob Reece (1978-1979) of *The Forgotten People*. Individual instances of kidnapping may have occurred as long as the labor trade lasted, but we know that the majority came willingly, many more than once. There is also the fact that most of the Islanders allowed to stay in Australia after 1906 had already been living there for more than twenty years, taking their recruitment back to a period when the labor trade was less regulated: some of them are likely to have been kidnapped. The transfer system that operated in varying degrees throughout the trade also gave the appearance of slave auctions down by the docks when ships arrived; this still holds its place in the contemporary memory of the labor trade. But the main arguments relate to the influence of the media and education, cosmology and psychology. The media still constantly sensationalize the labor trade, taking little notice of academic views expressed over the last twenty years. The education system, from primary and secondary schools to Christian Sunday schools, still does not present the revised view of largely voluntary participation by the laborers. Feedback from the media and formal education on a wide variety of topics connected to the Islanders is evident in the oral testimony collected from the Islanders in the 1970s and 1980s (Moore 1985: 338-340) and in community attitudes that bolster the Melanesian view.

No matter how willing they may have been to participate, for some the experience of leaving their small-scale societies was extremely traumatic. They feared pollution, diseases, strangers, malevolent spirits, strange foods, the alien nature of the land and its people, and the regimentation. Some seem never to have fully comprehended the migration they undertook. The explanations they gave to their children many years later seem to contain evidence of this incomprehension. Finally, perhaps we are dealing with a different reality and with a different concept of truth: the objective truth confronted with an emotional or psychological truth. It is possible that their alienation from mainstream Australian society for more than a century has produced a historical myth: an alienation that needs the balm of kidnapping? As Patricia Mercer suggests, the view that their forebears did not come willingly is an essential component of contemporary Melanesians' attitudes to white Australia: "so psychologically imperative is it, that 'blackbirding', if it did not exist, would have to be created" (1980: 126).

Quantification

A most valuable advance in the study of the immigrant Melanesian community came in the second half of the 1970s when runs of statistics were subjected to quantitative analysis. Charles Price and Elizabeth

Baker's useful research note (1976) contains a set of tables that divide the estimated 62,475 Melanesian labor contracts into island groups and individual islands over the years 1863 to 1904. Before these lists were available, the best accessible statistical source was an appendix to Parnaby (1964), which drew figures from the published annual Queensland immigration reports. Corris based his estimates on the same deficient sources. Price and Baker were able to compute better figures by using a number of official registers held in the Queensland State Archives to supplement official published sources. Their research note has become a standard reference point for later work on the Queensland labor trade, enabling researchers to know the island origins of the Melanesian labor force in Queensland at any particular time. This is important in estimating the degree of previous contact the laborers may have had with foreigners and the composition of the various categories of laborers in Queensland.

Ralph Shlomowitz, already skilled as an analyst of the economic history of American slavery, the plantations, and postbellum labor systems, applied his experience to the economics of the Pacific labor trade and the Queensland and Fiji sugar industries. He has now completed a detailed analysis of the Queensland indentured and time-expired labor market, the interrelationship of different categories of labor, and market arbitrage. Shlomowitz's work ranges from analysis of the profitability and viability of indentured labor categories, particularly the time-expired market (1981a, 1982a, 1985a, 1985b) and the search for institutional equilibrium in the sugar industry (1979a, 1982b), to statistical investigation of the recruiting voyages (1981b), the development of the Butty Gang as a system of organizing labor (1979b), and health and mortality on the plantations and farms (1987, 1989). These Queensland studies have now been supplemented by similar comparative studies on labor in Fiji, Papua New Guinea, Vanuatu, the Solomon Islands, and Ocean Island and Nauru.

There can be no doubt of the benefits that have accrued from the tabulation and analysis of demographic and economic data from the labor trade. The retrieval of data on individuals who made no individual impact upon the world gives us access to another historical dimension and facilitates developments in social history. Shlomowitz's findings on the extent and importance of time-expired and ticket-holding laborers in Queensland have stimulated our understanding of social and class developments in the colony. Nevertheless, undue reliance on statistics can be misleading. To use health and mortality as an example, in Melanesia the spiritual and physical worlds intermingle, and this

Melanesian world was transported to Australia. Sickness and death had supernatural causes and needed to be avenged if ancestors were to be appeased. Murders, either through sorcery or by physical violence, had to be avenged to maintain social equilibrium (Moore 1985:244-273). But how can this be revealed in any statistical analysis of mortality and health statistics? Just as care is needed in dealing with traditions and myths, statistics also should be treated with care.

Oral Tradition and Ethnohistory

Systematic collection of Kanaka oral testimony began with Robert Tan in 1960 and Tom Dutton's interviews in Ayr in 1964 (1980). Peter Corris interviewed some of the original Kanaka laborers and their descendants in the Solomon Islands and Queensland (1970a), and Edward Docker contacted descendants in northern New South Wales and Queensland (1970). An Islander-centered view of the labor trade was beginning to emerge, but what was missing in the research was large-scale involvement of the descendants.

The advance on Corris's valuable start came through an innovative approach to the collection of oral tradition from indigenous and immigrant peoples in North Queensland, sponsored by the History Department at James Cook University in Townsville. Under the auspices of a black oral history project, Patricia Mercer and I began in earnest to accumulate a bank of Islander oral testimony. Over several years, beginning in 1974, we spent our spare time traveling in coastal North Queensland gathering oral testimony, concentrating on the oldest of the Islanders and on what they could remember of the plantation days. We were already too late to meet any of the original Kanakas.

The Australian-born descendants of the Kanakas whom we interviewed, often children of the original immigrants, were aged up to ninety-nine, with many in their seventies. The majority lived along a thousand kilometers of coastal Queensland between Maryborough and Ingham; most around Mackay, at Bowen, at the twin towns of Ayr and Home Hill in the Burdekin delta, and at Ingham. Over eight years, eighty-seven tapes were recorded, varying in duration from two hours to a few minutes, most being an hour or so of structured conversations based on the lives led by Pacific Islanders in Queensland. The tapes form only a fragment of our experiences with Australian Pacific Islanders, but they are the kernel of these experiences and are designed to represent the full range of conversations in which we participated.³

While Mercer concentrated her research efforts in North Queensland,

my research extended to the Solomon Islands to study the island origins of leading Queensland families. Because of the relationship I had established with the Mackay Malaitan families, I was able to participate in the relinking of the Fatnowna (1989) family with their descent group, the Rakwane of Fataleka language district, east Malaita. I spent five months on Malaita in 1976 and 1978, mainly inland from Fakanakafo bay in east Fataleka; returning briefly in 1981, 1984, and 1987; and have visited Malaitans in Honiara on three other occasions. I went in search of oral testimony on the labor trade, but soon became just as immersed in the myth-history of Malaita, was adopted into a leading Fataleka family, and gained an appreciation that the labor trade years were but a small part of a history that began when their ancestors first reached Malaita.

First-indenture agreements were issued to 9,187 Malaitans in Queensland. This was 14.7 percent of the total Melanesian migration and 51.7 percent of all Solomon Islanders recruited to Queensland. In comparison with the next most important islands (Epi provided 5,084 laborers, Tanna 4,241, Guadalcanal 4,188, and Ambrim 3,400), Malaita was of outstanding importance (Price with Baker 1976). I was able to compile short biographical notes on 132 labor recruits and generally to follow Keesing's (1974) injunction to historians to stay longer and to learn more of the cast of characters and the local political system and culture. In excess of 9,000 first-indenture contracts were entered into by Malaitans arriving in Queensland. Unfortunately we do not know how many of the laborers were actually reenlisting, nor is there a complete list of their names or of the exact bays and passages from which they were recruited. By using several documentary sources from the government archives, company records, and diaries, 2,815 (30.64 percent) of their names were located, plus details of the ship's voyages on which they left for Queensland, and often the name of the passage or bay at which they boarded the recruiting ships. For 2,023 (22 percent) of them, enough details remain to indicate the dialect group or coastal area from which they came: this provided the first detailed picture of recruiting patterns from any one island (Moore 1985:81-100, particularly 83, 87).

The detailed Malaitan study confirmed many of Corris's conclusions but differed in showing that inland people were closely related to the coastal people and that they were recruited much earlier than had been previously supposed. Individual recruits also emerged as characters in history, their lives described and traced from Malaita to Queensland.

Back in Australia, access was gained to Anglican, Presbyterian, and

Catholic records of Islander baptisms, marriages, and deaths, as well as funeral parlor and municipal records. These sources were checked with the most knowledgeable informants, who were able to sort out some anomalies and provide supplementary information. Useful though this was, the data were still not in a readily accessible form. A solution was found in using the university's mainframe computer to reorder the records into chronological sequences of names, places of residence, and islands of origin. The initial 1,210 register entries from multiple sources, covering the years 1878 to 1959, when resorted (allowing complex variations in names used and in the spelling of names)⁴ produced 4,938 entries. The computerized data, partly based on the interpretations by informants of the original lists of names, and used in conjunction with other oral testimony and documentary sources, particularly files on crop liens and mortgages, have produced reliable biographical information on the recruits who remained behind in the twentieth century and on their families.

Mercer (1981) used a combination of oral testimony with colonial, state, Commonwealth, local government, parish, and company records to complete her history of the survival of the Pacific Islander communities in North Queensland from 1900 to 1940. She found abundant materials contained in the records of cemeteries, hospitals, churches, sugar mills, and other organizations, which, combined with oral testimony and more conventional documentary sources, enabled her to reconstruct family histories and general histories of the various Islander communities in North Queensland. From historical fragments Mercer has been able to produce an admirable study of the survival of immigrant Melanesian families in the unfriendly environment of a White Australia and a white-dominated sugar industry in racist North Queensland. As a result of the approach used by Mercer and me, exploiting a wider range of sources than ever before, and, crucially, involving the descendants of the Kanakas in the islands and Australia, many of the previous spatial and time barriers have been removed.

Australian Historiography and the Kanakas

The "decolonization" of Pacific history was under way from the 1940s and 1950s, yet in Australia in 1968 the Boyer Lecturer W. E. H. Stanner could still speak of "The Great Australia Silence" on Aboriginal matters (1969: 18-29; see also Howe 1988; Reece 1979; Reynolds 1984). While Pacific historians were writing island- and Islander-centered history, historians of Australia were still standing firmly on the European

side of the frontier. Corris's doctoral thesis on Solomon Islands labor migration to Queensland and Fiji, at the time seen as pathbreaking in its methodology and findings, was submitted in 1970 and achieved wide circulation after its publication in 1973. Through Corris, and to some extent through Saunders, the historiography of the Queensland labor trade in first half of the 1970s had more in common with Pacific historiography than with that of Australia.

In Australian history early in the 1970s there was still an emphasis on the destructive impact of European capitalism and racism, typified by Saunders's major studies (1974, 1982) and Evans, Saunders, and Cronin's study focused on race relations between Europeans, Aborigines, Melanesians, and Chinese in colonial Queensland (1975). A few years later the writings of Henry Reynolds and Noel Loos (Reynolds and Loos 1976; Loos 1982), and particularly Reynolds's *The Other Side of the Frontier* (1981), were lauded as original and pathbreaking in the emphasis they gave to Aboriginal resistance. But as Kerry Howe pointed out in his review of *The Other Side of the Frontier*: "Historians of culture contact in Africa, the Americas, New Zealand and the Pacific Islands have been looking at 'the other side' for twenty to thirty years now, and many of the issues Reynolds examines are *deja vu* in any context other than Australian history" (1983:82; see also Howe 1988:602).

Reynolds and Loos's emphasis on resistance served a useful purpose in quantifying conflict and showing that Aborigines were not passive victims, but their enthusiasm to prove the point led them to neglect Aboriginal accommodation, which was just as important to Aboriginal survival. The evidence is there in their writings, but they have not stressed it, leading Bob Reece to suggest that perhaps their arguments, largely formulated in the 1970s, are sometimes purposefully (Reynolds 1981:1), but often unwittingly, related to the needs of black radicalism at that time (Reece 1987:117). The same is true of Saunders's thesis (1974), and Evans, Saunders, and Cronin's (1975) and Ryan's (1981) books. In their preface to the 1988 edition of *Race Relations in Queensland*, Evans and Saunders provide an excellent description of the political milieu in which they researched and wrote, and discuss their motivations.

By the 1980s the "victims" and "resistance" approaches were less evident, replaced by a new paradigm for Aboriginal-Islander-European interactions on the frontier, typified by the later writings of Reynolds and by Ann McGrath (1987, 1989) and Marie Fels (Attwood 1990). Regional studies modified the general pictures and more rounded histories emerged, using notions of accommodation, action, and agency to show the complexity of alliances that emerged. Reinterpretation of

Aborigines on the frontier and of Chinese (May 1984) and Melanesian immigrants as active agents in retaining a goodly measure of control over their own lives has brought Australian history into line with that of the Pacific.

The methodology and the style of writing on the Queensland section of the Pacific labor trade and Australia's immigrant Melanesians, as well as studies of indigenous Australians, both Aboriginal and Torres Strait Islander, now fit equally into the historiographic traditions of Australia and the Pacific. But the bulk of the literature on the Queensland labor trade always has had an Australian, not a Pacific, orientation.⁵

Future Directions for Research

Historians often see their main task as to organize and establish a rhythm in history, to identify the pivotal moments, static phases, sudden accelerations, and periods. Certainly the biggest failing of research and writing on the Queensland labor trade and the Australian immigrant Melanesian community is that it has been too bound by British academic traditions, by European time boundaries (particularly the 1906-1908 deportation period), and by colonial and national boundaries between Melanesia and Australia. Most of the writing relates to the labor trade and terminates at 1906-1908, as if the world of the immigrant Melanesians stopped dead at the end of 1908. And most of the writing deals only with the actual recruiting process in the islands and the Islanders as immigrants within Australia, ignoring their retention of Melanesian cultural values and the circular nature of the migration. Little detail is known of the effect on island communities when the laborers returned home.⁶

Although the history of the British Pacific has theoretically been "decolonized" since the 1960s, in fact colonial periodization is usually still observed and the historical methods are still of a British school.⁷ Islander-centered history falls short of the total social history approach of the French *Annales* school begun by Lucien Febvre and Marc Bloch, which emphasizes the long perspective and integral relationships among all facets of human existence. Jacqueline Leckie noted that much of the writing passed off as social history in the Pacific is not total history, merely narrative empiricism from an Islander perspective (1983:55).

David Routledge followed Leckie's earlier advocacy in examining what he termed a potentially negative fragmentation in the study of Pacific history:

First, Pacific historians who wish to maintain a unity and coherence in their speciality, must study the past of entire societies, not merely multicultural situations that formed only a part of the actions of these societies. Secondly, they must study process, and not merely sequences of events. And thirdly, they must emphasize social categories rather than individuals, even if such a category can only be defined through an accumulation of detail about individuals. (1985:90)

English-language Pacific historians have too often maintained arbitrary starting and finishing points that relate to the British imperial history they claim to have left behind, and too rarely ventured beyond a timid empiricism. Colonial episodes such as the Queensland labor trade are still unthinkingly bound to firm dates, almost as if the people involved came out of thin air in 1863 and went back into the same ethereal realm in 1908. We need to include as much as possible of the pre-1863 period to provide the context from which the laborers came, and to go beyond 1908 to the 1990s. Immigrant Melanesians have lived in Australia for more than eighty years since the labor trade ended, double the time the labor trade operated. The 1863-1908 years can be reliably assessed only within a much wider perspective.⁸ Despite being ethnographically informed and innovative in many ways, the writings of Scarr, Corris, Saunders, Graves, and Shlomowitz do not seek to achieve a "total history" approach, mounted as they are within a colonial time frame and bereft of Melanesian cosmological context.

A "total history" approach could use industrial archaeological investigations on plantation mill or barracks sites, or Melanesian settlement areas, of the type that made vivid plantation life in Jamaica (Craton 1978), other areas of the Caribbean, and the American South. Although Melanesian sensitivities could be offended (for fear of disturbing ancestral spirits) if archaeological digs were carried out at the sites of Islander encampments, there are intact plantation mill sites, such as Richmond and Nindaroo at Mackay, that could reveal a great deal about the dynamics of the plantation regime of more than a century ago.⁹ There has been one successful attempt at marine archaeology, a Queensland Museum investigation of the wreck of the *Foam*, a recruiting schooner that foundered on the Great Barrier Reef in 1893 (*Daily Mercury* [Mackay], 18 Nov. 1982; *Townsville Daily Bulletin*, 1 Dec. 1982; *Australasian Post*, 27 Jan. 1983), but given the lack of access to similar wrecks more work of this type is unlikely.

The collection of oral history in the Islander community from Mackay northward during the 1970s was immensely successful in preserving testimony that has added a totally new perspective to our understanding of immigrant Melanesian life in Australia, not only during the years of the labor trade, but perhaps more crucially in capturing the tribulations of the community during this century, allowing a total picture to emerge. No substantial additions have been made to the collection since the early 1980s. Urgently needed is an extension of the original project, beginning at Rockhampton and moving southward into northern New South Wales. Very few of the children of the original Kanaka laborers are still alive. But it is possible to extend our knowledge of the southern Queensland and northern New South Wales Melanesian community during this century. Indications are that the findings would be rather different, as the southern communities are better integrated into local society than those of northern Queensland. The other area where oral testimony needs to be collected is in Torres Strait, the home of a substantial number of Pacific Islanders, many of whom are descendants of the same indentured laborers.

It is feasible to write a history of all or sections of the southern immigrant Melanesian community, which would complement the earlier work done by Mercer and me in the north. It would also be possible to work on the Vanuatu-Australia links, through families such as the Corowas from Tanna or the Lammon and Henaway families from Epi and Tongoa, to provide a Vanuatu comparison with the Solomon Islands-Mackay link already researched through the Fatnowna-Bobongie families from Lau lagoon and Fataleka in Malaita.

At the end of the 1970s Howe suggested six directions by which to escape what he aptly called "monograph myopia": writing the histories of specific islands and island groups and concise histories of the Pacific, adopting thematic approaches, viewing the South Pacific from the perspective of the Pacific rim, and engaging in more comparative studies and interdisciplinary investigations (1979:87-89). The last decade has seen most of these avenues explored, and much of Howe's criticism from a decade ago is no longer valid. There are now three new general histories of the Pacific (Howe 1984; Campbell 1990; Scarr 1990), making redundant earlier efforts (Oliver 1951; Barclay 1978). Several important histories of island groups have been written collectively, through the University of the South Pacific's Institute of Pacific Studies,¹⁰ and independently, such as Judith Bennett's history of the Solomon Islands (1987). Thematic approaches have now been taken to many issues,

including labor, migration, gender, domestic violence, leadership, resistance to colonial regimes, and Islanders' experiences in the Second World War.

But two general criticisms still stand, in relation to theory and comparative studies. Howe (1979:89), Leckie (1983:52-57), and Routledge (1985:95) all acknowledge the general reluctance of Pacific historians to theorize. Howe suggests more interdisciplinary investigation using theory developed by the social sciences. Leckie and Routledge advocate the adoption of the methods of French social history rather than just select borrowings from anthropology and sociology. The interpretation of culture systems and cultural change in the Pacific needs to be given emphasis, rather than empirical narratives, no matter how much they may purport to be Islander-centered.

Australia's immigrant Melanesian community is the largest group of Melanesians living outside of the islands. They now range from the children of the original recruits to sixth-generation Australians. The quantification of information concerning their lives, the oral testimony collected, and the depth of regional knowledge now available allows fairly intense scrutiny of their history as individuals, as families, and as communities. A distinct minority group within Australian society, they have maintained strong links with their original culture. They are not just a historical curiosity left over from a plantation era. They have evolved a pan-Melanesian society, an amalgam of elements from dozens of island societies, yet are now firmly Australian. Wedged between indigenous and other immigrant Australians, and linked to the Pacific Islands, the Australian South Sea Island community is suited for any study of the dialectical process of change. Equally, knowledge of their history allows historians to theorize about cultural change and class analysis in the Pacific. The methodology used in piecing together their past should serve as an example for ethnohistorians elsewhere.

We need to pursue more vigorously a comparative approach, within the Pacific but particularly internationally. As far back as 1963 Geoffrey Bolton noted that plantation life in Queensland was "unlike the social pattern elsewhere in Australia" and in "the tradition of planter paternalism found in other British colonies" (1963:87, 89). Adrian Graves noted that "the hierarchical management of the Queensland estate with its tiered status and ethnic-cum-class structures" was "not unlike wage labour plantations in other parts of the colonial world" (1986:253). Ross Johnston compared British jurisdictional policy in Africa with that of the Pacific (1973), which included bringing the labor trade under legal and administrative supervision. Saunders's doc-

toral thesis (1974) compared Queensland with the American South, and she described the Queensland sugar plantations as a “classical sugar plantation system” locking Queensland “inextricably and securely into the patterns in former slave societies of Mauritius and the Caribbean” (1982:40). Shlomowitz has used a comparative approach, relating the Queensland sugar industry and its Melanesian labor component to other Pacific colonies and world cotton and sugar economies (1984). Graves and Richardson (1980) have compared sugar production in Natal with that in Queensland, and Saunders and Graves have published chapters on Queensland within the context of Pacific and international labor migration, convict labor, and indentured labor worldwide (Graves 1984, 1986; Saunders 1982, 1984). And recently Munro has attempted to define and characterize “protectors” of plantation laborers (1989), taking examples from Samoa, Fiji, Hawaii, and Tahiti, as well as from Queensland.

The comparison is not straightforward. Queensland was one British colony among several in Australia. Sugar was only one element in the Queensland economy, alongside mining and pastoralism. The Queensland plantation era was brief, and land ownership in the colony was not as concentrated as in many plantation areas in the Americas. And by the 1880s the industry was being transformed by the introduction of central mills served by small farms (Shlomowitz 1979a). The Melanesian workers were not slaves (Moore 1985:153-155, 197-199; Buckley and Wheelwright 1988:251) and were divided into a series of categories of labor, some quite removed from indenture. Wage labor became more significant than indentured service. But further comparisons with rural immigrant workers in the Americas and Africa may well enhance our understanding of the Queensland sugar industry in the nineteenth century and of Pacific migration and cultural change, both back in the islands, where returning laborers were catalysts for change in village societies; and in Australia, where the immigrant Melanesian began as a separate category of indentured worker and slowly melted into the wider immigrant working class while maintaining closer links with indigenous Australians than any other immigrant group.

Conclusion

Although much of the revisionist writing on the Queensland labor trade from the 1960s and 1970s has stood the test of time, essential parts have been revised or at least refined by use of quantification, oral history, and the involvement of South Sea Islanders in writing their own history.

Part of the change is due to technique; sampling has given way to detailed analyses of statistics and communities. Part is due to more recent writings being centered on the people in the labor trade, not just on the labor trade itself. This article has argued that there never was a coherent revisionist school of thought relating to the Queensland labor trade and that the best avenue for future research is the history of the people in a total history context, without the strictures of dates, industries, and governments.

NOTES

1. Kanakas, Polynesians, and South Sea islanders were terms commonly used in the last century to describe Australia's Pacific islands immigrants. Today their Australian descendants prefer to be called South Sea Islanders.

2. Roger Keesing's editorial work and consultation with historians of the labor trade and the local district ensured the books veracity.

3. We also collected early Islander family photos, now stored with the tapes at the Department of History and Politics, James Cook University of North Queensland.

4. For example, the searches keyed in for the modern family name "Fatnowna" included all of the following variations: Fatnahoonia, Fatnhoona, Fatnahona, Fatnahonia, Oleania, Olerum, Orani, Orrani, Malta, Abelfai, Kawi, Kwailiu, Kwan, and Coquasha. Searches for islands of origin also included six or seven variations in names and spelling for some islands.

5. Perhaps the only point of disjunction remaining is that Aborigines, Melanesians (indigenous and immigrant), Chinese, and other Asian groups in Australia are usually treated as discrete entities, relating to Europeans but seldom to each other. I am indebted to Doug Munro for drawing my attention to this point.

6. A few studies give some emphasis to the effect of the returning laborers: Heath 1974; Bedford 1971, 1973a, 1973b; Bennett 1974, 1979, 1987; and Whiteman 1983.

7. I am not aware of any substantial writing on the Queensland labor trade in any language other than English. Gundert-Hock's study of Vanuatu (1986), written in German, includes material on the motivations of the recruits and their effect on their home societies. Panoff's excellent but neglected interpretative article in the French language discusses the Bismarck Archipelago in the years directly after the cessation of recruiting for Queensland (1979). And Firth and Munro (1990) have published some of the results of their work on the German labor trade to Samoa in the French language.

8. Ron Adams's study (1984) of a century of European contact with Tanna island is an excellent example of placing the Queensland labor trade within a wider indigenous context.

9. There is also the possibility of investigating old village sites in the islands.

10. Though the quality of the histories produced by the Institute of Pacific Studies is highly variable.

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EDITOR'S FORUM

**THE POLITICS OF RACE AND ETHNICITY:
THEORETICAL PERSPECTIVES ON PAPUA NEW GUINEA**

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In the current crisis on Bougainville, arguments about racial and ethnic difference are used to justify secession from the rest of Papua New Guinea. Conflicts are often expressed in racial terms: between blacks and whites and between "black-skinned" Bougainvilleans and "red-skinned" Papua New Guineans. Connor's phrase "ethnonationalism" refers to the doctrine that distinct ethnic groups are--by that token--entitled to independent statehood (1973). Premdas used this concept to analyze the first round of Bougainville secession in the 1970s (1977). Francis Ona, Bougainville's rebel leader, has argued in cultural terms: "our diverse customs will not allow us to live peacefully together as Papua New Guineans" (*Times of Papua New Guinea*, 7-13 Sept. 1989).

But more seems to be involved than simple assertions of difference. Why have these differences led to conflict? And conflict over what? And why is Bougainville secessionist but other "different" provinces of Papua New Guinea not? Is secession--as Australia's foreign minister, Senator Gareth Evans, has suggested--a disease that will spread unless it is stamped out (*Hobart Mercury*, 30 Jan. 1990)? Or does it thrive only in particular political, economic, and historical circumstances, so that other provinces may be less susceptible?

Bougainville was Papua New Guinea's richest rural province. The rebellion began around the site of the copper mine, which has provided some Bougainvilleans with a high standard of living and drawn thou-

sands of non-Bougainvilleans to the island looking for work. The mine has been the site of classic forms of class conflict between organized workers and management, who argue about wages and conditions of work. Early newspaper reports of the crisis in late 1988 linked protests by landowners to parallel, class-based actions by industrial workers protesting housing conditions. So, what was the link, if any, between the economic opportunities and conflicts opened up by the development of the mine and between the development of ethnic and racial conflict and secessionism? And how exactly would independence resolve Bougainvillean grievances about mining?

In dictionary terms, "ethnicity" originally meant simply "pertaining to race." But "race" is now usually treated as a subcategory of ethnicity. Enloe has pointed out that analysts of Third World politics have been uncomfortable with ethnicity, believing it to be a transitory identification (to be replaced by "nation" or "class") or a concept tarnished through manipulation by colonial governments. However, she says, "Ethnic categorisations have served political elites well precisely when they have struck some vital nerve in a given collectivity" (Enloe 1978: 338). Discomfort with ethnicity is rarely shared by Melanesian intellectuals (e.g., Narokobi 1983a, 1983b); and throughout the South Pacific region the politics of identity and cultural renaissance have often had a liberating, "anti-hegemonic" quality that is hard to square with the view of ethnicity as a simple product of colonial manipulation (Devalle 1989; Chapman and Dupon 1989).

If ethnicity is a sometimes discomforting term, then race is even more so. While scientific racism--such as in theories of eugenics--has been discredited, ideas about race are returning to social science from two directions. First, from behavioral genetics, which has apparently discovered nontrivial differences between racial groups, particularly in IQ scores, though the extent, causes, and significance of these results are strongly contested (see Hay 1985 for a textbook discussion). Second, from sociobiology, which has been less concerned with differences within the human species than with similarities between the "human race" and other animal species (Hay 1985:24-25). The implications of behavioral genetics are still not very clear, but Van den Berghe has constructed a theory of ethnicity derived from sociobiology, which sees ethnic identity as an extension of kinship, while downplaying racial differences as "biologically trivial" (1978). This theory is further discussed below.

Meanwhile, a popular view is that racial differences are not trivial.

This view is sometimes reflected in routine administrative practices, such as censuses, immigration controls, and laws affecting indigenous minorities.

In this article I shall partly disentangle "race" from the more inclusive term "ethnicity," and then look at relationships between them, and between these and class. Finally, I shall consider how the concept of the state, and its activities, influence and are influenced by these differences. Another dimension of difference--gender--needs to be combined in the future.

Race and Ethnicity in Papua New Guinea

Racial discrimination between "whites" or "Europeans" and "natives" was enforced in great detail in colonial Papua and New Guinea (Wolfers 1975; Inglis 1975). While resenting discrimination, Papua New Guineans did not necessarily reject the ideas about racial differences on which it was based. Some Papua New Guineans were suspicious of a late colonial shift in policy towards "multiracial" institutions, and the debates that took place in the House of Assembly before independence about the national constitution rehearsed complex arguments about race, nationality, citizenship, economic opportunity, and the relationship among them (Wolfers 1977). Wolfers concludes:

Almost every proposal for, or denial of, social, political and economic change had a racial aspect, including the devising of citizenship legislation which would alter the basis on which legal rights were allocated from race, loosely defined, to citizenship. . . . The citizenship debates were remarkable for the frankness with which race relations were discussed and the openness with which the racial attitudes of particular groups were expressed. (1977:382)

The debates were not simply about exclusion of the economically advantaged white settlers. They led to the extension of offers of citizenship to other Melanesians, from Irian Jaya, the Solomon Islands, and the Torres Strait Islands (who at that time posed no serious threat of economic competition).

However, since independence the economic advantages of non-Melanesians have continued to provoke resentment, although the categories have partly shifted from "race" to "citizenship." There have been

regular parliamentary outbursts against the alleged economic opportunism of "naturalised citizens" (PNG 1981). The "dual wage" system (whereby noncitizen contract workers get paid more for the same managerial and professional jobs than citizens) is regularly condemned as a form of apartheid. According to Dorney:

The black/white division on Bougainville was perhaps more noticeable than almost anywhere else in PNG because of the nature of the Bougainville mine. . . . In this insulated setting BCL was the largest single employer of expatriate labour in PNG, excepting the national government itself. In 1988 the expatriate workforce was 610 out of 3560--seventeen percent. (1990:127-128)

Racial discrimination in wages and conditions has been a persistent theme in the industrial disputes at Bougainville Copper Limited (BCL). Mamak and Bedford have documented the systematic inequalities between "black" and "white" earnings in the early 1970s (1977). In November 1988 mineworkers demonstrated against the racial discrimination in proposals to rehouse them up at the mine site.

Nash and Ogan have detailed the changing perceptions that the Nasioi people, who live around the mine site, have had of themselves and others (1990). The late Mrs. Sereo, chairperson of the Panguna landowners' association and Francis Ona's sister, criticized environmental damage caused by BCL and complained in a newspaper interview that "our own black-skin race is losing its real identity fast because of the intermarriage and sometimes unwanted mixed race children" (*Times of Papua New Guinea*, 23 Feb.-1 Mar. 1989). The improper influence of white people on the PNG government was part of Ona's case for secession. In a letter written in February 1989 he blamed a "white mafia network" for subverting the PNG government (*Times of Papua New Guinea*, 16-22 Feb. 1989, 4). There were also many reports of racism in the security forces on Bougainville. In a typical newspaper report a young man described being beaten up by police who said: "You think you black people are smart? We will make you feel it" (*Times of Papua New Guinea*, 19-25 Oct. 1989).

Apart from race, Papua New Guinea's famous variety of language groups, cultural forms, and Christian religious sects provides a range of potential ethnic markers (Premdas 1988). Clan forms of organization provide the myth of common descent that features in more historicist

conceptions of ethnicity, such as Smith's (discussed below). Given the numerous possible bases of ethnic organization, the interesting question perhaps becomes why social movements do *not* embrace ethnicity. Of "spontaneous local movements" characterized by May as "micronationalist," "few placed much emphasis on ethnicity" (1982:2).

The strategy of Papua New Guinea's constitution makers was to manage ethnicity. Questions of ethnicity, as distinct from race, frequently arise in discussions about local and provincial government boundaries. Papua New Guinea's nineteen provinces are based on colonial administrative districts that did not necessarily correspond to preexisting political, cultural, linguistic, or religious divisions.

Several provinces have introduced systems of "community government," typically on a smaller scale than the old local governments. In Morobe Province, "community government" has been defined and justified by the premier in ethnic terms.

A community government is made up of 1,500 to 3,000 people, who constitute an ethnic or cultural grouping of people speaking the same language. Within most ethnic-cultural groupings there may be a linguistic minority in a dominant ethnic group which is allowed direct representation in the community government. So for us in Morobe province ethnic-cultural grouping is taken as the basis of the state system. It is not ignored, it is not negated, it is taken as a basis because that is the reality: whatever problems you encounter will start from there. People don't just jump from heaven. They grow from their cultural units. (Samana 1988:42)

Papua New Guinea's Constitutional Planning Committee had specifically preferred "districts" as a basis for provincial government to larger "regions," on the grounds that the latter might become the focus for "ethnic sentiments" and hence "deep national division" (PNG 1974:10/3). Judged "too small" for broader "regional" purposes, provinces have also proved "too large" for others (Jackson 1979:21). Ethnicity in Papua New Guinea has turned out to be almost infinitely divisible: it would be interesting to see whether it has stabilized at the level of the language group (the social atom that Samana proposes) or whether subdivisions emerged among even these small units.

At the very smallest scale, "landownership" has become the typical basis for political mobilization against the state in Papua New Guinea.

“Landowners” are well understood as a kind of ethnic group, defined by myths of common descent, a shared history of perceived injustice (at the hands of foreign plantation owners, the colonial government, or mining companies), and common interests (in getting their land back or a better compensation deal).

Ethnicity

Theories of ethnicity differ in the extent to which it is regarded as something “given,” “constructed,” or “chosen.” The first emphasizes the “primordial” (in Geertz’s 1963 language); the second the social, malleable, political character of ethnicity; and the third the process of rational choice among the various potential identities available.

“Given”

Mason locates the question of primordialism in a general theoretical tendency to look for explanations in terms of origins (rather than, say, circumstances or consequences). He argues that the question of primordialism in race and ethnicity can be broken down into at least three sub-questions: (1) the extent to which overt physical differences, and/or culture, can have an independent effect, unmediated by meanings; (2) the extent to which nineteenth- and twentieth-century racism in Europe and its offshoots is different from what went before (does it need explaining in its own terms, or simply as an extension of primordial ethnocentrism?); and (3) the extent to which racial and ethnic differences are immutable and natural (Mason 1986:5).

My short answers to these three questions would be “no,” “both,” and “neither.” Theories of perception, particularly the way perception involves stereotyping and typification, might help solve the first question. But if some physical differences are unmediated by meaning, we then need to ask at what point does meaning start to intervene, particularly in relation to culture, which is itself a system of meanings. Geertz, after all, referred to “assumed givens” as he recognized that “culture is inevitably involved in such matters” (1963:109).

The second question is about the distinctive character of the modern world. Clearly, new and distinctive conditions for racial and ethnic conflict, particularly massive movements of population, have emerged through European colonialism and settlement, slavery, and the movements of indentured, “guest,” and migrant labor (see Pettman 1988).

Such shifts of population have been particularly recent and relatively, but not absolutely, large in the South Pacific, a feature emphasized by Howard in his account of ethnicity in the region (1989). But the distinctive character of the modern world has not been a simple one of transition from, say, status to class. New ethnic identities have been developed and old ones revived. Rapid economic growth has also created more things to have conflict about, and those succeeding have sometimes turned to theories of racial superiority to justify their advantages to themselves and to others.

The questions of "mutability" and "naturalness" are probably best kept separate. Physical characteristics may in fact turn out to be more mutable--through intermarriage and so on--than cultural characteristics, which may be all the more persistent because taken for granted. And what counts as "natural" (and whether it is, for that reason, valued or devalued) is also a cultural product,

"Constructed"

We shall call "ethnic groups" those human groups that entertain a subjective belief in their common descent because of similarities of physical type or of custom or both, or because of memories of colonization and migration; this belief must be important for the propagation of group formation; conversely it does not matter whether or not an objective blood relationship exists. Ethnic membership differs from the kinship group precisely by being a presumed identity. (Max Weber, quoted in Hechter 1976:1163)

Just as anthropologists have shown that notions of kinship and common ancestry do not necessarily have any biological basis (Sahlins 1977), so historians of the "invention of tradition" have shown that some cultural traditions are quite recently, and deliberately, established (Hobsbawm and Ranger 1983). However, just because some kinship is fictive and some traditions are invented does not mean most or all are. In both cases the words "manipulation," "extension," or "revision" are probably better than "invention." To paraphrase the famous Marxist tag, people make history, but they do so from material transmitted from the past.

So it may be most sensible, following Yinger (1986), to distinguish "thick" and "thin" forms of ethnicity: in the first, the "given" factors predominate; in the second, the "taken." Anthony Smith's work on

ethnicity and nationalism gives support to both positions. Smith is particularly strong on the role of common history--real, blending into the mythic--in the development of ethnic sentiment. Historians, just as much as linguists or genealogists, produce the material from which ethnic identity is constructed. A common historical experience--for example, of oppression, or migration--becomes for Smith a necessary, but not sufficient, plank for the construction of an ethnic group (1986). In Papua New Guinea we can already see a common history of oppression by BCL, with the P.N.G. security forces becoming an important reinforcement of Bougainvillean claims to ethnic separatism.

On the one hand, Smith's earlier work on "ethnic revivals" emphasized the construction of ethnicity and the role of nationalist intellectuals in researching and reconstructing folk tradition (1981). There are plenty of parallels in the modern South Pacific. A similar process of recovery and revaluation of folk traditions--oral history, traditional land tenure, languages--has been carried out by nationalist intellectuals in the region, aided by institutions such as the University of the South Pacific's Institute of Pacific Studies and the Institute of Papua New Guinea Studies. On Bougainville, Francis Ona's biography reads somewhat like those of the young, educated people identified in Smith's sociology of ethnic revivals, who, being blocked in their careers, return to their villages to rediscover their roots (1981: 116-133).

On the other hand, Smith's more recent work on the "ethnic origins of nations" emphasizes persistence, and describes how some, but not all, current nation-states can draw on ethnic identities that go back long before the development of capitalism (1986).

"Chosen"

A third tradition of thinking about ethnicity focuses on the individual, rather than the group, and (if applied to Papua New Guinea) to towns and plantations rather than the countryside. Borrowing from economics the assumption that people are generally rational, self-regarding maximizers, proponents of this tradition note that we are often presented with a range of ethnic identities from which to choose--and we may prefer not to choose any (Hechter 1986). Such rational-choice theories address questions about why individuals join ethnic groups and why some refuse, backslide, or identify with the "wrong" group. These are questions that theories of the givenness or social construction of identities are not good at resolving. We will return to this approach below.

Relationships between Race and Ethnicity

While "race" is often dissolved into broader theories of ethnicity, two approaches distinguish between racial and other markers in ways that seem relevant to understanding Papua New Guinea and the South Pacific.

Van den Berghe's sociobiological argument considers ethnic identity an extension of kinship: "look after your own," particularly your own genes, or those similar to yours (1978). But, perhaps surprisingly, biological notions of race form little part of the argument. First, he argues that "race," as socially defined, has no intrinsic biological significance: "Social race typically seizes on biologically trivial phenotypes, and, equally typically, corresponds only imperfectly with genetically isolated populations" (Van den Berghe 1978:406).

Second, he argues that, historically, cultural markers--language, dress, etc.--have been a much better test of genetic relatedness. Among settled populations, physical differences have been matters of gentle gradients and "physical criteria became salient only after large, strikingly different looking populations found themselves in sudden and sustained conflict" (Van den Berghe 1978:408)

Van den Berghe's argument thus accounts for the modernity of "racial" forms of ethnic identification and discrimination. We do not need to accept or reject the underpinning argument about gene selection to recognize the importance of migration and settlement bringing physically different-looking populations into sudden conflicts: labor migration, after all, is a key plank in Howard's Marxist account of ethnicity in the South Pacific (1989). And the double conflicts on Bougainville--between black and white, and between red and black--are parsimoniously explained in terms of conflicts over resources between immigrants and indigenous people, marked by visible but trivial physical differences.

Banton suggests a more sociological kind of relationship between race and ethnicity. He suggests that ethnicity usually refers to processes of inclusion ("us") whereas race refers to categories of exclusion ("them") (1983:106). Ethnicity thus points to processes of group formation and maintenance and to the possibility that potential members may reject the identity proposed for them or decide to drop out. These questions are addressed by theories of rational choice. Racial categories are less open to such individual strategic choices, though categories of people excluded on racial grounds may then turn around and form themselves into an ethnic group. Other excluded categories of people may do the

same; Epstein has analyzed the U.S. gay movement in ethnic terms (1987).

Class

We can distinguish weak and strong notions of class. In weaker versions, class is simply a classificatory scheme, based on income, wealth, or status. Stronger versions, particularly in Marxist approaches, tend to see classes as historical actors "out there," driving history. They emphasize historical changes, irreconcilable conflicts between classes, and the problem of class consciousness: members of a class "in itself" may not recognize their common interests and so not act as a class "for itself" (Przeworski 1977). The notion of ethnicity raises similar questions of consciousness and collective action. A racial group, by contrast, has less of a problem; it is already categorized and stigmatized by others. As with race, popular conceptions of class (e.g., of "the rich") may not correspond to social scientific definitions.

Class in Papua New Guinea

Marxist writers recognize the extreme difficulties of applying class analysis to Papua New Guinea. The industrial working class is tiny, while the bourgeoisie is in some ways absent overseas. In between are a number of awkward classes, such as a "big" and "small" peasantry or the "bureaucratic petty bourgeoisie." While Good believes the process of class formation to be "central" to understanding the Third World, he recognizes that examples of overt, self-conscious class action in Papua New Guinea are few and ephemeral (Amarshi, Good, and Mortimer 1979:100). Fitzpatrick writes of the "inchoate" and "emerging" character of classes in Papua New Guinea (1980), of the use of "exotic adjectives" to describe them, and ends up using Wright's (1976) idea of "contradictory class locations" to explain why people act inconsistently with the expectations of class analysis. More recent Marxist writing on Papua New Guinea has given greater attention to class action by different factions of the growing indigenous bourgeois class (Stewart 1985; MacWilliam 1986, 1988).

Given the difficulties of class analysis in Papua New Guinea, why should we bother? Without a methodological predisposition to Marxism, one reason might be the assumption, so familiar that it is hardly spoken, that Papua New Guinea is best understood as a country undergoing a particular kind of historical change: a "young" country, "devel-

oping," and so on (in spite of thousands of years of settlement). Marxism is among other things a theory of historical development and the forces that drive it. Class divisions and class action may become more pronounced as the nation's economy develops (though class analysis of advanced industrial societies is not particularly straightforward either; see Przeworski 1977 on the proliferation of new and intermediate classes).

If Papua New Guinea is "developing," then Bougainville should be of particular interest as among the most advanced provinces. Until the rebellion it had the highest per capita income of the nation's rural provinces (Bird 1984:22), and the mining work force was like a classic nineteenth-century proletariat: skilled, well organized, and concentrated in a single location. At the same time, commercial opportunities provided by the mine have created a small class of Bougainville bourgeois, looking for investment opportunities. So if class action was going to take place anywhere in the country, it would likely be in Bougainville.

If we take a broad definition of "class action" to mean action by groups defined, or defining themselves, in economic terms (particularly in relationship to the means of production), then we might look at several potential groups: the landowners, the mineworkers, and the rebels. Class action need not encompass action only by employees or the disadvantaged. The bourgeoisie has also to organize itself. In Papua New Guinea we need to take into account the existence of an indigenous as well as an international bourgeoisie. We also might look for intellectuals of uncertain class position who seek to heighten class consciousness or to organize class action, and at the "imperialist" role of the Australian government.

However, the initial actors in the Bougainville rebellion--traditional landowners in 1987-1988--do not fit easily into a class analysis based, like Marxism, on a labor theory of value. To the extent that they survive on rental and compensation payments, they are parasitic on the labor of the mineworkers (though in practice few landowners would be completely dependent on such income). As MacWilliam has argued (1988), to the extent that their income is concentrated and invested through such institutions as the RMTL (Road Mine Tailings Leases) Trust, which invested in plantations, they are, or hope to become, bourgeois.

Relationships between Race, Ethnicity, and Class

In looking for relationships, we first need to sharpen the focus on conflict. Ethnicity need not necessarily involve conflict. Ethnic groups may

be happy to "live and let live," their hostility reserved for backsliders among their own ranks rather than for other ethnic groups.

Modern "race" seems more intrinsically conflictual. It has often involved the involuntary categorization of one group by another (though members of the categorized group may be sufficiently intimidated, or brainwashed, into accepting the categorization, at least for a while). In the weak form of class, differences in income, wealth, or status are not necessarily conflictual (though may give rise to resentment if they seem unjustified). In the strong Marxist form, however, class differences are inherently conflictual: more wages means fewer profits and vice versa.

Treating race and ethnicity together, six possible relationships can be distinguished between them and class: (1) that, historically, racial/ethnic forms of organization are being replaced by class forms; (2) that race/ethnicity may conceal, but are fundamentally subordinate to, class; (3) that race/ethnicity may express class; (4) the reverse, that class (or at least economic claims) may disguise racial/ethnic claims; (5) that they simply overlap; and (6) that race/ethnicity provide the selective incentives required to overcome free-riding on collective action.

The first goes back to where we began: the persistence and revival of racial and ethnic forms of organization tends to disprove it. Weber distinguished between class and status groups, and expected that identification with status (such as ethnicity) would be more salient in times of economic stagnation and identification with class more salient in times of economic change.

When the bases of acquisition and distribution of goods are relatively stable, stratification by status is favoured. Every technological repercussion and economic transformation threatens stratification by status and pushes the class situation to the foreground. Epochs and countries in which the naked class situation is of predominant significance are regularly the periods of technical and economic transformations. And every slowing down of the change in economic stratification leads, in due course, to the growth of status structures, and makes for a resuscitation in the important role of social honour. (Weber, quoted in Hechter 1976:1166)

Hechter argues that his own research on industrialization and the Celtic fringe in British politics tends to disprove the Weberian hypothesis, though Cross (1978) has restated it in a more complex way, which

accounts for decolonization in the Caribbean. For Papua New Guinea the efflorescence of ethnic sentiments in such an economically changing society as Bougainville suggests that there is no simple historical transition from race/ethnicity to class.

The second is a kind of classic Marxist formulation. Fitzpatrick expressed it directly in relation to Papua New Guinea: "racial and ethnic divisions are seen in the present work as subordinate to class division. It is perverse to assert the dominance of race and ethnicity when the purpose of the maintenance of these divisions is to contain class formation" (1980:18). This formulation seems to me too closed. The reference to "purpose" is functionalist: the purposes are presumably those of the economically dominant class, but there remain questions about how they recognize each other and their common interests. Even then there may be disagreements about how those purposes might best be served (and they may get it wrong). The notion of subordination also suggests a traditional Marxist model of "base" determining "superstructure" that may not accept the possibility of complex and reciprocal determinations between them.

Rex's formulation of the third position opens up an opposite possibility, that ethnic identity provides a ready-made framework of feeling for class action: "the existence of common ethnicity of a primordial kind gives the class-in-itself (i.e., the group with the same relation to the means of production) an immediate basis for action" (1986:80). This, for example, would explain the demonstrations by BCL employees in November 1988, justifying a class claim (about housing) in terms of racial discrimination.

The fourth position is suggested by Smith (1981): that some groups pursuing ethnic political purposes may put their demands in an economic form to gain wider legitimacy for them. It seems to be a possibility not worth excluding by definition. A strong version of this (for example, among some Fijian nationalists) would discount claims to multiracialism or class action as covers for the interests of particular racial groups.

The fifth position is that the relationship between class and ethnic action is merely contingent: a matter of overlap, coincidence, or opportunity. They are different bases for organization but not necessarily linked through disguise, mystification, or subordination. However, such disaggregation leaves us with the problem of explaining what ethnic and racial groups might be fighting about: mere "difference" seems a little limp as an explanation. As Wolpe asks: "How is it possible to conceive of race as an 'independent basis' for the acquisition of political and

economic power without specifying the conditions (including the structures of political and economic power) which make it possible for race to operate in this way?" (1986:115).

The sixth position responds to the weakness of disaggregating the concepts by introducing ideas of competition and material interests. In the rational-choice model of ethnicity, discussed above, people choose from potential available identities those that will maximize their interests: if a group of them do it, then that presumably corresponds to class action. However, as Olson has pointed out, such a group will be subject to a collective action problem: it will be in each member's individual interests *not* to act, on the assumption that he or she may free-ride on the actions of others (Olson 1965; Elster 1986:129-139).

"Ethnicity" may provide the emotional ties and fear of shame--what Olson called the "selective incentives"--that overcome such opportunism. Hechter takes the argument further. As well as providing the private rewards and punishments that induce people to participate in larger-scale action, ethnic organizations also provide a means of controlling the information on which individual rational choices are made (for example, by overestimating the chances of success, by suppressing consideration of alternative courses of action, and by presenting opponents as more wicked and calculating than they really are). Ethnic organizations may also contribute to the formation of preferences that guide rational action (Hechter 1986:271).

Relationships with the State

Race and ethnicity are sometimes used to make special claims for state resources, just as governments may use race and ethnicity as a basis for granting or withholding jobs and services. Fitzpatrick sees "law and state" maintaining ethnic divisions to contain class formation (1980). The theorists of the "invention of tradition" also give an important role to the state in shaping ethnic identifications.

The concepts have different relationships to ideas about the state. Ethnicity, as Smith argues, is not necessarily a political concept (1986), though the modern ideology of "ethnonationalism," defined by Connor (1973, 1987), asserts that every ethnic group has the right to a state of its own. Race, and particularly racism, more clearly involve relations of power and subordination. They need not necessarily involve the state (bully boys can enforce racism), but the processes of categorization and exclusion that are characteristic of racism are well adapted to the apparatus of the modern administrative state.

The relationship between class and state has been the subject of a

flowering of recent Marxist analysis, turning particularly on the notion of the "relative autonomy" of the state and of state officials from the immediate demands of the economically dominant bourgeoisie. The relative autonomy of the state and--more generally--of political from economic relations provides one route to a nonreductionist Marxist theory of race and ethnic relations.

Rational-choice theories also have much to say about the role of the state, for example, as an arbitrator in a whole class of situations called "prisoners' dilemmas," whereby individually rational behavior is collectively disastrous. Racial and ethnic claims often lead to unintended, suboptimal outcomes: if one group makes an ethnic claim, it may do well; but if every other group does the same, they all end up worse off. Lebanon provides a model.

We can begin to see how state activity influences, and is influenced by, race and ethnicity through the familiar argument that state elites use ethnicity to divide and rule their populations. Three theories extend this argument in ways that seem relevant to Papua New Guinea. Each is a kind of rational-choice theory, or at least it assumes that state elites or (in the third case) aspirant elites use ethnic claims to promote their own interests.

Laitin's "Ancestral Cities"

In an argument about alternative bases of political cleavage, Laitin describes how the British in Nigeria deliberately revived and promoted political identification with ancestral cities, while actively discouraging the politicization of religious antagonisms between Christians and Muslims. These identities have persisted into independence. Laitin's surveys show that while Christians have much greater economic opportunities than Muslims, religion has not been politicized. People feel that "ancestral city identification is real, whereas religious identification is not" (Laitin 1985:299).

Arguing against primordial and rational-choice theories of ethnic identity, Laitin concludes that such identities are neither completely "given" nor completely "chosen," but powerfully shaped (though perhaps not completely invented) by state action. In relation to Papua New Guinea, Laitin's argument suggests attention to the bases of division recognized and promoted by government officials and--in particular-- the intersection of religious and other bases of identification. Catholicism, for example, is often cited as a basis for Bougainvillean identity and resistance to central government officials.

Enloe's "State-Building Formulas"

Enloe argues that ethnicity is not just a "concoction of manipulative elites" (1978), but can be understood as an instrument of state building; that is, a means for ensuring that the state is centralized, coordinated, and relatively autonomous of the society the state elite seeks to govern. By a process of international comparisons, she identifies several distinct "state-building formulas" that use ethnicity in various ways. The familiar tactic of "divide and rule" is only one such formula. Others are: displacement, as when marginal, typically indigenous groups are pushed into the interior or highlands; internal colonialism, in which members of the dominant ethnic group are posted to rule other groups, whose own elite is co-opted into junior positions in the state hierarchy; and assimilation, when elites allow access to the bureaucracy, but usually on condition that applicants adopt their ethnic values.

As in Howard's account of ethnicity and the state in the South Pacific (1989), Enloe treats "divide and rule" as a tactic of weak, but not necessarily colonial, states. It is one also followed after independence, though the state's inability to penetrate and mobilize the divided society means that it may be disproportionately dependent on foreign aid. Papua New Guinea seems a typical case.

Ballard's "Official Construction"

Ballard extrapolates from colonial policy in Nigeria to Papua New Guinea that colonial administration may serve to constitute ethnic identity as well as reflect it (1978). I would extend his argument about ethnicity and access to a simple, rational-choice model of how ethnicity is reproduced, as follows. The establishment of a district headquarters, later becoming a small town, tended to privilege the group on whose land the center was built. The perception of uneven development created feelings of resentment among those who now found themselves distant from the new centers of power. They then turn secessionist, seeking a separate administrative unit and in particular a central place of their own. The process may repeat itself on an ever smaller scale. A neat example is the following argument by a university student supporting a separatist movement in the eastern Highlands in 1983.

[T]he argument that there is less development in the eastern half of the province than the western, for which the provincial government has been partly blamed, is just one issue that has

fuelled separatist politics . . . there were obvious aspects of favoritism . . . in the provincial government . . . most senior and key ministries were given to Goroka leaders while the junior ministries went to Kainantu leaders . . . people want to run their own area with their own ideas, knowledge and customs. (Ayamaso 1983)

Ayamaso's complaint could be applied word for word to a number of provinces or countries with only a change of proper names. It describes a strategy for an aspirant elite, wanting a state of its own.

Conclusions

Several accounts--from Van den Berghe's sociobiology to Howard's Marxism--emphasize the need to periodize any theory of race and ethnicity. Groups may have always distinguished themselves by ethnic markers, but "race" particularly has become more salient in the modern context of intercontinental migration that has been both driven by and resulted in sharpened competition for land and jobs.

An emphasis on history is also supported from a different direction by Anthony Smith's arguments about the role of historical myths in the formation of ethnic groups. Thus Bougainvillean stories of oppression by security forces are likely to join other elements in shaping a sense of a separate Bougainvillean identity. As elsewhere (for example, Northern Ireland) the repression of ethnic expression becomes its most powerful reinforcement.

In Papua New Guinea a focus on the relationship between migrant and settled populations and competition for land and work helps explain why ethnic and racial conflict is more prevalent in some parts of the country (such as Bougainville, Morobe, or the National Capital District) than in others. If we follow a Popperian philosophy of science, a good theory should be falsifiable, and a theory based on migration and competition predicts that ethnic and racial conflict will be less elsewhere. Thus, in policy terms, secessionism may not be quite the virulent disease that Senator Evans, quoted at the beginning of this article, has suggested.

As well as looking to history, there is probably a need to develop a "regional" theory of the politics of race and ethnicity for the South Pacific. Just as theories of race relations developed in the United States are different from those accounting for the Caribbean or Latin America or the position of racial minorities in postwar Europe, so we need to

take into account the historical and geographical circumstances of the South Pacific, whose "regional" character is partly accounted for by similar experiences of incorporation of small political systems into wider colonial states, weak administrative penetration, slow economic growth, long-distance labor migration, and the persistence of pre-contact forms of organization and belief, which can be invoked and reconstructed to suit current political purposes.

While theories about the politics of race and ethnicity must take into account historical and geographical contexts, they must also make sense at the level of the individual and the group. We should not take the existence of racial or ethnic groups as given: they must be created, recognized, and sustained by collective action. So we need to ask why people join ethnic groups, and why they leave them, and why they sometimes reject "race" or "ethnicity" as a basis for organization, preferring something else.

What selective benefits and sanctions are needed to keep groups together? Again there is a historical dimension, as ethnic choices are presumably more easily available in mines, towns, and plantations than in the village in which you were born. However, collective action problems are perennial; they offer a different kind of explanation from those offered by history and anthropology. We should not assume a simply "corporate" character for traditional life, without conflict between individuals and conflict between individual and collective aspirations. To do so would be to concede to the mythology of race and ethnicity that we were all "one people" back then.

NOTE

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BOOK REVIEW FORUM

Nicholas Thomas, *Out of Time: History and Evolution in Anthropological Discourse*. Cambridge Studies in Social Anthropology, no. 67. Cambridge: Cambridge University Press, 1989. Pp. x, 149, index. US\$37.50/£22.50/A\$55.00 cloth.

Review: JONATHAN FRIEDMAN
UNIVERSITY OF LUND

This book is precisely that which is indicated by one meaning of the title, that is, "what it means for anthropological texts and comparative discussions to be out of time," in the sense of outside of time (p. 1). In the standard temporal sense, one may hope *Out of Time* has come in time. This is a timely book by a prolific author who has recently published a historical monograph on the Marquesas. It is a statement of a modernist and an objectivist historian of the Pacific, part of a concerted effort by a number of researchers to counter what they probably believe is a creeping culturalism and to establish another kind of historical anthropology of Oceania, both for the very long term stretching backward to prehistoric foundations and for the colonial and postcolonial periods of contact between indigenous societies and expanding Western hegemony.

The purported inspiration for this book is the desire to be able to "connect the sophistication of analysis with the actualities of political and economic crises, and in the mutual entanglement of observers and observed in colonial (or ostensibly 'post-colonial') inequalities" (p. 7). This is parallel to--and perhaps springs from--the critique of ethnography that has emerged in recent years, an endeavor to lay bare the degree to which anthropological texts are monologues dependent upon a predefined objectivity of the anthropological observer and his or her

ethnographic authority, which, in its turn, is based on the asymmetry of the colonial context within which ethnography developed. Now in its extreme form this argument is part of a more general postmodern disillusion with and dissolution of the very position of objective observer, of the *sujet supposé savoir* as the French would say, in a discourse where all knowledge is an immediate form of power. This is a hot issue nowadays, as the surging ethnification of the world is producing innumerable bodies of subaltern historical knowledge. But it is an issue not easily dismissed by entrenched academics defending their rights to speak objectively in contradistinction to everyone else. And this is surely a question of rights, all the more proof of the fact that objective knowledge is indeed founded on social authority. Thomas is sympathetic to this problem, but he is not terribly concerned to mess around with such issues. Instead, he proposes to analyze the ideological content of anthropological classifications, both functionalist and evolutionist, in terms of their social historical foundations. He is more explicit here than Clifford and others have been in his focus on the actual nature of historically situated academic discourse, and he seems to argue for a critical self-reflexivity reminiscent of Bourdieu. The latter, however, took a dangerous next step in assuming that "all objectivist knowledge encompasses a claim to legitimate domination" (Bourdieu 1980:49). Thomas's own objectivism is never in question here, and this might be seen as a lack of consistency in his exposition.

Thomas begins by arguing that somehow the fieldwork orientation of anthropology is itself to blame for the fixation on a society or culture extracted from historical context and objectified for the sake of relativistic comparison or even evolutionary ranking. One of his central assertions is that this ahistorical conception of social life generates an opposition between society as an internally coherent systemic field and a larger context that consists essentially of an unsystematized space of events, like a universal ether through which social bodies move. There is more than a sneaking suspicion that Thomas does not like fieldwork, although he certainly does not go so far as to argue for its elimination. He prefers to decenter it and allow the historical context its rightful place in our analyses. While it is true that the practice of fieldwork may tend to limit the field of analysis and to close it off to both larger spatial and temporal contexts, I would argue that fieldwork itself has been molded by the ideology of societal wholes as organisms, and by a similar ideology of culture as distinctive attributes of a given population. These are in turn variations on a superordinate cosmology of societal identity, implying a conflation of society, culture, and population. It is, I would

suggest, this cosmology that informs the similarities between British functionalism and American culturalism, as well as evolutionism. This problem, in any case, deserves more serious discussion in terms of historical context. This is all the more so since there is a systemic relation between the static nature of social typologies that informs functionalism and structural functionalism and the evolutionary schemes of the nineteenth century from which such typologies are by and large derived. Thomas is quite aware of this connection, but he appears to be more interested in accumulating debating points--which he does successfully. I am certainly in agreement with his general attitude toward the professional ethnographic mystique and his contention that the often hysterical attack on other sources (missionaries, travelers) of ethnographic data is and never has been founded upon anything other than the hocus-focus of supposed anthropological competence.

A major topic of *Out of Time* is evolutionary models as they have been applied to the supposed laboratory situation of the Pacific. Here again he offers strong arguments to the effect that the ranked social types of Sahlins, Goldman, and others are little more than abstracted, detemporalized social types aligned along an abstract scale of the degree of political hierarchy and driven by technological development or status competition. Such models have little to do with "real" history since they are based on rearranging the ethnographic--that is, "contemporary"--examples on a predefined scale. Thus, while structural functionalism and cultural anthropology openly practice the detemporalization of social reality, evolution--in spite of the illusion of change--is similarly grounded in the atemporal classifications while making use, at most, of an abstract and imaginary time scale. And the latter time scale is also part and parcel of nineteenth-century evolutionary ideology, the Victorian anthropology that arranged the extant and extinct societies-races of the world in a pseudo-order of progress toward civilization. An essential aspect of Thomas's argument consists in demonstrating the continuity between the classifications of the last century and the theoretical interests of the present.

Thomas does not simply dwell on the history of ideas; he provides examples of alternatives. In discussing Goldman's categorization of the Marquesas as "on the way" to stratification, he suggests from a broader historical foundation that the society was more likely "on the way" *from* stratification and that there were numerous transformational processes involved other than progressive development. I cannot but agree, seeing as I have argued for a similar kind of transformational process in general. But this leads me to Thomas's chapter 7.

Chapter 7 deals with the global systemic approach with which I have been associated. Thomas presents it, most flatteringly, as an alternative that has managed, for the most part, to free itself from atemporal evolutionary bias. But this is only for the most part since I, at least, am accused in the end of similarly using ethnographic types from the present in the construction of a historical model. The model that I proposed, essentially as a research program, suggested that an original prestige-goods system associated with the Lapita expansion--one whose systemically related features include asymmetrical marriage exchange (matrilateral), diarchy at all political levels, bilineality, and monopolies of varying degree over external exchange of goods necessary for the social reproduction of local groups (marriage, death, and all other essential payments)--stabilized to the point of being able to maintain historical continuity in western Polynesia. Eastern Polynesia, mostly settled after the terminal crisis of Lapita trade systems, never established such stable, long-distance trade in prestige goods because the extreme distances between island groups prevailed against the emergence of such trade. Rather, diarchy was violently collapsed into monarchy where the original two functions, fertility and politics-warfare, were absorbed in a single position, where the generalized exchange system linking ranks was reorganized in terms of a strategy of high-status endogamy, where production of prestige goods was replaced by increasing intensification of agricultural production for feasting and the support of a warrior aristocracy, where the sanctity of chiefs was increasingly sanctioned with violence in conditions where they had no evident monopoly of strategic goods, and where expansion was based on warfare and the redistribution of titles to lands (not property). I have referred to this as theocratic feudalism. In Melanesia, especially northern Melanesia, on the other hand, an increase in trade density led to the breakdown of exchange monopolies. This led to increasing competition among increasingly smaller political units in which production for feasting became increasingly dominant and resulted in cultural fragmentation and the emergence in the long run of big-man types of strategies.

The model was merely a sketch and Thomas rightfully criticizes some of its empirical shortcomings. He also offers certain more general criticisms. This kind of a model, he claims, while averting the assumptions of evolutionism, preserves the basic fallacy of using contemporary examples as building blocks. The charge would be true if the kinds of structures to which I refer were "societies," but such is not the case. Rather, my argument is a structural one in which the properties of dif-

ferent kinds of strategies and organizational tendencies are seen as transforms of one another. There is no abstract time dimension, nor a rank order of stratification. There is only the processual time of transformation itself, that is, a theoretical historical temporality, that depends on the properties of social reproduction and the conditions of reproduction of such structures. Prestige-goods systems can stabilize for long periods in the right conditions. They can also collapse and reappear in the right conditions. One might interpret certain archaeological material from Melanesia in terms of precisely such variation. Variation among the societies of the Kula Ring has been interestingly analyzed in terms of just such a historical model (Persson 1985). Thomas seems to conflate the structural model with empirically observable societies in a way strangely reminiscent of Radcliffe-Brown himself. Furthermore, to argue that all societies of the past must be qualitatively different than those we find in the present contains an assumption of continuous development in which all societies constantly transform themselves, yet another false evolutionism.

At the same time, a diametrically opposed argument is suggested as to the inadequacy of my schema. First, there is no reason why eastern Polynesia could not have developed regional systems of considerable scale. Second, there is some evidence that the western Polynesian regional system centered in Tonga emerged as late as the fifteenth century. Underlying this argument is an assumption that somehow conditions of operation or reproduction ought to be necessary and sufficient to account for particular social forms. It is true that it is said that the great geographical distances of eastern Polynesia prevented the establishment of trade systems, but I would add that the actual process of transformation is clearly more complex. I have always stressed that the existence of a particular structure can only be accounted for in terms of its morphogenesis. In such a framework, I would argue that the establishment of prestige-goods systems in western Polynesia was related to the extension of such systems into the area from further west, that is, the geographical expansion of an already existing trade system. This system may have experienced numerous crises, but the potential exchange network was always present so that variations in degree of hierarchy and control do not transform the basic properties of the system. This would account for the emergence and disappearance, and reemergence, of western Polynesian types of structures in Melanesia. In eastern Polynesia, on the other hand, it might be argued tentatively that settlement occurred largely on a different basis (with, perhaps, the exception of the Marquesas). It seems to have followed upon the collapse, crisis, or

decline of the earlier Lapita-based system, so that initial strategies combined with the absence of viable conditions for prestige-goods exchange might have channeled development in a novel direction. Further, it must be reiterated that these structures cannot be confused with societies or homogeneous populations defined by anthropologists. They are strategic properties of social processes of reproduction. As such, any population may make use of multiple strategies, even where they are in conflict with one another. Prestige-goods strategies may and have certainly emerged and reemerged in eastern Polynesia, even if they did not succeed in becoming dominant, that is, in colonizing all domains of social existence. Certainly, such goods were distributed by chiefs to their subordinate aristocrats (as in feudal Europe), and other evidence of such tendencies might be found in what exists of Marquesan dualism, in certain aspects of Tahitian social organization, and even quite late in precolonial Hawaii: Before his death, King Kamehameha withdrew to an increasingly sacred sphere, leaving the monopoly of external exchange and politics in the hands of his wife's family, that is, in the hands of his affines, thus tending to produce during a very brief period a dualism of functions replicated in an alliance relation.

With respect to my argument concerning the way in which different structures articulate with expanding Western hegemony, I suggested that if Europeans began to trade freely in the peripheries, eastern Polynesian strategies of expansion by warfare could lead, via monopoly of trade in weapons and military aid, to centralization and state formation, whereas prestige-goods systems that were previously centralized would break down due to loss of monopolistic control over trade. This is simply a statement of what I consider to be the logic of such relations. The degree to which they are manifested in the historical material was, I argued, exemplified by the divergent developments in Hawaii (and Tahiti) and Tonga in the initial period of contact. The model for the articulation of prestige-goods systems and European trade was developed on the basis of studies of the Kongo Kingdom (Ekholm 1972, 1977). The differences in some respects are great, especially the dimension of the Kongo Kingdom, well into the multimillion population class, as opposed to the Tongan "empire." But the rapidity of breakdown is just as strikingly rapid after trade becomes decentralized. It did not, as Thomas states, take "decades of systematic commercial exchange" (p. 98). I would add here, however, that it is very difficult to ascertain the way and the extent to which prestige goods functioned in Tonga, even if Thomas appears, very unlike his usual historian's style, to accept my assertion of the existence of such a system at face value. His own asser-

tion that weapons were used primarily as prestige goods in eastern Polynesia is true in one sense (pp. 98-99), but the history of Kamehameha's conquest of the Hawaiian Islands makes clear that monopoly of just such British imports was the key to centralization. To argue, contrary to my own approach, that "obvious military purposes were less important than prestigious display, and, most crucially, exchange value" (p. 98) is a misunderstanding of the social form of military strategy. No weapon is merely a weapon, of course. It is an expression of cosmic force and its mere possession or display may be enough to vanquish an enemy. Its exchange or prestige value resides in its *mana*, so to speak. But this does not detract from its place in a strategy of conquest, as is clearly evident in the historical texts.

My own discussion was limited to initial contact and I would argue that the emergent political structures of the nineteenth century are increasingly dependent on the intervention of Western forms of organization in the political life of a region successively integrated into the "world system." The fact that Tonga emerges as an autonomous "constitutional" monarchy just as the Hawaiian "constitutional" monarchy becomes totally dominated by a white settler class cannot be accounted for in terms of the initial articulations to which I refer.' When a local society becomes integrated into a larger system in such a way that the latter penetrates and disarticulates the former, then we cannot simply speak of a relation between or even confrontation between two structures. We have instead a new social field--for example, colonial society--that must be understood in its own terms.

Thus far, Thomas's argument would seem to run from the proposition that an original ahistorical--even antihistorical--bias in the foundation of anthropology via ethnography is carried over into evolutionary anthropology and to some residual extent into global systemic anthropology, even if the latter is based on an explicit critique of evolutionary thinking. Sahlins is last on Thomas's list as a former evolutionist who is today concerned with how culture produces history via a dialectic of structure and practice, sense and interest, sense and reference. Thomas argues, correctly I think, that while Sahlins's approach is well suited to confrontations--to the "encompassment of events by a cultural order" (p. 106)--"the process of transformation seems to have got lost hereabouts" (p. 107). It might be more accurate to say that the model does, theoretically, at least offer an explanation for changes in structures of meaning, of semantic categories, but not of social process in a more holistic sense. An elementary structure of kinship as Lévi-Strauss uses the concept refers to the exchange properties of kinship rather than to

the semantic, that is, culturally specific, categories that might be used to designate relevant kin by a given society. In Sahlins's discussion only the latter are relevant. On this point Thomas, in my view, does not go far enough in his critique. The problem with this kind of structuralist history is that it continuously eliminates essential social processes, those properties of the latter that do not belong and cannot be deduced from anyone's cultural code. The opposition between sense and reference and sense and interest is organized as an opposition between that which is structured and encompassing and that which lies, momentarily, beyond structure and encompassment, that is, the world of happenings. This might have easily been brought back to the earlier discussion of structural functionalism and its implicit opposition between society as a structured whole and history as a mere externality. Structuralist history is an extension of this premise in which there is now a dynamic interaction between the two terms of the opposition, but where the latter are preserved intact.

The chapter ends with a consideration of several factors that ought to play a role in the construction of a historical anthropology. Thomas stresses the role of agency, of active structural creativity, as opposed to models of acculturation that are passive or that assume historical process is only about categories in transformation. He also points out that not everything changes or disappears in processes such as colonialism and modernization, to be replaced by the culture of the conquerors. Notions of personhood and agency among dominated peoples may have "little to do with those of the dominant culture" (p. 113). The latter, of course, may inform in various ways local strategies of survival, of cultural production and interpretation, and of the formation of local movements. All of this is nice to know, but it is difficult to see how it is related to the general argument. One gets glimpses of an attempt to grasp the multiplex nature of a world that has too long been categorized as Other but which in reality has been very much a part of our own world. In this sense, there is a continuity from the attack on static notions of primitive coherence to the statement of the need to study colonial and postcolonial historical worlds in their total presence and coevalness.

The importance of Thomas's overall statement is both that it criticizes the way in which the categories of anthropological analysis have been abstracted by ideological lenses from the concrete context of historical documents and ethnographic realities and that it prods us to always and everywhere take this concrete context into consideration in our work. Many will experience Thomas's modestly polemical gesture as a serious

threat to the anthropological mystique. I myself would applaud it and would claim that it could have been even more polemical in the sense of systematically stating a position. This is absolutely necessary when one considers the enormous number of cultures of the so-and-so that are paraded as the result of a research method that consists of locating the pieces of other people's essences in disparate texts and statements strewn over hundreds of years, all legitimated by the criminally insane notion that such facts are "held together" by some cultural totality.²

If I sense an essential contradiction in the argument, it is best revealed in the summary statement at the end of the book.

A refocussed anthropological vision would often take a greater interest in archaeological evidence about longer-term social change. It would also deal much more extensively with historical events and their consequences: this evidence would assume the same sort of importance that observed ethnographic minutiae and informants' statements now carry. (P. 122)

Although very much in sympathy with such a plan, I find it loaded with problems that need systematic exploration. The archaeological record cannot be read, nor has it ever been read, like a series of events. Its interpretive categories have usually come from the static typological schemes produced by anthropology, schemes that necessarily deal with macroscopic changes that are about as far from evenemential history as one can get. A scenario for the actual emergence of a state, a neolithic transition is perhaps what we are waiting for, but its categories are bound to be saturated with the categories of the present unless we find a new way of doing things; and no new way has been suggested in *Out of Time*. At the other end of the scale the concrete historiographical method that Thomas suggests ought to be applied to ethnography would take the form of the history of what happened when the field-worker was there to record it. How to get to structure, to locate it in the flow of events at the local level, is not addressed. And the ultimate question remains open of how to arrive at the "systemic process" (p. 121) that ought to be the new object. But the question is a good one, and that is most important. One may hope that we're not all out of time.

NOTES

1. Here again, comparison with the Kongo Kingdom is incorrectly invoked. Thomas argues that both Tonga and Samoa maintained variants of traditional hierarchical rela-

tions until very late while the Kongo-- which is, apparently, the sole basis of my model-- "vanished into a mass of localized, egalitarian societies" (p. 99). This is quite mistaken. The Kongo region produced a large number of hierarchical societies throughout the several hundred years following the collapse of the first contacted kingdom and the final colonial onslaught. Even today, the area exhibits, as a dominant feature, transformed versions of such relations, i.e., just as found in Tonga and Samoa.

2. I recall having a conversation with a very well known specialist on central Africa who was convinced beyond all repair that the Kongo had a system of patrilineal cross-cousin marriage as a matter of essence, so that they could absolutely not have ever practiced matrilineal marriage in the past. But the list is long of those who have built models of "societies" based on the data of cannibalism, witchcraft epidemics, and egalitarian reciprocity that were the results of disturbances, *crises*, and catastrophes produced by colonial penetration (MacGaffey 1986; Sahlins 1985; Geertz 1980).

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Review: DAVID HANLON
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Time Is History?

E. H. Carr asked not so very long ago, "What is history?" It is a question that haunts my reading of Nicholas Thomas's essay on history and evolution in anthropological discourse. Thomas's primary concern is to

reveal the discipline of anthropology's conscious neglect of history and the theoretical errors and substantive misinterpretations that such a neglect has engendered. A consideration of such an important, worthy topic requires meticulous definitions of the key concepts involved; yet, such definitions remain elusive in Thomas's text--intentionally so. In the introduction, Thomas writes of his belief that meanings subsist in the uses of concepts in text; hence, he has refrained from any extended definitional consideration of terms such as "history" and "evolution." This posture leaves the reader to manage the intellectual void and confusion that result.

Throughout much of his text, Thomas seems to confuse history with time or temporality. At other times, there are references to history as "events and social processes" (p. 4), "systemic process" (p. 121), and "the orderly march of people and their thoughts and doings" (p. 118), but there are no attempts to explain these ideas. Thomas's statement that history itself possesses no unified theory of cultural or social systems only compounds the dilemma of meanings. Given the fact that most historians acknowledge the incredible diversity of topics involved in the study of the human experience, concede their efforts to be essentially interpretive, and admit to the plurality of contending interpretations for any given event, Thomas's references to "actual history" (p. 115) and "real history" (p. 121) become highly problematic, even disconcerting. Do these labels represent a regression to an earlier, archaic understanding that the historian's task lay simply in the development of a single, authoritative, and uncontested exposition of what really happened in the past through the professional examination of written documents? Thomas's general sensitivity to the issues of historical representation and cultural context suggest not, but there persists an ambiguity toward concepts and their definitions that permeates the text and confounds its reading.

The book's position on evolution is likewise perplexing. Thomas is clear enough about the atemporal bias in "old" evolutionary thought, but fails to be specific about a more historically sensitive, reconstructed evolutionary approach. What constitutes this "new" approach to evolutionary analysis other than its incorporation of change over time? Evolutionary ideas have often provided both a powerful political instrument and an intellectual justification for the colonization of non-Western peoples. Thomas's writing certainly evidences a consciousness of these facts, but his apparent endorsement of a still evolutionary, albeit radically altered, scheme for the discipline of anthropology necessitates a careful elaboration of the argument being made. There is little.

Problems involving the issues of theory and methodology follow from the author's failure to be more specific about his understanding of what history is. Thomas's disdain for "conventional, uncompromising empiricists" is unequivocal, as is the general inadequacy or ineffectiveness with which he regards Marxist, *Annales*, and regional systems approaches. Recent efforts by different schools of anthropological thought to address the past through symbolic or structural approaches are all held to be seriously flawed by pronounced professional biases, inappropriate concepts of time and change, or implicit evolutionism. Thomas, however, offers little indication in this work of how he would go about the practice of history or a more historically sensitive anthropology. There is repeated reference to the incorporation of archaeological evidence in a refocused anthropological vision, but this proposed agenda ignores the heavy evolutionary bias and lack of critical thought that underlie much of the archaeological endeavor.

What Thomas would have us draw from archaeology is something much closer to chronology than history. Relying heavily on archaeological research from Rapa Nui and the Marquesas, Thomas writes of the patterns in the eastern Polynesian past, patterns that diverge significantly from the still current preoccupation with chieftainship as the sole locus and measure of stratification and power in greater Polynesia. The end result is an evolutionary revision that depends heavily upon simple linear developments and that expresses itself in the tentative language of "probably," "likely," and "seems to have." A speculative chronology is thus substituted for an old, unilineal, evolutionary progression and with none of the "conditions of life and cultural variables across time and space" (p. 109) that Thomas later castigates structural historians for ignoring. The author's recasting of the patterns of the eastern Polynesian past is not history; it is instead something much closer to a redirected time line.

The problems in translating between "prehistoric" and more recent sociohistorical concerns involve more than differing time scales and a limited number of long-term processes. There are the critically divergent constructions and perceptions of time that separate the observer from the observed. The temporality or sense of time endorsed by Thomas is very much a culturally determined one. The text in question evidences no appreciation for the ways in which other societies might construe, express, and utilize a very different sense of time. Thomas, in effect, suggests that others' pasts can be discerned, charted, and understood through Western notions of change over and in time. Rather than destabilize the existing evolutionary paradigm, Thomas inadvertently

promotes it by limiting anthropological understanding to a very specific cultural understanding of chronology and sequence. The discipline of anthropology thus remains constrained within a Western construction of temporality, oblivious to or unconcerned with local conceptions of time.

Bound by its own professional agenda, *Out of Time* also gives little attention or credibility to indigenous sources and modes of historical expression. In challenging the late E. S. C. Handy's study of Marquesan society, Thomas attempts to discredit the memories and histories of his informants. Indeed, the only consideration given to indigenous conceptions of colonial contact is an oblique reference in footnote 7 of chapter 5 to Marquesans' designation of the "time of foreigners" as *te tai hao'e*. Such limited frames of analysis do not take anthropology very far away from the neocolonial contexts that Thomas says still influence its practice. Until historians and anthropologists alike understand that history (which I would define as the present's expressed consciousness and understanding of the past through a variety of forms) is culturally distinct in both its practice and articulation, efforts to decolonize the study of others' pasts will continue to founder.

There is a manner in which Thomas's book is itself somewhat "out of time." As the author notes, there is currently a surge of interest in the conjuncture between history and anthropology. Thomas cites a number of these works favorably, including Renato Rosaldo's *Ilongot Headhunting*, James Fox's *Harvest of the Palm*, and Jean-François Baré's *Le Malentendu Pacifique*. Marshall Sahlins has in recent years turned to the consideration of history and anthropology, though not to the author's liking because of his alleged failure to allow for an identification of longer-term structural transformations. The fact of this surge of interest between the two disciplines suggests to me that anthropology's aversion to history may not be as pronounced or as deep-seeded as Thomas argues. Rather than focus exclusively on anthropology's ahistorical (atemporal?) bias, Thomas might have undertaken an extensive examination of the ways in which historical anthropology or ethnographic history has sought to reconcile culture and event. Thomas, however, declines the task, claiming that the above-cited works and like others are "concerned much more with local substantive issues than with the question of compatibility or otherwise of particular forms of knowledge" (p. 7).

There are problems with emphasis, orientation, and evidence of argument on other issues as well. Thomas takes pains to delineate anthropologists' calculated disregard of voyager, missionary, and other

forms of early "amateur" ethnography; yet, Handy and Sahlins, two of Thomas's principal antagonists who have indeed made extensive use of such writings, are indicted for their uncritical or overly structured approach to these historical sources. Perhaps Thomas would have been better advised to formulate his general intellectual concern around "how" rather than "whether or not" early ethnographic sources are used. There are also larger claims in the text that do not get much beyond the realm of contention. Never fully developed or substantiated are Thomas's insistence that synchronic thought overwhelmed arguments for a more diachronic analysis and that complex conceptual and discursive reasons led to the *deliberate* exclusion of history from most anthropological practice. What is needed to sustain his overall critique is an intensive intellectual history of anthropological thought of the kind called for by Johannes Fabian and, ironically, acknowledged by Thomas himself.

In the end, the author's own words best describe his text. The reader is presented with a polemical collage that jumbles and tangles together epistemological critique, evolutionary theory, a revision of Polynesian anthropology, and thoughts on the practice of Pacific history. I do not mean, however, to be demeaning of Nicholas Thomas's efforts; I find some of his more recent journal articles, especially those on Fijian colonial history, to be quite scintillating. In writing *Out of Time*, he is to be thanked for challenging persisting paradigms of evolutionary thought, for reminding us--as have Clifford, Marcus, and others--of the contexts that promote and shape ethnographic investigation, for calling attention to professional biases that are at once limiting and exclusive of alternative ways of knowing and understanding, and for affirming that there is more to the study of the past than its historical representations. Given more time, a clearer idea if not definition of history, and a better sense of the politics and poetics involved in studying the pasts of others, Thomas might well have produced a more profound, convincing, and needed challenge to some of the reigning categories of Western anthropological inquiry.

Review: BRADD SHORE
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Out of Tune

In this small volume, Nicholas Thomas casts out after some big fish. In 122 pages, Thomas takes on Radcliffe-Brown, Malinowski, Marshall

Sahlins, Clifford Geertz, Jonathan Friedman, Kajsa Ekholm, Irving Goldman, and by implication a whole delinquent wing of the anthropological establishment that has collaborated in masking the historicity of those they presume to study.

In the current round of scholarly self-abuse that attends our discipline's late reflexive mood, Thomas takes up where Johannes Fabian left off (Fabian 1983). *Out of Time* aims to convince us that, in failing to historicize its subjects, ethnological discourse has perpetuated--wittingly and otherwise--the occidental myth of the historical Self and the timeless Other and thereby abetted the exploitation of oppressed peoples.

"History," Thomas claims, "has not been neglected simply through an oversight, but for complex conceptual and discursive reasons" (p. 1). What better way to lay bare such deeply-rooted error than to expose the faulty assumptions behind the writings of several major figures in anthropology who, at first blush, might seem to be among the most receptive to the historicizing of ethnology. Goldman, Sahlins, Friedman, and Ekholm have all, in different ways, attempted to reconcile anthropology and history. In each instance, though for somewhat disparate reasons, Thomas finds the approach seriously wanting. Pointing to "the absence of historical time, and . . . the explicit or implicit negation of the notion that history has any constitutive effect on the social situation under consideration" (p. 5), Thomas laments the misconstrual of the authentic historicity of human events even in such deceptively sympathetic hands. *Their* history, it appears, is not *his* history, not what he terms "real history" (p. 121).

Thomas positions the trajectory of "real" history somewhere between the Scylla of specific event and the Charybdis of general process. An account of what he terms "the actual process of history" (p. 50) must provide an analysis of change that manages to disclose global processes underlying particular events while somehow remaining "neither directed nor abstracted" (p. 4). Yet it is difficult to imagine how the depiction of historical processes can evade either direction (development, intention, cause-and-effect, evolution) or abstraction (generality) of some sort. This volume never actually discloses what sort of general processes Thomas has in mind (other than a vague and qualified commitment to world or regional systems theory) as the true foundation of historical knowledge.

Though quick to underscore the ideological nature of anthropological discourse with its "unstated rationales, hidden agendas, covert classifications, and simple muddles" (p. 12), Thomas appears to exempt his own work, and by extension the historical project, from the same sort of

critique. This is, at best, ironic, in light of the dependence of much of his argument on unexamined premises, disingenuous metaphors (Western "penetration" of the Pacific being the most egregious), and a presumption of the reader's agreement with the author on the basis of a self-evident correctness of the author's implied political and epistemological stance. For example, consider Thomas's strategic deployment of quotation marks to certify the credibility of *history* while discrediting *anthropology* as a kind of contestable knowledge: " 'Ethnology' may have been replaced by 'anthropology', but in each case the orientation of the discourse has concealed the actual process of history" (p. 50). The effect is to naturalize history as an unmarked category, cuing the reader through punctuation how to correctly view each of these disciplines--one real, others illusory. Imagine how different this passage would read if the quotation marks were removed, or if they were employed around the term "history" or, even more interesting, around "actual."

Thomas alludes to Radcliffe-Brown's famous antihistorical bias, which he suggests characterizes much anthropology. In Thomas's account no distinction seems possible between *antihistorical* and *non-historical* approaches in anthropological writing. To be antihistorical is to deny significant contingency to human affairs and to fail to understand that the present is always saturated with the past. To be nonhistorical is to focus the analytical lens on something other than this historical dimension of human life.

But while Thomas would join the chorus of voices in contemporary anthropology in privileging a historical perspective in the illumination of human affairs, I would contend that history is only one of a number of important ways of understanding the human condition. History in anthropology takes on its importance only in relation to specifically historical questions. But surely they are not the *only* questions anthropologists have the right to ask.

To suggest that human action is somehow more fundamentally "historical" than it is anything else would amount to a reductionism and a kind of essentialism with which Thomas would probably not be comfortable. Such a position would be incoherent, since a claim that human affairs are *essentially* historical constitutes a paradox.

Thomas is right that synchronic models always exact a cost in perception, by rendering certain kinds of variability, conflict, and history relatively inarticulate. But then historical accounts exact their own costs in explanation. At other times and places, social thinkers have had different intellectual priorities, priorities that themselves usually emerged not from political conspiracy so much as from a sense of the inadequacy of

antecedent theoretical questions. True enough, the anthropological object of knowledge has evolved in relation to particular social and intellectual contexts. But this is inevitable.

Out of Time makes its argument largely in relation to particular ethnological texts by attempting to expose the weaknesses of several influential treatments of Polynesian cultural history. I will devote the remainder of this review to a close reading of only one of these critiques --that of Goldman's *Ancient Polynesian Society* (1970). I have chosen to leave the evaluation of Thomas's critiques of Sahlins, Friedman, and Ekholm to others, and limit my remarks to Thomas's treatment of Goldman for several reasons. First, I know Goldman's book the best of any of the works critiqued in *Out of Time*. Second, since I have long held Goldman's book as a model of fastidious and insightful cultural comparison, I found the scale of Thomas's attack on this book thoroughly provocative. Finally, Thomas devotes the major part of his critique to a deconstruction of Goldman's work, and his most trenchant general criticisms of anthropology are raised in relation to *Ancient Polynesian Society*. A detailed evaluation of this critique is called for.

Ancient Polynesian Society is taken to be paradigmatic of the anthropologists's irresponsible use of time constructs. Thomas faults Goldman for his reliance on empirically and politically untenable notions of unilinear evolution in place of "actual" history. Goldman is accused of confusing geographic variation within Polynesia with a unilinear evolutionary sequence. This alleged theoretical distortion in Goldman's book is perpetrated through "an ahistorical use of ahistorical sources" (p. 15), especially works generated by what Thomas calls "museum anthropology" (notably Bishop Museum monographs). By employing the anthropological convention of an unchanging, precontact baseline culture understood in contrast to postcontact decline, *Ancient Polynesian Society* is accused of denying "real" historicity to Polynesians. Additionally, Goldman's work is held to manifest a disregard (common to many anthropologists) for nonprofessional ethnographic sources by South Seas missionaries and travelers in favor of "professional" ethnology.

These would all appear to be grave defects in Goldman's work. Unless, that is, one is familiar with the book. The book that Thomas claims to be reviewing bears only a faint resemblance to the one that Goldman actually wrote.

In relation to the alleged bias against nonanthropological ethnographic sources, I direct readers to Goldman's book, particularly his extensive bibliography. Not only has Goldman always championed the virtues of what he likes to call "naive ethnography," but such sources are

liberally used throughout *Ancient Polynesian Society*. Moreover, Goldman is always careful to comment on the limits of his sources, both professional and otherwise (see, for example, 1970:70, 73, 115, 117, 118, 204). Readers can decide for themselves, but it seems to me that Goldman is, in fact, far more careful than Thomas to alert readers to the limitations and biases of his historical sources.

What about Goldman's evolutionism? Though a student of Boas, Goldman does employ evolutionary language. Certainly his book would have benefited from an explicit discussion of how his use of the term "evolution" fits the several kinds of evolutionism in anthropology. Nonetheless, it is clear from Goldman's writing that he does not use evolution to mean inevitable, step-by-step "progress" on a single track. His complex and quite subtle position attempts to take account of *both* specific and general evolution, and he is clearly aware of the distinction between the two concepts. He is at pains to clarify his approach from the very outset:

At bottom the aim of evolutionary theory is precisely that of delineating the continuity of patterns of change in specific structures. The linear interest, the concern with stages and direction of evolution, is part of the general aim, but hardly primary. Direction and sequences of stages represent at best selected strands from the multiple foliation of variations. If the characteristics of a structure are to be defined from their variations, then of course all variations must be taken into account. (Goldman 1970:xv)

Yet Thomas inexplicably attributes to Goldman a view of evolution that is as simplistic in relation to evolutionary theory as it is inappropriate to Goldman's book: "'Paths' is perhaps the wrong word [to use in relation to Goldman's theory], since the exercise was directed not at a plurality of meandering routes, expressing the diverse purposes of people . . . but sought instead to define a necessary road which climbed from one condition to the next" (p. 59). Thomas seems to assume that Goldman is employing exclusively a notion of "general evolution" in his division of Polynesian polities into Traditional, Open, and Stratified types.

In a now-classic formulation, Sahlins reminds us that exclusive unilinear evolution is not even assumed by the nineteenth-century evolutionists like Spencer or Tylor with whom it is commonly linked:

It. . . seems grossly inaccurate, however frequently it is done, to characterize the perspective of the anthropological pioneers as “unilinear,” which is the idea that every culture goes through the same general stages. The locus of unilinear evolutionism is not in anthropology, but . . . in “crude Marxism” . . . and Bourgeois History . . . strange bedfellows. . . . [T]he nineteenth century anthropological evolutionists should all be acquitted of the unilinear charge, once and for all. (Service and Sahlins 1960:41)

Thomas appears to have done a highly selective reading of Goldman, culling “progressive” and teleological language to convey the impression that Goldman was employing an unsophisticated notion of evolution as directed development in fixed sequence, from lower to higher social forms. Not understanding the difference and failure to link general and specific evolutionary arguments leads Thomas to claim that Goldman confuses geographic variation with temporal sequencing.

To be sure, Goldman is not always as explicit and lucid as he might be about how he is using and relating these two sorts of argument. For Goldman, what lends directionality to the evolution of Polynesian societies is not a metaphysical *telos* that lies beyond the concrete lives of real people. Rather, directed social transformations in Polynesia are held to be generated *from within* by structural contradictions at the heart of Polynesian political institutions.

Polynesian conceptions of *mana* imply a sort of paradox of power. On the one hand, *mana* is usually associated with only certain descent lines, often those believed to be descended from gods. In this sense, the possession of *mana* was treated as an ascribed attribute *intrinsic* to chiefliness. On the other hand, *mana* can only be known by its effects in the world --by success in warfare, in food production, in fecundity, in physical perfection. This view of *mana* suggests that potency was mobile, even fickle, and was historically contingent.

Status for Polynesians thus involved structural ambiguity, combining considerations of what Goldman calls ascribed “rank” and achieved “power.” While notions of authentic rank provided for Polynesians a chiefly ideology and a charter for political continuity and legitimacy, struggles for power guaranteed that this charter would be frequently reformulated in relation to political contingency and violence.

Although Thomas accuses Goldman of using evolutionary theory to undercut the awareness of local historical developments in Polynesia,

Goldman's understanding of Polynesian political evolution is thoroughly imbued with historical consciousness:

[W]hile prerogatives of rank are, in principle, fixed in an aristocracy, they are, in actuality, subject to the vicissitudes of political life. *The political vicissitudes, it must be realized, are not somehow accidental and extraneous to the "system." They are part of the system; indeed, a central part. . . .* Status and prerogatives are always in active interplay; indeed this particular interplay is at the very heart of active variability in Polynesian status systems. (1970:17; emphasis added)

The variations in political systems that Goldman describes for Polynesia are not understood simply as random events. Nor are they fully determinate, innate properties of structures or systems. The variations he delineates are held to be the outcomes of the interaction between structural constraints of traditional cultural forms and a whole range of political and environmental contingencies. For Goldman, historical variability is not treated as an annoying thorn in the side of a pristine system but is understood as *an intrinsic aspect of the system itself*:

[The ideal of stability] may suit functionalist theory, which regards "equilibrium" as the summum bonum of social life, but hardly historical theory, which sees change, development, growth, conflict, opposition as the most characteristic social processes. Thus we may consider variability not as "instability" in the sense of inadequacy and structural defect, but as inherent flexibility. (1970:436)

In light of these and similar statements that occur throughout Goldman's book, and in light of the pains to which Goldman goes to document the endogenous histories and transformation through which Polynesian societies appear to have gone, it is remarkable that Thomas can still write of *Ancient Polynesian Society*: "The stress upon the ordering role of tradition (or specifically genealogical rank) tends to preclude serious consideration of the dynamics of systems which are never entirely encompassed by the apparently inflexible cultural orders" (p. 40).

The structural predisposition to political transformation that Goldman proposes for Polynesia is not accurately characterized as a steady

and automatic “unilineal path from more to less stratified” societies (p. 59.) It is a highly contingent (and thus highly historical) dialectic with an original (traditionally ranked) system generating its (open) antithesis, leading under certain circumstances to a new (stratified) synthesis. A system based on traditional chiefly authority, legitimated through genealogical rank and seniority, produces the conditions of its own undoing, through alternative sources of power, structural ambiguities in rank, and a dynamic conception of power readily accessible to historical appropriation. Goldman’s open societies are the results of such historical transformations--contingencies constrained by the structural properties of the systems they engage.

But the result of violence and warfare is not only change or disarray. Under certain (or, perhaps, uncertain) historical and ecological circumstances, violence can produce cumulative aggregations of power. Anthropologists have long documented the growth of state-level social formations as conquest states; in this sense, the emergence of stratified societies in Polynesia through cumulative violence has parallels throughout human history.

If this sequence is directional, it is no more so than Weber’s model of political transformation defined by traditional, charismatic, and rational bureaucratic political authority. Goldman does not claim that such transformations are inevitable in Polynesia, only that when internal transformations *did* occur, they were structurally constrained to occur in the predicted direction.

Thomas complains that Goldman approaches purely geographic variation as if it were developmental variation (p. 37). But Goldman’s arguments rest more on the reconstruction of local historical transformations within individual Polynesian societies than they do on cross-cultural comparisons. It is difficult to understand how Thomas can claim that Goldman employed homogenized and ahistorical conceptions of the cultures he was studying when so much of his analysis was an attempt to use archaeological evidence and oral traditions to document the shape of local historical transformations.

Thomas also faults Goldman for excluding consideration of the effects of European interactions with the islanders, treating each society as if it were the product of only its own internal developments (p. 38). Aside from the fact that many of the changes that Goldman documents antedate the European presence, this statement would suggest that he ignores European involvement in Polynesian affairs. This is not the case. Goldman discusses at some length the crucial influence of the English intervention on Pomare’s behalf (1970: 175-176). His position is

that European power and influence were critical catalysts in power struggles that were nonetheless Polynesian in character: "[T]he ultimate unification of the Society Islands must be recognized as the result of adding external means to native intent. Without English arms and missionary effort, Pomare might have been defeated and the balance of unstable powers restored. The intent of conquest, and even the concept of unification were, however, Tahitian" (Goldman 1970: 176). Similarly, Goldman does not ignore the role of Europeans in the evolution of the Hawaiian monarchy. He notes, however, that the consolidation of power in Hawaii that eventually led to the ascendancy of Kamehameha was well underway "even before the newcomers could intervene." Europeans "were only the midwives. Without them the new births might have been delayed, but probably not for too long" (Goldman 1970:200).

Is Goldman wrong here? As a historian of Polynesia, Thomas surely is in a position to have an informed response. By Thomas's standards, Goldman's book undoubtedly underplays the specific role of European influence in the islands. Goldman's assignment of European influence to an enabling and catalytic role rather than a definitive one may be a controversial position. But it is not the account that Thomas describes for Goldman. Though Goldman's claims are open to empirical refutation, none is offered by Thomas.

Ironically, if there is an unsatisfying quality to Goldman's account, it may well have more to do with its overdependence on local historical contingency rather than its overdeterminacy. For anyone looking for watertight explanatory systems, Goldman's vast treatise is not likely to prove satisfactory. In the end, he admits that he cannot explain to his own satisfaction *why*, for instance, Maori warfare never produced a truly centralized stratified hierarchy like Tonga's or Hawaii's. Even in Goldman's own version, there are too many exceptions to the rule to inure his structuralist account from the contingencies of "real" history.

Goldman himself put it best: "Only history can define the character of institutions" (1970:419). His scholarship discloses an impeccable integrity, an antipathy to allowing theoretical predilections to overwhelm the complexity of the material he analyzes. But the cost of this probity is that, as theories go, Goldman's is replete with unanswered questions and finely nuanced descriptions that are not quite explanations.

Though Thomas takes Goldman to task for pretending "that a particular text is simply a 'source' which may contain omissions or even errors but is not laced with interpretations" (p. 51), Goldman is actually far

more modest than this, “Our subject matter” he says in his preface, “is not chemistry. I would be overstating my intentions grossly if I did not make it clear that at bottom this is a work of interpretation. Its findings are to be regarded as hypotheses” (Goldman 1970:xi-xii).

In chapter 5 of *Out of Time*, Thomas sets out to show his hand at last and provide the historically informed alternative account to Goldman’s treatment of the Marquesas and Easter Island, two societies that Thomas knows well. Considering the amount of work he has put into clearing the way for this reanalysis by attempting to thoroughly discredit Goldman’s work, one has the right to expect a point-by-point refutation of Goldman’s account and an equally detailed alternative.

A close look at this refutation is revealing. Thomas’s alternative to Goldman’s reading is that Marquesan political institutions be seen simply as different from others in Polynesia. Marquesan society had departed “from a Polynesian pattern of hierarchical solidarity in which chieftainship encompassed society and was central to it” (p. 56). Note that here Thomas is willing to accept the existence of some essential “Polynesian pattern,” only insisting that the Marquesas be recognized as an anomalous case. Unlike the typical Polynesian case, Marquesan hierarchy was not based on chiefly lines. Both Marquesan and Easter Island societies reveal a distinctive pattern of historical movement “entailing a general shift from the prominence of chiefs to the prominence of warriors. Secondly the chief tends to become less of an orderly ruler and receiver of offerings, and more of a usurper or conqueror” (p. 65). But in Thomas’s view (contra Goldman) this shift to warrior power does not suggest that Marquesan society was egalitarian. Flexibility in status, we are told, is not the same thing as equality.

In a peculiar claim in which Thomas seems to actually adopt a kind of evolutionism, he argues that

[t]hese shifts [in Marquesan politics] could be seen as ‘devolutionary’ in the sense that a centralized structure is diminished, but do not necessarily entail a diminution of inequality or stratification. . . . Societies such as the Marquesas represent a divergent step, which is more likely to have been away from stratified systems than towards them. If ‘evolution’ is seen as directional change, and ‘devolution’ its undoing, then it is virtually impossible to understand these cases. (P. 65)

But such cases of “devolution” do not challenge the notion of cultural evolution, only that of unilinear general evolution.

Thomas's claim is simple. The Marquesan divergence from the general Polynesian pattern of hierarchy is simply noncomparable and not some sort of evolutionary development. It is purely a product of local history--"real" history. But this claim simply begs the question of whether such divergent patterns (such as lack of chiefly authority over religious rituals and productive resources) can be viewed as *significant variations* on more general Polynesian patterns of chiefly authority, understandable in relation to the very general historical processes that interest Thomas.

Thomas seems to dismiss such comparison out of hand. For example, he refers disapprovingly to E. S. C. Handy's use of a report of an early visit to the Marquesas by a Russian named Krusenstern. Krusenstern noted that if a chief were to strike anyone, it would inevitably be met by a return blow. Thomas comments:

This observation about behaviour or expected behaviour hardly bears upon larger patterns of respect, authority or dependence. Unless one saw the capacity to hit subjects with impunity as a standard chiefly prerogative, the point has little significance, since there are in many societies people with certain kinds of power or authority (such as European judges) who would meet 'like with like return' or at least public censure if they assaulted others. But there seems to be an uncomplicated notion that power is manifested in various ways which require no contextual specification. (P. 52)

The strained use of the European judges as a parallel for the Marquesan case would be a good candidate for an example of decontextualization of a cultural institution. One would think that a more relevant context of comparison would be with other Polynesian cases, in relation to which the ability to freely return a blow to a chief would be, I think, strikingly anomalous and warrant some kind of explanation.

Despite his recognition of a general Polynesian pattern of political organization, Thomas seeks to cast doubt on the widely shared belief that Polynesia is "a diverse but fundamentally unitary area" (p. 29). While not actually denying or providing evidence against the claimed cultural unity of Polynesia, Thomas still leaves the reader with the impression that this unity is not a fact but rather an ideologically motivated fabrication maintained by interests variously evolutionary, racist, and diffusionist.

This may be because granting that different Polynesian cultures

represented variations on a basic common pattern would open Polynesia to both evolutionary and diffusionist analyses. Rather than empirically refuting the considerable evidence for the unity of Polynesia as a culture area, Thomas occludes such analyses by pronouncing the very notion of a culture area in Polynesia to be politically unacceptable. No evidence for the unlikely claim seems to be necessary. Might this political agenda be what motivates Thomas's desire to limit the account of chiefly status in the Marquesas to a local pattern rather than a historical variation of a larger Polynesian pattern?

As to the determinants of this unusual political system in the Marquesas, Thomas stresses the devastating effects of contact "and many sorts of violence" during the nineteenth century (p. 55). Yet this implies that the Marquesan pattern of chiefly power did not evolve or develop *until European contact*. Does Thomas want us to understand historical transformation as beginning only with the coming of the Europeans? In this case we have a highly ironic appropriation of historical process in the Pacific as synonymous with Western "penetration," a position one would assume Thomas would be quick to abjure.

Certainly Goldman's account of the Marquesas suggests a long history of endogenous political transformations preceding the contact era. Archaeological evidence and oral traditions suggest increasingly violent status rivalry between 150 B.C. and 1790, fueled in the expansion period (1100-1400) by population pressure and land shortage. Goldman does not ignore the violence and population decline of the contact period (see 1970: 131). But he does suggest that the erosion of chiefly power was an endogenous development that began long before contact, and thus cannot be sufficiently accounted for by an appeal to contact history alone.

Thomas's precise view of chiefship in the Marquesas is not clear from his account. On the one hand, he insists the traditional Marquesan chiefs were simply different from other Polynesian chiefs and did not have ritual status or significant economic power as chiefs. On the other hand, he also implies that chiefs once had such power but lost it in historical times. Goldman's version seems more coherent, though it is also speculative. Goldman emphasizes the internal struggle in the Marquesas between traditional chiefs (*ariki*) and "made chiefs" (*haka-iki*), which was an honorary title for all first-born males.

Thomas links Goldman via Handy with a view that Marquesan society could be defined as egalitarian and loosely structured, in the interest of placing it at the "open" end of the evolutionary continuum. This seems to be his main complaint against Goldman's account. Yet I have not found where Goldman employs the term "egalitarian" in relation to

the Marquesas. His point is that there is evidence in the Marquesas for the increasing recognition of achieved status (wealth, military conquest, religious potency) beyond the domain of chiefly pedigree.

It was not that the Marquesas became more egalitarian but rather that the basis of inequality was less exclusively chiefly status. As for Thomas, his claims strike me as basically following the same lines as Goldman's, try as he will to differentiate his position:

While Marquesan chiefs were prominent and powerful figures, they were not really central to Marquesan life: a distinct, complicated hierarchy not connected with the chiefly line had developed. Chiefship was disconnected from Shamanism which controlled the fundamental life-giving ritual capacities . . . the privileged positions had become generalized among landholders. (P. 57)

What it would mean for prominent and powerful figures to be "not really central" to their society is somewhat puzzling. Again, Thomas seems to want to acknowledge the power of chiefs and deny it at the same time. If what he means is that multiple and competing channels of status and power had developed in the Marquesas, then all he is doing is making Goldman's point, but less cogently and in far less detail. In any case, Thomas's version does not strike me as a particularly lucid or compelling way to describe the Marquesan polity. And it is certainly no refutation of Goldman's account.

Sophisticated and thoughtful analyses like Goldman's deserve sophisticated and thoughtful critiques, analyses that treat their arguments with the subtlety and seriousness they deserve, even as they seek to improve on them. Spun out in a different key, Nicholas Thomas's *Out of Time* might have stood as a constructive and helpful cautionary tale for the pitfalls of ignoring history in anthropology. Unfortunately, its maladroit theorizing and mishandling of the texts it claims to surpass do not enhance the book's larger purposes.

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Cannibalizing Theorists of Polynesia

Out of Time by the Cambridge Oceanic specialist Nicholas Thomas constitutes theoretical reflections that parallel his extensive archival research resulting in the companion volume *Marquesan Societies: Inequality and Political Transformation in Eastern Polynesia* (1990). For some reason, Thomas decided not to publish a single work that would express a methodological and empirical synthesis; so readers will need to switch back and forth between the two volumes (and in doing so find that some material appears in both books). Whereas *Marquesan Societies* is clearly organized, exhaustively documented, and fully argued, *Out of Time* is the opposite. I do not know what stimulated the author's anger toward other Oceanic researchers and his hostility toward the discipline of anthropology, but the bitter tone will certainly annoy many readers. Traditional Marquesan society is often described as highly competitive, with pervasive warfare, personal aggressiveness, and even cannibalism; *Out of Time* can be seen as the ultimate expression of this cultural theme.

Thomas is out to show that anthropological discourse about Pacific societies (and by extension all societies) is fundamentally flawed by the neglect of short-term historical processes and long-term evolutionary transformations. To advance his critique, Thomas seeks to demonstrate that precisely those works that are intended by their authors to contribute to the study of short-term and long-term change are vitiated by unarticulated, "submerged" antihistorical assumptions. The widespread contemporary attention to ethnohistory and local "concepts of history" is rejected as a diversion from the real task of studying "history," by which Thomas always and only means concrete events. What angers Thomas are three sorts of anthropological discourses: those grounded in fieldwork experience, those based on comparative evolutionary reconstruction, and those informed by an interpretive approach to cultural analysis. The assault on fieldwork depends on the idea that ethnographers either privilege the ethnographic present and thus tend to offer a synchronic, functionalist account of society or rely too heavily

on informants' memories and as a result generate an ahistorical image of "traditional" culture. The attack on evolution focuses on the problem that linear typologies of development violate the actual historical connectedness of societies and fail to account for the possibility of local devolutionary transformation. The campaign against interpretive analysis is motivated by the opinion that the effort to show the coherence and consistency of symbolic systems fails to pay attention to "historicizing culture" itself.

Thomas's quest for a truly historical anthropology and his critique of existing theory and ethnography is summarized in the following prospectus (which also nicely reveals the flavor of the book's rhetoric):

The opposition is not, of course, between ideology and actuality, or between internal, culture-bound models and scientific description. Deep-seated notions of various kinds animate all descriptions, whether they are enunciated by or elicited from tribal people, fabricated in the heat of the moment or with scientific detachment by intruders, and whether they allude to moments or propensities. I am not asserting that accounts of events and notions can be construed as transparent and opaque respectively, but the permeation and constitution of depiction is an uneven process which therefore permits different conclusions to be drawn from different kinds of descriptions, precludes others sometimes and perhaps some all the time. Some accounts can be read against the grain and turned perversely to an analyst's purpose. The circumstances in which cultural structures are manifested and played out in action can be drawn into a discussion of cultural and social dynamics, but the notions and metaphors of the structure itself often cannot. Ideas do not usually offer a commentary upon their own formation. Anthropologists must therefore attend to events and practices, as well as representations. (Pp. 67-68)

This perspective can usefully be compared to the position of Sahlins, long an advocate of the analytical linkage among events, practices, and representations:

Empirical realities, then, are appropriated as social meanings, worldly instances of cultural classes. The meanings may or may not have been known before; moreover, as selective valuations of experience they can only imperfectly notice the "objective properties"--descriptions of what are inexhaustible. Nonethe-

less, worldly experiences are socialized as referential tokens of cultural types, of concepts that can be conceivably motivated in the existing scheme. Notice that just because there is a culture this does not mean there is no invention or novel response to material realities--albeit *by the same token*, the realities will then have effects of a distinct cultural type. (1988:45)

The principal victims of Thomas's "polemic collage" are Irving Goldman, E. S. C. Handy, Marshall Sahlins, and Jonathan Friedman. Smaller stabs are taken at John Beattie, Peter Buck, Raymond Firth, Clifford Geertz, Claude Lévi-Strauss, Ralph Linton, Sherry Ortner, Nancy Munn, and A. R. Radcliffe-Brown. (While reading the book, I wondered if Marquesan warriors consumed long-deceased enemies or restricted their feasting to freshly killed victims?) These and other scholars are accused of a variety of errors, confusions, misinterpretations, biases, dishonesties, and limitations, which, thanks to this 122-page essay, have not only been identified but corrected.

The line of argument runs something like this: when Handy went to the Marquesas in the 1920s as part of the Bishop Museum team, he was fooled into reconstructing a picture of early Marquesan society on the basis of informants' recollections. The resulting ahistorical report, *The Native Culture in the Marquesas* (Handy 1923), was then used by Goldman to build a general evolutionary account in *Ancient Polynesian Society* (1970), which uses unilineal typologizing in place of real developmental sequence. (How the alleged synchronic perspective of Handy influenced the evolutionary perspective of Goldman is never revealed.) The evolutionary argument found in two articles by Friedman is then dismissed on the theoretical grounds that it subordinates regional variation to "a teleology of increasing centralization" (p. 92) and on the empirical grounds that intragroup exchange is neglected. Finally, Sahlins's work on Captain Cook in Hawaii and on Maori cosmology and history is rejected as saying "nothing about the historical processes which actually make the conditions of life and culture variable across time and space" (p. 109)--a charge that will shock anyone who has actually read Sahlins's many books and articles. (I can recall the summer when I worked as Sahlins's research assistant in the Hawaiian archives wondering why he was so tireless in establishing the historical context for the "conditions of life" of the Hawaiians.) The fact that Sahlins triangulates among Hawaiian, Maori, and Fijian ethnographic cases is evidence enough for Thomas to label his research program "implicit evolutionism" (p. 109).

Rather than subjecting these four scholars' work to extended, critical

examination, Thomas devotes several pages at most to each, claiming all along that he is "out of time" and does not have space to engage in more substantive treatment. The issue of space is not convincing, since Thomas finds the opportunity in this short book to talk about non-Polynesian topics such as economic development in Madagascar, Kongo exchange systems, Javanese ritual, Australian aboriginal social organization, Andaman Island contact history, and Melanesian obsidian trade.

What is the ultimate source, in Thomas's opinion, of all this bad anthropology? He insists that the bulk of the problem is not that these ethnographers consciously constructed fallacious arguments but that their work is undermined by disciplinary assumptions, hidden metaphors, and unexamined biases, which often run counter to their explicit intentions and published rationalizations. Of course, the biggest "submerged" assumption is that the empirical object of anthropological research is essentially non-evenemential. This "exclusion of history"--remember that "history" for Thomas does not include cultural categories, discursive forms, or semiotic records--strikes those who, on the surface, appear most open to a historical approach. The second hidden cause of error is that anthropologists' honest efforts to understand social processes are vitiated by the uncritical use of ethnographic source materials. Thomas is especially critical of the massive corpus of works on Polynesia published by the Bishop Museum in Honolulu. These books are marred not only by vague memories of informants living long after "traditional" cultures had disappeared in the islands, but also by careless use of writings by voyagers, missionaries, colonial officials, and castaways. I find these twin accusations paradoxical in two senses. First, while affirming the power of the demon of ahistoricity to mislead even the most processually oriented researcher, Thomas is confident that he alone has developed the ability to overcome these submerged disciplinary hazards in his work on the Marquesas. Second, while well-intentioned scholars are led astray by biases in archival sources, Thomas's use of these same sources is not similarly affected. Theoretical assumptions and tainted sources seem to conquer everyone not armed with Thomas's uniquely keen insight and Archimedean hubris.

This leads to a particularly puzzling problem when Thomas then argues that one of the solutions to this morass is for anthropologists to combine a "decentering" of fieldwork with a return to the utilization of missionary records, which should not be too quickly dismissed just because they are systematically biased. At one point Thomas states, "Forms of evidence and analysis tend to be mutually implicated in an

implicit and almost surreptitious way. The force of argument arises frequently from unexamined metaphors, rather than from overt claims" (p. 42). At another point, however, he claims that "ethnographic depiction is not generally or uniformly invalid because a major or the principal interpretive threads are rejected [*sic*]" (p. 79). In other words, Thomas can dismiss the work of his professional colleagues because, despite positive merits or demonstrated expertise, they are victims of theoretical assumptions and, at the same moment, rescue a source of historical data the biases of which are part of the reason many scholars are misled! It seems only one principle guides whether a given body of written material is to be rejected because of submerged assumptions (i.e., fieldwork evidence) or embraced despite understandable bias (i.e., missionary records) : evidence central to anthropology's disciplinary identity is to be shunned while evidence generally regarded with suspicion is to be welcomed. (In a concluding summary, Thomas makes the troubling suggestion that "judgements about the worth of particular texts can only be made on a case-by-base basis, and depend as much on the project of the reader as the intrinsic features of the text" [p. 121].)

Since Friedman and Sahlins can fully defend themselves, I turn now to a brief consideration of Marquesan society as a background for evaluating Thomas's critique of Handy and Goldman. The Marquesas are a fascinating case because the group seems, on the one hand, to be the central locus for the development of eastern Polynesian culture (Kirch 1984) and, on the other hand, to manifest transformations in the system of chieftainship that parallel changes in Easter Island and that contrast with the better-known centralization processes found in Tahiti and Hawaii. Archaeological, linguistic, and ethnobotanical evidence points to the settlement of the Marquesas by voyagers from the western Polynesian hearth (Fiji, Samoa, Tonga) about 200 B.C.; from the Marquesas were then settled the outposts of Polynesia such as Easter Island and Hawaii about A.D. 500. Unfortunately for those interested in lineal typologizing and historical reconstruction, the settlement sequence of Samoa-to-Marquesas-to-Hawaii does not correspond to a stepped increase in hierarchy or stratification. In fact, something seems to have happened in the Marquesas so that the power of chiefs became disjoined from the spheres of ritual efficacy, economic wealth, and military might.

The question then becomes: Does Marquesan society represent an evolutionary midpoint--that is, on the way "toward" Hawaii and Tahiti, where chiefly power encompasses all domains of the society--or does the evidence point to a historical collapse from a fully stratified sys-

tern to one with a more fluid hierarchy and disconnected spheres of power? Thomas not only thinks the latter more probable (and thus assumes the real existence of Ancestral Polynesian Society in the Marquesas) but adds the point, made earlier by Dening (1974:26-29) and others, that some of the changes are the direct result of the penetration of Western colonial forces.

Turning to Thomas's specific criticism of Handy's interpretation of early Marquesan society, let me list seven of his claims: (1) Handy mistakenly sees Marquesan society as "egalitarian" and achievement-oriented in contrast to the ascribed rank systems of Hawaii and Tahiti; (2) Handy's book is subject to hidden "Boasian" diffusionist assumptions about sequential waves of migration, especially in accounting for stronger hierarchical phenomena in certain islands; (3) in focusing too simply on the potential for violence, Handy fails to see that political power was contextually specific; (4) Handy makes uncritical use of missionary generalizations; (5) Handy falsely assumes that the memories of his informants refer to a precontact period, when in fact they refer to a substantially altered postcontact period; (6) Handy's account neglects the presence of nonchiefly landowners; and (7) Handy's view of Marquesan chiefs is blurred by his lumping Marquesan data with the typical Polynesian pattern.

These are serious charges against a distinguished Polynesianist, but one only has to read Handy's ethnography to see that many of Thomas's accusations are false and contradictory. (1) Though Thomas uses the term repeatedly, Handy does not refer to Marquesan society as "egalitarian." Rather than describing an egalitarian society, Handy talks about chiefs as wearing fine ornaments, having servants and retainers, using special regalia, engaging in intrarank marriage alliances, inheriting by primogeniture, being the objects of sacred *tapus*, embodying the fertility of their social groups, and having the benefit of elaborate funeral rites--hardly data pointing to egalitarianism! Contrary to Thomas's direct claim, Handy's use of a quotation from the missionary Stewart does not "posit" (p. 58) an identity between the United States and the Marquesas as egalitarian societies; after citing the passage from Stewart, Handy says *absolutely nothing*. (2) Handy's book does not rely heavily on a submerged diffusionism; rather, he merely suggests that some systematic intergroup differences might be attributed to different settlement histories. (3) Handy is perfectly aware of the contextualization of power and in fact carefully distinguishes chiefly, religious, military, and economic dimensions of power. (4) Despite the small bibliographic slip that Thomas makes so much of, Handy makes extensive use

of missionary evidence, something strongly advocated by Thomas himself, and at every point in his book balances ethnographic testimony, voyage literature, and archival records. (5) Handy is careful to present the temporal referent of his data and several times suggests that the memory of his informants is to be questioned. To Handy's reference to "one great war" in which "the Mata'a and the Mo'ota were driven to take refuge temporarily at Vai Tahu, Tahu Ata, where they were subsequently returned" (1923:30), Thomas objects on the grounds that Handy thinks it "belonged to some distant, pre-European past" (p. 56). But Handy's sentence is located in a section giving a brief summary of tribal divisions and makes no such claim that the "great war" occurred in precontact times. In fact, Handy speculates about three possible historical trajectories of tribal divisions and explicitly discusses the differences between political patterns recalled by his informants and those appearing in the writings of early foreign visitors. And, to express the general importance of warfare in the society, Handy cites battles that took place in 1837, that is, in the postcontact period. (6) Handy makes specific references to nonchiefly landowners and even makes the same comparative reference to the Tahitian case that Thomas uses by way of criticism. (7) Handy is not at all confused by the typical pattern of Polynesian chieftainship; in fact, Handy repeatedly states that the Marquesan case differs from Hawaii and Tahiti. He is also aware of variation in degrees of inequality within the group and notes historical changes in the strength of chiefly authority.

What, then, is Thomas's new view of Marquesan chieftainship? His discovery boils down to four points: (1) that chiefly relations did not structure fixed group relations, (2) that the chief was only one of several power roles in the society, (3) that the chiefs relation to the people was based on patronage rather than hierarchical encompassment, and (4) that the operation of *tapu* was localized rather than regionalized. *All* these points can be found in Handy's excellent ethnography. Not only are Thomas's accusations against Handy false, but his reconstruction of Marquesan chieftainship is not original.

I wish I could end by noting that this heavy-handed book is relieved by lighter moments. The only amusement I found was in Thomas's description (p. 90) of generalized exchange as group A giving "husbands" to group B and his statement that in matrilineal cross-cousin marriage systems wife-takers normally outrank wife-givers! Such errors would not be made by any ethnographer who has actually struggled to analyze marriage data gathered in the field or by any anthropologist familiar with the standard works in the discipline. Finally, readers

interested in a clear and comprehensive assessment of the "state of the art" in Polynesian studies should consult the essays in Alan Howard and Robert Borofsky's recent edited collection, *Developments in Polynesian Ethnology* (1989).

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Historical Anthropology and the Politics of Critique

Cavilling here and there at some Expression, or little incident of my Discourse, is not an answer to my Book

--Locke

In the several years since I wrote *Out of Time* I have been concerned with rather different projects, and thus have a certain distance from the work that is not only temporal but conceptual. I am conscious of various deficiencies, and now feel, for example, that rather than confining my discussion to Polynesia, it would have been useful to have consid-

ered the ways in which anthropological dealings with histories and with historical sources emerged in the scholarly traditions of other regions. I wonder also whether I should have been more explicit about the way in which the book sought to cut across genre distinctions in the social sciences, by trying to force questions of methodology, substantive interpretation, disciplinary history, and theory to react against one another.

Certainly, it would seem from these reviews that I failed to make my intentions clear; but what is puzzling is that many other readers and reviewers found the book readable, straightforward, and informative.¹ In other words, while I might have expected *Out of Time* to be most accessible to scholars familiar with the Pacific, it emerges that some of them find it much harder to deal with than nonspecialist readers. Confusion and intelligibility are, however, not so much properties of either texts or persons as states that emerge in the relation between them, in particular readings; and while there are no doubt passages in my text that should have been more orderly or coherent, there may be good reasons why Oceanic specialists such as Richard Parmentier and Bradd Shore need to discover confusion in the book, which also account for their rather singular constructions of its project and argument. As we all know, it is no longer appropriate to claim that there is a single true rendering of any event or a single authoritative interpretation of a text. But this fact does not preclude assessments of competing readings; I would insist that some interpretations may be not only less adequate than others, but simply wrong, and it appears that because of their commitments to the disciplinary procedures and protocols that *Out of Time* interrogates, Parmentier and Shore are obliged to distort the text they anathematize in a distinctly extravagant way.

I begin here by briefly summarizing the books argument. It was concerned with the relation between overt theoretical interests, particularly with the historicization of anthropology, and paradigmatic features of the discipline that might be at odds with those interests. I suggested that the problem arose in the first place because the professionalization of anthropology, in both its British and American forms, was premised on radical exclusions that enabled specialists to establish a monopoly of competence, particularly by sealing off their subject matter from historical contingency, and secondly by dismissing or marginalizing nonprofessional ethnographic sources. This provided a background to more specific arguments about the ways in which history had been neglected or suppressed in Polynesian anthropology, in part for these general reasons and partly because of certain features of the interpretation of Polynesian hierarchy and transformation. The critique at

this point worked in two directions, establishing that ahistorical ethnography led to particular misinterpretations, but also more generally that theory-laden sources carried a burden of interpretation that was often not appreciated or disarticulated as the sources were incorporated into another theoretical edifice.

Another strand of the argument was that the repudiation of evolutionary theories, or of generalized notions of unilinear evolution, did not prevent arguments from being saturated with metaphors and connotations that implied progressivist development, archaic/advanced juxtapositions, and equations between remoteness in time and difference in space. I argued that despite the interest of Sahlins's structural history, and the extent to which it did help build a more historical anthropology, it remained inflected by evolutionary discriminations of this kind; the extent to which it actually historicized indigenous culture was also rather limited. These critiques led to certain recommendations, for example, that in the Oceanic case any interest in precontact social forms had to consider archaeological evidence more seriously, though without succumbing to the crude materialism and positivism that is still surprisingly widely encountered in that discipline.

More generally, I suggested that ethnographic fieldwork needed to be decentered--meaning not that it should not be done, but that it should no longer occupy a sovereign place as the source of anthropological evidence. Since these wider conclusions are very much at issue in this debate, I discuss them further below.

Histories

A book that dealt with such a range of themes obviously needed to delimit its subject matter in various ways. Hence I made it quite clear that I did not venture into debates about historical representation, about what history is, in the abstract. David Hanlon considers that any inquiry of the kind I engage in "requires meticulous definition of the key concepts involved." He notes that, far from doing this, I intentionally and deliberately avoided defining "history" and "evolution" on the grounds that meanings subsist in the uses of ideas in texts, thus regrettably leaving "the reader to manage the intellectual void and confusion that result." Setting aside the interesting question of how one can produce *both* void and confusion, this strikes me as rather like an ethnographic situation in which an inquisitive stranger might ask about the meaning of a word such as "politics" or "democracy," and you respond by suggesting that you can take the visitor to political meetings and

watch television news programs in which these words are used frequently; exposure to the discourse, you suggest, will give the inquirer a sense of the sorts of things that “politics” can signal or allude to. No, says the visitor, I want *you* to tell me what it means, to give me a definition. While it is entirely understandable that glosses and cognitive maps are demanded (cf. Bourdieu 1977:2), it is evident that definitions, whether of “politics,” “history,” or “evolution,” are spurious and unhelpful. The terms have been used in eclectic and contested ways, and the important exercise entails tracing those uncertainties, rather than attempting to legislate them. To expect that a certain definition of a word--advanced, say, in a preface--actually encloses or determines its meanings in the remainder of any particular text, or its meanings as registered by readers, seems to entail a peculiarly rationalistic view of the ways in which written arguments are ordered. Even if a concept is a neologism or theoretical novelty, and thus might seem less susceptible to “misreading,” one cannot presume that its meanings are coherent, that it has the same value or function wherever it is deployed.² I suggest that if one is situating certain problems in the prior meanings of concepts, it becomes important not to declare redefinitions in a flag-waving fashion, but to implement a different usage in analytical practice. My stance on this owed something to later Wittgenstein, and I would go back to his work if it seemed worth debating the abstract issue at greater length.

I am puzzled by the suggestion that I conflate history with mere temporality, since at several points I argued that some social theorists have effectively temporalized systemic models (generally by privileging some conception of reproduction) but did not effectively historicize them; that is, they did not create terms that would enable us to understand culture or social relations as historically constituted. What was pivotal to this argument, obviously, is the assumption that time and history are entirely different. Hanlon proceeds to raise the question of whether my references to “real history” and “actual history” imply regression to a naive idea of “what really happened” that would seem oblivious of all the reflection upon historical method and interpretation since Carr. But what these words meant is quite clear from the context:³ in one case I was juxtaposing historical social transformations with the kind of transitions imagined in evolutionary narratives, and in the other I was differentiating a historical anthropology from one that had merely been installed with a certain temporality. Though my adjectives were evidently injudicious, I cannot apologize for suggesting an important difference between the transformations imputed in a general model of the

progression from chiefdoms to states and those postulated or reconstructed in an inquiry into the social transformation of, say, Tongan or Marquesan society over the last thousand years. In this case, "real history" means particular history as opposed to a generalized or conjectural scheme. This is not to say that I take my reconstructions of such histories to be less theorized, or to somehow reflect developments in a less-mediated fashion, than an explicit or implicit evolutionary model; but it does say that one has an object of quite a different kind to the other.

In fact, I think that both my general meanings of history and evolution and the direction of argument were made quite clear in the opening discussion, where I noted that there was "a tension between former characterizations--in which history is an empirical succession of events and evolution refers to progressive development--and a merging of meanings in a more satisfactory analysis of change which is processual and systemic but neither directed nor abstracted" (pp. 3-4). In other words, what I was proposing was that neither conventional narrative history nor evolutionary anthropology could constitute an adequate historical account, if one's interest was in the short- and long-term dynamics of social forms. These juxtaposed discourses could, however, be superseded by a kind of history that was systemic (and hence interested in structures of meaning, political dynamics, and the expression of structural change in immediate events and representations), that thus appropriated the deterministic character of evolutionary argument, and its interest in larger dynamics and transformations, while repudiating its teleologies and directionality--features that were unsatisfactory both on analytic grounds and because of their ideological implications.

I am also perplexed by the claim that my use of archaeology in reinterpreting eastern Polynesian social transformations leads to an account that is chronological and heavily dependent "upon simple linear developments" rather than historical in any admissible sense. I am fully aware of the reductive and theoretically unsatisfactory character of much of the archaeological literature (and had written earlier on this topic myself), but its limitations do not make it irrelevant or wholly unusable from the perspective of social history or anthropology. Of course, there is no general question about whether archaeological data are useful or not that can be posed in the absence of a particular agenda or theoretical problem. My questions concerned how indigenous societies and political hierarchies on Rapanui, in the Marquesas, and elsewhere had developed, with the distinctive results that were apparent in the early contact period. What I extracted from archaeology was not

merely chronology, but implications of social processes and various correlates of political and ritual transformations. For example, the elaboration and expansion of Marquesan *tohua* grounds suggested that competitive feasting might have expanded dramatically at the same time as various other developments, such as accelerated ecological degradation, intensified warfare, and so on; with respect to Rapanui, it was important to establish that indigenous society had not always been the battlefield apparent in the nineteenth century, that the particular features of that disorder owed something to the more stable hierarchy that preceded it. In other words, these discussions were concerned with transformations of social orders, which did impinge very substantially on the conditions of life that Hanlon suggests I neglect. I was, for instance, trying to account for the different configurations of ritual authority in the Marquesas and elsewhere, and the extent to which food supplies and lived hierarchical relationships were far less secure on Rapanui and Niue than elsewhere in Polynesia.

The underlying problem, I sense, is not that there is a coherent complaint about what this investigation produces, but that Hanlon believes I ought to be doing something different. My text “evidences no appreciation for the ways in which other societies might construe, express, and utilize a very different sense of time”; having my own culturally determined sense of time, I moreover effectively suggest “that others’ pasts can be discerned, charted, and understood through Western notions of change over and in time.” So, far from breaking from an evolutionary argument, I inadvertently promote it by “limiting anthropological understanding to a *very specific* cultural understanding of chronology and sequence” (emphasis added). In other words, the suggestion here is that *because* I am “oblivious to or unconcerned with local conceptions of time,” my analysis cannot transcend the evolutionary problematic that it criticizes; Hanlon thus stipulates that a real theoretical break can only derive from an appreciation of non-Western temporalities. What is unacceptable here is the homogenization of “Western” notions of change and time and their conflation with “the existing evolutionary paradigm”; obviously, ideas of contingency, time, and historical change in European thought have been enormously diverse (see, e.g., Pocock 1975), and it is not *in principle* necessary to step outside that tradition in order to criticize or reconstruct particular ways of representing histories and social transformations (however desirable it may be from the stance of particular arguments). Of course, I nowhere claim that my conceptions of change and history are not culturally informed, and insofar as they are, they are obviously peculiarly constrained and inflected to the

same extent as any other cultural representation. So what? Is this not also true of accounts that do take an interest in others' constructions of their histories, that after all have often been accused of ventriloquism, of purporting to present others' voices while structuring and enframing what they say?

It is beside the point to claim that *Out of Time* "gives little attention or credibility to indigenous sources and modes of historical expression" since this was a task that the book explicitly bracketed off. Hanlon makes the point that in challenging Handy's account of Marquesan society, I attempted to "discredit" his informants' memories, but neglects to consider the context of my argument. I was considering specifically whether the salvage ethnography Handy engaged in during 1920-1921 could provide an adequate account of precontact (late eighteenth-century) social relations, that is, the "native culture" prior to transformations attendant upon European contact and dispossession. I was discrediting the method, not the indigenous knowledge; Handy himself accorded no importance to indigenous constructions of the history, but was merely using memories to answer ethnological questions. If it is these ethnological questions--concerning, say, the nature and significance of chieftainship--that are at issue, I would suggest that what islanders were reported to have said in 1800 is more useful than what their descendants were reported to have said 120 years later, which I wouldn't expect to be more reliable than an account I could give of social circumstances during my grandfather's infancy. I would suggest, moreover, that if we are concerned with such questions as the character of chiefly or shamanic agency, it is most important that practices and events--the circumstances under which cultural precepts are put at risk and contested--are examined. This can only be done by working through early contact descriptions, obviously in a fashion that reads them critically, taking account of the interests of observers and authors. Indigenous recollections are in principle an equally important source for practical contests and other events, and are likely to be more important to the extent that the outsider often has a poor grasp of the cultural dimensions. However, the type of information obtained by Handy mostly took the form of generalized statements rather than particular narratives that might have enabled a more nuanced understanding (as is apparent not only from his publication but also from his field notes). More importantly, these informants were simply too distant in time to be in a position to give an account of the practices and relations of the precontact or early contact period. This is, moreover, a context in which it is problematic to treat the accounts of modern islanders as "internal"

and Europeans as “external.” For contemporary islanders, who are mostly Christians, pagan ancestors may be stereotyped in a variety of ways that are almost equivalent to the “othering” imposed by early European observers. Without suggesting that their accounts are therefore uninteresting, I do contend only an ethnic or cultural essentialism can presume some organic continuity between modern views and those of indigenous people in the early contact period or before.

All of this, however, was an argument that specifically related to the reconstruction of indigenous social relations in places such as the Marquesas, where contact histories were comparatively long and also highly disruptive. It was not argued that this was the only appropriate historical project or that indigenous perceptions of the past were generally unimportant. Nor did I suggest that the wider issues that Hanlon alludes to concerning the problematic and contested character of historical representation could be passed over. The extent to which I have taken these issues seriously is to be measured from extensive discussions in separate publications (Thomas 1990a, 1990b, 1991a), not from *Out of Time*, which focused on the anthropological occlusion of history and stated that it did not deal with associated issues such as these.

The major charge--that I fail to tackle the problem of what history is --thus seems equivalent to a complaint that Hobsbawm's *Age of Empire, 1875-1914* does not tell us what we need to know about the age of capital, 1848-1875. Hanlon is an ethnohistorian whose book on Pohnpei I very much respect. But I think that in this context he is failing to acknowledge that one publication of mine had a particular agenda; its writing was not haunted by the question of Carr's that haunted his reading, but by a variety of other problems; instead of questioning whether the book effectively addressed the issues it did raise, he objects in effect that it does not deal with his preoccupations--though these, ironically, have also been preoccupations of mine in other contexts.

The Marquesas and Polynesian Social Transformations

Richard Parmentier makes a very basic mistake right at the beginning of his review, in assuming that *Out of Time* was motivated by some personal “anger” toward other Oceanic researchers and to the discipline of anthropology (an interpretation that no other reviewer has advanced). This leads him to read the book as a sort of anthology of personal attacks and to ignore or misconstrue its key arguments. He thus pays no attention to the central points of my critique of Sahlins--that the structural history model could account only for externally prompted, not endoge-

nous, change; and that the argument could not provide an adequate account of transformed living conditions over the longer colonial period, being restricted in its effectiveness to the cultural dynamics associated with the relatively brief phase of early contact. These were obviously comments on Sahlins's *texts*, and *Historical Metaphors* and *Islands of History* in particular, yet Parmentier reads them as if I was imputing some reproachable lack of interest in Hawaiian living conditions to Sahlins personally. If critique of theory is only legible as critique of individuals, the preconditions for an adequate reading of my book, or any other work of contemporary analysis, are lacking.

This reduction is not peculiar to one moment of Parmentier's comment, but is also manifest in his attempt to defend Handy's work on the Marquesas. He states that I argue that Handy "was fooled into reconstructing a picture of early Marquesan society on the basis of informants' recollections." It is quite crucial that I nowhere suggest that Handy was "fooled" into producing an ahistorical distillation: my argument was that he produced precisely the sort of synchronic construct of "the native culture" that Bishop Museum modes of investigation were designed to produce (a construct only partly derived from informants' memories, though I did suggest that Handy attributed what was remembered to a generalized traditional culture rather than to the singular circumstances of the 1850s and 1860s). I pointed out that within the spectrum of Bishop Museum reports, Handy's effort would have to be regarded as a competent and unusually extensive description; for this reason, in fact, it stood as a good example for assessment.

Parmentier subsequently raises the issue of how I can be critical of those who use voyage writers and missionary sources carelessly, while claiming to overcome difficulties with such sources myself; for him, this manifests "Archimedean hubris," but the example of Handy's work makes it obvious that there is a simple and concrete difference of method. In suggesting that Marquesan society "was always of the very simplest order," that chiefs did not have elaborate powers over their people, Handy made no effort to investigate what individual chiefs actually had done; he did not refer to incidents in which their capacities were at risk or at issue. Instead he quoted the generalized impressions of certain early visitors, such as Krusenstern and Stewart, to the effect, for instance, that the government was "anything but monarchical" (Handy 1923:35). Now, it should be obvious that statements of that kind often neither refer to nor draw upon any especially elaborate knowledge of the manifold rights and capacities that constitute power; in the case of the missionary source, I suggested good reasons why the missionaries

would understate the degree of chiefly control: they attributed the failure of their own project to a lack of centralized chiefly support. In contrast to this, my argument about the alternate character of chiefly power was based on a reading of events, in some cases on the basis of day-to-day accounts, that manifested the actual capacities of chiefs such as Keatonui and Iotete, and the ways in which these were represented by themselves and other islanders at the time. While Shore suggests that Krusenstern's statement that if a chief hit anyone "he would infallibly meet with a like return" requires more serious consideration than I accord it, he fails to note that this is merely an impression that was not based on observation or on definite information of any kind; to the contrary, it was no more routine for commoners to physically assault chiefs in the Marquesas than anywhere else in Polynesia.⁴

The central point of this discussion was not one that Parmentier alluded to, such as my reference to Handy's diffusionism (which I did not claim his book relied "heavily" on; such an argument would have undermined my emphasis on the museum's preoccupation with synchronic native culture). Rather, I was concerned that Handy fundamentally misrecognized Marquesan forms of property,⁵ and was thus able to misrepresent Marquesan society primarily as a less centralized or stratified version of other eastern Polynesian systems rather than as a distinct and divergent development. This, in turn, is what makes it possible for Goldman to imagine "Open" Marquesan society as a possible precursor to "Stratified" societies, whereas if its specificities are recognized, it is apparent that Hawaiian- or Tahitian-type hierarchies cannot develop out of this form. Both Parmentier and Shore take exception to my suggestion that Handy saw the Marquesas as "relatively egalitarian" in relation to Tahiti and Hawaii, on the grounds that he did not use that word. Handy, however, alludes to "the communism and simple democratic nature of the tribe" (1923:35), which, if anything, carries rather stronger implications.⁶ I do not accept that it is unscholarly to paraphrase a writer's usage, especially given that "communism" in this sense has dropped out of the anthropological vocabulary and is potentially misleading.

Parmentier's attempt to salvage Handy's account of the Marquesas as an "excellent ethnography" comes as something of a surprise. There are major misinterpretations concerning hierarchical forms: contrary to what Parmentier asserts,⁷ Handy had no understanding that a system of *tapu* grades associated with particular forms of tattooing and exclusive eating fraternities existed, though this was the closest approximation to an encompassing hierarchy in the Marquesan polity. Among Handy's

many careless assertions is the claim that unqualified individuals could rise to positions such as chief on the basis of achievement, which is not documented at all prior to the 1840s and not even common thereafter, even though chiefly positions were subject to direct interference from the French and had suffered from a loss of influence and prestige for a variety of reasons. A more specific example of the shortcomings of Handy's information is the question of the political unity of the island of 'Ua Pou. Though he noted correctly that the island was unified under one chief, that this was an indigenous development, and that the situation was unique in the group, he thought the unity was effected only at a late date: "About 1860, before European influence was really felt in Ua Pou, Te-iki-tai-uao . . . secured control of the whole island" (Handy 1923:31). In fact, it is clear, in part from documents that Handy himself used or had access to, that there was a line of paramount chiefs earlier, of whom three individuals can be identified by name (see Thomas 1990c:215-216); and, although it is not clear what European influence being "really felt" means, there was actually a good deal of missionary intervention before 1860 that was partly responsible for social instability by that date. In other words, even when Handy happens to be correct about a general point, he had not located the most relevant evidence and attached dubious significance to that which he did use.

Turning to my alleged misreadings of Goldman, Shore similarly misunderstands the exercise by interpreting *Out of Time* exclusively as a critique of Goldman's explicit argument, rather than also as a discussion of the theories and evolutionary adjudications implied in the text. Far from forcing Goldman's account into a straitjacket, I acknowledge and discuss the varying formulations of difference and change (pp. 35-36, 127-128), which I suggest are ambiguous and not fully coherent. It is clear, however, that Goldman sets up some societies as antecedents of others (the Maori of the eighteenth and nineteenth centuries being posited as a "Traditional" antecedent to other types) and, moreover, that he postulates an overall developmental categorization and sequence. Shore says that "Goldman does not claim that such transformations are inevitable in Polynesia, only that when internal transformations *did* occur, they were structurally constrained to occur in the predicted direction." I never suggested that Goldman assumed that Polynesian societies *had* to evolve toward the Stratified form. My argument relates mainly to the second general claim; I suggest that the Open/Traditional/Stratified categories are inadequate both for map-

ping Polynesian social variation and as a basis for theorizing transformations. The western and eastern Polynesian "Stratified" societies include Hawaii and Tonga, which can only suppress the significance of exchange and regional integration that gives the latter much of its distinctiveness as a form of hierarchical reproduction.

Secondly, Shore asserts that I argue that Marquesan society is simply different, as though I was merely pointing to some empirical exception to Goldman's scheme. It is clear, though, even from what he acknowledges himself (since he notes that I show that Easter Island accorded with a similar pattern), that I was suggesting Marquesan society exemplified a divergent transformational path, one in which shamanism, warrior dimensions of chiefship, other forms of *tapu* hierarchy, and nonchiefly based property relations became consequential and overshadowed the structures from which the eastern Polynesian type of "Stratified" society could develop. Contrary to the inadmissible suggestion that I attributed this pattern merely to postcontact developments,⁸ Thomas 1990c contained a detailed argument that attempted to link these developments to Suggs's phases of Marquesan prehistory; this and the analogous argument for Rapanui was summarized in *Out of Time* (pp. 59-65). If either Shore or Parmentier really wanted to explore further the differences between my construction of the Marquesas and those of Handy and Goldman, they should have read *Marquesan Societies*, which presents much fuller ethnohistorical documentation and analysis.

Jonathan Friedman's comments are the only ones here that I find consequential or informative. Regarding his response to my criticism of the world-systems approach, I would concede that I imposed something of an empirical reduction onto a structural model; although I would also suggest that if empirical illustrations are being used to evoke structural processes, it is important whether the proposed model can, in fact, generate the range of variants that are adduced. Friedman's suggestions in the article I discussed (1981) and in this context concerning the distinctive character of the eastern Polynesian societies and the nature of western Polynesian contact history remain stimulating; rather than commenting further here I would prefer to revise my arguments more extensively elsewhere. Part of the problem is that much detailed ethnohistorical research remains to be done; and many accepted views--concerning, for example, the nature of relations within the Fiji-Samoa-Tonga triangle--might need to be reformulated in the light of closer analyses of the eighteenth- and nineteenth-century sources.

The Politics of Critique

The reader will, of course, have noted the generalized hostility that underlies Shore's and especially Parmentier's critiques. This leads to innumerable minor misrepresentations that add up to a travesty of *Out of Time*. I can only draw attention to a few of these by way of example. Shore follows Hanlon by complaining that I do not problematize history but neglects my explicit statement (p. 4) that that is a separate task (one I have in fact addressed in several other publications). He also suggests that my argument complains that ethnological discourse reinforces Western postulates of a historical self and a timeless other, though I allude to generalized ideological dimensions of evolutionary thought only in passing and never in that particular form. What Shore is doing is homogenizing certain critiques he evidently considers undesirable (myself, Fabian, "the current round of scholarly self-abuse," etc.) and casting them as crudely political. A bewildering aspect of this is Shore's claim that I seek "to cast doubt on the widely shared belief" of Polynesia's unity. What I actually say, referring to early perceptions of the shared background of Polynesian populations, is that "the basic insights remain far more credible than those of any other diffusionist scheme" (p. 31). Additionally, I am explicit that my arguments depend on an elaboration of Kirch's construct of "ancestral Polynesian society" (1984), but Shore is eager to find me "pronouncing the very notion of a culture area in Polynesia to be politically unacceptable"--a pronouncement that, needless to say, appears nowhere in *Out of Time*.

This attitude is even more extreme in Parmentier's comparisons between the book and Marquesan-warrior aggressiveness and cannibalism. This is so extraordinary that I can hardly find it insulting, but am disturbed to find that such hackneyed colonialist stereotypes of Marquesan behavior (for cannibalism was restricted to very specific ritual contexts indeed) remain current among Oceanic anthropologists, of all people. This predictably leads into a series of complaints that have no basis whatsoever in a competent reading of my text. For example, Parmentier complains that a number of authors are criticized in brief discussions, but (apart from attempting to salvage Handy) he makes no attempt to argue that these are *too* brief, that is, that their points are insufficiently substantiated. Beattie's work was, for instance, discussed briefly to illustrate the simple point that methods of investigation could encode theoretical and explanatory models. Why should twenty pages be devoted to this matter instead of two? Where Parmentier does comment on the interpretation of specific writers, what he says makes no

reference to my main arguments (with respect to Sahlins, for instance); he merely asserts that those familiar with Sahlins's work will be "shocked" by what is claimed.⁹ As if the arts of misreading have not been fully displayed, he proceeds to assert that for *Out of Time* "history" "does not include cultural categories, discursive forms, or semiotic records." In fact, what I set aside at the beginning of the book was the definition of history that was *limited* to the representation of the past; this did not mean that my view of the past *excluded* representations. Even the most cursory reading of the work in question, of *Marquesan Societies*, or of any of my other books could not sustain the view that the history I sought to construct was not both cultural and political.¹⁰

Parmentier obviously has a stake in the legitimacy of the whole edifice of Polynesian studies that leads to his hysterical characterizations of "cannibalistic" critique. Shore also revealingly notes that his response to *Out of Time* stems from his own high regard for Goldman's work, though he makes no effort to argue on the basis of his Samoan expertise that Goldman's construction of that society is informative or defensible. Had demolishing Goldman been my main concern, many factual or interpretative errors unconnected with the evolutionary issue might have been mentioned. As Judith Huntsman has recently noted, *Ancient Polynesian Society* "has been celebrated far beyond its merits as a basic source and major contribution--even by scholars who should know better. It is seriously flawed in both conception and substance and far too many scholars through naïveté or laziness have allowed themselves to be misled by it" (Huntsman 1991:331). Responses of the Parmentier-Shore variety do not amount to "an answer to my Book" but merely express a subdiscipline's insecurity: my sort of critical discussion is unacceptable because it fails to defer before the profession's hall of fame (Handy is a "distinguished Polynesianist"). To take offense, to personalize the issue, to refrain from any critical engagement with currently authorized and established texts, are all part of a problem that can be explicated by the sociology of the academy, hardly peculiar to Pacific studies. Shore wittily suggests that my book is "out of tune," but in scholarly milieux of this kind--in which senses are dulled by the guild members' narcotic--discordant notes seem called for, indeed. Had the book been widely misread in the manner of these reviewers, I would be disturbed and disappointed; within the spectrum of responses that I have received, I can only situate Shore's and Parmentier's difficulties in their own defensive professional agendas; and, conscious as I am of the many ways in which the book might have been improved, I cannot say that their comments would prompt me to revise a word of the text. I am amused, however,

that another reviewer found fault with the book because it had no radical content--most anthropologists would agree with everything it said!

NOTES

1. See, for example, Coronil 1991, Leaf 1991, and Roseberry 1991.
2. Consider, for instance, the rather various uses of the notion of "the structure of the conjuncture" in Sahlins's *Historical Metaphors* (1981).
3. In fact, the allusion to "the orderly march of people and their thoughts and doings" (p. 118) was not a gloss on history at all, but rather an ironic reference to functionalist-anthropological views of society, as is quite apparent from the context.
4. In the same way, Shore's defense of Goldman on the grounds that he did draw on what is called "naive ethnography" is not to the point. While true that Goldman cites many works other than the Bishop Museum bulletins, in most instances the latter are his key sources (see *Out of Time*, pp. 41-49 and 128 for detailed discussion). Goldman's account would have been more adequate if he had had access to a wider range of material, but adequate appreciation of that material would have required him to exercise greater contextual sensitivity, which would have separated out accounts relating to say 1800 and 1850 in various cases. In its homogenization of "native cultures" in each case, Goldman's approach to ahistorical distillation is much the same as that of the museum bulletins--though I do take the point that certain transformations such as the consolidation of the Pomare's authority in Tahiti lead to more specific and historically staged characterizations. But this is merely to point out that what Goldman should have attempted for all the Polynesian societies was gestured toward in two or three cases.
5. Parmentier correctly notes that Handy makes a few references to nonchiefly landowners, but these are exceptions at odds with his overall characterization. The general view Handy advances, that there was encompassing titular ownership on the part of the chief (1923:57), must be categorically rejected (for full discussion, see Thomas 1990c: ch. 3).
6. Parmentier also dismisses my comment on Handy's quotation of the missionary Stewart's reference to a "republic *en sauvage*": "after citing the passage from Stewart, Handy says *absolutely nothing*" (Parmentier's emphasis). What this ignores is the whole section in which Handy quotes a sequence of texts to establish the "communism" and simplicity of Marquesan society, to reject the view that Marquesan chiefs were in any way like kings (1923:35-36). I made it clear that Handy could not be taken to be making a simple identification between American and Marquesan egalitarianism, but that something of the sort was necessarily implied by his claim that American visitors could understand the society better than those from Europe: as distinct from European sailors "imbued with the European conception of kings and nobles and commoners," Porter "from republican America . . . speaks always of chiefs, never of kings" (1923:37). My point here was not, of course, that Marquesan chiefs actually were like kings, but that this axis of characterization was blind to distinctive forms of inequality in Marquesan society, which needed to be examined in the context of domesticity and property relations rather than with reference to political centralization.
7. If this is what he means when he refers to my view that "the operation of *tapu* was localized rather than regionalized."

8. This is linked with an argument that I overemphasize colonial “penetration,” even though such emphasis is rejected in the book (p. 113), as it is in a more extensive account of Pacific colonial histories (Thomas 1991b).

9. “The fact that Sahlins triangulates among Hawaiian, Maori, and Fijian ethnographic cases is evidence enough for Thomas to label his research program ‘implicit evolutionism’ (p. 109).” What is actually being referred to there is my comment on Sahlins’s contrast between Maori and Hawaiian ritual regimes, in which “from . . . to” idioms are used, implying that the Hawaiian polity emerges from a Maori beginning point. As I made clear, that form of argument, which makes contemporary society A the ancestor of society B, can only be seen to carry evolutionary implications. Although Valeri has suggested in a constructive and critical review (1991) that Sahlins’s implication is not evolutionary but is situated in the “purely logical space” of Levi-Straussian method that does “not prejudge a historical account of the relationships between Maori and Hawaiian cultures,” it hardly seems accidental that the transformations are from Maori to Hawaiian rather than vice versa.

10. With respect to other points, Parmentier complains that because *Out of Time* complements *Marquesan Societies* “readers will need to switch back and forth between the two volumes” (though the review conveys nothing to indicate that he is familiar with the second book) and suggests that for some bewildering reason I “decided not to publish a single work that would express a methodological and empirical synthesis.” This ignores the different ways both works were at once empirical and theoretical, and overlooks the different horizons and audiences of each project. Second, Parmentier’s assumption that I do not believe in fieldwork is incorrect: my critique concerned the place of this form of research activity in the construction of disciplinary authority. Third, I take it that the juxtaposition of a passage of mine concerning ideas, representations, and practices and a quote from Sahlins’s article in *Critique of Anthropology* is supposed to indicate that far from being new, my ideas are anticipated by one of the writers I criticize. The passage quoted is not actually a summary or “prospectus” (on p. 68?) of the larger argument, but relates to more limited issues about the interpretation of the different properties of expository and event-oriented description. And at a number of points I do agree with Sahlins’s general objectives and formulations; the debate was about the extent to which certain of his concepts and interpretative methods effected a historicization of anthropology. Fourth, while the “exchange of husbands” notion is supposed to attest to my ignorance of alliance matters and what real anthropology is all about, this was part of a description of the matrilineal, uxorilocal prestige-goods system; what was intended to be a lighthearted inversion of androcentric terminology is not, in this case, inaccurate anyway. Finally, I would not accept being labeled “a historian” if that is supposed to make me external to the discipline of anthropology; as it happens, in both Cambridge and Canberra my affiliations have been with anthropology departments.

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REVIEWS

Bambi B. Schieffelin, *The Give and Take of Everyday Life: Language Socialization of Kaluli Children*. Cambridge: Cambridge University Press, 1990. Pp. viii, 278, figures, appendix, glossary, bibliography, index. US\$49.50 hardcover, \$16.95 paperback.

Reviewed by Terence E. Hays, Rhode Island College

This is an extraordinary book, both for its focus and for its accomplishments. The exotic aspects of New Guinea societies and cultures have long been celebrated, and each year sees the appearance of new analyses of ritual, ceremonial exchange, and warfare. Usually lost in the background is the less glamorous, routine social interaction that fills most of the lives of the people. Schieffelin's book is different--"a particular type of ethnography, one that focuses on the microanalysis of everyday speech and conduct between caregivers and children" (p. 13). Her overall concern, to be sure, is with "linking their practices and patterns to others expressed through myths, rituals, song, exchange, and other symbolic systems" (p. 13), but the center of attention throughout the book is just what its title indicates: "the give and take of everyday life."

More specifically, the "everyday life" that especially interests Schieffelin is that of language socialization, that is, the contexts in which Kaluli children are "socialized to use language and socialized through the use of language" (p. 239). Studies of socialization, under the rubric of "childrearing," have a long history in anthropology, and the acquisition of language is a common subject of interest for many linguists, psychologists, and others. But, as Schieffelin correctly notes, research on the former topic has usually ignored the pivotal role of language and speech, while our understanding of how children acquire language has depended too much on observations of white, middle-class children,

with "culture" seldom considered explicitly as a significant variable. Schieffelin, like others who have focused on language socialization in the past fifteen years or so, argues for an *integrated* approach, combining "a semiotically based ethnographic perspective with an ethnomethodological interest in examining the details of social interaction and talk for what they reveal about members' methods and preferences" (p. 13). The results are both impressive and persuasive.

The book, as with Schieffelin's preceding doctoral dissertation in 1979, is based on her extensive fieldwork in Sululib, a longhouse community of about one hundred Kaluli-speakers on the north slopes of Mount Bosavi in the Southern Highlands Province of Papua New Guinea. The Kaluli are well known through the ethnographic publications of Steven Feld and Edward L. Schieffelin, perhaps justifying the brief section provided here on "the ethnographic setting" (pp. 2-7). However, more information on Kaluli society and culture is appropriately supplied throughout the book, and sufficient detail is provided on topics that are the main contexts for caregiver-child interaction, such as daily activity routines (pp. 37-43).

Following a clear exposition of Kaluli notions of child development (some, but not all, of which Kaluli themselves articulate), Schieffelin effectively organizes her material in terms of three "structural themes" in Kaluli society: autonomy versus interdependence; authority; and gender and reciprocity. In this nonhierarchical society, the first two "themes" represent problematic dimensions of social interaction, reflecting "a central tension in Kaluli social life: the importance of giving and sharing in the face of the desire to keep what one has for oneself" (p. 136), and, in the absence of rigid social stratification, the need to deploy personal sentiments "to negotiate interpersonal boundaries" (p. 241). Moreover, while Kaluli relations between the sexes are said not to be as "antagonistic" as those reported for many New Guinea societies, gender-appropriate behaviors can be distinguished clearly at two levels: in "the social organization of domestic and expressive activities" and "the conventions and preferences within particular social interactions" (p. 205). The author of a conventional anthropological monograph might be satisfied with the systematic demonstration of the salience and embeddedness of these "themes" in everyday social life, and we would be well served in the process. But Schieffelin aspires to, and accomplishes, more in showing us how Kaluli children learn and gradually manifest the "themes" in their behavior and speech.

By the age of three, Kaluli children "are expected to participate in the reciprocal sharing that is part of family life" (p. 5), which itself incorpo-

rates and is built around the three “structural themes.” Thus, for caregivers (especially mothers and older siblings) faced with “soft” infants, the “goal of socialization and development is the achievement of ‘hardening,’ the production of well-formed individuals in control of themselves as well as able to control and influence others” (p. 5). Schieffelin shows us how this “hardening” occurs by focusing on four children as they are learning to talk. She draws on eighty-three hours of audio-taped, transcribed, translated, and annotated natural interaction that occurred between caregivers and the children, all of which she was able to contextualize through her own direct observation of the events.

Schieffelin deploys this rich data base by providing numerous detailed examples and analyses in a series of chapters “linked by cultural themes” (p. 10). In chapter 4, *elima* routines—in which a mother provides a child with an appropriate utterance followed by *elima*, “say like this”—illustrate well the Kaluli concern with “showing” language to children so their speech will be “hard.” The subject of chapter 5 is the *ade* relationship, primarily important between elder sisters and younger brothers; through explicit linguistic instruction, a child is taught both to appeal to others by making them “feel sorry” for one and thus give up some desired object (especially food), and how “to respond sympathetically and empathetically to the appeals of others” (p. 112). Chapter 6, “Socializing Reciprocity and Creating Relationships,” focuses on the strategies used by older children and caregivers to socialize younger children in what to expect and from whom, how to ask as well as how not to ask, and how to share or refuse to do so. The development of requests, which can be either by appeal or assertion, is the subject of chapter 7. Chapter 8 deals with the socialization of gender-appropriate behaviors and, since *context* is crucial to defining these, how social identities are socially and culturally constructed. Schieffelin’s judicious use of detailed examples of speech interaction gives all these issues an immediacy and a credibility that are seldom achieved by other expository methods.

While masterful, Schieffelin’s study has its limitations and grounds for further questions. Her sample of children (four) was admittedly small, although one could hardly cavil at the masses of data obtained nevertheless, and a larger sample might prove overwhelming to any analyst; moreover, one must acknowledge the small size of the community itself, with demography imposing its own limits on such a study. On the positive side, the smallness of the community and sample made it possible for Schieffelin to achieve the degree of intimate knowledge of the society and culture that is necessary to contextualize properly all of

this talk. On the other hand, the fact that uncontrollable circumstances led to the loss of one-half of her sample before her research was completed (see pp. 53, 64) further reduced the amount and comparability of data available for the four children. More serious, perhaps, is the fact that the loss was of the two girls in the sample. As Schieffelin admits, "relationships between gender and language . . . are extremely subtle" (p. 247), leaving one to wonder if her analyses of the "socialization of gender-appropriate behaviors" (chapter 8) may require more qualification than otherwise appears to be the case.

Against such limitations, however, one must place Schieffelin's obvious sensitivity, insightfulness, and skills as an ethnographer (supplemented with the knowledge available from her co-researchers). Still, one might raise the concern that vexes all good ethnographers, namely, the degree to which our understandings and representations of others faithfully reflect, or at least can accommodate, their own. While Kaluli are perhaps unusually straightforward in some domains, for example, in the *elema* "say it" routines, we are told that they "do not have a verbally explicit set of beliefs concerning the nature and development of the child, nor do they elaborate on the metaphor of 'hardening' . . . when asked about it" (p. 64). Moreover, even with respect to the *elema* routines, it "is hard to determine how conscious anyone is about the socializing functions of these exchanges while they are occurring," although they might be later, on "reflection," inspired by the ethnographer (p. 96). So, as with any good work, a certain amount of Schieffelin's argument must be taken on faith (although she invites us "to consider alternative interpretations" and, to be sure, her "presentation allows that" [p. 34]). In this case, I believe such faith would be well placed.

Anthropologists often tell students that a fieldworker needs to be like a child, learning to understand a culture the way an infant must--by starting "from scratch" with the simplest, most mundane aspects of daily life and, making no assumptions, by asking the most basic questions imaginable. Yet few of us have paid much attention to such learning as it occurs among real children in front of our eyes. Schieffelin has paid a great deal of attention to this "everyday life," and we as well as she have been enriched by such a shift in perspective.

Elinor Ochs, *Culture and Language Development: Language Acquisition and Language Socialization in a Samoan Wage*. Social and Cultural Foundations of Language Series, no. 6. Cambridge: Cambridge University Press, 1988. Pp. 272. US\$49.50 hardcover, \$16.95 paperback.

Reviewed by Jeannette Marie Mageo, University of California at San Diego

Ethnography is multilingual, having the unique capacity to speak between varying domains of experience and in a host of academic tongues. It is for this reason that I can comment on Elinor Ochs's *Culture and Language Development*. Perhaps also for this reason I find Ochs's work richest when it reaches beyond the perimeters of linguistics and touches my own work and that of other Samoan ethnographers. Therefore, in this review I will provide connectives between Ochs's work and other ethnographies on Samoa. Supplying these connectives will also allow me to draw out some larger implications of her study.

Ochs's book opens with a graphic example of how ethnography, by contextualizing ideas, bridges disciplines. In Samoa there is a genre of formal speech and one of informal speech. Sitting in a *fale*, a traditional house, Ochs realizes that her subjects are talking to her in formal speech and thus putting on a performance for her benefit. It occurs to her that she is located at the front and center of the house and that, according to Bradd Shore (1977, 1982), this territory is the "face" of the house. Hence, it is the area in which Samoans feel it incumbent upon them to put on a mask of special politeness. Ochs relocates herself in other areas of the house. To use Goffman's metaphor, she attempts to get backstage. We should then judge her book in its own terms, upon how well it succeeds in transporting the reader to the linguistic backstage of the socialization process.

There are several major areas in which Ochs attempts to get backstage through the analysis of language practices that I shall consider here: the self, social relations, social values, socialization, and gender.

In Samoa the *'aiga*, the extended family group, is the basic social unit, rather than the individual. This communal organization has profound linguistic implications. For example, in Ochs's chapter on clarification she tells us that Samoans prefer "the minimal grasp strategy," in which speakers ask that a remark be repeated, to "the expressed guess strategy," in which the speakers venture a guess at the other's meaning (pp. 133-136, 144, 219). It is not immediately obvious what this dispreference has to do with a communal orientation. However, elsewhere I have argued that Samoans tend to repress and to dissociate subjective experience (Mageo 1989a: 187-190).¹ Because of the resulting distance between subjectivity and social commerce, Samoans lack avenues through which to access the inner life of others. Thus, Samoans believe they cannot know what is going on in another person's mind (Gerber 1975; Mageo 1989a).

Ochs traces the roots of the dissociation of subjectivity in linguistic childrearing routines. In our society we treat infants as subjects in parent/child exchanges by guessing at what infants' personal responses might be and by verbally imputing meaning to their first garbled utterances. In Samoa, however, the language of infancy and early childhood is treated as nonsense and children are encouraged to imitate the speech patterns of their elders (pp. 23-26). By documenting these and other techniques, Ochs shows us how the child is oriented in a sociocentric direction (pp. 23, 25, 142, 164-165).

Samoan styles of employing linguistic practices, such as clarification, have social as well as psychological implications. According to Ochs, in Samoa persons of higher status do not clarify for those of lower status; rather, the lowly clarify for their superiors (pp. 137-138). If those who serve require further information about the dictates of their elders, they learn to acquire it through peers (p. 139). These rules for clarification exchanges are one aspect of the Samoan hierarchy. One does not question authority, even as to what they mean to say. Samoan hierarchy does not simply entail the placing of one individual above another, though. Rather, the Samoan hierarchy is the core of an organism, the group. Those who are lower in status are perceived as the active limbs (pp. 81-85; see also Gerber 1975:49; Mageo 1991a:410). By implication, those "higher up" are associated with the more central and less mobile parts of the body.

Ochs delineates the linguistic dimension of this organic model of the group through her analysis of what she calls the *taapua'i*, or doer/supporter relationship (p. 199). In this relationship all tasks are regarded as collectively undertaken. Although only one person may actually perform the task, others are seen as supporting it and, therefore, as equally responsible for its accomplishment. For example, when one person drives another to a destination the polite exchange that follows is, "Thank you for your good driving," responded to with, "Thank you for your support" (pp. 199-200). Similarly, several ethnographers have noted that family members are held responsible for one another's misdeeds (Mead [1928] 1961:22-25; Shore 1981:197-199; Mageo 1989a:186).

What is perhaps lacking in Ochs's portrayal of the organic model of group life is a sense of the conflict generated by this model. A Samoan comedy skit (*faleaitu*) I once saw, called "Malo People" (Government of the People), illustrates Samoan ambivalence about that allotment of tasks predicated by the Samoan hierarchy.² In this skit, various body parts complain to the stomach that it takes the profit of their labors

without doing any work. Like the stomach, the role of elevated persons in Samoa is to distribute that which is acquired and produced by other members of the body politic. Thus, commoners *tautua*, “support,” their chief, feeding him on Sundays and contributing food, goods, and money when someone in the *aiga* has a *fa'alavelave* (a problem or a ceremonial obligation). However, Samoans are wont to remark, “Too many *fa'alavelave*” (Mageo 1991b). But it is useless to complain to one's stomach, as the body parts in the play quickly discover. After all, stomachs are simply in the nature of things. Thus, this organic model of the group functions as an ideology. Ideologies are by definition self-confirming, but they do not allay the feelings of the working class.

The value that sustains the Samoan hierarchy is *fa'aaloalo*, “respect for status.” Ochs argues that the base word here is *alo*, which in formal speech means “attentiveness” (p. 162). *Fa'aaloalo* is shown by service, that is, by attending upon others (Mageo 1989b:399-401). It follows that, when the child is taught to attend upon and imitate the speech of others, it is learning a linguistic form of *fa'aaloalo*. Ochs finds this capacity for attendance ubiquitous in Samoa. She is amazed by her informants' ability to hear conversation at a distance and to hear simultaneous conversations (p. 47).

But there is a linguistic counterpoint to listening in Samoa, with which Ochs does not concern herself. To carry out commands is to *fa'alogo*; *fa'alogo* means both “to obey” and “to listen” (Mageo 1989b: 399-401). Service is a form of “listening” in the sense that it involves obedience. Those in authority expend a great deal of energy trying to transform their children into attendants; nonetheless, parents are apt to remark that children are incorrigibly *fa'alagogata*, “disobedient,” which literally translates as “hard to make listen.” Children are apt to act like *gutu oso*, “jumping mouths.” Children who do so are likely to be beaten and to be called *tautalaitiiti*, literally “to talk above one's age,” --the most general term for childhood misdemeanor.

Ochs's lack of concern with the more conflictual aspects of Samoan cultural models is compensated for by her interest in the inherent duality of these models. This duality was first brought to light in Shore's work (1977, 1978, 1982). He dubbed as complementary the incorporative type of relation, evident in *taapua'i* interactions and in hierarchical interactions (Shore 1982: 197-220). However, Shore also found competitive relations in Samoa, to which he referred as symmetrical because the participants normally have an approximate status.

Ochs believes that Samoans socialize for this dual ethos by being systematically inconsistent in the demand for respect. Sometimes the child's

tautalaititi behavior is taken as a joke (p. 161). Thus children learn that in certain contexts attendance is called for and in others, expressiveness.

Samoans also reinforce the tendencies that they covertly encourage by classifying them as innate and, by implication, "natural" (Mageo 1989a:191-194). Ochs tells us that Samoans see the child as innately willful and cheeky (p. 161). Inasmuch as Samoans believe that it is natural to be willful, they also extend a social and a conceptual place to those competitive behaviors that are generated by willfulness and thereby foster the tendencies required in symmetrical exchanges.

Perhaps Ochs's major point throughout is that "children develop concepts of a socio-culturally structured universe through their participation in language activities" (p. 14). Samoan children do not learn language habits primarily from instruction, or from being told what to say. Instead, they learn to play at communicating with others, who know the Samoan version of the game and who structure it accordingly. This analysis of the structural features of language socialization resembles that of Nancy Chodorow's, albeit in another domain and another discipline. Chodorow shows us how children are gendered through the structure of early interpersonal relationships (1974); Ochs, how they develop a sense of self and sociality through the structure of early linguistic relations.

Another Samoan ethnographer, Penelope Schoeffel, actually applies Chodorow's structural theory of gender acquisition to the Samoan context (1979). Schoeffel concludes that, because the interpersonal structure of early life is similar for boys and girls in Samoa, status is a more determinative factor than gender in the shaping of adult personality. Similarly, Ochs's data on the linguistics of gender in Samoa indicate status to be more influential than sex in the shaping of language habits. For example, Ochs shows us that in Samoa women express more empathy than men. In the expression of empathy, though, status trumps sex--those lower in status consistently express empathy to their superiors (pp. 180-181).

Shore based his ethnography of Samoa upon the counterpoint between the brother's and the sister's roles (1977, 1982). One cannot but wonder how gender can be the primary grounding for Samoan meanings and yet at the same time of only limited importance in linguistic exchanges. Elsewhere I have suggested that, because of the social orientation of Samoan society, personality is based upon the persona (Mageo 1989a:182-187; Mageo 1989b:410-412). The persona consists of the social roles one plays (Hobbes 1950:33-134; Jung 1966:156-159). Perhaps Samoan gender inheres not in intrinsic features of personality but

rather in extrinsic personae. This would explain how sex roles in Samoa might have import as social symbol, as Shore suggests, yet might also be easily shed when hierarchical exchanges warrant it.

Bodies of data, such as Ochs's, always evoke more questions than they answer. Still, there is no question that Ochs brings us backstage, to the linguistic dressing rooms of socialization and social interaction in Samoa. I have heard a Samoan remark upon the Mead/Freeman controversy that we ethnographers are like the blind men, one of whom based his conclusions about the elephant's form upon an examination of the trunk, the other upon the tail, and so forth, thereby deriving highly contradictory pictures of the subject of their research. Perhaps our models do not fit like transparencies upon one another, but it is comforting to know that when ethnographers do not assume the postures of dispute our disparate sets and kinds of data tend to augment one another's insights, lending them new dimensionality.

NOTES

1. Many ethnographers have shown that the Samoan orientation is away from the personal and internal aspect of the self (Mead [1928] 1961:122-130; Gerber 1975:12-14; Gerber 1985:137; Holmes 1987:127-136; Shore 1982:148).

2. As the title of this skit includes the English term "people" it is likely that the dissatisfaction dramatized within it is associated in Samoan thought with modern and Anglo influences. There is evidence, however, that this dissatisfaction has a long history (Mageo 1991b).

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Ron Brunton, *The Abandoned Narcotic: Kava and Cultural Instability in Melanesia*. Cambridge: Cambridge University Press, 1989. Pp. viii, 219, illus., bibliography, index. US\$39.50 cloth.

Reviewed by Mac Marshall, University of Iowa

Fifteen years ago I noted that anthropologists working in Oceania had given relatively little attention to either alcoholic beverages or kava in the post-World War II period (M. Marshall 1976). Happily, in the years since that literature review appeared this situation has changed. Much has been written on alcohol use, and more recently various other drugs --including kava--have been the focus of research (for example, Lindstrom 1987; Prescott and McCall 1989). Now, with publication of a revised version of Brunton's doctoral dissertation, contemporary kava studies have come into their own. Brunton has done for kava what anthropologists working elsewhere in the world have done for qat (Kennedy 1987), marijuana (Rubin 1975; Rubin and Comitas 1976), tradi-

tional tobacco use (Wilbert 1987), peyote (Aberle 1982; LaBarre 1959), and a variety of hallucinogens (de Rios 1972; Furst 1976).

Brunton was led to his investigation by the intrigue of an unsatisfactorily resolved puzzle originally posed in 1914 by W. H. R. Rivers in *The History of Melanesian Society* (Cambridge). Seeking to explain the scattered and discontinuous distribution of kava in Melanesia, Rivers observed that its use seemed to be mutually exclusive with the use of betel. From this he suggested that the two drugs had been brought to the islands by two separate immigrant groups (the “kava people” and the “betel people”), that the kava people arrived first, and that wherever these two peoples came into contact betel use gradually supplanted kava use. Rivers believed this happened because betel ingredients were easier to obtain, easier to carry about, and simpler to prepare than kava ingredients. (A problem with Rivers’s idea, not noted by Brunton, is that studies from many parts of the world suggest that drug use is much more likely to be additive than substitutive.)

Brunton acknowledges various problems with Rivers’s speculations on Oceanic culture history, but feels that “he raised important questions about the cultural similarity of widely separate areas of the Pacific, and he showed considerable sensitivity to the dynamics of leadership and change in Melanesian societies” (p. 3). Given kava’s strange geographical distribution in Oceania, and given the huge distances between the different kava-drinking regions of the Pacific, Rivers inferred that its use had once been much more widespread. As noted above, his solution to this puzzle was to suggest that in Melanesia its use was given up in favor of betel. Brunton finds this solution problematic for various reasons, and offers instead his own answer to the kava puzzle.

The book is both a reappraisal of Rivers’s argument, using a wealth of data that have accumulated over the past seventy-five years, and a detailed case study of kava use on Tanna, Vanuatu, based largely on Brunton’s own field research. The case study is joined to the reappraisal via an assumption that particular events that have occurred on Tanna since European contact are probably representative of similar events that occurred in many other parts of Melanesia. The disappearance and reappearance of kava-drinking on Tanna in the historical period becomes a general model for its disappearance from wide areas of Melanesia in precontact times.

To set the stage for his reconsideration, Brunton begins with a six-page chapter summarizing Rivers’s ideas on kava and briefly discussing its psychoactive properties. A chapter follows whose nineteen pages provide the most thorough discussion to date of the traditional and con-

temporary geographical distribution of kava-drinking. In assembling this material Brunton draws on both the published record and on an extensive correspondence he initiated with numerous Pacific scholars during the 1980s. In the next two chapters (sixty-seven pages in all) Brunton impressively pulls together the evidence from archaeology, botany, ethnology, and linguistics to evaluate his hypothesis that kava once was used in many parts of Melanesia that abandoned its use prior to the arrival of Europeans. Here he dismisses the likelihood of the independent discovery of kava in several different regions and the probability of direct links between kava-drinking regions. Instead he concludes that the evidence provides "very strong grounds" for accepting the likelihood that "the links between at least some of the known kava-using regions were indirect," that is, that kava-drinkers who abandoned the practice sometime before European contact once existed in the intermediary areas separating the kava-using regions in Melanesia (p. 80). Also in this chapter Brunton presents the "not inconsiderable" arguments for thinking that kava-drinking may have originated in the Bismarck Archipelago. This will be of interest to a range of Pacific scholars since, as Brunton notes, "there are strong reasons for thinking that the Bismarck Archipelago [also] was the Proto-Oceanic homeland" (p. 81).

In the next three chapters, Brunton abandons general considerations about kava to focus on the specifics of kava use on Tanna. He devotes eighteen pages to traditional kava ritual and its contemporary modifications, fourteen pages to the development of secular patterns of kava consumption, and a somewhat extended (thirty-eight pages) discussion to what he calls "problems of Tannese society." This chapter is crucial to his argument since his "overall intention is to point to social processes which were shared with other Melanesian societies" (p. 129). The "problems and processes" to which he refers are discussed under the headings of traditional social organization; traditional political hierarchy; fighting and social order; and religious volatility, power, and taboo.

In his concluding chapter (ten pages), Brunton holds that "the ritual and religious significance of kava made it vulnerable to disappearance, because of the religious instability of many Melanesian societies" (pp. 168-169). This, coupled with divisiveness, distrust, and ineffective institutions of leadership and social coordination led many Melanesians --like the Tannese--to reject "their current cultural 'package' in favour of another" (p. 169). Brunton endeavors to show that the problems and processes characteristic of Tanna were endemic to much of Melanesia. These include weak institutions of authority, suspicion, individual

autonomy, and a recourse to external religious and ritual powers. In the face of adversity, Brunton believes that the mechanisms that might have protected Melanesians' "current interpretive systems" of religious powers frequently failed, and people feared the diminishment and dissipation of the particular potency of their system. "This opens the way for the enthusiastic adoption of a new package--or a return . . . to a previous one that had been prematurely rejected--that can protect them from the dangers of the one they are abandoning" (p. 166). Thus did kava disappear from wide areas where it formerly had been drunk.

What are we to make of all of this? Brunton's major contribution is that he has greatly clarified and expanded our knowledge of kava's geographical distribution, its botanical diversity and fragility, and its cultural patterns of use. He is less convincing when he tries to generalize from the Tannese case to much of the rest of Melanesia. For one thing, Melanesian societies are considerably more diverse than he seems to grant. But more importantly, I believe he gets chewed up and spat out by his own argument.

Over time various groups of Tannese abandoned kava use in favor of some other source of power--usually one or the other brand of Christianity. So far, so good. The problem is that over time various groups of Tannese also abandoned Christianity and resumed using kava (for example, members of the John Frum movement; see also p. 122). Why, if kava was both "the abandoned narcotic" and "the recovered narcotic" on Tanna, was it not also thus elsewhere in Melanesia--in the historical period as well as in precontact times? I find it hard to believe, for example, that if kava had been used throughout the Solomon Islands at some point in the past, and if Brunton's "problems and processes" apply to most Melanesian societies, that kava would not have been readopted by at least *one* Solomon Island society over the past 150 years.

Brunton documents (tables 1-7) that the names in different Pacific languages for various psychoactive plants (kava, *Piper betle*, *Areca* palm fruit, ginger) are often cognates. This interesting finding may reflect a general linguistic association among pharmacologically active plants that produce behavioral or physiological changes. For example, it may be relevant in this regard that "*kava*" is the Hiri Motu word for "mad, insane, stupid" (Dutton and Voorhoeve 1974:197).

The first half of the book contains fourteen maps, which, though useful, are wanting in certain respects. For instance, in map 1, Samoa--a major kava-consuming area--is conspicuous by its absence, and map 4 lacks two islands mentioned in the text: Paama is not shown and Pentecost is labeled "Raga." Use of the same dark pattern in map 4 to illus-

trate areas where kava was *not* drunk as is used in maps 2 and 3 to show areas where kava-drinking *did* occur is also confusing.

Finally, there are a few other problems. Brunton twice refers to a western Polynesian myth in which kava is said to have grown originally from the body of a leper (pp. 21, 68). It is difficult to consider this a myth of much antiquity when one learns that leprosy was a nineteenth-century introduction to the Pacific Islands from Asia (L. Marshall n.d.). In this case Brunton should have heeded Denning's caution about accepting myths and legends as reflective of a precontact world (1966: 32). Brunton states that kava's soporific effects appear to help explain its association with peaceful relations (p. 70). There are difficulties with this assumption. Beverage alcohol is a central nervous system depressant, and by the same logic it, too, should be associated with peaceful relations. Adding to the dilemma is Schwimmer's observation that the Orokaiva use betel to create equanimity in potentially tense social situations (1982:322-323), yet the arecaidine in the betel chew is primarily a central nervous system stimulant (M. Marshall 1987: 17).

On balance, this valuable book consolidates the literature on kava as none has done before, raises numerous questions for further scholarly investigation, and presents a plausible alternative hypothesis to Rivers's less-than-compelling explanation. Although it was not his stated intention, Brunton has performed a valuable service to alcohol and drug studies by making accessible material on kava that heretofore has been scattered and difficult to locate.

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Reviewed by John Charlot, University of Hawai'i at Mānoa

Malcolm Naea Chun has been accomplishing important work in the most difficult and exacting areas of Hawaiian studies: the discovery of materials and the publication and translation of Hawaiian-language texts. For instance, besides the books under review, *I ka Wa o Kamehameha* (Kamakau 1988) makes available five essays by the important nineteenth-century writer Samuel M. Kamakau in both the original Hawaiian and English translation, along with a biographical sketch and a list of Kamakau's newspaper articles not already published in a bibliography. Such work provides a valuable basis for further research.

No single Hawaiian-language work has been more influential than David Malo's *Ka Moolelo Hawaii*, written in the 1840s, circulated widely in manuscript copies, and translated with notes by Nathaniel B. Emerson (Malo 1951), but never before published in the original Hawaiian. Born in 1795 and attached to a high chiefly court, Malo received a classical Hawaiian education from at least one of the encyclopedically learned men of the time. In his book, Malo preserved not only a vast amount of detailed information about Hawaiian culture, but formulated it in the classical educational genres, such as vocabulary groups and lists. His work was highly appreciated by other Hawaiians. Writing probably in the 1880s, Bicknell (n.d.:3) repeats what was perhaps only a rumor about Kaliikaua:

The King, it is reported, is striving to bring the system of fetich worship into a concise form of which he shall be the acknowledged head. In the palace is a small room the only furniture in which is a table with a book lying upon it. The book is David Malo's history of Hawaiian traditions and legends, which after his death came into his daughter's possession; the King obtained it through her husband, John Kapena.

Usually, before reading, a circuit of the table is made seven times, after which the book is opened with a show of reverence, and then the credulous owner of the sanctum holds converse, in imagination, with the gods and demi-gods. This book is the basis of the present Hale Naua.

Malo's book was used extensively by other nineteenth-century Hawaiian writers. Dorothy Barrère goes so far as to call it the "skeleton upon which Kamakau and I'i put clothes" (Finney et al. 1978:312). Malo's book continues to be a prime source for scholars and students today,

with the important difference that most are able to read it only in translation.

Emerson's translation is a valuable document in itself, based on his extensive knowledge of Hawaiian language and culture and his wide acquaintance with knowledgeable Hawaiians of his day. Moreover, he was able to add considerable material from other sources in his notes. No translation, however, is a substitute for the original, and the use of Emerson without reference to Malo's Hawaiian text is improper. For instance, the claimed basis in Malo of some widespread opinions--that commoners did not keep genealogies and that women were less religious than men, among others--can be challenged from the original text. Moreover, passages that bear on current discussions can be overlooked because of mistranslation, as I have shown for the subject of women as the creators of feather ornaments (Charlot 1991:146, n. 11).

Malo's form is even more vulnerable to problems of translation than his content. For instance, he often uses the classical educational form of lists composed very strictly and regularly for ease of memorization. This use of form is sometimes perceptible in Emerson's translation (for example, Malo 1951:45ff.; Malo n.d.: ch. XV, sects. 5-20); in other cases, it is completely concealed (such as Malo 1951:44; Malo n.d.: ch. XIV, sects. 15-17). The loss is major: content is distorted, the original memorized forms are hidden, and Malo's own style and use of traditional materials is obscured. The basic impression made on the reader by the original text is lost.

The Hawaiian text of Malo's book, in view of its intrinsic importance and extensive influence both earlier and today, clearly requires a scholarly edition based on all available manuscripts and provided with a critical apparatus. A close translation should be done along with a detailed commentary using all of Malo's writings, other nineteenth-century sources, and the work of Emerson and others. Only such a full treatment can adequately define Malo's thinking and style, separate the materials he received from his own additions and views, and evaluate his writings as sources. Such a treatment would also make possible an evaluation of Malo's writings as influences on the history of Hawaiian thought and, usually through Emerson's translation, on modern scholarly and popular descriptions of Hawaiian culture.

Chun's edition, *Ka Mo'olelo Hawaii (Hawaiian Antiquities)*, is an important first step towards such a goal: a transcription of the manuscript that he considers the best available (pp. xxi-xxv). The handwriting of the manuscript is difficult, and Chun's transcription provides a useful reading copy. For exact scholarly work, however, the original

manuscript must still be consulted because the transcription contains a number of errors including misreadings, many of which affect the sense,¹ and omissions.²

There are also unmarked editorial changes and inconsistencies. Connections between words are irregular in the manuscript and are changed irregularly in the transcription. The punctuation of the manuscript is followed more regularly but sometimes omitted. Capitals are irregularly changed. Arabic instead of roman numerals are used for the chapter numbers, and added for the first. Chun follows Emerson's correction of the numbering of the manuscript: from XXXVIII 56, 56 [bis], 57, 58, 59, 61 to 56, 57, 58, 59, 60, 61. Chun has moved Malo's table of contents from the back to the front of the book but has not adjusted his translation of the title: "Contents of the Foregoing." Editorial brackets are used for several purposes--e.g., for Malo's own superscript additions to the texts (XVIII 51, XXII 2) and for Chun's additions to or corrections of the text (II 11, XVII 8, XVIII 50, XXI 3, XXII 1)--so the reader is not always sure who is writing what. Furthermore, brackets are used regularly to change *lii* to [a]lii and *kua* to [a]kua, but both are recognized, respectively, as a short form and a variant (Pukui and Elbert 1986: *li'i* 2, *kua* 6).

That errors have escaped such a careful worker as Chun demonstrates the imperative need for teamwork on Hawaiian language texts, especially in view of the increasing activity in publishing educational materials. A single person cannot adequately proofread a text in Hawaiian or any other language, which--in view of the shortage of people with the requisite knowledge, training, and experience--largely explains the problems with most publications of Polynesian texts, including my own.

Chun provides a useful introduction and biographical sketch of Malo along with a checklist of articles by and about him (this difference is not indicated). Further articles by Malo can be added to this list, such as Malo 1843a, 1843b, and 1844. The location of the manuscript "He buke no ka oihana kula" is not provided (p. xviii).

Chun's *Hawaiian Medicine Book, He Buke Laau Lapaau* is an example of the wide current interest in Hawaiian medicine, a subject of intrinsic interest and an area in which Hawaiian expertise was arguably superior to its contemporary Western counterpart. Hawaiian medicine was closely connected to Hawaiian culture, worldviews, and religious and other practices, and was transmitted in a variety of literary forms, such as chants, stories, genealogies, descriptions, instructions, and case reports. *Hawaiian Medicine Book* is therefore a valuable source for a number of fields.

Chun has done extensive research in Hawaiian medicine and related areas, such as plants. This background is obviously useful in his translation of the often extraordinarily difficult and concise Hawaiian text. Among my very few disagreements, I mention Chun's translation of *kiai kukui iuka* as "guards the *kukui* in the uplands" (p. 45, n. 15); I would choose the alternative possibility, "the *kukui* in the uplands guards," based on the pattern in the *Kumulipo* (Beckwith 1972:188, 1. 36 and parallels). Some of Chun's translations are summarizing or explanatory rather than close (e.g., p. 32, par. 7). The translation on pages 45-46 does not follow the format of the original on page 11. Page 3, paragraph 1 (*Hele ia, aole . . . mamuli o ke ola*) has not been translated. The Hawaiian text for the English translation on page 59 ("Ka. This is . . .") through page 60 is missing from page 19. Chapter headings seem to be added (as well as the explanation in parentheses on p. 63, par. 2), but I have not checked Chun's transcription against the original newspaper articles, and he does not provide full bibliographical information on them.

Finally, the design of the book--with its distribution of materials and use of the same typeface and size--does not enable the reader to tell at a glance whether he is reading the translation or introductory or explanatory material.

Again, many of the above problems could be solved by trained publishing teams. Chun deserves every credit for being a pioneer in work that he amply demonstrates is important.

NOTES

1. Examples of misreadings: I 3, Chun *hookuka* instead of manuscript *hookuke*; II 9, *naau* instead of *naauao*; V 16, *papamu* instead of *papanui*; VI 4, *hookokoli* instead of *hookokohi*; VI 7, *hamu* instead of *mau*; XIII 8, *iwa* instead of *inoa*; XIII 17, *inoa* instead of *moa* (the pen slipped, but the sense is clear from the context); XIV 2, *ono* instead of *ano*; XV 18, *i ano* instead of *ia ono*; XVII 6, *hoi* instead of *koi*; XVIII 3, *huna* instead of *hewa* (difficult to read but clear in the context); XVIII 18, *ila uuku* instead of *ila muku* (for *ilamuku*); XVIII 30, *kioloa* instead of *kialoa*; XIX 32, *kalo* instead of *lako*; XXXII 1, *hana* instead of *kane*; XXXVI 16, *apu* instead of *pa u*; and XXXVII 89, *kcili* instead of *kuili* (for the name of a ceremony).

2. Omissions: II 11, missing after *a he pele: no ma na moku a pau ma keia moana, o na pohaku a pau, he pohaku*; IV 5, missing after *Kuaihelani: na aina ma na pule, o Uliuli*; XVIII 74, missing after *akamai: loa, ua kapaia lakou he mai au, he poe akamai*; and XXXI, sect. 14 is missing.

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John Wilson, ed., *From the Beginning: The Archaeology of the Maori*.

Auckland: Penguin Books in association with the New Zealand Historic Places Trust, 1987. Pp. 175, illus., maps, appendix, bibliography, index. NZ\$32.50.

Reviewed by Ian G. Barber, University of Otago, Dunedin, New Zealand

From the Beginning is a landmark work for New Zealand archaeology. In a popular and attractive format, with clear text and color as well as black and white photographs and illustrations, editor Wilson has assembled a series of essays detailing the principal results of recent archaeological research into the New Zealand Maori past. The introduction sets out clearly the book's objective: to simplify archaeological results so as

to inform “those who are not familiar with what archaeologists do or what they have learned” about the Maori past (p. 9). The essays retain the scholarly trappings of footnotes and suggestions for further reading.

For the purposes of this review, I shall consider the individual essays and their contribution to the books stated objective under several broad themes of my own choosing.

As *context and background*, Tipene O’Regan’s introductory essay, “Te Kupenga o nga Tupuna” (The Net of Ancestry), discusses the pre-European past from the perspective of Maori tradition and contemporary cultural identity. For O’Regan, the record of the past is made relevant to Maori society today through the expression of *whakapapa* (genealogy), which relates individuals to each other, to their wider community and physical environment, and to other autonomous but (ultimately) related groups. In introducing the reader first to a contemporary Maori view of the past, the archaeological essays that follow are placed in perspective as addressing matters of importance to “a people whose culture lives on” (preface, p. 7).

O’Regan’s concluding essay, “Who Owns the Past?: Changes in Maori Perceptions of the Past,” deals with the Maori response to Pakeha (non-Maori) scholarship. Asserting a long-standing Maori resentment towards, and alienation from, the institutional scholars of Maori life, as well as a growing sense of tenure over things Maori, O’Regan also concedes that Maori perceptions of the past have been extended favorably where Pakeha scholars have communicated effectively. O’Regan reminds the reader that myth, custom, and, most importantly, *whakapapa* remain the essential Maori links to their own past, constituting areas where the outsider proceeds at “peril” (p. 145). The caution is salutary in reinforcing both the limitations of archaeological scholarship and the too-often overlooked perspective of those who are ultimately the subject of enquiry in New Zealand’s pre-European archaeology. I shall return to this last point later.

Janet Davidson’s essay “Origins of the Maori” provides the archaeological background from Oceania. The expected discussion of the “Lapita people” as Polynesian progenitors is relatively orthodox although Davidson acknowledges that, as the subject of “intense investigation” at present, new light may yet be shed on such matters (p. 36). For east Polynesia, Davidson gives Kirch’s 1986 reevaluation of the earliest settlement evidence credence against the more usually cited A.D. 300 date for first colonization (pp. 31, 33). On the vexed question of initial Polynesian settlement of New Zealand, Davidson allows the likelihood of first arrival “several hundred years” before A.D. 1100 while ac-

knowledging a date earlier than A.D. 600 to be problematic (pp. 35-36), a compromise position unlikely to satisfy any of the antagonists in the current debate.

Under a theme of *ecology and subsistence economy*, I include first Bruce McFadgen's "Environmental Change." McFadgen is insightful and original in synthesizing evidence for the environmental impact of the Maori over time, especially deforestation and depletion of food resources. Phil Houghton's challenging if brief essay, "Health and Well-Being" (with authorship incorrectly ascribed in the table of contents to Davidson), describes the paradox of a tall, robust, and relatively healthy pre-European Maori population for whom longevity was generally proscribed by diet and environment.

Specifics of this diet are provided by Atholl Anderson and Helen Leach. In "Hunting and Fishing," Anderson continues McFadgen's argument in demonstrating that a greater reliance on finfish and shellfish over time can be attributed to the scarcity (and eventual extinction) of moa and, especially, seal. Anderson corrects a popular perception that moa was a "mainstay" of the early Maori diet, though acknowledging the significant dietary contribution of these large birds in certain districts. The dietary contribution of the domestic dog is also discussed. Overall, Anderson provides a comprehensive summary of foods fished and hunted, omitting only the introduced (and hunted) Polynesian rat.

In one of the book's most detailed essays, "Gathering and Gardening," Leach amplifies the dietary perspective by documenting the vegetable foods gathered and cultivated by the pre-European Maori. Leach notes the relative success of gardening in some warmer, northern locations, but stipulates that the seasonal climate of temperate New Zealand forced the Maori to put much time and effort into processing wild plants for food as well, especially in the south.

Cumulatively, these four essays clearly communicate the unique challenges that faced tropical Polynesian settlers in temperate New Zealand and the relative success of Maori subsistence adaptation.

Janet Davidson's essay "Cultural Change" may be considered under the theme of *culture change and material culture*. Davidson proposes a three-period archaeological sequence for the Maori past ("settlement," "expansion," and "rapid change"), a model that is discussed along with Maori scholar Sid Mead's stylistic/art-history chronology of change. Davidson acknowledges earlier theories that attributed change to the arrival of new migratory groups, including the variant of a single North Island locality where Maori culture purportedly developed and from whence it (rather suddenly) spread. In a challenging counterinterpreta-

tion to this last view, Davidson suggests instead that “it is just as likely that changes took place in different regions at different times,” spreading in a manner that was “gradual and complex” (p. 44). Davidson’s model introduces the general reader to more useful categories of Maori culture change than earlier two-period sequences have allowed. In common with the earlier scenarios, she acknowledges the importance of environmental and demographic influences but also highlights the need to consider regional factors, both in the timing and very nature of pre-European culture change itself.

Kevin Jones considers the skill base and variety of Maori material culture in “Maori Technologies.” Much illuminating detail and great space is given to adze manufacture, a situation influenced by both the durability of stone in archaeological contexts and the concentration of previous research. Descriptions of wood, fibre, and (very briefly) bone technologies are also provided.

Wendy Harsant’s “Arts of the Maori” deals with material culture from the perspective of (archaeological) art history. Harsant documents the richness of ornamentation, variety, and skill in archaeological examples of wood carving, rock art, necklaces, pendants, amulets, and the fibre arts. In this regard some comparative, regional aspects of change are elucidated. However, the paucity of material objects interpreted as Maori art from securely dated archaeological contexts means that, at best, only broad generalizations can be made about diachronic development and change in art forms. Harsant does at least place Maori art in context as a unique development from earlier Polynesian precedents.

From a *settlement-pattern* perspective, Nigel Prickett’s “Houses and Settlements” considers the physical evidence of settlement and domestic sites in particular. This, at least, is one area of cultural adaptation where the archaeological and early historical evidence facilitates a useful level of reconstruction. Prickett’s essay generally accomplishes this, emphasizing the persistence of both larger settlement and (especially) individual house forms over time, and even space. Prickett discusses and compares the later *pa* (fortification) sites, Maori archaeology’s most impressive landscape form, as defended settlements, though he does not deal explicitly with the challenge these sites pose to a scenario of settlement-pattern continuity.

In “Warfare and Fortifications,” Janet Davidson takes up this last point. The “cycle” of *pa* building, she observes, began about five hundred years ago, and then spread rapidly throughout both the North Island and the northern South Island. Before that time, “unfortified villages and hamlets, usually on coastal flats,” were the dominant settle-

ment pattern (p. 109). On the basis of other archaeological indicators, however, Davidson observes that this earlier settlement pattern should not necessarily be construed as negative evidence for a lack of aggression (pp. 109, 120). This is a sensible conclusion that highlights the difficulty in explaining the later proliferation of defensive earthworks, as does her critical review of theories for "*pa* warfare" (p. 111). In some instances, this last term may even be something of a misnomer in my opinion. Just as weapons could serve as symbols of prowess (p. 120), *pa* may sometimes have had a primarily symbolic, territorial function, as much linked to increased competition for status and prestige as to specific patterns or incidents of Maori warfare.

Under a final theme of *archaeological legislation and the general public*, Brian Sheppard's appendix, "Protection and Management of Archaeological Sites," is an excellent summary of relevant legislative, ethical, and practical site management issues. The confusion since engendered by the creation of the government Department of Conservation and the legislative review of the Historic Places Act means that some of his discussion is dated already, however. As one might expect, this essay from Historic Places Trust employee Sheppard argues an official, management perspective.

This last observation leads to an issue I wish to consider in penultimate conclusion. In a review of this book published outside of New Zealand, O'Regan's concluding essay is characterized as overtly political, Marxist rhetoric, ultimately about Maori nationalism, which "entirely alters the balance of the book" and is in "fundamental conflict" with its aims (Shawcross 1989:80, 81). Although it is not my intention to review someone else's review, a response to this possible interpretation is deemed appropriate, relating as it does to fundamental issues concerning the book and its New Zealand context.

Certainly, from the prefatory remarks of Historic Places Trust chairperson Dinah Holman (p. 7) and editor Wilson's introductory remarks (p. 12), there is no sense that O'Regan's chapters are out of harmony with the book's overall intent. Wilson introduces O'Regan's essays in a discussion of the relation between archaeological and traditional Maori views of the past (pp. 11-12). Archaeology and tradition illuminate different aspects of that past, Wilson notes, and Maori people act "of right" in requiring consent for any archaeological investigation of the same. Archaeologists proceeding from a position of respect recognize "that the past they are helping to piece together belongs in a special sense to the Maori people," while many Maori now recognize that archaeological findings (*per se*) "do not infringe on Maori ownership"

or “distinctively Maori uses of that past.” These last are “the uses described by Tipene O’Regan,” Wilson concludes (p. 12).

O’Regan’s essays describe those uses explicitly and, as no Pakeha archaeologist could convey, the resentment and protest that has resulted from the co-option of the Maori past by Pakeha institutions, frequently without consent. In that context, O’Regan’s observation that where Pakeha scholars are prepared to dialogue with the Maori, “there is, happily, another side to all this,” becomes all the more meaningful to the general reader. This is certainly asking that archaeologists do more than “become good mannered towards the Maori” (Shawcross 1989:81), but this is entirely in harmony with Wilson’s previously cited introductory remarks (see also Sheppard’s comments, p. 149) and the contemporary requirements of successful Maori archaeology in New Zealand. The issues O’Regan raises of indigenous consent, communication, and respect are now being defined as crucial to the future discipline of archaeology in a number of countries (Gathercole and Lowenthal 1990). Incorporating such an indigenous perspective is no more partisan (or inappropriate) than the justification of the official statutory perspective by Sheppard. In New Zealand today, the exclusion of either perspective from a text such as *From the Beginning* would be as political an act as inclusion.

Overall, *From the Beginning* accomplishes admirably what it sets out to achieve. For undergraduate students and the general reader, there has never before been such a user-friendly introduction to the findings, scope, and limitations of the archaeology of the Maori. It is an example that could (and should) serve as precedent for archaeology and archaeologists in other Pacific and Pacific rim countries.

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Peter Tremewan, *French Akaroa: An Attempt to Colonise Southern New Zealand*. Christchurch: University of Canterbury Press, 1990. Pp. xx, 383, illus., maps, appendixes, bibliography, index. NZ\$29.95.

Reviewed by Alan Clark, University of Canterbury

“The story of Akaroa is the story of grand ambitions brought to nothing” (p. xvii). It is doubtful that French ambitions were in fact as grand as Peter Tremewan claims--their realization was too shambolic and frequently pathetic--but brought to nothing they surely were, and quickly too. This minor episode in the colonization of the Pacific spans the handful of years from August 1838 (when the young French whaler Langlois purchased Banks Peninsula from only some of the local Maori for 1,000 francs) until April 1845 (when the last official link between Akaroa and the financially stricken Nanto-Bordelaise Company was broken). From this point, “the French settlers were on their own in a British colony” (p. 296).

French intentions to establish a settlement and eventually colonize New Zealand’s South Island by means of a commercially based joint-venture (the Paris authorities operating through the French navy in conjunction with private business interests) were doomed from the outset. In classic fashion the French arrived too late: British Governor Hobson’s declaration had extended British sovereignty over the South Island in May 1840, a matter of weeks before either of the two French ships--the *Aube*, under the lucidly pessimistic Lavaud, and the *Comte de Paris*, under Langlois--had reached Akaroa. The French state’s level of commitment had been reflected in the modest scale and mediocre quality of the venture: the original settlers--just fifty-nine of them--were mainly poor peasants and workers, desperate to flee grinding poverty in France rather than positively aspiring to a new life in New Zealand. In these circumstances and with these means at their disposal, French commercial and governmental objectives stood no chance. Indeed, the potential for disaster was considerable. In any event, France-British relations in the first half of the 1840s concerning Akaroa, and the South Island as a whole, were characterized by cordial if understandably cautious diplomacy and accommodation. Relations between French and Maori on Banks Peninsula were considerably less satisfactory; muddled agreements over land purchase (and repurchase . . .) were the rule rather than the exception.

The book’s second half offers the classic account of a young colonial settlement: land distribution and clearing, building and communications, subsistence farming, missionary and scientific activity. Small achievements, many trials and tribulations: it was anything but a glorious imperial rampage. In the protracted initial stages the colonists were entirely dependent on the sponsoring Nanto-Bordelaise Company for

food, capital, equipment, many skills, and security. The fledgling Akaroa economy found itself persistently hampered by the absence of a significant market to which to direct the settlers' produce. By the end of the 1840s, what productive dynamism had developed had petered out.

Before the close of 1841, Paris had effectively recognized the priority of British claims to the whole of New Zealand, abandoned whatever aspirations it had entertained for a foothold in the country, and was looking with more determination towards the Marquesas—which, indeed, became French a matter of months later, in mid-1842. By the end of the following year, the French settlers in Akaroa had been outnumbered by the British. In demographic terms “French” Akaroa had survived a bare three years.

This study is a model of sound documentation; its reflection of original French sources is a particular strength. As befits its academic publishing origins, the volume is equipped to a high standard. Three appendixes list European and Maori populations of Akaroa in the 1840s and shipping movements there in 1841. The source notes and bibliography together run to just short of fifty pages. The index contains comprehensive, rather than exhaustive, coverage of names; a thematic index would have been welcome. The text is both agreeably and informatively illustrated, although some of the watercolors presented have paled on reproduction to indecipherable insignificance.

It is difficult to imagine anyone—however subregionally minded—wanting to know more about Akaroa's humble French beginnings. It might indeed be held that *French Akaroa* sins by providing excessive detail to the relative neglect of narrative flow and a broader interpretative perspective. Those who so argue would scarcely contest that Tremenwan's monograph will long serve as the reference text on its subject.

David Robie, *Blood on Their Banner: Nationalist Struggles in the South Pacific*. London: Zed Books; Sydney: Pluto Press, 1989. Pp. 313, bibliography, index, notes. A\$19.95 paperback.

Reviewed by John Connell, University of Sydney

The title and cover of this book proclaim its stance: “The South Pacific is no longer pacific. Nationalist struggles against colonialism, indigenous claims for sovereignty and superpower rivalry have turned it into a zone of growing tension. . . . The policies of France, Indonesia and the United States pose the greatest threat to the stability of the region.”

After more than a decade of committed journalism in the region, David Robie has drawn together here accounts of various recent problems and struggles. The most substantial part of the book deals with the struggle for Kanaky; there is also a briefer account of nuclear testing in Moruroa and the opposition to the French presence there. Indonesian repression is discussed in a chapter on Irian Jaya and East Timor, while the United States' involvement in Palau completes the trilogy of threats. By contrast Vanuatu appears intermittently and prominently as the lone Pacific nation that has taken "a remarkably courageous independent stance for an economically vulnerable nation" in its championship of a nuclear-free and independent Pacific.

Robie is at his best, as a good journalist, on the front line, notably in New Caledonia. By contrast the historical analysis is relatively thin, entirely from secondary sources and descriptive rather than analytical. A significant part of the bibliography is unused; Robie lists Hempenstall and Rutherford's excellent *Protest and Dissent in the Colonial Pacific* (Suva, 1984), but he makes no use of it, and appears unaware of Mamak and Ali's *Race, Class, and Rebellion in the South Pacific* (Sydney, 1979). Both provide wider perspectives that would have enabled contemporary struggles to be seen as something more complex than merely conflicts with colonialism and neocolonialism. The range of examples of crisis and violence discussed by Robie cannot simply be attributed to variants of these twin peaks of oppression.

In the accounts of Palau and New Caledonia especially, there is little attempt to understand the manner in which traditional divisions (based on ethnicity, language, or social structure) were related to accommodations with colonialism. In the case of New Caledonia, the most prominent Melanesian opponent of the struggle for independence, Dick Ukeiwe, is quoted as being a "lackey Kanak" and a "puppet" (pp. 96, 138), but there is no attempt to explain why around 20 percent of all Melanesians supported his position. For Palau, no mention is made of a social organization oriented around principles of opposition. Indeed, it is only in Fiji that divisions among Pacific islanders really surface since it is in Fiji that nationalism has apparently departed from the preferred script. But "nationalism" is here an undefined and elusive term, which manages to incorporate both the Fiji Labour Party and the Taukei Movement as variants of the search for domestic legitimacy.

Consequently, this book presents a relatively simplistic analysis of colonialism (and neocolonialism) in the South Pacific, exemplified in Robie's assertion, "Both France and the United States have refused to surrender even the smallest part of the South Pacific--except under

duress, as in Vanuatu” (p. 17). Yet Vanuatu is exceptional precisely because it is the only Pacific state where violence has accompanied the emergence of an independent nation. In New Caledonia violence occurred because the ballot box was (and remains) no vehicle for independence. Independence was not denied either French Polynesia or New Caledonia; it was denied to the minority who sought it. (This is, however, not to say that France has almost always sought to defer any possibility of independence, as the recent Matignon Accord has so obviously done; attempted to sow the seeds of division within independence movements; or killed and imprisoned prominent supporters of independence.) Similarly, none of its “colonies” has sought independence from the United States; it defies the imagination to envisage an independence movement in American Samoa (as long as migration is possible and a transfer economy in place). The Northern Marianas especially, but also the Marshall Islands and the Federated States of Micronesia, have negotiated long-term and wide-ranging political and economic links with the colonial power and Guam debates a stronger political tie. While colonialism may pose psychological and political problems, it simultaneously generates welfare provision and wages beyond those of most independent island states. In these contexts, and elsewhere, immediate economic issues have triumphed over the more nebulous rewards of confrontational nationalism.

Ultimately Robie argues that the insurrections in New Caledonia, the gangland-style “execution” of the Palau president, and the coup in Fiji mark a “turning point” for South Pacific island states--the loss of “geopolitical innocence” (p. 14). But that loss occurred long ago, when Pacific islands were incorporated into the periphery of the world capitalist system. It may be, as Bruce Knapman has suggested for eastern Fiji, that their size, limited resources, and remoteness often spared the islands “the journey through hell” that occurred in areas closer to the centers of the capitalist world. If this was so in eastern Fiji it was certainly not the case in Banaba or Nauru, Samoa or Pohnpei. Invariably the most rapacious forms of colonialism were firmly opposed (as they were in nineteenth-century New Caledonia) while, in other places, islanders made their own accommodations to the outside world through various forms of dissent and ultimately a degree of acquiescence. In a global economy the issues that now trouble the Pacific island states are often those of incomes, wages, education, and health--based around continued authority over land--rather than the achievement of an independent and nuclear-free Pacific that so beguiles outside observers. This pragmatism, apparent in the Fijian context, alongside wider

acquiescence to the neocolonial world--especially in the cultural arena --is lacking here. As recent events in Bougainville demonstrate, however, the South Pacific is too complex for easy generalization.

There are troubling features of this book. The large number of typographical errors and spelling mistakes should have been corrected; so too the sloppy footnotes and bibliography. But these are mere quibbles. More serious are the number of quotations that are unattributed and the several instances of apparent plagiarism (for example, see *Islands Business Pacific*, June 1991, 5). This is most unfortunate since many of the quotations that Robie includes from his own work are revealing and useful additions to the documentation on contemporary dissent in the region. In this he has done himself a disservice in what would otherwise have been a useful addition to political journalism on the South Pacific.

Arnold H. Leibowitz, *Defining Status: A Comprehensive Analysis of United States Territorial Relations*. Dordrecht, Boston, London: Martinus Nijhoff Publishers, 1989; distributed by Kluwer Academic Publishers, Hingham, Mass. Pp. xxii, 757, bibliography, index. US\$189.00.

Reviewed by Robert F. Rogers and Dirk A. Ballendorf, University of Guam

This book is without doubt the most thorough and incisive critique of United States federal-territorial relations written to date. In fact, its very comprehensiveness is daunting to those, such as the present reviewers, whose interest is focused on Oceania. Leibowitz's analysis addresses a major and abiding problem of the American polity, namely that the United States now finds itself, somewhat to its own astonishment, as the largest overseas colonial power in the world.

The American insular empire encompasses not only the four "old-line" possessions in the Caribbean (Puerto Rico and the Virgin Islands) and the Pacific (Guam and American Samoa), but also the four nonsovereign--but largely self-governing--island groups in Micronesia: the Northern Marianas, Palau, the Federated States of Micronesia, and the Marshalls. Despite worldwide decolonization elsewhere, these eight island entities remain in a kind of neocolonial limbo in which their final political statuses are still very much in question.

Arnold Leibowitz is exceptionally well qualified to write on U.S. territorial issues. A constitutional lawyer with extensive legal experience in

the Pacific and Caribbean territories on both the U.S. federal and the local territorial sides, he writes with perception and authority. He begins his analysis with a review of the legal basis of federal American authority over its island territories. This authority is founded on the territorial clause of the U.S. Constitution and on the early national experience in transforming frontier areas into U.S. states. Leibowitz summarizes the legal history that established current U.S. territorial doctrine, and he details the separate politico-legal evolutions of all eight U.S. territories.

The *Insular Cases* of 1901, and the U.S.-U.N. trusteeship agreement in 1947 (which established the Trust Territory of the Pacific Islands in Micronesia, excluding Guam), placed all the American-controlled islands of the Pacific except Hawaii outside U.S. constitutional protections. None of these islands was intended by Washington, D.C., to become a U.S. state. None was treated in the same manner as the American frontier territories on the mainland. Consequently there have been considerable ambiguities and numerous anomalies in U.S. policies toward America's Pacific colonies. One strength of Leibowitz's book is his cogent explication of the issues and legal factors in the many arguments between Washington and the territories over policies.

Under the U.N. agreement, the trusteeship islands were to be brought to self-governance. But for decades they remained under a benevolent but inert American paternalism that satisfied U.S. defense interests but not local developmental needs. Even the development of Guam, made a permanent part of the United States in 1950 and its inhabitants given U.S. citizenship, was subordinate to U.S. national security interests well into the 1960s. In the Pacific only American Samoa, Leibowitz notes, was largely satisfied with its status as an unincorporated territory.

Leibowitz describes how and why the peoples of these islands became politically aware and progressively more astute in the 1960s and 1970s, demanding and obtaining more control over their own destinies. As a consequence, by the 1980s American Micronesia had fragmented into five separate entities, each negotiating its own future with Washington. Although American officials professed adherence to anticolonialism, it was largely the Micronesians themselves who took the initiative in bringing about the compacts of free association in the Marshalls and the Federated States of Micronesia, both of which now govern themselves, and in the case of the Northern Marianas, which became an unincorporated U.S. territory under the mantle of a commonwealth.

The gradual liberalization of U.S. territorial policies in the Pacific

did not jeopardize American defense goals as some military officials feared, but liberalization did facilitate enormously Micronesia's political development, except in two cases. One of the exceptions is Palau, which remains locked in the trusteeship status due mostly to Washington's inflexibility over obsolescent Cold War defense issues. The Palau problem has deteriorated to the point where the United States has now reinstated in Koror a district administrator, the old colonial position of the trusteeship period under a different title, to override the local government whenever Washington wishes to do so.

The other exception is Guam's quest for a commonwealth status similar to that of the Northern Marianas. Guam's quest is at a near standstill, but the lack of progress in its case is caused largely by the refusal--or inability--of Guamanian leaders to negotiate realistically. Ironically, Guam--the oldest U.S. colony in the Pacific--is therefore the least developed in terms of political status. As Leibowitz correctly notes, "The result is growing mistrust and personal rancor between federal officials and those on Guam with each demeaning the other's efforts."

Leibowitz's book carries forward, but with considerably more legal material, the political history of American Micronesia begun by observers such as Norman Meller (*The Congress of Micronesia*, 1969), Roger Gale (*The Americanization of Micronesia*, 1979), and Timothy Maga (*Defending Paradise: The United States and Guam, 1898-1950*, 1988). In addition to its rich substantive content, the book is extensively documented with footnotes and bibliographic sources. Students, scholars, island government officials, and citizens of the territories will all find Leibowitz's analysis definitive and immensely helpful in understanding the evolution of American policies toward acceptance of local self-governance in the U.S. insular territories.

Of even greater value, however, is the advice Leibowitz provides on what policies should be considered by all parties to resolve the many territorial problems that remain. It is hoped every official who has anything to do with U.S. territorial matters will consult this book, especially officials in Agana, in Koror, and in Washington.

BOOKS NOTED

RECENT PACIFIC ISLANDS PUBLICATIONS: SELECTED ACQUISITIONS, SEPTEMBER-OCTOBER 1991

This list of significant new publications relating to the Pacific Islands was selected from new acquisition lists received from the libraries of Brigham Young University-Hawaii, University of Hawaii at Manoa, Bernice P. Bishop Museum, University of Auckland, East-West Center, University of the South Pacific, South Pacific Commission, and the National Library of Australia. Other libraries are invited to send contributions to the Books Noted Editor for future issues. Listings reflect the extent of information provided by each institution; some entries may be arranged by title in cases of an edited or compiled work and may include only primary author in cases of multiple authorship.

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