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"TO STUDY NATURE RATHER THAN BOOKS": CAPTAIN JAMES COOK AS NATURALIST OBSERVER AND LITERARY AUTHOR

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This article explores several apparent inconsistencies in the self-promulgated image of Captain James Cook as the epitome of the eighteenth-century explorer, an observer who recorded exactly and only what he had seen. Cooks ongoing habit of appropriating and simplifying the observations of others can be reconciled with his reputation as a scrupulous observer; on his first voyage, for instance, he simplified the naturalist observations of the specialist Joseph Banks better to reflect what he had seen. Moreover, as Paul Carter has argued, Cook's often highly subjective language reflects the scientist's awareness that even the most carefully made observation is inescapably subjective: the scientist's duty is to acknowledge and precisely to locate that subjective stance. But I shall also argue that Cooks integrity as a scientific observer became increasingly impaired by his aspirations to authorship. In the manuscript journal of his final voyage, at least one borrowed (and unacknowledged) passage did not describe what Cook himself had seen. And though in his writings Cook sought to sustain the rhetorically effective nonspecialist perspective of the "plain" and thus "honest" man, his problematic attitude to specialist and nonspecialist terminology indicates how Cooks subjective stance becomes increasingly difficult to locate.

CAPTAIN JAMES COOK (1728-1779) is perhaps the epitome of the eighteenth-century scientific explorer. It is difficult to summarize his achievements. Some have claimed that his early hydrographic work in eastern Canada--particularly his charting of the St. Lawrence River--was directly responsible for the British conquest of Canada that concluded the Seven Years' War.[1] And it is indisputable that he and the scientists and artists who traveled with him on his three great circumnavigational voyages contributed immeasurably to knowledge in numerous disciplines. Sent on his first circumnavigational voyage to discover a supposed southern supercontinent, Cook

found none. However, his exploration of Tahiti, New Zealand, and Australia yielded not only valuable maps and charts but also observations of immense zoological, botanical, and ethnographic interest. Cook returned to the southern hemisphere on his second great voyage, circumnavigating the globe at high southern latitudes and proving his hypothesis that no such supercontinent existed. Shortly after returning in triumph, Cook relinquished retirement and chose to make yet another voyage; the Admiralty hoped that this time Cook would prove a generally held hypothesis and discover a navigable passage linking the northern Atlantic and Pacific Oceans. But in 1779 he was killed in Hawai'i, where the expedition had spent the previous winter.

Cook contributed not only to science but also to the fields of language and literature. He is a curiously familiar figure in historical linguistics, for instance. His voyages introduced several new words into the English language and began to spread that language into several new worlds. Moreover, the grammar of Cooks circumnavigational voyage journals as well as contemporary editorial corrections to it are of sociolinguistic value, illuminating the development of a written standard English. When he wrote his first voyage journal, Cook did not anticipate that it would be read by anyone other than his superiors at the Admiralty--or that its editor, John Hawkesworth, would earn £6,000. Cook duly wrote his second and third voyage journals with publication firmly in mind. The Admiralty had initially appointed him for his expertise as a hydrographer and an astronomer, not for his ability to write an elegant journal of "remarkable occurrences." But a comparison of his first and last voyage journals illustrates that he learned quickly. His grammar speedily approached (though never attained) the standards of correctness proper to published prose.[2] And just as Cook improved his grammar, reflecting the period's linguistic anxiety, so he learned and implemented conventions of travel literature that were new to him.

As a travel writer, Cook was always representative of his time: he wrote at a time when the concept of "travel literature" denoted a wide range of material. Indeed, changes in his own practice may have resulted from tension among the contemporary demands, for example, for authentic firsthand observation, for novelty, and for comprehensiveness.[3] This article will argue that on Cooks first circumnavigational voyage, his practice as a journalist generally coincided with his practice as an observational scientist. Even when borrowing observations from others, Cook generally--I argue--recorded as precisely as possible only what he had seen, thus fulfilling one of the first principles of scientific observation. For instance, Cook not only borrowed but simplified a number of natural-historical descriptions made by Joseph Banks, the young, wealthy, university-educated representative of the Royal Society. I shall suggest that even here Cook simplified Banks's observations,

better to reflect the nonspecialist perspective that Cook often exploited in his writings.

Cooks experience as an astronomer and hydrographer had also taught him of the limitations of even the most meticulous firsthand verbal and visual observations. A secondary focus of this article is the flagrantly figurative and approximative language characteristic of Cook's comparisons of the new to the known: drawing on and supporting Paul Carter's study of Cooks place names, I shall suggest that such apparently "imprecise" scientific language signals to Cooks readers the inescapable subjectivity of any observation. Such imprecision is not necessarily incompatible with Cooks self-presentation as a champion of accuracy. Throughout his career as a circumnavigator, Cook associated himself with such conventional symbols as the plain style and ungeneralized particulars.[4]

But I shall also argue, as I chart the complicated and increasingly incompatible relationship between Cooks practice as a scientist and as a journalist, that Cooks integrity as a scientific observer became increasingly impaired by his aspirations to authorship. I am particularly interested in how Cooks changing conception of his role as a travel journalist is reflected in his ongoing habit of appropriating observations from the journals of others. In his second and third voyage journals, Cook continued to appropriate the observations of others--sometimes acknowledging them, sometimes not. And at least one borrowed (and unacknowledged) passage did not describe what Cook himself had seen: confined to the beach, Cook could not have seen the enormous statues on Easter Island that were described in his journal. The public's demands on its author had begun to compromise Cook's principles as a scientist.

In the true Cook spirit, this project has a strong empirical base. I have used the text-retrieval and analysis program *TACT* to analyze electronic texts of Cooks first and third voyage journals. With this tool I have been able to assess with more speed and more certainty the extent to which any particular passage is representative of Cooks general practice.[5]

Legends of Cook: Popular and Not Merely Posthumous Praise of Cook as an Accurate and Discriminating Observer

Shortly after Cooks second voyage, James Boswell met him at the house of Sir John Pringle, the president of the Royal Society. Boswell's admiring description repeats key terms: Cook is insistently associated with "truth" and "veracity," truth for Boswell being something that could be measured in gradations "nice" and "very fine." Boswell describes Cook as "a plain, sensible man with an uncommon attention to veracity. My metaphor was that he had

a balance in his mind for truth as nice as scales for weighing a guinea."[6] Boswell also relates an anecdote about Cook illustrating this "uncommon attention to veracity": "It was supposed that Cook had said he had seen a nation of men like monkeys, and Lord Monboddo had been very happy with this. Sir John happened to tell Cook of this. 'No,' said he, 'I did not say they were like monkeys. I said their faces put me in mind of monkeys.' Here," said Boswell, "was a distinction very fine but sufficiently perceptible."[7]

Misrepresentation of his fine and accurate observations unfailingly irritated Cook; he had been particularly incensed at the alterations made to the account of his first voyage by its editor, John Hawkesworth. Hawkesworth, though writing in the first person as Cook, the captain of the ship, had supplemented many of Cooks observations with those of Cooks shipmate Joseph Banks and with Hawkesworth's own philosophical reflections. At the same meeting, Cook complained to Boswell that Hawkesworth had also made "in his book a general conclusion from a particular and [taken] as a fact what they had only heard."

As Boswell describes him, Cook presents himself as wary of generalization and misrepresentation, and aware of the subjectivity even of his own fine observations. To the Royal Society's president, John Pringle, Cook contrasted himself not only with Hawkesworth, who took hearsay as fact, generalized from a particular, and conflated the reports of separate observers, but also with Lord Monboddo's informant, who had transformed faces that "put [Cook] in mind of monkeys" into men that were "like monkeys." And in other contexts, public and private, Cook continued to portray himself as the epitome of precision. In the preface to the published account of this voyage that appeared the subsequent year, Cook invoked what Lamb has called "the rhetoric of antirhetoric," characterizing himself as a "plain man," "determined to give the best account he is able of his proceedings," unable to deliver "elegance" or "plausibility," and restricted to the plain style that was traditionally associated with objectivity and accuracy.[8]

Cooks association with unmediated fact not only was perceived by Boswell but was central to the popular legend surrounding him. During Cooks lifetime, the concreteness and minuteness of Cooks vision had been parodied by the *Critical Review*, which, in a dispute over official authorship rights to the account of Cooks second voyage, had taken the side of his rivals, who argued that a mere navigator could not be expected to write the official account of a voyage so important to science. The *Critical Review* characterized Cook's style as minutely paratactic in a long list of ship's supplies, disdainfully concluding the list with "astronomers": "malt, sour krout, salted cabbage, portable broth, saloup, mustard, marmelade of carrots, inspissated juice of wort and beer, the frame of a small vessel, fishing-nets and

lines, additional cloathing, a landscape-painter, botanists, and astronomers."[9] But posthumously, Cook's observational practices were praised. Reviewing the published account of his third and final circumnavigational voyage, the *English Review* and *European Magazine* in 1784 characterized Cook, respectively, as "exact," "accurate," "minute," with no "bias to speculation"; not' as fine a writer as Captain King, but less likely than Mr. Anderson to "transfer his own ideas to the objects before him."[10] Two different doggerel poems present the conventional vision of the man, associated with a panoramic perspective ("the whole," "ev'ry object," "survey") as well as with the ever-present minute details ("the parts," "accurate").

> Near the Circle Antaractic *[sic]* he sails round the Pole
> Attentively viewing the parts and the whole.
>
> Firm and compos'd he steers his destin'd way,
> And ev'ry object views with accurate survey!

And Cooks twentieth-century editor and biographer J. C. Beaglehole described Cook as "the genius of the matter-of-fact."[11]

The accuracy of Cooks portrayal of the external world is further suggested by the elusiveness of his inner world, never projected outwards: not a few biographical and literary portrayals of Cook foreground the silence or even the absence of their subject. Cooks habitual taciturnity, a cause of frustration for all those who sailed with him, is recalled in a frequently cited nineteenth-century anecdote told by a very old Maori recalling his childhood. Besant, the first original biographer of Cook since his contemporary Kippis, resorted to "reading" such concrete things as countertops and portraits in the absence of anything else. Twentieth-century imaginative treatments of Cook also characterize him as silent and enigmatic: Kenneth Slessor's poem "Five Visions of Captain Cook" (1931) concludes with a secondhand report of Cooks death by a blind Scotsman surrounded by empty chairs, a culmination of the remoteness and mysteriousness associated with Cook through the poem. And in several twentieth-century plays inspired by his life, Cook is silent or even absent.[12]

Indeed, as he presented himself to his public, this self-effacing and extraordinarily accurate observer corresponds closely with the idealized eighteenth-century explorer-scientist described by Barbara Stafford in *Voyage into Substance* (1984). Stafford's observer "reads natural characters as they actually are rather than skimming over, personifying, or transmuting them into what they are thought to be" and, though relating the hitherto unknown to the known, is careful "to draw minute distinctions where a careless mind

might have interjected broad associations."[13] Despite any idealization of the "piercing" and discriminating eye of the eighteenth-century sailor, Stafford's idealized observer differs very much from the real seamen in Cook's crew, whose fondness for applying old names to new species was repeatedly deplored by the Royal Society's representative Joseph Banks: "Besides these were many species which tho they did not at all resemble any fish that I at least have before seen, our seamen contrivd to give names to, so that hakes, breams, Cole fish &c. were appellations familiar with us, and I must say that those who bear these names in England need not be ashamd of their nam[e]-sakes in this countrey."[14] And like Cook, Stafford's idealized observer is often the first to see and record the previously unseen, but scrupulously records only what he has seen firsthand.

Complication: Cook Copies and Simplifies the Naturalist Observations of Joseph Banks

And yet in the journal of his first circumnavigational voyage, Cook sometimes drew his naturalist and ethnographic observations from sources other than his own perceptions. Perhaps this was to be expected, even of Cook. Engaged by the Admiralty specifically for his expertise as a navigator and hydrographer, in his official journal Cook was expected to record everything "remarkable." The majority of Cook's observations are his own. But especially during the first half of the voyage, Cook silently appropriated the observations of others. He relied particularly on Joseph Banks, at twenty-seven already a fellow of the Royal Society, its representative on the *Endeavour's* voyage, and eventually its president. That Cook used Banks is well known. Beaglehole's comparison of the journals of Cook and Banks has established that extensive "extracts from Banks" appear in Cooks summary description of Tahiti, the *Endeavour's* first extended port of call.[15] Banks's influence has also been detected by Glyndwr Williams in Cooks reflections on the inhabitants of Australia,[16] and it can also be seen in Cook's summary descriptions of the flora and fauna of New Zealand and Australia (Banks 2:6; Cook, 276-277). Both Beaglehole and Williams have observed that Cook did not tend to copy Banks exactly: he often supplemented or summarized Banks, or left things out. Cook rarely incorporates Banks's descriptions of insects, for instance.

Given Cooks inexperience, his diligence, and his ambition, his use of Banks is unsurprising: Banks was indubitably the best resource on board. But two particularly curious habits of Cook the copyist must be addressed. Unlike Hawkesworth, who in the preface to his *Voyages* explicitly acknowledged that the observations and reflections of several men were subsumed

under the person of the captain--and whom Cook deplored for this practice--Cook never acknowledged his borrowings from Banks. The seams appear only occasionally. The non-classically educated Cook refers to two male Tahitians as Lycurgus and Hercules, names assigned by Banks to a just man and a large man, respectively, before their real names (as Cook puts it) were discovered.[17] And when describing more exotic species of plants and animals, it is clear that Cook must have copied Banks's taxonomy: it is unlikely that Cook had previously seen "gum dragon" or an opossum, although he uses these terms as analogues.[18]

However, what is most striking is that, in a number of his descriptions of species similar to those in England and Europe, Cook not only copies Banks but eliminates some of the fine distinctions in Banks's descriptions of plants and animals by substituting words like "exactly" for words like "somewhat." For example, "Quails **differing but little at first sight from those of Europe**" (Banks 2:5) becomes "Land fowl are likewise in no great plenty and all of them except quals are I beleive unknown in Europe, these are **exactly like those we have in England**" (Cook, 276-277). I will suggest that on Cooks first voyage, his alteration of Banks's journal reflects the professional astronomer's desire to record only and exactly what he had observed:

The *Endeavour* Voyage (1768-1771): From Astronomy to Ethnography

The ostensible object of Cook's first voyage was to observe from Tahiti the transit of the planet Venus across the face of the sun, the timing of such observations from widely separated locations then being critical to finding the distance of the sun and the scale of the solar system. Though the astronomical event was rare, for Cook the making of astronomical observations was routine. But in secret instructions, Cook had also been instructed to make discovery of the supposed southern "Continent or Land of great extent."[19]

As captain of the *Endeavour,* Lieutenant Cook was required by the Admiralty to keep not only a log and such visual records as charts and coastal profiles,[20] but also a journal containing such details as "place where the ship is at noon; changes of wind and weather; etc.," along with "remarks on unknown places" and "remarkable occurrences" generally.[21] This was a completely new task for him. Cook had already kept ship's logs in northeastern North America and, indeed, as a marine surveyor and practical astronomer, was even a published author--of a report (communicated by a fellow) to the Royal Society of his observations of an eclipse and of several volumes of sailing directions accompanying his charts of the Newfoundland coast.[22] But he

had not previously been expected to record naturalist and ethnographic information.

What aids could Cook have used? Cook lacked much formal education, but was enterprising and highly intelligent. A Yorkshire estate foreman's son, he spent some time at the Postgate School, where he and seven other poor children would have been taught "reading, writing, and arithmetic."[23] At age eighteen, he apprenticed himself to a ship owner; at twenty-seven, he joined the navy. Throughout his working life, Cook supplemented his professional training as a navigator with private study: for instance, he spent the winter months in Canada grounding his surveying techniques by reading mathematics and astronomy.[24]For naturalist information, Cook had a number of resources on board.

The written instructions given to him by the Admiralty and by the Royal Society do not seem particularly helpful:[25] a little more useful than the four untranslated lines of Virgil concluding the Royal Society's discussion of transplantation (Cook, 518) were the instructions that observations should be made "carefully" (the Admiralty; Cook, cclxxxii) and "minutely" (the Royal Society; Cook, 517), and that local names and medicinal properties should be recorded. Specimens should also be collected: the Admiralty's instructions assumed that "proper Examination" of botanical objects would be made in England. More specific instructions could not be given: the "Hints" offered by James Douglas of the Royal Society acknowledged that "the Natural productions of the Country, in the Animal Vegetable and Mineral Systems . . . open so vast a field, that there is no room in this place for descending to particulars" (Cook, 517). The emphasis was indeed on the "minute" and the "particular": a century earlier, Boyle, writing in the Royal Society's *Philosophical Transactions* in 1666, had emphasized the importance of both the "peculiar" and the "particular."[26] Douglas does not rule out metaphor: he makes no mention of the unmetaphorical, plain, and paratactic prose of the kind described by Sprat in the seventeenth century and stereotypically associated with the Royal Society. Indeed, despite any idealization of the plain style for scientific travel writing, Cook would have been confronted with a variety of styles in the books that he referred to that he could have used as models.[27]

Cook was a reader: his constant consultation of books has been well documented. On board were the manuscript journals of previous British voyages in the Pacific and between sixty and seventy books of travel and natural history brought by Banks.[28]Both Banks and Cook consulted these texts: Cook records seeing "a Sea lyon the head of which was exactly like the head of the male one described [by] Lord Anson" (234), and Banks, near Pepys Island, disagrees at some length with Dampier's description of the distribu-

tion of the “small shoals of the red lobsters which have been seen by almost every one who has pass’d these seas” (1:210).

Joseph Banks himself was by far the most useful resource for Cook. After his education at Oxford, where he had personally established a botany tutor, the wealthy Banks had traveled to Newfoundland on a kind of Grand Tour, and on Cooks voyage was a full-time observer and journalist, accompanied by Linne’s former assistant Solander and by the artists Buchan and Parkinson. Though Banks later failed publicly to disseminate his collections and his writings, he was always generous in allowing others, including Cook, access to them.

When Cooks naturalist observations betray the influence of Banks, their accounts are sometimes reasonably comparable. For example,

> about noon saw one like a snipe but less and with a short bill which I judge to be a land bird (Banks 1:395)
>
> At 11 Am saw a Bird something like a snipe only it had a short Bill, it had the appearence of a land bird. (Cook, 165)

More often, Cooks and Banks’s accounts differ. The differences identify a few distinctive characteristics of Cook as a naturalist observer. As stated above, Cook occasionally removes Banks’s fine distinctions. This habit of Cooks is especially striking in two contexts. In his descriptions of edible plants in Tahiti, most of the distinctions removed are those relating to taste:

> a fruit **not unlike an apple which when ripe is very pleasant** (Banks 1:342)
>
> a fruit **like an apple** (Cook, 120)

> a fruit in a Pod **like the large Hull of a Kidney bean,** which when roasted **eats much like Chestnuts** and is call[d] *Ahee* (Banks 1:343)
>
> a fruit in a Pod **like a Kidney bean** which when roasted **eats like a chestnut** and is called Ahee (Cook, 121)

> Bread fruit cookd in this manner becomes soft and something **like a boild potatoe, tho not quite so farinaceous as a good one yet more so than the midling sort** (Banks 1:344)
>
> Bread fruit, Bananoes and Plantains Cook’d this way eats **like boild Potatoes** and was used by us by way of bread when ever we could get them (Cook, 122)

Later in the voyage, Cook simplifies a few of Banks's descriptions of fauna, particularly of birds: he not only removes distinctions and qualifications but twice substitutes the unequivocal adverb "exactly" for qualifiers like "differing but little at first sight" and "very like if not quite the same":

> Of Birds there are not many species, and **none except perhaps the Gannet the same as those of Europe. There are however ducks and shags of several kinds sufficiently like the European ones to be calld the same by the seamen,** Both which we eat and accounted good food (Banks 2:5)
>
> Sea and water fowles of all sorts are however in no great plenty, **those known in Europe are Ducks, Shags, Gannets & gulls** all of which were eat by us and found exceeding good (Cook, 276)

> Quails **differing but little at first sight from those of Europe** (Banks 2:5)
>
> all of them except quals are I beleive unknown in Europe, **these are exactly like those we have in England** (Cook, 276)

> **The Land Birds were crows, very like if not quite the same as our English ones. . . a Crow in England tho in general sufficiently wary is I must say a fool to a New Holland crow** and the same may be said of almost if not all the Birds in the countrey (Banks 2: 118)
>
> and **Crows exactly like those we have in England** (Cook, 311)

It is tempting to postulate that Cook simplified Banks's distinctions in descriptions of edible and inedible flora and fauna because he was unable to perceive them. To those who sailed with him, Cooks insensitive palate was infamous: he ate "acrid" roots and a toadfish that was not only hideous but poisonous. Moreover, in George Forster's version of the toadfish incident, Cook stubbornly insisted on serving up the fish, against the advice of Forster and his father, because it was the "identical sort of fish" that Cook had eaten "without the least bad consequence" "on the coast of New Holland."[29] And in his descriptions of these few birds, Cook may also have simplified what he had read in Banks so that his account did not credit him with observational abilities he did not at that time possess: to him these birds may have looked exactly like crows and quails.

A further examination of Cook and especially a more comprehensive comparison of Cook and Banks suggests that Cook was capable of making fine distinctions in his naturalist observations, more often using qualifiers than

words like “exactly.” In his first voyage journal, Cook uses approximations and qualifiers like “something like” (8x), “very (much) like” (3x), “most like” (1x), “some such like Animal” (1x), or “of the . . . kind” (8x) or “sort” (3x) more frequently than he uses “exactly like” (3x) or “in every respect like” (1x).[30] Especially in his ethnographic observations, Cook often emphasizes the approximative use of terms by pairing them with the coordinator *or:* “speach or prayer,” “cheif or king,” “tribe or Family,” “Hamlet or village,” “servants or slaves.”[31] Indeed, Cooks observations are occasionally more precise than those of Banks. On one occasion when his written observations may otherwise have been influenced by those of Banks, Cooks numerical estimate of “some hundreds” seems more accurate than Banks’s “many millions”:

> the little silver backd bird which we saw off Faukland Isles and Cape Horn . . . **many millions** I may safely say of the small bird mentiond yesterday about as large as a dove, greyish on the back, some with a dark colourd mark going in a crooked direction on that and its wings (Banks 1:389-390)
>
> **some hundreds of Birds** that were smaller than Pigeons, their backs were grey, their bellies white and the ends of their tails black, and have a blackesh line along the upper parts of the wings from the tip of one to the other. We saw birds very like these near Faulklands Islands on the Coast of Patagonia, only they had not the black strake along the wings. They fly low like sheer-waters or Mother Caries Birds and are perhaps of the same tribe. For distinction sake I shall call them Doves. (Cook, 161)

Moreover, Cooks observations are sometimes qualitatively more accurate than those of Banks. In the ancient spirit of the Royal Society--“to study Nature rather than Books”--Cook seems less likely than Banks to be influenced by his reading. Glyndwr Williams has argued that Cooks first descriptions of the inhabitants of Australia show fewer preconceptions and more open-mindedness than those of Banks, “a readiness to view a strange and primitive life style on its own terms.” Even when Cook borrows ideas and expressions from descriptions of American aboriginal peoples for his summary description of the native Australians, he makes significant changes to the content: Cooks description, unlike those of his sources, distinguishes between “necessary Conveniences so much sought after in Europe” and “all things necessary for life.”[32]

Cook’s relative ignorance of natural science sometimes meant that his observations were more accurate than those of his more knowledgeable companions. On his second voyage Cook won a dozen bottles of wine from

Johann Reinhold Forster in a "hotly contested 'philosophical dispute' . . . between the 'Experimental-Men' and the 'military Men' ": Forster insisted that what Cook correctly identified as tall trees were basalt columns, then " 'all the fashion in the philosophical world.' " And Banks's beloved Latin binomials, simultaneously signaling similarity and difference, sometimes provided only an illusion of precision: on at least one occasion, Banks applied his names--whether Latin or English--as rashly as the common seamen. His "birch" was in fact a counterpart of the beech; and the "cranberries" (a term perhaps influenced by Wallis's journal), the berries of the shrub *Pernettya mucronata.* [33] Cook's corresponding comparison is more cautious and thus in this case more accurate: his berries are not "cranberries" but merely "like Cranberries," and even his "wood of the Birch kind" differs more explicitly from "that in England or North America" than Banks's *Betula antarctica.*

> The trees here are cheifly of one sort, **a Kind of Birch** *Betula antarctica* with very small leaves, it is a light white wood and cleaves very straight; sometimes the trees are 2 or 3 feet in diameter and run 30 or 40 feet in the bole; possibly they might in cases of nescessity supply topmasts. Here are also great plenty of **cranberries both white and red,** *Arbutus rigida.* (Banks 1:217)
>
> The wood is **of the Birch kind,** but of a different quallity to that in England or North America, here are likewise . . . **some berries like Cranberries, but growing on bushes.** (Cook, 51)

The preceding pair of passages also exemplifies how the non-classically educated Cook, despite his temporary adoption of the classical proper names Lycurgus and Hercules, generally did not appropriate the Latin terminology that Banks applied so freely to new flora and fauna. Instead, Cook would use the English nomenclature with which he was familiar--"Gull" or "Stearing" (66) for Latin *sterna* (Banks 1:241), "Gum dragon" (307) for *sanguis draconis* (Banks 2:57); "Possums" (367) for "of the Opossum kind and much resembling that calld by De Buffon Phalanger" (Banks 2:117). Although Cook problematically invoked *gum dragon* and *possums* as analogues, as acknowledged above, he scrupulously used his own "instruments'‘--plain English rather than specialist terminology, apparently unwilling to pretend to a specialist knowledge that he did not yet have. Moreover, that Cooks naturalist observations were at least occasionally more accurate than those of Banks suggests that elsewhere his simplification of Banks's observations might be regarded as his attempt to record exactly and only, what he had seen. In each case, the object of study has been re-presented from a new if more limited perspective--Cook's.

Cook, Banks, and the Kangaroo

A comparison of Cooks account of the kangaroo with that of Banks provides a useful test case for what Cook had learned on the *Endeavour's* voyage. The kangaroo and the wallaby (unlike the Australian opossum, which Banks rightly recognized as a distant relative of the American opossum and the East Indian phalanger) presented a problem in that they were completely unlike anything their European observers had ever seen before. Europeans resolutely described these marsupials in terms of European animals. But different Europeans selected different animals. Muller, publishing in 1777, "attributed the kangaroo to the genus *Mus*"; 150 years earlier, in a report that "probably never reached the eyes or ears of Englishmen," the Dutch Pelsart had described the dama or tammar wallaby as a kind of cat, "the size of a hare," with a "very long tail" "like that of a long-tailed monkey." A kangaroo or wallaby may have been Dampier's short-armed raccoon or "Beast as big as a great Mastiff-Dog" with a corresponding print.[34] That the beast was essentially unlike anything the *Endeavour's* observers had ever seen is clear from its prolonged namelessness: for a month after its first sighting, Cook referred to it as "the animal before spoke of" (351-363); even Banks, generally swift to apply Latin binomials,[35] described it only as "the animal" or "the wild animal" (2:100).

The travelers on the *Endeavour* saw the kangaroo's dung before they saw the animal itself. On 1 May 1770, Cook and Banks, along with Dr. Solander and "some of the people, making in all 10 musquets" (Banks 2:57), made an excursion inland at Botany Bay. The similarity of their accounts indicates collaboration of some kind.

> We saw one quadruped about the size of a Rabbit, My Greyhound just got sight of him and instantly lamd himself against a stump which lay conceald in the long grass; we saw also the dung of a large animal that had fed on grass which much resembled that of a Stag; also the footsteps of an animal clawd like a dog or wolf and as large as the latter; and of a small animal whose feet were like those of a polecat or weesel. (Banks 2:57)
>
> Dr Solander had a bad sight of a small Animal some thing like a rabbit and we found the dung of an Animal which must feed upon grass and which we judged could not be less than a deer, we also saw the track of a dog or some such like Animal, (Cook, 307).

Beaglehole suggests that the stag-sized grass-eater was probably the kangaroo (Cook, 307).

There are once again significant differences between the two accounts. More cautiously than Banks, Cook concludes that the "large animal which had fed on grass" was about the size of a deer rather than "resembl[ing]" one. But in general Cook is less precise than Banks. Banks establishes loose similarities between the unknown animals and European ones only with respect to particular qualities--size, dung, footprints. In contrast, Cook extends the similarity beyond a single point of resemblance, though compensating for the generalization by adding more qualifiers: Banks's "quadruped **about the size of a Rabbit**" is for Cook "a small Animal some thing **like a rabbit**"; similarly, the "animal **clawd like a dog or wolf** and **as large as the latter**" is for Cook "**a dog** or some such like Animal." Banks's observation that the large animal "had fed upon grass" corresponds to Cooks conclusion that it "must feed upon grass"; *must* alerts us to Cooks reasoning processes.[36]

Once seen, the large animal is initially compared to a greyhound:. that Banks had two on board may well have influenced the description.

> The People who were sent to the other side of the water in order to shoot Pigeons saw **an animal as large as a grey hound, of a mouse coulour, and very swift.** (Banks 2:84)
>
> one of the men saw an animal **something less than a grey hound, it was of a Mouse Colour very slender made and swift of foot** (cook, 351)

The sustained similarities between Cooks and Banks's reports confirm that, although neither of them had seen the animal, it was of sufficient interest to warrant some degree of discussion. The next day, June 23, Banks reports that the "people who went over the River saw the animal again and describd him in much the same manner as yesterday" (Banks 2:84). In the entry for June 24, Banks, already irritated at not yet having seen the animal himself, seems not to have found the reports of others particularly helpful: his mention of "descriptions of the animal" is followed by a patronizing account of the "Seamanlike" tendency to compare other new specimens to distinctly unhelpful referents:

> Gathering plants and hearing descriptions of the animal which is now seen by every body. A seaman who had been out in the woods brought home the description of an animal he had seen composd in so Seamanlike a stile that I cannot help mentioning it: **it was (says he) about as large and much like a one gallon cagg, as black as the Devil and had 2 horns on its head, it went but Slowly but I dard not touch it.** (Banks 2:84)

Only the next day do Banks and Cook have the good fortune to see "the beast so much talked of." Again, both men's descriptions are similar, and constrained by the earlier analogue of the dog. Cooks, longer and in one respect more accurate than Banks's, is a modification of the earlier report: while sustaining and developing the comparison of the animal to a canine, he also qualifies two of the previous comparisons: the animal is "of a **light** Mouse colour" and "the **full size** of a grey hound and shaped in every respect like one, with a long tail which it carried like a grey hound, in short I should have taken it for a wild dog, but for **its walking or runing in which it jumped like a Hare or a dear.**" Cook also relays a description of the animal's small legs and goatlike feet, noting that he had not seen them himself (Cook, 351-352). Banks, in contrast, has become even more constrained by his initial point of comparison: he reported that the beast was "like a grey hound in size **and running**"; only on July 7 does he observe much to his "surprize that instead of Going upon all fours this animal went only upon two legs, making vast bounds just as the Jerbua *(Mus Jaculus)* does" (Banks 2:89). Banks also concludes his initial firsthand description of the animal with the characteristic, candid, and really rather accurate exclamation "what to liken him to I could not tell, nothing certainly that I have seen at all resembles him" (Banks 2:85).

Both men revise their descriptions when the eternal sportsman Gore shoots a young male specimen of "the animal that had so long been the subject of our speculations" (Banks 2:93-94) or "the Animal before spoke of" (Cook, 351, 359). The greyhound is no longer a useful analogue; indeed, -Banks finds it difficult to find any analogue at all. With a male specimen, Banks could not perceive this marsupial's similarity to the opossum.[37] Its method of locomotion explained Banks's comparison to the jerboa and perhaps reinforced Cooks comparison of its head and lips to that of a hare. For Cook, just as he had avoided using specialist terminology, had undoubtedly never seen a jerboa and therefore did not use it as an analogue.

> its body was long the head neck and shoulders very small in proportion to the other parts--it was hare lip'd and the head and ears were most like a Hares of any animal I know (Cook, 359n)
>
> To compare it to any European animal would be impossible as it has not the least resemblance of any one I have seen. Its fore legs are extremely short and of no use to it in walking, its hind again as disproportionaly long; with these its hops 7 or 8 feet at each hop in the same manner as the Gerbua, to which animal indeed it bears much resemblance except in Size, this being in weight 38 lb and the Gerbua no larger than a common rat. (Banks 2:94)

Beaglehole's examination of the manuscript sources establishes that Cook rewrote his description after examining the specimen more closely and then amplified it after consulting Banks's account. The passages in boldface below, inserted subsequently, correspond to Banks's text and do not appear in the Mitchell transcript (MS M), sent to England from Batavia in 1770: it is possible that Cook added them well after the fact.[38]

> it was a small one of the sort weighing only 28 pound clear of the entrails. The head neck and shoulders of this Animal was very small in proportion to the other parts; the tail was nearly as long as the body, thick next the rump and tapering towards the end; the fore legs were 8 Inch long and the hind 22, its progression is by hoping or jumping **7 or 8 feet at each hop** [interlinear addition; not in MS M] upon its hind legs only, for in this it makes no use of the fore, which seem to be only design'd for scratching in the ground &ca. The Skin is cover'd with a short hairy fur of a dark Mouse or Grey Colour. Excepting the head and ears which I thought was something like a Hare's, **it bears no sort of resemblance to any European animal I ever saw; it is said to bear much resemblance to the Gerbua excepting in size, the Gerbua being no larger than a common rat** [sentence written closely at end of page; not in MS M]. (Cook, 359)

In this, Cooks summary account of the kangaroo, he modifies an observation derived from Banks that he cannot corroborate at firsthand: Banks's "any European animal" becomes "any European animal **I ever saw**," although Cook retains, with severe qualification, his own initial comparison of the animal's head and ears with those of the (European) hare: "Excepting the head and ears which **I thought** was something like a Hare's" He also flags Banks's reference to the Gerbua with **"it is said."** He records not its original weight of thirty-eight pounds, given by Banks, but its weight "clear of the entrails" when he must have examined it. And where Banks describes only the general proportions of the limbs of this particular specimen, Cook measures their precise dimensions--perhaps because the specimen would remain in Banks's possession, perhaps because of a cultural compulsion for measurement,[39] or perhaps because as an exact scientist Cook was more interested in concrete kangaroos than in kangaroos in the abstract.

Cook and the Limits of Verbal and Visual Precision

These verbal descriptions of the kangaroo were not only supplementary but even secondary: once killed, the kangaroo was weighed, measured, drawn,

and perhaps even preserved. On this, perhaps the first voyage with professional artists on board, the navigator and natural historians alike relied on the interplay of word and drawing in their records, and it is unsurprising that textual records were felt to be inferior to visual records in their ability transparently to convey an accurate description. Cook himself often observed that "drawings and paintings . . . give a more perfect idea . . . than could be formed from written descriptions only," and on Cooks final voyage, the only two occasions on which he suggests that even drawing has its limits are when he must describe a dance.[40] The vicious dispute between (the ultimately victorious) Cook and the Forsters over possession of the sixty-plus engravings from the second voyage indicates the importance of visual records, and in a private letter Cook observes that these illustrations will compensate for any defects in his writing style.[41]

Yet as an astronomer and hydrographer, Cook would have been. well aware of the limited precision even of graphic and numerical records. The best that one could do would be to make as many observations as possible, as accurately as possible: Stafford's solitary scientists recorded their observations, however incomplete, on the spot, at an "identifiable moment," and recorded as many different observations from as many different perspectives as possible. Cook describes his practice as a journalist in similar terms, defending his second voyage journal (somewhat untruthfully) as "my own narrative" "as it was written during the voyage."[42]

The conventions of astronomy demanded that a recorded observation reflect the perception of a single observer from a fixed point in time and space, and that each numerical observation be accompanied by the observer's name and perhaps also by a description of the quality of the observer and of his instruments. Indeed, the first publication of Cooks own astronomical observations, communicated to the Royal Society by J. Bevis, begins by describing "Mr. Cook, a good mathematician, and very expert in his business" and his "very good apparatus of instruments."[43] However, even expert astronomers routinely failed to achieve objectively exact results, even with a multitude of multiple observations: for instance, the many careful observations of the transit of Venus made by Cook and other observers on board the *Endeavour* varied considerably from each other, and indeed none yielded estimates for the distance between the earth and sun that were anything near the actual distance calculated later. That the longitude of Queen Charlotte's Sound could not be settled decisively even after multiple observations enraged the natural historian Johann Reinhold Forster, who concluded that the astronomers were either "negligent" or that their accuracy was "pretended."[44]

The conventions of hydrographic observation--visual and written--demonstrate the same insistence on multiple observations from different per-

spectives. On his first voyage, Cooks most frequent comparison using the word *like* is "like an island" or "like two islands"--from a particular direction at a particular time. More than once Cook noted in his journal that a landmark changes in appearance with the position of its observer. These multiple perspectives conventionally were not conflated, but were instead recorded on a single sheet of paper. It is significant that the last of Barbara Stafford's many references to Cook in her *Voyage into Substance* introduces the section asserting that "even singularities do not possess a unique aspect."[45] Very often a feature that was "like an island" from one direction was not an island: the truth, if Cook could ascertain it, would be evident on his very accurate chart.

Cooks charts acknowledge that however meticulous the observer and however minute and concrete the ungeneralized particular observations, it was impossible utterly to suppress or screen the idiosyncratic or subjective.[46] Indeed, as described by Paul Carter, Cooks hydrographic practice vividly demonstrates the link between the scientific observer's unapologetic subjectivity and his integrity. Carter argues that when Cook once emended his clerks fair copy of his journal, he was not being petty but was restoring the document's single point of view: "The Soil to all appearence nothing but white sand thrown up in low irregular hills lying in narrow ridges parallel with the Shore: this occasion'd Mr Banks to give it the name of *Sandy bay*" (Cook, 222). In the Mitchell transcript, "Mr Banks" is heavily deleted and "me" written above it by Cook. Carter lists other practices of Cooks that demonstrate the same scrupulous subjectivity. Cook carefully preserved the gaps in his text and on his charts, retaining the trace of his unique passage.[47] And the accuracy of these charts meant that his place names did not have to be as descriptive as "Sandy Bay." Many were overtly figurative ("Chain Island") or occasional ("Thirsty Sound," "Providential Channel"). Indeed, Cook "[did] not intend to preserve the delusion of objectivity, for his standpoint is neither neutral nor static. Instead, they [his place names] draw geographical objects into the space of his passage."[48] His similes illustrate the same subjectivity, the same insistence on recording information as he had seen it at the time. Although Cook sometimes compares an unfamiliar landscape with a site in England (180) or with such other widely known landmarks as the Peak of Tenerife (232), he just as often compares it to something encountered within the course of that particular voyage, making no concessions to later readers. For instance, when in Newfoundland, Cook had described a "remarkable Rock, that at a Distance appears like a Shallop under Sail." A Newfoundland shallop (from French *chaloupe*) is a shallow-water fishing boat; Cook mentioned them elsewhere in the text that accompanies his chart.[49]

Cooks fanciful place names also foreground the figurative use of language and thus the very arbitrary link between word and thing and the limitations of language as a descriptive tool. The subjectivity and singularity of these place names, so persuasively discussed by Carter, is evinced with particular clarity in Cooks naming of the first Pacific islands. Osnaburgh Island, named and unmemorably described by Cooks precursor Wallis, captain of the *Dolphin*, as "nearly circular and about two miles over," by Banks as "like a very short cone," and by Johann Reinhold Forster as "a high Peak with a flat top," to Cook from northwest by west "looks like a high crown'd Hatt, but when it bears north the top is more like the roof of a house."[50] Cook's text describes mountains like camels and dromedaries (222, 300), pigeon houses and glass houses (301, 319); islands and rocks like hay stacks (30, 178), com stacks (185), arched bridges (21), castles (192), and so forth. His charts give them names to match. Several of Cook's similes reflect, perhaps, an unconscious sense of conflict as the Pacific encounter begins: on the first island, Lagoon Island, two notable trees look like a "Large Tower" and "very much like a flag," respectively. Bow Island ("of a Bow-like figure") is surrounded by a "border of land and Reef. . . like a wall." The subjectivity of Cooks place names can be illustrated vividly by comparing Cooks name for Hao, *Bow Island,* with Bougainville's: *La Harpe.*[5 1]

Many other features of Cook's journals illustrate his conscious awareness of the approximative nature of language, of its limitations. But on his first voyage, he was rarely deterred by these limitations and diligently set about describing the unfamiliar and extraordinary sights that he had diligently sought out. Indeed, he typically acknowledges difficulty only when describing strong emotions, formulaically "better conceived than described."[52] Both Cook and Banks, before facing "the animal before mentioned," had been sorely challenged by the costume of the chief mourner in Tahiti. Banks resorted to drawing: "He put on his dress, most Fantastical tho not unbecoming, the figure annexd will explain it far better than words can" (1:288-289). Even Cook was provoked into a rare admission of perplexity, but unlike Banks he ploughed ahead with his pen: "I cannot tell how to describe or to convey a better Idea of it than to suppose a man dress with Plumes of feathers something in the same manner as those worn by Coaches hearses, horses &ca at the funerals in London" (136).

A reader of Cooks journal is often aware of Cooks awareness of the approximative nature of language: doves are so named "for Distinction sake"; settlements are called "Hamlet or village"; rulers called "cheif or king." But unrigorous synonyms are more often used less explicitly in Cooks prose. In the *Endeavour* journal, for instance, a Malaysian word, *proe,* describes a Tahitian boat (129); and the word *king* appears thirty-two times.

For, as a writer, Cook seems to have taken the subjectivity of individual perception and the approximative nature of language for granted. Indeed, readers of John Douglass edition of Cooks journal, which appeared in 1777, a year after the dinner party at Sir John Pringle's, could marvel at the men of "this ape-like nation" with their "monkey countenances."[53] And editor Douglas had not misrepresented Cook: in Beaglehole's edition of Cook's own journal, which appeared almost two hundred years later, we can read Cooks own words describing the "Monkey faces" of "this Apish nation."[54] In both editions, words could be compared with illustrations: the man in the engraving in the 1777 text differing visibly from the man in the reproduction of William Hodges's original in the 1961 text and neither looking anything like a monkey.[55] Cooks seemingly erroneous memory of his observations--"I did not say they were like monkeys. I said their faces put me in mind of monkeys"--must have taken for granted, consciously or unconsciously, his inescapably subjective stance: even his most carefully chosen words necessarily presented a singular vision of the world.

Cook and Contemporary Conventions of Travel Writing

Cooks convictions about scientific authorship were put in the spotlight in 1775-1776, in a controversy that pitted him against the Forsters, sponsored by the Royal Society to travel on the second voyage (1772-1775). The Admiralty, determined "that there should be no more Hawkesworths," spent 1775 deciding whether the second voyage narrative should be written by Forster senior, by Cook, or jointly: their deliberations have been described at length.[56] One issue was the ability of either man to write with "correctness": in a letter to Lord Sandwich, Forster attacked Cooks grammar and style; but the foreign-born Forster's own writing was found by Lord Sandwich in need of "correction."[57] Cook was eventually given official sponsorship, along with rights to the plates. But George Forster's quickly written *Voyage Round the World* (1777) appeared six weeks before Cooks version.[58]

In his preface, Forster made several attempts to discredit his competitor's scientific and authorial credibility. Like Cook, Forster championed accounts that--unlike Hawkesworth's much-maligned volumes--were firsthand; he also acknowledged the usefulness of separate accounts that presented the view of the "same objects" by "different persons" with "different points of view." Forster made his own point of view, that of the natural historian and philosopher, sound infinitely more interesting to the public than that of Cook the navigator, compiler of "nautical details," "bearings and distances," and "instructive particulars." More seriously, Forster also intimated that Cooks text was not in fact a firsthand report: Cook had not only been too

busy "to superintend the printing of his own Journals" but had not even written them himself. These statements of Forster's were not without foundation: Cooks text had been ghost-edited by John Douglas.

Forster described the greatest difference between him and Cook as their treatment of "facts." Forster mocked what he implied was Cooks "simple collection of facts," "which no art could reunite into a whole," "a single proposition," and argued that it was necessary for the traveler to "have penetration sufficient to combine different facts, and to form general views from thence." Dr. Johnson also believed that the collection of facts was in itself insufficient, being the necessary prelude to the "ascent to principles." Moreover, Hawkesworth's version of Cook had been reviled for not synthesizing, for its "multitude of frivolous particulars," and in particular for "le[aving the reader] totally in the dark" as to the "rationale of the many singular customs of these islanders."[59]

As characterized by Boswell and in the preface to his account of the second voyage, Cook had insistently allied himself with particulars, with the authority and accuracy of unmediated firsthand observation. In practice, Cook was an accomplished former of hypotheses and synthesizer of facts. It is important to note that his objection to Hawkesworth was that he drew "a general conclusion from **a** particular" rather than from a number of them. Cook certainly did not refrain from speculative synthesis, though, as Forster's biographer Hoare observes, he flags it as such: his account of the formation of icebergs is clearly speculative, though given credibility by his concluding reminder that it "is written wholy from my own observation." Moreover, his entire second voyage can be seen as a nautical fact-collecting expedition to prove his scientific hypothesis that no southern supercontinent existed.[60]

Cooks editor Beaglehole asserts that Cook, throughout his authorial career and especially after Hawkesworth's treatment of his text, was compelled to see to it that any reader of his journals should see things as he had.[61] His second and third voyage journals thus differ in some ways from his first. Perhaps recalling the £6,000 earned by Hawkesworth, Cook became increasingly "authorial" when he recorded the events of his subsequent voyages. On his second voyage, for instance, he occasionally referred to "the reader" and kept several concurrent manuscript journals, revising constantly; it is difficult to describe the resulting records as made "on the spot." By the third voyage, he writes consistently in civil time rather than ship's time, with dates confined to the margins; suppresses most nautical details; and writes with reasonable correctness.[62] He was no longer writing a private document for the Admiralty but was composing a narrative for the reading public.

Cook the scientist also became more "literary." Though on his first voyage Cook had copied Sydney Parkinson's drawing of the naturally arched rock in

Tolaga Bay, his written text had ignored the natural masterpiece whose "romantic" prospects had charmed Parkinson and his employer Banks.[63] In contrast, on his second voyage Cook verbally described for his reading public the "curious and romantick Views many of these [Antarctic ice] islands exhibit and which are greatly heightned by the foaming and dashing of the waves against them and into the several holes and caverns which are formed in the most of them, in short the whole exhibits a View which can only be discribd by the pencle of an able painter and at once fills the mind with admiration and horror." As on his first voyage, Cook attempts the description with his pen. But on this voyage, he asserts that his words desert him as he aspires to the sublime, to describe "Lands doomed by nature to everlasting frigidness and never once to feel the warmth of the Suns rays, whose horrible and savage aspect I have no words to describe." While such statements of verbal inadequacy are less characteristic of the "early" Cook, they do increasingly pervade travel texts in the later part of the century.[64]

Cook the naturalist becomes more prominent as Cook the navigator disappears from his text. Although the introduction to the published account of the second voyage acknowledges the presence of Johann Reinhold Forster, the "person skilled in Natural History. . . engaged to accompany me," Cook's reaction to the Forsters' plan to publish a botanical account of the voyage suggests that he regarded them as competitors even in this field.[65] Cook had certainly done his homework. By the third voyage, for instance, he cites the zoologist Thomas Pennant's recently published *Synopsis of Quadrupeds* (1771) in a descriptive paragraph appended to his lengthy account of the edibility and social behavior of the arctic walrus. The passage also shows Cooks characteristic fondness for measurement and drawings, and his continuing concern with nomenclature, evident ten years earlier in his account of the "Egg bird" (66):

> *Pennant* in his *Syn. Quadr.* p 335 has given a very good discription of this Animal under the Name of Arctick Walrus, but I have no were seen a good drawing of one. Why they should be called Sea horses, is hard to say unless it be a corruption of the Russian name Morse, for it has not the least similitude to a Horse; It is without doubt the same Animal as is found in the Gulph of St Lawrence and there called Sea Cow, it is certainly more like a Cow than a Horse but this likeness consists in nothing but the Snout. In short it is an animal like a Seal but incomparably larger; the demensions and weight of one, which was none of the largest, were as follows. . . .[66]

And by the third voyage, Cook avoids using the adverb *exactly* to modify comparisons of unfamiliar plants and animals to familiar ones, as it had three

times in the *Endeavour* journal.[67] The quality (and of course the novelty) of Cooks naturalist observations are evinced by the fact that in 1784 Thomas Pennant hounded Cook's ghost editor John Douglas, seeking to use Cooks observations in his forthcoming *Arctic Zoology*.[68]

Although Cook had certainly rewritten parts of the journal of his first voyage, he obsessively revised the account of his second voyage. These revisions seem to deprive the text of the immediacy and authenticity signaled by the lacunae and lack of polish in his first voyage journal. One might be reminded that Dr. Johnson preferred Boswell's "Journal" over his "History" in his *Account of Corsica:* "Your history was copied from books; your journal rose out of your own experience and observation." But here Dr. Johnson is praising firsthand observation rather than condemning synthesis. Johnson transformed his own on-the-spot records into his *Journey to the Western Islands of Scotland,* "one of the main subjects" of which is what Johnson believed to be the imperative "transition from empirical recording to discursive reflection."[69] As Beaglehole has observed, Cook revised and synthesized in order to convey his perceptions as precisely as possible, so that he would never again be misrepresented.

And yet one final change in Cooks practice as a journalist signals his potential loss of integrity as a scientific observer. Having spent the relatively brief time between voyages in the company of professional scientists,[70] Cook further distances his observations from those of the common seamen by eschewing their terminology. On the first voyage, he had seen some "fish like skip jacks" (166); on the second voyage, he describes the locomotion of Antarctic penguins as "something like the Fish known to seamen by the Name of scip jacks."[71] This distancing continues on the third voyage. Curiously, all these "distancing" passages in the third journal are clustered in one of the first summary descriptions, of Kerguelen Island. But the explanation lies less in Cooks desire to define his bias as an observer than in his (here) acknowledged parroting of "Mr Andersons own words":

> one of those birds **which sailors call** Noddies settled on our rigging and was caught
>
> Seals (or **as we used to call them,** Sea Bears, being that sort called the Ursine Seal)[72]

The "Mother Caries" bird of Cooks first voyage (39, 161) is now "the small black [Petrel]" as well as "Mother Careys chicken"; "the largest of the petrels" is "**called by the Seamen** Mother Careys goose." Once Anderson's journal is no longer in front of him, Cook continues to distance himself from his men: "the most of them were of that sort **known to Seamen by the**

name of Elephant fish."[73] But Cooks failure to provide a term of his own and his subsequent failure to flag similar terms epitomizes a rather greater loss of integrity in his process of making and recording observations: the position of the observing subject is no longer in focus.

Cooks awareness of his reading public apparently caused him increasingly to violate one of the first principles of scientific observation--recording only what he himself had seen. It is well known that on his later voyages Cook continued to supplement his journals with the observations of fellow journalists, often but not always acknowledging the borrowings. Beaglehole has argued that "Cook was a discriminating borrower," that--for instance--his "discussion of the morals of the women of Tahiti . . . is Cook as well as Wales," but acknowledges in the introduction and through the texts of the journals that there is much that is "not of Cook." Moreover, Beaglehole admits that Cooks account of the interior of Easter Island is completely derived from both Pickersgill and Wales: Cook was ill and confined to the beach. While Pickersgill's report is acknowledged and reported directly, that of Wales is not.[74]

"Travel literature" embraced a variety of methodologies and subjects, and both the "old" Cook and the "new" had their place. But what Paul Carter has described as the "dissonance" between (and within) Cooks texts alerts their readers to the difficulty--much discussed in the period--of reconciling the conventions of several disciplines simultaneously. And perhaps the elusiveness of Cook for his more imaginative biographers can be attributed partly to the ever-shifting status of the subject--no longer just a navigator, not quite a naturalist.

NOTES

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1. Richard Hough, *Captain James Cook* (London, Sydney, and Auckland: Hodder and Stoughton, 1994), 19, refers to these "fanciful claims."

2. See Douglas Gray, "Captain Cook and the English Vocabulary," *Five Hundred Years of Words and Sounds: A Festschrift for Eric Dobson,* ed. E. G. Stanley and Douglas Gray (Cambridge, 1983), 49-62; Barbara M. H. Strang, *A History of English* (London: Methuen, 1970), 73; Carey McIntosh, *Common and Courtly Language: The Stylistics of Social Class in Eighteenth-Century English Literature* (Philadelphia: University of Pennsylvania Press, 1986), 105-114; Carol Percy, "In the Margins: Dr Hawkesworth's Editorial Emendations to the Language of Captain Cook's *Voyages,*" *English Studies* 77, no. 6 (November 1996), 549-578; Carol Percy, "English Normative Grammar in Practice: The Case of Captain Cook," *English Historical Linguistics 1994,* ed. Derek Britton (Amsterdam and Philadelphia: John Benjamins, 1996), 339-362.

3. Charles L. Batten, *Pleasurable Instruction: Form and Convention in Eighteenth-Century Travel Literature* (Berkeley, Los Angeles, London: University of California Press, 1978), 57, 85, 109.

4. Ibid., 44, 72-73.

5. John Bradley and Lidio Prescutti, *TACT 1.2* (University of Toronto, 1990). I am grateful to the Hakluyt Society, copyright holders of the Beaglehole edition of Cooks journals, for their permission to scan it and to quote from it here.

6. James Boswell, *Boswell: The Ominous Years 1774-1776,* ed. Charles Ryskamp and Frederick A. Pottle (New York, Toronto, and London: McGraw-Hill, 1963), 308.

7. Ibid., 308-309.

8. James Cook, *A Voyage Towards the South Pole, and Round the World* [ed. John Douglas], 2 vols. (London: W. Strahan and T. Cadell, 1777), 1:xxxvi. See also Percy Guy Adams, *Travel Literature and the Evolution of the Novel* (Lexington: University Press of Kentucky, 1983), 243-271; Barbara Maria Stafford, *Voyage into Substance: Art, Science, Nature, and the Illustrated Travel Account, 1760-1840* (Cambridge, Mass.: MIT Press, 1984), 46-52; Jonathan Lamb, "Minute Particulars and the Representation of South Pacific Discovery," *Eighteenth-Century Studies* 28, no. 3 (1995): 287.

9. *Critical Review: or, Annals of Literature* 43 (1777): 371-372.

10. Ibid.; *English Review, or an Abstract of English and Foreign Literature* 4 (1784): 281-297; *European Magazine, and London Review* 6 (1784): 290-292.

11. Poems, respectively, from *Geography Epitomised; or, a Tour Round the World: Being a Short But Comprehensive Description of the Terraqueous Globe: Attempted in Verse (for the Sake of the Memory) and Principally Designed for the Use of Schools* (Philadelphia and London, 1786), 46; and W. B., "Irregular Ode on the Death of Captain Cook," *Lady's Magazine* 21 (London, 1790): 100. James Cook, *The Voyage of the* Endeavour, *1768-1771,* vol. 1 of *The Journals of Captain Cook on His Voyages of Discovery,* ed. J. C. Beaglehole, Hakluyt Society extra series 34 (Cambridge: Cambridge University Press, 1955) [hereafter "Cook"], cxcii, quoted in Glyndwr Williams, " 'Far More Happier Than We Europeans': Reactions to the Australian Aborigines on Cook's Voyage," *Historical Studies* 19 (1981): 499.

12. The Maori anecdote is quoted in Helen Wallis, "Publication of Cooks Journals: Some New Sources and Assessments," *Pacific Studies* 1, no. 2 (1978): 194; Walter Besant, *Captain Cook* (London and New York: Macmillan & Co., 1890), 10-12, 32-36, 89; Kenneth Slessor, "Five Visions of Captain Cook," in *Trio: A Book of Poems,* by Kenneth Slessor, Harley Matthews, and Cohn Simpson (Sydney: Sunnybrook Press, 1931), n.p.; Marlies Thiersch, "Cook Plays Now and Then," in *Captain James Cook: Image and Impact; South Seas Discoveries and the World of Letters,* 2 vols., ed. Walter Veit (Melbourne: Hawthorn Press, 1972-1979), 1:46-49, mentioning in particular on p. 53 Shan Benson's *Voyage on a Dinner Table* (radio play, 1954).

13. Stafford, *Voyage into Substance,* xxi, 46, 381, 424-425.

14. Joseph Banks, *The* Endeavour *Journal of Joseph Banks, 1768-1771,* ed. J. C. Beaglehole, 2d ed., 2 vols. (1st ed. 1962; [Sydney]: Trustees of the Public Library of New South Wales in Association with Angus and Robertson, 1963) [hereafter "Banks"], 2: 6-7. See also Banks 1:153, 2:5, and 2:117.

15. Harold B. Carter, *Sir Joseph Banks, 1743-1820* (London: British Museum [Natural History], 1988), 72; Cook, cciv-ccvii, ccxiii-ccxiv.

16. Williams, " 'Far More Happier Than We Europeans,' " 505-506.

17. John Hawkesworth, *An Account of the Voyages Undertaken by the Order of His Present Majesty for Making Discoveries in the Southern Hemisphere,* 3 vols. (London, 1773), iv-v; Cook, 78, 82, 85.

18. E.g., "a gum **much like sanguis draconis**" (Banks 2:57); "Gum one sort of which is **like Gum Dragon** and is the same as I suppose Tasman took for gum lac" (Cook, 307). "The third was **of the Opossum kind and much resembling that calld by De Buffon Phalanger**" (Banks 2:117); c.f. "**Possums** . . . " (Cook, 367). Bold emphasis is used here and in subsequent quotations to draw attention to textual differences.

19. Quoted in Cook, cclxxxii.

20. See Cook, xix-xx, cclxv-cclxxi; and *Charts & Views, Drawn by Cook and His Officers and Reproducedfrom the Original Manuscripts,* ed. R. A. Skelton (Cambridge: Published for the Hakluyt Society at the University Press, 1955).

21. *Regulations and Instructions relating to His Majesty's Service at Sea. Established by His Majesty in Council,* 10th ed. (London, 1766), 158.

22. E.g., James Cook, *Directions for Navigating the Gulf and River of St. Laurence* (London, 1760); *Directions for Navigating on Part of the South Coast of Newfoundland* (London, 1766); *Directions for Navigating on Part of the North East Side of Newfoundland, and in the Streights of Bell-Isle* (London, 1766); J. Bevis, M.D., F.R.S., "An Observation of an Eclipse of the Sun at the Island of New-found-land, August 5, 1766, by Mr. James Cook, with the Longitude of the Place of Observation Deduced from It," *Philosophical Transactions . . . for the Year 1767,* 57 (London, 1768): 215-216.

23. J. C. Beaglehole, *The Life of Captain James Cook,* completed by T. H. Beaglehole (Stanford, Calif.: Stanford University Press, 1974); John Graves, *The History and Antiquities of Cleveland, in the North Riding of the County of York* (Carlisle, 1808), 199.

24. Cook, cvii-cviii.

25. Quoted in Cook, cclxxix-cclxxxiv, 514-519.

26. Mr. Boyle, "General Heads for a Natural History of a Countrey, Great or Small," *Philosophical Transactions* 11 (2 April 1666): 186-189.

27. See Brian Vickers, "The Royal Society and English Prose Style: A Reassessment," *Rhetoric and the Pursuit of Truth: Language Change in the Seventeenth and Eighteenth Centuries,* ed. Thomas F. Wright (Los Angeles: William Andrews Clark Memorial Library, 1985), 1-76; Adams, *Travel Literature,* 243-271.

28. Cook, 66; Carter, *Sir Joseph Banks,* 72.

29. The roots of "wild Yamms or Coccos" were "so Acrid [MS M, *bad*] that few besides my self could eat them" (Cook, 353). The Mitchell manuscript (MS M) is a transcript of Cooks journal by his clerk Richard Or-ton. It was sent by Cook from Batavia to England in 1770 and is now in the Mitchell Library, Sydney. See Cook, xix, ccxviii-ccxxi. The toadfish incident may have been rewritten with hindsight by George Forster. Cook claims that it was eaten "without the least suspicion of its being a poisonous quality"; Johann Reinhold Forster's journal notes that "it was a kind of *Tetrodon*" but describes it neutrally as "a fine large fish"; only his son George's published account claims that the diners had realized beforehand that "several species" of Tetrodon "are reckoned poisonous." See James Cook, *The Voyage of the* Resolution *and* Adventure, *1772-1775,* vol. 2 of *The Journals of Captain James Cook on His Voyages of Discovery,* ed. J. C. Beaglehole, Hakluyt Society extra series 35 (Cambridge: For the Hakluyt Society at the University Press, 1961), 535; Michael E. Hoare, *The Tactless Philosopher: Johann Reinhold Forster* (1729-98) (Melbourne: Hawthorn Press, 1976), 121; Johann Reinhold Forster, *The* "Resolution" *Journal of Johann Reinhold Forster 1772-1775,* ed. Michael E. Hoare, 4 vols. (London: Hakluyt Society, 1982), 4:649.

30. E.g., "very much like ducks" (166); "very much like rosin" (204); "something like a Pine Apple" (121); "something like Maple" (186). These figures have been obtained from searching the electronic texts of Beaglehole's edition of Cook's *Endeavour* journal.

31. Cook, 141, 153; James Cook, *The Voyage of the* Resolution *and* Discovery, *1776-1780, Part One,* vol. 3 of *The Journals of Captain James Cook on His Voyages of Discovery,* ed. J. C. Beaglehole, Hakluyt Society extra series 36 (Cambridge: Published for the Hakluyt Society at the University Press, 1967), 61, 62, 101.

32. "Directions for Sea-men, bound for Far Voyages," *Philosophical Transactions* 8 (8 January 1665/6): 140. Williams, " 'Far More Happier Than We Europeans,' " 499-512, especially 499, 501, and 507-509.

33. Hoare, *Tactless Philosopher,* 122; Banks 1:217; cf. Hawkesworth, *Account* 1:386.

34. J. M. Thomson, "The Significance of the Voyage of the *Endeavour* to Botany and Zoology," *The Significance of Cook's Endeavour Voyage: Three Bicentennial Lectures* (James Cook University of Northern Queensland, 1970), 30; William Dampier, *A Voyage to New Holland,* ed. James A. Williamson (London: Argonaut Press, 1939), 85; idem, *A New Voyage Round the World,* intro. Sir Albert Gray (London: Argonaut Press, 1927), 312.

35. E.g., Banks 1:174.

36. Beaglehole points out that since rabbits had not yet been introduced to Australia, it is impossible from either description to establish just what had been seen (Banks 2:57n; Cook, 307n).

37. Thomson, "The Significance of the Voyage of the *Endeavour* to Botany and Zoology," 29-30.

38. Cook, 359n-360n, ccxviii-ccxxi.

39. Samuel Johnson, for instance, liked to measure things exactly and in a letter to Sophia Thrale asserted that "the computist" could dispel "a thousand stories which the ignorant tell"; see Richard B. Schwartz, *Samuel Johnson and the New Science* (Madison, Milwaukee, and London: University of Wisconsin Press, 1971), 3132; and Batten, *Pleasurable Instruction,* 72.

40. Cook "intended chart and verbal description to be read together" (cclxv); the Forsters "described . . . both in word and drawing" "new and various miracles of nature," quoted in Hoare, *Tactless Philosopher,* 89. See also Cook, *A Voyage Towards the South Pole* 1:xxxiv; Cook, Resolution *and* Discovery, *1776-1780,* 61, 165, 308; 109 and 131 relate the difficulties of describing dance.

41. Hoare, *The Tactless Philosopher,* 163; letter quoted in Cook, Resolution *and* Adventure, *1772-1775,* cxlii.

42. Quoted in Cook, Resolution *and* Adventure, *1772-1775,* cxliii. Stafford, *Voyage into Substance,* 405, 421-426.

43. Thispractice is demonstrated in a paper summarizing the results of worldwide observation of the transit of Venus, 3 June 1769: Don José Joaquin de Ferrer, "On the Determination of the Parallax of the Sun from the Observations of the Transit of Venus over His Disk, June 3, 1769," communicated by M. Cerquero through Sir James South, *Memoirs of the Royal Astronomical Society* 5 (1833): 253-296. For an example of Cooks own practice, see Cook, Resolution *and* Adventure, *1772-1775,* 78; Bevis, "An Observation," 215-216.

44. Rupert Thomas Gould, *Captain Cook* (1935; rpt. London: Duckworth, 1978), 55; Hoare, *Tactless Philosopher,* 124.

45. Stafford, *Voyage into Substance,* 260, 430.

46. Batten, *Pleasurable Instruction,* chap. 2; Stafford, *Voyage into Substance,* 440.

47. Cook, 222, quoted in Paul Carter, *The Road to Botany Bay: An Essay in Spatial History* (London and Boston: Faber and Faber, 1987), 28-29.

48. Ibid., 28.

49. Cook, *South Coast of Newfoundland,* 8-9.

50. Hawkesworth, *Account* 1:432 [Wallis]; Banks 1:249; Hoare, *Tactless Philosopher,* 96 [Forster]; Cook, 72-73. The rest of Forster's description, however, is more imaginative, illuminating his great interest in vulcanism.

51. Cook, 69-72.

52. Cf. Cook, cxciii. Stafford, *Voyage into Substance,* xxi; Cook, 346; see also Cook, Resolution *and* Discovery, *1776-1780,* 187.

53. Cook, *A Voyage Towards the South Pole* 2:34.

54. Cook, Resolution *and* Adventure, *1772-1775,* 462, 466.

55. William Hodges's "Man of Malekula," in Cook, Resolution *and* Adventure, *1772-1775,* fig. 71a, facing 576; J. Caldwell's engraving in Cook, *A Voyage Towards the South Pole* 2:34.

56. Beaglehole, *The Life of Captain James Cook,* 456-471; Hoare, *Tactless Philosopher,* 151-162; Wallis, "Publication of Cook's Journals: Some New Sources and Assessments," 163-194.

57. Beaglehole, *Life,* 462, 467; Hoare, *Tactless Philosopher,* 156-157.

58. Cook, *A Voyage Towards the South Pole.*

59. George Forster, *A Voyage Round the World in His Britannic Majesty's Sloop, Resolution,* 2 vols. (London: B. White, J. Robson, P. Elmsley, 1777), viii-xii; Schwartz, *Samuel Johnson and the New Science,* 67. Reviews of Hawkesworth quoted in Lamb, "Minute Particulars," 292.

60. Hoare, *Tactless Philosopher,* 126; Cook, Resolution *and* Adventure, *1772-1775,* 643-646.

61. J. C. Beaglehole, *Cook the Writer: The Sixth George Arnold Wood Memorial Lecture* (Sydney: Sydney University Press, 1970), 13.

62. J. G. Forster, author of the competing account of Cook's second voyage, makes this point to discredit Cook: "Dr Hawkesworth's publication which had been sent to meet Capt Cook at the Cape, with the news of the prodigious profits of the compiler, inspired him with the desire of becoming an author." *A Letter to the Right Honourable the Earl of Sandwich, First Lord Commissioner of the Board of Admiralty, &c.* (London, 1778), 12. Beaglehole notes that Cook himself mentions money in a letter to a friend: "I am to have the sole advantage of the sale." Cook, Resolution *and* Adventure, *1772-1775,* cxliii. For Cooks changing authorial practice, see also Resolution *and* Adventure, *1772-1775,* cxv-cxxiii; Beaglehole, *Cook the Writer,* 13; Cook, Resolution *and* Discovery, *1776-1780,* clxxi-clxxvi.

63. See Cook, cclxv--cclxvi, 187n. Add. MS 7085, British Library, London, contains Cooks copies of originals by Sydney Parkinson.

64. Cook, Resolution *and* Adventure, *1772-1775,* 98-99, 646; Batten, *Pleasurable Instruction,* 107-109.

65. Cook, *A Voyage Towards the South Pole,* xxxiv; Hoare, *Tactless Philosopher,* 140.

66. Cook, Resolution *and* Discovery, *1776-1780,* 420-421.

67. On the first voyage, *exactly (. . .) like* occurs three times, in descriptions of "a Sea lyon" (234), "quals" (276), and "Crows" (311). On the third voyage, *exactly (. . .) like* occurs five times (Cook, Resolution *and* Discovery, *1776-1780*), but in descriptions of buildings (270, 412), armour (280), coastal topography (425), and "a new Malt beer" (478).

68. Wallis, "Publication of Cooks Journals," 183.

69. Schwartz, *Samuel Johnson and the New Science,* 66, 74.

70. See, for example, D. E. Allen, *The Naturalist in Britain: A Social History* (London: Allen Lane, 1976), 45.

71. Cook, Resolution *and* Adventure, *1772-1775,* 69.

72. Cook, Resolution *and* Discovery, *1776-1780,* 43, 44.

73. Ibid., 45, 45, 51.

74. E.g., Cook, Resolution *and* Adventure, *1772-1775,* cxxi-cxxii; see also Carter, *Botany Bay,* 18.

HOW MANY HAWAIIANS LIVE IN HAWAIʻI?

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Lately, the size of Hawaiʻi's precontact population has again become a matter of passionate debate, with estimates ranging from Dye's 110,000-150,000 to Stannard's 8000,000-1.5 million. A less publicized but similarly vexing question has recently surfaced with respect to the *current* Hawaiian population of the state. The 1990 census failed to distinguish between pure and part Hawaiians, and reported different totals by ancestry and by race. Growing numbers of part Hawaiians either misclassify themselves as either pure Hawaiian or non-Hawaiian. The lack of accurate population data makes it almost impossible to calculate meaningful birth, death, crime, unemployment, poverty, and similar rates by race.

SEVERAL YEARS AGO, David Stannard revived an old controversy: how many Hawaiians were living on these islands in 1778, when Captain James Cook and his officers reported the first known contact between Europeans and the Islanders? Stannard, in a vigorously argued, take-no-prisoners monograph, *Before the Horror,* asserted that the true figure was at least 800,000, and perhaps as high as 1.5 million (Stannard 1989:31, 37). Previous estimates had ranged from Bucks 100,000-150,000 through King's revised estimate of 400,000 to King's original guess of 500,000. Most clustered between 200,000 and 300,000 (Schmitt 1968:18-22).

A number of interested persons joined the ensuing debate. Many Hawaiian activists, revisionist mainland authorities, and reviewers supported Stannard, some quite enthusiastically. Objections, mostly indirect or muted, came from island demographers, anthropologists, and historians. Dye offered an alternate estimate, based on a sophisticated analysis of radiocarbon dating, of

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110,000-150,000 (Dye 1994:1-20). As observed by one critic, Andrew Bushnell, "*Before the Horror* has provided a new orthodoxy for many who are either unable or unwilling to critically evaluate its assumptions" (Bushnell 1993:116). In any event, the question is still unresolved, and will undoubtedly remain so for many years.

Less publicized but equally vexing, a similar controversy exists at the other end of the time scale: how many Hawaiians live in Hawai'i today? Is the true total the U.S. Census count of 138,742 listed as "Hawaiian" in the 1990 *racial* statistics? Or is it the corresponding Bureau of the Census 1990 total of 156,812 persons of Hawaiian *ancestry?* Both of these census totals, moreover, differ radically from the State Health Department's 1990 sample survey finding of 8,711 unmixed Hawaiians and 196,367 part Hawaiians. All of these figures, it should be stressed, exclude ethnic Hawaiians living in other states, numbered at 72,272 in the 1990 census (Hawaii State Department of Business, Economic Development and Tourism 1994:37, 39-41).

One might well ask: if we can't more accurately establish the number of Hawaiians living in the islands in 1990, when a full census of the population was conducted, how closely can we estimate the number in 1778, more than half a century before any complete headcount was attempted?

The major problem in earlier years seems to have been the misclassification of part Hawaiians as full-blooded. In 1937, pioneer University of Hawai'i demographer Bomanzo Adams estimated from vital statistics that 9,780 persons classified in the 1930 census as pure Hawaiians were actually part Hawaiians. As a consequence, the number of unmixed Hawaiians in the islands in 1930 was actually 12,856 instead of 22,636, and the number of part Hawaiians was 38,004 rather than 28,224 (Adams 1937:15). An unpublished study by W. A. Lessa, who attempted to interview every pure Hawaiian living in the Territory of Hawai'i in 1930-1932, turned up only about 1,700 truly unmixed Hawaiians (Lessa 1964). And a 1967 analysis of population genetics by Newton E. Morton, Chin S. Chung, and Ming-Pi Mi concluded that the avowedly pure Hawaiians registered at the Honolulu Blood Bank between 1948 and 1958 averaged 8.5 percent Caucasian admixture and 13.7 percent Chinese admixture (1967:13, 26, 29, 127).

More recently, many persons of part-Hawaiian ancestry apparently have been classified as non-Hawaiians, certainly in census statistics and less frequently in data from the Hawai'i State Department of Health. This conclusion necessarily follows any comparison of census and Health Department data.

The 1960 decennial census was the last to show separate statistics for "Hawaiians" and "part Hawaiians." Thereafter, no "part Hawaiian" category was provided, and persons of mixed background were asked to mark the

race they most closely identified with. Those who could or would not make such a choice were instructed in 1970 to use the race of their father, and in 1980 and 1990 to indicate mother's race. The number of "Hawaiians" accordingly rose from 11,294 in 1960 (when there were also 91,109 "part Hawaiians") to 71,274 in 1970, 115,500 in 1980, and 138,742 in 1990 (Schmitt 1977: 25-26; Hawai‘i State Department of Business, Economic Development and Tourism 1994:39).

In addition to these "racial" data, the 1980 and 1990 censuses included information on "ancestry." Respondents were asked to indicate either one or two ancestral backgrounds; entries with three or more ancestries were tabulated only for the first two mentioned. The 1990 data showed 156,812 persons who had marked Hawaiian, either alone or in combination with another ancestral group (Hawai‘i State Department of Business, Economic Development and Tourism 1994:41).

The Hawai‘i Health Surveillance Program (HHSP), a large ongoing sample survey conducted by the Hawai‘i State Department of Health from 1964 to 1994, handled the racial question in a different fashion, by requesting the race of each respondent's four grandparents. Provision was made for "Hawaiian" and "part Hawaiian" as well as other races. Thus, any person who had at least one grandparent classified as part Hawaiian shared that racial classification. Only those with all four grandparents reported as Hawaiian were considered to be unmixed. The 1990 HHSP found 8,711 allegedly pure Hawaiians and 196,367 part Hawaiians (Hawai‘i State Department of Business, Economic Development and Tourism 1994:37).

Although the Health Department's data seem to be more objective than corresponding census figures, neither source appears to be wholly credible. It seems probable, from inspection of the Adams, Lessa, and Morton studies cited earlier, that some part Hawaiians are continuing to claim unmixed ancestry. It is also likely (although less readily proved) that many part Hawaiians are becoming so fractional in ancestry as to ignore or be unaware of their drop of Hawaiian blood.

Birth statistics for Hawai‘i provide some evidence regarding these points. In 1991, 6,620 babies were born with at least some known Hawaiian ancestry, but only 21 of them were totally unmixed. More than half, or 3,672, had one non-Hawaiian parent, and 745 others had one parent of unknown race (Hawai‘i State Department of Health 1993). The net result was a sharply increased average proportion of non-Hawaiian blood in these part-Hawaiian babies.

Similar evidence comes from the enrollment records of the Kamehameha Schools, which admit only students with at least some documented Hawaiian ancestry. The percentage of pure Hawaiian students dropped from 7.3 in

1929 to 0.1 in 1992-1993. Those with one-eighth or less Hawaiian blood rose during the same sixty-four-year period from 3.4 percent of the total to 36.1 percent (Hawai'i State Department of Business, Economic Development and Tourism 1994:40).

Careful review of these sources inevitably leads to the following conclusions regarding the state's current Hawaiian population:

1. The 1990 census greatly understates the Hawaiian population, in both its racial and ancestry tabulations. Not only does it fail to distinguish between "pure" and part Hawaiians, but it apparently assigns about one-third of all part Hawaiians (based on HHSP racial data) to a non-Hawaiian category.
2. The Hawaii Health Surveillance Program, although subject to sampling variation not characteristic of the census full-count data, is more forthright about race mixture, and thus a better guide to both pure and part Hawaiian categories. Even so, it probably overstates the number of pure Hawaiians and underreports the part-Hawaiian group.
3. The unmixed Hawaiian population has declined so precipitously as to have reached the edge of extinction. This conclusion stems from consideration of HHSP data, the findings of Adams, Lessa, and Morton cited above, and the minuscule totals for pure-Hawaiian births recorded in recent years.
4. Calculating accurate demographic and social rates for Hawaiians and part Hawaiians is becoming progressively more difficult and perhaps impossible, at least when using census totals as denominators. Precision in comparative birth, death, crime, unemployment, poverty, and similar rates is an unattainable goal, particularly for Hawaiians.
5. Nobody knows exactly how many Hawaiians lived here in 1778. Nobody knows, with satisfactory accuracy, how many live here today.

NOTE

The author was formerly State Statistician with the Hawai'i State Department of Business, Economic Development and Tourism and its predecessor agencies. This paper has been updated and extensively revised from two earlier versions (Schmitt 1967; Hawai'i State Department of Planning and Economic Development 1971).

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DEMOGRAPHIC CHANGE IN THE REPUBLIC OF PALAU

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With the onset of sustained contact with Europeans more than two centuries ago, the islands currently composing the Republic of Palau experienced substantial population change. The following study examines the demographic evolution of these islands, considering changes in total population and shifts in the geographic distribution of population. The study first discusses interaction with cultures from outside Oceania and the demographic impacts of this interaction. It then explores shifts in the regional distribution of population, focusing on data from censuses conducted between 1920 and 1990. Data on fertility, mortality, and mobility provide clues to possible causes of population change in the Republic of Palau over the past two centuries. An examination of the ecological, economic, and sociocultural repercussions of demographic changes point to fundamental development challenges facing this small island nation.

OF THE MANY CHANGES that have occurred throughout Micronesia following the onset of sustained interaction with people from outside Oceania, one of the most important in terms of its effect on native cultures has been demographic change. For those islands that established frequent contact with outsiders, the same demographic scenario usually emerged. Shortly following the beginning of this contact, a period of depopulation commenced --generally due to diseases introduced to native peoples with no previous immunity. Eventually depopulation ceased, most often as a result of increased survivability brought about through improved health care and increased natural immunity. The continued decline in mortality ultimately led to an imbalance with fertility, resulting in population growth throughout much of Micronesia (see Taeuber 1963). As demographic trends shifted from depop-

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ulation to population growth, demands on the natural environment and socio-cultural systems changed accordingly--generating various adjustments as native cultures attempted to cope with fundamentally different adaptive challenges.

The above scenario holds, with some modifications in timing and other specifics, for most major island groups in Micronesia (see Gorenflo and Levin 1991, 1992, 1994; Gorenflo 1993b, 1995). This article examines demographic change in the Republic of Palau, a collection of islands that experienced particularly severe depopulation over a prolonged period of time and that has witnessed only modest population growth during the current century.[1] The study begins with a brief overview of interaction between residents of these islands and people from outside Micronesia, providing the historical foundation necessary to understand the demographic impact of outsiders. Attention then shifts to a discussion of available population data, focusing particularly on eleven censuses conducted between 1920 and 1990. Through examining information on mortality, fertility, and mobility the study attempts to isolate the cause(s) of documented population change. Through examining supplemental information on cultural and economic change, in turn, the study explores repercussions of the demographic evolution of Palau. I conclude by considering the role of recent population change on the future of this emerging island nation.

The History and Demographic Impacts of Non-Micronesians

The Republic of Palau consists of approximately two hundred islands lying at the western edge of the Caroline Archipelago in the west-central Pacific Ocean (Shinn 1984:341-342). The islands that compose the republic are distributed along a northeast-southwest axis roughly 450 miles in length. The main island group in the republic, called the "Palau Islands," contains most of Palau's 171 square miles of land area as well as the majority of its population. The main components of the Palau Islands are Babeldaob, a volcanic island that comprises most of the land area of Palau, volcanic Arakabesan, Koror, and Malakal islands immediately south of Babeldaob, and the coral and limestone islands of Peleliu and Angaur lying south-southwest of the last three (Chief of Engineers 1956:27-31; Soil Conservation Service 1983:1-2). A barrier reef up to twelve miles wide encloses all of the Palau Islands except for Angaur (Force 1960:21; Johannes 1981:1). The remainder of Palau consists of coralline outer islands. Six of these--Dongosaro (formerly Sonsorol), Fanna (Fana), Hatohobei (Tobi), Helen Reef, Meleih (Merir), and Puro (Pulo Anna)--form the "Southwest Islands," a name reflecting their location with respect to the Palau Islands. The other outer island,

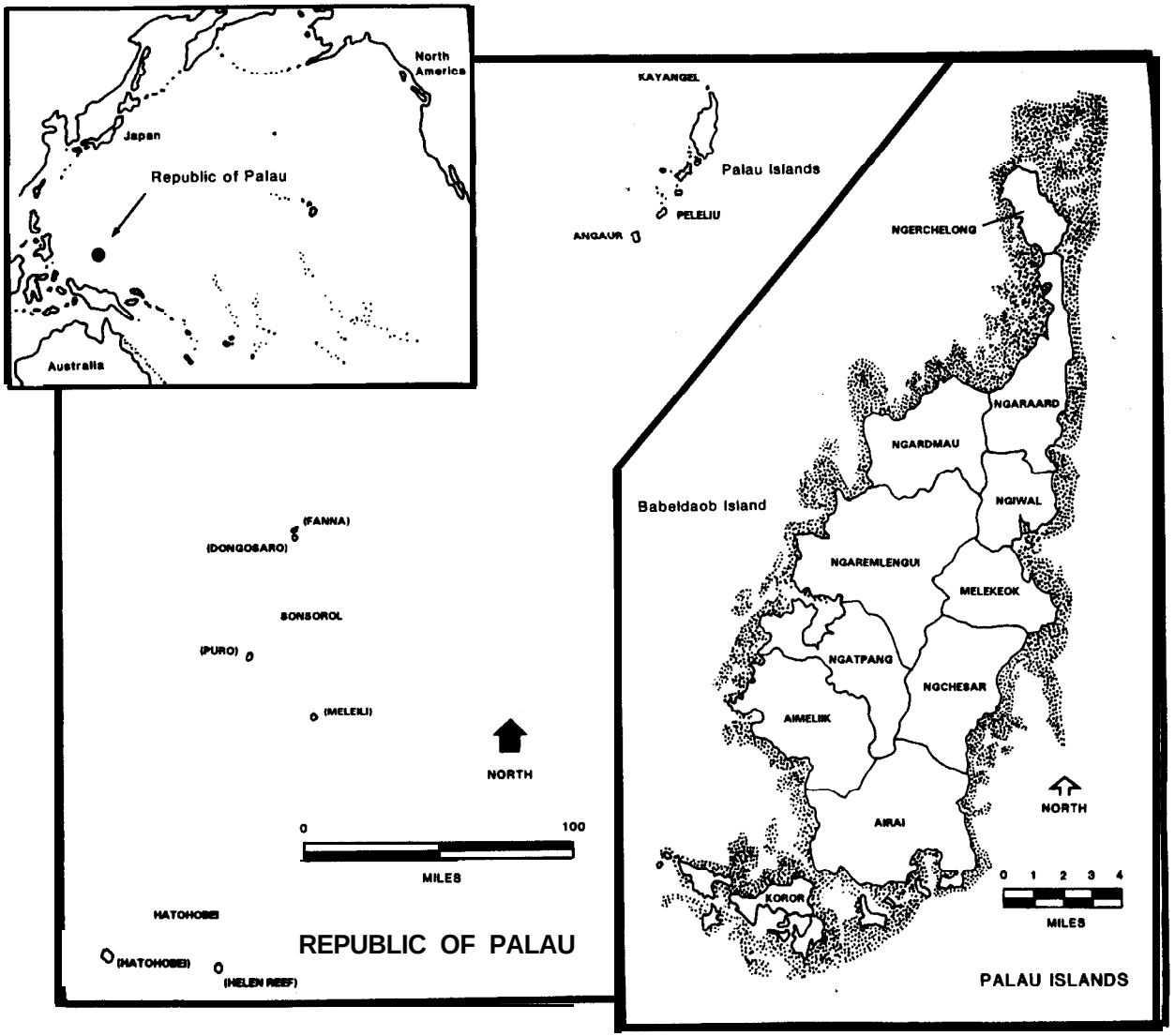


FIGURE 1. **The Republic of Palau (inset of Palau Islands excludes Peleliu and Angaur).**

Kayangel Atoll, lies immediately north of Babeldaob (see Gressitt 1952). In the current political organization of Palau, the outer islands compose three states in the republic: Hatohobei (Hatohobei Island and the usually uninhabited Helen Reef), Kayangel, and Sonsorol (Dongosaro, Fanna, Meleili, and Puro islands). Koror, Malakal, and Arakabesan islands form another state, called Koror. The remaining twelve states in the Republic of Palau consist of divisions of Babeldaob (Figure 1).

People originating in the Philippines, eastern Indonesia, or New Guinea began to colonize the Palau Islands possibly as early as 3000 B.C. (Osborne 1966:464), although proposed dates of early prehistoric occupation vary widely and tend to favor the first millennium B.C. (see Osborne 1966:1-2, 464; Bellwood 1979:281-283; Kiste 1984:14; Lucking 1984:22-23; Masse, Snyder, and Gumerman 1984:123-124; Nero 1987:4; Morgan 1988:3). Intermittent colonization likely occurred throughout the ensuing millennia, with migrants from the Southwest Islands coming to the Palau Islands as recently as A.D. 1600-1750 (Osborne 1966:451). The Southwest Islands themselves, in contrast, probably were settled more recently from the east--possibly as late as the sixteenth century A.D.--a scenario consistent with the general wave of Micronesian colonization that flowed westward from the Marshall Islands or Kiribati as well as the oral traditions of the islanders themselves (see Black 1977:21-23; McKnight 1977:17). Over the centuries complex societies evolved in the Palau Islands, as indicated by the remains of elaborate systems of terraces and roads (Kubary 1873:193; Osborne 1966:150-155, 1979:269; Morgan 1988:2-16). Some consider such elaborate archaeological remains as evidence of large prehistoric populations (de Valencia 1892: 402-403; Chief of Engineers 1956:14).[2]

For European nations active in Oceania during the sixteenth and seventeenth centuries, the legend of Palau's existence tended to precede firsthand knowledge. The Spaniard Gonzalo Gómez de Espinoza, commanding the *Trinidad,* apparently sighted Dongosaro Island in 1522 shortly after he and what remained of Magellan's expedition left the Philippines to return to Europe (Hezel and del We 1972:26). Despite Spanish explorer Ruy López de Villalobos's claim that he discovered the Palau Islands in 1543 (see Burney 1967:231; Office of the Chief of Naval Operations 1944b:21-22), the English privateer Sir Francis Drake and his crew were probably the first Europeans actually to land on these islands, in 1579 (see Lessa 1975). These early visits notwithstanding, the "Pelews" (as the Caroline Islands east of the Philippines came to be called during the seventeenth century) were known more from the accounts of Carolinian castaways in the Philippines than from European experience. Information provided by two groups of castaways in the second half of the 1600s generated sufficient interest for the

local Spanish authorities to organize a series of voyages in search of these islands (see Hezel and del Valle 1972:28-29; Palau Community Action Agency 1976:85-86). Eight such voyages occurred between 1697 and 1711, with success finally coming in late 1710 when Francisco Padilla and the crew of the *Santissima Trinidad* sighted Dongosaro Island and the Palau Islands (Krämer 1917:34-58; Eilers 1935:1-10). A voyage in 1712, led by Bernardo de Egui aboard the Santo Domingo, also reached the Palau Islands and Dongosaro (Krämer 1917:74-101; Eilers 1935: 15-16; Hezel 1983:46-47).

European ships remained rare in the western Carolines throughout the early and mid-eighteenth century. British ships (predominantly) began to appear in the Southwest Islands by the 1760s (Hezel 1983:63), but no noteworthy interaction with islanders from Palau occurred until 1783. In August of that year, the British East India Company's ship *Antelope* struck a reef southwest of Koror. Captain Henry Wilson and his crew of fifty survived, remaining on the Palau Islands under the protection of the paramount chief (the Ibedul) of Koror for the following three months. Wilson's account provides an invaluable glimpse at a society that had experienced little contact with cultures from outside the Pacific (see Keate 1788). In addition, despite the short duration of their stay the *Antelope* castaways had important cultural impacts on Palau, introducing Western items (including firearms) as well as assisting the people of Koror in their battles with other native polities. In the hope of promoting trading ventures in Oceania and Asia, the East India Company sent additional ships to Palau beginning in the early 1790s (see Hezel 1983:76-80). But trading on the scale envisioned never materialized, and despite continued visits by ships from England as well as other countries throughout the early 1800s, Palau began to lapse into obscurity (Office of the Chief of Naval Operations 1944b:23; Hezel 1983:179-180). Even the British, French, and Russian scientific expeditions into the Pacific during the late 1700s and first half of the 1800s failed to shed much light on Palau (see Meares 1790:296-297; Dumont d'Urville 1835:509, 1843: 208-209).

On the rare occasion that outsiders visited Palau during the early nineteenth century, hostility on the part of the islanders often precluded systematic observations--such as the attack on the British whaleship *Syren* by Angaur Islanders in 1823 (Ward 1966:144-149) and the mistreatment in 1832-1834 on Hatohobei of survivors from the wrecked American whaleship *Mentor* (Martin n.d.). The presence of traders, often an important source of early information on Micronesia, was intermittent throughout the early nineteenth century. The first systematic trading ventures were by the Spanish, who searched the Palau Islands in the early 1800s for bêche-de-

mer (Hezel 198383). A resurgence in trading began in the 1840s, led by Scotsman Andrew Cheyne, with subsequent activity by an Australian entrepreneur named Edward Woodin, a German captain called Alfred Tetens, and a German trader-adventurer named Eduard Hernsheim (Tetens 1958; Purcell 1967:37; Shineberg 1971:14, 231-241; Hezel 1983:181-195). But by the 1880s Palau once again had fallen into obscurity, as European trading attention turned toward Yap.

Despite the discontinuous nature of European contact with Palau during the eighteenth and nineteenth centuries, many changes on these islands occurred. As discussed in detail below, one of the most notable was depopulation, with the 20,000-100,000 islanders who possibly inhabited Palau in 1783 declining to roughly 4,000 a century later (see Kubary 1885145; Semper 1982:290).

Ignoring any possible British claims to Palau, Spain had claimed sovereignty over all the Caroline, Marshall, and Mariana Islands since the sixteenth century. As a reaction to growing trading activities throughout Micronesia during the second half of the nineteenth century, Spain established a more active presence in the area beginning in the 1880s. In the case of Palau, this presence was limited primarily to missionary activity (Office of the Chief of Naval Operations 1944b:24, 69; Vidich 1949:53-57; Force 1960:70-71; Force and Force 1972:5). In 1886, shortly after Pope Leo XIII's confirmation of Spain's sovereignty over the Carolines, Spanish Capuchins visited Palau, promising to return once established on Yap; two missionaries eventually arrived in April 1891 (de Valencia 1892:394; Hezel 1991:195-196). Despite their hard work, the Spanish missionaries enjoyed little success in Palau. With such a limited presence, it is not surprising that few demographic impacts occurred during the Spanish administration, apart from a likely reduction in mortality through reinforcement of the ban on native warfare imposed by the British in 1883 (Palau Community Action Agency 1978:iii; Vidich 1980:153-157; see also Kubary 1885:140-141). Depopulation nevertheless *apparently* continued during the Spanish period, with estimates placing the total population somewhere between 3,000 in 1892 (de Valencia 1892:403) and "considerably over 3,000" in 1896 (Christian 1899:16).

After decades of commercial activity in Micronesia by German traders, Germany purchased the Caroline Islands from Spain in 1899 (Fischer and Fischer 1957:47; Brown 1977). As elsewhere in the Pacific, Germany's interest in Palau was mainly commercial, focused primarily on copra production, trepang and shell gathering, and phosphate mining (Vidich 1949:58; Chief of Engineers 1956:5; see also Schnee 1920:352). The Germans introduced key changes to Palauan culture, preparing the way for modernization through modifications of native social and political order designed to benefit selected

trading interests (Hezel 1995:111-124; see also Office of the Chief of Naval Operations 1944b:24-25; Force 1960:71-72; Force and Force 1972:5). Although, the Germans required each adult male to plant coconut trees (see Abe 1986:68), as they did elsewhere in Micronesia, ultimately copra production throughout most of Palau fared poorly in comparison to other parts of the Pacific (see Krämer 1926:42-44). But mining phosphate deposits on Angaur Island, beginning in 1909, compensated for the loss in copra income (Office of the Chief of Naval Operations 1944b:29; Purcell 1967:44; Abe 1986:100-102). To administer its Micronesian possessions, Germany established a network of government offices on main islands throughout the Carolines, with a branch office established in Koror in 1905 and another opened on Angaur in 1910 (Office of the Chief of Naval Operations 1944b:24-25; Vidich 1949:58; Kaneshiro 1958:308). In addition to administration, the Germans built roads, conducted numerous studies of Palau, and in general attempted to improve the lives of the native population. On the whole, Germany succeeded in establishing various programs that led to economic development in Micronesia. Nevertheless, because much of the German effort focused on other parts of their Micronesian empire (particularly Yap, the Marshalls, and the Northern Marianas), development in Palau lagged behind.

Although relatively few Germans relocated to Palau during the early 1900s, important demographic changes occurred. Probably the most important was the likely end of depopulation that had plagued Palau for more than a century. Although available demographic estimates paint a confusing picture, Palau's population fluctuated around 4,000 during the German administration, with figures ranging from as low as 3,000 in 1907 (Hermann 1909:634) to as high as 4,543 in 1914 (Yanaihara 1940:42).

Japanese traders also had been active in Palau since the 1880s, providing the main competition for their German counterparts (Office of the Chief of Naval Operations 1944b:29; Purcell 1967:20; Abe 1986:109-110; Peattie 1988:23-24). When Germany became involved in World War I, Japan quickly moved to take control of all German possessions in Micronesia--on 7 October 1914 landing a small military garrison on Koror, and occupying Angaur and its phosphate mines two days later (Palau Community Action Agency 1978:304; Peattie 1988:43). The Japanese began organizing these newly occupied lands almost immediately, making Palau one of five naval districts established in the area. The League of Nations eventually recognized the Japanese administration of Micronesia in 1920, granting Japan a Class C Mandate for the Carolines, Marshalls, and Northern Marianas (Clyde [1935] 1967). In 1922 Japan formed the Nan'yo-cho (South Seas Bureau) to administer the Mandated Territory, establishing its capital at Koror that same year (Abe 1986:113-114; Peattie 1988:68).

In contrast to Germany's commercial emphasis, Japan viewed Micronesia as an integral part of its expanding Pacific empire, investing considerable funds in development and introducing a much more elaborate colonial administration (Useem 1946:83; Purcell 1967: 161-162; Palau Community Action Agency 1978:316-317). To ensure Palau's integration within their overall plans, the Japanese combined efforts to generate broad commercial development with the introduction of Japanese culture and language to the native population (Useem 1946:65-67; Vidich 1980:188; Abe 1986:119-120, 144-151). During its administration, Japan expanded mining operations in Palau-beginning to mine phosphate on Peleliu, Hatohobei, and Dongosaro in addition to Angaur and introducing bauxite mining to Ngaremlengui (1936) and Ngardmau (1940) (see Office of the Chief of Naval Operations 1944b:158-159; Chief of Engineers 1956:6; Black 1977:25; Palau Community Action Agency 1978:326-331, 345; Abe 1986:127-131; Peattie 1988:67, 82-83). In addition, the Japanese promoted agriculture (on Babeldaob), commercial fishing, and limited industry (Chief of Engineers 1956:6; Abe 1986:140; Peattie 1988:170-174; Hezel 1995). The success of commercial fishing generated considerable immigration of fishermen to Koror during the early 1930s, mostly Okinawans, which in turn produced secondary economic growth of stores and other infrastructure. At the beginning of the Japanese administration Koror was a sleepy Micronesian settlement; by the mid-1930s it had become a bustling modem town and center of the Mandated Territory, complete with paved streets, a' radio station, a newspaper, and several shops (Price 1944:109-110; Vidich 1980:207-208; see also Force 1960:85).

As the late 1930s approached, Japan began to prepare Palau for possible military action. Although Angaur, Peleliu, and (especially) Babeldaob all provided good sites for military airfields, Palau's main strategic value was its proximity to the Philippines (Peattie 1988:232). General military preparations began during the late 1930s throughout much of the Palau Islands. By early 1941 the construction of military installations was well under way throughout Micronesia, with much of the work done by Korean laborers imported for that purpose (Peattie 1988:252-253). The Japanese built airfields on Babeldaob, Ngesebus (a small island immediately north of Peleliu), and Peleliu; two seaplane bases on Arakabesan; and a navy base on Malakal (Chief of Engineers 1956:7). Most troops were stationed on Babeldaob, although outer islands such as Hatohobei also hosted small garrisons (Black 1983:9). U.S. military forces fought their way westward through Micronesia in 1944 and began to turn their attention toward Palau. U.S. forces bombed locations on Arakabesan, Malakal, Babeldaob, and Koror from March through August 1944 (Hough 1950:14-15; Chief of Engineers 1956:12; Peattie 1988:

279). But the greatest American military efforts were saved for Angaur and Peleliu, both needing to be secured to protect the American flank on a planned, approach to the Philippines. The ferocious battle for Angaur lasted about one month, during September-October 1944 (Smith 1984:499-531); the even-bloodier battle for Peleliu lasted more than two months, between September and November of the same year (Hough 1950; Smith 1984:496-499, 532-575). After bypassing Japanese forces on Arakabesan, Babeldaob, Koror, and Malakal following their neutralization through heavy bombing (Chief of Engineers 1956:12), the fall of Peleliu on 26 November 1944 marked the end of the great World War II battles in Micronesia.

Although the Japanese administration of Palau lasted scarcely three decades, several noteworthy demographic changes occurred. The number of islanders residing in Palau grew very slowly, increasing by about 500 between 1920 and 1935 (Nan'yo-cho 1937). This modest increase occurred amidst dramatic overall population growth due to the immigration of Japanese and Okinawans--initially in pursuit of commercial opportunities and later as part of Japan's military preparations for World War II (Hezel 1995). By 1937, prior to the main military buildup in Palau, about 11,400 Japanese nationals lived there (Japan 1937:88). By 1943 the foreign population of Palau had reached approximately 27,500 persons (Chief of Engineers 1956:15). In preparation for anticipated battles, by 1944 the number of Japanese military personnel alone had increased to roughly 50,000 (Nero 1989: 120).

The United States began to administer Palau and other Micronesian island groups following the defeat of Japanese forces in 1944 and 1945. In 1947 Palau became part of the Trust Territory of the Pacific Islands (TTPI), a strategic area established by the United Nations with the United States named as "administering authority" (Shinn 1984:304-305). Although in some ways promoting a return to traditional ways, the U.S. administration introduced fundamental changes to Palau, including an overriding emphasis on democratic principles of government (Useem 1952a; Force and Force 1965). But for more than a decade following the onset of Trust Territory status, the United States focused little attention on and invested little money in its Micronesian responsibilities. In the early 1960s the United States finally began providing additional funding to support development in Palau, enabling marked improvements in infrastructure and services (Epstein 1986: 61-65; Parmentier 1987:51-57). One of the services most improved was health care, further controlling the effects of disease and generally decreasing mortality. Throughout most of the period following World War II, the population grew steadily. The first postwar estimate placed Palau's population at about 6,000 persons (McGrath 1972:143); by 1990 the total number of inhabitants exceeded 15,100 (U.S. Bureau of the Census 1992c:1).

By mid-1979 each component of the TTPI had formally decided to leave the Trust Territory (Shinn 1984:314, 323, 332, 341). But for many years the newly formed Republic of Palau did not complete the process of establishing independence (see Parmentier 1987:52-53). Several special referendums failed to yield the 75 percent approval necessary to ratify the Compact of Free Association, which would clearly establish Palau's relationship with the United States (including financial commitments) and for all intents and purposes terminate the Trust Territory. With the Northern Mariana Islands becoming a U.S. commonwealth in 1978, and both the Republic of the Marshall Islands and the Federated States of Micronesia forming independent nations that had implemented individual compacts in 1986, for years Palau remained in political limbo as the lone remnant of the TTPI. In November 1993 the people of Palau finally ratified their own Compact of Free Association, in the process taking yet another step toward formal independence. As with the other compacts, the Palauan version includes a stipulation enabling citizens of the republic to migrate freely to the United States and U.S. territories. For a republic whose people have shown a strong propensity to emigrate over the past several decades (see Hezel and Levin 1990:43-46), the Compact of Free Association between Palau and the United States likely will have important demographic implications--both for Palau and for several potential migrant destinations.

Demographic Change in the Republic of Palau

Demographic data for Palau are quite scanty prior to the first Nan'yō-chō census of the Mandated Territory in 1920. This is particularly true for the precontact and early contact periods. Certain types of evidence, such as the archaeological remains of terraced hillsides and large numbers of abandoned villages, bear witness to what almost certainly was a large prehistoric population (see Osborne 1966:150-155; Morgan 1988:10-12). Unfortunately, the amount of depopulation during the eighteenth and nineteenth centuries remains clouded by a lack of reliable precontact demographic figures.

Early population estimates for Palau run as high as 100,000 persons (see McKnight 1960:166), though the most frequently cited maximum is 40,000-50,000, calculated by Semper based on information gleaned from Henry Wilson's account of Palau in 1783 (Semper 1982:289-290).[3] Although islanders with whom Semper spoke felt this estimate was reasonable, the German anthropologist Kramer believed it was too high when he visited Palau during the first decade of the 1900s. Based on an extensive survey of 235 villages (both inhabited and uninhabited), an estimated average of 100

persons per settlement, and a general belief that the resources could never support as many as 40,000 people, Krämer concluded that between 20,000 and 25,000 was a more realistic estimate for Palau's population in 1800 (1919:292). Semper calculated another estimate while in residence in 1862, arriving at a figure of 10,000 based on a survey of men's clubs and extrapolating for average family size to account for women and children (1982:289). Kubary estimated that the population of Palau had declined even further by the early 1870s, proposing a total of 5,000 inhabitants--falling even further to about 4,000 a decade later (1885:145). Demographic estimates from early in the German administration indicate continued depopulation, to 3,823 in 1901, 3,101 in 1903, and as low as 3,000 in 1907 (Senfft 1902:264; Hermann 1909:634). Unfortunately, specific trends are unclear even during the German period of Palau's history; for example, another estimate in 1908 places Palau's population at 4,321 (Yanaihara 1940:42), implying a demographic resurgence that would have been impossible without a massive influx of people from elsewhere (for which there is no indication). The last population estimate for Palau during the German administration was 4,543 (Yanaihara 1940: 42), a figure that included several hundred people exiled in 1911 by the Germans from Pohnpei to Aimeliik following the Sokehs rebellion.[4]

Early population data are even more scarce for the Southwest Islands, which received much less attention from Europeans during the nineteenth century than did the Palau Islands. Table 1 presents available figures for the Palau Islands, Sonsorol State (broken into its main island components), and Hatohobei prior to the Japanese administration. Perplexing are those figures indicating high populations for certain outer islands, including roughly 800 for Dongosaro in 1710 (Eilers 1935:36) and 968 for Hatohobei in 1909 (Eilers 1936:86). Although the Dongosaro figure is highly questionable given its basis on brief observations by an early explorer, the Hatohobei figure resulted from a census and in all likelihood is accurate--though representing an extremely large population for a small, isolated coral island. Oral history supports the notion of high populations and associated pressure on Hatohobei (Johannes and Black 1981:86), though certain researchers suggest that the population of any Southwest Island probably never exceeded 300 individuals (McKnight 1977:15).

Various agencies from different governments conducted censuses of Palau during the twentieth century: four by the Japanese South Seas Bureau (1920, 1925, 1930, and 1935), two by the TTPI administration (1958 and 1973), one by the U.S. Peace Corps in conjunction with the University of Hawai‘i School of Public Health (1967), three by the U.S. Bureau of the Census (1970, 1980, and 1990), and one by the Republic of Palau Office of Planning and Statistics (1986).[5] The U.S. military conducted what certain

TABLE 1. **Early Population Estimates for Palau**

Year	Palau Islands[a]	Sonsorol				Source
		Hatohobei	Dongosaro	Meleili	Puro	
1710			800			Eilers 1935
1788		200				Eilers 1936
1832		350[b]				Eilers 1936
1860[C]	3,000	200	200	100	100	Gulick 1862
1862	10,000					Semper 1982
1870[d]					100	Eilers 1935
1872	5,000					Kubary 1885
1878	200					Eilers 1936
1882	4,000					Kubary 1885
1892	3,000					de Valencia 1892
1896	>3,000		350			Christian 1899
1898		950[b]				Eilers 1936
1900			400[e]		150	Hermann 1909; Eilers 1935
1901	3,823					Senfft 1902
1903	3,101					Hermann 1909
1906				200[f]	43	Hermann 1909
1907[g]	4,155	900	350[e]			*Deutsches Kolonial-Handbuch* 1909; Hermann 1909
1908	4,321					Yanaihara 1940
1909		968	304	73	44	Eilers 1935, 1936
1910	4,000					Krämer 1919
1914	4,543					Yanaihara 1940
1916	4,880					Matsumura 1918

[a] The geographic portion of Palau associated with many of the earl population estimates is uncertain. Because many of these figures come from individuals whose experience focused mainly (if not exclusively) on the Palau Islands and because population figures for the Southwest Islands tended to appear separately, it is likely that the figures under the heading "Palau Islands" in this table in fact refer only to that geographic portion of Palau.

[b] 1832 and 1898 figures appear in Eilers (1936:86) as the ranges 300-400 and 900-1,000, respectively.

[c] Gulick provides no dates for his estimates (1862:363), but notes that his data are "the result of eight years' research" (1862:358), thus *possibly* dating from the early 1850s. Many of the figures also have other years associated--though their connection with population (vs. date of discovery or an earlier visit) is unclear. Given the uncertain dates of Gulick's estimates and their 1862 publication, I use 1860 as a basic point of reference.

[d] According to Useem (1952b:153), 50 persons lived on Angaur Island in 1870.

[e] Includes Dongosaro and Fanna islands. The 1907 figure refers to the beginning of that year; a *November* 1907 total for these two places is 224 persons (Hermann 1909:635).

[f] Refers to an unspecified month in 1906 (Hermann 1909:635); the same source presents a population of only 27 individuals on Meleili Island in November 1906.

[g] *Deutsches Kolonial-Handbuch* 1909:330 presents figures for Dongosaro (350), Hatohobei (900) and the Palau Islands (4,074) without specifying a particular ear. Given the dates of other western Caroline Islands population data in this source and the year of its publication, the Palau figures probably date between 1905 and 1909. Figures for Hatohobei and Dongosaro islands presented in Hermann (1909:635) for 1907 agree, supporting the 1905-1909 range for the undated figures.

TABLE 2. **Population of Palau by Year, Showing Population Change between Census Years: Select Years**

Year	Population	Change from Previous Listed Census Year	Average Annual Change from Previous Listed Census Year	Source
1920	**5,754**	**—**	**—**	**Nan'yo-cho 1937**
1925	**5,957**	**203**	**0.7%**	**Nan'yo-cho 1927**
1930	**6,009**	**52**	**0.2%**	**Nan'yo-cho 1931**
1935	**6,230**	**221**	**0.7%**	**Nan'yo-cho 1937**
1946	5,972	...	...	Useem 1946; McGrath 1972
1947	6,156	...	...	Chief of Engineers 1956
1948	6,357	...	...	U.S. Dept. of the Navy 1948
1949	6,555	...	...	U.S. Dept. of the Navy 1949
1951	7,321	...	...	U.S. Dept. of Interior 1952
1954	7,726[a]	...	...	U.S. Dept. of State 1955
1955	7,656	...	...	U.S. Dept. of State 1956
1956	7,999	...	...	U.S. Dept. of State 1957
1957	8,563	...	...	U.S. Dept. of State 1958
1958	**9,344**	**3,114**	**1.8%**	**Office of the High Commissioner 1959**
1959	9,072	...	...	U.S. Dept. of State 1960
1960	9,320	...	...	U.S. Dept. of State 1961
1961	9,674	...	...	U.S. Dept. of State 1962
1962	9,965	...	...	U.S. Dept. of State 1963
1963	10,280	...	...	U.S. Dept. of State 1964
1964	10,628	...	...	U.S. Dept. of State 1965
1965	10,832	...	...	U.S. Dept. of State 1966
1967	**11,365**	**2,021**	**2.2%**	**School of Public Health n.d.**
1968	11,904	...	...	U.S. Dept. of State 1969
1969	12,291	...	...	U.S. Dept. of State 1970
1970	**11,210**	**-155**	**-0.5%**	**U.S. Bureau of the Census 1972**
1971	12,686	...	...	U.S. Dept. of State 1972
1972	13,025	...	...	U.S. Dept. of State 1973
1973	**12,673**	**1,463**	**4.2%**	**Office of Census Coordinator 1975**
1975	13,446	...	...	U.S. Dept. of State 1977
1976	13,150	...	...	U.S. Dept. of State 1978
1977	13,520	...	...	U.S. Dept. of State 1978
1978	13,910	...	...	U.S. Dept. of State 1979
1979	14,320	...	...	U.S. Dept. of State 1980
1980	**12,116**	**-557**	**-0.6%**	**U.S. Bureau of the Census 1983a**
1984	13,000	...	...	U.S. Dept. of State 1985
1986	**13,873**	**1,757**	**2.3%**	**Office of Planning and Statistics 1987**
1990	**15,122**	**1,249**	**2.2%**	**U.S. Bureau of the Census 1992c**

Notes: Census years in **boldface.** Data for 1920-1935 are for Pacific Islanders only. Intercensal estimates are probably de jure population; figures for remaining years are de facto population. For all tables, "-" denotes zero or a percentage that rounds to less than 0.1; "NA" = not available; ". . ." = not applicable.

[a] Excludes Japanese residing in Angaur State.

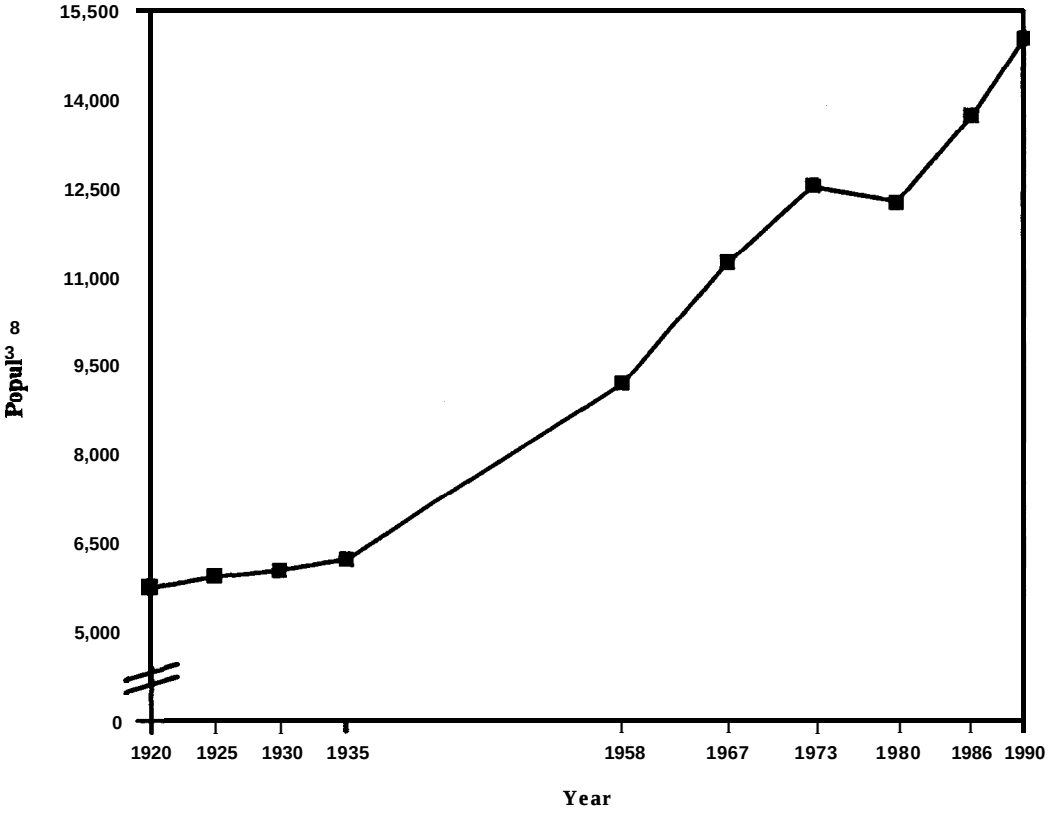


FIGURE 2. **Change in the population of Palau over time (1920, 1925, 1930, and 1935 are Pacific Islanders only).**

researchers have called "censuses" in 1946 and 1947 (Useem 1946:63; Chief of Engineers 1956:19), providing population figures for Palau immediately following World War II, though due to lack of information on the methods employed and an absence of detailed information on demographic structure I do not discuss these figures with the other censuses.[6] The data available indicate that Palau's population grew throughout most of the twentieth century, though at varying rates and (possibly) interrupted by occasional periods of decline (Table 2; Figure 2).

Demographic change has varied between individual states within the Republic of Palau (Table 3). In part, these differences reflect varying development emphases. For example, Koror maintained the largest population in Palau throughout the period covered by twentieth-century censuses, with considerable growth occurring since 1967. Angaur, on the other hand, featured a relatively large population throughout the pre-World War II years, its postwar demographic decline compensated for by recent growth in Airai

TABLE 3. **Population by State: Census Years**[a]

Area	1920	1925	1930	1935	1946	1947	1958	1967	1970	1973	1980	1986	1990
Republic of Palau	5,754	5,957	6,009	6,230	5,972	6,156	9,344	1,365	11,210	12,673[b]	12,116	13,873	15,122
Aimeliik	NA	165	200	200	300	334	412	364	366	306	273	283	439
Airai	NA	322	365	395	585	733	442	538	561	738	668	1,021	1,234
Angaur	759	798	708	751	316	334	428	429	438	277	243	214	206
Hatohobei	NA	225	180	172	129	141	103	60	64	48	74	35	22
Kayangel	NA	101	117	92	132	113	181	199	209	162	140	115	137
Koror	972	1,255	1,277	1,214	658	1,094	3,585	5,667	5,431	7,669	7,585	9,442	10,501
Melekeok	NA	357	357	304	342	284	310	356	328	315	261	254	244
Ngaraard	NA	569	578	663	767	592	773	770	622	725	457	468	310
Ngardmau	NA	110	126	124	148	186	558	227	254	206	160	157	149
Ngaremlengui	NA	196	210	217	266	255	316	436	428	387	358	301	281
Ngatpang	NA	50	50	66	68	67	88	119	103	89	166	219	62
Ngchesar	NA	329	316	344	443	380	450	449	485	341	364	271	287
Ngerchelong	NA	425	435	522	510	464	558	615	745	427	372	277	354
Ngiwal	NA	210	229	250	265	238	366	381	355	237	267	218	234
Peleliu	582	629	641	716	834	790	679	682	759	657	609	545	601
Sonsorol	NA	216	220	200	209	151	95	73	62	88	79	42	61
Unorg. Is.[c]	...	...	...	...	...	...	...	...	...	...	40	11	...

Sources: Nan'yo-cho 1927, 1931, 1937; Useem 1946; Chief of Engineers 1956; Office of the High Commissioner 1959; McGrath 1972; U.S. Bureau of the Census 1972, 1983a, 1992c; Office of Census Coordinator 1975; Office of Planning and Statistics 1987; School of Public Health n.d.

[a] Figures for 1946 and 1947 are from data collected by the U.S. military government, presented here for the sake of thoroughness. However, as discussed in the main text, because the methods used to collect these data are uncertain I do not accord them the same accuracy as the other censuses (excluding the 1970 census).

[b] Includes 1 individual whose place of residence was "not stated."

[c] The unorganized islands consisted primarily of the Rock Islands off the coast of Koror. These islands, listed either as "Palau Islands (unorg.)" (1980) or "Rock Islands" (1986), were not enumerated separately for other census years--either because they contained no permanent population or because they were combined with another jurisdiction (as in 1990, when they were combined with Koror State).

TABLE 4. **Population Density by State: Census Years (Persons per Square Mile)**

Area	1920	1925	1930	1935	1958	1967	1970	1973	1980	1986	1990
Republic of Palau	34	35	35	37	55	67	66	74	71	81	89
Aimeliik	NA	8	10	10	21	18	19	16	14	14	22
Airai	NA	18	21	23	25	31	32	42	38	58	71
Angaur	230	242	215	228	130	130	133	84	74	65	62
Hatohobei	NA	375	300	287	172	100	107	80	123	58	37
Kayangel	NA	144	167	131	259	284	299	231	200	164	196
Koror	137	177	180	171	505	798	765	1,080	1,068	1,330	1,479
Melekeok	NA	33	33	28	29	33	31	29	24	24	23
Ngaraard	NA	41	42	48	56	55	45	52	33	34	22
Ngardmau	NA	6	7	7	31	13	14	12	9	9	8
Ngaremlengui	NA	8	8	9	13	17	17	16	14	12	11
Ngatpang	NA	3	3	4	5	7	6	5	9	12	4
Ngchesar	NA	20	19	21	28	27	30	21	22	17	18
Ngerchelong	NA	104	106	127	136	150	182	104	91	68	86
Ngiwal	NA	20	22	24	36	37	35	23	26	21	23
Peleliu	124	134	136	152	145	145	162	140	130	116	128
Sonsorol	NA	240	244	222	106	81	69	98	88	47	68
Unorg. Is.[a]	...	...	...	...	...	...	...	...	NA	NA	...

[a] Area of unorganized islands unavailable.

State. Changes in population density over time further document the differences in demographic evolution experienced in various parts of Palau, pointing up the considerable densities that emerged in Koror in contrast to relatively sparse occupation found elsewhere (Table 4).

Let us now turn our attention to the demographic evolution of Palau, discussed in seven sections. The first covers the Japanese administration, a period during which Palau's population maintained slow, steady growth. Each of the remaining six sections discusses one of the postwar censuses, covering a period that saw Palau's population more than double from prewar levels, with increasing numbers of people residing in Koror. This examination of census results emphasizes data on the geographic distribution of population and major characteristics of demographic structure, exploring potential causes of population change when possible.

The Population of Palau during the Japanese Period: 1920, 1925, 1930, and 1935

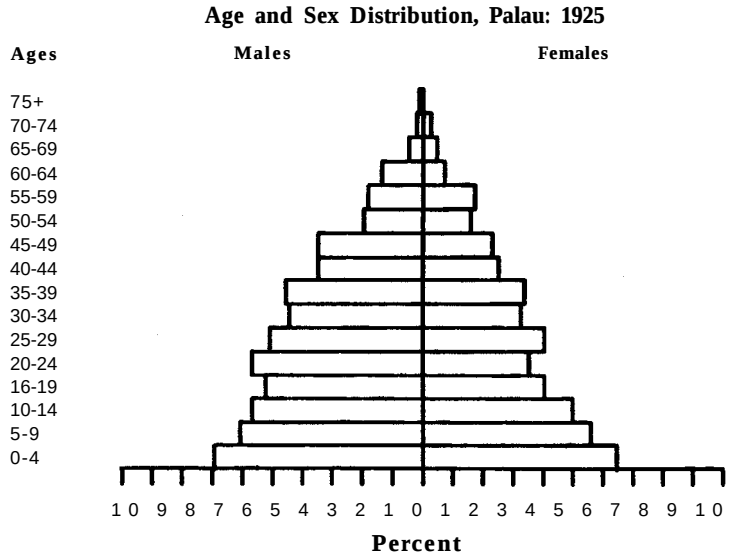
The Japanese South Seas Bureau (Nan'yō-chō) conducted the first census of the Republic of Palau (then the Palau District of the Mandated Territory) in

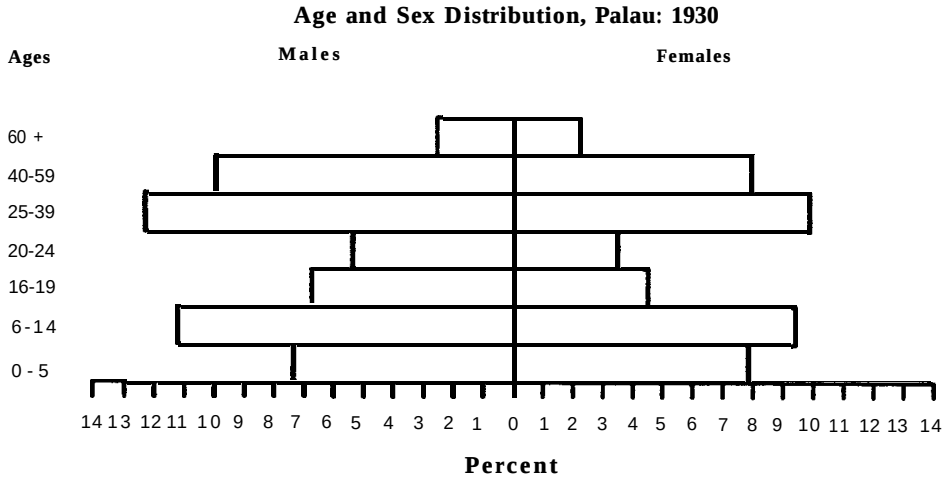
1920, recording slightly more than 5,750 Pacific Islanders (see Table 3; Nan'yo-cho 1937).[7] Population counts are available for three individual states: Angaur, Koror, and Peleliu, Together these three places accounted for slightly more than 40 percent of Palau's population. Although equally detailed demographic data are unavailable for the German period of administration, given the figures discussed earlier it appears that the population increased considerably over the six years preceding the 1920 census.

The number of Pacific Islanders in Palau increased by more than 200 persons between 1920 and 1925, a consequence of 0.7 percent average annual growth over that five-year period (see Table 2; Nan'yo-cho 1927). In addition to recording total population, the 1925 census also provided data on the population of each state in Palau (see Table 3). The three states mentioned above continued to dominate Palau's demography, though Ngaraard also featured a relatively large population. The 1925 census also reported information on the age-sex composition of Palau, recording a population with a median age of 22.8 years and a sex ratio (number of males per 100 females) of 125.4 (Figure 3).

The Pacific Islander population of Palau continued to increase slowly during the second half of the 1920s, the 1930 total of 6,009 resulting from 0.2 percent average annual population growth over the preceding five years (see Table 2; Nan'yo-cho 1931). Koror, Angaur, Peleliu, and Ngaraard states once again had the largest populations (see Table 3). Most states experienced slight increases during the late 1920s, although Angaur's population declined. Data on the age-sex composition of the 1930 population suggest only slight changes from the situation recorded five years earlier, with small declines in both the sex ratio (122.2) and the median age (21.8 years) (see Figure 3). In addition, the 1930 census recorded for the first time information on the age and sex composition of each state (Table 5). Available data indicate considerable variability between places. Some of this variability, such as that found in Hatohobei and Sonsorol states, *in part* is a consequence of the relatively small populations involved--although the contrasts of these states compared to others in Palau do indicate fundamental differences in resident populations. Angaur's 1930 population included a disproportionately large share of individuals aged 15-24 years, no doubt a consequence of importing young-adult Micronesians from elsewhere in the Mandated Territory to labor in the phosphate mines.

Few data on births, deaths, and mobility are available for Palau during the Japanese administration. The general fertility rate between 1923 and 1930 for women aged 15-50 years ranged from 71.1 (1926) to 129.3 (1929) (Yanaihara 1940:35). Crude birth rate for the years 1925 through 1929 averaged 24.4 for Palauans, slightly higher than the average crude death rate of 21.5 for the same years (Yanaihara 1940:46). Relatively detailed data on





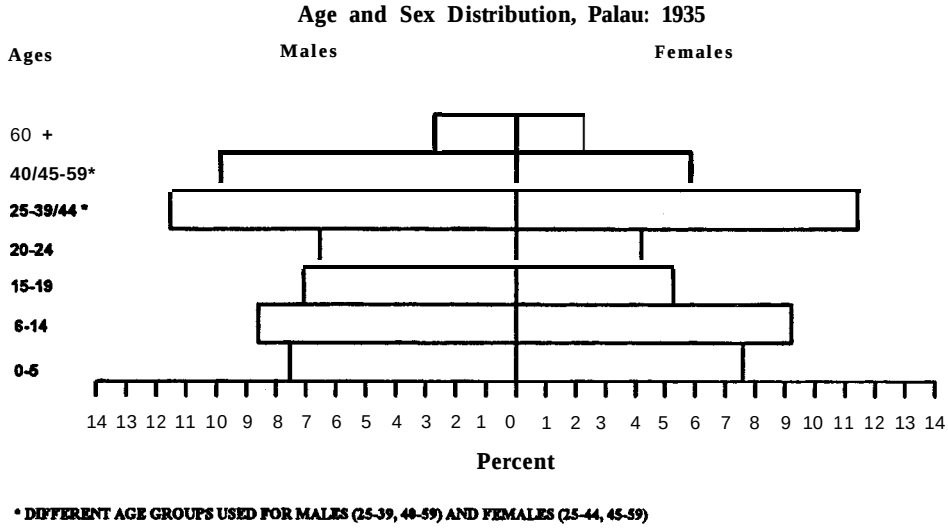


FIGURE 3. **Population pyramids (Pacific Islanders only): 1925, 1930, 1935.**

TABLE 5. **Pacific Islander Population by Age and State: 1930**

Area	Total Persons	Age Group (Percentage)			
		<15	15-24	25-59	60+
Republic of Palau	6,009	35.6	20.1	39.7	4.7
Aimeliik	200	34.0	24.0	34.5	7.5
Airai	365	33.7	15.9	45.8	4.7
Angaur	708	26.3	32.5	39.8	1.4
Hatohobei	180	5.6	18.3	71.1	5.0
Kayangel	117	37.6	19.7	36.8	6.0
Koror	1,277	37.0	24.7	35.3	3.0
Melekeok	357	45.7	13.7	35.0	5.6
Ngaraard	578	46.5	13.0	34.6	5.9
Ngardmau	126	35.7	19.0	42.1	3.2
Ngaremlengui	210	31.4	18.1	43.3	7.1
Ngatpang	50	30.0	14.0	48.0	8.0
Ngchesar	316	44.0	14.9	37.0	4.1
Ngerchelong	435	33.8	19.8	40.2	6.2
Ngiwal	229	36.7	18.8	38.0	6.6
Peleliu	641	46.2	17.0	31.4	5.5
Sonsorol	220	4.5	9.1	78.2	8.2

Source: Nan'yo-cho 1931.

Note: In this and following tables, percentages ma not sum to 100.0 due to rounding. Other reasons for not summing to 100.0% are noted in each case.

TABLE 6. **Pacific Islander Population by State, according to Place of Registration: 1930**

Area	Total Persons	Place of Registration (Percentage)			
		Same Locality	Same District	Other District	Other Location
Republic of Palau	6,009	57.2	31.3	9.8	1.7
Aimehik	200	51.0	42.5	4.0	2.5
Airai	365	65.8	25.5	6.3	2.5
Angaur	708	25.3	17.8	52.4	4.5
Hatohobei	180	99.4	0.6	-	-
Kayangel	117	55.6	42.7	1.7	-
Koror	1,277	30.5	56.3	12.1	1.0
Melekeok	357	66.9	32.2	0.8	-
Ngaraard	578	63.3	33.9	1.9	0.9
Ngardmau	126	65.9	31.0	2.4	0.8
Ngaremlengui	210	61.9	33.8	4.3	-
Ngatpang	50	42.0	56.0	2.0	-
Ngchesar	316	76.9	23.1	-	-
Ngerchelong	435	63.2	27.8	0.5	8.5
Ngiwal	229	72.9	26.6	0.4	-
Peleliu	641	89.7	10.3	-	-
Sonsorol	220	84.1	15.9	-	-

Source: Nan'yo-cho 1931.

mobility exist for Palau in 1930, revealing patterns of lifetime migration through comparing place of residence (in 1930) with place of registration by the Japanese administration (Table 6). These data indicate a particularly mobile population, with only about 57 percent of the Pacific Islander population living in the same locality where registered. Most movement occurred within the Palau District, though nearly 10 percent of the total Pacific Islander population came from another part of the Mandated Territory. Most of the individuals registered elsewhere in the mandate lived on Angaur, probably representing mine laborers. Relatively large proportions of the 1930 Angaur and Ngerchelong state populations comprised islanders from outside the Mandated Territory, the former no doubt mine laborers from elsewhere in Oceania and the latter possibly agricultural laborers.

The Pacific Islander population of Palau continued to grow slowly during the early 1930s, with 0.7 percent average annual increase adding another 221 persons between 1930 and 1935 (see Table 2; Nan'yo-cho 1937). The greatest numbers of people once again lived in Koror, Angaur, Peleliu, and Ngaraard states (see Table 3). Demographic change varied geographically, with some states gaining and others losing people. Koror, whose population had barely grown during the late 1920s, experienced a decline in Pacific Islander population between 1930 and 1935, though the immigration of Japanese caused the overall population to grow considerably as the community emerged as a major demographic, economic, and cultural center in the Mandated Territory.

The age-sex characteristics of the Pacific Islander population remained similar to those documented in 1930 (see Figure 3). A slight reduction in the excess of males over females occurred, as the sex ratio fell to 119.4. Overall age composition remained largely unchanged, the median age holding constant at 21.8 years. The age structure of individual municipalities continued to vary, with a decrease in the proportion of islanders aged less than 15 years compensated for by an increase in the percentage aged 15-24 years (Table 7). The presence of particularly high percentages of individuals aged 25-59 years in Hatohobei and Sonsorol states persisted. Disproportionately high representation of individuals aged 15-24 years continued on Angaur. Considerable variation in age composition occurred in all states.

Our understanding of the reasons underlying population change in Palau between 1930 and 1935 once again rests on minimal data. The crude birth rate and crude death rate for islanders both remained low between 1931 and 1935, the former ranging from 22.7 to 30.8 and the latter from 12.5 to 24.2 (Office of the Chief of Naval Operations 1944b:36). Fertility exceeded mortality in each of these five years, indicating that a slow rate of natural increase would have accounted for at least some of the population growth expe-

TABLE 7. **Pacific Islander Population by Age and State: 1935**

Area	Total Persons	Age Group (Percentage)			
		<15	15-24	25-59	60+
Republic of Palau	6,230	33.9	22.9	38.3	4.9
Aimeliik	200	31.5	24.0	41.5	3.0
Airai	395	29.9	23.0	41.5	5.6
Angaur	751	25.4	33.6	39.0	2.0
Hatohobei	172	6.4	12.8	78.5	2.3
Kayangel	92	31.5	18.5	40.2	9.8
Koror	1,214	38.4	22.7	35.3	3.6
Melekeok	304	38.8	21.7	32.9	6.6
Ngaraard	663	43.4	19.5	31.8	5.3
Ngardmau	124	31.5	23.4	40.3	4.8
Ngaremlengui	217	30.0	24.9	37.8	7.4
Ngatpang	66	24.2	30.3	37.9	7.6
Ngchesar	344	41.3	20.6	31.1	7.0
Ngerchelong	522	35.4	22.2	36.4	5.9
Ngiwal	250	36.8	23.2	35.6	4.4
Peleliu	716	39.0	23.2	32.8	5.0
Sonsorol	200	5.0	6.0	79.0	10.0

Source: Nan'yo-cho 1937.

rienced during the early 1930s (see also Yanaihara 1940:46). Detailed mobility data comparable with those presented for 1930 unfortunately are unavailable for 1935.

The Population of Palau in 1958

A 1946 population estimate prepared by the U.S. military reported 5,972 residents of Palau in August of that year (McGrath 1972:143; see also note 6). A second estimate for November 1947 reported 6,156 inhabitants (Chief of Engineers 1956:16). But it was not until 1958 that another census of Palau occurred, conducted by the TTPI administration (Office of the High Commissioner 1959). That census indicated that the population had increased by more than 3,100 persons over the total recorded in 1935, the result of 1.8 percent average annual growth (see Table 2). If the estimate for 1947 is accurate, the annual rate of population growth over the eleven years preceding the 1958 census increases to 4.7 percent--an exceptionally high rate that is highly unlikely, even allowing for the post-1947 repatriation of some Palauans and the inclusion of non-natives in the 1958 census (probably excluded and certainly very rare in 1947).

Despite overall demographic growth during the twenty-three years preceding the 1958 census, four states experienced slight declines in their populations (see Table 3). Two of these decreases occurred in the Southwest Island states, the populations of both Hatohobei and Sonsorol each totaling about 100 individuals in 1958. Angaur also witnessed a population decline, no doubt due to closure of the phosphate mines that for so long had employed immigrant labor. But the population growth in other parts of Palau easily compensated for these declines--particularly in Koror, where due to a nearly threefold increase in population since 1935 more than 38 percent of the republic's inhabitants resided.

Dramatic changes in population structure also occurred over this same period. Although males continued to exceed females, the sex ratio had declined to 102.7 by 1958--once again possibly a result of closing the mines at Angaur. The Palau population had become much younger as well, with the median age declining to 17.8 years (Figure 4). The presence of a larger, more youthful population suggests that natural increase (an excess of births over deaths) played an important role in demographic change between 1935 and 1958. Unfortunately, reliable data on fertility and mortality are unavailable for 1958, as is information on mobility.

The Population of Palau in 1967

The 1967 census of Palau recorded nearly 11,400 total inhabitants, the result of 2.2 percent average annual growth over the preceding nine years (see Table 2; School of Public Health n.d.). Demographic change once again varied among states (see Table 3). The populations of Hatohobei and Sonsorol states continued to decline, with the total inhabitants in each falling below 75. Ngardmau experienced the greatest population loss, largely erasing the population gains recorded in that state between 1935 and 1958. But once again demographic growth in Koror more than compensated for these losses, as the proportion of the total population residing in this state increased to nearly 50 percent.

Demographic composition remained similar to that recorded in 1958. Although Palau continued to contain more males than females, the sex ratio declined to 102.4. Palau's population continued its trend towards increasing youth as the median age fell to 15.0 years (see Figure 4). Once more the age composition of individual municipalities varied considerably (Table 8). In some cases this variability was a function of the small resident populations, such as in Hatohobei and Sonsorol states. But in most instances the differences in demographic composition probably provide indirect evidence of key population dynamics, including the migration of adults (comprising the 15-24 and 25-59 age groups) to Koror State from more rural places.

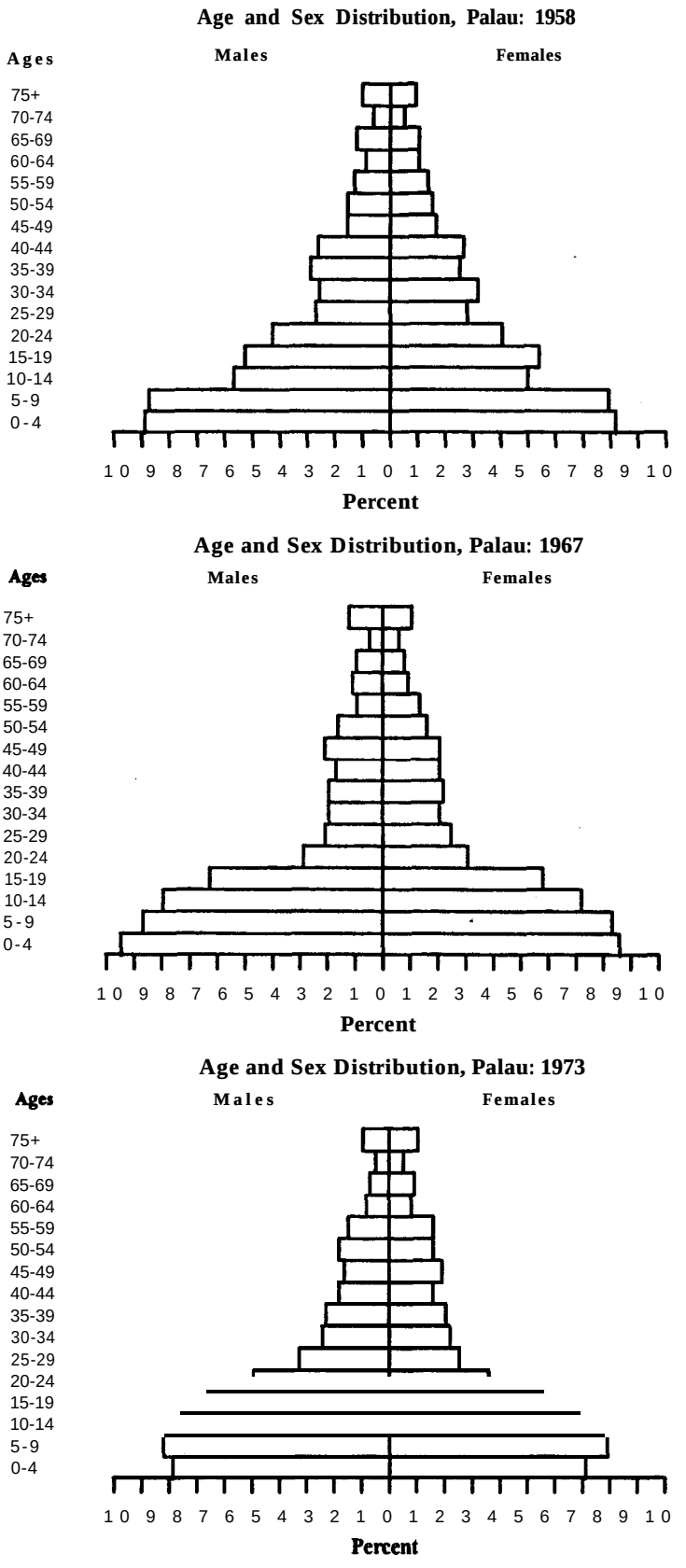


FIGURE 4. **Population pyramids: 1958, 1967, 1973.**

TABLE 8. **Population by Age and State: 1967**

Area	Total Persons	Age Group (Percentage)[a]			
		0-14	15-24	25-59	60+
Republic of Palau	11,365	48.4	17.3	24.5	6.5
Aimeliik	364	47.0	19.5	24.2	9.3
Airai	538	51.7	14.5	24.5	8.0
Angaur	429	48.7	19.1	20.7	7.0
Hatohobei	60	28.3	3.3	38.3	30.0
Kayangel	199	48.7	21.1	24.6	3.5
Koror	5,667	46.7	17.7	26.0	4.2
Melekeok	356	51.1	15.2	21.9	8.7
Ngaraard	770	50.3	18.2	21.7	8.6
Ngardmau	227	50.7	17.2	23.8	6.6
Ngaremlengui	436	51.6	17.4	22.7	7.1
Ngatpang	119	49.6	19.3	20.2	10.9
Ngchesar	449	50.8	15.8	24.3	7.8
Ngerchelong	615	50.7	15.3	23.3	10.1
Ngiwal	381	53.5	14.4	23.9	7.6
Peleliu	682	49.6	17.7	21.8	10.6
Sonsorol	73	34.2	11.0	23.3	24.7

Source: Nan'yō-chō 1937.

[a] Percentages may not sum 100.0 due to exclusion of 385 individuals whose ages were "not specified" or who were "foreign born" (whose precise ages similarly were not specified).

TABLE 9. **Fertility and Mortality Measures for Palau: Select Census Years**

Year	Total Persons	Total Births	Fertility[a]			Mortality	
			Crude Birth Rate	General Fertility Rate	Total Fertility Rate	Crude Death Rate	Infant Mortality Rate[b]
1967	11,365	424	37.3	200.1	7,472	5.7	42.5
1970	11,210	336	30.0	145.2	5,223	6.9	47.6
1973	12,673	401	31.6	162.9	5,574	4.2	44.9
1980	12,116	302	24.9	116.8	3,476	7.8	26.5
1986	13,873	347	25.0	NA	NA	6.3	25.9
1990	15,122	326	21.6	90.1	3,046	7.7	24.5

Sources: School of Public Health n.d.; U.S. Bureau of the Census 1972, 1983a, 1992c; U.S. Dept. of State 1981, 1982; Office of Planning and Statistics 1992.

[a] Fertility measures for 1967 and 1980 differ from those presented in Table 10, due to conflicting data. The data here are reported births in the entire Republic of Palau for each year and thus should be comparable across years. Unfortunately, these same data are not available for each state, requiring that I employ different data sources for Table 10.

[b] Infant mortality rates for 1967, 1973, 1980, and 1990 differ from age-specific mortality rates for individuals aged less than 1 year, presented in Table 12, due to slightly different methods of calculation and data. Calculations here are based on the number of infant deaths (of individuals aged less than 1 year) per 1,000 *live births,* which is the conventional method of calculating infant mortality. The age-specific mortality for persons younger than 1 year presented in Table 12 represents the number of deaths of individuals in that age group per 1,000 *individuals aged less than 1 year.*

Vital statistics from 1967 provide important clues to the mechanisms underlying population change in Palau (Table 9). Fertility grew beyond pre-World War II levels, with crude birth rate increasing by as much as 64 percent (or as little as 21 percent, depending on the prewar figure one uses) and the general fertility rate increasing by as much as 181 percent (or as little as 55 percent, once again depending on the choice of prewar figures). Complementing this apparent growth in fertility, mortality declined to well below the levels recorded during the Japanese administration, the 1967 measures ranging from 24 to 46 percent of those recorded prior to the war. This growing imbalance between mortality and fertility no doubt accounted for much of the postwar increase in population, as discussed in greater detail below. Fertility measures calculated from a different data source than used for Table 9 reveal considerable variability between individual states (Table 10). Two of the three measures calculated were higher in Koror than for Palau as a whole, suggesting that natural increase may have played a relatively important role in the demographic growth experienced in this state (though the measures possibly also reflect people from other areas reporting births in the- Koror hospital; see note 8 below). As one might expect, the largest numbers of deaths in 1967 occurred among the very young (aged 0-4 years) and old (aged 70 years or more) (Table 11), as reflected in age-specific death rates (Table 12). Unfortunately, reliable data on mobility are unavailable for 1967, although as noted above the age composition of rural places compared to Koror State suggest that movement to Koror accounted for much of its postwar growth.

The Population of Palau in 1973

The U.S. Bureau of the Census conducted its first census of the TTPI in 1970 (U.S. Bureau of the Census 1972). Unfortunately, the results are of questionable accuracy, probably due to a combination of errors in recording, editing, and tabulation that occurred in varying degrees throughout much of the Trust Territory. I have presented the total population and the population of individual states above (see Tables 2 and 3) for the sake of completeness. But because of certain puzzling results of this census--such as indications of depopulation during a period marked by steady population growth--coupled with known problems in data processing, I do not present an analysis of the population counts. The lack of confidence in the 1970 census led the TTPI administration to conduct another census in 1973 (Office of Census Coordinator 1975).

The 1973 census recorded nearly 12,700 persons in Palau (see Table 2). Compared to the total recorded in the 1970 census, the 1973 population

TABLE 10. **Fertility Measures by State: 1967 and 1980**

Area	1967					1980				
	Total Persons	Total Births[a]	Crude Birth Rate	General Fertility Rate	Total Fertility Rate	Total Persons	Total Births	Crude Birth Rate	General Fertility Rate	Total Fertility Rate
Republic of Palau	11,365	408	35.9	176.0	6,565	12,116[b]	225	18.6	87.2	2,850
Aimeliik	364	9	24.7	125.0	4,292	273	5	18.3	116.3	5,625
Airai	538	11	20.4	125.0	5,813	668	10	15.0	78.7	2,772
Angaur	429	20	46.6	250.0	9,729	243	2	8.2	55.6	1,667
Hatohobei	60	3	50.0	500.0	15,000	74	2	27.0	200.0	5,000
Kayangel	199	7	35.2	127.7	3,810	140	-	-	-	-
Koror	5,667	230	40.6	188.6	6,390	7,585	160	21.1	88.4	2,846
Melekeok	356	16	44.9	250.0	11,149	261	4	15.3	117.6	3,635
Ngaraard	770	15	19.5	85.1	4,362	457	5	10.9	59.5	1,508
Ngardmau	227	10	44.1	179.5	6,306	160	3	18.8	142.9	4,333
Ngaremlengui	436	18	41.3	212.5	10,669	358	6	16.8	74.1	2,440
Ngatpang	119	1	8.4	55.6	2,500	166	5	30.1	135.1	3,500
Ngchesar	449	14	31.2	170.7	7,937	364	4	11.0	69.0	3,089
Ngerchelong	615	17	27.6	153.8	6,583	372	5	13.4	78.1	1,566
Ngiwal	381	13	34.1	191.2	9,497	267	2	7.5	60.6	1,964
Peleliu	682	21	30.8	150.0	6,483	609	12	19.7	107.1	4,154
Sonsorol	73	3	41.1	300.0	7,667	79	-	-	-	-

Sources: School of Public Health n.d.; U.S. Bureau of the Census 1983b.

Note: Includes infants born to mothers aged <15, >49, and of unknown age; the "unknown" group is used for crude birth rate but not general or total fertility rates.

[a] 1967 natality based on infants aged 1 year and younger recorded by the census for that year, and thus does not account for individuals who died during the first year of life prior to the census.

[b] Includes the 40 individuals on the "Unorganized Islands," where no births were recorded in 1980.

TABLE 11. **Registered Deaths in Palau by Age Group: 1967, 1973, 1980, and 1990**

Age Group	1967[a]	1973	1980	1990
	Number			
Total Deaths	68	53	94	117
	Percentage			
All Ages	100.0	100.0	100.0	100.0
< 1	14.7	34.0	8.5	6.8
1-4	10.3	1.9	3.2	4.3
5-9	1.5	3.8	-	1.7
10-14	1.5	1.9	1.1	-
15-19	2.9	1.9	1.1	2.6
20-24	1.5	7.5	2.1	3.4
25-29	5.9	-	1.1	6.8
30-34	2.9	1.9	2.1	4.3
35-39	1.5	3.8	2.1	3.4
40-44	1.5	-	1.1	5.1
45-49	7.4	1.9	4.3	-
50-54	-	3.8	4.3	4.3
55-59	2.9	9.4	5.3	5.1
60-64	5.9	5.7	8.5	10.3
65-69	1.5	9.4	6.4	4.3
70-74	8.8	11.3	8.5	11.1
75+	20.6	1.9	40.4	26.5

Sources: 1967 calculations based on deaths in the 11.5 months preceding the 1967 census, as presented in School of Public Health n.d.; 1973 and 1980 calculations based on deaths in calendar year, in U.S. Dept. of State 1982; 1990 calculations based on deaths in calendar year recorded in Office of Planning and Statistics 1992.

[a] Percentages do not sum precisely to 100.0 due to 3 individuals whose age at death was "not stated."

represents the result of 4.2 percent average annual increase--a very high growth rate that further suggests an undercount in the 1970 effort. Compared to the 1967 total, the 1973 population represents a more likely average annual increase of 1.8 percent. Population grew in only three of the sixteen states, however, over the six years preceding the 1973 census (see Table 3). One of these places was Koror, where the addition of more than 2,000 residents between 1967 and 1973 easily compensated for the declines experienced elsewhere. As a result of these population changes, in 1973 Koror State accounted for nearly 61 percent of Palau's total inhabitants.

Data on demographic composition reveal certain structural differences between the 1973 and 1967 populations. Males once again exceeded females

TABLE 12. **Age-Specific Death Rates in Palau: 1967, 1973, 1980, and 1990**

Age Group	1967	1973	1980	1990
Total	5.72[a]	4.18[a]	7.76	7.74
<1	24.27	40.45	33.33	41.24
1-4	4.49	0.69	2.58	3.79
5-9	0.54	0.99	-	1.31
10-14	0.60	0.55	0.58	-
15-19	1.52	0.65	0.64	2.05
20-24	1.56	3.71	1.85	2.99
25-29	8.30	-	1.21	5.70
30-34	4.71	1.80	2.88	3.74
35-39	2.28	3.79	3.98	3.22
40-44	2.43	-	2.02	6.87
45-49	11.52	2.29	10.10	-
50-54	–	4.78	10.42	9.75
55-59	8.37	13.77	12.25	14.89
60-64	20.00	14.22	27.87	31.01
65-69	5.62	24.39	21.13	15.06
70-74	50.42	42.86	61.54	52.21
75+	57.38	3.95	165.22	92.54

Sources: See Table 11.

[a] Includes individuals whose age group was "not stated."

in the republic as a whole, the sex ratio increasing considerably to 109.4. Moreover, the 1973 population was older than that recorded in 1967--reversing a trend towards increasing youthfulness that had persisted since 1925 (see Figure 4). Evidence for an older population appears immediately in the median age, which had increased to 16.9 years; much of this change in age occurred as an increase in the proportion of individuals aged 15-24 and 25-59 years, at the expense of individuals younger than 15 years. Available evidence on the age composition of selected components of Palau indicates that most of the republic continued the trend towards increasing youthfulness; the main exception was Koror State, which compared to Palau as a whole contained proportionally fewer individuals in the youngest age group examined and proportionally more in the two central age groups (Table 13).

Available vital statistics for 1973 indicate a decline in all three fertility measures calculated for Palau compared to 1967 levels (see Table 9). This reduction in postwar fertility levels has continued until the present (see Levin and Retherford 1986:52, 58). However, crude death rate also declined over the same six years, preserving a considerable excess of births over deaths.

TABLE 13. **Population by Age and State: 1973**

Area	Total Persons	Age Group (Percentage)[a]			
		<15	15-24	25-59	60+
Republic of Palau	12,673[b]	45.2	20.7	27.3	6.4
Angaur	277	51.6	16.2	22.7	9.4
Babeldaob[c]	3,771	51.3	14.5	25.2	8.4
Hatohobei	48	41.7	10.4	16.7	31.3
Kayangel	162	64.8	9.3	20.4	4.9
Koror	7,669	41.5	25.2	28.6	4.4
Peleliu	657	46.4	10.5	28.6	14.2
Sonsorol	88	46.6	13.6	22.7	17.0

Source: Office of Census Coordinator 1975.

[a] Percentages do not sum to precisely 100.0 due to exclusion of 43 individuals whose ages were "not stated."

[b] Total persons includes 1 individual whose place of residence was "not stated."

[c] Includes Aimeliik, Airai, Melekeok, Ngaraard, Ngardmau, Ngaremlengui, Ngatpang, Ngchesar, Ngerchelong, and Ngiwal states.

TABLE 14. **TTPI-Born Population by Area of Usual Residence, according to Home District: 1973**

Usual Residence	Total Persons	Home District (Percentage)			
		Same Area	Elsewhere in Palau	Elsewhere in TTPI	Outside TTPI
Republic of Palau	12,091[a]	61.3	34.4	4.2	0.1
Angaur	260	92.3	6.9	0.8	-
Babeldaob[b]	3,726	92.2	5.0	2.8	-
Hatohobei	46	89.1	10.9	-	-
Kayangel	159	88.1	11.9	-	-
Koror	7,194	40.1	54.2	5.5	0.1
Peleliu	650	93.5	5.2	1.1	0.2
Sonsorol	55	100.0	-	-	-

Source: Office of Census Coordinator 1975.

[a] Total includes 1 individual whose usual residence was "not stated."

[b]Includes Aimeliik, Airai, Melekeok, Ngaraard, Ngardmau, Ngaremlengui, Ngatpang, Ngchesar, Ngerchelong, and Ngiwal states. "Same Area" in this case means on Babeldaob Island--movement between states on the island not qualifying as a change in location.

Age-specific mortality data indicate a substantial increase since 1967 in the mortality of individuals aged less than 1 year, a change compensated for by decreases in the mortality of several other age groups (including 70 years and older) (see Tables 11 and 12).

Mobility data for 1973 provide few clues for the geographic arrangement of Palau's population (Table 14). In general, the TTPI-born population was highly mobile, nearly 39 percent claiming as home some place other than their state of residence. As one might expect, Koror State featured the greatest proportion of immigrants, with nearly 60 percent of its TTPI-born population originating elsewhere. Although the vast majority of the immigrants to Koror came from another part of Palau, more than 5 percent came from elsewhere in the Trust Territory (possibly to attend the Micronesian Occupational College). Koror State clearly dominated the mobility statistics, with at least 88 percent of the residents of other parts of Palau living in their respective home areas.

The 1973 census also collected information that provides insights on emigration, although in comparing place of birth with usual residence for TTPI-born individuals these data are not strictly comparable with those presented immediately above.[8] Available information indicates that of the more than 12,500 Palau-born individuals counted in the 1973 census, less than 66 per-

TABLE 15. **TTPI-Born Population by Place of Birth, according to Area of Usual Residence: 1973**

		Usual Residence (Percentage)			
Place of Birth	Total Persons	Same Area	Elsewhere in Palau	Elsewhere in TTPI	Outside TTPI
Republic of Palau	12,555[a]	65.5	26.5	8.0	-
Angaur	572	41.8	40.7	17.3	0.2
Babeldaob[b]	5,385	58.1	36.3	5.7	-
Hatohobei	101	41.6	55.4	3.0	-
Kayangel	225	60.0	37.8	2.2	-
Koror	4,943	81.5	8.9	9.5	-
Peleliu	1,183	48.7	43.2	7.9	0.2
Sonsorol	116	63.8	27.6	8.6	-

Source: Office of Census Coordinator 1975.

[a] Total includes 30 individuals whose place of birth was "not stated"; calculations exclude these persons.

[b] Babeldaob includes Aimeliik, Airai, Melekeok, Ngaraard, Ngardmau, Ngaremlengui, Ngatpang, Ngchesar, Ngerchelong, and Ngiwal states. "Same Area" in this case means on Babeldaob Island--movement between states on the island not qualifying as a change in location.

cent resided in the area of birth (Table 15). As one might expect, more-rural parts of Palau experienced the greatest emigration, with more than half the persons, born in Angaur, Hatohobei, and Peleliu states moving elsewhere (most often to another part of Palau, probably Koror). In contrast to the rest of Palau, only about 18 percent of the persons born in Koror relocated outside their place of birth, most often to another part of the Trust Territory. In general, these data support the proposition that rural-urban migration in Palau accounted for much of the demographic growth experienced in Koror State.

The Population of Palau in 1980

The U.S. Bureau of the Census conducted its second census of Palau in 1980, recording a decline of more than 550 persons over the preceding seven years (see Table 2; U.S. Bureau of the Census 1983a).[9] All but four states experienced depopulation during this period, although no decrease exceeded 268 persons (see Table 3). Koror joined the list of states whose population declined following the 1973 census, though its share of the total grew to nearly 63 percent.

Population composition in 1980 generally resembled that documented in 1973. Males continued to exceed females, though the sex ratio fell slightly to 107.6 (Figure 5). Median age, on the other hand, continued to increase, reaching 18.8 years as the population grew slightly older (U.S. Bureau of the Census 1983a:11). The age structure of individual states once again varied widely. The relatively high percentages of individuals in Koror State aged 15-24 and 25-59 years suggest a continuation of people moving to the main economic and demographic center in search of education or employment opportunities (Table 16).

Data on fertility for Palau indicate a decrease between 1973 and 1980 for all three measures calculated (see Table 9; see also Levin and Retherford 1986:58). Fertility measures for individual states, once again employing different data than used for Palau as a whole, indicate low fertility in most parts of the republic (see Table 10)--in many cases falling well below that found in the Japanese period. Mortality apparently increased over the same seven-year period, a decline in infant mortality (evident from both data sources employed) more than compensated for by an increase in mortality among individuals aged 70 years and older (see Table 12). Based on these two shifting trends, one would expect a reduction in the population *growth rate,* though because fertility still exceeds mortality natural increase should continue.

The third main component of demographic change, namely migration,

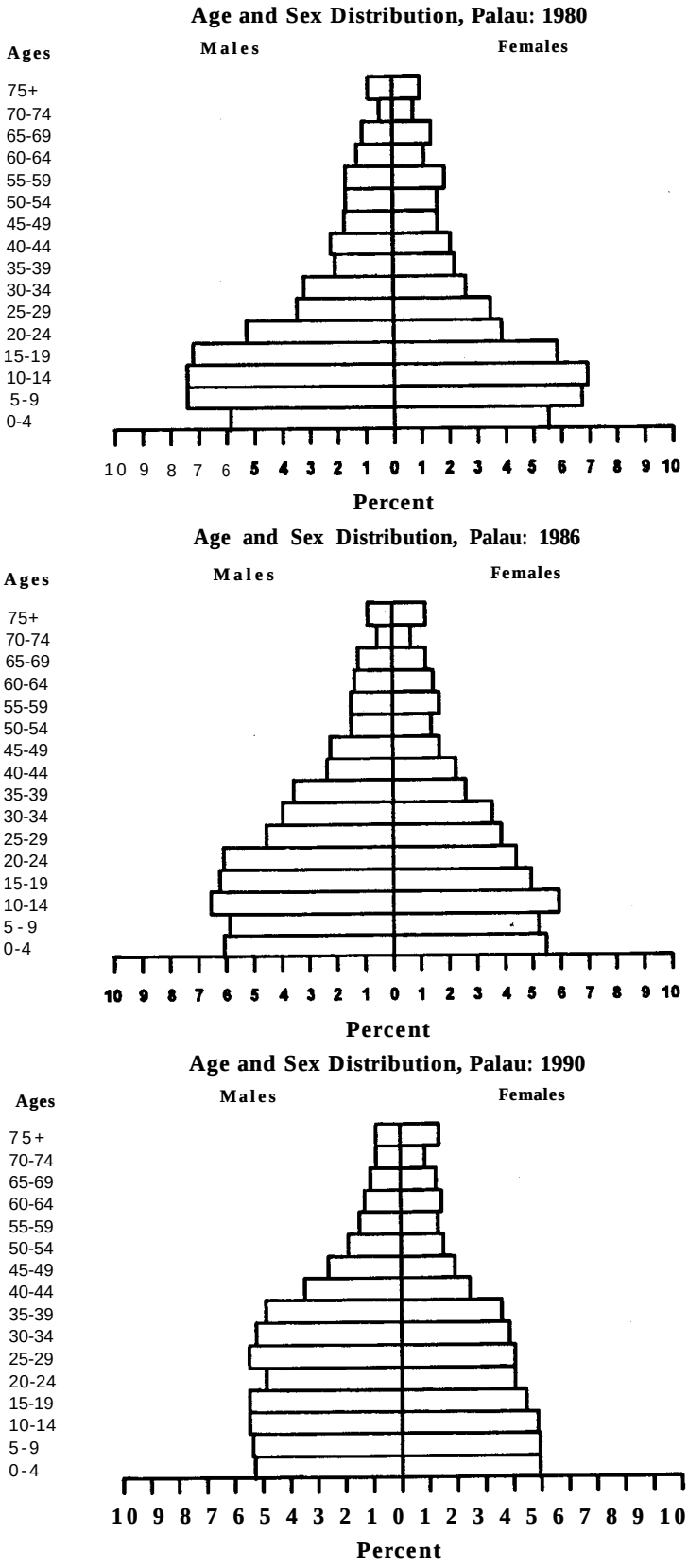


FIGURE 5. **Population pyramids: 1980, 1986, 1990.**

TABLE 16. Population by Age and State (or Area): 1980

Area	Total Persons	Age Group (Percentage)			
		<15	15-24	25-59	60+
Republic of Palau	12,116	39.9	21.8	30.6	7.7
Aimeliik	273	45.8	19.0	24.9	10.3
Airai	668	40.7	20.2	28.1	10.9
Angaur	243	46.5	14.0	26.7	12.8
Hatohobei	74	44.6	10.8	29.7	14.9
Kayangel	140	50.7	14.3	25.7	9.3
Koror	7,585	38.2	23.4	32.9	5.5
Melekeok	261	41.4	20.3	25.7	12.6
Ngaraard	457	42.5	17.1	28.0	12.5
Ngardmau	160	44.4	21.9	23.1	10.6
Ngaremlengui	358	38.5	25.4	26.5	9.5
Ngatpang	166	39.8	27.7	27.7	4.8
Ngchesar	364	44.0	18.7	27.5	9.9
Ngerchelong	372	40.6	19.6	25.3	14.5
Ngiwal	267	42.3	18.7	26.6	12.4
Peleliu	609	42.4	17.9	28.1	11.7
Sonsorol	79	60.8	7.6	21.5	10.1
Palau Is. (unorg.)	40	40.0	25.0	20.0	15.0

Source: U.S. Bureau of the Census 1983a.

could account for the population decline between 1973 and 1980. Unfortunately, detailed data on emigration from Palau largely are unavailable. It is likely that two of the main destinations of Palauan emigrants were the Commonwealth of the Northern Mariana Islands (CNMI) and Guam, for decades attractive to other Micronesians because of employment and educational opportunities. Data from the 1980 census indicate that 735 Palauans resided in the CNMI (most probably on Saipan, capital of the TTPI) and 1,335 Palauans lived in Guam that year (U.S. Bureau of the Census 1984a:9, 1984b: 9). Another 692 to 1,027 Palauans resided in the United States in 1980, the discrepancy depending on how one defines "Palauan" (see Barringer, Gardiner, and Levin 1993:286, 298). Such levels of emigration certainly could account for the population decline between 1973 and 1980, *if* many relocated after the 1973 census.

More detailed data are available on mobility *within* Palau, through comparing residence in 1980 with residence in 1975. These data indicate relatively little short-term migration, with only about 8 percent of the total population aged 5 years and older having relocated from outside their 1980 state of residence (Table 17). As with age composition, these mobility patterns

TABLE 17. **Population by State, according to Place of Residence in 1975: 1980**

Area	Total Persons	Place of Residence in 1975 (Percentage)			
		Same State	Elsewhere in Palau	Elsewhere in TTPI	Outside TTPI
Republic of Palau	9,938[a]	91.9	4.2	1.7	2.0
Aimeliik	229	93.9	4.4	1.3	0.4
Airai	574	85.4	11.8	0.9	1.2
Angaur	199	96.0	2.0	1.0	1.0
Hatohobei	61	95.1	3.3	1.6	-
Kayangel	125	100.0	-	-	-
Koror	6,088	90.6	3.9	2.3	3.0
Melekeok	224	100.0	-	-	-
Ngaraard	380	87.9	9.2	1.8	0.5
Ngardmau	142	100.0	-	-	-
Ngaremlengui	320	91.6	7.2	0.6	0.6
Ngatpang	142	96.5	-	3.5	-
Ngchesar	316	98.4	1.6	-	-
Ngerchelong	322	98.4	1.2	0.3	-
Ngiwal	222	100.0	-	-	-
Peleliu	534	97.2	2.4	-	0.2
Sonsorol	60	65.0	33.3	1.7	-

Source: U.S. Bureau of the Census 1983b.

[a] Includes only those individuals aged 5 years and older; percentages may not sum precisely to 100.0 due to exclusion of 23 individuals whose residence in 1975 was not given; data in this table exclude 40 individuals whose residence was recorded as "Palau Islands, Unorganized."

varied between places. In four rural states, none of the population aged 5 years and older had relocated from elsewhere, either from within Palau or beyond. In contrast, slightly more than 9 percent of the population of Koror State immigrated over the five years preceding the 1980 census, with people from other parts of Palau, other parts of the Trust Territory, and beyond the Trust Territory all represented in similar amounts.

The Population of Palau in 1986

In 1986 the Palau Office of Planning and Statistics, with technical assistance from the United Nations Development Program, conducted the tenth census of Palau (Office of Planning and Statistics 1987). The census recorded nearly 13,900 persons, the result of a resurgence in demographic growth that averaged 2.3 percent annually over the preceding six years and added more than

1,700 to the total population (see Table 2). Most states actually lost population following the 1980 census, with the Southwest Island states each reporting populations below 50 individuals (see Table 3). But growth in two states more than compensated for these declines: Koror, which added more than 1,850 to its population as its share of the total grew to more than 68 percent; and Airai, a state whose population increased by nearly 53 percent (though by only 353 persons) over the same time period, probably attributable to the bridge connecting Koror and Babeldaob (at Airai) that opened in the late 1970s.

Certain characteristics of demographic composition that had emerged in the early 1970s persisted in the 1986 census. The sex ratio, which had reached its lowest point in 1967, increased to 114.3. Similarly, Palau's population once more increased in age, reaching a median of 21.8 years, last recorded more than five decades earlier at the height of the Japanese administration (see Figure 5). The increased age occurred primarily as reductions in the percentages of the total population aged less than 25 years, complemented by growth in the proportions aged 25 years and older. Relatively high percentages of individuals in the two central age groups characterized both Airai and Koror, likely due to immigration from rural areas (Table 18). The demographic struc-

TABLE 18. **Population by Age and State (or Area): 1986**

Area	Total Persons	Age Group (Percentage)			
		<15	15-24	25-59	60+
Republic of Palau	13,873	35.0	21.3	35.9	7.9
Aimeliik	283	39.6	17.3	32.5	10.6
Airai	1,021	31.5	22.7	37.3	8.4
Angaur	214	41.6	15.9	33.2	9.3
Hatohobei	35	45.7	11.4	22.9	20.0
Kayangel	115	55.7	13.0	22.6	8.7
Koror	9,442	33.2	22.8	37.8	6.2
Melekeok	254	37.8	13.4	33.5	15.4
Ngaraard	468	37.8	27.1	24.4	10.7
Ngardmau	157	40.8	17.8	29.9	11.5
Ngaremlengui	301	42.2	13.6	32.9	11.3
Ngatpang	219	34.7	33.3	26.5	5.5
Ngchesar	271	41.7	12.5	31.4	14.4
Ngerchelong	277	40.8	10.8	33.2	15.2
Ngiwal	218	40.4	14.2	28.0	17.4
Peleliu	545	42.0	10.6	32.5	14.9
Sonsorol	42	69.0	4.8	19.0	7.1
Rock Islands	11	-	27.3	36.4	36.4

Source: Office of Planning and Statistics 1987.

TABLE 19. **Population by Place of Usual Residence, according to Previous Place of Residence: 1986**

		Previous Residence (Percentage)			
Usual Residence	Total Persons	Same State	Elsewhere in Palau	Outside Palau	Not Stated
Republic of Palau	13,704[a]	66.6	20.2	12.6	0.6
Aimeliik	288	59.7	32.6	7.3	0.3
Airai	1,027	49.4	35.1	15.6	-
Angaur	220	85.9	10.9	3.2	-
Hatohobei	35	11.4	88.6	-	-
Kayangel	119	71.4	24.4	4.2	-
Koror	9,206	68.2	16.2	14.9	0.6
Melekeok	242	73.1	16.9	7.4	2.5
Ngaraard	481	50.3	33.3	16.4	-
Ngardmau	159	56.6	39.0	4.4	-
Ngaremlengui	307	77.9	15.3	6.2	0.7
Ngatpang	217	42.4	54.8	2.8	-
Ngchesar	276	74.6	23.9	0.7	0.7
Ngerchelong	294	62.9	34.0	2.4	0.7
Ngiwal	223	83.0	14.8	2.2	-
Peleliu	556	83.1	11.5	3.2	2.2
Sonsorol	42	26.2	73.8	-	-
Rock Islands	12	-	83.3	16.7	-

Source: Office of Planning and Statistics 1987.

[a] Does not include 168 individuals enumerated by the 1986 census whose usual residence was outside Palau and 1 person whose usual residence was not given.

ture of the remaining states continued to vary, with many of the more-rural places featuring particularly high percentages of old and young persons who tend to stay home while others emigrate in search of schooling or work.

Available vital statistics indicate that natural increase probably played an important role in the demographic growth recorded in Palau between 1980 and 1986. Based on limited data, fertility apparently grew only slightly over these six years (see Table 9). Mortality, in contrast, declined over the same period, increasing the difference between births and deaths.

The contribution of migration to total population change, either into or out of Palau, is uncertain due to a lack of data. Immigrants comprised 1,550 persons in the Republic of Palau in 1986 (Office of Planning and Statistics 1987:33), although their contribution to 1980-1986 population growth is unknown because precisely *when* they immigrated is not given. The contribution of mobility to the geographic distribution of Palau's population, on the other hand, is clearer--though playing varying roles throughout the republic.

Data on lifetime mobility indicate that two-thirds of the 1986 population lived in the same state as their previous residence (Table 19). About 20 percent had lived elsewhere in Palau, with nearly 13 percent coming from outside the ‘republic. Airai and Koror states, which experienced considerable demographic growth between 1980 and 1986, both featured populations consisting of large percentages of immigrants; indeed, more than half the population of Airai in 1986 claimed a previous residence elsewhere in Palau or beyond. Of those who had relocated, nearly 48 percent had moved within two years of the 1986 census (Office of Planning and Statistics 1987:35). Without exception, every state in Palau contained persons who claimed a previous residence outside that state. The apparently high incidence of immigration characterizing many of the rural parts of Palau, including the Southwest Island states, probably represents some type of circular mobility--people moving for a time somewhere else (most likely Koror) before returning to their home island.

The Population of Palau in 1990

The 1990 census of Palau, conducted by the U.S. Bureau of the Census, recorded a total population of 15,122 persons--the result of 2.2 percent average annual growth over the preceding four years (see Table 2; U.S. Bureau of the Census 1992c). Nine of the sixteen states experienced population increase (see Table 3). The greatest gains once again occurred in Airai and (especially) Koror states, Koror now accounting for more than 69 percent of the republic’s population. Many rural parts of Palau continued to feature small populations, in some cases (such as Ngatpang) losing a considerable number of people during the second half of the 1980s.

The gap between males and females increased further in 1990, the sex ratio growing to 116.6. Palau’s population aged considerably over the same time period, with the median age reaching 25.6 years (see Figure 5; see also U.S. Bureau of the Census 1992c:6). Much of the change in age composition occurred as a substantial decline in the percentage of individuals aged less than 15 years and a substantial increase in the percentage of individuals aged 25-59 years. The age composition of Koror State featured an even smaller proportion of the youngest age group and a larger proportion of the 25-59 group than the republic as a whole, once again indirect evidence of immigration of working-age individuals (Table 20). The populations of rural states tended to comprise larger percentages of young and old persons than the republic as a whole.

Available vital statistics indicate that fertility declined between 1986 and 1990, with all three fertility measures calculated lower than 1980 levels as well

TABLE 20. **Population by Age** and **State: 1990**

Area	Total Persons	Age Group (Percentage)			
		<15	15-24	25-59	60+
Republic of Palau	15,122	30.3	18.5	42.6	8.6
Aimeliik	439	33.3	21.2	36.9	8.7
Airai	1,234	30.5	18.6	42.3	8.6
Angaur	206	30.1	18.0	35.9	16.0
Hatohobei	22	27.3	36.4	18.2	18.2
Kayangel	137	36.5	20.4	29.2	13.9
Koror	10,501	29.1	18.9	45.3	6.7
Melekeok	244	34.8	16.8	31.1	17.2
Ngaraard	310	35.8	18.4	30.0	15.8
Ngardmau	149	35.6	11.4	44.3	8.7
Ngaremlengui	281	38.8	16.7	33.5	11.0
Ngatpang	62	29.0	8.1	50.0	12.9
Ngchesar	287	35.5	17.1	29.6	17.8
Ngerchelong	354	35.6	16.9	30.2	17.2
Ngiwal	234	34.2	12.8	35.0	17.9
Peleliu	601	29.5	17.0	37.6	16.0
Sonsorol	61	34.4	31.1	26.2	8.2

Source: U.S. Bureau of the Census 1992c.

(see Table 9). Mortality, in contrast, increased above levels recorded in 1986, a shift one might expect in a population that increased in age as much as Palau's did. The greatest number of deaths occurred among individuals aged 70 years and older. Although the percentage of total deaths accounted for by infants younger than 1 year declined during the 1980s, the age-specific death rate for this age group actually increased over the same period due to changes in the overall demographic structure (see Tables 11 and 12). Nevertheless, fertility greatly exceeded mortality in 1990, indicating that natural increase once again likely accounted for much of Palau's population growth over the preceding four years.

Rural-urban migration no doubt continued during the late 1980s, but unfortunately data that provide insights on such movement are at present unavailable from the 1990 census. As a result, one must explore internal mobility indirectly, such as through the contrasting age profiles found in various states discussed above. International migration, which has played some role in Palauan demography throughout much of the twentieth century, increased in importance in 1990. Nearly 17 percent of the 1990 population of Palau was non-Palauan, the vast majority of these immigrants coming from Asia (particularly the Philippines). Of those individuals born outside the republic, nearly

52 percent relocated between 1988 and 1990, with only 19 percent living in Palau prior to 1985 (U.S. Bureau of the Census 1992c:16, 56).

Given evidence for natural increase as well as considerable immigration, one would expect much more population growth in Palau during the late 1980s than apparently occurred. The most feasible explanation is that Palauans continued to emigrate outside the republic, with the CNMI and Guam among the most likely destinations. The 1990 census of each of these places recorded 1,407 and 1,233 residents born in Palau, respectively (U.S. Bureau of the Census 1992a:16, 1992b:15). Although as of 1990 Palauans could not migrate freely to the United States, the 1,439 respondents in the 1990 U.S. census who identified themselves as Palauan indicate that a considerable number had found some means of relocating there (U.S. Bureau of the Census 1991).

The Mechanisms of Population Change in the Republic of Palau

Due to a variety of causes, Palau experienced considerable depopulation over the century following the onset of sustained contact with Europeans in 1783. Estimates of Palau's population about this time range from 20,000 to 100,000 (McKnight 1960:166). By the early 1880s the naturalist Kubary estimated that Palau's population had declined to roughly 4,000 (1885:145). Although Krämer (1919:292) felt that depopulation had ceased by Kubary's time, available evidence suggests that population decline may have continued through the nineteenth century--with one estimate by a newly arrived missionary placing about 3,000 people in the Palau Islands in 1892 (de Valencia 1892:403). Certainly by the time of the German administration the population had stopped its decline, exceeding 4,500 by 1914 and approaching 5,800 Pacific Islanders by 1920 (Yanaihara 1940:42; see Table 2). Between the first Japanese census in 1920 and the most recent census in 1990, Palau's population increased by more than 262 percent, at a steady growth rate of about 1.4 percent annually. Thus over a period of about two hundred years, Palau's population declined by at least 70 percent (perhaps considerably more) and then more than quadrupled (though probably still not reaching pre-European levels). Given the nature of Palau's demographic evolution, two questions naturally follow: what were the causes of the massive depopulation and subsequent population growth observed in Palau over the past two centuries, and what were (and are) the repercussions of these dramatic demographic shifts? The rest of this article focuses on these two issues.

Of the three potential causes of demographic change in any setting--mortality, fertility, and mobility--the first probably was the main cause of depopulation in Palau during the nineteenth century. Although some of the

most knowledgeable researchers familiar with Palau during this period attributed depopulation to, among other things, slavery, papal decrees, avarice, proselytizing, the introduction of metal tools, abuse of alcohol and tobacco, and the general destruction of native culture by Europeans (Semper 1982: 291-293; Krämer 1919:293-294), the most important cause surely was increased deaths due to illnesses introduced from outside Micronesia. Despite claims that smallpox and other contagious diseases were absent during the 1860s (Semper 1982:292), a host of introduced illnesses occurred throughout Palau in the nineteenth century. Influenza, tuberculosis and other respiratory disorders, smallpox, dysentery and other intestinal illnesses, whooping cough, and measles caused many deaths between the 1780s and 1880s (see Robertson 1876/1877:45; Kubary 1885:145-146; Krämer 1919:293-298; Office of the Chief of Naval Operations 1944b:32; Price 1944:164; Force and Force 1972:7; Palau Community Action Agency 1978:371; Hezel 1983: 271). Kubary witnessed firsthand a massive number of deaths in several villages during an influenza epidemic in February 1872; by then the disease had become so prevalent that Palauans expected an outbreak annually (Kubary 1873:187, 1885:145-146). A later visitor attributed much of the disease-related mortality to dysentery, with one outbreak in the late 1800s allegedly killing up to half the native population (Price 1944:164). These diseases came to Palau from outside Micronesia, through direct contact with people from several nations (Shineberg 1971:231-234) as well as indirect contact via Palauans returning after exposure to foreign illnesses (e.g., Delano 1817:74-75). Although one tends to attribute the introduction of such maladies to Europeans, given the proximity to southeast Asia and evidence that Asians occasionally visited Palau at least by the late eighteenth and early nineteenth centuries (e.g., Dumont d'Urville 1843:208-209), it is entirely possible that others brought the maladies as well. Deaths due to introduced illnesses continued throughout much of the nineteenth century, with an influenza epidemic occurring as late as 1892 (Hezel 1991:199).

Hard data on mortality associated with diseases are scarce for the entire nineteenth century, unfortunately. Kubary recorded 56 deaths, many probably resulting from introduced illnesses, between November 1882 and 1883 in thirteen villages on Babeldaob--from a total population of only about 400 (1885:146). He also recorded 50 deaths (in eight villages) on Arakabesan, from a population of about 300, and 35 deaths (in ten villages) on Koror, from a total population of roughly 750 (Kubary 1885: 147). Assuming that Kubary's figures are accurate and cover about one year, crude death rates in the three described settings would equal 140.0, 166.7, and 46.7, respectively--all quite high (especially the first two), particularly after roughly a century of exposure to the illnesses concerned. Although introduced diseases occurred main-

ly on the Palau Islands because of their greater contact with Westerners, the outer islands did not escape such maladies. For example, the population of Hatohobei declined by as much as one-half during the two years that the *Mentor* castaways resided there in the early 1830s, apparently from a combination of disease and malnutrition (Black 1978:311-312).

Frequent warfare between native polities also plagued much of Palau, particularly the Palau Islands, providing another potential source of high mortality during the nineteenth century. But the warfare conducted during traditional times served primarily as a social institution, a means to gain status and extract tribute (Kubary 1873: 197-198, 1885: 124-137; McKnight 1960:84; Palau Community Action Agency 1976:70-72, 107; Semper 1982: 23; see also Keate 1788:136-137). Palauans recognized two types of warfare: small-scale raids, often involving head-hunting and designed primarily to kill one or two enemies; and large-scale conflicts designed to inflict greater damage on enemy forces and property (Kubary 1885: 127-141; Krämer 1926: 298-307). Despite the presence of the latter type of conflict, with few exceptions (such as Koror's defeat of Peleliu in 1790; see Nero 1987:236) traditional Palauan warfare rarely caused large numbers of deaths. Although some have argued that the introduction of firearms in the late eighteenth century by the British made native warfare a much deadlier affair (e.g., Delano 1817: 68; Office of the Chief of Naval Operations 1944b:54; Vidich 1949:38), the difficulty of using period firearms probably reduced the number of fatalities rather than increased them (see Nero 1987:295-297). Conflicts also occurred in the Southwest Islands, with forces from Dongosaro, Meleili, and Puro occasionally clashing (Office of the Chief of Naval Operations 1944b:54). Mortality levels from warfare are unknown.

Other sources of mortality no doubt contributed to depopulation in Palau during the 1800s, though their magnitude is elusive. Typhoons occasionally strike the western Carolines. For example, at least three noteworthy storms occurred during the 1860s alone: in 1862, 1866, and 1868-1869 (Office of the Chief of Naval Operations 1944b:8; Parmentier 1987:27). But the demographic impacts of these storms is unknown, and there is no indication that any storm in this or any other period caused deaths comparable in magnitude to those from disease.

Complementing the increase in mortality between the 1780s and 1900 was an apparent decline in reproduction. Many observers remarked on the increasingly small families and fewer births (see Delano 1817:37; Kubary 1873:194). Pertinent data once again are rare. Robertson observed in 1875 that many young adults were unmarried, that fewer than 40 percent of the women who had spouses had children, and that two or three children defined a large family (1876/1877:45). Kubary's observations support the

general picture of low fertility. In the same thirteen communities on Babeldaob studied in 1882-1883, he recorded only seven births (Kubary 1885: 146)--for a population of 400 this represented a crude birth rate of 17.5. Seven of the thirteen villages had no live births at all during the year in question. Writing about the early 1890s, de Valencia also remarked on how few women had children (1892:420-421). Based on tabulations of the numbers of births for selected families between 1783 and 1910, Krämer eventually concluded that low birth rate in conjunction with an unsettled family life were the main causes of depopulation in Palau (1919:294-296).

Likely reasons for low fertility appear to be several. Semper felt that the health risks of pregnancy and childbirth reduced many women's desire to reproduce (1982:106). Kubary suggested that the heavy work required of women coupled with postmarital residence patterns--husbands and wives often living separately--were the main causes of reduced fertility (1885: 147-148). Venereal disease probably also contributed to low fertility (Force and Force 1972:85; Nero 1987:300; Hezel 1995:116). Syphilis possibly arrived in Palau prior to European contact by way of the Philippines (Krämer 1926: 330-332). Widespread gonorrhea during the second half of the nineteenth century, however, likely played the greatest role in decreasing fertility (Kubary 1895:89; see also Pirie 1971). Deaths among persons of childbearing age no doubt also reduced fertility.

Little immigration occurred during this phase of Palau's past. A few beachcombers or small numbers of traders did little to compensate for the increased loss of population (e.g., Hezel 1983:86). In only a few documented cases did larger numbers of people relocate to Palau. As early as the 1790s several Burmese, Indian, Malayan, and Indonesian servants accompanied a former East India Company captain when he tried to establish a trading station on Koror, apparently departing a few months later (Force 1960:68; Hezel 1983:80). The German trader Tetens brought 50 Chinese laborers in 1867 to work on his cotton plantations, repatriating them at a later date (Tetens 1958: 88). As late as 1901 immigration to Palau remained limited; a survey conducted by a German official from Yap recorded only 75 foreigners in the islands, mostly Chamorros (43) and Japanese (23) (Senfft 1902:264).

Ultimately it was the imbalance between high mortality and low fertility that caused the massive depopulation of Palau during the nineteenth century. Migration had little effect, neither contributing many people to the population nor removing many people from it. The inability of reproduction to keep step with mortality eventually reached a point where two individuals familiar with Palau during the late 1800s predicted extinction for the islanders (Kubary 1885:145; Tetens 1958:4).

Depopulation in Palau apparently ceased sometime during the last decade

of the nineteenth century and the first decade of the twentieth century, a more precise determination precluded by conflicting population counts for uncertain geographic foci (see note 3). Pertinent data are scarce, but the most likely reason for this shift in demographic trends was a change in the very processes that led to depopulation, ultimately bringing mortality and fertility roughly in balance with one another. Given the lack of reference to high mortality caused by diseases that had plagued Palau for decades, it appears that their impacts had diminished by the onset of the twentieth century. Although one of the aims of the German administration was to improve public health, the only medical facility in Palau was a small dispensary on Angaur and the only physician in the western Carolines a German doctor on Yap (Office of the Chief of Naval Operations 1944b:109; Palau Community Action Agency 1978:369). Acquired natural immunity probably had as much as anything to do with the reduction of deaths associated with epidemics. Indeed, the greatest instance of disease-related depopulation during the German administration occurred on Hatohobei--one of the most remote parts of Palau generally not subject to frequent contact with outsiders (and whose people were thus less likely to develop natural immunity to introduced diseases)--where roughly 200 died (and another 52 were evacuated) shortly after the German Südsee Expedition visited there in 1909 (Eilers 1936:82-83). German efforts to promote better home life, notably through improved treatment of women and a reduction in prostitution that characterized clubhouses (Hezel 1995:116-117), had an unknown effect on mortality (or fertility).

Changes in migration patterns possibly also helped to curtail depopulation in the early 1900s. Most immigration occurred primarily in support of phosphate mining on Angaur. When mining operations began on this island in 1909, the personnel on hand included 23 Europeans, 55 Chinese, and 98 Yapese (Office of the Chief of Naval Operations 1944b:29; Chief of Engineers 1956:251); by 1913 the workforce on Angaur had increased to roughly 100 Chinese and 500 Pacific Islanders, the latter mostly from Yap and other parts of Palau (Purcell 1967:48). Lesser numbers of islanders migrated elsewhere in Palau. For example, during the first decade of the 1900s, 36 Chamorros lived in Palau, many possibly remaining from the preceding Spanish period (*Deutsches Kolonial-Handbuch* 1909:329); by 1911 approximately 80 Chamorros resided in northern Ngerchelong (Hezel 1991:206). The German government relocated several hundred persons from Pohnpei Island to southern Babeldaob in 1911, following a rebellion the preceding year (Office of the Chief of Naval Operations 1944a:20; see note 4). The emigration that occurred during the German administration was of a much smaller scale--such as the relocation of islanders from Dongosaro and Hatohobei islands to

Yap as laborers in the copra industry (Office of the Chief of Naval Operations 1944b:169) and the exile of native religious leaders who resisted German rule to Yap between 1910 and 1914 (Vidich 1980:171-172). Mobility during this time period also began to change the distribution of population *within* Palau. The most important instance of such movement occurred in the evacuation of most (possibly all) islanders from Dongosaro, Fanna, Meleili, and Puro islands to the Palau Islands in 1905 following a devastating typhoon the previous year (Eilers 1935:303; Office of the Chief of Naval Operations 1944b:60; McKnight 1977:17-18). Relocated initially to the south coast of Babeldaob (in Aimeliik), the Southwest Islanders eventually moved to Arakabesan Island. Many people from Hatohobei Island also moved to the Palau Islands about the same time--with their Southwest Islander neighbors beginning a trend of migration from outliers to the main islands of Palau that has persisted to the present (see Black 1977:49-50, 1983:8-9).

The steady demographic growth that has characterized Palau throughout most of the twentieth century probably began shortly before the Japanese administration, as indicated by apparent population increases during the second half of the preceding German period of rule. Despite the many changes that occurred between the first decade of the twentieth century and 1990, similar demographic mechanisms likely caused population growth throughout much of this period: moderate fertility, slightly in excess of mortality, augmented by net in-migration.

Certainly by the time of the Japanese administration, mortality had fallen well below the levels observed during the second half of the 1800s. Health services had improved, with hospitals established on Angaur and Koror islands in 1922 providing much more access to modem health care than before (Office of the Chief of Naval Operations 1944b:109; Purcell 1967:238; Peattie 1988:87). Nevertheless, several diseases that helped devastate the Palauan population during the nineteenth century persisted at least into the late 1930s, though with markedly less impact, including tuberculosis (11 dead in 1937), bronchial pneumonia and lung fever (17 dead in 1937), and dysentery (14 dead in 1929 and 1931, 3 dead in 1937) (Office of the Chief of Naval Operations 1944b:104-106). Other diseases present during the Japanese period, including dengue fever which erupted in major outbreaks in 1927 and 1929, apparently caused few if any deaths (Purcell 1967:243). Certain other illnesses continue to cause deaths in Palau to this day, especially diseases of the circulatory and respiratory systems (Office of Planning and Statistics 1992:122), but their effects are considerably less than during the previous century.

Given the ferocity of the battles on Angaur and Peleliu, World War II could have devastated the population on those islands. Fortunately, the Japa-

nese evacuated most natives from both places prior to the U.S. landings (see Hezel 1991:223), limiting the fatalities among islanders. The demographic impacts of bombing those parts of Palau where American forces did not land, particularly Babeldaob and the main islands immediately to its south, are uncertain, though the damage was considerable (McGrath 1972:139; Nero 1989: 117). Conditions deteriorated markedly during the last year in the war; more than 2,000 Japanese and an unknown number of Palauans died of disease or starvation (Peattie 1988:300, 304; see also Nero 1989:120, 127-130). Alleged Japanese plans to execute the entire islander population of Palau thankfully were never carried out (Hezel 1995:241). Although the overall effects of the war on islanders are unknown, by 1973 Palauans had lodged 10,700 claims for deaths of relatives (Palau Community Action Agency 1978:419). Despite the potential devastation of the war, the first systematic population estimate following the Japanese surrender indicated only slight demographic decline.

Prior to the war the Japanese administration had promoted a program to reduce women's work in an attempt to raise fertility. But reproduction remained low throughout their administration with an average of only 2.7 children per family (Useem 1946:64-65). Possible reasons for persisting low fertility were several. Childbearing remained precarious both for mother and infant--even with increasingly modem health care, infant mortality measured 134.3 throughout the Japanese administration (Useem 1946:64). Venereal diseases also possibly played a key role in suppressing fertility. Useem mentioned a high incidence of such diseases immediately following the war, though he does not identify the specific illnesses (1946:64). Gonorrhea apparently was widespread throughout the western Carolines during the Japanese administration (Office of the Chief of Naval Operations 1944b:105). Although likely more prevalent in the Yap District, which experienced depopulation into the 1950s (Gorenflo and Levin 1991:101-102), gonorrhea apparently occurred in Palau as well (Palau Community Action Agency 1978:371). This malady was not limited to the Palau Islands; for instance, virtually all women on Hatohobei had become sterile due to gonorrhea by the late 1920s (Black 1978:317-318). Nevertheless, fertility has exceeded mortality in all census years for which we have vital statistics. During the Japanese administration, the excess of births over deaths was slight, probably helping to account for the relatively small increases in native population, during that phase of Palau's history. During the years following World War II, fertility has been much greater than mortality (see Tables 9, 10, and 12), although levels of fertility remained much lower than in most of Micronesia and have decreased since the late 1960s (see Levin and Retherford 1986:48-66).

Mobility has played a very important role in Palau's demography throughout most of the twentieth century. During the Japanese administration, this

primarily took the form of immigration. With the addition of administrative personnel, the Japanese government of Palau was much larger than its German predecessor. Moreover, Koror also served as the capital of the Mandated Territory, requiring yet additional staff. Japan actively promoted migration to parts of the Mandated Territory, including Palau, in the interest of general development that continued into the late 1930s (Palau Community Action Agency 1978:357). Although present during the 1920s, Japanese and Okinawan civilians flocked to Koror beginning in the early 1930s, to capitalize on either certain successful Japanese development schemes (such as the commercial fishing and mother-of-pearl industries) or the secondary economic activity that subsequently evolved (see Office of the Chief of Naval Operations 1944b:35; Chief of Engineers 1956:35). In contrast, relatively few Japanese moved to Babeldaob; even after the agricultural success enjoyed there during the second half of the 1930s, fewer than 300 Japanese families resided on the large island as late as 1940 (Peattie 1988:172-174; see also Kaneshiro 1958:292).

Pacific Islanders also relocated to Palau during the Japanese administration. Since the inception of phosphate mining on Angaur, foreigners had supplied much of the labor for the mines. This practice continued throughout the Japanese administration, with a heavy reliance on islanders from other parts of the Pacific who remained for periods of four months to one year (Useem 1946:79; Purcell 1967:191-194; Palau Community Action Agency 1978:333; Peattie 1988:82-83; see also Decker 1940:130-150). Available data for the period 1933-1937 indicate that 250 to 300 laborers came to Angaur annually from elsewhere in the Mandated Territory, mostly from the Truk District (Chuuk) (Office of the Chief of Naval Operations 1944b:170). The mining operations initiated by the Japanese on Peleliu, which were smaller than those on Angaur, relied primarily on Japanese, Okinawan, and native Palauan workers--at one count numbering 120 individuals in all (Chief of Engineers 1956:255). Mining on Hatohobei, conducted at an even-smaller scale, used local workers as well as an unknown number of Polowat Islanders (Black 1977:25). Labor recruitment practices for the mines on Dongosaro are unknown.

As conflict in the Pacific neared, the Japanese government brought additional people to Palau to help in its military effort. The number of Japanese military personnel in Palau reached an estimated 25,000 by 1943 and roughly double that number by the end of the war, with large contingents stationed on Angaur, Babeldaob, Koror, and Peleliu (Chief of Engineers 1956:8; Johannes 1981:4; Nero 1989:120) and much-smaller garrisons stationed on selected outer islands (Black 1977:92). The Japanese also imported foreign laborers to help in military preparations. Most of these laborers were Oki-

nawans and Koreans, the latter numbering nearly 2,500 by 1943 (Office of the Chief of Naval Operations 1944b:169; Chief of Engineers 1956:15-16). Another 435 Indians and 103 Indonesians, repatriated in 1947, as well as a number of Chamorros (about 200, according to Hezel 1995:238) who escaped from Babeldaob to U.S. Navy ships in 1944, probably also served as laborers during the war (Palau Community Action Agency 1978:425-427; Nero 1989: 126). The U.S. Navy repatriated all foreigners residing in Palau in 1946 and 1947, except for 350 Japanese laborers and technicians pressed into service on Angaur by an American firm that took over the phosphate mining operation after the war (Useem 1946:79-80, 105; Chief of Engineers 1956: 15,252; Palau Community Action Agency 1978:481-482; see also Useem 1952b:149-150). Roughly 300 Japanese remained on Angaur as late as 1952 (Connell 1983:5).

Internal mobility within Palau played only a minor role in the demography of Palau during Japan's administration. The Japanese controlled all such movement, generally to further commercial development early in their administration and for military purposes later. The Japanese regularly recruited labor for various government projects, with young men often required to relocate temporarily to other parts of Palau (Useem 1946:106). As the war approached, the Japanese repatriated most Southwest Islanders living on Arakabesan to their home islands, dispersing the remainder throughout Babeldaob (McKnight 1977:20). The Japanese moved most of the Chamorros who had worked on Angaur to Ngardmau State by 1941, to work in the newly opened bauxite mines (Hezel 1991:223). Many of the islanders working on Angaur had been moved elsewhere as mining operations expanded (Hezel 1991:223). A few young Palauans actually joined the Japanese war effort, providing support as "survey groups" to troops in New Guinea (Hezel 1995:223). As the conflicts on Angaur and Peleliu approached, the Japanese supposedly evacuated most islanders to other places (primarily Babeldaob) (Useem 1952b:154; Peattie 1988:298; Nero 1989:128). Although American forces found few Dongosaro Islanders on Peleliu, they encountered 182 Palauans on Angaur (Palau Community Action Agency 1978:431, 446). When the U.S. military government became established in Koror State in October 1945, all Japanese nationals were relocated to Babeldaob Island to await repatriation (McGrath 1972:139); Pacific Islanders similarly were evacuated from the town of Koror, to valleys on that island or to Babeldaob (Useem 1946:71; Hezel 1995:225).

Both external and internal mobility increased markedly following World War II. Among the most energetic islanders, Palauans began emigrating to other parts of the Pacific earlier than other Micronesians--despite lacking a heritage of mobility through seafaring (McCutcheon 1981:82). As early as

1970 an estimated 2,500 Palauans lived outside the republic, mostly in Guam, the CNMI, and Hawai'i (Force and Force 1972:123; McGrath 1972:134; Hezel and Levin 1990:45). The number of Palauans living on Guam increased slightly over the ensuing two decades, reaching 1,233 Palau-born individuals by 1990 and 1,858 ethnic Palauans the same year (U.S. Bureau of the Census 1992b:15, 19). Since the war, the Palauan community had become so stable on Guam that it retained the basic family and sociopolitical structure found in the republic itself (Shewman 1981). The number of Palauans residing in the CNMI increased substantially over the same twenty years; in 1990 the commonwealth contained 1,407 residents born in Palau and 1,620 ethnic Palauans (U.S. Bureau of the Census 1992a:16, 20). Note that the figures on ethnicity do not include mixed ethnic groups, thus overlooking many individuals in both places who are part Palauan, Large numbers of Palauans also live in the United States, the total reaching 1,439 by 1990 (U.S. Bureau of the Census 1991).

Although the topic demands further study, most Palauans probably emigrate in search of improved employment and educational opportunities. With the recent adoption of a Compact of Free Association, which includes a provision for open migration to the United States and its territories as noted above, Palauan emigration likely will increase considerably over the coming years unless the republic develops increased economic opportunities for its citizens.

As Palauans leave their home republic, citizens of other countries have begun to immigrate in increasing numbers to Palau. As recently as 1980 the ethnic composition of Palau was predominantly Palauan; only about 1 percent of the total population was ethnic Asian, most coming from the Philippines (U.S. Bureau of the Census 1984c:122). During the first years of the 1980s, foreign workers began to move to Palau in greater numbers, reaching an estimated 350 by about 1982 (Connell 1983:25). By 1990 this situation had changed considerably. Although still composed primarily of Palauans, nearly 13 percent of the republic's population was Asian, the vast majority once again coming from the Philippines (U.S. Bureau of the Census 1992c: 19). As with emigration, immigration into Palau by many of these Asian ethnic groups likely will increase over the coming years--if the republic continues to provide employment opportunities desirable to the groups in question.

Lastly, mobility has played an important role in shaping the geographic arrangement of population within Palau over the past four decades. Shortly after World War II, people living elsewhere in Palau began to migrate to Koror (Kaneshiro 1958:292-293). As noted above, evidence of this trend emerged in the form of increasing percentages of Palau's population living in Koror each census year, growing from about 38 percent in 1958 to slightly

more than 69 percent in 1990. Much of this increase is a result of migration in search of employment opportunities, education, modem health care, and other Western amenities (Force 1960:26). The vast majority of immigrants from other countries also move to Koror, most likely searching for employment (McGrath 1972:141). More than a decade ago, one researcher identified decentralization as the greatest challenge facing Palau, as well as the most elusive goal given the disproportionate investment in Koror's infrastructure (Connell 1983:27-29). The construction of a bridge between Koror and Babeldaob had some effect on the distribution of population, allowing people to live in nearby Airai and still have an easy commute to Koror itself. Plans to relocate the capital of Palau to Melekeok in the near future theoretically could change the geographic arrangement of population considerably, although its ultimate impact will rest largely on the changing distribution of opportunities that accompany the relocation.

Repercussions of Changing Regional Demography in the Republic of Palau

As discussed above, Palau's population has changed considerably over the past two hundred years, declining throughout the nineteenth century and growing throughout most of the twentieth century. Demographic shifts of the magnitude documented almost certainly introduced many other changes to the republic (Force and Force 1972:4). Considerable acculturation for two centuries makes it difficult to isolate the main causes of certain changes, but it is possible to identify likely consequences of the demographic evolution of Palau, as well as their implications for the future of this small island nation. Let us begin by characterizing traditional geographical, political, and social organization.[10]

At the onset of interaction with Europeans in the late eighteenth century, Palau consisted of a collection of independent districts or village-states, best known as the *beluu* of the Palau Islands but present in similar form as independent chiefdoms on the outer islands as well (Kubary 1873:209; Vidich 1980:80; Black 1994:10; see also Service 1971:133-169). Currently the republic comprises sixteen states, which represent the remnants of this traditional organization. With the possible exception of Sonsorol, which consists of four separate islands, the modem states generally correspond to the native districts present at the end of the nineteenth century when the German administration began to stabilize islander politics.

Thanks to the efforts of various eighteenth- and nineteenth-century explorers, traders, and scientists, the Palau Islands are the area best known in terms of traditional geopolitical organization--particularly Babeldaob, Koror,

and the islands proximal to Koror. At the beginning of the twentieth century, each district on these islands usually consisted of a main village and several small hamlets. Even on Babeldaob, districts included coastline as well as interior areas. All settlements tended to lie near the coast, most having moved from more elevated, defensible inland locations earlier in the nineteenth century and consolidated into village-hamlet complexes (Robertson 1876/1877: 46; Kaneshiro 1958:291; Masse, Snyder, and Gumerman 1984:112; Parmentier 1987:58). Competition among native polities occurred throughout the Palau Islands and Kayangel Atoll in the past. By the time of the German administration, two main confederacies existed in the Palau Islands: a southern group led by Koror and a northern group led by Melekeok (Force 1960:34, 38; Force and Force 1972:10).

Access to resources in Palau was organized systematically within the *beluu* (McCutcheon 1981:44-66). Each *beluu* had access to territory that included terrestrial areas--cultivated lands, settlements, mangrove swamps, and so on--as well as parts of the lagoon and reef, with boundaries extending into the sea (see Johannes 1977:123). In general one may classify the land associated with a particular *beluu* into two categories: public community land and land owned by a corporate kin group. The first type of land, called *chutem buai,* ultimately was administered by the paramount chief (McCutcheon 1981:47-51). It could be used by anyone living in the *beluu,* as well as by anyone whose mother was a strong member of a clan in the *beluu.* The second type of land, *chetemel a blai,* consisted of land belonging to the *blai,* a term meaning "house" that Palauans use in a generic sense to signify some type of corporate kin group. Administered by the chief of the relevant corporate kin group, in pre-European times *blai* land included house sites, garden plots, woodlots, and palm forests (Kaneshiro 1958:297; McCutcheon 1981:56-65). Matrilineal members of the particular kin unit automatically had the right to use *blai* land; patrilineal descendants and more-distant relatives could achieve the right through fulfilling certain obligations to the kin unit in question.

Over the years the geographic and social organization of Palau, as well as access to resources, have deviated from the ideals so far discussed. Although it would be unfair to ascribe all sociocultural developments that occurred in Palau to demographic change, in some cases underlying population change certainly would have played an important role. At the heart of such a statement is the notion that traditional social structure, land use, authority, and inheritance rested on strict rules that required the presence of certain components of society--components that dramatic population change would modify. Kubary began to observe sociocultural shifts in the 1870s and 1880s that he attributed to demographic decline. Such changes occurred within

communities, taking the form of shifts in political authority as certain powerful clans became increasingly weak and certain important families had virtually no male heirs (Kubary 1885:80-81). A few generations later, many clans survived only in memory, having long since disappeared due to depopulation; minor villages rarely featured either the ten clans or four lineage classes that characterized *ideal* sociopolitical structure (McKnight 1960:44). Kinship, which had long served as a means of acquiring wealth, evolved accordingly, producing shifts in inheritance rules and marriage patterns (Force 1960:36). Inheritance evolved to include the transfer of titles to men's sons, and adoption emerged as an important means of providing heirs in a society where they no longer existed under traditional social regulations (Kubary 1885:122-123, 149-150; de Valencia 1892:420-421; Smith 1983:35). Change also occurred on a larger geographic scale. Once separate clans and communities combined, based largely on kinship affiliations of those who remained and in response to declining populations in the original settlements (Kubary 1885:142-144; Useem 1946:68). As late as the 1940s Palauans remained concerned about small clans and villages that disrupted various aspects of social organization (McGrath 1972:133).

Many features of modem Palauan culture probably represent the persistence of earlier responses to population decline. For instance, although descent ideally is matrilineal, in reality individuals also trace descent through the male line. Rather than a truly bilateral system, descent in modem Palau is ambilateral--meaning that both lines of descent are recognized but one usually dominates (see Firth 1957:5). In the case of Palauans, for whom the acquisition of power and wealth has long been of considerable importance, the descent line emphasized is the one providing the greatest advantage (Vidich 1949:31; Force 1960:48-49; McKnight 1960:42; Force and Force 1972:42-43). Some researchers see the shift from unilineal to ambilateral descent as a result of depopulation (and possibly acculturation) (Useem 1946: 67; Force and Force 1972:42-43; see also Kubary 1885:80).

Land tenure, which remains very important in modem Palau, similarly has changed dramatically since traditional times, in part possibly due to depopulation and its disruption of inheritance and land-use patterns. Both the German and Japanese administrations viewed much of the *chutem buai* as unused and associated with no individual or component of the native society, perceiving that there was more land than the Palauans could use (Vidich 1980:256; McCutcheon 1985b:67-69). As a result, both administrations annexed this land for use by the government, commercial enterprises, and even private individuals (Bamett 1949:102; Force 1960:73; Vidich 1980:256; McCutcheon 1981:52, 91-93; Abe 1986:69-74). Although the incorrect evaluation of the status of this land can, in part, be attributed to a lack of cultural

understanding by foreign administrators, ultimately a reduced population did in fact require fewer resources, thus making more land available. Moreover, as various corporate kin groups died out or were incorporated into other kin groups, the associated *blai* land could have become either *chutem buai* or part of the *blai* land belonging to other corporate groups. In the case of the former consequence, such land simply became part of the inventory viewed as unused and unowned, and hence available to one foreign administration or another. In the case of the latter consequence, depopulation of the inheriting *blai* could result in a similar loss of land control. Regardless of the ultimate disposition of the land in question, population decline enabled (indeed, in some cases *required*) a shift in tenure patterns. Traditional land inheritance patterns ultimately became so disturbed that in many parts of Palau they have never recovered (Force 1960:73), the trend today leaning toward individual ownership.

The slow population growth that has occurred more recently in Palau has accompanied different social and geographical developments. One demographic change that demands attention is the increasing concentration of population in Koror State. Koror has played an important role in the political, economic, and (probably) demographic composition of Palau since the early 1800s, when that polity rose to power in part abetted by various British benefactors. Nevertheless, despite receiving disproportionate attention from the German and early Japanese administrations, this state still contained fewer than 17 percent of the total Pacific Islander population in 1920. Subsequent nurture of Koror's development throughout the remainder of the Japanese administration and the TTPI period resulted in more than 69 percent of the total population residing in this one polity by 1990.

Such a concentration of population can have both positive and negative consequences. On the one hand, it represents a concentration of demand that reduces the need to redistribute consumer goods, public services, and so on. Moreover, the concentration of population in a single place provides a focus for developing potential productivity. Both of these qualities are important in developing island nations where people often are scattered across vast expanses of ocean and movement is difficult. But such geographic distributions of population can have negative effects as well. Excessive demographic concentration often causes severe pressure on limited natural resources, public services, and economic components of the emerging population center. Social deterioration also can occur as traditional mores become less important among populations with differing social connections--such a breakdown was possibly responsible for some of the juvenile delinquency, drunk and disorderly conduct, criminal behavior, and alcoholism that emerged in Koror town by the late 1960s (McGrath 1972:142). Moreover,

because such demographic change largely results from rural-urban migration, as early as the mid-1950s many migrant origins had lost key components of their populations to Koror (Force 1960:26), mainly young and middle-aged adults who relocated in search of education or employment. As a result, much of rural Palau has become under-productive, due to a lack of key human resources, and increasingly unable to maintain its own sociocultural system--a problem occurring with alarming regularity throughout more isolated parts of Micronesia (see Marshall 1979; Levin and Gorenflo 1994). In Palau's case this process goes one step further than in most developing countries: many individuals migrating from rural parts of the republic to Koror as well as those originating in Koror ultimately leave Palau, removing a key component of the society from the republic as a whole. Emigration of Palauans has been countered recently by immigration from other countries, particularly the Philippines. Immigrants often are working-age males, skewing the age and sex composition of the republic. Moreover, these immigrants bring with them vastly different sociocultural backgrounds. Although such immigration dampens the numerical effects of Palauan emigration, it introduces additional cultural impacts.

Demographic changes also have affected the Southwest Islands and Kayangel Atoll. Our best glimpse of the outer-island cultures in Palau comes from the research of Black on Hatohobei Island (see Black 1977). Based on this work, we know that the matrilineal chiefdoms characteristic of most of the Carolines were found in the Southwest Islands as well. All the outer islands in Palau with the exception of Kayangel Atoll have experienced considerable depopulation. In the case of Hatohobei this is particularly true, its population declining by nearly 98 percent between 1909 and 1990. In 1972 when the islands population numbered 126, composed mainly of the old and young, three of the five main clans had become virtually extinct (Black 1977:35, 38-41, 52); with a 1990 population roughly one-sixth that recorded eighteen years earlier, any social order founded on these clans is a thing of the past. Similar changes occurred on other outer islands. By the mid-1950s Meleili Island had lost most of its population to Koror State, the few young and old persons remaining poorly suited to collect coconuts or grow taro (Osborne 1966:49); in 1990 this small island was uninhabited. Although chiefs retained remnants of their traditional power in many of these places (see Black 1983:7-9), the matrilineages that formed the basis of social organization had all but disappeared.

Since World War II, Palau has joined the other island groups associated with the former Trust Territory in becoming increasingly dependent on assistance from the United States. With the recent signing of the Compact of Free Association, the question of Palau's national status appears resolved.

Yet if other Micronesian examples of ratifying the compact are any indication, the funds earmarked for development assistance that accompany the agreement do not guarantee a move toward self-sufficiency. During the Japanese administration earlier this century, Palau emerged as a productive part of the Mandated Territory despite supporting a population much larger than today's With the recollection of this prior success, and given the constraints as well as possibilities embodied in the current demographic situation, what are Palau's prospects for the future?

The Japanese administration focused on three main areas of development in Palau: fishing, agriculture, and mining (Peattie 1988:170). Of the three, agriculture played a relatively minor role until late in the Japanese administration. Copra, the agricultural mainstay of most of the Mandated Territory, never realized the success it enjoyed elsewhere in the region. Although the Japanese attempted to explore the potential of other crops at three agricultural research stations and eventually introduced commercial alternatives to copra (such as pineapple; Useem 1946:114), agricultural development was never the unqualified success it was elsewhere in the Mandated Territory. That stated, after considerable effort agriculture eventually became quite productive during the 1930s (Peattie 1988:102, 174). Fishing became very successful in Palau, also during the 1930s, attracting large numbers of Okinawans and generating much of the secondary economic development experienced in Koror during that decade. As previously discussed, mining helped to compensate for some of the early disappointments in agricultural development.

The development experienced during the Japanese administration required considerable investments of funds and planning, but success with a population much larger than currently found attests to the republic's potential. Unfortunately, although useful in terms of providing insights to possible areas of development, certain realities during the Japanese administration no longer hold. Most notably, the phosphate deposits on Angaur have long since been exhausted, while deposits elsewhere in Palau proved inadequate to justify continued mining. Bauxite deposits similarly were not sufficiently rich to attract extended attention. But fishing and agriculture remain potentially important options for development, along with tourism.

A focus on fisheries potential in Palau is justifiable given the richness of the natural environment, particularly that associated with the Palau Islands and the barrier reef that defines an enormous lagoon rich in resources (Kubary 1895:122; Johannes 1981:1-2). Estimates indicate a potential annual harvest of up to eleven thousand tons of fish per year within the reef alone (Johannes 1981:79), the waters lying beyond the reef containing many additional resources (Read 1971), though reduced catches in recent years pro-

vide a keen reminder of the need to manage national fisheries carefully (Stolzenberg 1996). Traditional adaptive strategies reinforce the wisdom of a focus on fishing, as Palauans became some of the best and most knowledgeable fishermen in Micronesia (see Helfman and Randall 1973). Combining rich natural resources, an indigenous population with great talent for exploiting the sea, and energetic Okinawan immigrants, Palau successfully made the transition from subsistence to commercial fishing during the Japanese administration. Despite rich resources and a heritage of high productivity, by the early 1970s Palau imported about one-third of its animal protein in addition to other types of food (Johannes 1981:71). Reasons for this pattern include damage to marine resources during World War II, but the main problem is underdevelopment of both subsistence and commercial fisheries, compounded by poor marketing and distribution, which yield irregular supplies.

Agricultural development at first glance shows great promise. During traditional times agriculture played an important role in providing subsistence --dominated by taro, but with coconut, banana, sweet potato, tapioca, pineapple, citrus, mango, breadfruit, tobacco, and assorted vegetables also important (many of these crops introduced by early explorers) (Kubary 1892: 156-162; Krämer 1926:41-107; Eilers 1935:24-27, 169-175, 304-306; Useem 1946:61; Force 1960:29-31; Read 1972:14-15; Black 1981; Abe 1986: 37-39; McCutcheon 1986). Current agriculture in the republic generally consists of small-scale subsistence production practiced by only part of the population. Although the growing urban center generates a constant demand for fresh produce, the potential identified in the mid-1980s for rural market gardens to meet this demand never came close to realization (McCutcheon 1985a; see also Hankin and Dickenson 1972). The reason for the lack of recent agricultural development is not solely an absence of desire on the part of Palauans; there also are fundamental organizational problems, primarily nonspecialization (McCutcheon 1981:232). The eventually successful agricultural production during the Japanese period was not without cost. One of the most important was soil depletion, primarily through deforestation that led to widespread erosion and nutrient reduction.

As is the case with islands throughout Micronesia, most of Palau is not well-suited for large-scale agricultural production. Limitations of coral and limestone islands are obvious, with a relatively thin layer of humus atop otherwise very poor soil (Useem 1946:61; Wiens 1962:332-352). But important limitations also characterize the volcanic islands of Palau (Vessell and Simonson 1958). Most bottom land is mucky and poorly drained, its agricultural potential limited to a crop such as the wetland taro that was so important in traditional Palau. Wetland taro, however, requires much more intensive cul-

tivation than most modem Palauans are willing to invest for a commercial crop. Upland soils are better drained and fine-textured, making them more attractive for a broader range of crops. But as in many tropical settings, the quality of these soils relies heavily on the maintenance of a forest canopy that protects them from leaching by heavy rains, retains nutrients, continually replenishes organic content, and provides protection from erosion. Soil scientists recommend forest crops in this natural setting. Consistent with such a general strategy, they recommend the avoidance of clean-tilled agriculture. Unfortunately, most of the modem agriculture in Palau is the latter type, emphasizing crops such as taro, cassava, sugarcane, sweet potato, and various vegetables (like eggplant and tomato). This has produced widespread erosion that continues to hinder much of Palauan agriculture (Office of the Chief of Naval Operations 1944b:17; Chief of Engineers 1956:92-94; Soil Conservation Service 1983:11-39). Compounded by additional problems, such as the presence of aluminum in some soils that inhibits plant growth (Soil Conservation Service 1983:42), agriculture unfortunately is not the development panacea that many think.

Palau's development options were explored systematically in the recently adopted national Master Development Plan (Republic of Palau 1995). The above paragraphs at best provide a cursory examination of certain major development options explored in the plan in greater detail. But one cannot help thinking that the levels of success once achieved in the republic should be within reach again. In one sense demography supports such a contention: there are substantially fewer people living in Palau now than during the Japanese administration, even discounting the massive influx of military personnel during World War II. But the demographic situation also provides certain constraints. One is the geographic distribution of population, with nearly 70 percent of the total residing in the urban center of Koror. Another is the demographic composition of the republic, greatly altered by both internal and external migration patterns. As in other parts of Micronesia, successful development in Palau will require careful attention to demographic issues. But whereas elsewhere this attention must focus on controlling excessive demographic growth, Palau's challenge is one of retaining a sufficient Palauan population to provide a solid base for development.

Concluding Remarks

Population change in Palau over the past two centuries in general has resembled that found throughout most of Micronesia: an extended period of depopulation following the establishment of prolonged contact with Europeans, and a subsequent period of population growth throughout most of

the present century. The main cause of depopulation was an increase in mortality, probably accompanied by reduced fertility. Demographic growth initially resulted from a reversal in the relationship of these two mechanisms, the number of persons born into the population each year greater than the number that died. Over the past three decades international migration has begun to play a particularly important role in the demographic evolution of Palau--initially in the emigration of many Palauans to other places, in part counteracted more recently by the immigration of relatively large numbers of southeast Asians. Inadequate data on the size of Palau's precontact population leaves uncertain the amount of depopulation that occurred during the nineteenth century. However, even using the conservative estimate of 20,000 inhabitants prior to European contact and a minimum of 3,500 inhabitants around 1900, depopulation in excess of at least 82 percent apparently occurred in little more than one century. In part due to the dampening effect of emigration, Palau's population has yet to reach 20,000 again.

Palau has been blessed with much potential for future development. To begin with, Palauans have long been known as some of the most energetic, competitive people in Micronesia. Building on the sustained contact with non-Micronesians that began in the late eighteenth century, Palau rapidly became one of the most highly acculturated parts of Micronesia, its inhabitants anxious to learn the ways and acquire the technology of outsiders. In addition, the nation's natural setting provides more potential for development than much of Micronesia. Although probably the most valuable mineral deposits were removed earlier this century, the potential for fisheries and agricultural development remains unrealized. With the great majority of Palau's population residing on Babeldaob and Koror islands, and a recently constructed road on Babeldaob, transportation to match availability with demand is not nearly as much of a challenge as found in other multi-island polities throughout Micronesia (see Gorenflo 1990, 1993a). Finally, Palau's proximity to Japan already has borne fruit in the form of a well-established tourism industry, an area of development that also shows great promise.

With the recent signing of a Compact of Free Association between Palau and the United States, the republic will have available considerable funding with which to pursue various development options. Many of these options have emerged in the national Master Development Plan. Unfortunately, for Palauan development the compact is a two-edged sword. On the one hand, it will provide the funding necessary to pursue many development goals, but on the other, it will also make it easier for Palauans to emigrate to the United States and any U.S. territory. Given their tendency to emigrate, Palauans may well take advantage of this new status and leave the republic in greater numbers, thereby removing many of the young, educated, and more enter-

prising people from the republic. Time will tell with regard to the ultimate impact of the Compact of Free Association on Palau. Given the potential for successful development in the republic, as well as the potential for increased loss of the Palauan population, the implementation of a realistic, workable national Master Development Plan is increasingly important in this emerging island nation on the western edge of Micronesia.[11]

NOTES

Mayda Riopedre, of the Smithsonian Institution Anthropology Library, kindly made available several rare sources central to this research. Dave Kelly and other members of the Library of Congress reference staff helped track down other key sources. Some of the data compilation and analysis discussed in this study occurred while I examined the demography and economy of Palau as a consultant to the U.S. Department of Interior Office of Territorial and International Affairs (OTIA). The OTIA work was a collaborative effort with Huan Hosei and Mike Levin, both of whom provided a number 'of insights on the evolution of Palau's population. Mary McCutcheon generously made available both her personal library and her time, lending sources and answering several questions about the economy, ecology, and social organization in the republic; in addition, she read and commented on a preliminary draft of this article, recommending several improvements.

By way of a more extended acknowledgement, I would like to thank Dale Robertson for publishing the six-paper series of demographic studies (three coauthored with Mike Levin) of the Caroline and Marshall Islands in *Pacific Studies*--despite their length and difficulty of publication--thereby providing a single, high-quality location for these related efforts. Finally, I thank Sharlene Rohter for her tireless and absolutely first-rate editorial efforts in producing all six articles, making all look and read better than I would have ever thought possible.

1. In the interest of clarity and consistency, I follow modem conventions and use the terms "Palau" or "Republic of Palau" to denote the polity on the western edge of Micronesia currently designated by the latter name. I use the term "Palau Islands" to identify the islands extending from Babeldaob Island in the north to Angaur Island in the south, thus excluding Kayangel Atoll as well as the Southwest Islands. Throughout this article I refer to the sixteen main political components of the Republic of Palau as "states," although these geopolitical entities were known as municipalities, divisions, or *beluu* earlier this century. The spellings used for the individual states correspond to those in the *Statistical Yearbook 1992* (Office of Planning and Statistics 1992): Aimeliik, Airai, Angaur, Hatohobei, Kayangel, Koror, Melekeok, Ngaraard, Ngardmau, Ngaremlengui, Ngatpang, Ngchesar, Ngerchelong, Ngiwal, Peleliu, and Sonsorol. These names differ slightly from those used in the 1990 census publications (as well as other sources).

2. The role of prehistoric terraces on Palau is uncertain. Functions as diverse as agricultural field boundaries, defensive earthworks, and village locations have been proposed by various researchers (see Parmentier 1987:30-32; Masse; Snyder, and Gumerman 1984: 119-120). The most popular interpretation is that the terraces were agricultural (see Osborne 1966:79, 269; Lucking 1984), though research still has not demonstrated this con-

clusively. The presence of such elaborate agricultural terraces, evidence for intensive food production in parts of the republic where such land use has not occurred during historic times, helps support the contention that Palau once supported a very large population.

3. There are two main problems with the pre-Japanese population figures. The first concerns the accuracy of the numbers themselves. Although this difficulty is characteristic of noncensus population data, it is particularly a problem when discussing the precontact estimates for Palau, which range widely. Other researchers with an interest in the history of Micronesia believe that precontact estimates of 40,000 or higher are excessive (F. Hezel, personal communication, 1995), a point with which I tend to agree. Unfortunately, neither adequate data nor systematic studies exist to support a convincing argument in favor of one particular figure. For present purposes, I have provided the estimates prepared by others interested in this topic, for the reader's consideration. Regardless of the precontact figure accepted, the important point to bear in mind is that considerable depopulation occurred during the century following the onset of reasonably sustained contact with Europeans in the late 1700s.

The second problem with the pre-Japanese population figures is geographical,. concerning precisely *what* part of Palau any one estimate represents. Many early figures probably refer to Koror, Malakal, Arakabesan, and Babeldaob islands, the places where several of the early estimators were most active, including Semper, Kubary, and Krämer. In some cases, estimates possibly included Peleliu and Angaur as well, although data available for Angaur alone suggest that this was not always the case (see Table 1). Pre-Japanese estimates of Palau's population probably excluded the Southwest Islands, a part of the modem Republic of Palau that contained relatively large numbers of people during the early 1900s but remained conceptually and politically separate until the Japanese administration.

4. Figures for the number of people exiled from Pohnpei following the Sokehs rebellion range from 369 (Yanaihara 1940:42) to 460 (Hezel 1995). One of the higher totals, 447, included 21 individuals from districts other than Sokehs (Office of the Chief of Naval Operations 1944a:20), possibly helping to explain part of the discrepancy with smaller figures.

5. The Palau Community Action Agency conducted a census of Palau in 1979-1980. I have not discussed this census with the others largely because key aspects of how it was conducted are uncertain and because it did not involve systematic census techniques, such as a formal edit that would help to minimize problems such as double-counting. The omission of this census does not imply a belief that it was inaccurate, an issue that is impossible to address given the techniques employed. Instead, its exclusion reflects a lack of methodological comparability with the other censuses considered.

6. The precise nature of the counts of population in the former Japanese Mandated Territory conducted shortly after World War II are uncertain. Most likely these efforts fell somewhere between estimates and formal de jure or de facto censuses. The "head count" conducted by native police in Chuuk State probably represents what occurred throughout the area (Pelzer and Hall 1946:6). In a previous study of Kosrae I treated the data collected by one of these postwar enumeration efforts as a census, in part because the relatively simple geographical setting should have enabled an extremely accurate population count and in part because the data included information on age and sex composition

(Gorenflo 1993b:85-86). Greater caution probably is in order when examining most of the censuses conducted in Micronesia immediately after the war, however, particularly in a complex geographic setting such as Palau. Tables 2 and 3 present the population figures recorded by the U.S. military in 1946 and 1947, in the interest of making available as much data as possible, but I have avoided careful analyses or drawing many conclusions from these figures.

7. Because my primary aim is to explore demographic change in a functioning sociocultural system, this study focuses mainly on Pacific Islanders in the 1920, 1925, 1930, and 1935 censuses. The numbers of Japanese residing in parts of the Mandated Territory varied dramatically over the three decades that Japan controlled the area. The Japanese government regulated migration in its Micronesian possessions, promoting increased relocation of Japanese and Okinawans to many of these islands for commercial or military purposes during the 1920s and 1930s. Considering such *imposed* in-migrants would cloud our understanding of the demographic evolution of Palau, Only on Angaur, where the Japanese promoted the immigration of Pacific Islanders (which all four Nan'yo-cho censuses would have recorded), do the census counts from the Japanese administration include many islanders who probably would not have resided in a particular place unless somehow strongly encouraged to do so. Similar migration *possibly* occurred on Dongosaro, Hatohobei, and Peleliu islands. Unfortunately, particulars on the mining operations at each place are sketchy--though we do know that the mines were much smaller and more short-lived than those on Angaur and thus would have required fewer imported laborers.

8. Exploring modem Micronesian mobility patterns defined with data on "place of birth" can introduce problems beyond those of simple consistency. Women from rural places often give birth in hospitals, returning home shortly thereafter. Subsequent questions concerning place of birth often receive responses identifying the location of the hospital instead of where the mother normally resided, thereby providing confusing information on mobility. In lieu of alternative information, I present data on emigration where "origin" is defined by place of birth, to provide a glimpse of emigration in a Pacific nation where mobility has long played an important role.

9. A population decline between 1973 and 1980, during a period generally characterized by demographic growth, might cause one to question the accuracy of the 1980 census. However, as noted elsewhere in this study other factors apparently played an important role in shaping Palau's 1980 population, notably evidence of considerable emigration. These additional considerations, coupled with the tendency of mobility to occur in cycles (introducing the possibility that emigration might have been more important in the late 1970s than during the early 1970s or mid-1980s), make it difficult to dismiss summarily the accuracy of the 1980 census. More easily discounted is the 1970 census, with its questionable counts and widely recognized methodological problems.

10. The outline of Palauan social structure presented here considerably simplifies a very complex situation. One reason I cite so many references on social organization is that several researchers have interpreted Palauan social structure differently. In addition to complications of the system itself, Palauans use key terms in various ways--often employing the same term to describe vastly different components of the native social order (see Force 1960:44-48; Force and Force 1972:43-49). Even characterizing Palauan social structure as matrilineal is not strictly correct, as discussed in greater detail below.

11. The government of Palau conducted a census between September and December 1995 (M. Levin, personal communication, 1996). Unfortunately, data from the 1995 census were unavailable when this article was completed. Preliminary results indicate continued population growth during the first half of the 1990s, with the number of inhabitants in both Koror and Airai increasing as well. A strong representation of Asians also appears to persist, suggesting continued immigration, though specific conclusions on this and other aspects of the demography of 1995 must await a careful analysis of the most recent census data once they are processed and released.

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"WAY BACK IN PAPUA": REPRESENTING SOCIETY AND CHANGE IN THE PUBLICATIONS OF THE LONDON MISSIONARY SOCIETY IN NEW GUINEA, 1871-1932

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From the commencement of their mission to New Guinea in 1871 to the early 1930s, members of the London Missionary Society (LMS) published a stream of articles and books describing the peoples they encountered. Addressing mission supporters and scientific audiences, the missionary ethnographers inevitably drew on the anthropological assumptions of both in making sense of Papuan societies. This article examines the shift in LMS representations of Papuans from racialist constructions of the person to more relativistic understandings of whole societies. I argue that this shift reflects changing fashions in metropolitan anthropology, greater familiarity with Papuan society, and a deepening and more humble sense of the missionaries' power to bring change. The missionary ethnography literature forms a kind of intermediary discourse, drawing on both evangelical and anthropological assumptions and concerns of the day.

THIS ESSAY DISCUSSES the large body of writings describing the indigenous peoples of southeastern New Guinea published by members of the London Missionary Society (LMS) from the organization's arrival there in 1871 to its consolidation in the 1920s and 1930s.[1] These writings provide a rich, if controversial, source of information on Papuan societies at the time of contact,[2] on early missionary efforts, and on missionary aims and understandings. I intend to examine these publications more broadly as an example of "colonial discourse." In particular, I wish to explore how the missionary authors' thinking about Papuans may have been conditioned by the necessities of winning continuing financial and material support from home congregations,

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on the one hand, and by changing fashions in the anthropological representation of non-Western cultures, on the other.

If we define ethnography in its broadest sense, most missionaries working in the Pacific Islands can be said to have attempted at least some ethnographic description in the course of their careers. They needed some familiarity with indigenous cultures and languages if they were to have any hope of success. Most of these fragmentary efforts ended up in letters, journals and reports, but a few missionaries attempted more sustained descriptions inspired by personal inclination and talent and often the direct encouragement and guidance of metropolitan scholars. Prior to the First World War, and in a few places for a time afterwards, missionary ethnography dominated the published firsthand accounts of Oceanic peoples. These contributions dropped off owing to a combination of factors including the consolidation of missionary work and, most importantly, the gradual displacement of amateur ethnographers by professionally trained academic anthropologists. Missionary anthropologists, however, have continued to make significant contributions to the academic literature up to the present (e.g., Böhm 1983; Gesch 1985; Tippett 1967; Whiteman 1983). More intriguing, and still awaiting study, is the critically influential position the Pacific churches have developed in supporting regionally based and published studies of indigenous society, culture and language.[3]

At mid-century, no less an authority than A. P. Elkin, himself an Anglican minister, dismissed most mission ethnography dealing with New Guinea as hopelessly ethnocentric (1953:3). The charge is far too sweeping. Even flawed writings provide an indispensable record of cultures in the early contact period--and, in fact, many are very good.[4] As with all documentary sources, missionary ethnographies need to be read critically; and critics, in turn, need to be humble enough to acknowledge the concerns and biases they bring to their readings (Whiteman 1985).

In this article, I limit my attention to the *published* LMS writings, viewing them as public statements addressed to specific audiences. Hence I do not consider the ethnographic value of these books and articles, an assessment which can be made only from intimate local knowledge of the people described. Nor do I read this published literature as a direct reflection of missionary attitudes toward local people or as a history of their efforts in New Guinea.[5] Though a handful of reports, theses, and books have appeared we still await a solid history of the London Missionary Society in Papua.[6] Such a history will depend less on the published works than the very extensive archival sources available as well as oral histories from local historians.[7] Here I consider what the missionaries *wanted* the outside world to know about their mission and about the people they were encountering.

The LMS authors addressed two distinct audiences in their published writings: scholars interested in New Guinea and mission supporters. The literature in turn reflects two genres of colonial writing, tailored to the audience. The first genre can be described loosely as that of the natural sciences. From the 1870s on, missionary writers addressed audiences of anthropologists and geographers, attempting to make use of both their "language" and key concepts. The second genre can be described, for lack of a better term, as "propaganda": information designed to solicit support from readers and encourage new recruits.[8] Missionary propaganda, as Thomas observes in the case of Methodists in the Solomon Islands (1992), employed ethnographic descriptions coupled with reports of mission progress to drive home the point that the natives needed salvation, were capable of salvation, and were actually being saved through the intervention of the missionaries.

In their respective histories of Western ethnographic thought, Fabian (1983) and McGrane (1989) draw a sharp distinction between the universalist and relational discourse of salvation, associated with Christian thought, and the particularist and essentializing discourse of nineteenth-century natural science. The LMS writings strike me as a kind of intermediate discourse, lying in a state of creative tension between the developing practices and guiding ideas of the missionary project and the shifting natural-scientific discourse of scholarly anthropology. Although salvation and the natural sciences rest on radically different premises, they each informed the other in the LMS publications, leading to complementary shifts in mission propaganda and the "scientific" presentation of Papuans. Before turning to the literature, we need to briefly review the historical circumstances that favored and conditioned this particular merger of salvation and science.

Missionaries and Anthropologists in Southeastern New Guinea

In 1870, the year the directors of the London Missionary Society decided to extend their seventy-three-year-old Pacific mission westward, the south-central coast of New Guinea remained largely unexplored. While Holland had already claimed the eastern half of the island, fourteen years would pass before Britain and Germany annexed the rest. The pioneer missionaries, all veterans of the established missions to the east, operated with meager resources. They concentrated on exploring the coast and coastal hinterland, establishing friendly relations with the inhabitants, and settling Islander teachers from the older missions in local communities. These teachers, who greatly outnumbered their European supervisors, carried out the routine work of teaching and preaching (Crocombe and Crocombe 1982). A. W. Murray and William Wyatt Gill, who wrote early reports, only briefly visited

the Torres Strait and southern shores of the big island. Samuel McFarlane, who left his successful mission on Lifou in the Loyalty Islands, supervised teachers mostly in the western end of the mission field from a base in the Torres Strait between 1871 and 1886. William Lawes became the first European to settle on the southern New Guinea coast, building a house at Port Moresby in 1874. The most famous of the early LMS missionaries, James Chalmers, arrived from Rarotonga in 1877. He spent most of his years in New Guinea exploring the coastal regions, until he was murdered at Goaribari in 1901 (Langmore 1974).

These pioneer LMS missionaries attracted a large audience both inside and outside missionary circles. They were invited to speak before political, geographical, and anthropological societies. The early missionary ethnographers appear to have been independent and respected partners in the ethnographic enterprise.[9] William Wyatt Gill, for example, became an Oceanic ethnographer of note and one of the founding members of both the Australasian Association for the Advancement of Science and the Polynesian Society (Gunson 1974:193). He is also remembered for convincing A. C. Haddon to visit the Torres Strait in 1888 and later encouraging him to switch from zoology to ethnological studies (Quiggin 1942:21, 90).

The LMS missionaries gradually consolidated their hold over the south-central coast and Torres Strait islands. By 1900, they had divided the mission field into twelve districts, had trained the first Papuan pastors, and had established schools for advanced education and industrial training on several mission stations. A steady flow of prospectors, planters, administrators, naturalists, and travelers had by this time entered the region, many of whom also put their impressions to paper. With the arrival of the Cambridge Expedition to the Torres Strait in 1898, professional anthropologists became the latest invaders.

Many of the LMS missionaries from the 1890s on had some direct contact with British anthropologists. They began by responding to questionnaires prepared by the Royal Anthropological Institute and other organizations (see, for example, Chalmers 1890, 1903a, 1903b; Hunt 1898). The association developed into a partnership after 1898 (Langmore 1989: 110-116). Several missionaries served as translators and informants for A. C. Haddon and C. G. Seligmann during their survey tours of (then) British New Guinea (Seligmann 1910). Haddon sponsored J. H. Holmes as a special correspondent for the Royal Anthropological Institute (Reid 1975, 1978; Holmes 1902a, 1902b, 1903, 1905, 1907). Haddon also edited the mission handbook (Martin 1908) to reflect the anthropological orthodoxy of the day and provided seminars for missionaries on the way to the field. In the 1920s, three missionaries with extensive experience wrote ethnographic monographs on Papuan peoples.

Holmes summed up his studies of the Gulf of Papua tribes (1924). E. Baxter Riley, encouraged by linguist Sydney Ray, wrote about the Kiwai people at the mouth of the Fly River (1925). And William Saville (1926), once the reluctant host to Bronislaw Malinowski, wrote a monograph on the Mailu people after attending Malinowski's seminar at the London School of Economics (Young 1988).

This collaboration resulted in articles and books differing significantly in content and tone from earlier works. Utilizing questionnaires provided by anthropologists, missionaries now covered a broader range of topics more systematically and in far greater detail. The mission ethnographers attempted to adopt (not always successfully) a neutral stance in their writings. They dropped most personal references and they described native ideas and behavior in dispassionate, seemingly objective terms (e.g., Hunt 1898:5). For example, Holmes began his major work on the gulf tribes with a salute to scientific relativism: "at the cost of disappointment to many old friends, I have deliberately endeavoured to portray the Papuans I have known as they regarded themselves. Their views of life do not lack a philosophy which was intelligible to them. I do not endorse them, neither do I condemn them" (1924:1). Several of missionary ethnographers adopted some of the ruling anthropological concerns of the day: social evolution, the diffusion of cultural traits, the integrative function of social institutions. This literature thus marks a notable instance of the "capture" of missionary ethnography by a professionalizing anthropology, based on the discourse of the natural sciences (cf. Thornton 1983).

This anthropological makeover, however, was far from complete. When read in conjunction with missionary propaganda of the time, as I do below, the "scientific" writings of the missionaries show clear connections with contemporary reflections on the progress of the mission. The more relativist and sociological stance adapted by missionary anthropologists complemented a gradually liberalizing mood in the missionary movement as a whole, on the one hand, and a more realistic assessment of the pace of possible change in Papuan communities, on the other. There was no simple cause and effect at work here, but one can discern a dialogue between two quite distinct perspectives over time.

Early Impressions, 1871-1890

Most of the LMS writings on New Guinea in the 1870s and 1880s presents native life in the context of the missionary encounter. The missionary is usually present in the text; his direct observations and anecdotes provide the key source of information. Missionary writings fell along a continuum, rang-

ing from the popular to the scientific. At one extreme, James Chalmers, the "Livingstone of New Guinea," wrote popular travel books describing his adventures and explorations. At the other extreme, Williams Lawes wrote systematic descriptions of the customs of certain New Guinea peoples, making only limited references to his own presence and experiences (1879, 1880). Narratives of mission visits to local communities, peppered with ethnographic observations, occupied the broad center.

Race and Character in Early LMS Ethnology

Samuel McFarlane opens his book, *Among the Cannibals of New Guinea,* with a vision of a lost world.

> Whilst empires have risen, flourished, and decayed; whilst Christianity, science and philosophy have been transforming nations, and travelers have been crossing polar seas and African deserts, and astonishing the world by their discoveries, New Guinea has remained the same . . . where the natives can be seen in the cocoanut groves mending their bows and poisoning their arrows, making their bamboo knives and spears, and reveling in war and cannibalism as they have for ages . . . it comes with a sense of relief to visit a country really new, about which little is known, a country of bona fide cannibals and genuine savages, where the pioneer missionary and explorer truly carries his life in his hand. (McFarlane 1888: 14-15)

Laying on the purple prose, McFarlane evokes a picture of New Guinea as a place simultaneously exotic and familiar, new and ancient, filled with unspeakable savagery and dangers, indescribable beauty and riches.[10] Although the land and peoples of New Guinea really were new to the Victorian reading public, this picture was surely familiar. Brian Street describes a similar setting found in contemporary novels set in Africa. The distant lands possess a "dream-like quality" into which intrude "the latter-day orders of chivalry": white explorers, travelers, military men, and, of course, missionaries (1975:21).

New Guinea was nonetheless "really new" to the missionary pioneers. Confronted with strange and sometimes frightening practices, struggling with languages they could not understand, they initially made sense of the encounter through what they already knew of exotic places, of "savage" peoples, and of their own ancestors in ancient Europe.. Hence even the most novel discoveries took on familiar colorations: "a creature of the monkey tribe" and a twelve-foot-long feline that sounded like a tiger, both rumored

to live in the deep jungle (Chalmers and Gill 1885:110, 112); a "queen" holding such great power over her people that she was carried in a hammock by men whenever she traveled (ibid.:189-190); and an island inhabited by "Amazons" reported by the Motu living at Port Moresby (ibid.:74-75; McFarlane 1877:356).[11]

Reflecting popular racialist attitudes of the day, the pioneer missionaries believed that the physical appearance of New Guineans provided immediate insight into native society and character. On their maiden voyage to New Guinea from the Loyalty Islands, McFarlane and Murray concluded that two races inhabited south-central New Guinea: the light-skinned "Malays" in the east and the dark-skinned "Papuans" in the west. They unhesitatingly identified the former with Polynesians and the latter with Melanesians. The racial framework gave the missionaries comfort, for they believed they knew what to expect. Hence William Turner's confident ethnographic description of the Motu. Although he had lived with them only a few months, he could draw upon his longer experience with comparable Samoan "Malays." Like the Samoans, the Motu demonstrated "ingratitude, circumlocution in speaking, kindness to children, freedom and carelessness in boating"; both peoples became excited and noisy only "when working in great companies . . . and in war" (Turner 1878:493). There were differences as well, but Turner had no doubt he was dealing with essentially the same people.

Turner (1877) and Lawes (1880) refer to Wallace's *The Malay Archipelago* (1869) as their source on the Malay/Papuan races. But the hierarchical racial distinction already had wide currency in the South Pacific and survives today, according to Thomas (1989), in the Polynesian/Melanesian cultural contrast. For those nineteenth-century writers inclined to polygenist positions, the distinction was undoubtedly racist.[12] Langmore (1989), however, argues convincingly that the missionaries were monogenists, who saw the contemporary differences between the New Guinea races as the result of history and environment, not biology, and hence remedial. Otherwise there would have been no possibility of conversion and no point to missionary efforts. McFarlane (1888), for instance, placed the New Guinea races within the broad framework of Christian history, identifying them as descendants of Noah's cursed son, Ham. New Guinea society and language, he claimed, showed definite evidence of degradation from a higher state of civilization. Only Christianity could stop and reverse this decay. Mutability, however, need not imply equality. Few missionaries speculated on the future relationship of whites and natives and their writings leave one with the impression of deeply ingrained differences between the races.[13] In this racialist framework, therefore, we find a convenient marriage of natural science and biblical history.[14]

Race provided a very loose framework within which the missionaries could

locate their key concern: the nature of native character and morality. This they directly deduced from outward appearance and actions. Impressed by the attractive village setting of the Kerepuans, McFarlane saw them as intelligent and industrious, with a keen sense on how to drive a bargain (1877: 353). Hanuabada's "filth," on the other hand, spoke to Turner of the conservative and indolent character of its inhabitants (1878:494). The "Papuans" and "Malays" were compared and ranked according to various indices. Most missionaries pointed to male treatment of women. "Papuan" women appeared as little better than slaves (e.g., Gill 1876:223-224; Murray 1876: 455), but "Malay" women enjoyed a much stronger position (and thus the missionaries could seriously consider that their numbers included some "Amazons"). During the initial contacts, clothing (or its lack) also provided a good indicator of civilization. While the Papuans, according to Gill, "glory in their nakedness, and consider clothing to be fit only for women," the Malays of Redscar Bay readily accepted cloth offered by the missionaries, thus demonstrating "the instinct of shame, which alone elevates them immeasurably above the black aborigines of the south-west coast of New Guinea" (Gill 1876: 230). Chalmers considered the Maiva to be superior in many ways to the Motuans, not least because they were "more respectfully dressed, having nearly as much covering as necessary. When they came to call on me each had a piece of native cloth hanging down his back" (1902:194). The missionaries were less critical of women for their nakedness, seeing this as evidence of their suppression by men. "To woman, in her lowest degradation," wrote Gill (1892:7), "is given an instinctive modesty; but the men in heathenism are very different, 'glorying in their shame.' " Gill thus approved of the "Malayan" men of Redscar Bay who both appreciated gifts of clothing and treated their women well (1876:242).[15]

While not regarding the "Malays" as saints, most of the missionaries considered them superior to the "cannibalistic" and dark-skinned "Papuans" to the west. According to Turner, "The opinion that the Motu are distinct from the Papuan is strengthened also by the fact that they look down upon the black people as being much inferior to them. For example, Papuan teachers from the Loyalty Islands were looked upon as an inferior class of men, while Malays--Polynesian teachers from Eastern Polynesia were received and treated as equals" (1878:473).[16]

Gill proposed even finer distinctions: "Although the Papuans of New Guinea were superior to the natives of the islands of the Torres Strait, they were inferior to the Papuans of the South Seas; while . . . the Malays of New Guinea were superior to any he had met on the South Sea Islands" (1874: 48). Yet the missionaries did note anomalies. The New Guinea "Malays," for

instance, were surprisingly rude. Gill complained about an annoyed woman who ran him out of a house he was inspecting in Manumanu, and Turner reported, “When visiting in a European house [Motu natives] choose the best seat . . . and never squat upon the floor in token of respect, as do the South Sea Islanders” (Gill 1876:249; Turner 1878:495). Further, the missionaries soon discovered that the “Papuans” in the Gulf of Papua-Fly River area had elaborate ritual complexes with superior architecture and arts to those of the “Malays.” This led Chalmers to suggest “there is a kind of civilization amongst these people” (1902:54). McFarlane, who made his base in the Torres Strait and worked closely with the “Papuans,” rose to their defense. They were “as a rule a good-tempered, liberal people--greatly superior in these qualities to their lighter coloured neighbours who look down upon them” and also possessed advanced material skills compared to easterners (McFarlane 1888:103-104). Gill also noted “an inventiveness about the Papuans which I have never seen in Polynesia; and yet in the matter of clothing the latter far surpass the former” (Chalmers and Gill 1885:321). He concluded that “the darker race has learnt much from the light-coloured” (Gill 1892:7). Lawes, on the other hand, was already arguing in 1878 that the racial and tribal diversity along the coast prevented easy generalization (1879, 1880).

Assessing the Good and Bad in Native Life

An implicit comparison with European life lay not far beneath the surface of these early accounts. This is very clear in Lawes’s ethnography survey of people around Port Moresby (1880), which is written as a compendium of facts concerning native houses, canoes, occupations, customs, government, and moral conditions. Lawes assumed a neutral stance toward those subjects that could be seen as primitive forms antedating modem European practices. Native dwellings reminded him of prehistoric pile houses while canoes “furnish an interesting illustration of the earliest stages of naval architecture” (ibid.:609). The natives deserved praise for well-made fishing nets, skillful bartering, and “healthy exercise” gained from the daily round (ibid.:612). Many of their customs were indeed “peculiar,” such as greeting a person by running one’s hands from the forehead to the nose to the stomach; yet, although ignorant of civilized shaking hands or kissing, such practices demonstrated that “these rude and barbarous tribes have recognised rules of politeness and etiquette which are rarely violated” (ibid.). In these and other ways, New Guinean practices could be compared favorably with those of Europeans--indeed, they obviously reminded Lawes, as they had Murray, of

"customs which seem to link [New Guineans] to countries far remote, and ages long gone by" (Murray 1876:451) and thus to European civilization itself.

There was also much to object to. "The Moral Condition of the people is deplorably low," sighed Lawes (1880:613). The natives had no "religion," only unchecked superstition.

> All calamities are attributed to the power and malice of. . . evil spirits. Draught and famine, storm and flood, disease and death are all supposed to be brought by "Vata" [i.e., sorcery] and his hosts, so that the people are an easy prey to any designing individuals who claim power over these. Some disease harmers and rain-makers sometimes levied heavy toll on the weak-minded and superstitious people. They seem however, to have no idea of sacrifice, worship, or prayer, by which to avert their wraith or secure their favour. (Ibid.:615)

Similarly, the New Guineans lacked a sense of private property, and thus had "no sense of shame . . . in stealing"; they had little government, and thus continually engaged in warfare; no sense of the sanctity of human life, and thus proudly murdered one another (ibid.:613). In these and other ways, the natives suffered for a lack of "civilization."

The early LMS ethnographic writers in this fashion focused on surfaces. The appearance of New Guineans and their routine practices were considered in themselves sufficient to say much about the inner character of the people. As the quotations I have reviewed indicate, different writers read the signs of race and behavior rather differently. All could agree, however, that the New Guineans fit into the racial picture of Oceanic peoples and consequently shared certain features with other Pacific Islanders. Further, all did agree that although the New Guineans shared elements of civilization with the white "race," they were deficient in several regards, especially in their morality and spiritual beliefs.

Early Writings as Mission Propaganda

The propaganda function of missionary writing is rarely far from the surface in this early literature, even in articles like Lawes's ethnographic survey that was directed at an anthropological audience. The "fact" that New Guinea was inhabited by two races already familiar to the missionaries not only had scientific merit but seemed to answer the pressing question of how minister to so pagan a population given the tiny resources available. This constraint should not be a problem, McFarlane announced:

> It so happens that the London Missionary Society has just the kind of agency needed for the evangelisation of these two races. In the Loyalty Islands it has what is needed for the dark race; and for those of Malay origin it has the Tahitian Mission, the Hervey Island Mission, and Niue Mission, and the Samoan Mission; each of which is in a position to furnish its quota of labourers for the great undertaking, who are just the kind of labourers needed. (1873:385-386).

"These native teachers," McFarlane explained elsewhere, "are better acquainted with the habits and manners and customs of the heathen than missionaries are, and so are well adapted to fill the gap between the debased savage and the European missionary" (1888:137-138). Indeed, the Islander teachers were themselves perhaps the best advertisement of the civilizing power of the gospel. Many of them, according to Lawes, had come from savage backgrounds "in many respects worse then those in New Guinea" (1879:376).

The missionaries did not consider the racial characteristics of the New Guineans to be fixed and immutable. The Islander teachers would reach the people by stressing common traits, but the point was to use the commonalities to bring change. The positive accomplishments of New Guineans and other Pacific Islanders served to assure European readers that, however revolting certain beliefs and practices, the natives shared enough common humanity to be redeemable (cf. Thomas 1992). The artisan practices, which the missionary writers openly praised, formed the foundations for a new society. Christianity itself would fill in the moral vacuum that blighted the New Guineans' otherwise promising way of life. The missionary authors imagined the changes they wished to stimulate and guide in New Guinea as a kind of completion.

Ignoring such ambiguities, missionary authors went to some length to suggest that New Guineans themselves anticipated and desired the great gifts brought by the mission. "Savage life," wrote Chalmers, "is not the joyous hilarity that many writers would lead us to understand" (1887:334). Far from it. The pagan native was trapped in a world of constant fear of attack from warriors, ghosts, and sorcerers and longed for freedom. This assumption sets the stage for a recurrent scene in LMS accounts in which a previously uncontacted village welcomes the unarmed missionary into their midst and listens raptly to his words about the Savior (Chalmers and Gill 1885:108, 165). A Motumotan "chief" thus thanks Chalmers for bringing peace, concluding: "Soon our fathers' ancient customs will be given up, and you will see us, old and young, coming to be taught the word of the great and good Spirit" (ibid.:311). And while visiting the village of Par-i, Gill hears a "cry of

surprise and pleasure" when a "temple" for unspecified reasons bums down (ibid.).

And what of the future Christian society? McFarlane, the most enthusiastic of missionary propagandists, imagined a total overthrow of the native past:

> Instead of the war song, the cannibal feast, and the night dance, churches and schools and family worship are established. Instead of the wild-looking appearance of the people, dressed in feathers and shells and paint, they are now respectably clothed, and ashamed of their former appearance and habits. Instead of dirty huts, lazy and cruel husbands, and neglected children, there are now well-built houses, industrious and kind husbands, and bright and intelligent children. Instead of every man doing as he liked, which led to village quarrels, plunder, and war, there are now laws established, magistrates and policemen appointed, and law and order prevail.[17] (1888: 188-189).

This radical vision was unusual. More often the missionaries pictured the change to come as a liberation and fulfillment of the good already present among natives. Christianity would replace the evil of native superstition and violence, but the New Guineans were not to be "Anglicized" (Chalmers 1887: 125). Instead, a new Christian order would rise on the already present agrarian foundations of native society. To this end, the New Guineans had to be protected from the corrupting temptations and the outright threats of European society. Bad as New Guineans undoubtedly were, "white savages" were far worse and much more difficult for the gospel to reach (McFarlane 1888: 129-136; Chalmers and Gill 1885:11; Lawes 1880:614; McFarlane 1893). With the disastrous example of the Australian Aborigines fully in mind, the LMS missionaries imagined a society in the future in which independent native farmers would remain firmly in control of their lands and their destinies.[18] Only the mission, they insisted, could bring peace to warring tribes, touch the hearts of the people with the gospel, spread the benefits of civilization, and protect the people from unscrupulous whites to allow this vision to come to reality.

This was an audacious vision, although common in the grand missionary rhetoric of the time (cf. Comaroff and Comaroff 1991:55-70). The next generation of LMS missionaries proclaimed more modest goals in the face of slow progress, the reality of the colonial presence after 1886, and their growing appreciation of the complexities of local societies. In this last respect,

some were greatly assisted by anthropologists and developing anthropological models of tribal societies.

Custom and Social Transformation, 1890-1932

The Tyranny of Custom: Three LMS Ethnographies

Starting in the 1890s, missionaries began writing ethnographic accounts specifically for anthropological audiences. In this section, I examine the work of the three most prominent LMS ethnographers: J. H. Holmes, E. Baxter Riley, and William Saville. These men wrote monographs concerning "tribes" that were far more detailed and comprehensive than earlier reports. The thirty-two chapters of Saville's book on the Mailu, for instance, dealt with kinship, social institutions, village, food, canoe building, warfare, burial, and religion, among many other topics. Whereas earlier missionary ethnographers dismissed the presence of a religious sense among New Guineans, these three writers detailed ritual, magic, and religious ideas. Saville stated that religious beliefs and concerns "permeated" Mailu society, forming a key basis for the social order (1926:296). Riley wrote careful descriptions of Kiwai initiations and other rituals (1925). And, in a manner reminiscent of the old Malay/Papuan contrast, Holmes distinguished between the Ipi[19] and Namau peoples of the Gulf of Papua along religious lines. Ipi society, he argued, was rooted in beliefs in gods and totems. Every Ipi, like Eni in Holmes's novel *Way Back in Papua* (1926), found comfort in the knowledge that the gods created and controlled all things. The embracing totemic religion of the Ipi set a moral standard between the sexes that was even higher than modem civilization (Holmes 1924:52). The Namau tribes inhabiting the swamps of the Purari delta represented a lower stage in the evolution of religious sensibility. They were pure animists, living in awe and fear of objects containing the mysterious essence, *imunu*. Holmes argued that the Namau's cannibalism and polygamy "were excrescences of their animism," sharply distinguishing the tribe from the morally upright Ipi (ibid.:156).

It is "by no means easy," Holmes stated, "to interpret [native] theology in modem ideas and language" (ibid.:177). This is a striking assertion in light of earlier attitudes. Holmes accepted, as did his colleagues, that New Guinean societies had to be approached as complete entities. Their distinctiveness could not be adequately understood in terms of the absence or presence of familiar Western institutions and sensibilities.[20] Nor could such societies be understood in terms of the character of the natives, for this character was itself formed within a specific social and religious milieu. Age-old customs de-

fined New Guinea societies, customs that had taken shape many years before and then atrophied in the jungles, beaches and swamps. Thus Holmes wrote,

> The Papuan of the Ipi and Namau groups of thirty years ago was what his forbears were three hundred years ago. He was aggressive but not progressive. His laws, customs and mental outlook were fundamentally the same as governed his ancestors from time immemorial; to depart from them would be to incur the displeasure of the spirits of the ancestors. His past circumscribed his present and future; it was the vicious circle that encompassed his being. (1924:37)

"Custom is a tyrant that must be obeyed irrespective of consequences," wrote Saville. "I have not been able yet to discover the much-talked-of liberty of the savage; to me, he has always seemed the most fettered and hidebound individual I have ever met" (1926:33). Where once the "savage" had appeared to lack law and personal control, he was now revealed as the slave to or victim of a comprehensive system of customary rules and demands.

While agreeing that custom formed the essence of primitive New Guinea society, the three missionary ethnographers worked from different premises concerning its origins and purpose. Riley thought Kiwai custom reflected senior males' interests. Initiated men staged dramatic initiations, backed by elaborate supernatural threats, to terrify and keep women and children in line. "To see the women in tears is a source of much enjoyment to the male members of the community" (Riley 1925:65).[21] The initiations also served to teach young boys the power of custom, in effect bringing them into collusion with the senior men they would eventually replace. Male domination and fear thus locked the Kiwai into their ancient customs.[22]

Writing of culturally related peoples to the east of the Kiwai, Holmes also saw custom as an oppressive presence in native society. But Ipi and Namau custom oppressed all equally, for it embraced every aspect of life. In a harsh world where enemies might attack at any moment, children learned that whatever "serves the highest interests of the tribe is justifiable" even to the extent of determining what is "right and wrong" (Holmes 1902a:422). Custom made each person part of a superorganic body.

> His tribe was a complete entity: the whole man. Its conscience was its supreme chief; its headmen, or sub-chiefs, were the respective faculties which thought for the tribe, but the right or wrong of their combined deliberations was ultimately determined by the super-

> chief. His concern was the maintenance of the best interest of his tribe; his tribe was the alpha and omega of his life, the sole reason for his being, for his social position in it. His people were the respective members of that corporate body, his tribe. To do his bidding without question was natural to his people, not merely because he was their chief by inheritance, but for their good. (Holmes 1924:37)

Every person was born into social divisions with set responsibilities. The distant past had bequeathed the gulf people a comprehensive but rigid social system: generations had selected the best man for each position, and each man did his specific duty with clockwork precision.[23]

Saville, however, observed that custom caused as much dissension as unity among the Mailu. Mailu society was organized at the levels of kin, community, and clan. Units at the three levels operated in a state of tension in a sort of segmentary opposition. Unity at any level could be eroded by "continual jealousy between clan and clan, man and man--and still more between woman and woman--which every provocation will cause to burst out into flame" (Saville 1926:75). Villagers constantly watched each other, looking for the slightest breach in public etiquette, in custom. A Mailu did not always find customs easy or possible to follow, but breaking them inevitably led to quarreling and fighting. But the Mailu also had positive reasons to follow custom. Ritual practices and religious beliefs, for instance, provided the means to address the supernatural powers "for the perpetuation of the good. . . that shall work for the maintenance of life and the well-being of the community" (ibid.:294).[24] In Durkheimian fashion, though, the Mailu forged a tribal unity at times of major feasts and rituals. "In spite of all his tabus, in spite of the lurking evil of the sorcerer, in fact in spite of everything, it would be unfair to the native to think of him as going about his daily tasks with some ghost always dogging his steps. He moves as a free man because he is the willing slave of the community's conscience, and as long as he does not offend that he is care-free" (ibid.:294). Rather like the Eloi in H.G. Wells's *The Time Machine,* the Mailu willingly gave up personal freedom and responsibility for the security of customary ways.

The three ethnographic monographs differ from earlier missionary writings on the New Guineans in one final respect. They describe societies existing in the past. This is signaled in several ways. The subtitles of the books, for instance, suggest that these are memoirs of extended missionary careers.[25] The authors relate anecdotes from the early years of their mission, they refer to old men who remember the "former ways" as key sources of information, and they remind us from time to time that the society has changed. Holmes

drove home the point by writing In *Primitive New Guinea* entirely in the past tense. Although the missionaries say little about the changes that have taken place, their writing conveys a much stronger impression of change than the earlier missionary literature that focused on immediate encounters.

From Custom to Christianity in Mission Propaganda

Custom provided a general framework for detailed and often sophisticated ethnographic descriptions. LMS authors developed the custom theme much further in writings directed to mission supporters and detractors. The notion of custom suggested that the New Guinea native was willing to relinquish responsibility and even consciousness to the ways of the ancestors. Harmless in some contexts--making a garden, sharing food with one's family, and so on --custom drove New Guineans to unspeakable acts of barbarism in others. The severest critic of native custom, Charles Abel, observed that the war practices of the New Guineans placed them in "a very low scale of savage peoples." Whereas for more civilized people, like the Maori of New Zealand for whom warfare reflected underlying and redeeming patriotic and nationalist sentiments, war for New Guineans was "very often nothing better than murder" (Abel 1902:134). While leading them to violence against each other, custom also left New Guineans victims to their own superstitious fears. In any heathen community, one would find a sorcerer extorting wealth and spreading terror, as in the Kerepunu district: "The old man's face was one whose evil expression of cunning could not easily be forgotten, and his house was surrounded by a most unattractive and unsavoury collection of the skulls of sharks and dogs and pigs, stuck upon poles or ornamenting the walls" (Thompson 1900:50).

Children, who appeared to the readers as "naturally innocent," were especially vulnerable to the dictates of custom (Thomas 1992:376). Almost all missionary writers described Papuan childhood as a time of great freedom and license. Parents provided little moral guidance or protection for their children. Writing of Suau parents, Abel declared,

> They have no love. It seems a terrible thing to say of any human beings, but it is true of these people. . . . They have no word corresponding to our great word "Love". . . . I know of no other animal, except perhaps the duck, which is more careless in attending to its young, than the average Papuan mother. . . . I do not mean you to understand that there is no kindness shown by mothers to their children. I mean that their interest never rises to what we know as love. It is a mere animal propensity, compared with the love which reigns in a Christian mothers heart. (1902:42)

Edith Turner described finding a very young boy and girl playing together in an indecent manner while the girl's mother ("an absolutely ignorant heathen woman") watched them "smiling and unconcerned." "Such instances point," she wrote, "not to the depravity of individual children, but rather to the degradation of society, where the purity of even little children is not protected" (Turner 1920:21).[26] Henry Dauncey agreed. In Delena on the central coast, the "father does not interfere with the child's actions, or thwart its wishes, and so arises one of the greatest defects in the Papuan character, and most serious obstacles in the way of progress. Of obedience the Papuan knows nothing, unless there is a big stick, or a heavy hand, or the fear of the sorcerer, at the back of the command" (Dauncey 1913:5). Parents did work hard to teach the "conceited youth" one lesson, however: "for every wrong he must exact payment" (ibid.:20).

Such passages seem to suggest that while the child might be saved from the tyrannical hold of custom, the parents are beyond redemption. But this was countered in other passages where New Guineans as a whole were represented as infantile (Langmore 1989:126-130)--at once the witless victims of past traditions and innocents open to the saving words of the Gospels. G. Currie Martin stated: "The people are as a whole very responsive to kind and frank treatment. They have shown themselves faithful and loyal friends when their trust has been won. They are reliable if suspicions of the foreigner as such have been allayed. Naturally the savage looks upon a man of different colour and habit at first as an enemy, for all strange things are inimical to childlike people" (1908:17). The key weakness of the Papuan --his simplistic trust in what the elders told him to be true--actually made him a good prospect for missionary efforts, for conversion then became a matter of substituting one authority for another. Martin praised the sincerity of new converts: "they quickly grasp the deepest essentials of the Christian faith" (ibid.91). Indeed, in this context, Martin found even the treatment of children to be praiseworthy: "One excellent feature of the native life is the love of children and the great freedom that is permitted them" (ibid.:17).

If the New Guineans were like children, the missionaries thought they must be their parents and educators. Much of the mission propaganda from this period was taken up with descriptions of the "home" and "school" in which the social transformation was supposed to take place: the residence of the missionary himself. Although most routine missionary work took place within scattered villages, missionary writers tended to focus on the operations of central head stations. These stations, which varied greatly in size, offered several specialized services, all under the direct supervision of white missionaries: training schools for pastors and their wives,' advanced general schools, technical shops, stores, and so forth. The accomplishments of converts on the stations testified to their natural abilities, abilities stymied within the vil-

lages. Furthermore, missionaries could point to the successes of their station converts as evidence that they, alone among Europeans resident in New Guinea, were best prepared to bring civilization to the natives. Wrote Abel,

> In theory, our country protects the aboriginal natives of her Colonies; in practice she destroys them. . . . The quickest way to this end is to refuse to educate them in industrial and civilized pursuits. . . . The fact that the Papuan is capable of being taught to use his brains, as well as his hands, and to rise in the scale of humanity from the low position in which we find him, is surely an argument that it is our duty to give him the opportunity he is able, and even anxious, to turn to good account. (1902:207)

Missionary books included testimonials from administrators praising mission education on the stations (e.g., Thompson 1900:38).

It was a short step to accord the mission station with an even greater symbolic importance: as the model of a new Christian order. An LMS director visiting the districts in 1917 was moved to write, "Amid the wilderness each station is an oasis. Travel anywhere along the coast, and, after passing raw native villages . . . suddenly you will come to the Mission Station. There . . . you will feel that you have reached a haven for spirit as for body. . . the place is physically and morally clean" (Lenwood 1917:202-203). Abel, the most radical of the second generation of LMS missionaries, regarded his station on Kwato Island as the abode of a utopian social order, in which Christian morality and practical knowledge could flower and replace the degraded existences New Guineans lived under the rule of custom. He advocated taking young children from their parents so they could be brought up within the "clean" environment of the station. Once on the station, the children had to be kept separate from their kin, forcibly if necessary. 'We put ourselves in the place of Christian parents to them," Abel explained to British Sunday school classes, "and just as your father and mother would prevent you from going where you would get harm, and would shield you from mixing with evil companions before you were old enough to judge rightly for yourselves, so we acted towards our large Papuan family" (1902:193).

Reared on a strict regimen of hard work, discipline, and cricket, the graduates of Abel's school on Kwato Island could either settle into their new life or return to their natal villages and begin to replace them with Christian communities (Wetherell 1973). The other LMS, missionaries writing at this time took the familial metaphor less literally. Dauncey, for example, suggested that the people of Delena were very gradually becoming Christian,

assisted by the teaching and examples of the church (1913). His book shows the evils of custom--fears of the sorcerer, warfare, and the lack of care of children--to be gradually fading while good aspects of customary life merge into the developing Christian society.[27]

The LMS ethnographic writings of this later period showed the New Guinean living within social orders, albeit societies stymied by tyrannical custom. There was a corresponding shift in the way mission propagandists portrayed change. They now suggested a lengthy process, a process that entailed resistance from elders still mired in custom and backsliding on the part of converts. It was a process that involved not only the invisible guidance of God, but the very visible mechanics of mission schools, regulations, and bureaucracy.

Conclusions and Further Reflections

Colonial discourse includes several distinct genres, which together share a broad "common ground" (Thomas 1992, 1994). Missionary propaganda, travel accounts, and ethnographic studies differ not so much in the range of topics and tropes they employ as in the ways these are put together to form coherent narratives. Sharing overlapping elements and concerns, as well as participants and audiences, colonial genres appear ever entangled with one another (cf. Scott 1992:331). Particular genres find their distinct voices in creative tension with others. Thus the books and articles the LMS missionaries wrote concerning their work in New Guinea reflect, in style and content, several concurrent genres: mission biographies and popular histories, popular secular works on exotic peoples, memoirs by colonial officers, travel literature, and scientific works. Most of the LMS writings conformed. to themes and styles found in contemporary evangelical missionary propaganda from around the world.[28] But several authors wrote for wider publics, borrowing themes and narrative styles from other colonial genres. Hence Chalmers, like his hero David Livingstone, published popular exploratory accounts; and he also wrote ethnographic articles for scientific audiences.

Chalmers and other LMS writers were clearly aware of the differences between "scientific" description and mission apologetics. Yet there is nothing to suggest that they regarded their ethnographic excursions as inherently contrary to evangelical principles and practices. The LMS writings reviewed here form a kind of intermediate discourse, reflecting a sensitivity towards both anthropological audiences and mission supporters. The anthropological signature is clear enough. During the 1870s, missionary writers borrowed from an already declining racialist anthropology. Their accounts, however, took the form of narratives of encounter, narratives in which the missionary

purpose and practice remained visible. Professional anthropologists gradually influenced missionary ethnographic writings more directly, first by providing questionnaires and later through direct training and sponsorship. Missionaries abandoned their initial focus on the character of the New Guinean for evolutionary and functionalist understandings of society, thus reflecting the theoretical orientations of their mentors.[29] Although overt missionary concerns never entirely disappeared from ethnological writings, they became far less conspicuous (cf. Thomas 1992:379).

Nonetheless, the missionaries had a purpose that differentiated them from secular anthropologists and that inevitably found voice in even their most "scientific" publications. Indeed, ethnological writings provided missionaries with an apparent ethnographic basis on which to proceed and to defend their efforts. Holmes was most explicit about this intention in the introduction to his monograph: "The savage is soul-sick and we cannot help him satisfactorily until we can diagnose his disease of heathenism" (1924:2). Taken in the context of other missionary writings, public and private, even the most relativistic of the ethnological reports cannot be seen as arguments in support of maintaining traditional societies, even if the missionaries sometime suspected anthropologists of making precisely this kind of association.

This article has shown that there was also a strong, if often implicit, resonance between ethnological writings and shifting concepts of the mission's role in social change. The pioneer missionaries, encountering the New Guineans for the first time, associated outward appearances with inward character. There was a certainty to the early ethnographic assessments, which measured the New Guineans against other Pacific races and, more importantly, European civilization. The missionaries' aims seem astoundingly confident, given their small resources and ignorance of New Guinea. Imagining the New Guineans to be incomplete, having the basics of life but lacking in civilization, the missionaries pictured them anticipating a conversion that would complete them. Later generations of missionaries, however, observed the New Guineans from the vantage point of large mission stations that formed the hubs of routinized evangelistic efforts. They came to know those people settled around their stations as communities in their own right. This changing perspective supported the shift in ethnographic descriptions from race and character to community and custom. The missionaries now pictured themselves as patient teachers, coaching converts as together they rebuilt New Guinea societies previously locked into and victimized by age-old custom. The missionaries likened such change to the raising of children within a family. Eventually--not yet--the Papuan Christians would be ready to assume their responsible role as "parents" of their own communities.

In creating these shifting images of New Guinea, the LMS missionaries

necessarily glided over some complicating realities, downplaying their own doubts and uncertainties. The missionaries maintained, first of all, a virtual silence about the presence of other Europeans in New Guinea. Only exploitative whites, the blackbirders and harsh planters, appear in mission propaganda and then only for brief moments. In effect New Guinea appears, quite misleadingly after the early 1870s, as inhabited almost wholly by the natives and their missionaries. Everyone else is an intruder. As with the Methodists in the Solomon Islands, the "mission . . . is identified as the sole author of positive social change" (Thomas 1992:381). The one exception to this are some positive remarks on the administration. But this exception proves the rule, for the missionaries were themselves instrumental in the establishment of the British colonial administration (see Prendergast 1968).

The missionaries were only slightly more forthcoming about the New Guineans' own initiatives and creative responses to themselves and other Europeans. Ethnographies portray passive natives: living out racially received characteristics or cowed by unchanging custom. The silence concerning indigenous actions and desires accords the mission a godlike power to mold New Guinea character and society. The missionaries naturally considered their influence to be benevolent. The duty of the missionary, wrote Martin, "is not primarily one of destruction, but of rearing a new fabric whose beauty and strength will soon replace the poor and unsatisfactory shelter of the old one" (1908:91). The missionaries imagined themselves as the artisans of this transformation. They portrayed New Guineans as followers reacting to but not instigating change. The reality was far more complex. On the southeast coast of New Guinea, as elsewhere in Oceania, converts shaped Christianity along the lines of received cultural assumptions and structures, their developing understanding of the colonialists, and their own shifting aspirations (Barker 1990, 1992).

On rare occasions one hears a note of doubt. In his novel, *Way Back in Papua* (1926), Holmes portrays a gulf village a generation after the arrival of the mission. The village is ordered and clean, with cannibalism and polygyny things of the past. But the people, for all these changes, have not become joyful, confident Christians. Old Eni, the chief character, lives with his memories of a nobler past, and the younger villagers respect and consult him. Much of the novel is taken up with Eni's stories about tribal days and the portentous coming of the "Peace-maker" (presumably Chalmers, or possibly Holmes himself). Although now Christians, the people are still New Guineans, bewildered by the complexities of the new world while hanging on to the thread connecting them to a rapidly receding past. It is not hard to detect in Eni's confused reminiscences Holmes's own doubts and regrets concerning the mission's impact "way back" upon the indigenous peoples of Papua.

APPENDIX
Publications by Members of the London Missionary Society about New Guinea
(By Date of Publication)

This listing includes publications by resident missionaries, visitors, and mission historians. Although I have tried to be inclusive, I expect that I have overlooked several pamphlets and perhaps some of the more obscure journal articles. I have made no attempt to track down missionary correspondence in newspapers.

McFarlane, S.
1873 *The Story of the Lifu Mission.* London: James Nisbet.

Gill, W. W.
1874 Three Visits to New Guinea. *Proceedings of the Royal Geographical Society* 18:31-50.

Murray, A. W.
1874 *Wanders in the Western Isles.* London: Yates and Alexander.
1875 *Forty Years Mission Work in Polynesia and New Guinea, from 1835 to 1875.* London: J. Nisbet

McFarlane, S.
1875- Ascent of the Fly River. *Proceedings of the Royal Geographical Society*
1876 20:253-260.

Gill, W. W.
1876 *Life in the Southern Isles; or, Scenes and incidents in the South Pacific and New Guinea.* London: Religious Tract Society.

McFarlane, S.
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Lawes, W. G.
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McFarlane, S., and James Chalmers
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1880 Notes on New Guinea and Its Inhabitants. *Proceedings of the Royal Geographical Society* 2:602-616.

Chalmers, James
1880 New Guinea. *Proceedings of the Royal Geographical Society* 2:315-316.
1881 Further Explorations in New Guinea. *Proceedings of the Royal Geographical Society* 3:226-227.

Lawes, W. G.
1883 An Excursion in the Interior of New Guinea. *Popular Science Monthly*.
1884 Recent Explorations in South-eastern New Guinea. *Proceedings of the Royal Geographical Society* 6:216-218.
Gill, W. W.
1885 *Jottings from the Pacific*. London: Religions Tract Society.
Chalmers, James, and W. W. Gill
1885 *Work and Adventure in New Guinea, 1877-85*. London: Religious Tract Society.
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1886 *Adventures in New Guinea*. London: Religious Tract Society.
1887 Explorations in South-eastern New Guinea. *Proceedings of the Royal Geographical Society* 9:71-86.
1887 New Guinea: Past, Present, and Future. *Proceedings of the Royal Colonial Institute* 18.
1887 On the Manners and Customs of Some of the Tribes of New Guinea. *Royal Philosophical Society of Glasgow, Proceedings* 18:57-69.
1887 *Pioneering in New Guinea*. London: Religious Tract Society.
1887 Two New Guinea Stories; History and Description of the Pottery Trade. In *Picturesque New Guinea*, by J. W. Lindt, 106-117, 118-125. London: Longman's, Green.
Lawes, W. G.
1887 The Condition of New Guinea. *British New Guinea, Annual Report*, 1886-1887:24-26.
1887 The Effect of the Climate of New Guinea upon Exotic Races. *Australasian Medical Gazette* 6:185-186.
Murray, A. W.
1888 *The Bible in the Pacific*. London: Nisbet.
McFarlane, S.
1888 *Among the Cannibals of New Guinea*. London: London Missionary Society.
Lawes, W. G.
1890 Comparative Views of New Guinea Dialects. *British New Guinea, Annual Report*, 1889-1890: 158-167.
Anonymous
1890 The Bibliography of the Australasian, Papuan, and Polynesian Races. *Australasian Association for the Advancement of Science, Report* 2.
Chalmers, James
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Chalmers, James
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Lawes, W. G.
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Timoteo
1897 Notes on the Kabadi Dialect of New Guinea, translated and edited by J. E. Newell. *Journal of the Polynesian Society* 6:201-208.

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Holmes, J. H.
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Martin, G. C.
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1920 *Greatheart of Papua.* London.

Turner, Edith
1920 *Among Papuan Women.* London: London Missionary Society.
Abel, R. W.
1922 *Tales Told in Papua.* London: Livingstone Press.
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1923 *Chalmers of New Guinea.* London: Hodder & Stoughton.
Riley, E. B.
1923 Dorro Head-hunters. *Man* 23:33-35.
1923 Sago-making on the Fly River. *Man* 23: 145-146.
Holmes, J. H.
1923 *By Canoe to Cannibal Island.* London.
1924 *In Primitive New Guinea.* London: Seeley, Service.
Riley, E. B., and S. H. Ray
1924 Kiwai Seasons. *Man* 24:56.
Turner, Robert L.
1924 Malignant Witchcraft in Papua and the Use of Poisons Therein. *Man* 24: 117-119.
Riley, E. B.
1925 *Among Papuan Headhunters.* London: Seeley, Service.
Parry, C. G.
1925 *Everyday Life in Papua: An Illustrated Handbook.* London: Livingstone Press.
Saville, W. J. V.
1926 *In Unknown New Guinea.* London: Seeley, Service.
Holmes, J. H.
1926 *Way Back in Papua.* London: George, Allen and Unwin.
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1935 *Guinea Gold: The London Missionary Society at Work in the Territory of Papua.* London.
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1938 *Papuan* Journey. Sydney: Angus & Robertson.
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1960 *A Man Life Bati, the Rev. Reginald Bartlett.* London: Independent Press.
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n.d. *Many Faiths: One Essential.* Sydney: Sven Productions.
Goodall, Norman
n.d. *Pacific Pilgrimage.* London: Livingstone Press.
Turner, Robert L.
n.d. *Our Papuan Mission.* London: London Missionary Society.
Seton, W.
n d. *Chalmers of New Guinea.* London: Sunday School Union.

NOTES

This article began its long journey as an appendix to my master's thesis (Barker 1979). I want to take this opportunity to give my long overdue public thanks to Ann Chowning for her marvelous support, advice, and encouragement as my supervisor. Harold Turner thought the appendix worth publishing but I was not able to return to it until a few years ago when Mac Marshall organized a session on representations of Oceania at the Association for Social Anthropology in Oceania meetings. I owe a special debt to Nick Thomas for his careful reading and critique of an early draft. My thanks as well for the criticisms and suggestions of the anonymous readers for *Pacific Studies,* which together have made this a much better paper, I believe. I first encountered and read the publications reviewed here in the lovely (if cramped) former Victorian home of the Alexander Turnbull Library in Wellington, New Zealand. My belated thanks to the research staff for their assistance and interest.

1. A full listing of LMS publications related to New Guinea is provided in the appendix.

2. Over the six decades reviewed in this article, LMS writers referred to the southeastem coast of New Guinea as either "New Guinea" or "Papua." I use the terms interchangeably here.

3. In the case of Papua New Guinea, the best known are the Point and Catalyst series published by the Melanesian Institute, a pastoral and socioeconomic research organization based in Goroka. The Summer Institute of Linguistics has published an extensive collection of indigenous narratives, often in the vernacular, as well as studies of languages. Students at the theological colleges often tackle aspects of indigenous cultures for thesis topics. Finally, all of the major churches have published ethnographic materials in their own magazines, as privately commissioned reports, and in collections. Most of this scattered material is difficult to access outside of the region. To date, Gary Trompf is the only international scholar who has made much use of church-sponsored local studies (Trompf 1991, 1994).

4. A small selection of outstanding studies would include Codrington 1891; Ivens 1930; Saville 1926; Vicedom and Tischner 1962.

5. Nelson (1969) and Langmore (1989) have published comparative reviews of the ethnographic thought of the major missions in early colonial Papua. See also Wetherell 1977 and Hilliard 1978 on Anglican writings in Papua and the Solomon Islands. Huber (1988) and Nancy Lutkehaus (in Böhm 1983) provide useful overviews of the important ethnographic work undertaken by members of the Society of the Divine Word on the northwestern coast of New Guinea. The most intimate review available on the ethnographic thought of a missionary, Maurice Leenhardt in New Caledonia, is provided by Clifford's biography (1982). Gunson has recently published a comprehensive review of the scientific writings of British missionaries across the Pacific Islands (1994).

6. The most detailed academic histories of the LMS are Prendergast's (1968) unpublished doctoral dissertation and Langmore's biography of James Chalmers and comparative history of the four main Papuan missions (1974, 1989). Wetherell provides some information on the LMS in his history of the Anglican Mission and has also published a series of papers on Kwato and Charles Abel as well as LMS Polynesian teachers (see Wetherell 1973, 1977, 1982, 1986).

7. Langmore (1974) makes limited use of oral histories in her biography of James Chalmers. This is clearly a critical area crying for immediate attention from researchers.

8. Given the negative connotations that many associate with "propaganda," I am reluctant to use the term but can find no synonym that conveys the idea of writings meant to solicit support. Suffice it to say that I do not consider missionary writings to be any more (or less) misleading, let alone dishonest, than other works written to serve colonial and anticolonial projects.

9. Mulvaney notes that missionaries accounted for fifty-one of the ninety-four ethnographic papers read before eight sessions of the Australasian Association for the Advancement of Science between 1888 and 1900 (1989:200).

10. Lawes and Chalmers took issue with McFarlane's claims that New Guinea held immense riches, and they worried that these claims would attract white prospectors and

settlers. While never matching McFarlane's flamboyance, Chalmers nevertheless presented equally exotic pictures of New Guinea and its inhabitants.

11. This turned out to be Mailu Island, which women had to themselves during those times their men left for the mainland to plant new gardens.

12. On nineteen-century debates between polygenists and monogenists, see Stocking 1968.

13. I would thus hesitate to attribute "an ethic of potential human equality" to the LMS missionaries (Thomas 1992:386; cf. Langmore 1989). Sanders (1992:223-225) and other historians give innumerable examples of Christian leaders drawing on biblical verses in support of racist regimes. This is a question that, at least in the case of the LMS missionaries, requires more careful study of both the published and archival sources.

14. Trautmann suggests that racialist ethnology rested on the assumption that humans had existed on earth for a very brief time, a period established by the Bible and other ancient texts on which scholars relied "as sources of our knowledge of the original human state" (1992:388). Following the 1859 publication of Darwin's *The Origin of the Species,* scholars began to perceive a vastly expanded human past. For those accepting this new conception of human history, racialist and Mosaic ethnology became equally impossible.

15. Lawes, Chalmers, and their successors did not share the older generation's obsession with European clothing (Langmore 1989). A later missionary working on Murray Island blamed dirty European clothing, which had been originally donned at the urging of Murray, Gill, and McFarlane, for the precipitous drop in the island's population (Hunt 1898).

16. Compare with the Presbyterian missionary John Inglis, writing about the New Hebrides: "wherever these Malays, these children of Shem, go in the South Seas, these Papuans are willing to be their servants . . . but nowhere do we see the Malays serving the Papuans, or Shem being the servant of Canaan" (1890:10).

17. McFarlane's vision provides a vivid example of the "before and after" motif common in much missionary writing and imagery (see Thomas 1992). In this instance, it also may have had some basis in truth. Beckett writes that a mission theocracy, with considerable local support, did develop in the Torres Strait islands during this period (1978).

18. As both Prendergast (1968) and Langmore (1974) clearly show, the LMS developed and proclaimed their ideas about the future of the New Guineans in the face of the pending annexation of the region by Australia. When Germany revealed its own interest in northwestern New Guinea, Chalmers and Lawes changed course, advocating that a protectorate be set up, but under British control with full protection of indigenous lands (see Barker 1979:27-32).

19. More generally known as the Elema.

20. I do not want to suggest by this that the missionaries made no comparisons, overt or covert, in understanding New Guinea society. My point is that they now *presented* the New Guineans as living within distinct social orders. This discursive construction required a more relativistic stance than the earlier racially grounded accounts.

21. There are parallels here with earlier literature on the "Papuan race," which was partly defined in terms of the men's mistreatment of women.

22. I should note that Riley's ethnography was the least cohesive of the three reviewed here. His observations on custom and male domination form a very small, if important, part of a compendium of ethnographic notes. Apparently the publisher deleted large sections of the monograph without first consulting the author (Ray 1926).

23. Whatever the value of Holmes's more detailed ethnographic observations, particularly concerning religious beliefs, his general claims appear exaggerated and implausible. F. E. Williams dismissed most of them in his monograph, *The Drama of Orokolo* (1940). Williams found polygamy common among the Elema ("Ipi"), no gods of any sort except for the introduced Christian one, and not even a vague memory of a "super-chief." "As for our respective results," Williams concluded, "any one might shrink from the task of trying to make them square" (1940:xii).

24. Saville's debt here to Malinowski requires little comment.

25. For example, Saville's *In Unknown New Guinea:* "A record of twenty-five years of personal observation & experience amongst the interesting people of an almost unknown part of this vast island & a description of their manners & customs, occupations in peace & methods of warfare, their secret rites & public ceremonies" (1926: title page).

26. As Margaret Jolly observes in her review of early Presbyterian writings from Vanuatu (1991:36), such attitudes also reflected Evangelical missionaries' notions of domesticity and their widespread concern that women were more interested in raising crops than moral children.

27. Thompson writes of the sorcerer, for instance: "His power is declining as knowledge spreads among the people. Meanwhile he regards the missionary with no great favour as a rival medicine man who has greater power than he has" (1900:50).

28. Missionary journals, rather like professional anthropological journals, included articles from around the world. Such publications seem to have conditioned the expectations new recruits had of the various mission fields (e.g., "cannibals" in New Guinea, "rice Christians" in China) while providing an evangelical ideology that could embrace very different situations and cultures.

29. See Trautmann, who has recently argued that anthropology went through two major transformations during this period (1992): from a contracted biblically based concept of time to an extended developmentalist one, and from studies of the broad sweep of human history to synchronic functionalist studies of single societies. Both transformations are clearly visible in LMS ethnographic writings.

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EDITOR'S FORUM

COLLABORATIVE REGIONAL ANTHROPOLOGY IN NEW GUINEA: FROM THE NEW GUINEA MICRO-EVOLUTION PROJECT TO THE A. B. LEWIS PROJECT AND BEYOND

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Several collaborative research projects in New Guinea aimed at understanding regional variation and diversity are examined, with particular attention to two projects: the University of Washington's New Guinea Micro-Evolution Project (1959-1983) and the Field Museum's A. B. Lewis Project (1987-1994). Regional research projects require different field strategies from community-based ethnographic projects; collaborative field projects offer one solution. After summarizing a number of collaborative projects, discussion turns to the specific hypotheses and research design used by James B. Watson and his collaborators in the Eastern Highlands of New Guinea. The Micro-Evolution Project expected to find correlations between environmental factors and local diversity, but instead found that social fields were the most important environmental variable. Working on the Sepik Coast of New Guinea, Robert L. Welsch and John Terrell have built upon the Micro-Evolution Projects conclusions and developed a project to explore the relationship between social fields and human diversity on this coast. Future studies of cultural, linguistic, and biological diversity in New Guinea will require more explicit models that simultaneously and explicitly model processes of both diversification and social interaction.

SEVERAL COLLABORATIVE RESEARCH PROJECTS in New Guinea have attempted to understand regional variation and diversity, in particular the New Guinea Micro-Evolution Project, sponsored by the University of Wash-

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ington (Seattle) in the late 1950s and 1960s, and the A. B. Lewis Project sponsored by the Field Museum (Chicago) since 1987. The first examined local (or micro) evolutionary processes in communities that were assumed to have diverged from a common ancestor in the Eastern Highlands; the second has been examining the role that interaction and social networks have played in shaping diversity on the Sepik Coast. Despite dramatic differences in environment, social patterns, economic organization, political processes, and levels of linguistic heterogeneity these two study areas have provided useful venues for examining one of the most perplexing regional problems that confronts anthropologists working in New Guinea: explaining cultural, linguistic, and biological variation and diversity. My goal here is to identify common objectives, issues, and findings of these two projects as well as their relationship to other regional and collaborative projects that have also examined Melanesia's remarkable diversity.

In recent years there has been a growing interest in regionally oriented anthropological research (see, for example, Gregory and Urry 1985; Lomnitz-Adler 1991; Terrell 1993). Regional analyses have emerged, in large part, because traditional ethnographic studies of particular villages often miss social and cultural relations beyond the village, relations that are increasingly recognized as being important. It is unlikely that regional studies will ever completely replace village-based ethnography as anthropology's standard method. But regional and historical approaches are increasingly being suggested as ways of expanding anthropology's temporal (e.g., Biersack 1991) and spatial (e.g., Gewertz 1983) horizons to avoid the limitations of the "ethnographic present."

Toward Collaboration and Regional Studies in Melanesia

One criticism often raised by ethnographers against regional approaches is that in order to survey several ethnolinguistic communities, the anthropologist must inevitably sacrifice the fine-grained and rich detail that has been the hallmark of good ethnography. On the one hand, to get breadth one must give up depth. On the other hand, to sustain this depth of local cultural understanding one must sacrifice any possibility of understanding how different communities are and have been linked with other communities whose languages or cultures are different.

Three solutions to this conundrum seem possible: (1) focused research problems, that is, research that seeks to understand only certain aspects of village life in some depth in several places, settling for a more modest and general understanding of other cultural aspects; (2) collaborative field projects that involve several researchers, each responsible for a different

aspect of the research question; or (3) repeated visits over many years to a study area that includes more than one ethnolinguistic community. Only the first two of these strategies have been attempted in any systematic way. It should be noted, however, that these two strategies are not mutually exclusive, for collaborative projects have usually assigned different aspects of a larger problem to individual members of a team. Margaret Mead's repeated visits to Manus might have been an example of the third strategy, except that her own work in Manus was not regional, focusing almost exclusively on Peri village.[1]

Mead was one of the earliest to attempt strategy number one, first when she compared childrearing practices in several communities along the Sepik and again when she tried to understand how the Mountain Arapesh interacted with their neighbors (1935, 1938). In some ways Whiting and Reed's work in the Kwoma area--although unsuccessful as a regional study--was an early attempt at collaborative ethnography.[2] These research projects are perhaps best understood as systematic field research projects aimed at answering (or at least addressing) a specific, narrow research question. In this respect, Mead (1935), Whiting (1941), and Reed (1943) are distinctively American in research style, particularly when compared with contemporaries who were trained in the British school of ethnography (e.g., Bateson 1936; Fortune 1932, 1935; Hogbin 1934/1935a; Powdermaker 1933; Williams 1930, 1936, 1940). It is unclear how American and British research might have developed had these early studies not been interrupted by the Second World War. What is certain is that after the war ethnography became the standard research protocol for American, British, and Australian trained anthropologists alike.

There are, however, a number of exceptions to this trend toward focused ethnographies. Two kinds of studies in the postwar period have had a regional orientation. The first group were by individuals whose work was motivated by regional questions but whose research was generally ethnographic. These studies would include among others the work of Burridge (1960), Lawrence (1964), Schwartz (1962, 1963), and Harding (1967). Of these, only Harding's was initially conceived of and designed as a regional project, which led him to study trade relations intensively in more than one ethnolinguistic group.[3] And all of these researchers except Schwartz--who collaborated to some extent with Mead as well as with two of his successive wives--were essentially individual projects.[4]

The second group of studies consists of collaborative projects organized and developed to address particular regional questions. Although I make reference below to a number of collaborative projects, the New Guinea Micro-Evolution Project directed by James B. Watson stands out as the most

carefully and systematically developed regional research program in New Guinea to date. For this reason, I will give it more attention than several of its contemporaries, but it is important to note that all of these projects were inspired by similar regional concerns. I do not mean to suggest that the Micro-Evolution Project was either unique or had emerged independently of the others, but it was a comprehensive effort to study a single set of regional questions.

After discussing these earlier collaborative projects I turn to the Field Museum's A. B. Lewis Project, directed jointly by Robert L. Welsch and John Terrell since 1987. Both the Micro-Evolution Project and the Lewis Project were designed to study cultural and linguistic variation in subregions of New Guinea. My goal here is to show that collaborative research is still a productive strategy for addressing questions about New Guinea's diversity and that younger researchers can (and should) build on the findings of, earlier projects. Collaborative regional projects may not be the only way to study variation in Melanesia, but they are likely to be the only way to gather data systematically from diverse communities.

Ours is not, of course, the first generation of anthropologists to focus attention on regional questions; Franz Boas and A. L. Kroeber, together with their students and colleagues, had long ago specifically asked regional questions about relationships between adjacent or nearby societies. These early regional research programs were largely abandoned in the 1930s, 1940s, and 1950s when community-centered ethnography became the standard research tool and the ethnographic monograph became the standard anthropological product.

Renewed interest in regional issues parallels a similarly growing interest in contextualizing anthropological data within a historical context (see, e.g., Biersack 1991). Both trends may be a reaction to the growing awareness that traditional ethnographies written in the "ethnographic present" are artificial constructs. Such monographs present tribal and village-based societies as if they were communities essentially isolated in space and time. Such study communities appear to have little or no history and to be insulated both from neighboring societies and from the effects of colonial, national, and global linkages. Indeed, only by essentially ignoring the presence of government, missions, and modem economic processes were the classic ethnographies possible (e.g., Malinowski 1922, 1926, 1927, 1929, 1935; Evans-Pritchard 1937, 1940; Fortes 1945; Firth 1936, 1939, 1940).

Regionally and historically contextualized research is a partial corrective to some of the criticisms that have been leveled against the ethnographic method for the past two decades. Though a new generation of researchers may embrace new regional and historical methods, it is important to recog-

nize that similar research programs were attempted in the not so distant past. It behooves us to take note of their accomplishments and inadequacies, lest we ignore their successes and replicate their failures.

This article considers the successes and failures of the New Guinea Micro-Evolution Project, which was begun in 1959 to understand the "evolution" of four study peoples in the Eastern Highlands of what is now Papua New Guinea. The Micro-Evolution Project was directed by James B. Watson at the University of Washington (Seattle), who had previously conducted field research in Brazil and in an adjacent part of the Eastern Highlands. Involving nine field anthropologists, archaeologists, and linguists, as well as several members of the Summer Institute of Linguistics based at Ukarumpa, and spanning a decade of field research, the Micro-Evolution Project remains the most ambitious regional anthropological research program ever mounted in Melanesia.

It is not my intent here to provide a detailed history of the Micro-Evolution Project or to offer a systematic commentary on the project's many participants and their specific research findings. Rather, I want to assess the project's general findings in light of its broader objectives. My discussion of the project's general successes and failures illuminates how some of the project's greatest accomplishments were unexpected but have proved useful by motivating subsequent regional research.

After examining some of the more important conclusions of the Micro-Evolution Project, discussion turns to the A. B. Lewis Project, an ongoing collaborative research program based at the Field Museum that is currently attempting to address related regional questions about culture change and stability. When John Terrell and I originally developed the Lewis Project, we explicitly attempted to build on the Micro-Evolution Project's findings. As such, from the outset the Lewis Project made use of the Micro-Evolution Project's results and these led to a quite different analytical model and selection of a very different kind of field setting. We see this ongoing research as growing directly out of the findings and conclusions of the Micro-Evolution Project.

Regional and Collaborative Research in Melanesia

The New Guinea Micro-Evolution Project was the first modem research program in what is now Papua New Guinea to have a genuinely regional theme and hypotheses that demanded a systematic, collaborative protocol. It was not, of course, the first collaborative anthropology project in Melanesia; the Cambridge Torres Straits Expedition of 1898-1899 was both collaborative and multidisciplinary (Haddon 1901-1935). The Torres Straits

Expedition also had a (vague) regional orientation, since most of the researchers visited several sites in the Torres Strait followed by visits to several parts of Papua before returning to England.

These regionally oriented researchers included expedition leader A. C. Haddon (1906, 1920, 1924, 1927, 1936), who would oversee anthropological research in the Papuan Gulf and Western Papua for the next forty years. They also included W. H. R. Rivers (see Pataki-Schweizer 1990), who later turned his attention to island Melanesia where he collaborated with A. M. Hocart and G. C. Wheeler on the collaborative Percy Sladen Trust Expedition in 1907-1908. Comparative data from this expedition became the basis of his magnum opus, *The History of Melanesian Society* (Rivers 1914). Perhaps most notable for his regional interests among this Cambridge team was C. G. Seligman (1904, 1905), who returned to Papua a few years later on the Cook-Daniels Expedition to conduct research for his comparative study, *Melanesians of British New Guinea* (1910).

Over the next fifteen years, most anthropological research in New Guinea was regional in scope--although generally not very collaborative in style.[5] During this period, which I call the "expedition period" in anthropology (Welsch 1998), virtually all anthropologists conducted research of a peripatetic nature. The expeditions mounted by this diverse set of anthropologists working in Melanesia were, for the most part, explicitly comparative in orientation; researchers collected various kinds of data in the form of word lists, museum collections, anthropological measurements, and observations of customs and social practices.[6] They often attempted to use such comparative data to shed light on the movements and migrations of peoples into and through Melanesia or in a few cases to suggest patterns of cultural evolution within the region.

This group of researchers also included Malinowski, who came to Papua on three expeditions (1914-1915, 1915-1916, and 1917-1918). On the first two expeditions he explicitly planned to conduct research in several field sites, intending to fill in some of Seligman's lacunae in southeastern Papua. But during his second expedition, Malinowski became interested in what was happening in the Trobriand Islands; he settled in on Kiriwina and later made his name as an ethnographer dealing with a single ethnic community rather than as an ethnologist considering regional questions. Readers should note that Malinowski's first monograph about Trobriand Islanders, *Argonauts of the Western Pacific* (1922), was a regional study that examined the structure of relations among different island communities rather than a narrow functionalist ethnography of life within a single community. It was, of course, Malinowski who championed the ethnographic method and (together with A. R. Radcliffe-Brown) turned anthropology toward functionalism and away from regional and comparative questions.

The Micro-Evolution Project was also not the only collaborative research project of its own time. About the same period A. P. Vayda at Columbia University had put together a team of researchers in the Western Highlands of New Guinea, working on a joint project to study "Culture and Environment in the New Guinea Rainforest." Vayda and his students were attempting to understand cultural diversity by exploring environmental or man-land relations (1966); they specifically hoped to achieve an "analysis of the cultural adaptation of a primitive horticultural population to its environment" (Clark 1971:x). Although cultural adaptation was a stated goal of the Columbia project, its major anthropological and geographical contributions have been in more specific relationships between the Maring people and their rain forest environment and, in practice, have had relatively little to say about cultural variation per se (see, e.g., Clark 1966, 1971; Rappaport 1968, 1984; Vayda 1971, 1989; Vayda and Cook 1964; Vayda, Leeds, and Smith 1961).[7]

In the early 1960s Albert Damon, in consultation with Douglas Oliver and William Howells, put together a team of anthropologists and biomedical researchers to work on the Harvard Solomon Islands Project (see, e.g., Friedlaender et al. 1987). The project's objective was

> to investigate the mutual relationships between culture, natural selection, and disease: the effect of habitat, occupation, nutrition, custom, and acculturation on the etiology, manifestation, and natural history of disease; the effect of inbreeding, isolation, mating patterns, demographic structure, and disease on physical, biochemical, and immunological characteristics; and conversely, the interpretation of biological variation, including disease, by the culture. (Howells 1987b:3)

The Harvard Project was thus explicitly regional, collaborative, and interdisciplinary.[8] Researchers conducted a combination of intensive ethnographic studies and shorter biomedical surveys in eight different communities in the Solomon Islands and on Bougainville Island in Papua New Guinea.

But, although the ethnographic component of the project was substantial --besides Oliver, the team included eleven anthropologists--it would appear that the main questions addressed overall were largely biological or biomedical, albeit issues firmly grounded in the ethnographic realities of the study communities.[9] This emphasis on medical and biological questions is entirely appropriate given that funding was primarily from the National Institutes of Health. Such a bias toward biomedical questions is also natural given the composition of the rest of the study team: besides Damon and Howells, the team included at least twenty-six human biologists, physicians, and other biomedical researchers.[10] Like Vayda's project, the Harvard Project has con-

tributed much to the study of Melanesia, but its regional and comparative findings have been largely biological rather than ethnological.

In certain respects, the Washington, Harvard, and Columbia projects each built on the New Britain Project, the first postwar multidisciplinary project in Melanesia, which was organized by Ward Goodenough at the University of Pennsylvania. Although somewhat more limited spatially than the projects that would follow, the New Britain Project followed a regional reconnaissance survey by Goodenough (1952) and developed as an intensive study of the West Nakanai, a little-known group on the north coast of New Britain.

Goodenough was clearly interested in exploring the linguistic, cultural, and biological relationships between this part of New Britain and other parts of the Pacific. To do this he assembled a team of five researchers that included himself and four graduate students: Ann Chowning, Daris R. Swindler, C. A. Valentine, and Edith Valentine. Each team member was responsible for a certain part of the research, but since four of them were cultural anthropologists, in practice there was considerable overlap in their data. The New Britain Project produced considerable data about the Nakanai, but as so often happens with collaborative studies, the various results were published more or less independently of one another and in only a few cases made comparative use of data gathered by different researchers with different perspectives (e.g., Goodenough 1961; Chowning and Goodenough 1966, 1973).[11]

The New Guinea Micro-Evolution Project: A Collaborative Project

To date, the Micro-Evolution Project is still the most systematic collaborative effort to explore cultural variation in Papua New Guinea. It has produced a lengthy bibliography about its four study peoples in the Kainantu district of the Eastern Highlands (Awa, Auyana, Gadsup, and Tairora). In addition, the project inspired (or encouraged) several other ethnographic studies in adjacent communities (Agarabi, Binumarien, and Southern Tairora, to list but a few). This small part of the Eastern Highlands is one of the best documented parts of Melanesia; probably only the Massim area is as well studied.[12] I should note, however, that despite two Kula conferences (Leach and Leach 1983; Damon and Wagner 1989), studies in the Massim have not been a coordinated, collaborative effort but have nearly always been the work of individual scholars dealing with their own idiosyncratic questions, problems, and concerns.

The Micro-Evolution Project began as an exploration of culture change using an explicitly stated evolutionary framework. The project started with several assumptions: (1) language is a good marker of past historical identi-

ties; that is to say, although language changes over time, linguistic similarities would remain as a marker or survival of previous associations;[13] (2) the cultures of a single ancient community would diverge over time as a natural part of the process of cultural evolution through diversification, as each segment of the original population adapted to its own local environment; and (3) the process of culture change through diversification may partly involve random changes, but changes would also be influenced by the environmental conditions that different parts of the original population had experienced.[14]

Early reconnaissance had already established that the Auyana, Awa, Gadsup, and Tairora languages were members of the same branch of the same language family; it was, thus, assumed that their four modem populations were descendants of the same ancestral language and people. The intensive study of these four peoples, their languages, their biology, their environments, their prehistories, and their contemporary cultures, would--it was assumed--illuminate how differences in local environments had (or had not) shaped cultural evolution through processes of natural selection. The project, thus, did not employ a naive rendering of evolutionary process viewed (à la Leslie White) as "progress" or as improved methods of energy capture. The study of microevolution (evolution at the local level) was an attempt to find an intellectual framework that would allow anthropologists to understand variations among the project's four peoples.

The four study peoples differed from one another in language, culture, and biology, though it must be admitted from the outset that these differences were not great along any of these dimensions. To be sure, one could easily select four other communities in New Guinea living less than one hundred miles from Kainantu that differed far more dramatically in all three dimensions (language, culture, and biology) than these four, but such peoples would not share the close linguistic associations present among these four groups. The Micro-Evolution Project, thus, was specifically designed to explore the processes of cultural evolution acting on a common ancestral community or *Urstamm* (see Watson 1963).

The Micro-Evolution Project's Research Questions and Conclusions

James B. Watson, who organized and was largely responsible for the design of the project, recognized from the beginning that

> a certain risk was involved in selecting four closely related and (as it happens) contiguous peoples for study. Obviously, they would differ far less from each other than New Guinea peoples purposely chosen for contrast. The less they differed, furthermore, the harder

> it would be to be clear about differences and similarities. Some present methods of anthropology are better suited, admittedly, to the comparison and analysis of fairly gross differences than of minute ones. (1963:190)

These considerations did, in fact, prove problematic for the analysis of microevolution, though such problems had almost no bearing on the work of the individual field researchers. Each fieldworker brought back great heaps of data and, for the most part, each has published a considerable amount about this tiny comer of New Guinea.

The basic descriptive task was originally planned to consist of four ethnographies as well as four comparative studies of human biology, language, prehistory, and the environment. Each of these eight studies was to be published by the University of Washington Press as a volume in the series Anthropological Studies in the Eastern Highlands of New Guinea. Of these, six volumes in the series had been published by 1983: the four comparative volumes (Littlewood 1972; McKaughan 1973; Pataki-Schweizer 1980; Watson and Cole 1977) and two of the ethnographies (Robbins 1982; Watson 1983). Although the series monograph for the Gadsup was never published, an independent monograph (Du Toit 1975) and a dissertation (Leininger 1966) together accomplish nearly the same end.

Watson's ultimate goal, however, was not simply a shelf of monographs about the project's study peoples. In addition, Watson had originally planned a final volume that would synthesize all the comparative data. He sought ways to document and describe differences and similarities in order to address the empirical question of how and the analytical question of why these four study peoples had diverged in the ways they had. Watson hoped (and I believe he expected) to find covariations between environmental and cultural differences, because--like many other anthropologists who (in the early 1960s) were interested in the issue of culture change--he assumed that such covariation would elucidate the processes of cultural adaptation. That is, such covariations between environmental variation and cultural, linguistic, and biological variation could be expected to identify the main factors that had shaped cultural diversity in the study area.[15]

The difficulty Watson faced from 1963 on was that he and his colleagues could not readily sort out how much local variation among these contiguous peoples was attributable to distinct environments and how much was because they had influenced one another since their initial stages of diversification. The four study peoples are too close to one another geographically and have had too much interaction to be able to separate these two processes very readily. One should not criticize Watson and his project team for

selecting the wrong four study peoples; no other potential study area on the island of New Guinea would resolve this difficulty effectively. Diversification and interaction may be distinct analytically but as processes they rarely occur separately in the real world.

This version of Galton's problem was explicitly recognized at least as early as 1972, when the first volume of the Eastern Highlands series (on physical anthropology) was published (Watson 1972:x-xi). The problem was that the fluidity of relations both within the study area and with outside groups made it impossible to distinguish original endowments from the effects of diffusion for many traits and features. In the end, such analytical problems overwhelmed the fairly crude protocols used to describe differences in culture, language, and biology (Littlewood 1994; McKaughan 1994; Newman 1994). In their own ways and dealing with their own data, Littlewood (1972:90-103), McKaughan (1973), and Pataki-Schweizer (1980:110-131) admit that the measures of variation chosen to study differences in biology, language, and environment were inadequate to the task of understanding cultural adaptation as a process of diversification. Though these measures were too crude for their intended task, the difficulty that all of these comparative studies faced was the inevitable confusion between inherited and diffused traits destined to emerge in this rendering of Galton's problem.[16]

Even in Polynesia, with its insular nature and great distances between islands, mutual contacts are far more important (and more difficult to understand on the ground) than one might assume.[17] The fact of the matter is that diversification (in an evolutionary sense) and influences that come from interaction (what used to be called simply "diffusion") are equally parts of the human social experience. One would be hard pressed to find any case where both processes have not been at work simultaneously.

Thus, one of the most important findings of the Micro-Evolution Project is that the four study peoples have *not* evolved separately and independently, but have evolved and diversified together as a region (see, e.g., Watson 1983:326-334). Some villages in the study area may be better connected than others and a few may even have been veritable backwaters for a very long time. But the fact remains that all of these communities have evolved together and have mutually influenced one another. They were adapting together and to one another and to their respective neighbors.

Such a conclusion differs markedly from the project's most important original hypothesis: that the four study peoples had diverged from a common ancestral community and that their present characteristics reflected differing local microenvironments. Ironically, the project's findings seem to suggest that social fields, interaction networks, and flow into and out of individual communities have played far more prominent roles in shaping the

study communities than any single set of local environmental factors. What Watson, in particular, has convincingly shown is that social fields are the key environmental factor that must be taken into account in the study of culture change or cultural evolution.

Many of the project's various volumes and papers suggest that nearly all of the participants were aware of this conclusion to some extent in their own data. But none of these researchers has been more aware of this pattern in their data than Watson, who made it a centerpiece of his own monograph (1983), although he began developing the concept in his important paper "Society as Organized Flow" (1970). Subsequently he developed this theme analytically in his discussions of the "Jones effect" and "crowded fields" (e.g., Watson 1974, 1977, 1983).

It would appear that Watson and the project team originally expected that some local microenvironmental factor or another would explain a large part of the local variation found in the study area. But in fact the entire corpus of the project's findings suggests that social fields, not the physical environment, are most critical. In these terms, the Micro-Evolution Project has disproved its original hypothesis in favor of quite another. In my view, these conclusions constitute the project's real success. Until the Micro-Evolution Project no one had looked at local environmental variation closely enough to see if the environment played more than a trivial role in explaining local diversity.[18]

If the Micro-Evolution Project chose the wrong hypotheses, it was not because members of the team were naive; they were drawing on general themes about cultural evolution that were current in the discipline at the time. Goodenough was attempting something quite similar in his summaries of the New Britain Project (1957, 1961). Sahlins and Service's book, *Evolution and Culture* (1960), and Irving Goldman's *Ancient Polynesian Society* (1970) suggest just how common this perspective was, even though such approaches have become less fashionable today.[19]

The Micro-Evolution Project's Design

Recently, several members of the project team have raised both methodological and analytical criticisms of it (Du Toit 1994; Newman 1994). They have suggested that fieldworkers were sent to New Guinea with too little preparation and with inadequate protocols to collect systematic data about local variation.[20] These critiques illuminate some methodological problems faced by team members, but they largely reflect the limitations of social research in the 1960s; they are hardly flaws in the project's conceptual design.

From a rather different perspective, Littlewood has questioned the project team's lack of interest in resolving, explaining, or accounting for anomalies (1994), particularly the apparent differences between the biological and linguistic findings.[21] This criticism is more compelling, but the fact that neither Watson nor any other project member has resolved these discrepancies is a consequence of the inadequacy of the working hypothesis for such an explication. By itself, the lack of concordance between linguistic similarities and biological similarities (Littlewood 1972:90-96) proves little, but when considered in light of the mobility, social flow, and social field data collected by various team members, it suggests that the original diversification model is itself incomplete. Far from being a flaw in the project's design, this conclusion is an extremely important finding, even if it leaves many empirical questions unanswered.

If the Micro-Evolution Project had a design flaw, it has to do with which villages were chosen to represent each of the study peoples. Following what has long been something of a tradition in ethnographic research, the project accepted the presumption that one language = one people = one culture. This presumption is common in anthropology. No matter how many times we are told (as students) that race, language, and culture do not always covary, the ethnographic method, particularly in a place as diverse as New Guinea, routinely leads anthropologists to see the ethnolinguistic group as the unit of analysis: the people, the culture, the society, or the generalizable area being studied.

In 1965 Watson himself had pointed out the problem of ethnographic generalization, noting that ethnographers routinely face the challenge of how widely they can extend their generalizations in New Guinea (1967:61). To illustrate this point, he referred specifically to the work of Ronald Berndt, who "violated" the discipline's long-cherished view that ethnographic description should be about a single ethnolinguistic unit, or "society." In Berndt's research among the Eastern Highlands Kamano and their neighbors the Usurufa, Jate, and Fore, these four peoples were said to vary primarily in language; Berndt believed that in other respects they were essentially identical and interchangeable (1954-1955, 1962, 1965, 1973).

Watson described Berndt's work as a "composite" description drawn from observations in several places; in this he helps clarify what problems exist in nearly every ethnographic work: each ethnographer must decide how far or for which local communities his or her generalizations hold true. Most fieldworkers choose to limit their generalizations to villages that speak the same language, because there is an (often unspoken) bias that language is important in defining culture, although as Berndt's example suggests this need not be so. But as Watson noted, confining one's generalizations to people speak-

ing the same language does not alter the fact that one is generalizing and thus similarly providing a composite view.

Ironically, although Watson was far more astute than most anthropologists in understanding such problems of generalization, he and his team were not immune from the same difficulties. For each of the ethnographies, the project had to limit itself to particular ethnographic study villages and the villages chosen were used to represent the "study people," all of whom spoke the same language. In nearly every case, the project's ethnographers (like most ethnographers before and since) produced composite views. The difference here is that this was a project that required data about specific villages, not composite pictures from several villages.

In all fairness, Watson and his team did try to sample environmental differences within the language group they worked with by living in two sites, because it was clear that some villages were located in the bush and others in the grasslands. But they each made decisions in choosing field sites that affected how much local variation they might observe.

My point here is not to quibble with the particular villages that were selected as study communities, because as an ethnographic field site any study village is about as good as any other. But, the study villages selected were in several cases chosen specifically because they were more or less centrally located (in Awa and Auyana), or because (in the case of Gadsup and Tairora) they did not lie directly on a linguistic frontier. The reason for selecting these villages, of course, has to do with the project's initial assumptions and its working hypothesis: each ethnographer was to document one of the four modem peoples. Given an interest in differentiating the four study peoples as much as possible, widely scattered sites might be expected to yield the greatest differences between the study peoples in the sample.[22]

Unfortunately for the project's most important findings, those about social fields and the effects of interactive networks on cultural change, centrally located villages were the worst possible choice. Living far from a linguistic frontier made it virtually impossible to collect rich data about multilinguistic social fields. Such a selection of field sites made the effects of social fields that crossed language boundaries far more difficult either to perceive or to observe. It is a tribute to Watson and his team that they were able to identify such social networks as important factors in shaping local variation within the region.

Whether optimal choices or not, such decisions, of course, are not crimes, nor are they really mistakes since one can always gather lots of useful and interesting data in any village.[23] But this issue does lead me to question anthropology's preferred field strategy of picking centrally located villages to represent entire ethnic groups. Such a strategy is a methodological barrier

to understanding just how communities have changed and just how they have maintained their ethnic distinctiveness in New Guinea. By choosing study communities or field sites that are--as far as possible--representative of what is presumed to be a "pure" or most common culture, we (as a discipline) have guaranteed that our ethnographic descriptions will reaffirm the conclusion that ethnolinguistic groupings are the "natural" units with which the inhabitants of New Guinea should be analyzed. One gets quite a different view of ethnic traditions, cultural stability, and ethnicity in New Guinea communities if one views New Guinea societies from mixed communities where more than one language is spoken.[24]

If the objective is to understand culture change and how communities change, or why there is so much cultural, linguistic, and biological diversity in New Guinea, then comparative studies that treat ethnolinguistic groups (rather than villages) as the unit of analysis are destined to fail, for the simple reason that *each* village is likely to be influenced by all of its partners or neighbors. Thus, any particular ethnolinguistic group is bound to show some variation among its constituent villages. At present, however, we have few systematic protocols for assessing this diversity and most of the time anthropologists generalize from one village to most or all of the villages in the ethnolinguistic group. The result is that researchers have largely missed the local variation within ethnolinguistic groups. As such, those small variations that emerge from differential contacts and influences with external groups, which are likely to be critical in understanding broad patterns of cultural change, are often ignored.[25]

The A. B. Lewis Project and Diversity on the Sepik Coast

Since 1987 John Terrell and I have collaborated on the A. B. Lewis Project sponsored by the Field Museum. Drawing on Anthropology Curator Albert Buell Lewis's vast collection of Melanesian material culture, his field notes, diaries, correspondence, and photographs, we have been attempting to describe and understand cultural diversity on the Sepik Coast of New Guinea. With support from the National Science Foundation, the Field Museum, Northwestern University, and the Walgreen Company--and with the help of several graduate students, undergraduate interns, and museum volunteers--we analyzed variations in Field Museum's collection from this coast. Terrell and I visited the Aitape area on a reconnaissance survey in 1990. Then during 1993-1994 my colleagues and collaborators (John Terrell and Wilfred Oltomo) and I conducted a year of regional fieldwork on the same coast.[26]

In the pages that follow I present some of our findings about cultural

diversity. In particular, I explore certain regional hypotheses we have adopted in this research and our reasons for accepting these hypotheses. In many respects we began our research where the Micro-Evolution Project's conclusions left off, attempting to examine how social fields and interactive networks have shaped local diversity on New Guinea's Sepik Coast. Here, interactive processes are unmistakable, but they are also very complex.

The Lewis Project began in 1987 with an analysis of the field notes and expedition diaries of A. B. Lewis, anthropologist and Field Museum curator, who visited most parts of Melanesia during the four-year Joseph N. Field South Pacific Expedition, 1909-1913 (see Welsch 1998). Lewis was the first American anthropologist to conduct systematic field research in Melanesia. His research was regional and emphasized material culture; he assembled a collection of nearly fifteen thousand items from some three hundred communities throughout Melanesia. One of the best parts of his collection and associated field notes (and photographs) is from the coast around what is now Aitape and Wewak, which he visited in 1909 and 1910.

Two features of life on this coast struck Lewis most forcefully when he visited what is now the Aitape area: (1) the extraordinary importance of what he glossed as "trade" along the coast, and (2) the profusion of different languages spoken by people who seemed to be in regular contact with one another. Of the 328 items he purchased on Ali Island, for example, nearly half had actually been made elsewhere, with at least 80 percent of these exotic goods being from places whose languages were quite different from that spoken on Ali. Some sixty mutually unintelligible languages are spoken along the seven hundred kilometers of coast and offshore islands between Jayapura and Madang (Wurm and Hattori 1981). These languages belong to at least five (Wurm and Hattori 1981) but perhaps as many as seven distinct language phyla (Foley 1986). Linguistically, this area is one of the most diverse places on earth.

The Sepik Coast is environmentally, culturally, and linguistically very different from the conditions Watson and his colleagues found in the Eastern Highlands. While the Eastern Highlands is a continental world made up of numerous valleys with steep mountain ridges between them, the Sepik Coast is a maritime world that was (formerly) linked by ocean-going outrigger canoes. Many mainland people along the coast visited their friends and relatives on foot as in the Highlands, but the geographic extent of their social networks was much greater on the Sepik Coast. And the Bismarck Sea played a vital and ever-present role in people's lives.

Unlike the largely homogeneous subsistence economy of the Eastern Highlands, the coastal economy varied from place to place as much as did the coastal environment. Drawing on early German sources about the Sepik Coast, Tiesler confirmed that economic specialization was important in this

coastal region (1969-1970), much as Harding had described in the Vitiaz Strait to the east (1967). Much of the "trade" along the coast could be explained, in part, by local differences in available resources and the limited number of communities that produced certain specialized items. The uneven distribution of resources and economic specialization, however, did not explain how this "trade" had been traditionally organized. What accounts were available suggested significant variation along the coast as well as differences from patterns described in better-known trading networks in the Vitiaz Strait, the Massim, the Papuan Gulf, and Geelvink Bay (e.g., Hogbin 1934/1935b; Barlow 1985; Lipset 1985; Tiesler 1969-1970).

Clearly, this coast presented quite a different set of social conditions from those encountered by the Micro-Evolution Project. If primordial differences had existed on this coast in the distant past, it was clear that the linguistic and other differences that had been reported since the nineteenth century (Finsch 1888; Parkinson 1900; Schlaginhaufen 1910, 1959; Neuhauss 1911; Lewis, in Welsch 1998) were *not* a simple consequence of diversification during a long period of isolation.

Sepik Coast communities were anything but isolated from one another. Whatever the mechanisms and processes that maintain linguistic and other differences might be, they did not include isolation in the historic past, and probably not in the last three to five thousand years. For this reason, our original hypotheses assumed that interaction, cultural diffusion, and trade were likely to play important roles in explaining the melange of linguistic differences found on this coast.

By using Lewis's field notes and collection as a starting point (see Welsch 1998), supplemented by other early published accounts, we felt that we had something of a baseline about what the region was like at the beginning of the colonial period. What initially attracted my attention was determining just how much of the "trade" Lewis reported was associated with actual interaction with people in distant communities. As is the case elsewhere in the world, we expected people on this coast to have more contact with people living nearby than with those living some distance away. Following Harding we also anticipated that the inhabitants of some of the small, resource-poor islands might have played a critical role as middlemen traders in moving products around, much as the Siassi Islanders had in the Vitiaz Strait (1967).

Historical Background and Initial Reconnaissance

By the 1990s the region had long been exposed to outsiders. The Neu-Guinea Compagnie had opened a station on Seleo Island in 1894 and the SVD Roman Catholic missionaries established their first New Guinea sta-

tion at nearby Tumleo in 1896. The German government opened Eitape patrol post (now Aitape) in 1906, allowing a handful of European and Asian planters and traders to establish themselves at various places along the coast. With the company and the government came labor recruiting, although this does not seem to have been much more than a trickle of laborers from any one village before the First World War. The war itself had little effect on the Sepik Coast, but the Australian mandate brought in many Australians to replace German planters. Recruiting continued, as did the expansion of copra plantations. But, unlike the area around Madang where large tracts of the most productive village lands were alienated, most villages retained control of their best land and both the German and Australian administrations even made arable tracts on the mainland available to the resource-poor islanders.

The Aitape region experienced its share of "cargo cults" but these seem to have been much smaller and more localized messianic cults than those reported by Burridge (1960) and Lawrence (1964). Some cults were in progress during our field research (both in 1990 and 1993-1994) but had an extremely local character. Others had assumed a more routine nature during the past half century; for example, the Barjani cult on Walis Island has become a local healing cult with a shrine that belonged to one of the islands several lineages. Yali's men had reached the coast east of Aitape in the early 1950s, but Yali's message got transformed into a local cult in the Suain-Ulau area. This was a local cult, dealing with very local concerns, not the coordinated and widespread cargo movement it became on the Rai Coast near Madang (Lawrence 1964).

The Japanese occupation during the Second World War brought a complete collapse of the local economy, which had continued almost entirely as a subsistence economy. Later Allied bombing, first on the islands and then on the mainland, devastated large parts of the coastal zone. One still sees evidence of the horrendous battles around Wewak, Aitape, and Jayapura.

Counterintuitively, despite the extraordinary destruction and hardships of the war, the region has proved remarkably resilient. By the mid-1950s virtually everyone displaced during the war had returned to their villages. The Australian-run, copra-based plantation economy was limping along, and villagers seemed to have returned essentially to their prewar subsistence-based lifestyles.

During the 1950s and 1960s government officers successfully introduced rice cultivation in several areas, but these small-scale projects ultimately failed for lack of machines to husk the harvested rice. Other cash-earning opportunities remained extremely limited. In short, up to independence in 1975 much of the Sepik Coast, particularly the area around Aitape, was as much the economic and political backwater it had always been. When Vanimo was

chosen provincial headquarters of Sandaun (West Sepik) Province, Aitape was destined to remain an economic backwater for at least the next twenty years. Until the Ok Tedi project in the 1980s, the Western Province had been the least developed of Papua New Guinea's nineteen provinces, but the West Sepik had always been runner-up; it now ranks as least developed.

Sale of copra, fish, and a handful of other products provides some cash to villagers. Similarly, a small number of local jobs and remittances from family members employed in bigger towns add their share to the local cash economy. Overall, though, in 1994 the region was still cash-poor and at least 90 percent of the resident population was still primarily dependent on traditional subsistence activities, supplemented only nominally by cash crops or wage-earning activities.[27]

Before our first reconnaissance visit to Aitape in 1990 we expected that the fieldwork we planned for the Lewis Project would be to a greater or lesser extent a kind of "salvage ethnology." Anonymous reviewers of our proposals and other colleagues were skeptical that we would find anything at all worth studying on this coast. Thus, we were both delighted and surprised to find that in the eighty years since Lewis had visited this coast the basic patterns of life had changed in remarkably small ways (Welsch and Terrell 1991; Terrell and Welsch 1990a). People now wore clothes instead of loincloths and everyone had access to some manufactured goods. Roman Catholic ritual had largely supplanted earlier religious activity, but the people retained their interest in religious ritual. People still depended on the same diet of fish and sago, they still got products from partners along the coast, and village life was still much as it had been in Lewis's day. John Woichom, a talented anthropology student from Ali Island, had a decade earlier discussed the continuity of the canoe-building industry on his home island (1979), noting that while the large overseas outrigger canoes were no longer being built, the islanders had taken up diesel boatbuilding to replace their earlier industry.

We found that some of the older, specialized craft industries had disappeared, such as the shell-ring industry on Ali, Seleo, and Angel. Much to our surprise, however, in 1993 we found this industry active on Tarawai Island to the east. Fishing had expanded as a local industry in several places. Production of earthenware pots, which had always been Turnleo's most important product, had declined, but these pots were still in great demand in most places along the coast, as they had been in Lewis's time.

New kinds of transport in the form of roads, outboard motors, and diesel boats had reshaped the region's geography; better transport brought some places much closer together just as it made other communities more remote and isolated than before. The coastal geography was also affected by urban-

ization at Wewak, which had disrupted the flow of Murik baskets from the Murik lakes. As Wewak grew, social ties with people in Aitape became less important to the Murik, who have largely allowed these relationships to become inactive. Now, if Aitape people want or need Murik baskets they buy them at the market in Wewak.[28]

Even during our brief visit to Aitape in 1990 it was clear that the "trade" relations Lewis had written about were based on friendship ties between unrelated individuals and getting information about these relationships would be much easier than we had suspected. I had assumed that the traditional relationships between villages that Lewis had observed would be "ethnographically salvageable" through a series of structured interviews with older individuals about their fathers' networks, if not their own. Thus, in 1993-1994 I planned to work up and down the coast interviewing older men about their partners in other places. It was reasonable to assume that a great deal of reliable data could still be elicited from this sort of interview. But it was obvious in 1990 that it would be impossible to understand the extent to which different villages were linked to one another without conducting the same kind of survey or structured interview in many villages.

Social Networks, Ethnographic and Prehistoric

It was with considerable astonishment that soon after my return in 1993 I realized that not only could I readily elicit information from older men about their *own* friends along the coast, but I could also conduct participant observation of these relationships and interactions. During a year's stay on Ali, working up and down the coast I conducted 130 interviews representing some 80 villages or hamlets. In addition, I was able to observe directly about two dozen interactions (some extending over several days or weeks), all of which gave me some firsthand confirmation about the information I had elicited in my interviews.

As unlikely as such findings may seem, the social networks that Lewis had observed in 1909 were still active in 1993-1994. In some areas, of course, these networks had contracted. But in others, particularly in the hinterland villages a few miles inland, these social networks were growing and expanding inland. As enterprising and socially sophisticated men saw the benefits a large network of friends provided, they exploited better transportation--and their own strategic geographic position on a road--to enlarge their own personal networks.

Although it took many interviews and many months to understand what my informants were saying, it finally became clear that what Lewis had glossed as "trade" and the relationships I had previously glossed as "trade

partnerships" were, at their core, actually "friendships" between individuals in different, often distant, villages. These friendships were passed on to close kin from generation to generation, warranting the term "inherited friendships."

In 1993, for example, I was present in Ulau when a group of brothers--all mature men--formally assumed their father's role as a friend to the Ali Islanders I had come with. The Ali people brought a few earthen pots on this visit but took back to Ali some fifteen bundles of sago, eight large bags of yams, as well as smaller quantities of bananas, betel nut, and other garden produce. There was no barter, no bargaining, and no trade as such. This interaction was an extremely warm and friendly social visit, marked with the warmth and affection that comes from a lifetime of mutual generosity. Everyone laughed and joked and gossiped as friends do in most places. But on this occasion, the most important transaction came when the middle-aged brothers formally assumed their late father's friendships by presenting the formal gift of a large bundle of tobacco, which represented one generation replacing another.

This example demonstrated the important role that friendship still plays along the Sepik Coast. No kinship ties, no intermarriages, no blood relationships exist between these two families of Ulau and Ali. There is reciprocity, since everyone expects that in one form or another all the gifts of sago, yams, betel nut, and vegetables will eventually be balanced by gifts of fish, pots, pork, foodstuffs from town, and the like. But there is no balance sheet and the key message both parties wanted to impress upon me was how much they enjoyed one another's company *and* how their own actions were motivated by generosity toward friends who could always be counted on. I heard such sentiments in many other villages.

As I made my way from village to village it was clear that these friendship relations were present everywhere I visited along the coast. The gifts that were given and received varied greatly from place to place, but the manner in which they were given and the emphasis on generosity hardly varied. A year of research in some eighty villages clearly revealed that despite some contraction in most people's individual friendship networks over the past generation or two, the institution is organized much as it was a century ago. These friendship networks are extremely important for people in most villages and still vital to people who inhabit the small, resource-poor islands.

While I was conducting these interviews, my colleague John Terrell was conducting an archaeological survey of much of the same region. Although during this field season he only made surface collections rather than excavations, he was able to sample 121 sites in some 20 communities. These surface collections yielded 10,644 potsherds, 1,472 obsidian flakes (1.517 kilo-

grams), 75 chert flakes, 23 pieces of worked shell, and 10 stone or shell adzes/axes. The obsidian flakes have now been sourced to three different quarry areas in New Britain and Manus, suggesting that importation of at least this exotic material has considerable antiquity, reaching back at least a few thousand years. Although the potsherds found along the coast exhibit considerable variation, we suspect that the earliest pottery-making industries on the Sepik Coast were local expressions of a widely distributed "family" or "tradition" of ceramic industries in the western Pacific. We now hypothesize that these widespread commonalities in pottery making gave rise to a number of distinctive local traditions at Vanimo, Serra, Aitape, and Wewak. The existence of local ceramic traditions does not seem to have eliminated transactions that involved pottery, since we find apparently nonlocal sherds in many of these prehistoric assemblages.[29] So at least some of the socioeconomic processes we observed in 1993-1994 and that Lewis observed in 1909-1910 would appear to have been operating in the prehistoric past. The character of social networks may or may not have been based on friendship in the distant past, but whatever their character these networks clearly organized the regional economy and integrated broad sections of the Sepik Coast.

Conceptualizing Social Fields

Our analysis suggests that such networks have played an extremely important role in organizing cultural diversity along the coast. But social fields alone cannot account for all of the observed local variation in our study area. Environmental differences clearly play a role in shaping diversity, since at the very least the environment limits the kinds of specialized production that is possible in different localities. In certain ways cultural traditions that are passed on from generation to generation also play some role, although our preliminary analysis confirms earlier project findings (Welsch, Terrell, and Nadolski 1992), which suggests that interaction is a more significant factor than tradition in explaining variation.

What is quite clear on the coast is that any analytical model that begins with ancestral cultures and modifies these in light of differing natural environments (the process of environmental adaptation) will fall sorely short of any meaningful explanation of observed diversity. There are simply too many goods, people, and ideas moving along this coast to imagine that observed similarities are primarily due to traits that have been inherited from a common ancestral group in the distant prehistoric past. Diffusion is a factor of profound importance in our study area, but the same social fields that have promoted diffusion must have simultaneously promoted the creation and

maintenance of certain kinds of differences among these local communities. Thus, these social networks have not brought the creation of a uniform or homogeneous culture, but they have produced what we call a shared "community of culture."

When we began working on a collaborative project in New Guinea, we wanted to address some of the same basic issues that Watson and his colleagues had addressed nearly thirty years earlier: how can we describe and understand patterns of cultural variation?

The Micro-Evolution Project had, of course, phrased the question as an evolutionary one. We phrased the question as one explicitly involving social fields and interactive networks (Terrell and Welsch 1990a, 1990b; Welsch 1988; Welsch and Terrell 1991, 1994; Welsch, Terrell, and Nadolski 1992). While Watson sought a study area that shared a single linguistic ancestor, we sought a study area that had as much linguistic heterogeneity and environmental homogeneity as possible. (In practice, it happens that the coast possesses considerably more local environmental niches than we had originally assumed, but this was a factor we could not control for.)

Toward this end, we decided to focus primarily on the area around Aitape. Lewis had reported broad similarities along the Sepik Coast as well as a remarkable amount of local variation within a common cultural pattern (Welsch 1998:60-150). Even a brief visit confirmed that however one chose to conceptualize communities on this coast, interaction with communities that spoke different languages, had different products, and different needs had to be included; intervillage contacts and transactions were simply too important to be disregarded or ignored.

We set out to explore how these linkages and interactive networks could exist without homogenizing the language and culture of this region. For the Lewis Project's research design it would have been nice if every community spoke a different language. But even in this extremely diverse linguistic environment, related languages are clustered in distinct groups along the coast.[30] For this reason, in specific cases it is often difficult, if not impossible, to sort out which similarities are due to shared ancestral traditions and which have developed in the context of vigorous interaction among all groups. In this sense, the Lewis Project is no better off than the Micro-Evolution Project was.

After a systematic survey in more than eighty villages, we found a "community of culture" along the Sepik Coast. All of the communities visited were connected to many others through wide-ranging friendship relations with people in dozens of other places. On average, each individual was linked to people in about twenty different villages; nearly everyone had ties in at least eight or ten other villages, while a few individuals interviewed had ties in as many as eighty different villages. Everyone had ties to people

speaking other languages, often to friends from many different language groups.

Our research thus reveals important new evidence for a much more complex pattern of regional integration than had previously been suspected. These findings are important because of the region's extreme linguistic diversity, even by New Guinea standards. Yet despite this profound linguistic heterogeneity, extensive personal networks of inherited friendships formerly (and, as our research shows, still) linked broad sections of the coast, its offshore islands, and its immediate hinterland.

Inherited Friendships

Regional integration along the Sepik Coast emerges from the cumulative and overlapping ties produced by inherited friendships. These friendships are individual relationships that are inherited from one's father or mother, from an uncle, a grandparent, or a foster parent. Inherited friendships are centered on individuals and relatively small extended families; they are *not* organized as relationships between communities, villages, or hamlets.

At the core of these friendships is a relationship between two individuals (these may be man/man, woman/woman, or man/woman). As these individuals visit one another, accompanied by siblings, spouses, children, and in-laws, cordial relationships develop among many members of the two families. As new generations are born they too accompany parents to visits friends in other communities. They meet their parents' friends abroad and also get to know the friends of their parents who come to visit. And so these relationships are passed on from one generation to the next as younger people first come to know their elders' friends and gradually, as their elders age and die, take over responsibility for managing these enduring relationships.

Everywhere along the Sepik Coast gifts of goods make up a significant part of the content of friendship relations. This fact has led earlier writers to describe these relationship in terms of "trade" or "exchanges" of needed products. Viewed in this way, what we describe as "inherited friends" have elsewhere been described as "trade partners" or "exchange partners."[31] In every village visited, people spoke about their friendship relations not in terms of trade, commodities, obligations, or the need to reciprocate or exchange but in terms of generosity. The epitome of friendship along the Sepik Coast is to respond to whatever needs one's friends present in as generous a way as possible. Because of the combined effects of uneven distribution of resources and specialized production, everyone along the coast has periodic needs. It is one's friends who can be counted on to provide goods, resources, assistance, or other help when needed.

As families mature, as children and siblings marry and have their own children, and as families celebrate the life-cycle rituals of their close kin, friends in other communities are expected to participate, mutually assisting one another, giving gifts, and sharing in each other's lives. Participation at funerals is an important aspect of this relationship between friends, especially when members of the senior generation pass away, whereupon primary responsibility for maintaining the ongoing friendship formally passes from parents to children. As elders pass the torch, the younger generation takes on responsibility for keeping up the numerous gifts and periodic socializing that allows such potentially fragile relationships to persist for many generations. It is principally in this sense that friendship along the Sepik Coast is a hereditary relationship.

Interactive Networks and Diversity

These patterns of inherited friendship present everywhere along the Sepik Coast create a shared "community of culture." But notably, the villages that participate in this community of culture do not possess a single homogeneous culture. Villages or clusters of villages with distinct and different local identities continue to be in regular contact with one another; they share many important cultural features. This level of cultural similarity, though, has in no way prevented individual communities from maintaining local identities, local dialects or languages, local customs, rituals, and other subtle differences in behavior and practice.

More importantly, it now seems that economic, linguistic, and cultural differences, such as those that exist within our study area, actually encourage interaction within the region. Simultaneously, it appears that interaction encourages communities to maintain (and even generate) cultural and linguistic differences. Local economic specializations create demand for products (pots, sago, fish, yams, ornaments, and so forth) not available locally, providing a reason to create and maintain persistent, hereditary friendships. Most surprising of all, informants explained the advantages of having friends who did not really understand one's own language. As they regularly told us, if visitors speak your language, you have to make up euphemisms for anything you do not want them to hear you discuss. Thus, language differences made it easier to arrange within the family the gifts desired by the visiting overseas friends without having to resort to euphemisms. The rise of pidgin over the past century has made linguistic differences as helpful as they ever were without actually encumbering communication.[32] Linguistic differences between friends can actually enhance friendships by limiting oral communication to the most basic and important topics; where linguistic dif-

ference is great, both parties must *act* as friends rather than merely *speak* as friends.

In the end, the local cultural differences that actually exist within our main study area are rather minor and may be considered differences in cultural diacritics that are interesting and exotic without being different in any important ways.

These new findings, we believe, take us one step closer toward solving Melanesia's most perplexing conundrum: How can communities that are in constant and frequent contact maintain local distinctiveness? Why haven't these diverse communities gradually become part of a single ethnic group sharing a common language and culture?

The Persistence of Diversity

I suspect that many of these findings will not seem startling or surprising to many anthropologists who have worked in Melanesia. There is mounting archaeological evidence to suggest that this community of culture has flourished for a considerable period, at least several thousand years. This archaeological evidence takes the form of at least one distinctive regional pottery industry that is as early as Lapita but different, an ancient shell-ornament industry, and substantial quantities of obsidian from three sources in the Bismarck Archipelago.[33]

The friendship networks we see today (and which Lewis saw in 1909 and 1910) appear to have a very early presence in the region. The individual communities of the Aitape Coast have, thus, evolved together as a region. As in all regions some communities were more connected than others and some are veritable backwaters. Yet each community has contributed to the common culture that exists along the coast by participating in its most important institution: inherited friendship networks.

Let me make it clear at this juncture that these networks do seem to have generated a common basic culture in these dozens of different communities with many different Papuan and Austronesian languages. But we also believe these same networks or social fields have encouraged each community (if not each hamlet) to differentiate itself from its friends in other settlements. We see the coast as still differentiating or diversifying, but these differences are not changing the basic cultural rules that allowed (and still allow) communities to interact.

Conclusion

One problem that we have encountered in our research on the Sepik Coast is that the social fields we can now identify are clearly not sufficient to

account for all of the local variation (both similarity and difference) we observe. Microenvironmental variation plays a role in explaining some of the diversity on the coast; for the most part these environmental differences are so obvious as to be trivial. And while such environmental differences may explain who is likely to have ties with whom, such differences do not explain the content of the many cultural diacritics that differentiate these communities.

The social fields and interactive networks we can document on the coast are surprisingly good predictors of which communities are likely to use the same marriage patterns (sister exchange), the same interactive idioms (friendship rather than kinship), and the same political/diplomatic protocols. These networks, when taken together with the local environments, are reasonable predictors of what basic material culture will be present in each community. But neither interactive networks nor the environment can predict what specific kinds of cultural diacritics are most likely in different parts of the region.

At this point, we confront the same explanatory barrier that the Micro-Evolution Project encountered. What is clear, however, is that both of these study areas have each evolved and changed as connected regions. Common ancestral stocks almost certainly have played some role in the Aitape region, though it is as hard for us to decipher this role as it was for members of the Micro-Evolution Project. In sum such long-term processes are more complex than the artificially simple models that we as anthropologists tend to bring to them. What seems needed in future studies are more-complex models that simultaneously consider divergence, microenvironmental factors, and social networks. Perhaps most needed are analytical models of social fields that can accommodate settings as varied as those in the Eastern Highlands and the Sepik Coast.

NOTES

An earlier version of this article was presented at a symposium titled "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," which was organized for the 1994 annual meetings of the American Anthropological Association by Kerry J. Pataki-Schweizer and Robert L. Welsch, held in Atlanta, Georgia. David Eyde, Terence Hays, Thomas Harding, Kerry Pataki-Schweizer, Pamela Stewart, John Terrell, Donald Tuzin, James B. Watson, and Sarah L. Welsch graciously offered comments and suggestions on earlier drafts. My understanding of the Micro-Evolution Project has also benefited from discussions with other participants and discussants in the symposium, including David Boyd, J. David Cole, Brian Du Toit, Eugene Giles, Paula Brown Glick, Madeleine Leininger, R. A. Littlewood, Howard McKaughan, and George Westermark. I offer thanks to all of these individuals for their assistance; any shortcomings are, of course, my own.

1. Andrew Strathern, who has spent a career studying peoples in the Western and Southern Highlands of Papua New Guinea, both in the village and in town, probably comes closest to this third strategy. As is inevitable with such lifelong research programs, his research has dealt with a variety of research questions. See, for example, Strathern 1968, 1969, 1971, 1972, 1994.

2. Reeds 1939 anthropology dissertation at Yale was published as *The Making of Modern New Guinea* (1943). This volume has always struck most anthropologists as distinctly outside the genre of ethnography and even outside the scope of anthropology. In some respects it is a regional study that dealt with the whole of the Mandated Territory of New Guinea as its "region."

3. Harding's *Voyagers of the Vitiaz Strait,* a groundbreaking example of regional research in Melanesia, is essential reading for anyone considering regional questions and how different communities might be linked to one another in New Guinea. I have long considered this book so important as background to the study of trade and intergroup relations that I routinely gave copies of it to students and interns who worked with Terrell and me on the A. B. Lewis Project. Originally, Harding's project was conceived of as a collaborative project as discussed here, and Marshall Sahlins--who actually did visit Harding's field site--had planned to be a full participant in the project. Harding's work differs from those projects considered here because, as conducted, it was essentially the work of a single anthropologist working by himself in the field and is thus beyond the scope of this discussion. I do not by this exclusion mean to denigrate its importance as a regional study. Like Gewertz's *Sepik River Societies* (1983)--which was also both seminal and important for regionally oriented anthropology--it suffers from the limitation that one researcher can only learn so much in the field.

4. Schwartz's study of the Paliau movement was, like Burridge's and Lawrence's studies, an analysis of a cargo movement that extended well beyond the boundaries of any single village or ethnolinguistic community. In all three cases it was this aspect of the phenomenon under study that led these authors beyond their original village field sites. Each of these authors later extended their analysis to broader regions or comparative problems (Burridge 1969; Lawrence 1973; Lawrence and Meggitt 1965; and Schwartz 1963). Of these, only Schwartz's important study of regional relations in Manus pursued a regional perspective as discussed in this article. Unfortunately, Schwartz never followed up on this study with empirical data that might demonstrate the generality of some of his claims.

5. A major exception to this was the Dutch expedition to North New Guinea in 1903-1904, led by Arthur Wichmann (1917). This expedition was interdisciplinary, as interested in flora, fauna, and geography as in the ethnology. Van der Sande summarized the expedition's ethnological findings (1907), while Lorentz published a popular summary of the expedition (1905). Many of the biological findings appeared in early numbers of *Nova Guinea.*

6. A. B. Lewis conducted the most extensive expedition ever mounted in Melanesia, from 1909 to 1913; Lewis was the first American anthropologist to conduct systematic research in the region (Lewis 1932; Welsch 1998). Among the. British social anthropologists, Rivers, Hocart, and Wheeler conducted research in the New Hebrides on the Percy Sladen Trust Expedition, in 1906-1908 (see also Slobodin 1978), after which Hocart went

on to an extended stay in Fiji (1922, 1929). In 1904-1905 Seligman returned to central and southeastern Papua on the Cook-Daniels Expedition. In 1914 he sent his student Malinowski to conduct a comparative study in southeastern Papua, where he planned to visit Mailu, Gabagaba, Woodlark, the Trobriands, the Mambare River, and other sites. Among the Germans, Thurnwald (1910, 1917, 1918), Neuhauss (1911), Friederici (1912, 1913), Sapper (1910-1911), Pöch (1906, 1907a, 1907b), and even Parkinson (1907) had similarly conducted regional surveys in German New Guinea. Among the Swiss were Felix Speiser (1923, 1991) and Fritz Sarasin (1917), both of whom conducted systematic surveys, in the New Hebrides and New Caledonia respectively. As Stocking has noted (1983), Landtman was the first anthropologist in Melanesia to conduct a systematic ethnographic study (1927); his work is often overlooked because it was published much later than Malinowski's and received much less acclaim in English-speaking circles as he was a Finn (and not based in London).

7. Other members of Vayda's collaborative research team included Roy A. Rappaport, Cherry Lowman Vayda, Allison Jablonko, William C. Clarke, John Street, and Georgeda Bick (see, e.g., Clark 1971:ix-xii; Rappaport 1968:xv).

8. The project's original goals, as set out in applications to the National Institutes of Health, were "to relate culture and disease, and vice versa, in a spectrum of societies varying in respect to ethnic background (Melanesian, Polynesian), ecological setting (atoll, coastal, mountain, jungle), and exposure to Western culture" (Howells 1987b:3). These objectives are clearly concerned with understanding cultural variation through controlled, cross-cultural comparison despite the project's biomedical biases.

9. For a summary of publications that have emerged from the project see Friedlaender et al. 1987; for a discussion of participants see Howells 1987a. The original surveys were conducted from 1966 to 1972, with follow-up surveys done from 1978 to 1980. The anthropological team included Roger Keesing, Eugene Ogan, Pierre and Elli Maranda, Harold Ross, Jill Nash, Donald Mitchell, Tim Bayliss-Smith, Gilbert Hendren, John Rutherford, and John Terrell. Publications about the study communities by these scholars are impressive. It should, however, be noted that most of the ethnographic publications emerging from this project were local ethnographies rather than studies that were inherently dependent on the biomedical studies or on comparisons. As we shall see, this tendency is also present among those who worked on the Micro-Evolution Project.

10. Among the biologically oriented team on the original expeditions were Damon, Howells, and Eugene Giles as anthropometrists; Lot Page and Robert Moellering Jr. for cardiology; Melvin Clouse, Bela Ratocovic, and George Gundry with radiology; Jerry Bloom and Lawrence Lai, serology; Jonathan Friedlaender, Stewart Hindley, Henry McHenry, and Daniel Hrdy on photography; Howard Bailit, Vincent Lombardi, Norman Mills, and Marc Halpern for dental studies; Jonathan Friedlaender, Jeffrey Froehlich, Muriel Howells, and Daniel Hrdy on dermatoglyphics; John Biddulph, Irvin Emanuel, and Neville Henry with pediatrics; and David Verlee as ophthalmologist (Howells 1987a). Jonathan Friedlaender, John Rhoads, Kevin Mohr, Lot Page, Jesse Page, and Richard Stevens worked on the restudies.

11. The main results of the New Britain project would also include Chowning 1958, 1965; Goodenough 1956, 1957; Simmons et al. 1956; Swindler 1955, 1959, 1962; and Valentine 1958, 1961, 1963.

12. We have more-comprehensive studies of other ethnic groups, such as the Melpa or even perhaps the Enga, but these groups have not been so systematically studied as parts of a region. Arguably the Mountain Ok region in Telefomin and Kiunga districts has been studied as intensively, although much less systematically or comprehensively (see, e.g., Hays 1990). Similarly, the Sepik River has been fairly intensively studied by anthropologists (Lutkehaus et al. 1990), but coverage has been somewhat spottier, probably because of its much larger area.

13. Language similarities represent the one area in which cultural anthropologists and comparative linguists accept, almost unquestioningly, the existence of any kind of cultural survival. All other traits that late-nineteenth- and early-twentieth-century anthropologists considered survivals or used as markers of past cultural association have gradually been discredited.

14. Here, "environmental conditions" should be understood in their broadest sense, involving the social environment as well as the physical or natural environment. According to Watson (pers. com., 1994), a key concern of the project's design was to understand how much freedom the study peoples had had in diversifying and changing from what was assumed to be a single ancestral population.

15. By 1980, Watson rephrased the project's objectives somewhat more explicitly than before:

> In the case of a given feature we could identify no exogenous cause, we would presume that' we were dealing with an element whose continuity among the study peoples might be attributed to original endowment (viz. founder effect, historical causes) or to common development, or to both. Variation on the other hand might result from accident or drift. Beyond these familiar categories the purpose of the project was to look minutely at change with respect to relative rates of diversification in different aspects of the language, "race," and culture of the study peoples; to attempt to recognize long-term trends and short-term effects; and to determine as far as possible what development or evolution meant in a given, fairly short span of time, for people constituted as were the study peoples, and living where they lived. (Watson 1980:viii-ix)

16. It would seem that this indeterminacy has been the major barrier to writing or compiling the final summary volume in the series. The project has thus failed to accomplish its intended analysis of diversification, but instead has demonstrated how fluidity and interaction--issues at the heart of Galton's problem--have made the intended analysis almost superfluous. As I suggest in the rest of this section, the project's failure in this area is its greatest success. Hays (1994) and Tuzin (1994) from somewhat different perspectives similarly suggest the Micro-Evolution Project was successful, despite its failing to produce the specific analysis as originally proposed.

17. For example, even on Tikopia, which Firth depicted as an almost completely isolated island (1936, 1939, 1940), Kirch and Yen (1982) found archaeological evidence for an astounding amount of interaction with other communities, including some quite far away. As I suggest elsewhere (Welsch 1987), Kirch and Green (1987) make essentially the same mistake in Polynesia as found in the Micro-Evolution Project's original hypotheses and assumptions.

18. Newman (1994) argues that neither Watson nor any other member of the project team has, as yet, systematically attempted to analyze the variation among the four study peoples. While no such systematic comparison has been completed, Watson (1983) has analyzed the variation within the Northern Tairora area, where he concludes that social fields, not the natural environment, best account for observed differences. His knowledge of the entire study area as well as the findings of the other participants would allow him to generalize on these processes. Indeed, in Watson's own work, the unmistakable importance of adaptation to a group's social field over environmental adaptation seems to have led him to abandon the controlled comparison as originally planned.

19. For a more systematic presentation of the "1950s Agenda," see Terrell, Hunt, and Gosden 1996.

20. The main method for documenting variation was to code ethnographic field data using the HRAF *Outline of Cultural Materials* (Murdock et al. 1950; see Watson 1982:ix). Newman argues that the *Outline of Cultural Materials* was inadequate for the needs of the project (1994). Agreeing with Newman, Du Toit (1994) also suggests that the several graduate students were poorly trained when setting off for the field. The claim about inadequate training is hard to support today, since three (Du Toit, Leininger, and Pataki-Schweizer) of the graduate students on the project have had successful careers, holding positions as professors. Newman's critique is more telling, but there were few other instruments available during the main fieldwork period (1959-1964) and at the time there was also much more acceptance of such categories than would be true today.

21. In the 1960s it was clear that the similarities and differences in language did *not* correspond to similarities and differences in human biology markers (Littlewood 1972:92). Groups that were most similar linguistically were not most similar biologically. Rather than being a flaw in the project design, these discrepant findings are exactly what the project might have been expected to identify. That such discrepancies tax the project's original working hypothesis goes without question. The difficulty explaining such findings probably accounts for why so little has been made of them in the published record thus far.

22. Here, I do not mean to suggest that the team was attempting to bias their data; on the contrary, they were doing what most ethnographers have done in selecting field sites.

23. In 1977, I too chose a "central village" as a field site among the Ningerum. For my own research, I wanted to study the use of aid posts, so Hukim village, one of two with an aid post, was a logical choice for my field site (Welsch 1982, 1983). While this village was good for that particular study, the choice of a central village gave me less opportunity to observe how important or unimportant interethnic interaction was at the margins of my ethnolinguistic study area. I did not realize the effect of this decision until later, when writing about regional relationships in the Ningerum area (e.g., Welsch 1994).

24. Examples of these kinds of communities are not difficult to find. I saw several around Ningerum in the late 1970s, and Lorraine Sexton worked in an even more interesting village at Daulo Pass where three languages were spoken (1982, 1986).

25. That these are ongoing issues is demonstrated by Knauft's important study on the peoples of New Guinea's South Coast (1993). Despite many important new insights about

the region, his study is weakened by dealing with entire ethnolinguistic groups as the units for regional comparison.

26. Funding for the 1993-1994 phase of the A. B. Lewis Project came from the National Science Foundation (Grant DBS-9120301) and the National Endowment for the Humanities (Grant RO-22203-91). Welsch spent nearly a year on the Sepik Coast conducting an ethnological survey; he was assisted in this by Wilfred Oltomo, Chief Technical Officer, Department of Anthropology, National Museum and Art Gallery in Port Moresby, who spent almost five months in the field. John Terrell spent four months on the coast, primarily engaged in conducting an archaeological survey. He was assisted by other members of the team and two students from the University of Papua New Guinea (Michael Reupana and Alowis Kuaso).

27. Between our visits in 1990 and 1993 one logging operation had begun in Serra. By 1994 this was bringing in regular--if modest--wages for most of the younger men. As I left in 1994, other logging projects around Paup and behind Sissano lagoon were being negotiated at provincial government levels. If they go ahead, such projects will have considerable impact on both the local cash economy of the Aitape district and on the environments of these two areas.

28. These baskets are one item often purchased by tourists at the Wewak markets, but I would estimate that 60 to 70 percent of the stock is actually sold to nationals. There is no tourist industry as such in either Aitape or Vanimo.

29. This finding parallels a similar pattern in the Field Museum's ethnological collections from Tumleo Island, the most important pottery center around Aitape in the early colonial period. Fully 10 percent of all the pots collected on Tumleo before 1910 were actually made elsewhere, even though Tumleo people produced pots in extraordinary numbers during this period for their hereditary friends.

30. The fact that most language families are geographically clustered has been a conceptual problem for Romney and his colleagues, who still consider cultural variation on this coast as dependent on language affiliation, despite the incredible number of things, people, and ideas that previously and still move along the coast (Moore and Romney 1994, 1995; Roberts, Moore, and Romney 1995; cf. Welsch and Terrell 1994; Terrell 1995; Welsch 1995, 1996).

31. In some of our earlier reports (e.g., Welsch and Terrell 1991), we discussed these intervillage relationships as "exchange partners," following the usage of Barlow (1985), Lipset (1985), and in some respects Tiesler (1969-1970). During more intensive fieldwork, however, it was clear that this usage did not capture the meaning of these relations locally (see, for example, Welsch and Terrell 1994).

32. Pidgin is universally understood by old and young alike. Before the arrival of pidgin, communication was accomplished through multilingualism. Children were sent off to stay with a distant friend for weeks or months specifically so they could learn the basics of the friends language.

33. The obsidian we found appears to be partly from Talasea and partly from Lou Island. We also found one Lapita sherd on Ali Island. This sherd is the westernmost Lapita sherd

ever found at an identifiable site. That we found only a single Lapita sherd among many thousands of other potsherds demonstrates that, while the Sepik Coast was connected to the Bismarck Archipelago during Lapita times, Lapita pots were not the primary pottery used on the coast. We feel this is evidence that the coast had its own distinctive pottery traditions.

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REVIEWS

Willie Dari, Vasiti Ligairi, and Solange Petit-Skinner, *Adolescents in Fiji.* San Francisco: MacDuff Press, 1994. Pp. 128. US$15.

Reviewed by Ben Finney, University of Hawai'i at Mānoa

THIS INTRIGUING MONOGRAPH is co-authored by two Fijian researchers, Willie Dari and Vasiti Ligairi, and Solange Petit-Skinner, a French researcher trained in both anthropology and psychology who has considerable experience in the Pacific. The research on which this work is based was conducted, apparently in the early 1980s, under the aegis of the French Ethnological Mission of Fiji, which was directed by Petit-Skinner and funded by France's Ministry of Foreign Affairs. The mission's goal was to train Fijian researchers, have them tape-record Fijian traditions to establish an archive of indigenously gathered materials, and then have these researchers publish a series of monographs about various themes in Fijian culture based on the materials. Although the mission was closed during the political turmoil of the late 1980s, an archive of thirteen hundred tape cassettes has been deposited with the Fijian government. In 1985 the first of a series of three projected books was published: *Fijian Protocol,* by Dari and Petit-Skinner (San Francisco: MacDuff Press). This book, published nine years later, is the second of the series. The third of the series, featuring the research of Leata Saunua on birth rituals, will be published sometime in the future.

This monograph is divided into three parts: (1) "Circumcision," based on the interviews conducted by Dari; (2) "First Menstruation," based on the interviews conducted by Ligairi; and (3) "Interviews about Puberty in Fiji," which features transcripts of two of the interviews used by the authors.

Petit-Skinner states that she trained her colleagues in the nondirective, psychological interviewing techniques of Carl Rogers, designed to allow the persons being interviewed to express themselves freely about the topic, raising and elaborating on themes they consider important. This strategy carries over into the syntheses based on the interviews, for the descriptions of circumcision and first menstruation are written very much from a Fijian point of view without the overt intrusion of Western methodological and theoretical concerns. In addition to the technical and social details of circumcision and first menstruation, we therefore learn much about how Fijians feel about these two events. For example, the authors stress how proud the boys are to have gone through this portal into adulthood and to have endured the pain of having their foreskins cut--though now usually done with a razor blade rather than a bamboo knife. (Incidentally, the term "supercision" might be preferable to "circumcision" in that the top of the foreskin is slit, not cut all the way around and removed. The distinction is important, for, according* to a Fijian expert quoted [p. 37], the operation leaves the two halves of the foreskin bunched up underneath the head of the penis for "the pleasure of the two partners" that is part of "the sweetness of life.") In the last few pages of the menstruation chapter, the authors do venture some generalizations about the symbolism of the blood shed with supercision and first menstruation, but these too are linked to Fijian beliefs rather than exogenous theories. It is this focus on the Fijian view that makes this modest monograph particularly valuable.

In addition, I would like to commend the strategy on which this project was based. In the 1830s an American missionary, Sheldon Dibble, was teaching on Maui Island, Hawaii, at the Lahainaluna School, which had been set up to train Hawaiian teachers for missionary schools. Dissatisfied with the lack of teaching materials on Hawaiian culture and history, Dibble organized a seminar of his best and brightest pupils to do field research on Hawaiian traditions to develop the needed corpus of materials. This initiative stimulated a number of the members of that seminar, notably David Malo and Samuel Manaiakalani Kamakau, to go on to a lifetime of research and writing on Hawaiian traditions, the results of which now provide some of our best sources on Hawaiian culture and history. Although Petit-Skinner's initiative might seem to have come too late to have a comparable impact on indigenous Fijian scholarship, judging from the evident enthusiasm of both her colleagues and the elders they interviewed it may well be that her efforts will extend, through the continued research and writing of her Fijian colleagues, long past the short duration of the French Ethnological Mission.

BOOKS NOTED

RECENT PACIFIC ISLANDS PUBLICATIONS: SELECTED ACQUISITIONS, MARCH-MAY 1996

THIS LIST of significant new publications relating to the Pacific Islands was selected from new acquisitions lists received from Brigham Young University-Hawaii, University of Hawai'i at Mānoa, Bernice P. Bishop Museum,, University of Auckland, East-West Center, University of the South Pacific, National Library of Australia, and the Australian International Development Assistance Bureau's Centre for Pacific Development Training. 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.

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