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NETBAGS REVISITED: CULTURAL NARRATIVES FROM PAPUA NEW GUINEA

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In this article we reconsider materials on netbags from the Highlands of Papua New Guinea and Irian Jaya, as well as other parts of Melanesia, arguing that understanding of them is best set into a framework of ideas regarding embodiment and processes of trope expansion. Starting from the equation of netbag and womb, we trace the ramifications of this object of material culture and its contemporary significance in a commodifying context. Main emphasis is placed on materials from the Melpa or Mount Hagen area of the Western Highlands Province in Papua New Guinea, with comparisons to the cultures of the Ok region. Although netbags have predominantly female associations, modem Highlands netbag styles are a hybrid of elements from the netbags used by men and women in the past. We argue that the netbag's chief significance is as a container of life and death, deeply bound up with the human life cycle, and that it gains its significance from its use in both everyday utilitarian contexts and highly charged ritual events.

NETBAGS IN PAPUA NEW GUINEA carry a rich symbolic significance as markers of gender relations, the life cycle, and the cosmos. Maureen MacKenzie has made a pioneering analysis of their importance in the Ok culture area (1991), and here we extend the analysis to other parts of the Papua New Guinea Highlands, in particular Mount Hagen, drawing also on her wider ethnographic survey. Netbags are eminently worthy of study both for their role in life-cycle rituals and as the sites of individual creative activity in the context of contemporary changes in the social relations of production. As objects largely, but by no means exclusively, associated with the female sphere, they tend to play a muted role in the overall production of cultural

meanings by comparison with carvings, shell valuables, and pigs, for example. Yet their meanings are in fact central to overall cultural ideas of fertility and the cycle of life and death. It is this centrality that makes them apt also for further symbolic use as markers of gendered relations in the wider social and political spheres of life, as in male initiations among the Ok peoples.

In seeking to place netbags in their proper perspective, we have found it useful to use the ideas of embodiment and trope expansion (Csordas 1994; A. J. Strathern 1996; Wagner 1972). These concepts are suited to our purpose, because we argue that the netbag's primary significance is as a marker of the female womb, but this meaning is expanded in specific pathways to cover further social contexts, such as the attachment to land, the production of initiates, and the like. We suggest that the basic reason for this form of trope expansion is that the fact of birth from the female body and the immediate acts of caring associated with this fact form a powerful template for the expression of ideas about social transitions generally. The netbag becomes an elementary material form objectifying the powers of female fertility and making them accessible for further semiotic production.

This use of the netbag as a symbol corresponds to our decision to combine in our treatment two theoretical approaches that more often appear separately, that is, embodiment theory and trope-expansion theory. Embodiment in our usage here refers to the ways in which social and cultural values are encoded materially, either in the human body itself or in objects whose significance is related to the human body. The term also reminds us of the complex interrelationship between elements signaled as belonging to the realms of "mind" and "body," and the necessity to overcome the dichotomous representation of these categories in favor of a holistic and dynamic understanding of human experience. It also stands in a definite relationship to the concept of objectification, as Bourdieu has argued (1977): for example, a house that is divided into male and female gendered spaces can be seen as an objectification of the embodied relationships between persons. Put otherwise, an objectification is the product of trope expansion, and we could speak of the productive use of the netbag as a symbol in terms of either concept. Trope expansion is a relevant concept to use here, because beginning with the fundamental equation of netbag with womb we find that the netbag can stand as a container for life in wider contexts, for example, those of male initiation. Netbags, in other words, not only carry objects, they convey multiple sets of meanings that bear vividly on notions of the body, the skin, reproduction, nurturance, and female versus male social capacities (see also Turner 1980, 1994, 1995). They contain objects and an enormous potential for communication and self-expression. They are replete with social significance, including notions of growth, nurturance, ancestral transformations, generational

continuity, and land rights. These bags contain a record of human experiences and acquired meanings.

Netbags are also a functional component of dress and adornment that can be used to conceal or advertise certain facets of identity. They can be seen as convincing analogs for the regenerative and degenerative processes of life and as a great connector binding people to the ancestors of their past and the progeny of their future. They carry social and political significance in marriage bestowal and exchange contexts, they play a part in life-cycle celebrations and rituals of death, and they are used in bringing together kin groups and in the generation of political power through commitments of loyalty and obligation.

Netbags: The Overall Context

This section draws on the earlier work by Maureen MacKenzie (1991) and Annette Weiner (1976) in order to indicate the overall contexts of meaning that are generated from netbags and as a prelude to considering some specific matters in greater detail. Weiner has pointed to the significance of both netbags in general and specifically banana-leaf bundles and skirts among the Trobrianders as markers of female wealth and female powers of reproduction (A. Weiner 1976, 1992). Moreover, for the Trobriands, she argues that these objects stand for sociocosmic reproduction generally. MacKenzie has shown that netbags are important throughout Melanesia and often carry meanings having to do with female powers of reproduction and nurturance. Her materials also show the importance of netbags in the wider gendered cosmos.[1] Linguistic evidence supports this point.

Netbags are called ***bilum*** in Tok Pisin, or Melanesian pidgin. The word ***bilum*** also refers to natural objects including the marsupial pouch, the human placenta, and the afterbirth (Mihalic 1971:71-72). The lexical link between ***bilum,*** placenta, womb, and marsupial pouch is common to the vernaculars of many ***bilum***-producing cultures (Forge 1973: 180; Biersack 1984: 130; Bowden 1984:457). Cell notes that among the Umeda people the womb is called "spirit ***bilum" (uda midi)*** (1975:142-143); while Schieffelin reports that the Kaluli do not have a term for childbirth, and the term they use for "to give birth" means literally "to put (a child) in a netbag" (1977:124). In the North Wahgi Wall language, when a set of brothers offer a girl in marriage back to their mother's clan, the phrase for this action translates literally as "giving a skull in a netbag," and the "netbag" may also be used as a synonym for both "bride" and "womb" (O'Hanlon and Frankland 1986:181-198). The term in Melpa for giving birth, ***mei,*** means "to carry." The child is carried during gestation in the woman's bodily womb, and after birth it is carried in her net-

bag womb (cf. J. Weiner 1995:127 on the Foi). It is significant here that the woman manufactures the netbag as a cultural act and that many different kinds are produced, with both practical and ritual uses, as the next section discusses.

Manufacture: An Embodied Technique

There are many types of New Guinea netbags, but in general women carry large, open-looped work ***bilums*** from a tumpline (formed by tying the two long loose ends of the bag together) on the head, as well as small, more ornately decorated ones that are slung over the shoulder. Men wear small ***bilums*** either across the chest or slung over the shoulders, leaving their heads free. Among the Huli people of the Southern Highlands, whose men regularly carried netbags with handles knotted across their chests, the purpose of dealing with the netbag in this way was perhaps to ensure that the men could run, hold, and use weapons and look around for enemies in a way that women, loaded down and with their heads bowed, could not. But both types of bag can be worn and used by either sex.

Bilums are produced through a distinctive looping technique that differs from weaving or knotting in the looseness, flexibility, and elasticity of the product. They are employed to carry or contain diverse objects, including garden produce, firewood, arrows, betel nut, personal property, and infants: the latter use provides a hammocklike cradle. They are used as toys; Heider and Gardner report that children of the Grand Valley Dani in Irian Jaya played a game with their ***bilums,*** throwing one up into the air like a ball and shouting out "boy" as they caught it or "girl" if they missed (1974:68). They are pocket, quiver, attire, dancing regalia, ornament, amulet, trade commodity, wealth item, marker of identity, spirit catcher, and shrine. In addition to these multifarious uses, ***bilums*** are exchange items, elements of ceremonial clothing, bearers of ancestral power, and items of aesthetic value.

In the Telefol area there are many types of bags, ranging from large, expandable all-purpose ***bilums*** for women that are a basic item of domestic equipment to a variety of small "amulet" ***bilums*** constructed in a tighter fashion and containing charms or pieces of an ancestor's or warrior's bones. The Telefol distinguish twenty-seven named types and subtypes of ***bilums*** (MacKenzie 1991:46-50).

The technique of netting used to produce netbags is also employed to make headnets, aprons, cloaks, and fishing nets. Netted items are looped from a single thread rolled on the thigh, with new sections continually added as work proceeds. This process, which uses the thigh as a work station, invests each thread with hair, skin cells, body oils, and body odor from the manufac-

turer. Literally a part of the creator is incorporated into each netbag that she or he makes. In New Guinea, artifacts are often thought to be imbued with the personal "essence" and creativity of their makers, a fact that contributes to their meanings as gifts and is related to Mauss's concept of the "spirit of the gift" (Mauss [1925] 1990: 11). Although "skin cells" may not be a local notion, the idea of objects as embodiments of the substances and powers of persons is well known (see, for example, Gregory 1982). The entire spun thread is pulled through the body of the work during construction of each loop, resulting in a very strong bag in which a severed thread cannot unravel. The more tightly looped small ***bilums*** are worked with a bone or nowadays a steel needle.

The raw material for making ***bilums*** has traditionally been plant fibers (mostly ficus) or, more recently, imported wool or nylon or strands from unraveled garments, together with a pandanus or plastic strip. Along with the adoption of new materials in more recent times have come fresh multicolored designs.

Bilums are made individually rather than cooperatively, but women often work together at looping in casual groups and fit the preparation of string and the actual looping into any available time between other tasks. Children pick up basic techniques as they mature, acquiring first a basic competence in making standard ***bilums*** and subsequently, to a varying degree, an aptitude in more specialized looping techniques that enable them to make other kinds of bags and produce more aesthetically effective textures and patterns. Older women pass on their knowledge progressively as younger women acquire competence and responsibility. ***Bilum*** making is thus closely integrated with the flow of life and sociality. Among women the netbag is used informally to denote the passage through the life cycle. Almost universally, the looping and subsequent carrying of a large domestic ***bilum*** indicates that a young girl is accepting the responsibilities of being a productive adult member of the community.

Netbags, Creativity, and Cultural Efflorescence

Among the Daribi, the decorative motifs (stylized geometrical symbols) and the talent and skill involved in making netbags are thought to be acquired through dreams (Wagner 1972:77). The metaphorical character of the geometric patterns used in Daribi netbags is closely linked to the structural codification of dreams themselves. This attribution of acquisition of creative knowledge through dreaming is similar to that seen among some Australian Aborigines, for example, the Pintupi (Myers 1986:242). A Daribi who saw a poorly crafted product would conclude that the maker had not had a dream

providing inspiration to produce a well-made, functional, and attractive product. The knowledge that is accrued through dreaming comes from ***izara-we,*** spirit women living under the surface of the earth. These women live without men and have skins that are said to be slippery, coated with a whitish substance like saliva from which an unpleasant odor emanates. Through the interpretation of dreams, the Daribi bring the capacities revealed in dreaming to bear on the events of day-to-day life; the knowledge acquired through encounters with ***izara-we*** in dreams is creatively depicted through netbag design and decoration (Wagner 1972:72-77).

For New Guinea as a whole, netbags have undergone various alterations. Their role has been extended (for example, providing a source of income in the tourist market), new materials and designs have been incorporated, and their production has expanded into regions of New Guinea where they had not been made before.

In the contemporary context netbags also are used to make statements about personal desirability and worth, particularly by young women. In the highly colored and decorative netbags they make, young women stress beauty rather than utility, by contrast with the work netbags used for carrying foods. The decorative netbags are also often carried with shoulder straps rather than suspended in traditional fashion on the head. If they are worn from the head, in Mount Hagen they may have multiple pastel-colored lightweight scarves attached to them that gracefully fall to one side of the woman's face or flutter in the wind as she moves. Such scarves are "modem" and sold for wearing around the neck, but Hagen women have attached them to netbags in a blend of old and new forms of adornment.

Women in Mount Hagen nowadays also use artistic forms from the past in innovative ways through their selection of brilliant fabrics to be used as source material in their netbags. The use of these bright fibers, which are woven into flamboyant crisscrossing geometric shapes, is reminiscent of the explosive body coloring done by young girls and women before missionization condemned the practice as ungodly. The bright yellows, reds, and blues that women choose as the fibers for their netbag designs are precisely those colors that were predominantly "female" in body painting as it was practiced in the recent past. The base color for the female face was in fact red, signifying friendship, vitality, and female ties of marital alliance, while the base color for men was black, signifying male solidarity, aggression, and the power of the ancestors (A. J. and A. M. Strathern 1971:152-170). The transposition of these colors painted on the skin to the woven netbags also shows the tropic association of the netbag with the skin itself, as a covering that closely molds itself to the contours of the body. Previously, netbags were colored in the taupe and brown colors of the natural fibers and the dark blue of the dyed

fibers, When colored fabrics became readily available to Mount Hagen women and the practice of body painting as a form of making oneself more sexually desirable was criticized, women devised new ways to express their sexuality and pride that the Christian church did not condemn. These brightly colored, highly ornate bags are also appealing to tourists, who purchase them from women who come to airports to sell them. They have thus been transformed from gifts into commodities (Gregory 1982; Carrier 1995). The smallish, squarish bags, which may be slung over the shoulder or around the neck, are used by both sexes to carry personal possessions, while other kinds of bags, worn by men under the arm, serve as portable caches for war or love magic.

The names of the designs netted into the modern netbags capture something of the changing experience of Highlanders of New Guinea. Some designs, such as "Cross" and "Christmas" are influenced by the missionary presence; others, such as "One Ace" and "Diamond," reflect the popularity of card playing. Another, "Yonki Pawa," derives from technological change, Yonki being the site of a major new power-generating station in the Eastern Highlands, whose pylons are modeled in this netbag's design. "Skin Pik" suggests the roadside markets at which many of these new designs are worn (it is named after its resemblance to the lattice of squares into which pork is divided for sale at markets) (O'Hanlon 1993:71-72).

A prominent feature in the development of netbag manufacture and design in the New Guinea Highlands, then, has been the elaboration of designs for colored, tightly netted bags made by women. Several factors have combined to produce what can appropriately be called a cultural efflorescence in this regard. First, women are prime netbag makers, and this role has not been taken over by men or simulated by factory production (so far). Second, the movements of people between the coastal regions and the Highlands, and within the Highlands region itself, have brought knowledge of types of designs not previously available. Women have themselves traveled and observed designs, or they have made friends with people and exchanged netbags with them, or they have seen the designs brought in by others, in any case becoming able to expand their repertoires of knowledge. Third, women have found a way to use secondhand and discarded clothing bought from trade stores to supplement their access to different colors of thread, since they previously could employ only dark blue plant dyes and bark-thread, plus insets of marsupial fur wound into the surface of the thread itself. Such pieces of clothing are bought with money that women derive from their sale of vegetables in local markets or from their shares in money obtained from coffee sales. Fourth, women have been able to sell the new types of netbag to tourists as well as exchanging them among them-

selves in contexts of kinship and affinity. Fifth, and finally, women have adapted the style of netbag making that was previously used to manufacture bags for men to this wider context of production for their own use, for exchange among themselves, for gifts to men or boys, and for commercial sale. This widened context of production therefore represents an arena in which cultural innovation and social changes have met to stimulate a new cultural form. In this case the new form also represents a widening of the spheres of interest, experience, and capacity of individual women. The netbags a woman makes thus reflect her own individuated biographical experience and knowledge: places she has traveled to, contacts she has made, her capacity to earn money, and her own aesthetic choices in making bags with designs derived from various places. Simbu designs, for example, are borrowed and imitated by Hagen women, and it is likely that the reverse occurs also.

Netbags as Gendered Attire

In addition to netbags, men, women, girls, and boys wear caps, crocheted according to the traditional technique of netbag making. These items are made with the same sorts of materials and incorporate similar sorts of design elements. Men's aprons are made in the same way. In Mount Hagen the long, multiple-layered aprons ***(mbal)*** that men wear for ceremonial dances are all made by women with the same looping techniques as are employed for netbags. Women make these aprons for their male kinsfolk or spouses. Nowadays they may also make them for sale (at prices from US$100 upwards, twice the normal cost of a netbag of the new style). While netbags can be manufactured with various types of modem materials, such as plastic, and colored in almost every imaginable hue, aprons must be made only from traditional materials and colored with dark blue dye. A man tries to find two aprons to wear for a dance, in order to give his frontal appearance weight and to cover his legs and genitals comfortably. The apron is very much on display in the dancing line, as the dancers face outward to the spectators. They must learn to move the aprons gracefully in a swinging motion forwards and backwards, in time with their singing and the genuflection that accompanies both singing and drum beats in the prime dance for the ***moka,*** the ***mör*** (or ***mörl)*** dance. Often the woman who made a man's apron is known to the spectators, and they will comment on the level of her competence and skill in their evaluations of the man's decorations. The apron is thus a part of its maker's biography as well as of its wearer's standing.

Bilums are a standard element of female attire, and finely processed ***bilums*** exhibit a woman's productive capacities as well as often contain the results of her work and fecundity. This dual role is reflected in the Mount

Hagen and Wahgi areas by different terms for work and ceremonial women's netbags. The former are ***wal omb*** in Hagen (referring to the practice of tying the two free ends together in a knot for carrying the bag on the head), and ***wal kupin*** is the term for the decorative ceremonial netbags used in bridewealth. In Wahgi these terms are ***kon mengel*** for the work bag and ***kon kupn*** for the ceremonial bag (O'Hanlon 1993:69).

A properly manufactured ***bilum*** demonstrates the productive capabilities of a woman and serves as an indication of the care, energy, and love that she is likely to invest in all of her various activities as a wife and mother. The fact that the ***bilum*** is frequently used as a cradle makes its identification with the woman's body, and specifically her womb, unsurprising; its capacity to expand and its bulging position on her back make for obvious visual affinities between the full ***bilum*** and the pregnant body. ***Bilums*** can also project the desire for aesthetic expression by enhancing the form of the body. Females often wear brightly colored netbags that fall gracefully down their backs from an attachment point on the wearer's head, adorning their person. MacKenzie heads a section on the aesthetic value of netbags with the saying, "The bilums are our ***bilas*** [adornments]" (1991:133).

Such is the overall importance of netbags and their relationship to gender and personhood in everyday contexts. But they enter also into the realms of mythology, ritual, and the origin stories relating to major institutions.

Shells in a Netbag: Sexuality, Reproduction, and Politics

An examination of the uses to which netbags are put can illuminate women's contribution to social and political organization. Examples from the Mount Hagen area will exemplify this point.

Netbags appear regularly in passing in the collection of narratives from Mount Hagen published by Georg Vicedom (vol. 3) as well as in the other two volumes of ethnography that he produced along with Herbert Tischner (Vicedom and Tischner 1943-1948). In the first volume of this trilogy, the volume on material culture, the authors write about a myth as follows.

> The pearl shell acquires a marked religious and cultic significance through its use in the Female Spirit cult and in the closing scenes of a ***moka*** exchange ceremony. . . . According to a myth these valuables were at one time stolen by bad people of the Underworld. A brave woman, along with a few men, risked going down to the Underworld and brought the valuables back. This act is represented at the end of the ***moka*** festival. In order to display this return of the shells in a truly dramatic manner, they were first hidden from view

> in large, carefully prepared grass-filled netbags, with their shiny sides exposed. At the ceremony men with distinctive decorations carried the shells into the dancing place with many shouts and yells, and were received there with great jubilation. Later the shells were handed to the recipients of the ***moka.*** (Vicedom and Tischner, 1: 120; translated from the German)

The myth to which the authors refer here is a little more complicated than their own summary of it. The "brave woman" who went down to the Underworld was in fact the new wife of a polygynous man, and it was a pale-skinned cannibal woman who stole not the man's pearl shells, but his two sons Eklimb and Kuklup. The new wife was called Kopona Nde (perhaps derived from the term for ***kopong nde, Campnosperma*** sp. oil-bamboos used in decoration), and she made her way into the Underworld through a rock face and an underground lake. Kopona Nde burned the cannibal woman inside her house and rescued the sons, as well as other men kept captive there, and "took all her possessions, valuables, cassowaries and pigs and went home." After this the two sons made the ***moka*** shell festival with the shells brought back in this way (Vicedom and Tischner 1943-1948, 3:33- 39).

The myth gives a leading role to a woman, whose exploit is commemorated by hanging the shells in a netbag when they are brought into the dancing ground. The predominantly "female" associations of the netbag are thus conjoined with the "male" associations of pearl shells as forms of wealth into a composite symbol. In substituting pearl shells for the two sons in his summary of this myth, Vicedom was perhaps unconsciously recognizing the equivalence or substitutability of wealth and people that is a feature of the Hagen culture.

The netbag is a container that can contain a newborn baby (nestling in leaves within the bag) (Vicedom and Tischner 1943-1948, 2:241), the bones of a dead person, or items of wealth that stand for the person (pearl shells in this case). Carrying persons or objects in a netbag signifies their ***translocation*** from one place or one state of being to another. The pearl shells are first hidden, then brought into view in a revelatory sequence that is like the sequence of birth itself.

The female netbag as a container that may conceal and nurture life is exemplified in another Hagen myth, in which a human man discovers and captures a young woman of the Sky People (Vicedom and Tischner 1943-1948, 3:15-17). She then takes him and puts him into her netbag and climbs up a tree with him to take him to her home in the sky. Her kinsfolk say he cannot stay and she must go and live in virilocal marriage with him on earth. They descend to his place to receive the bridewealth payment for her, but

this time a piglet squeals in an old woman's netbag. They are startled (for they do not keep pigs, only marsupials) and return to the sky, taking their young woman with them. Since then there has been no traffic between the Sky People and the earth people.

The silent, mature man in the young woman's netbag being taken up to the Sky People's domain contrasts with the noisy, immature piglet in the old woman's netbag. Both netbags are containers of life, but they act as different operators, one as a promise of a heavenly alliance, the other as its destruction through the earthbound life in it. The netbags, then, play an important structural role in the overall story.

If netbags carry predominant associations as containers of life, their significance in gendered terms varies across the Highlands region. In Hagen, women's netbags ***(wal omb*** and ***wal kupin,*** "work" and "ceremonial" versions) are distinguished clearly from ***wal kumbana,*** which traditionally were carried only by men and had shoulder-strap handles. There is, however, no great symbolic elaboration of the ***wal kumbana*** category, whereas the female types of netbag are replete with associations of life support and nurturing.

Even the actions of making a netbag are associated with meanings that have to do with sexuality, reproduction, and in metaphorical form with the domain of politics. The action of pulling the long thread through each netted loop is described as ***walinga rui,*** "to strike backwards and forwards," the same term that is used for the sinuous inflection of the head by partners in the ***amb kenan,*** or courting ceremony for unmarried girls. In this ceremony the male sits cross-legged beside the female, who sits with her legs bent underneath her. The foreheads and noses of the two partners come together and roll rhythmically back and forth as their bodies sway, each sequence ending with a double movement of ducking the head down to the floor in a manner that mirrors the movements of sexual intercourse, although sexual intercourse itself should not take place during the courting. During this action the partners listen to courting songs sung by the spectators, which provide a beat to which their actions can be attuned. The girls are supposed to introduce love-magic substances into the oils and paints that they apply to their foreheads for the ceremony.

The needle employed for pulling the thread is traditionally made of flying-fox bone and is called ***ngal aipa,*** "flying-fox bone needle." The thread is called ***kan röngi,*** "manufactured rope." In a phrase used to express political alliance between male exchange partners, the sexual image of the union between thread and needle is transposed metaphorically into an image of the exchange bond facilitating a network: ***nim kan röngi, na ngal aipa,*** "you will be the thread, I will be the needle," and together we'll make the netbag, that is, the ***moka*** exchange.

This combination of sexual and political meanings can be seen in the context of widow's wear also, since the widows of war victims were supposed to wear piled-up netted materials over their heads to provide a house for the dead man's spirit until it was revenged by his clansmen. The widow's role here is both domestic and political, and the netbag again expresses both aspects, just as the netbag used for shells in ***moka*** did (Vicedom and Tischner 1943-1948, 2:225). The netbag as a home for the newly born spirit mirrors its role as the container for a newborn baby and also the term for the placenta, ***kunung wal,*** the "covering netbag," in the Melpa or Hagen language, a term that also applies to the netbag worn as head and back cover for protection against rain and excessive exposure to sunlight.

The netbags of bereavement in Hagen reflect the obligatory sentiment of mourning for the dead and perhaps also the widow's own feelings during the mourning period, when the spirit of the dead husband sits on her. The 'bags are not to protect her from the dead, man's spirit, but serve as a domain for the spirit to live in until the time when it can go to the spirit world to live. Approximately six months or a year after the death of her husband, a widow is traditionally freed from mourning if proper revenge for his death has been exacted (M. Strathern 1972:62).

In other parts of the Highlands the gendered dichotomy between types of netbags used by men and women found in Hagen does not play out in the same way. For example, among the Huli of the Southern Highlands Province, men also carried netbags like the Hagen ***wal omb,*** in order to transport their own supplies of sweet potatoes, kept "pure" from female menstrual contamination (Frankel 1986). And in the Ok region farther to the west, a rich set of male and female associations for netbags include their role in signifying male initiation stages. Here again the netbag acts as a "container of life," modeled on the womb, and is appropriately associated with the "rebirth" of boys and men into new initiation grades (see also below). Netbags can thus stand for gendered values in different configurations in these Highlands societies. All of the examples given here, however, illustrate our argument regarding trope expansion. Images and idioms having to do with birth, nurturance, and sexuality are transposed into wider social and political realms, and the netbag acquires meanings that belong to the domains of exchange and politics. The same point is illustrated in further materials, from Oro Province and elsewhere, relating to contexts of birth, death, and initiation.

Netbags and Life Cycle: Birth to Death

In the Orokaiva area of Oro Province, women carry their infants into the garden daily, putting the child in a netbag that is hung from a stick placed

firmly in the ground. As the child rests in this cradle, the ancestral spirit of the previous landowner enters into the child through the stick. The transmission of ancestral substances makes the child grow and imposes an obligation on the child to care for this particular plot of land in adulthood (Schwimmer 1973:92-95). Schwimmer does not specify in his ethnography whether both male and female infants are treated in this way. In any case, what is demonstrated here is the involvement of women in a process whereby land rights are transmitted and ancestral and generational continuity is maintained, and in which the netbag plays the crucial role of transducer between the ancestral owner and the present-day generation. The ancestral owner's power enters the child in the netbag in a manner analogous to the entry of the procreative power of the genitor into the mother's womb. In this analogical transposition the symbolic equation of womb and netbag enables the trope expansion to be effected.

Netbags may also contain the dead or parts of the dead, or conceal the bodies of bereaved persons. For example, in the Gulf and Western Provinces ***bilums*** are used by women in mourning to conceal the torso and veil the face (MacKenzie 1991:16-17; Landtmann 1933). Throughout the Gulf Province, and also in the Western Province, much of the Central Province, and parts of the West Sepik Province, a widow further wears her dead husbands personal possessions in or tied to a small amulet string bag around her neck. In the Gulf Province this sort of ***bilum*** is ceremonially burnt at the end of the period of intense mourning to ceremonially sever the widow's connection with her dead husbands spirit (Maori Kiki and Beier 1969:19; MacKenzie 1991:17).

In the Telefomin area of the Ok region, where there is a special elaboration of ideas and practices concerning netbags, the netbag itself is associated with Afek, the female creator figure. In one version of the myth, Afek "created all important cultural items from her vaginal secretions and menstrual blood" (MacKenzie 1991:45). These items would include the netbag itself, which is like an externalization of Afek's own womb. Telefomin women questioned by MacKenzie would only whisper the name of Afek and say they had always had netbags, passed down from mother to daughter since the origin times. One of the uses to which Afek put her own netbag was to wrap the bones of her husband/younger brother Umoim in preparation for his funeral. The netbag here appears again as the container of the body at all stages of the life cycle, in which death at least partly recapitulates birth (MacKenzie 1991:44-45). The type of netbag Afek prepared in this way is called ***olkupmen,*** "feces rotten taro ***bilum,"*** described as "an extremely sacred esoteric" netbag (MacKenzie 1991:223).

In the Ok region as a whole, men as well as women make and wear

bilums. Bird-feather ***bilums*** for initiation ceremonies are made by men in their roles as husbands and fathers, on the basis of a ***bilum*** made by the initiate's mother. Usually a man presents his wife with prepared fibers, from which she loops a ***bilum.*** The man then decorates this ***bilum*** with feathers (hornbill, eagle, or brush turkey, for example). These ritually important ***bilums*** are worn by their sons among the initiated men and connote maleness. Among the Telefol the ***uun kon men*** (bird-feather ***bilum)*** is made specifically for the particular male initiation ceremony with which it is associated as part of a rite of passage (MacKenzie 1991:113).

The ***bilum*** is used to mark male ritual status in many areas in East and West Sepik. In the Wewak Boiken area, young male initiates are given a special netbag by their maternal uncles that contains all the prerequisites of manhood in the form of powerful ingredients to make the boy strong, wise, and sexually potent (Aufenanger 1972:22). For the Mountain Ok people the netbag is a major focus of male initiation rites. The boys' first initiation ***bilum*** defines their newly acquired status and is a restatement of the existing social hierarchy among men.

Netbags and the Spirit World

Ritual and spiritual practices involving the use of netbags can be found throughout Papua New Guinea. The embodiment of ritual potency representing cultural knowledge, can be passed on through the medium of netbags. In the East Sepik Province, the Arambak provide their ***kamanggabi*** figures (carvings of ancestors) with their own netbags, which contain powerful substances associated with the fertility of crops and other knowledge of cultural resources (Forge 1960:7). Throughout the Star Mountains in central New Guinea, sacred netbags representing cultural knowledge hang on the back wall of men's houses (Zöllner 1988:130-134). Among the Mekeo a man of many bags appears to be a man with much ritual knowledge, and the passing on of this knowledge is to be preceded by the comment "I will show you my bag" (von Goethem 1912:795). Myths from many regions tell of spirit women who possess netbags (Biersack 1982:244; Brumbaugh 1980: 440). Witches may also capture people's souls in netbags and carry them off for consumption at cannibalistic feasts.

Among the Anga-speaking Baruya of the Eastern Highlands of Papua New Guinea, the initiation of new shamans contains a sequence involving netbags. The postulants place objects containing magical and spiritual powers they have found into their individual netbags, and these are then hung up overnight outside of the initiation house for spirits to enter and imbue with the requisite powers. A master shaman takes these bags down the next mom-

ing and inspects them to see signs of the spirits having entered them, in which case the initiate is permitted to advance in the ritual. The netbag here plays its familiar role as the container of spirit / life / power. (This sequence appeared in film footage by Jean-Luc Lory shown at a Wenner-Gren-sponsored conference on the Anga-speaking people held in Marseille, France, 20 September 1996. See Godelier 1986:112-126, where, however, this particular sequence is not alluded to.)

Heider tells how Dani men, in times of spiritual crisis, would conceal themselves from ghosts by draping a woman's netbag over their heads (1969: 382, 385). Small amulet ***bilums*** containing charms such as sacred stones or fragments of deceased ancestors' bones are common to almost every ***bilum***-making culture in the Highlands region (MacKenzie 1991:18). They are worn around the neck to strengthen the soul and conceal the upper sternum from the gaze of ghosts (Heider 1970:247). Depending on their secret contents and particular cultural context, small amulet ***bilums*** may be used as personal magic charms (Maori Kiki 1975). Among the Telefol, the ***men amem*** (secret, sacred ***bilums)*** contain individual ***usong*** (ancestor spirits) traditionally believed to be a source of spiritual and economic aid for the living. These bags are created by ritual specialists within the context of particular mortuary rituals.

Glasse records that the Huli of the Southern Highlands Province used string bags for divining (1965:39). Certain women had the power to attract the ghost of a recently dead relative into a tiny netbag, which, when animated by the ghost's presence, could answer questions put to it by the diviner owner. If the bag swayed to the right, an affirmative response was indicated; if the bag swayed to the left, a negative reply was implied.

Netbags in Exchange: The Mesh of Sociality

As a netbag is used to mediate between this world and the spirit world, so is it used, through exchanges within and between groups, to mediate between woman and man, parent and offspring, cognate and affine, and initiator and initiate.

The gift of a netbag may establish a relationship independently of kinship. In the Washkuk hills north of Ambunti, if a woman's son dies, she will loop a ***bilum,*** which is subsequently decorated with cowrie shell valuables by her husband. This elaborate ***bilum*** is offered to the best friend of their deceased son. The friends acceptance of such a ***bilum*** establishes a new surrogate child relationship with the bereaved parents (MacKenzie 1991: 12).

In all the ***bilum***-producing cultures of Papua New Guinea, women may signify the closeness of a particular friendship by informally exchanging net-

bags. Among the Foi of the Southern Highlands, this type of exchange, which expresses solidarity between women, is given a ritual formality. When a young bride transfers to her husbands longhouse, the coresident women of the wife's village accompany her. Each woman carries a netbag, or other object such as a sago-washing basket, containing female domestic items and apparel. The bride's mother receives these gifts and notes the identity of each of the donors. These gifts form the bride's dowry and express continuing support to the former coresident (MacKenzie 1991:12; J. Weiner 1988:98; 1995:138). The groom's father in return provides cowries (thirty-seven, probably representing the complete human body on a body-count system), and each woman who has brought a gift receives one (that is; male wealth for female wealth). The bride herself sits concealed in a new bark cloak beside her mother (awaiting her "rebirth").

When an Orokolo bride is led to her husbands house, she carries a special netbag that contains her coconut spoons, betel nut, and lime with which she will entertain in her new house. This ***avoa*** (conjugal ***bilum)*** is decorated by her parents with rows of shell valuables: the top row she will keep for her personal decorations; the rest she will distribute to her new affines (Maori Kiki and Beier 1969: 18).

In the Hagen area a bride came to her husbands kin with a dowry that included netbags. These bags were made specially for the marriage by father's and mother's sisters, her mother, and other wives who were married into her own lineage. All of these women would hope to receive a portion of the bridewealth in recognition of this support. The bride would distribute these netbags to her new sisters-in-law and her husbands married and unmarried sisters and his brothers (M. Strathern 1972:14-15). The giving of these gifts signals the friendly relations she hopes to establish with these women. In addition to these ordinary gift bags, the new wife would also have a specially fine one ***(wal kupin),*** which would have long tying strings. This bag she would have been using on special occasions since she was of marriageable age. She might give this bag to her husbands younger sister, who would wear it until she was married. Or the new bride might follow the custom of cutting the bag up and using part of the material as a bag, while converting the tying strings into a pubic apron for her husband (or sometimes her father-in-law) (M. Strathern 1972:14-15). In this example, as in those given previously and those that follow, by following the path or life cycle of a netbag, the relations and meanings that surround it can be explored. These objects have a life cycle of their own that weaves in and out of various situations and circumstances. Transformations in the form, function, and meaning of a netbag occur as it passes from one social context to another (cf. Kopytoff 1986 on the "cultural biography of things").

In the Goroka area during the betrothal ceremony, a netbag ***(owu,*** meaning womb) is symbolically displayed on a wooden post, the netbag being the primary symbol of the bride's womanhood. As the bride stands next to the post with its attached netbag, the girl's male relatives make speeches about her future marriage and the bridewealth they will receive. The bride is accompanied in this ceremony by one or two of her female relatives, who carry netbags filled with pork to be given as gifts to the groom's relatives. In addition, the bride's mother may also be given a netbag filled with pork or sweet potatoes (Sexton 1986).

The gendered symbolic significance attached to women's netbags is exemplified by the mortuary ceremonies of the Dugum Dani. On these occasions the women first exchange netbags among themselves. In later stages of the funeral rites, women bring their carrying netbags, which are spread out in the courtyard, and men's valuables for exchange (stones and shell bands) are placed on top of the rows of netbags (Heider 1970:161).

Woman, Netbag, Cassowary: Cosignifying Symbols

The association between the netbag, human skin, and the cassowary can be seen in several examples. The cassowary is associated with principles of femininity as well as masculinity. Gardner suggests that the cassowary is cosmologically significant to the Miyanmin, because it is associated with the procreative powers of women and because it is an embodiment of "essential characteristics of both men and women" (1984:141). The creator figure Afek is represented as the cassowary in one of her forms (Gardner 1981:201; Morren 1974:163). She is known as Fiitiir, "the cassowary," among the Miyanmin.

The ***bilum*** is often draped over the backs of women, where it serves as a raincoat and sunscreen. A visual similarity exists between a woman wearing a ***bilum*** in this manner and a cassowary. The ***bilum*** here is functioning as a skin of sorts. A story from the Sepik region, epitomized in Bernard Narokobi's play ***Death of a Muruk*** (1982), tells of a man who spies on cassowary-women while they are bathing. These women had removed their outer skins, which were cassowary skins. The man hid one of the women's skin to prevent her from turning back into a cassowary. The woman marries the man, but one day, after he has insulted her by stating that she is not a "real woman," she finds the old cassowary skin where it had been hidden, puts it on, and returns to her life in the forest as a cassowary (A. J. Strathern 1996: 93). This skin can be seen here as a marker of the. interface between what is hidden or inside and what is revealed or outside. This duality of skin is seen in the fact that the cassowary woman has two skins, one representing her

animal, the other her human self (cf. Turner 1980). Bark cloaks and netbags also function in this way.

One of the categories of Telefol ***bilums*** is the ***tiyaap men*** (cassowary ***bilum),*** which is a strongly made, squarish bag with an over-the-shoulder strap that has been decorated on the outer-facing side with cassowary plumes that form an elongated "tail" in the center. The Telefol refer to this "tail" part of their cassowary ***bilum*** as ***unam*** (women's grass skirt). This ***bilum*** can be owned and carried only by fully initiated men.

Netbags: Telefomin Reversals of Sexual Oppositions

In the myth of Afek, the mother of the Ok peoples, an explanation of the Telefol world and an account of its origins is given. A version of this myth is revealed to male initiates as they proceed through the stages of male initiation from boyhood to manhood. The ***bilums*** that the initiates wear mark their progression through their education in the male cult (MacKenzie 1991:224). A version of the myth follows: Long ago the couple Afek, the elder sister, and Umoim, the younger brother, had built Telefolip (the sacred cult-house). Umoim stayed in the ***unangam*** (family house, literally, "woman house") and Afek stayed in the Telefolip (the ***yolam).*** They stayed like this, but at night the pigs and the children in the ***unangam*** didn't sleep--they squealed and cried out. All the time it was like this, every night. Afek thought to herself, "Bah! This is no good. Things can't go on like this." Then she decided that she and Umoim should exchange places--she would sleep in the ***unangam*** and Umoim would sleep in the Telefolip. She decided this and then told Umoim about it. She told him she had left her feathers and decorations in the ***yolam*** for him. When the sun was about to come up, she explained, he should get up and go outside and wash himself with the dew she called "stars," then rub pig grease all over his body, paint himself, and put on her decorations. Afterwards Umoim was to return to the ***yolam*** and stand in the doorway in the light of daybreak so she might look at him. That night Afek slept in the ***unangam*** with the children and the pigs while Umoim slept in the ***yolam.*** For once all had a good night's sleep--the pigs and the children did not stir and all was quiet, and Afek slept soundly too. In the morning Umoim did as he had been instructed and made himself beautiful, decorated in paint and feathers with a boar's tusk in his nasal septum. Afek took one look at him and was delighted. Clapping her hands together she said, "That's it, that's more like it. It's really good." And so she decided that from then on men would have the ***yolam,*** and women would stay in the ***unangam*** with the pigs and the children (Jorgensen 1981:403). This cosmological myth details how Afek exchanges roles with her brother, reversing

the sexual roles. In the manufacture of netbags by male initiates, the basic bag is constructed by women and the feathers / decorative elements are incorporated onto the bag by males, a process that appears to mirror the basic creative role of Afek and the way in which she conferred decorations and ownership of the ***yolam*** on her younger brother / husband Umoim. The origin of Telefol society is represented in terms of a reversal of roles between Afek (female) and Umoim (male), which converts an unsatisfactory state of affairs into a satisfactory one or a state of incompletion to one of completeness, a theme that also appears in Melpa myths regarding social origins.

Netbags: A Melpa Symbolic Continuum and Cosmology

The symbolic continuum of womb to netbag is prefigured in Melpa images used to describe conception and also in mythical representations of the origins of sexual activity and reproduction. The basic ideas involved are those of actor / acted upon and container / contained, which can be described as having an analogous relationship. Interestingly, the gendered markings that go with these paired terms shift according to context. At the level of myth there is an idea that male and female coexisted, but the female was not cornpletely sexually differentiated, that is, she had no opening into a vaginal cavity. The male had observed that the female rubbed herself against a banana tree, and by a ruse he caused the female (or females) to cut herself open against fragments of shell and stone axe blades that he had inserted into the tree. The man then gave cooked meat to her. (The sexual imagery is twofold here: (1) the opening of the closed female is analogous to the breaking of the hymen during sexual intercourse, and (2) the giving of meat by a man to an unrelated female is often a metaphor for sexual intercourse.) Here, the male acts upon an unopened female to open her and thus make her into a potential container. Before being opened and reproductively complete, the female was not socially complete, being unable to perform her role as social female (Vicedom and Tischner 1943-1948, 3:25-28).

In another story an incomplete male (Pim) is featured: he has no mouth or eyes and eats through a hole in the top of his head (cf. J. Weiner 1995:135 for a similar Foi motif). The lack of a mouth and eyes symbolizes a lack of morality and sociality. This male is seen as a wild man who owns pigs but does not understand how to exchange his pigs properly with others and deserves to be treated differently from proper social males, even deserving to be stolen from.

The story of the man Pim is as follows: A young man living with his mother was the owner of a boar. One day he told her to look after it while he went out to the riverside. He found a garden beside the river and took some

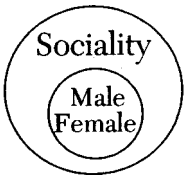
food from it. Then he found a pig's entrails on the path. He kept finding more entrails, and he washed them in the river to take home with him. He saw a man's footprints and came to an offering place, where the man Pim sat killing pigs. Pim cut up the meat with a bamboo knife and put the pieces in the top of his head--he had no eyes or mouth. As Pim prepared his meal, the young man quickly stole some meat and made off. His mother was amazed and on hearing his story praised him for a good young man. Pim himself was furious and made a pit for the thief to fall in, disguising the top with leaves. The next time, the young man fell in it. Pim had laid a netbag in the bottom, and he simply gathered it up and strung it between two trees with the culprit inside, telling him he could stay there and die. A flying fox came by and saw the prisoner. It flew back to the young man's home and began to eat ripe bananas in a garden where it heard the mother crying out for her son. It took bananas and fed the young man, rather to his surprise. The young man promised the flying fox his boar if it would free him, so the flying fox gathered up all the animals and birds to help free the young man. They took hold of the two handles of the netbag with their beaks and paws and gently conveyed him to the ground. His mother greeted him tearfully and fed him until he was fit again. The youth then went and set a hole trap for Pim with two sharpened spear-points. Pim fell in and was killed. The youth took his possessions and food, and divided them up among the animals and birds (Vicedom and Tischner 1943-1948, 3:143-145).

These two stories describe incomplete female and male forms and what is needed to transform them into complete social beings. For the female,, reproductive abilities and for the male, knowledge of appropriate exchange procedures and moral interactions with others are required. In addition, they show how sexuality and reproduction entered the world. The same kind of logic regarding the necessity for the sexes to be in a complementary relationship continues in the formal conception theory of the Melpa (see Figure 1). Here the male element (semen) is said to surround the female element (blood) to make a packet ***(kum ronom)*** or an egg ***(köi mukl)*** that later becomes a fetus. In this image the semen encompasses, constrains, or gives shape to the blood: the male element acts upon the female and contains it (although all this takes place within the female womb). In both the story of the opening of the female and this imagery of the semen encircling the blood, then, male agency is foregrounded. However, once the "packet" is complete, it is nurtured by female agency. The womb surrounds the fetus, feeds it, and makes it grow. Once the child is born, it is placed into the netbag, which is the equivalent of the womb--its material, extrasomatic "embodiment," and is taken care of by the mother.

The womb/fetus, netbag/child nexus provides the possibility for further

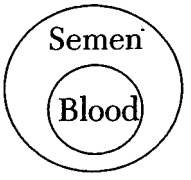
1. ***Mythical Plane***

Male:	***Female:***
not completely morally differentiated	not completely sexually differentiated



The female is opened after being tricked by the male into rubbing herself against a banana tree where the male has inserted sharp bits of shells and axe blades. Each is completed, however, by the complementary power of the other, and together they create the social world, depicted as the realm of "sociality" in which male and female generate their completeness.

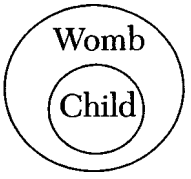
2. ***Conception Theory***



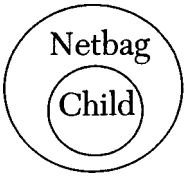
The male element (semen) surrounds the female element (blood) to form a packet ***(kum ronom,*** literally, "it wraps") that will eventually become a child if nurtured in the womb.

3. ***Nurturing Theory***

a. The female element (womb) surrounds the child to form its nurturing environment. It is at this point, during gestation, that the ***min*** (spirit/soul) enters into the developing fetus.



b. The netbag, a female element, surrounds the child after birth to form its nurturing, protective environment.



4. ***Embodiment and Objectification***

The netbag (objectified form of womb), a female element, surrounds the pearl shell (objectified form of person), an element that is ideologically male wealth (see "Shells in a Netbag" above).

FIGURE 1. The netbag as a container of life and the relationship of the netbag to the womb: a Melpa symbolic continuum and cosmology.

trope expansions, such as we find in the idea of pearl shells in netbags, where the shell is seen as a male valuable but also as equivalent to a (male) child. Netbags as containers gain their symbolic power in the cosmos from their contents as well as their power to contain. In the image of the netbag/shell relationship, we see the progression of a process of objectifications, from womb to netbag and from body to shell, corresponding to Pierre Bourdieu's delineation of the movement from embodiment to objectification (1977:87-95). In the netbag/shell image we also see the structural reversal of the semen/blood relationship, since in the latter male encompasses female, but in the former female encompasses male. Such reversals indicate that we are dealing with ideas of balance and pairing in symbolic thought rather than with one-way relationships, and the netbag plays a crucial role in the transformation sequence male/female to female/male. The netbag is also a necessary element in the movement from the incomplete to the complete, which both myth and practice inscribe.

Conclusion

We have been exploring some of the uses and meanings of netbags within several Papua New Guinea cultures, placing a particular emphasis on the Melpa speakers of the Mount Hagen area.[2] Through the exercise of examining the individually and socially expressive use of netbags as detailed in the ethnographic record, we have observed that netbags and their uses are frequently described in passing, but rarely is their cultural importance discussed at any length (MacKenzie 1991 is the obvious exception). So undervalued is the netbag that many ethnographic monographs do not even list the term "netbag" or its equivalent in their indexes. This lack of attention may have occurred because the individuals within the society did not highlight these objects in discussions with ethnographers. Also, the process of textualizing cultural themes is one that inevitably involves both conscious and unconscious choices by ethnographers, deriving from their perceptions during the field experience, as has often been pointed out (for example, Clifford and Marcus 1986; Marcus and Fischer 1986). It is sometimes diffcult to disentangle the voice of the ethnographer from the voices of different informants within the ethnographer's own seemingly seamless text. Netbags may perhaps be regarded as traditionally "muted" objects by comparison with "amplified" ones. Their values may not be verbalized yet may belong closely to embodied actions within the life cycle. They are just as "historical," as well as "cosmic," as other valuable possessions, even though they may appear to be out of the limelight of events (see also Jolly 1992). Although shells and pigs may be more publicly celebrated as wealth objects

in exchanges, netbags are important as a carrier of different and more nuanced values. Like the subtle shadows cast within a Georges de La Tour painting whose absence would change the entire equilibrium of the composition, the presence of the netbag in a ritual sequence places its own quiet imprint on the occasion, as when the female relatives of a bride among the Foi arrive with gifts including netbags that show their female solidarity with her--a sentiment not expressed overtly or verbally in the sequence otherwise.

As has been shown in this article, the netbag may on the surface appear to be peripheral to events, but on closer examination it can be shown to be quite central in establishing personhood within the life cycle and as a metaphor for gendered aspects of social and political life. The nuanced character of the symbolism involved here comes from the fact that the netbag belongs to both the utilitarian and ceremonial domains. Its ceremonial significance derives partly from its utilitarian functions, while its utilitarian functions are partly sacralized through its association with ritual. The two values are mutually implicated. In this regard we might further compare the netbag with the human body itself. The body is a part of everyday experience, it is "used" regularly, yet it is also the locus and origin of sacred meanings having to do with fertility, death, and regeneration. We have suggested that the netbag as a container is modeled on the womb, and in some contexts it also appears as a "skin" over the body. If this is correct, the comparison with the body does not depend on chance. Rather the meanings of the body and of the netbags spring from the same experiential roots. The netbag as a detachable "womb" that can transport values standing for the human body therefore makes good sense in phenomenological terms.

Historical change in the manufacture and use of netbags has partially shifted their status from ritual and utilitarian object to tourist item. This new aestheticization of netbags denotes a different form of personhood, one that is individualistic and commodified. In Mount Hagen the new netbags also stand in between the gendered forms of bags manufactured in the past. These new bags are the shape of men's bags but not as small as men's ***kumbana*** bags. They are not small enough to be suitable to carry the small, powerful possessions of a man nor are they big enough to carry a load of garden produce or serve as a baby cradle. The new shape is adapted to accommodate the urban context. Typically the netbag is carried simply as a colorful accessory. Although it can be used to hold money and small shopping items, it stands best as a kind of decoration or object of everyday display that draws its meaning from the fact of being empty rather than full, as is the case with the older style of bags. This empty quality is also a quality of multivalence. The bags can be carried by either sex and used for different

occasions as a sort of second best decoration. While in the indigenous context netbags could be decorations ***as well as*** having an everyday or ritual use, in their new form they can function as pure signs of decoration, separated from other contexts. It is this quality that we refer to as commodification. Cornmodification can also imply iconicity, as shown by MacKenzie in an illustration of an urban migrant from the Eastern Highlands at a Friday night dance in Port Moresby whose netbag was "the sole indication of his origins" (1991:13).

The multivalent uses and the expressed and implied meanings of these objects within New Guinea cultures signal their vital importance as items of material culture that could well be displayed in a museum case. They are as central to a set of focal values as the more commonly collected and flamboyant objects, for example, shells and plumes or carvings. Yet it is the shell valuables, the bird of paradise plumes; and the ancestral masks that tend to capture the attention of ethnographers and museum collectors. The foregrounding / backgroundng effect involved in ethnographic perceptions can be seen in the cover picture to the translation of Georg Vicedom's volume on Hagen myths published by the Institute of Papua New Guinea Studies in Port Moresby (A. J. Strathern 1977). This picture was taken by professional photographer Paul Cox at a Mount Hagen cultural show. The highly deco--rated male dancer shown in it was standing in a display booth for tourists, wearing plumes, a pearl shell, a long netted apron, and a heavy "judge's wig" made of human hair, resin, and paint. In the background hanging from an interior wall can be discerned a very large netbag of the new type, on display but partially hidden. The focus is on the standing figure, yet the netbag is also there, a reminder of other values important in the same cultural milieu. Undoubtedly, the striking decorations and stance of the figure were the reasons the photo was chosen as a cover design; but ironically the netbag also made its way into the tableau, almost unnoticed at the time! In earlier times, as we have seen, the female form of the netbag played a more foregrounded part in exchange ceremonies (as distinct from cultural tableaux). In a scene from one of the myths recorded by Vicedom, a young wife visits her new husbands settlement and comes upon his ceremonial ground (Vicedom and Tischner 1943-1948, 3:37). Planted with a grass lawn, fenced off, and decorated with cordyline shrubs, it is a fine-looking place, showing that the husband is a man of high status. She sees hanging there netbags filled with pearl shells and baler shells, and these please her. The tableau she observes has netbags at its forefront, while the ceremonial ground provides a supporting background. The netbags must have been of the large, loosely netted kind used as ah-purpose carriers by women, and she would recognize them as such. Shells in a netbag: male wealth encompassed by a unique female con-

tainer of life and death, together a moment in the gendered processes of the cosmos. The image can serve as the summation of our argument here.

NOTES

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1. MacKenzie has played a leading part in elucidating the significance of netbags throughout New Guinea by collating data from different ethnographies. In our own searches for materials and in checking on MacKenzie's references, we found it often frustrating to see that the term "netbag" did not appear in the index of a book, although it was clear from specific passages in the text that netbags were quite important. We also noted that authors adopt different rhetorical stances toward the topic of netbags, some implying that they are of little account, others granting them centrality as an expression of women's personal agency. One author, in addition to MacKenzie, has given the netbag sufficient prominence to include it in a book title (Hylkema 1974).

2. M. Strathern, in her article "Culture in a Netbag: The Manufacture of a Subdiscipline in Anthropology" (1981), argued that "woman-ness" is construed differently in different cultures, using the Melpa and Wiru peoples as her contrasting cases, and by implication arguing also that netbags, while signifying female capacities, must also vary in their meanings. The sociological contrasts she identifies may, however, be combined with certain cross-cultural similarities or overlaps in meanings such as we have delineated here. Melpa netbags do not, indeed, "function" like Trobriand skirts, but they enter into symbolic structures of life and death, and containment and revelation that do find parallels in other Melanesian contexts. (We are not concerned here with other aspects of the debate between M. Strathern and A. Weiner; see, for example, Weiner 1992.)

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WESTERN WOMEN'S TRAVEL WRITING ABOUT THE PACIFIC ISLANDS

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Although there has been a revival of interest in nineteenth-century women's travel writing, this is a neglected area of Pacific scholarship. Though the work of Western women travelers who wrote about the Pacific Islands has been drawn upon by some researchers, this literature has not received attention as a genre, as a body of source material, or as the focus of academic research questions. This article, while neither a detailed analysis of particular writers and their works nor an attempt to delineate the field, uses three categorizations--the realist, the protofeminist, and the orientalist--derived from analyses of women's travel writing elsewhere as a means of raising some of the major issues relating to such texts. The article argues that, while employing categories based on binary oppositions enables us to ask interesting questions and unmasks the complexity of these works and their authors, reductionist interpretations of this kind fail to provide adequate theoretical frameworks for understanding. Particular attention is drawn to the ambiguities and contradictions of the intersecting gender and "race" positions revealed through women's travel writing. It is suggested that this area is rich, largely untouched, and deserves further exploration.

IN RECENT YEARS there has been a marked revival of interest in nineteenth-century travelers and travel writing. It has been suggested that this late-twentieth-century revival represents a "bitter nostalgia for lost idioms of discovery and domination" (Pratt 1992:224) and a "choice of lifestyle that is a challenge to our own" (Russell 1986: 13). The "rediscovery" in the 1980s and 1990s of the books of women travel writers has been seen as part of a larger reclaiming of the Victorian period for women's history (Mills 1991:27), their contradictory lives being depicted as "more than simple portraits of feminist

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heroines" (Birkett 1989: preface). Travel writing has also become an important component of the renewed academic interest in imperialism, particularly "colonial discourse" and its contemporary impact in a "postcolonial" world in institutional forms, cultural understandings, and social interaction (Saïd 1979; Kabbani 1986; Breckenridge and van de Veer 1993). Current interest in travel writing also derives from the postmodem use of travel as a metaphor for a mode of thinking that questions the maps that position ourselves and others, and that is open to the retelling, revisioning, re-siting, and re-citing of historical and cultural knowledge, that is, the intellectual journey from the center into the periphery (Chambers 1994; Robertson et al. 1994). The interest in travel writing and tourism in the late twentieth century provides incentives--cultural and economic--for analyzing the significance and meanings of various forms of travel writing. Thus, many scholars are claiming that it is important to examine the travel writing of earlier times and different spaces, and they are approaching their analyses from a variety of disciplines and perspectives.

Some of the "rediscovered" women travel writers wrote about the Pacific, as did a variety of other women who for many different reasons traveled in one or more Pacific Islands countries during the colonial period. They include women as diverse as Sarah Maria Smythe (1864), the wife of a British' government official in precolonial Fiji in 1860; the model Victorian lady travelers Isabella Bird (1875) and Constance Gordon Cumming (1881, 1882, 1883) in the 1870s; temperance worker Lucy Broad (n.d.) at the turn of the century; Osa Johnson (1944), wife of the American photographer Martin Johnson, in the Solomons in the 1910s; and the enthusiastic British naturalist Evelyn Cheesman (1927, 1933, 1938, 1949, 1957, 1960) during the 1920s and 1930s.

Little attention has been paid by Pacific Islands researchers to this source material. This article attempts to bring into an academic focus books written about the Pacific by nineteenth- and early-twentieth-century English-speaking women travelers. In exploring these writings, it draws on the wider, growing literature on women travel writers. Jane Robinson has written a bibliographic guide to women travelers, which is the main sourcebook for those wishing to pursue what she regards as "an important, but little documented body of literature" (1991:vii). She lists nineteen women writers under the heading "Pacific Islands." This list is by no means exhaustive, but it compares well with her entries for other areas, reaffirming in a general context that women's travel writing about the Pacific is a recognizable body of literature.

Sara Mills (1991) points out that most scholars and nonacademic writers have looked for some thematic unity or other means of structuring the texts

or the authors they have examined. In order to find a coherent, organizing theme, Mills argues, women's travel writing has been selectively analyzed and variously categorized as realist literature, protofeminist literature, or as one kind of orientalist colonial discourse. These interpretations provide an entry point for thinking about Western women's travel writing about the Pacific Islands. Although it would be possible to devote an entire article to each category, my emphasis is on using them to open up discussion about this writing within Pacific studies. I will give some consideration to the first two interpretations--the realist and the protofeminist--before concentrating on the third.

The Realist Reading

Women's travel writing has been found to differ from men's in a number of ways, including the ways in which it is interpreted and evaluated. Because women's travel writing was not sponsored by governments or commercial organizations--in the Pacific, Beatrice Grimshaw (1907a, 1907b, 1910, 1912, 1930) is an exception who did write some commissioned pieces--it appeared to be personal autobiography. Therefore, it has been seen as particularly open to a realist reading. That is, texts are seen to reflect reality, and experience and authorial position are not problematized. The authors themselves frequently stated that, because of their own lack of scholarly training, they could claim no more than to be telling what they saw as it really was. Some of the better-known women's travel writing on the Pacific supports this interpretation. The much-published Isabella Bird, traveling for her health, claimed that her letters to a relation were "printed as they were written" (1875:vii-viii). Anna Brassey's extremely popular ***A Voyage in the "Sunbeam," Our Home on the Ocean for Eleven Months,*** which appeared in at least nineteen English editions and was translated into French, German, Italian, Swedish, and Hungarian, followed a day-by-day diary style. Her husband noted in the preface that she had a "painstaking desire not only to see everything thoroughly, but to record her impressions faithfully and accurately." She herself apologized for her inability to describe what she saw adequately (Brassey 1878:vii, 270; Marshall 1921:261-262). Margaret Stevenson's letters to her sister were edited and arranged by Marie Balfour, who claimed that "the South Seas . . . 'journal-letters' . . . are of course given in entirety" (1903:3), emphasizing that the text was not abridged, but a full and original account of Stevenson's direct observations and experiences. Caroline Edgeworth David noted in her preface that 'this does not claim to be a literary production, but merely an accurate, though unscientific account of the 1897 Funafuti coral-boring expedition" and insisted that her commen-

tary was based on her own observations, reinforcing this statement with frequent use of "I have seen" and "I never saw" (1899:153).

How can these claims to accurate description--to realism--be understood in a contemporary reading? Travel writing is a particular literary genre. It is not an outpouring of observations and experiences by unreflective, passive, and neutral observers. Travel writing accounts have in common "a particular reworking of the journey experience" in which diaries, jottings, and other sources "have been transformed into a new kind of text" (Millum 1994:132, 55).[1] Many of the letters that form nineteenth-century travel books were scrupulously written, edited, rewritten, and rearranged with a view to publication despite claims to be direct factual accounts of journeys and countries by a narrator (Mills 1991:69, 85-86; Millum 1994). Anna Forbes admitted that her account of the Eastern Archipelago of the Dutch East Indies (and including the tip of Papua) was pieced together from letters written home, which had given her the opportunity to remove the exaggerations of the moment and for "mature consideration of, and authentic information on, many points" (1887:viii). Her husband Henry Forbes's (1885) work included a few extracts from her journal when she was ill, and these are more detailed and interesting than the corresponding parts of her published version., Travel writing, whether authored by men or by women, cannot be' read in the late twentieth century as if it consists of unmediated descriptions of "real" events and places. The accounts by writers on the Pacific are no more "realist" in this sense than the works of travel writers about other places. Thus, in engaging with these texts, it is important to be aware of the sociocultural positioning of the authors and of ourselves as readers.

Women's use of letter or diary styles and their emphasis on being inexpert but honest observers formed part of the literary conventions governing women's travel writing. These literary prescriptions, relating to femininity, combined with women's particular interests and opportunities to construct women's texts as amateurish, unscientific, lacking literary merit, concerned with the domestic and private, and lacking authorial power (Mills 1991:12, 40, 69, 82; Pratt 1992:160; Okely 1992:12). All commentators on women's travel writing referred to in this article agree that women's writing did emphasize personal relations and domestic life, a focus that was derived from their actual social and narrative positions. Their works provide data such as extensive details regarding the proceedings and recipes used in a Funafuti cookhouse or bathing in the ***vai fafine*** (women's pool) (David 1899: chap. 16) and perspectives that are not available in male-authored works. These details support the claim that they were "realist," because they dealt with the details of "real" life. In their writing women authors often undervalued such observations and, thus, the significance of their own accounts. Katherine

Routledge (Mrs. Scoresby Routledge), who was a trained historian, apologized for the details of daily life--"the trivialities of this work"--in her account of the Easter Island Expedition of 1913-1915 (1920:vii). And Anna Forbes pointed out that she and her husband "shared for the most part the same experiences; but we looked upon them from an entirely different standpoint." Her account was "simpler," without "the admixture of scientific matter" (1887:vii). Even the Forbeses' titles reflected different, gendered claims. Hers indicated the text was about a wife's experiences (1887); his, that it was a (masterful and scientific) account of a naturalist's wanderings, travel, and exploration (1885). Other women stressed that they had written their books only because they were encouraged by others. American Mary Davis Wallis attributed her published account of her experiences in Fiji in the 1840s, when her husband was collecting bêche-de-mer, to the persuasions of her missionary friends and others (1967: preface); and Bird explained that she had been encouraged to publish by Hawaiian friends because she was less a stranger than other visitors (1875:vii-viii).

This "everyday life" focus and the constraints on being authoritative meant that women commented less frequently than male authors on political or economic questions and were perhaps less interested in politics (Middleton 1965:4). Although inattention to politics was considered appropriate for the genre, political and economic issues were not ignored by all women travel writers. A detailed study of South American travel writers led Mary Louise Pratt to conclude that women were often analytical and interpretive in their political commentary (1992:155-159), a point that could be made for certain commentators on the South Pacific as well. Isabella Bird, for example, commented extensively on Hawai'i's rulers and the prospect of American annexation (1875).[2] Nevertheless, if publication was desired, women's travel writing generally conformed to topics and stylistic conventions suitable for ladies (which is not to say that their writing was nonpolitical). Brassey, whose husband was a politician and who was herself a philanthropist and society leader in Britain, underplayed her own capabilities and deliberately refrained from making too many assertive or judgmental comments for a lady author about sensitive Hawaiian politics (Marshall 1921-1922). Her narrative about Hawai'i is warm and effusive, concentrating on sightseeing activities, her own domestic life and arrangements, and social engagements with members of the Hawaiian royal family (Brassey 1878). The masculinist heroic discourses of exploration, discovery, and colonialism were unavailable to women: theirs were not the experiences of exploration, economic expansion, or colonial government (Middleton 1965:4; Stevenson 1982:160; Mills 1991:5-6; Pratt 1992:213). And, as has been pointed out in relation to Africa, there was considerable opposition to women's traveling,

which was seen as an intrusion into a masculine domain that was essentially and textually "a man's country" (Adler 1992:l).

The scientific community was no more ready to accept women's travel texts as academic, although its members were ready to accept the products of women's travels in the form of ethnographic and botanical collections. The women themselves knew this. Brassey, for example, carefully understated her botanizing activities in her text, as if they were amateur undertakings rather than scientific exercises (1878). Nevertheless, she was made a fellow of the Royal Anthropological Institute in recognition of the excellence of her collection of ethnological specimens, which she contributed to that association. By the 1890s however, the number of interested and knowledgeable women could not be managed by a few honorary fellowships. Such women challenged the very core of male hegemony in the field of scientific travel. Isabella Bird addressed the British Association in 1891, 1892, and 1898 and was asked to give the prestigious Anniversary Address to the Scottish Geographical Society after being made a fellow in 1891 (Lucas 1917: 166-168). She drew such a large audience at the London branch in 1892 that the Royal Geographical Society, "seriously alarmed by the success of its rival," felt compelled to admit the members of all other British geographical societies (Middleton 1965:11). This meant women members, and the council formalized the situation by approving the election of "well-qualified" women, Bird being one of fifteen approved ***(Proceedings,*** November 1892, cited in Middleton 1965:11). This decision was reversed at a special general meeting, and no new women were admitted after 1893 until 1913. Constance Gordon Cumming was made a life fellow in 1914. This small foothold of acceptance was accompanied by reviews of women's work that were negative and diminishing, their sex rather than their achievements being emphasized (Birkett 1989:214-216).

There was, then, institutional and cultural resistance to women's fame and competence as travel writers. Travel writing in the nineteenth and early twentieth centuries was inescapably gendered in both style and content. If one of the ways this gendering was reflected was women's attempts to portray their writings as everyday observations and straightforward descriptions, minimizing their claims to expertise and authority, another can be found in their insistent expressions of femininity. For their work to be accepted, it was important that they did not challenge the travel-writing "territory" of men, discuss topics considered unsuitable for their sex, or convey the impression of being "unwomanly." The juxtaposition of their affirmations of Victorian femininity with their actions serves to underscore their unconventionality. Contemporaries frequently labeled their behavior as "eccentric," but it did, in fact, challenge many gender assumptions and practices.

The Protofeminist Reading

It is the ways in which women travelers did not conform to dominant feminine roles and expectations that have provided the evidence for interpretations of their writing as protofeminist. Can such characteristics as their refusal to remain "at home" in the private domain, their independence, and their love of adventure be read as corroboration of emergent feminist sentiment? Some writers have depicted women travelers as models of independence; as intelligent, resourceful, strong, struggling against the social conventions of the Victorian period; and, later, in terms of the end of the century's "New Woman" (Frank 1986:71, 90; Mills 1991:3-5). In her pioneering work on seven British Victorian lady travelers, Dorothy Middleton claimed "that the strongest impetus [to travel] was the growing desire of the nineteenth-century woman for independence and opportunity, a desire which crystallized in the great movements for women's emancipation and the fight for women's suffrage" (1965:7). However, she pointed out that some were more conscious of these influences than others and that all held a "high ideal of womanhood" and sought to maintain the image of "a lady" (1965:9). As they did not fit prevailing feminine models of domesticity very well, they often made strenuous attempts to deny improper or challenging behavior, reaffirming the feminine and conventional aspects of their persons and activities (Birkett 1989:199; Mills 1991:121). Closer examination shows that many did not approve of the physical freedoms claimed by the "New Woman" and actively opposed this categorization as well as opposing or being disinterested in women's rights and suffrage (Stevenson 1982:3; Birkett 1989:197-199). Women travelers cannot be made to cohere into a pro- or antisuffrage framework (Keays 1989:5-6; Adler 1992:3), just as they differed in terms of background, interests, character, purpose, or reputation. Precisely because they were nonconformist, they cannot be grouped neatly into narrow categories.

Women travel writers did not conform to conventional feminine behavior in a wide variety of ways, and women travelers in the Pacific Islands are not exceptions to this generalization. At the height of Victorianism, the 1880s Margaret Stevenson, a Scottish matron of nearly sixty years and mother of Robert Louis, found that within a very short time in the Marquesas, she had abandoned stiff or fitted bodices, boots, woolen garments, and other European clothing and "dressed like the natives, in two garments, one being a sort of long chemise with a flounce round the edge, and an upper garment something like a child's pinafore, made with a yoke, but fastening in front. As we have to wade to and from the boat in landing and coming back, we discard stockings, and on the sands we usually go barefoot entirely" (1903: 70-71, 86). Clothing was central not only to feminine propriety, but to the

"correct" representation of the civilized white man (or woman) abroad. On the question of feminine dress alone, this matron had violated conventions of both gender and "race" identity. In 1899, besides participating in many other activities unusual for a Western woman, Caroline Edgeworth David tried surfing Funafuti style, an exercise in which she was swept into the wave, scratched and bruised, to the "uproarious merriment" of her female Funafutian friends (1899:170-171). This behavior would have been considered quite inappropriate for a married woman at home in Sydney, where strict regulations covered male and female seabathing well into the twentieth century. Evelyn Cheesman's nonconformity was much more courageous. As a volunteer entomologist for the British Museum, she obtained a small grant to go on a solo collecting expedition to the New Hebrides in 1928. Such an expedition, even if well resourced, was considered inadvisable and unseemly for a woman alone. With "apprehensions generated by those terrible stories of brutal murders" in missionary and naval literature, and against advice, Cheesman set off, undertaking difficult and dangerous expeditions in the company of indigenous guides in the New Hebrides, Solomons, and Papua over a period of many years (1933; 1957: 158). Later she reflected that "it is not so much courage that is called for but endurance. I should place independence first and then endurance, neither of which are virtues but acquired habits"--habits that she recognized were regarded as "unsociable" and "haughty" in a woman (Cheesman 1957:239).

To interpret women travelers' lives and writings in terms of a pre- or protofeminist model is reductionist and homogenizing. It oversimplifies the complexity of their individual, cultural, and structural locations. In not being conventionally feminine, such women challenged the gender roles and relations of their home societies and the dominant Western representations of foreign places and foreign people. Their position as white women was inextricably related to the gender and racial constructions of metropolitan society and the sexist and racist character of colonialism, and they were ambivalent on both counts. The focus on personal encounters and daily life meant that they emphasized interaction at an individual level, rather than between representatives of "races." Yet, simultaneously, they were present only by virtue of being part of the imperialist "race." As women, they were supposedly in need of male protection, and yet they traveled alone safely in "uncivilized" lands, in some instances by adopting European male status. Such contradictions are captured nicely in a comment recorded by Cheesman. While she did not adopt male status, she wore breeches (her "hunting suit," that is, for hunting insects [1957:141]), and she benefited, from her European status on trails that were tabu for women. Her guides noted astutely: " 'You, Missus, all the same as woman but different' " (1957:159).

Women travelers were in a marginal position: both advantaged and disadvantaged by the combination of their "race" and sex. In Dea Birkett's words, women travelers were "exploited by and exploitative of the prejudices of their time" (1989: preface). Sara Mills claims they were caught between the conflicting demands of the discourse of femininity and that of imperialism; the former demanded passivity and a concern with relationships, and the latter demanded action and fearless behavior (1991:21). She finds that this conflict accounts for the "strange mixture of the stereotypically colonial in content, style and trope, presenting the colonised as naturally a part of the British Empire, whilst at the same time being unable to adopt a straightforwardly colonial voice" (Mills 1991:4).

The Orientalist Reading

What, then, did women travelers convey about the Pacific Islands and Pacific Islanders? Was their contribution to the colonial discourse of representation "orientalist"? Although travel writers never held a common view about colonialism, and both critical and uncritical comments in relation to colonialism can be found in male writing, Millum asserts that ultimately travel writers do represent the establishment (1994). Rana Kabbani argues that despite their diversity, and despite there being notable instances of women's travel writing, all travelogues served to bring the Empire home and contributed to a codified and static knowledge of "the other" that served the colonial vision and hierarchies of power (1986:7).

Following Saïd, whose work on orientalism did not address the discursive forms of writing available to women, we can ask whether such a " 'consolidated vision' " or cultural grasp of "the other" in overseas lands emerged in women's travel writing (1993:90). Were women, as Kabbani suggests, "token travellers only" (1986:139)? Most commentators on women's travel writing argue that because the relationship to colonialism was more problematic for women than for men, the images and options women offered their readers were much more ambiguous and complex. In short, it is argued that their representation of "the other" reflects the contradictions and ambiguities of their position as observers who are displaced from the dominant standpoint of colonialism.[3]

Visitors to Hawai'i

Let us draw on Bird's and Cummings experiences in Hawai'i in order briefly to explore the orientalist question.[4] Isabella Bird (later Mrs. Bishop) was born in Yorkshire in 1831. In 1854 she began her travels, recommended for

her health following a spinal operation. Over the next half-century she visited America, Canada, Australia, New Zealand, the Sandwich Islands (Hawai'i), Japan, Korea, China, the Malay Peninsula, Cairo, the Sinai Peninsula, and Morocco. She was the subject of a number of biographies and author of more than a dozen books. The ***Geographical Journal's*** obituary in 1904 claimed a place for her "among the most accomplished travellers of her time" (Lucas 1917:168).

In ***The Hawaiian Archipelago*** (1875) Bird recounted her experiences as a traveler in Hawai'i from January to August 1873. Bird was an inquisitive and adventurous traveler and undertook a variety of trips around the Hawaiian Islands that challenged her physically, as well as emotionally and intellectually. Bird liked to be well informed and well prepared, reading extensively about the places she visited. The letters that make up the text of her book show that her ease grew as she understood more about customs and character, became more proficient at horse riding, reduced her dependence on the comforts of a European lifestyle, and established her ability to undertake long and strenuous expeditions, especially to the volcanoes. Unsurprisingly, many of her views and assumptions reflected those common in English scientific, religious, and popular thought. But like many women travelers, she found her experiences provided comparisons that allowed her the leverage to critique aspects of her own society. There is, then, a tension between her ideas about "native" people's need for civilization and Christianity, her negative evaluations of much of mission policy and American residents' colonialist politics, and her feelings of enjoyment and security in the company of Hawaiians. Bird did not have a closed mind to what she saw, and she did not construct an image of Hawaiians as objects of colonial endeavor. Bird's observations were reflexive, as the examples below illustrate.

Bird found that life in the islands agreed with European settlers, especially foreign women, whom she had never seen so healthy, bright, and gracious, and whose dress was devoid of "the monstrosities, and deformities of ultra-fashionable bad taste." She asked herself, 'Where were the hard angular, careworn, sallow, passionate faces of men and women, such as form the majority of every crowd at home, as well as in America and Australia?" and concluded that "people must have found rest from some of its [life's] burdensome conventionalities" (Bird 1875:22). Bird implied that if island living had advantages in terms of dress conventions, stress, and health over life in metropolitan settings, it was not an inferior life to that of "civilized" societies.

Bird was most emphatic that "natives" are not "savages" (1875:4). She admired Hawaiians' appearance--"a handsome people" (p. 274)--and ease in natural settings, and found the chiefly class to be educated "ladies" and "gentlemen." She thought the women inclined to obesity, but qualified her

statement with the observation that ample drapery "partly conceals this defect, which is here regarded as a beauty" (p. 20). Her own prejudice was relativized by her cultural awareness. Bird saw advantages and disadvantages in the Hawaiians' construction of femininity. She praised the free way in which Hawaiian women rode, "flying along astride, barefooted . . . a graceful and exciting spectacle" (p. 31). She noticed that she was never offered any help to climb walls, for example, and attributed this to the fact that native women never needed help, being as strong, fearless, and active as the men (p. 238). However, these positive attributes she countered with the comment that "Hawaiian women have no notions of virtue as we understand it," affirming Victorian gender assumptions about the responsibility of women as guardians of morality, for, she asserted, "if there is to be any future for this race it must come through a higher morality" that will raise the women in their occupations and amusements (p. 255).

To Bird, Queen Emma, Kaleleonalani, conveyed "an unconscious dignity with ladylike simplicity" (1875:263). At a garden party at Kaleleonalani's house, Bird observed that beside the splendor and stateliness of the Hawaiian chiefs, "the forty officers of the English and American war-ships, though all in full-dress uniform, looked decidedly insignificant; and I doubt not that the natives, who were assembled outside the garden railings in crowds were not behind me in making invidious comparisons" (p. 265).

This quotation illustrates Bird's self-awareness, which grew from a sense of her own ridiculousness when out of her own environment. Wearing her "coarse Australian hat . . . [with a borrowed] riding costume" and "great rusty New Zealand boots," and riding on a Mexican saddle (to which she had not at that stage become accustomed), she saw herself as "grotesque" compared to her picturesque guide from Hilo to the volcano (Bird 1875:69-70). And later: "I was conscious that we foreign women with our stout staffs and grotesque dress looked like caricatures, and the natives, who have a sense of the ludicrous, did not conceal that they thought us so" (p. 80). She frequently commented on the negative ways in which Hawaiians must perceive Europeans. On an expedition to Waimanu Valley, she collected ferns, noting that the "natives think it quite idiotic in us to attach any value to withered leaves" (p. 232). And she asserted that a Hawaiian woman who interested her greatly, and whom she considered extremely shrewd and intelligent and a skilled mimic of Europeans, "evidently thinks us a sour, morose, worrying, forlorn race" (p. 227), a view that appeared to have been her own.

Although her enjoyment of Hawaiian company grew, Bird found the sunny side of native life countered by "dark moral shadows" (1875:103), marked by such problems as population decline and leprosy. Here, Bird conformed to stereotypical generalizations, her own upbringing in a highly reli-

gious family probably contributing to the ease with which she slipped into an analysis that was challenged by her own observations elsewhere. Prior to the success of European, civilizing influences, she asserted, the Hawaiians were "a vicious, sensual, shameless, herd" (p. 103). She was impressed by how far they had come along the paths of civilization in forty years: "Poor people! It would be unfair to judge of them as we may legitimately be judged of, who inherit the influences of ten centuries of Christianity. They have only just emerged from a bloody and sensual heathenism, and to the instincts and vitality of these dark Polynesian races, the restraining influences of the Gospel are far more severe than to our cold, unimpulsive northem natures" (p. 183). So there was sympathy for Hawaiians and a barb for the Europeans.

The Hawaiians' sense of ceremony impressed her greatly, but she was inclined to interpret it in terms of "a wonderful leap" to "correct and tasteful civilization" (Bird 1875265). At one " 'poetical' spectacle" she was reminded by "ancient and hideous females, who looked like heathen priestesses, [and] chanted a monotonous and heathenists-sounding chant. . . that this attractive crowd was but one generation removed from slaughter-loving gods and human sacrifices" (p. 207). On one of her expeditions, a "frightful old woman, looking like a relic of the old heathen days" and females with "low sensual faces, like some low order of animal" (pp. 141-142), prompted Birds most negative comments in an evolutionary racialist framework. And she disparaged her young friend Deborah for being childlike (p. 134). Although she broke many of the gender prescriptions of her own society, it is the way Hawaiian women did not conform to her ideas about appropriate female behavior that she connected most often to the character of the Hawaiian "race." By contrast, she did not link Hawaiian men's cruelty to horses, which offended her greatly, or the appearance or demeanor of males to their barbarous ancestry (pp. 133-134, 311).

By the end of her stay, Isabella Bird had undertaken numerous trips in the company of Europeans with Hawaiian guides, alone with Hawaiian guides or friends, and on her own. She had waded waist deep in rivers and ridden across in dangerous floods, ridden hundreds of miles astride and even barefoot, and camped in all manner of housing, including low grass shelters, with all classes of people. She decided that "society" is more demanding than the rough open-air life (Bird 1875:266), and that the more she saw of Hawaiians, "the more impressed I am": "I thoroughly like living among them, taking meals with them on their mats, and eating 'two fingered ***poi*** as if I had been used to it all my life. Their mirthfulness and kindliness are most winning" (p. 429). Before leaving, she concluded that the "life here is truer, simpler, kinder, and happier than ours . . . the natives, in spite of their

faults, are a most friendly and pleasant people to live among . . . and a white woman is sure of unvarying respect and kindness" (pp. 440-441). She had clarified her ideas on politics and was particularly critical of Americans, "destitute of traditions of loyalty or reverence for aught on earth" (p. 201), and of their designs for annexation (p. 279). Her notions of "race" and civilization included the earnest wish that Hawai'i should remain the inheritance of the Hawaiians (p. 473).

Constance Gordon Cumming, who was born in Scotland in 1837, was an even more prolific writer than Isabella Bird, producing two volumes on many of the subjects of her wide-ranging travels, including Hawai'i, although she spent less than two months there in 1879. She is better known in Pacific scholarship for her writing on Fiji, where she lived in the household of Governor Sir Arthur Gordon. Cumming was such a prolific gatherer of information that some readers have considered her books almost unreadable, they are so densely packed with information (Middleton 1965:5). Volume 1 of ***Fire Fountains: The Kingdom of Hawaii, Its Volcanoes, and the History of Its Missions*** (1883) consists of a series of letters recording her activities and impressions, while volume 2 is primarily her version of history, missions, morals, and general knowledge put together from other accounts, her contact with missionaries, and personal observations. It is volume 1, therefore, that is of most relevance as travel writing. The bulk of the work concerns her expeditions to the volcanoes but is liberally interspersed with comments on white and Hawaiian society. She filled out the narrative with outlines of history and customs, mainly from secondary sources.

Cumming made frequent direct comparisons with places and customs she knew were familiar to her readers in order to convey her impressions. Thus, 'Waikiki is to Honolulu as Brighton is to London" (Cumming 1883, 1:25); ***"poi,*** which is still the approved diet of the country. . . is to Hawaii as porridge is to Scotland" (1:35); the deification of idols of stone she found common to most lands, even the Scottish Hebrides last century (1:40); "the profession of minstrel ranked as high here as in Wales" (1:108); and human sacrifice was compared with the law of Moses (2:21). These comparisons were not an attempt to appropriate scenery and culture to metropolitan ideals. Cumming did not try to domesticate Hawai'i or the Hawaiians; nor did she specifically link such comparisons of cultural practices to evolutionary schemes of progress. Undoubtedly, she was a strong advocate of the "civilizing" process: "In the presence of such a civilised community as the Hawaiians of to-day, and of ladies whose fashionably made silks and satins are supplied by expensive American milliners, it is scarcely credible that only sixty years ago a sail in the offing was the signal for all these nut-brown maids and matrons, from the highest chiefess to her lowliest vassal, to swim

out to sea to welcome the newcomers" (2:81). Still, she acknowledged that cannibalism had almost ceased prior to the arrival of whites (2:77). And in her comparisons there were many observations and judgments that were critical of "civilization." On one level, Cumming adhered to common viewpoints with respect to the civilizing effects of white contact and conversion; but on another, she conveyed misgivings and criticisms. So, it is "horrible to relate, I saw 'advanced' girls wearing ***leis*** of artificial flowers! Such is progress!" (1:22); the "early navigators gave small equivalent for the hospitality lavished on them" (2:31); and "with all possible reverence for the great work so nobly accomplished by the early missionaries, it is certainly a matter much to be regretted that, in the wholesale sweeping away of idolatry, so many subjects deeply interesting to the ethnologist and the antiquarian should have been hopelessly swamped, and everything bearing on the old system treated as being either so puerile as to be beneath contempt, or so evil as to be best forgotten with all speed" (1:55). She suggested that "the British Isles retain far more traces of the old paganism than do the isles of Hawaii" (1:55-56). Cumming's use of comparisons, then, can be seen as a way of asserting the common humanity of all within a common geographical environment, and the cultural comparisons seem designed to relativize European customs as well as convey "foreign" subjects in ways that were meaningful to her readers.

Assiduous as they were in conforming to the appropriate literary conventions and emphasizing that they were "ladies," both Bird and Cumming provided subtle but persistent subthemes that were a critique of Western lifestyles. Their sense of achievement was not only about independently managing physical challenges. There is a strong thread of reflexivity in their writing, of self-learning that they wanted to convey to their readers. Women travelers often presented different "knowledge," leading to a relativized view of women's status, gender relations at home and abroad, and colonialism (see also Melman 1989). The "knowledge" they conveyed was formed through different experiences and different relationships with their hosts, relationships and experiences that altered the lives and self-perceptions of many women travelers.[5] The self-consciousness in much of their travel writing can be seen as one area in which the construction of the "colonized subject" was at least partly collaborative at the level of daily life and personal relations. Thus, we need to avoid considering cultural representation as only the work of the Western observer. The notion that European culture emanates out perpetuates an imperial tendency. Recognition of reflexivity and the incorporation of Pacific Islanders' self-representations in Western writing is an important antidote to ideas perpetuating metropolitan power and lack of agency of the colonized.

Crossing the Boundaries of "Race" and Gender

Women travel writers did not always construct difference between "us" and "them." As Birds and Cumming's works show, they often used similarities, not differences, to illustrate what they wanted to convey. Nor were the differences they did construct necessarily favorable to the Western "us."

Frequently, women travelers considered aspects of Pacific cultures superior to their own, and they sometimes sought to identify themselves with Pacific Islanders. Caroline David used humor to good effect to reduce the distance between "us" and "them" (1899). She found that "the Ellice Islanders hadn't the faintest desire to kill and eat us; on the contrary, they were quite as civilized as most white folks, and merely wanted to earn our dollars easily and eat our 'kaikai' freely" (p. 2). Throughout her "Unscientific Account of a Scientific Expedition," she undermined the knowledge and skills of the civilized "theoretical experts" (p. 3). She joked about the intransigence of the Funafutians who did exactly as they liked and refused to take the necessary precautions to cure ringworm, but simultaneously she poked fun at missionaries: "For the, first time I wished myself a missionary; they will obey a missionary," and "Oh for a medicine man for my obstinate, good, lazy, lovable Funafutians!" David's wit and judgments were dispensed equally between Westerners and Pacific Islanders. The following comment is typical of her style: "I never heard any Salvation Army drum that equalled a Funafuti mat-banging in ear-splitting misery" (pp. 56, 57, 61).

Even those who were wary about addressing the absolute distinction between "us" and "them" occasionally made comparisons to show that native practices were not "primitive." Brassey explained to her readers that the eating of live shrimps by Hawaiians "looks a very nasty thing to do, but, after all, it is not much worse than our eating oysters alive" (1878:287). And, although cautious about politics and claims to be authoritative, even she could provide a negative commentary that underscored the weaknesses of "civilization." She supported colonial endeavors, but not the French--"poor colonizers"--and she supported the Christian mission, but not the missionary squabbling between Protestants and Roman Catholics in Tahiti (Stuart-Wortley 1882:10, 16).

Bird's and Cummings critiques are sober indeed, compared to the warmth and enthusiasm for Marquesan and Paumotuan lifestyles and people shown by Margaret Stevenson. Their critiques are abstract and distant from the practical concern for the neglected health of the Funafutians demonstrated by Caroline David and moderate in the extreme compared to the scathing rebuttal of European constructions of savagery reiterated time and again by Evelyn Cheesman in Melanesia: "The natives had always been justified in

retaliation. . . . [The guides and carriers] helped me rapidly to change my whole conception of the natives' attitude towards all white people" (1957: 158). Cheesman discovered that on Malekula "a ***tambu*** had been set upon me!--which signified that my person was sacred and I must hot be interfered with, and that anyone accompanying me was ***tambu*** also." This discovery lessened her anxiety considerably, not for her own safety but for that of her guides: "The chief anxiety always concerns the boys" (Cheesman 1933: 16). Cheesman was no romanticist, but she learned from the people and knew that she had to respect and work within the parameters of local cultures.

My reading of more than forty books by women travel writers who visited the Pacific Islands shows that most used their marginal positions to ponder their own understanding of other peoples and cultures, of themselves, and of their home societies. However, some women writers were unreflective, lacked knowledge, were predisposed to prejudge and stereotype, were unlikely to be changed by their experiences, and stressed sensationalist items such as head-hunting.[6] Charlotte Cameron's anecdotes and comments about New Guinean cannibals and prisoners--"Never have I seen such degraded, criminal, distorted faces" (1923:71-72)--and the transformations effected by Christianity to "gentle, docile, and law-abiding people" (p. 86) place her work in this category. The naive and ill-informed Osa Johnson in the Solomons provides the strongest example of this kind (1944). So while it is possible to find many cases demonstrating aspects of women's travel writing that undercut white superiority and Western gender relations, the stereotypic writers--like Johnson, who reveled in tales of cannibalism and savagery --cannot be ignored.

Women's travel writing about the Pacific Islands and other recent research suggest that there is no coherent, uniform statement about "the other" in women's travel writing. Nor was such writing simply a footnote to a homogenizing, orientalist discourse (Melman 1989; Mills 1991:51-55). But, if this diversity can be seen as a positive interpretation of women's writing, it has been argued as well that the different versions of "the other" and "themselves" presented in women's travel writing, while less confident and more complex than the male versions, actually made visible what was formerly "uncolonized space." In other words, they opened up areas of life that had previously been screened from Western (imperialist) observers, enlarging the boundaries of the orientalist vision. From this perspective the various critiques--implicit and explicit--in this literature are seen as insufficiently strong (or perhaps lacking the mastery) to challenge the pervasiveness of the dominant colonial discourse (Nair 1990:42-43). These are issues that surely require further investigation and suggest that it is important to attend to the various discourses in Western women's travel writing about the Pacific and to the authors' individual voyages of self-discovery.

Realists? Protofeminists? Orientalists? Answering such questions leads to the danger of falling back on the old binary divisions: they were or were not realist, feminist, or orientalist. But the search for answers also reveals the complexity and richness of this barely examined source material. As Robinson found, when organizing her bibliographic sourcebook, women travel writers were "a nonconformist race" (1991:viii). She claims that one of the few things these women had in common was their originality, an originality that took them not just to foreign lands, "but across the boundaries of convention and traditional feminine restraints" (p. ix). We can see by looking even briefly at their impressions and accounts that this originality included conflicts, ambiguities, and contradictions, and that the boundaries they crossed were the intersecting boundaries of "race" and gender.

I have used three types of categorization to try to emphasize and elucidate some of the tensions apparent in their writing. These women did not provide a realist mirror of Pacific Islands society--since all reporting is mediated by authorial position and all accounts are selective--yet they claimed, in line with the genre of women's travel writing, to be reporting directly. And while they were not conventionally feminine, most stressed their femininity and few were suffragettes or identified themselves as feminists. Most owed their presence and safety in the islands to Western countries' colonialist endeavors and their "race," but many expressed dissatisfaction and some expressed anger at the results of Western impacts and the hubris of Western claims to superiority. As a group, these writers did not present a unified voice. Their individual output, expressed in a language and style that was diffident and often self-deprecating, was unlikely to have been strong enough to disrupt the dominant colonial and gender discourses. But challenge them they did on paper and in their own lives. Theirs was another of colonialism's cultures and a subversion of it (Thomas 1994). Margaret Stevenson found it "a strange, irresponsible, half-savage life" and wondered "if we shall be able to return to civilised habits again" (1903:86). One thing is certain: she was never the same again.

NOTES

I. The definition of travel writing is itself open to debate. I have rejected the inclusive approach, which incorporates any writing by those who lived in places other than their homelands, and have concentrated on published works that are concerned with and written as accounts of journeys and experiences in places where the authors did not intend to establish a home.

2. Isabella Bird was forced by her publisher to exclude some material from *The Golden Chersonese and the Way Thither* (Mills 1991:116).

3. Clearly, the time and place mattered, and, thus, the working out of this standpoint in women's travel writing differs. In southern Africa, for instance, women travelers often strove to identify with the most powerful group, white males, and even referred to themselves at times as Englishmen, taking advantage of their white skins to extend their boundaries of independence (Adler 1992:11; Frank 1986:70-72).

4. This article is a longer version of a paper delivered at the "History, Culture, and Power in the Pacific" conference organized by the Pacific History Association and the University of Hawai'i Center for Pacific Islands Studies in July 1996. The focus on visitors to Hawai'i in this section was especially appropriate to the conference location in Hilo. As well, Bird and Cumming are well known and acknowledged archetypical "lady travelers" on a world scale.

5. These points also emerge in Pratt 1991:27, 90; and Stevenson 1982:12.

6. This list of characteristics is taken from Millum's analysis of travel writing in Borneo, an analysis that ignores the gendered nature of travel writing and is based on masculine characteristics. Nevertheless, it provides a relevant interpretation of women travel writers like Osa Johnson, who was one of Millum's sources for Borneo (Millum 1994:80-81).

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THE SUBSIDIZATION OF EMPEROR MINES LIMITED

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The relationship existing between the Emperor group of companies and the government of Fiji is examined over a forty-year period. During a substantial part of this time the company has been subsidized by the state. Focus is on one particular period, beginning in the early 1980s when the company received tax-free status for its gold-mining operations in Fiji. The study attempts to quantify the value of subsidies offered to Emperor Mines Limited under the terms of the Vatukoula tax agreement between the state and the mining company. The authors argue that by the standards of gold-mining taxation agreements in developing as well as developed countries an extraordinary, if not unique, tax arrangement has been possible in Fiji. The tax arrangement with Emperor will also have implications for other possible mining operations in Fiji.

THIS ARTICLE CONSIDERS the history of state subsidization of Fiji's largest private-sector employer, Emperor Mines Limited (EML), and its principal Fiji subsidiary, the Emperor Gold Mining Company Limited (EGM).[1] We argue that EGM has been the recipient of extremely generous tax concessions and subsidies throughout the last forty years and that the company has, since the signing of the 1983 Vatukoula tax agreement (VTA), paid negligible amounts of tax to the government of Fiji.[2] The agreement is so structured that the company is unlikely to pay any corporate tax or royalties to the government until after the year 2011.[3] By the normal standards of international tax agreements in the mining industry the VTA is exceptional, standing

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virtually unique among such arrangements in small, low- and middle-income developing countries in allowing tax-free status for a gold mine.

First, we will consider the historical relationship between Fiji's government and EGM as it emerged through various concessions granted under the taxation system as well as through the systems of royalties and subsidies. Both the postwar colonial and postindependence taxation regimes that emerged have meant that the company has been dependent on state subsidy for close to half a century. In the second part of the study, we consider the terms of the Vatukoula tax agreement, negotiated in the early 1980s, and the circumstances that gave rise to it. The third section examines the corporate structure of EML, of which three companies at Vatukoula (EGM, Koula, and Jubilee) are the main revenue centers. In this part, we also consider certain tax implications of the agreement and the value of the implicit subsidy that the VTA has created. In conclusion, we evaluate implications of the VTA for future relations between Fiji's government and other mining companies, in particular Placer Pacific's intended development of a very large but low-grade copper deposit in the Namosi Province on the main island of Viti Levu. The fact that the government has permitted exceptional terms and conditions for mining in the case of EGM would suggest that this situation may be replicated in the future. We also consider what alternatives, if any, exist to the current taxation regime.

In comparison to other concessions granted to industry in Fiji, EGM's may appear unexceptional. When viewed over the longer historical sweep of a forty-year period, however, they are very substantial. The concessions are even more exceptional compared to tax agreements negotiated between mining companies and the state in other developing countries. In this light, the relationship between EML and the Fiji government can be seen as one of donor and recipient--with the donor being the state.

Other than Emberson-Bain's recent work highlighting the history of relations between the company and labor (1994), little has been written about EML and its exceptional governmental relations. However, the difficult industrial-relations situation at Vatukoula that Emberson-Bain analyzes (there have been a number of violent confrontations between Emperor and its labor force in the postwar era) has an uncomfortable parallel in the relationship between government revenue and the Emperor group's profits. The government has been preoccupied with the maintenance of employment levels, and although the mine has been an important source of jobs for the last fifty years, such employment has come at the price of major 'subsidies and concessions. The one exception has been the gold-export tax, reintroduced in December 1987. The relationship between Emperor and the government also mirrors the situation that prevailed during the colonial era

with the Colonial Sugar Refineries, then the most important firm in the country. With the government acquisition of Colonial in 1973, Emperor took its place as the most influential of the private-sector employers, always able to extract tax and other concessions from the state with the threat of redundancy and unemployment. This article analyzes not only the history of these concessions, but also the cost of the most recent concessions.

The History of Concessions to Emperor Gold Mining

The government of Fiji has, throughout the entire period under study, maintained a taxation regime in the mining industry that has been heavily reliant on ad valorem imposts. The system is based on three different components that evolved over time. First is a system of royalties, which can be as much as 5 percent of FOB value. Second is an export-tax regime, the rates of which vary but currently stand at 3 percent. Finally there are the usual corporate taxes under Fiji's ***Income Tax Act,*** which have developed over time but have been eroded by tax concessions as well as the company's tax-efficient accounting practices.

Taxation provisions have changed dramatically in Fiji since EML was first incorporated in 1935 and began operations. Almost at the same time, EGM was incorporated in Fiji.[4] Mine developers viewed Fiji's colonial-era tax regime as unconducive to the development of new mines.

In the case of the Vatukoula gold mine, neither the end product, gold, nor the revenue brought to state coffers appears ever to have been as significant to the government as the employment offered by extraction. The concessions offered by the state--to solve problems of what EGM claimed were inadequate finance; rising costs of production, exploration, and development; and a static gold price--have been, at least publicly, to keep an average of fifteen hundred workers employed and the town of Tavua viable. The state has granted concessions to Emperor since 1952, when the port and service tax was waived for bullion exports. Prior to this, the chairmen of the board of directors of EGM and of Loloma Goldmining, as well as the secretary of the Associated Mining Companies, had expressed their dissatisfaction with a royalty charge "based on profit on a sliding scale as against a fixed rate of 5 percent ad valorem" (Taylor Report 1960:15). The state was to collect approximately £300,000 in taxation (port and service tax, royalty, and income tax) from the company for the year. The directors, not uncharacteristically, viewed these taxes as excessive and pointed to the example of gold mining in Australia, where there was no income tax on mining profits, no royalty tax, and no tax on dividends from gold mining.[5]

To provide financial support to EGM, the state granted the company tax

concessions and subsidies throughout the 1950s. After 1952, income tax and royalty were waived for two years, beginning 20 June 1957. In addition, on 19 June 1958 the state granted the company a subsidy of £2 per fine ounce for three years--1958/1959, 1959/1960, 1960/1961--with a maximum of £150,000 per year: a potential total of £450,000 (Taylor Report 1960:15). Among the terms of the subsidy were that

(a) the Company shall carry out capital development costing an equal or greater amount on mine buildings, plants and underground workings (exclusive of staff or employees' housing) during the period of the subsidy; the Company will be required to refund to the Government all sums received by way of subsidy in excess of the cost of such capital works; and
(b) in the event of production in any one year falling below 75,000 ounces, the subsidy shall be reduced below £l50,000 at the rate of £2 per ounce of the shortfall. (Taylor Report 1960)

The relationship of corporate welfare recipient and donor that evolved between EGM and the state in the 1950s has continued unabated in various forms to the present. The government--whether colonial or postindependence--has been willing to provide ongoing subsidies to the mine, a regime deemed necessary to generate and sustain employment levels in the Tavua area. The question that appears to have been explored with only the most moderate interest by the government was whether the mine would have been viable without ongoing subsidy.

Despite the financial assistance offered in the early 1950s operating the mine remained uneconomical. The company requested further assistance in the form of a refund of the port and service tax paid from June 1958 to 1961, a sum of £75,000.[6] The company also asked that the gold subsidy be extended for a further three years and that it be increased to £3 per ounce as of June 1961 for up to 75,000 ounces per annum, that is, up to £225,000 yearly, at a total cost of £675,000. Further, the company also requested an exemption from income tax and royalty for five years, for the period 1962 to 1966.[7] This was to be the first of many such requests for tax concessions. Subsequent attempts at tax exemption were to prove more successful.

The granting of subsidies and requests for further financial assistance did not end in the 1950s. An agreement was signed between EGM and the state where EGM was given F$2.04 million over a three-year period beginning April 1967 for development and capital expenditure (Siwatibau Report 1977: 11). Among conditions for this assistance were an increase in tunneling and rising from 10,000 to 30,000 feet annually, the maintenance of annual ore

production at more than 285,000 tonnes, and the state's appointment of an observer to the EGM board. This F$2.04 million could be regarded as a goodwill measure to keep the mine operating, as Emperor did not repay the loan and the amount was "progressively written off" ***(Parliamentary Debates*** 1977:30).

Again in May 1971, following independence, a further agreement was signed between the state and the company. Provisions included "a government grant of $150,000 . . . for the extension of Borthwick shaft from level 11 to level 16 to be completed by 30th June 1974 . . . $450,000 . . . for liquidity purposes in an interest free loan . . . [and] an industrial estate 80% owned and funded by the State and 20% owned and funded by EGM Co. Ltd . . . in Vatukoula" (Siwatibau Report 1977: 12).

Following the U.S. monetary crisis of the early 1970s, the price of gold rose to almost US$200 per ounce in 1974. The Fiji government therefore briefly reintroduced an export tax on gold, at 2 percent of its value (ibid.). But the tax was short-lived: the state granted more concessions to Emperor after a drastic fall in gold prices in 1976. According to EGM's directors, the company was once again experiencing extreme liquidity difficulties. The concessions included a waiver of 1975 taxes due amounting to F$105,000; a waiver of the 1971 loan balance, which amounted to F$206,326; and an interest-free advance of F$300,000 over a three-year period (ibid.:13).

Yet another concession was granted in 1977.[8] In the Parliamentary Session of 28 November 1977 a motion was moved by the Hon. C. A. Stinson, finance minister in the ruling Alliance Party government, for advance of a further F$2 million to EGM. This loan had been recommended by a committee appointed by the labor minister to look into the intended closure of the gold mine at Vatukoula. In his speech, Stinson outlined the state's assistance to EGM since 1968. He justified the extensive subsidies on the grounds that the "economic and social contributions" EGM was making to the Fiji economy and people more than compensated for the assistance. Stinson stated that between 1968 and 1976, wages and salaries paid by Emperor were almost F$24 million, PAYE tax received totaled F$2.156 million, Fiji National Provident Fund took in F$2.008 million, and company tax, gold tax, and royalties totaled F$2.2 million ***(Parliamentary Debates*** 1977:30).[9]

Opposition leader Jai Ram Reddy replied to Stinson by arguing that even though the company claimed "liquidity problems" in 1974/1975, it had paid dividends of F$817,000 to the parent company in Australia and had paid out F$1.571 million to subsidiary companies in Fiji (ibid.:38). These subsidiaries, Reddy also pointed out, had made profits in 1974, 1975, and 1976 of F$609,000, F$475,000, and F$526,000 respectively. EGM, therefore, was making a profit through its subsidiaries.[10] The Siwatibau Report also pointed

out that certain intercompany transactions were incorrectly charged to the EGM account instead of the subsidiaries' accounts, though it found no strong evidence of transfer price manipulation.

Parliament approved a motion that EGM be given the following concessions: (1) an advance of F$2 million in total repaid at the rate of 50 percent of pre-tax profit of EGM, repayment to government to be exempted from tax, 50 percent of the advance to be written off, and the advance subject to interest at the rate of 3 percent per annum; and (2) workers be given a 5 percent wage cut ***(Parliamentary Debates*** 1977).

Soon after, EGM announced the layoff of one thousand mine workers as a cost-reducing measure ***(Fiji Times,*** 4 January 1978, 1).[11] On 19 January 1978, the Fiji government announced its intention to expropriate the company. Then Prime Minister Ratu Sir Kamisese Mara traveled especially to Vatukoula to make the announcement personally to the mine workers, who were on strike. Negotiations towards a takeover failed, though, after the government and the company disagreed on the price ***(Fiji Times,*** 11 May 1978, 1). The government offered F$6 million for Emperor's assets and operations while the company wanted F$37.5 million. Minister for Economic Planning Sir Vijay Singh, who had been given the task of negotiating, stated in Parliament that "there was no way in which Government could justify an expenditure of anything like $37.5 million" ***(Fiji Times,*** 30 May 1978, 4). The negotiations over nationalization ended and the company continued to be privately owned.

In 1982 the government of Fiji again offered financial help to Emperor. A cabinet subcommittee had studied "EGM's troubles" and found the company--in a manner consistent with its history--experiencing serious financial difficulties. As before, one of the reasons given by the government to justify aid was continued employment for mine workers. EGM and its new and much larger partner, the Western Mining (Fiji) Corporation, were granted twenty-one-year gold-mining leases. A condition of the leases was that workers not be laid off without prior consultation with the government. The consortium also received a special prospecting license covering 6,123 hectares "with an agreed 3 year $3.13 million exploration program in the Tavua Basin" ***(Fiji Times,*** 18 March 1983, 1).[12]

The historical evidence begs an important economic question. Were the concessions granted to the company actually necessary? Or was the government a poor negotiator or the unwitting victim of a company continually threatening unemployment? There exists no definitive answer, to this question, and it is not the purpose of this article to seek one: that would necessitate knowledge of the actual cost of operations over a long period of time. No doubt such data were given to both the Taylor and Siwatibau Commis-

sions. However, given the free-rider problem of which economists are well aware, it is simply not knowable whether the data provided were accurate or not. Clearly, Siwatibau was of the view that the subsidies were justified.[13] Nonetheless, a number of commentators over the years have viewed the decision to subsidize the company with great skepticism. Ward in particular, in one of his less sagacious comments, argued that "it is pertinent to ask whether the payment of subsidy worth $750 per worker plus the probable continued subsidisation of the industry is really prudent or justified when the long run prospects of the gold mining industry in Fiji (and in the world as a whole) are relatively dismal" (1971:140).

It would be easy but quite unfair to criticize Ward with the benefit of hindsight, given the floating of gold prices in 1973 and the continuation of production at Vatukoula for at least another thirty years. However, given what was in place at the time--the Bretton Woods system with a fixed gold price of US$35 per ounce, a rising wage rate--one can well understand the subsidy decisions prior to 1973 for a mine producing low-grade ore. Moreover, there appears to be evidence to suggest that in the 1960s and early 1970s Emperor was indeed caught in the classic position of being a marginal mine facing a fixed price of gold and rising costs of production (see Emberson-Bain:161, 238, 247).

This is not to suggest that the data necessarily vindicate the government's decisions. The foregoing is intended to demonstrate only that the government of Fiji had maintained a regime of subsidy for thirty years prior to the introduction of the Vatukoula tax agreement. Nonetheless, the taxation regimes that ***are*** the main focus of this study were in place in 1982, well after the end of the fixed gold-price system, and hence are far more questionable. Some commentators have generally supported the system of subsidies and tax exemptions as necessary to transform EGM. Kasper argued that the joint-venture agreement transformed Vatukoula "from a rather conventionally run mining operation that experienced difficulties in continuing operations into an efficient and profitable enterprise" (Kasper, Bennett, and Blandy 1988:117). It is not the purpose of this article to either justify or criticize the government decisions to offer subsidies and a taxation regime that is exceptionally generous. Rather, our purpose here is to document the concessions, demonstrate how exceptional they are, and show the consequences in terms of corporate structure and operations.

Particularly significant is the timing and nature of the concessions, subsidies, and "soft" loans offered Emperor over the years. The concessions have almost invariably followed periods of decline in output and employment, as can be seen in Figure 1. More significant is that the concessions have been followed by substantial increases in gold output. This tendency very much

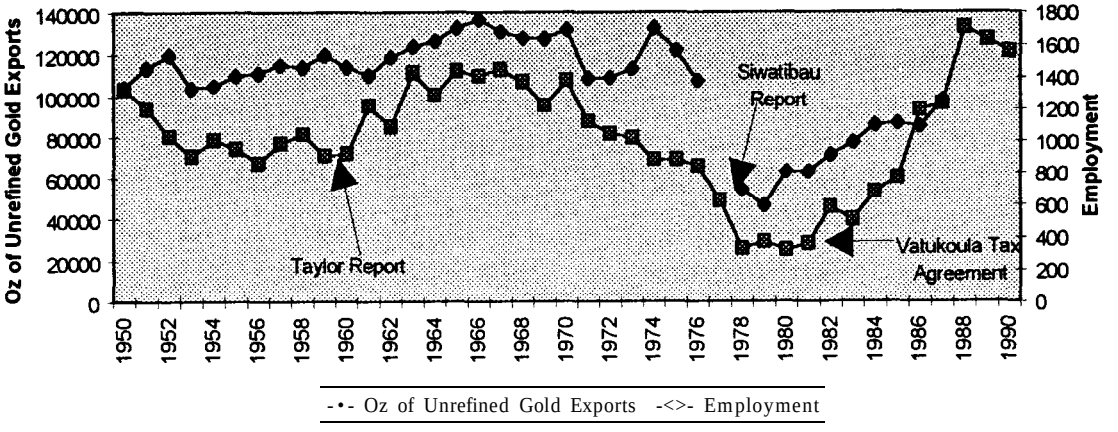

FIGURE 1. Fiji exports of gold and employment in the gold-mining industry. (Data from Fiji Bureau of Statistics; data on average number of employees at Emperor, Loloma, and Dolphin--Associated Companies of Vatukoula--from Emberson-Bain 1994:247; no employment data available for 1977)

falls in line with the arguments that the subsidies have been the basis for developing new shafts and mines. What is evident is that Vatukoula and the Tavua basin have substantial gold deposits that will continue to sustain production well into the twenty-first century: at a recent business meeting in Fiji, senior Emperor officials said they intend to develop two new mines by the year 2000 with an output of 200,000 ounces per annum at a cost of US$300 per ounce.[14] The government has helped the company subsidize its expansion.

However, one cannot argue that the concessions to Emperor were indispensable for the continued existence of the Vatukoula operations. Certainly from the magnitude of the reserves, the very high grades of the reserves, and the apparent willingness to continue to invest, it is by no means obvious that production would have ceased had the government failed to continue the subsidies. Such a determination would need some analysis of profitability at the very minimum, but even that would be inadequate to know whether the company would have invested without the subsidies.

Figure 2 compares the average operating statistics to the price of gold on the London market in Fiji dollars during the 1950-1970 period. The data are derived from published cost figures.[15] The figures indicate a definite decrease in profitability in the late 1950s, which was discussed in the Taylor Report. No cost data are available after 1970 and hence the arguments for later subsidies have less supporting evidence. None of the data presented answers the really significant counter-factual question of whether Emperor and its related companies would have invested in expanding mining capacity

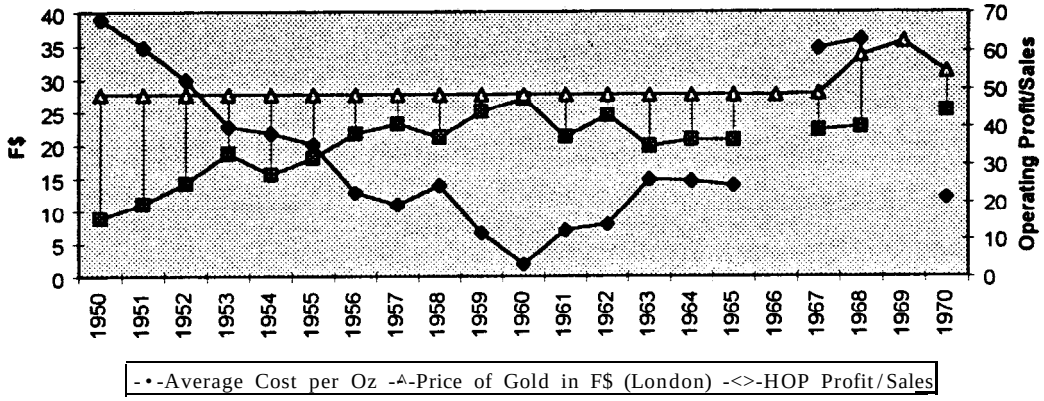

FIGURE 2. Estimated average costs, revenues, and rates of operating profit for Emperor Gold Mining Company Limited, 1950-1970. This estimate of operating profits underestimates the actual figure because it assumes that EGM profits were the same as those for the "Emperor Associated Companies," which during this period included Dolphin and Loloma. (Data from Emberson-Bain 1994:161, 247; no reliable cost estimates available for 1966 and 1969)

in the 1960s and 1980s without the concessions or with much less generous concessions. To answer this question with resort to ***ex post*** data is simply not possible.

In the final analysis, no judgment can or perhaps should be made of colonial or postindependence government policy towards Emperor. Certainly the published data, assuming they are accurate, support the arguments that profits declined in the 1950s and that production and profitability rose again when new investments were undertaken and new mines developed. Whether the company judiciously managed its relations with the colonial government to obtain subsidies to fund its expansion or whether it was genuinely on the verge of bankruptcy and the loans were absolutely essential remains largely unanswered and unanswerable. What is certain is that the Vatukoula tax agreement, as the most recent chapter of relations between government and corporation, was a generous and expensive subsidy probably far greater than necessary to induce investment in relatively high-grade deposits.

The Vatukoula Tax Agreement

In 1983 Emperor Gold Mining and Western Mining established joint ventures to conduct gold operations at Vatukoula.[16] The Vatukoula Joint Venture (with interests apportioned initially 90/10 percent and subsequently 80/20 percent between EGM and Western Mining, respectively) provided

for the working and development of the existing Emperor Mine. The Tavua Basin Joint Venture (with EGM and Western Mining holding equal interests) provided for exploration over adjacent areas of the Tavua basin. Westem Mining acted as manager under each joint venture. At the establishment of the ventures, Western acquired interests in EGM's mining and prospecting assets in proportion to the company's interests under the ventures.[17]

Exploration activity established the feasibility of further mining operations in a portion of the Tavua Basin Joint Venture tenements adjacent to the existing Emperor Mine. In 1985 a third joint venture, the Tavua Basin Mining Joint Venture, was established to develop and work the Philips shaft. As with the existing Tavua venture, EGM and Western Mining held equal interests. In 1987 EGM conveyed its interest in the new venture to Jubilee Mining Limited, a then-dormant subsidiary of its parent company, EML.

In 1991 EML acquired Western Mining from Western's Australian parent. According to EML's annual report that year, the acquisition was in part funded by the issue of fully paid-up shares in EML to the Australian parent, which thus remained indirectly interested in the Vatukoula operations. Tailings from the Emperor Mine were excluded from the Vatukoula Joint Venture and related asset sale.[18]

Exploration and mining activities under the Vatukoula Joint Venture, the Tavua Basin Joint Venture, and the Tavua Basin Mining Joint Venture benefit from a three-party agreement between the two original joint venturers (and their assigns) and the Fiji government concerning Fiji tax liabilities: the Vatukoula tax agreement.[19] The agreement operates from March 1983 to March 2004, or in the case of any new mine, to March 2011. Concessions granted by the agreement concern income tax, stamp duty, royalty, and customs and fiscal duties.[20]

The VTA is not supported by any special implementing legislation. It purports to rely upon provisions in existing tax legislation that authorize the finance minister to grant specific tax concessions.[21] The three general topics covered in the VTA are tax consequences of establishing the joint ventures, operations under the joint ventures, and withholding taxes on payments to nonresidents.

Establishing the Joint Ventures

On establishing Vatukoula JV and Tavua Basin JV, Western Mining acquired existing assets of EGM in proportion to its interests under the two joint ventures. Fiji tax rules would subject the asset sale to stamp duty,[22] require EGM to recognize for income-tax purposes any recouped depreciation,[23] and forbid Western Mining's deducting for income-tax purposes that portion of the

sale price concerning capital assets other than mining tenements.[24] The VTA annuls or even reverses each of these tax consequences of the asset sale. Stamp duties are waived,[25] EGM is relieved from recognizing any of the sale monies for income-tax purposes,[26] and Western Mining is granted an immediate write-off of its acquisition expenses.[27]

Operations

Mining operations are subject under Fiji law to income tax, royalties, customs and fiscal duties on imported supplies such as plant chemicals and explosives, and export tax. The VTA grants concessions on all but the export tax.

The operations of the joint ventures benefit from two independent income-tax concessions. The first and more significant is a seven-year tax holiday for any new mine.[28] "New mine" is defined as any mine established on the Tavua Basin JV tenements.[29] The Philips shaft established under the Tavua Basin Mining JV, with ore grades considerably richer than the very old Emperor Mine operated under the Vatukoula JV, benefits from this exemption.

A second concession is the accelerated write-off of exploration expenditure and capital expended in mining operations. Regular tax rules provide for such expenses to be written off by five equal deductions spread at the election of the taxpayer over an eight-year period.[30] The VTA permits these expenses to be wholly written off in the year incurred.[31] Alternatively, deductions may be postponed for up to eight years, thereby preserving their benefit until after any tax holiday.

Fiji law reserves precious metals to the state.[32] Royalties are the price paid to the state for their exploitation. The ***Mining Act*** requires the miner to pay the royalty "prescribed"; regulations to the act set gold royalty at 5 percent of the value of ore extracted. Royalties may be calculated alternatively, subject to the minister's consent, on a yield basis.[33] In essence the yield-formula royalty equals 10 percent of profits. Where profits exceed 20 percent in value of sales, the royalty rate increases by increments. The royalty formula in the mining regulations may be supplanted by terms in a mining lease, as the new joint mining leases issued to EGM and Western Mining at the 1983 commencement of their joint ventures did--the royalty was set at 2.5 percent of taxable income. An additional royalty due under the VTA (labeled "special royalty") insures the total royalty for a year equals 2.5 percent of taxable income before the deduction of any loss carried forward from earlier years. As is usual, royalty payments are deductible in calculating taxable income for income-tax purposes.

Fiji's customs legislation provides for the imposition of a customs duty,

fiscal duty, and export duty.[34] A general goods and services tax introduced in 1992 now replaces fiscal duty.[35] The VTA exempts from fiscal duty all equipment imported exclusively for use in mining operations.[36] Additionally, specified chemicals are exempted from customs duty. Customs duty on all other items is capped at 5 percent.[37]

No concessionary treatment is granted for the export duty on gold. At the commencement of the VTA in 1983, export duty on gold was 0 percent. Various rates of duty have applied in later years, ranging from 2 to 5 percent.

Withholding Taxes

Any consideration of Fiji tax liabilities must take into account the fact that these joint-venture companies are operating subsidiaries of foreign parents, thus requiring a look at the tax position of the whole group. VTA concessions on withholding taxes are not tax neutral and go a long way towards explaining why the parent EML derives much of its income by way of interest and know-how payments.

Fiji's ***Income Tax Act*** imposes withholding taxes on dividends, interest, and know-how payments made to nonresidents.[38] In the period under consideration, withholding tax on interest payments was 10 percent until 1984 and 15 percent thereafter.[39] The taxes on dividend and know-how payments were 15 percent until 1990 and 30 percent thereafter.[40] The withholding tax on know-how payments applies only to the profit component of a payment. Rates stipulated by the ***Income Tax Act*** may be reduced by double taxation provisions: for example, Fiji's 1990 treaty with Australia limits withholding taxes on interest to 10 percent and on dividends to 20 percent. The VTA exempts interest and know-how payments from withholding taxes but leaves in place the tax on dividends.[41]

The withholding-tax concessions are the most interesting VTA component. They prompt several questions. Why should the VTA deal with withholding taxes at all? Related to the first question, who is the intended beneficiary of the concessions? Finally, is the intended beneficiary the actual beneficiary or has a distortion arisen?

From a purely legal perspective the withholding-tax arrangements have no place in an agreement with Fiji joint venturers. Conceptually, withholding taxes are levied on the recipient of income and merely collected by the party making the payment. The substantive tax obligation waived is that of a third party, who is not party to the VTA. The situation is different from a commercial viewpoint. Commonly the benefit of a withholding-tax waiver is passed along to the party making the income payment. The benefit of abolishing withholding tax on interest income, for example, can pass to the

borrower as cheaper money. The commercial practice justifies the inclusion in the VTA of a concession that from the legal perspective concerns only a third party.

The obvious question arises about which party the government of Fiji intended to benefit with these particular withholding-tax concessions: income recipient or income payer. Here the pattern of concessions is revealing. Fiji's ***Income Tax Act*** has three relevant withholding taxes. Only two are waived: those on interest and know-how payments, which represent expenses to the Fiji joint venturers. The withholding tax retained concerns payments --dividends--on which no "commercial" benefit of waiver can be passed along to the party making the payment. The pattern of concession suggests the agreement's objective is to waive withholding taxes for the benefit of the joint venturers themselves rather than any third party.

Whether the intended beneficiary of the concession is the actual beneficiary will depend on the commercial practice of passing along the benefit. The practice presumes a competitive market with the parties, for example borrower and lender, dealing at arm's length. Where borrower and lender are related there is no assurance that the benefits of waivers will be passed along. The absence of arm's-length dealing does not of itself necessarily mean benefits will not be passed on. However, what is ultimately perverse about the withholding-tax concessions is that given the structure of the Emperor group the overall pattern of concession in the VTA almost guarantees such an outcome. As lender, supplier of know-how, and owner alike, the parent EML may extract money from the joint venturers as interest income, know-how payments, or dividend income. Retention of the withholding tax on dividends pushes the parent to ensure sums as large as plausibly possible are extracted as interest or know-how payments. Under such circumstances, no benefits will be passed on to the borrower or know-how user--the Fiji joint venturer. Thus the intended beneficiary will not benefit and the actual beneficiary becomes the parent company, EML.

The tendency for high interest and know-how payments to be charged by the parent to the joint venturers has a further consequence. Should the waiver produce the usual commercial outcomes, which are cheaper loans and know-how, then tax revenue forgone from the loan or know-how supplier would be balanced by increased tax revenue from the increased profitability of the joint venturers. The pattern of concession found in the VTA, however, encouraging as it does more expensive loans and know-how, insures lower profitability of the joint venturers. Rather than there being any balancing, further tax revenues are forgone.

If, as suggested by the pattern of concession under the VTA, withholding-tax relief is meant to benefit the joint venturers and not related third parties

such as EML, the concessions should be limited to loans and know-how from independent third parties. If loan capital and know-how is to come from the parent, the pattern of concession should be to waive the withholding tax on dividends alone. Loans and know-how will then be supplied at market rates or better. The balance that exists between debt and equity funding under the ***Income Tax Act*** in a wholly domestic situation will be restored. Tax forgone on dividend payments to the parent will be balanced by increased taxable profits of the joint venturer.

Structure, Finances, and Tax Contributions of EML, 1980-1992

EML is a listed company in Australia. Apart from the mining subsidiaries in Fiji, it has other subsidiaries incorporated variously in Fiji, New Zealand, Hong Kong, the United States, and Vanuatu. A number of these companies are dormant. Figure 3 depicts the corporate structure as of 1992.

Financial Analysis

The financial performance of the Emperor group of Fiji mining companies and joint ventures has fluctuated widely over the period 1980-1992. In general, while EML has made profits the main operating subsidiary, EGM, has recorded substantial losses and has effectively paid no income tax.

From the limited information available, it appears that EGM has borrowed heavily from its parent company (intercompany payables to EML totaled A$47 million in 1992) and that interest payable on these advances has run at approximately A$4 million per annum in recent years. During the period under review, EGM appears to have paid little or no taxes. Gold production averaged 80,000 ounces per year. At prices approaching US$400 per ounce, the revenue earned over the period would be in the order of US$400 million.

Since 1986 EML has been domiciled in the tax haven of the Isle of Man, although its shares are listed on the Australian Stock Exchange. A summary of financial indicators is presented in Table 1. Excluding the effect of associate-company losses and abnormal and extraordinary items, EML is a consistently profitable company that appears to pay no tax. Whether this is due in part or in whole to its domicile in the Isle of Man is unknown.

Associate-company losses refer to investment in Odin Mining. This investment was sold as a loss of A$4.1 million in 1993. Extraordinary and abnormal items relate in part to operations outside Fiji and thus their exclusion leads to the conclusion that EML's Fiji operations are extremely profitable. An analysis of the profitability of the Fiji operations appears in Table 2.

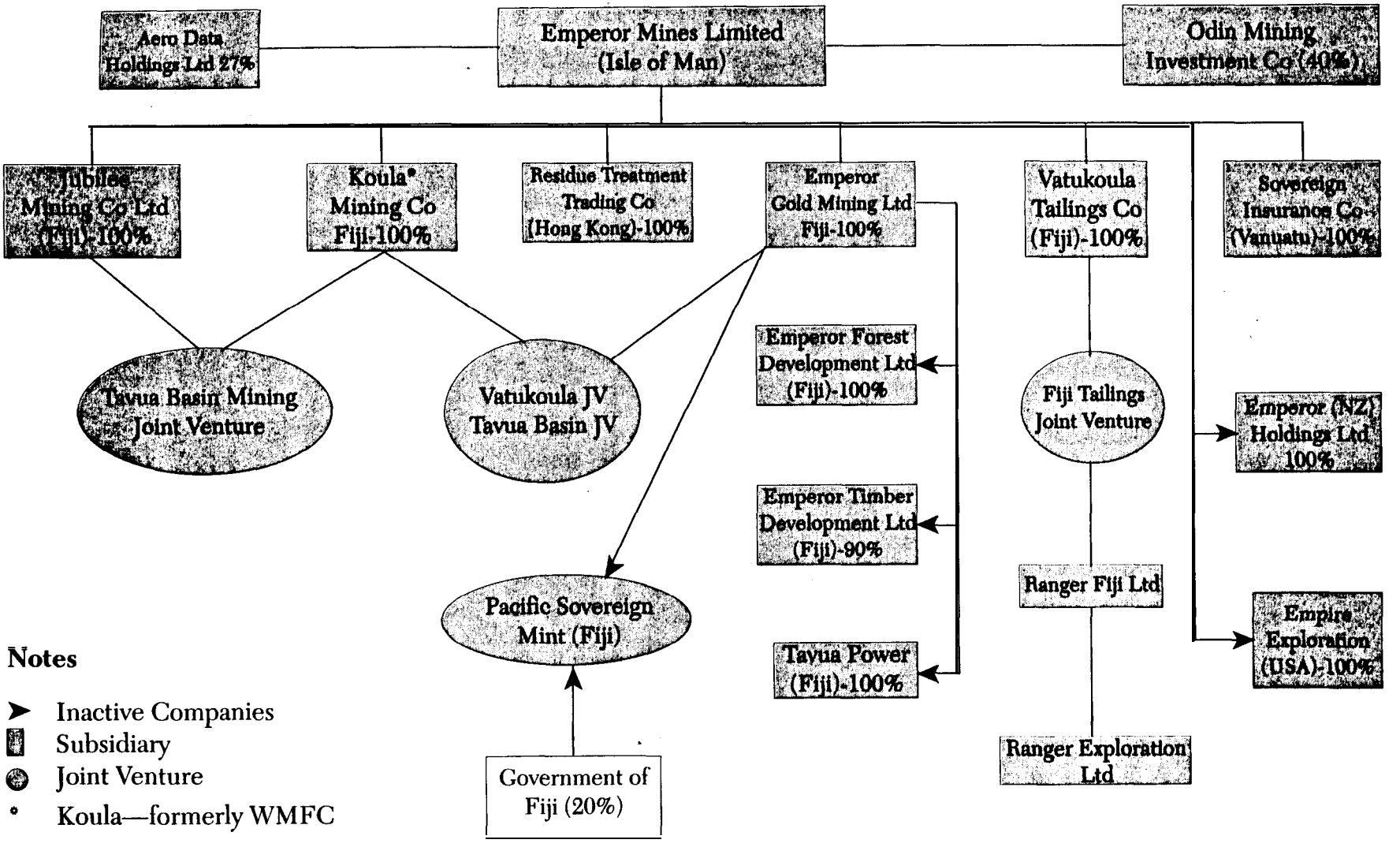

FIGURE 3. **Emperor Mines Limited corporate structure, 1992.**

TABLE 1. **EML Financial Performance, 1986–1992 (A$1,000s unless noted)**

	1992	1991	1990	1989	1988	1987	1986
Share Price (A$)	0.63[a]	0.51[a]	0.60[a]	3.90	3.10	6.10	3.25
Net Tangible Assets	75,442	73,196	86,694	93,569	96,037	67,964	43,457
Net Tangible Assets/share (A$)	1.23	1.19	1.55	1.70	1.88	1.40	0.99
Revenue	47,439	40,056	44,600	46,160	52,400	49,900	30,600
Operating Profits	1,080	(846)	(8,303)	13,822	14,390	22,676	3,220
– Associate Co. Contribution	722	(5,301)	(10,578)	(2,795)	(343)		
Operating Profits excluding Assoc.	358	4,455	2,275	16,617	14,733	22,676	3,220
Abnormal Items	2,245	(18,729)	(3,837)				
Extraordinary Items	722	(5,301)	(6,757)	(8,897)			
Income Tax Expense	0	(667)	(771)	618	581	(206)	327
Tax Payable	0	0	0	0	0	3	25

Source: Australian Stock Exchange 1992.

[a] A$0.10 shares.

TABLE 2. EML Profits Attributable to Fiji Operations (A$l,000s)

	1992	1991	1990	1989	1988	1987	1986
Operating Profit	358	4,455	2,275	16,617	14,733	22,676	3,220
Abnormal Expenses	2,147	(14,608)[a]	(2,687)				
Profits Attributable to Fiji Operations	2,505	(10,153)	(412)	16,617	14,733	22,676	3,220

Source: Australian Stock Exchange 1993.

[a] Result from charge to immediate write-off of normal development expenditure (previously this was written off over a rolling four-year mine life) and to write-off of ore stocks rendered inaccessible by fire and flood.

TABLE 3. Operating Profits of EML Subsidiaries in Fiji (A$l,000s)

	1992	1991	1990	1989	1988	1987
EGM	(10,589)	(19,648)	(14,662)	1,763	3,935	8,961
Koula Mining	3,019	4,903	5,469	7,109	1,824	(1,683)
Jubilee Mining	5,063	4,515	8,759	12,312	6,937	
Subtotal	(2,507)	(10,230)	(434)	21,184	12,696	7,278
Intercompany Loans	3,959	4,104				
Total	1,452	(6,126)				

Source: Australian Stock Exchange 1993.

From its Fiji gold-mining operations, therefore, EML appears to have reported profits of approximately A$50 million for the period 1986-1992. By way of contrast, the following analysis shows reported profits by EML's major Fiji operating companies and attempts to reconcile them to the parent-company figures (Table 3).

What is relatively clear is that the operations in Fiji were profitable. This can be demonstrated in two ways. If one relates profits to capital investment over the period 1986-1992, EML's return on assets averaged 12 percent. This average is likely an underestimate, as EML's assets include those outside Fiji. The second way is to compare return on assets with that of other mining operations. In the 1980s prior to its shutdown due to insurrection, Bougainville Copper Limited averaged a return on assets of 25 percent. Bougainville Copper, though, was considerably larger than Emperor. One Australian firm with revenues similar to that of Emperor was the Central Norseman Gold Corporation, which averaged 8.5 percent return on assets over the period 1986-1990.

However, the relative profitability of EML is not the issue--what remains of concern in light of its apparent profitability is the absence of direct com-

pany-tax payments. From the above analysis, incomplete though it is, two questions arise. First, why is the parent company reporting much higher profit over the period than the operating subsidiaries? Part of the answer may be due to intercompany indebtedness and interest thereon. The second question is, given that EML's operating subsidiaries have made substantial profits, why have they not paid tax in Fiji since the early 1980s? The Vatukoula tax agreement provides a reasonable explanation.

The Comparative Tax Position

How ordinary or extraordinary are the tax concessions found in the VTA? From a purely formal perspective, the concessions granted are not exceptional. All, with one minor exception, have a statutory basis in existing Fiji legislation. The joint venturers have received only tax advantages already envisaged and provided for in Fiji law.

Equally, from a comparative Fiji perspective the concessions are not exceptional either. Fiji's present ***Income Tax Act*** provides for a broad range of concessions to a variety of industries. For example, manufacturers of goods for export receive a thirteen-year tax holiday, exemption from customs and fiscal duties and the value-added tax, as well as tax-free repatriation of profits to foreign owners. Farmers may and have received tax holidays, most recently from 1986 to 1991. Parties investing in hotels and tourist watercraft benefit from accelerated depreciation and the waiver of withholding tax on interest. Seen against this range of concessions, the VTA appears unexceptional.

A quite different conclusion is reached if comparison is made with fiscal regimes imposed on mine operations in other jurisdictions. This is the more appropriate comparative perspective for at least two reasons. First, gold miners operate in a world market, not a Fiji market. Second, fiscal regimes for mines and other industries are not readily comparable. For example, a tax holiday has quite different implications for a mine, where operating profits will typically peak after capital-cost recovery in the early years, and for manufacturing, where steady growth may last indefinitely, with a corresponding profit profile.

What constitutes an appropriate taxation regime in cases where a resource is nonrenewable in nature and where economic rents are generated? There is in economics a long history of such analysis. This article will not consider these theoretical arguments; however, they have been made in mainstream economics with sufficient regularity for resource-rent regimes to be considered a standard part of the taxation rules employed in such situations (Garnaut and Clunies Ross 1983; see also Otto 1995 for a contemporary treatment of the subject).

Among developing countries, the VTA is unique for being the only taxation regime that has effectively exempted the developer from any income tax for a period of some twenty-seven years. Comparisons of tax statutes in different jurisdictions have been made in the past and their fundamental weakness is well understood (Grynberg and Powell 1992). In Table 4 we compare the VTA with actual contract terms, rather than legislation, in various countries that were examined in a recent Commonwealth Fund for Technical Co-operation report on mining regimes (1990:69-73): This report was commissioned by the Papua New Guinea government. The regimes examined in the Commonwealth Fund report have no names attached except in one case, as they are actual contract terms and hence are not normally available.

What is evident from Table 4 is that the VTA has the most generous terms of the actual contracts to be found in developing countries. The Fiji regime has no state-participation element (let alone one where the cost of state participation is borne by the developer), no additional-profits tax,[42] and almost invariably lower ad valorem taxes than the other agreements. Simply put, the VTA has no international parallel except for the case of Australia, which had no gold-mining taxes until the exemption was finally repealed in 1990 (Otto 1995:304). As a mining tax agreement the VTA is unprecedented, especially given the relatively high grades of ore in certain mines developed by the joint ventures.

Taxation Implications

The VTA effectively exempted the "new mines" owned by EML from taxation. In the years immediately following implementation of the VTA two companies, Jubilee and Koula, both products of the joint-venture agreement between Western Mining and EML, became active and both began to earn substantial profits (see Table 3). At the same time profits began to drop at EGM, which was not operating any "new mines" as defined under the VTA and hence was subject to taxes. The company appears to have become a significant liability. This condition is entirely consistent with a rational tax-planning approach to its operations.

The much higher recovered-gold rates of Jubilee and Koula can easily explain the disparities between the performance of the companies.[43] Other factors might offer a reasonable explanation as well. The ore from the high-grade Tavua basin operations was treated at Vatukoula JV treatment plant (Australian Stock Exchange 1993:3). In other words, the main operating cost of the facility was shared with the low-profit portion of the firm's Fiji operations. The methods of cost apportionment are unknown for this and other

TABLE 4. Comparison of Various International Mining Tax Contract Terms

Regime	Tax Holiday	Depreciation	Income Tax Rate	Additional-Profits Tax
VTA	7 years on new mine	accelerated	35%	none
A	none	accelerated	22.5% for 1st 4 yrs., 50% thereafter	3 tier: real thresholds at 15%, 20%, and 25%; tax rates of 5.6%, 11.8%, and 13.3% respectively
B	none	accelerated	37.5%	none
C	none	over 5 yrs.	35%	1 tier: threshold of 25% and tax rate of 35%
D	until investment recovery	over 10 yrs.	40%	3 tier: if ANCF[a] reaches 15%, 17.5%, and 20%, then rates rise to 50%, 60%, and 70% respectively
E[b]	none	over 8 yrs., some items accelerated	35% for 10 yrs., 45% thereafter	1 tier: 60% on profits above 15%

Source: For A-E, Commonwealth Fund for Technical Co-operation 1990:69-73.

[a] Accumulated net cash flow.

[b] Rio Tinto Zinc/Indonesia agreement, 1977. The additional-tax provisions are similar to the Bougainville Copper agreement.

Withholding Taxes	Royalty	Customs Duty	Export Tax	Equity / Other
exemption on interest & know-how	5% of tax-able income before any loss carried forward	exempt	0-5%: varies, was 3% in 1995	no free or carried equity
exemption on interest & dividend	3%	no exemption	none	10% free equity
no exemption	2.5%		none	10% free equity, 40% repayable from dividends
no exemption	1.25%	exemption until invest-ment recovery	3.75%-5%	20% free equity after investment recovery period, 5% tax on net cash flow
exemption on dividend until investment recovery	5%	no exemption	none	15% free equity after investment recovery
no exemption	3.75%	exempt 1st 10 yrs.	variable	option to buy up to 51% at predetermined price over 10 yrs.

overhead costs, which would almost certainly be shared between the "old mines" and tax-free "new mines."

The VTA has been a source of controversy within Fiji for a number of years. The Auditor Generals 1989 annual report cited several aspects of the agreement that were, in his opinion, questionable. The first was that the agreement appears never to have been gazetted.[44] Additionally, he noted that the VTA cost the government F$13 million in lost royalties. The report recommended the agreement be reviewed.[45]

Consideration of the EML Holding Company, which under the corporate restructure controlled the assets of the group, explains some of the behavior of the various firms under it. EML during this period was divided into the "Group," which included all the subsidiaries, and a "Holding Company," which, as the name implies, held the assets of related companies. In 1991 for example, the EML Holding Company made no revenue from trading; according to the company's annual report, all revenue from trading in that year was earned by the EML Group. According to EML's 1993 annual report, in 1992 the holding company earned revenues of A$6.4 million, of which A$3.36 million came from interest charged to subsidiaries (p. 27). The firm also earned A$1.2 million in dividends and A$1.34 million in "other income." The source of this income is unclear, but no doubt comprises management fees from its subsidiaries. In 1991 EML wrote off A$2.3 million in unrecoverable management fees charged to subsidiaries, according to its annual report (p. 29). EML Holding Company's main revenue source thus appears to be interest and management fees charged to its subsidiaries. Both fees are exempt under the VTA from the 15 percent withholding tax and these activities of the holding company, whose revenues constituted 12 percent of EML's 1992 total, may be one of the main mechanisms of tax avoidance for "old mine" companies such as EGM.

Tax Revenue from the Emperor Group in Fiji

The VTA's tax implications are of crucial interest in this study. The three tax sources of revenue from the Vatukoula mine are company tax, royalty, and export tax.

Though there were numerous revenue and cost centers, as the corporate structure depicted in Figure 3 demonstrates, the EML Group in Fiji during this period had three principal revenue centers: EGM, Jubilee Mining Company Limited, and Koula Mining Company Limited. These three companies were responsible for the vast majority of earnings in the period 1987-1992, but they appear to have paid almost no corporate tax in Fiji. Given the way in which reports are presented to the Registrar of Companies, it is im-

TABLE 5. Mining Royalties Paid to the Government of Fiji (F$)

	Sales Value during Period	5% of Sales Value[a]	Assessed Royalty Due[b]	Royalty Payment Received
June 1985	19,707,261	985,363	0	10,686 (1983)
1986	28,294,241	1,414,712	0	564 (1984)
1987	46,838,227	2,341,911	0	80 (1985)
1988	69,112,041	3,455,602	0	23,913 (1986)
1989	81,769,821	4,088,491	0	23,792 (1987)
1990	76,896,163	3,844,491	NA	23,744 (1989)
1991	48,460,806	2,423,040	NA	0 (1990)

Sources: Mineral Resources Department 1991; thereafter pers. corn., Mineral Resources Department.

[a] Royalty payable if paid under Regulation 6 of the *Mining Act.*

[b] Royalty payable at 2.5% of net profit upon which corporate income tax is levied. Amounts in this column as well as the 5% of sales value column are not received by the Fiji government but are published nonetheless by the Mineral Resources Department.

[c] Special royalty under provisions of the Vatukoula tax agreement. The date in parentheses is the year the amount was received.

TABLE 6. Nominal Levels and Effective Rates of Export Tax on Gold in Fiji

	Value of Gold Exports (F$)	Export Duty (F$)	Effective Rate (%)
1988	84,897,565	1,681,954	1.98
1989	76,196,661	1,719,547	2.25
1990	75,831,957	2,787,854	3.68
1991	44,758,887	945,985	2.11
1992	62,614,074	1,881,520	3.00

Source: Customs and Excise Department, Department of Finance, Suva, 1993.

possible to determine conclusively whether any tax was paid.[46] However, EML's report to the Australian Stock Exchange for the period 1986-1992 indicates income-tax expenses only in 1986, 1988, and 1989; the company last made provisions for tax in 1987 when it paid A$3,000 on a turnover of almost A$72 million and an operating profit for that year of A$22.7 million.[47]

Royalties paid by the mining industry are presented in Table 5. Interestingly, not only are these minuscule amounts, but they are often paid two years in arrears.[48]

Largely in response to the situation described above, the Fiji government has imposed export taxes of varying rates on gold (Table 6).[49] Since the VTA

does not prohibit export taxes, they have become the preferred device for dealing with the obvious loss of tax revenue from the mining operations. Export taxes, while ***revenue*** efficient, are the least ***economically*** efficient taxation instrument in mining operations, as they raise costs and shorten mine life.

Table 7 attempts to quantify the government's forgone revenues under the VTA. The estimates are based on the tax liability existing in Papua New Guinea, which by the standards of developing-country mining is widely considered to be quite reasonable.[50] The two estimates assume that EML should pay a rate of tax as a percentage of exports equivalent to the average paid to the PNG governments, both national and provincial, in the period 1986-1991.[51] The two estimates are based on the ratio of the contributions by mining companies to the PNG Mineral Resource Stabilisation Fund (MRSF) divided by exports. The lower estimate of 8.55 percent is based on the period 1986-1991 and the higher on the shorter period 1986-1989. The reason for the second estimate is that the Bougainville Copper Mine was closed by insurrection, and as a result contributions to the MRSF were negligible after 1990. In the period 1986-1989, the average government revenue as a percentage of exports in Papua New Guinea was 11.69 percent.[52] This contrasts sharply with the average take of the Fiji government of 1.87 percent of total exports between 1986 and 1992. Had the returns been the same as in Papua New Guinea, the very minimum that the Fiji government should have taken in revenue was F$37.04 million; using the higher estimate, the government should have received F$50.64 million. Thus the cost of the VTA is a subsidy of between F$4 and $6 million per annum to the company over the period. Assuming that the government objective is, as it has been throughout the last fifty years, the maintenance of employment at Vatukoula, the cost of the agreement is high. The government has been paying a subsidy over the period under study of between 42 and 62 percent of the gross salary of every employee of Emperor.[53]

Conclusion

Since the restructuring of EML following the Vatukoula tax agreement in 1983, there seems little doubt that EML has been profitable. EML has operated a tax-free mine in Fiji under the VTA's terms. The loss of revenue to the government, as calculated above, is significant. However, the real effect of the VTA is not simply that it has provided a mining company with an exceptional tax agreement, one that allowed the company to arrange its affairs to avoid taxes not only for new mines, but also for all its Fiji operations. What the government of Fiji has established is a precedent that may

TABLE 7. Estimated and Actual Government Revenues under Fiji and PNG Tax Regimes (F$)

	Paid by EML on Fiji Mining Operations					Hypothetical Revenue	
	Royalties	Export Taxes	Total Taxes	Exports	Total Taxes as % of Exports	Based on 8.55% of Exports	Based on 11.69% of Exports
1986	564	0	564	38,631,655	0.00	3,279,913	4,484,466
1987	80	0	80	50,557,000	0.00	4,322,624	5,910,113
1988	23,913	1,681,954	1,705,867	84,897,565	2.01	7,258,742	9,924,525
1989	23,792	1,719,547	1,743,339	76,196,661	2.29	6,514,815	8,907,390
1990	23,744	2,787,854	2,811,598	75,831,957	3.71	6,483,632	8,864,756
1991	0	945,985	945,985	44,758,887	2.11	3,826,885	5,232,314
1992	0	1,881,520	1,881,520	62,614,074	3.00	5,353,503	7,319,585
Total			9,088,953			37,040,113	50,643,149

well prove inescapable. The government is presently entertaining the possibility of granting a development permit to Placer Pacific to develop the huge but low-grade Namosi copper deposit. This project is being negotiated at the very time when investment in Fiji is at a low ebb. Investment as a percentage of GDP has fallen to 15 percent according to recent estimates, down from 24 percent at the beginning of the 1980s.

The government of Fiji wishes to promote investment so as to create employment. In many ways the situation that existed in the mid-1930s when investment dried up because of the Depression, is being replicated--but for quite different reasons. The failure of private investment in Fiji is very much a result of factors particular to the Fiji economy, whether they be political or economic, rather than a result of global recession. Since beginning its operations in Vatukoula in the 1930s Emperor has throughout most of its life received tax rebates, subsidies, export bounties, and interest-free loans. This is because the state has been willing to accept Emperor's arguments that if it failed to receive what it claimed were needed subsidies and concessions, the company would be forced to retrench large parts of its predominantly Fijian labor force.

The Namosi copper deposit is the largest project in the history of Fiji in terms of employment creation and investment. However, Placer Pacific claims that the deposits are very low-grade ore. Hence it will not be difficult for the developer to pursue a strategy of pressure on the Fiji government as EGM has done. There is every likelihood that the government will find it difficult to escape the precedent created by the VTA. In fact Placer already has applied precisely the same sort of pressure that was used by Emperor. In June of 1995 it ceased further exploration and development activities at Namosi pending a successful outcome of tax negotiations for the development of the mine. In light of the experience of EML and EGM, there is little reason for optimism that an appropriate tax regime will evolve.[54]

Given the mining tax situation currently existing in Fiji, one is at least obliged to suggest some form of alternative. The case of Papua New Guinea, which has been a source of uncomfortable and invidious comparison, is a basis from which to begin. The best aspect of the PNG system is its transparency and clarity, which contrast sharply to the situation in Fiji. A mine developer in Papua New Guinea knows exactly the tax regime it will confront because the terms are published and available in legislation. It is not possible to negotiate tax concessions without an act of Parliament. This puts considerable pressure on policymakers not to depart from the published terms. Thus, the very first measure needed to improve the mining tax situation in Fiji is a clear and published draft tax agreement. While a change in

legislation may provide a proper ***de jure*** framework, the ***de facto*** situation will not change until the discretionary power available to ministers and cabinet in Fiji is severely curtailed or eliminated. In light of the Vatukoula situation and the Namosi tax agreement, companies will feel, irrespective of whatever legislation or draft agreement is available, that they are in a position to negotiate terms until the power of the ministers and cabinet is tempered by the authority of Parliament to review such arrangements in open and transparent discussion.

Although the form of the PNG mining tax legislation would constitute a desirable transplant in Fiji, the content and substance of various parts of the PNG tax regime are less advisable. In particular, the much vaunted additional-profits tax--seen as the cornerstone of the PNG taxation regime--should not be introduced into Fiji. With rates of return that averaged 25 percent during the 1980s Bougainville Copper Limited managed to avoid additional-profits tax on all but one occasion.[55] In large part this was possible because the additional-profits tax regime negotiated with Bougainville Copper was based on a percentage rate of return. But cash-flow systems make possible distortions in behavior that result in avoidance of the additional-profits tax even in years when returns are very high.[56] What is needed in the case of Fiji is an additional-profits tax system based on the price of the resource, which is known and transparent. With such a basis, mining activity cannot be distorted to avoid the payment of tax. Moreover, these systems of taxation are easier to administer in countries where the tax administration is in need of strengthening.

In light of the Fiji government's commitment to the tax regime at Vatukoula, it is highly unlikely that the government would seek, or the company would agree to, a renegotiation of so exceptional and generous a taxation regime. The government is perfectly within its legal rights under the VTA to induce the company to negotiate a new agreement by raising the export tax on gold, which it has done on a number of occasions. Given that such a measure would be perceived by the private sector as an attempt to renege on an important agreement and hence further erode the state's credibility, it is unlikely that the government of Fiji would pursue such a course of action.

NOTES

1. Total mining employment at Vatukoula has varied between 650 and 1,800 since World War II.

2. This study reviews EGM and EML until the end of 1992. In 1993 there was a significant corporate restructuring.

3. Although the agreement expires in 2004, it is possible that a mine established in that year would enjoy a seven-year tax holiday.

4. EML was initially incorporated in 1935 in Victoria, Australia. In 1986, following a change in Australian tax law, the company was incorporated in the Isle of Man. EGM was incorporated in Fiji in September 1934.

5. *Fiji Times,* 26 July 1950, 6; 2 December 1950. EGM has consistently used the comparison with the Australian taxation regime to argue for cuts.

6. This was to be refunded by June 1961 (Taylor Report 1960:16). The tax had been paid from the government subsidy of 1958-1961 and by asking for a refund, the company was in fact asking for the waiving of port and service tax for the three years that had been paid for by the state in the first place.

7. What remains unclear from the historical records is whether the state agreed to further subsidy requests by EGM. Not the Siwatibau Report nor the parliamentary debates nor secondary sources appear to have suggested that EGM received the requested tax exemption.

8. The company announced its intention to close its operations at Vatukoula on 14 February 1977, when negotiations with the Mine Workers Union broke down.

9. Stinson played a key role in the defense of EGM and its interests in Fiji.

10. The opposition argued for the nationalization of the company.

11. It was subsequently announced that the company would lay off only 770 workers *(Fiji Times,* 17 January 1978, 1).

12. No details of the exact financial help offered to Emperor were made public. The government attempted to keep the agreement between the state, EGM, and Western Mining a secret. Some terms of the agreement have been published, however. Unlike all previous tax arrangements between the government and Emperor, this agreement was never debated on the floor of Parliament.

13. 'We do not believe that Government assistance to the Gold Mine over the past 10 years [is] excessive in light of all the economic and social benefits which the country would otherwise have lost if mining operations were to close down" (Siwatibau Report 1977:37, recommendation 3).

14. Address by the Emperor Gold Mines CEO, Mr. Colin Patterson, to the Fiji-Australia Business Council, 15 September 1996, Nadi.

15. The cost-of-production figures are derived by taking the average cost per tonne mined by EGM, multiplying by total tonnes of ore treated by Emperor, Loloma, and Dolphin Mines (the Emperor Group), and dividing by total Fiji exports of gold. The estimate is imperfect because tonnes treated does not necessarily equal tonnes mined and because the cost data are on a financial-year basis with output and exports on a calendar-year basis.

The results may overestimate operating profits/sales, as the method assumes that all gold exported from Fiji was exported by EGM and associated companies. However, as a counterweight, the figures do not include silver exports. The significance of the results lies in the trend rather than in the value of the actual profitability estimates.

16. Details of the joint ventures appear in the annual reports of the Fiji Mineral Resources Department and the annual returns of EGM, EML, Western Mining, and Jubilee Mines Limited for the years 1983 to 1993 (Registrar of Companies, Suva, Fiji).

17. The asset sale as it concerned exploration and mining tenements was implemented by the issue of new tenements to EGM and Western Mining, jointly covering areas previously held by EGM. Initial grants were Special Mining Lease (SML) S.54, Special Site Right (SSR) No. 4 and SSR No. 5 to EGM and Western Mining, apportioned 90/10 and subsequently 80/20; and Special Prospecting Licence (SPL) S.1201 and SML S.55 to EGM and Western 50/50. Subsequently SSR No. 4 was converted to SML No. 56. A further prospecting interest, SPL S.1271, was issued to EGM and Western 50/50 in 1985 and renewed in part in 1989. In 1989 SSR No. 5 was replaced by SSR No. 6 and SPL S.1208 was converted to SML S.1344.

18. During the eighties EGM had attempted to interest a number of foreign parties in working these tailings. Eventually a 50/50 joint venture was established in 1991 between the Vatukoula Tailings Company Limited (a wholly owned subsidiary of EML) and Ranger Fiji Limited to rework these Emperor Mine tailings. The venture was unsuccessful and tailings retreatment ceased at year's end, 1992. EGM subsequently acquired all assets of the two joint venturers and mothballed the retreatment facilities. Details of attempts to exploit tailings appear in the annual reports of the Mineral Resources Department and EML for the years 1985 to 1993.

19. The VTA, unlike the tax-free factory system that is widely publicized, has never been published by the Fiji government. Its terms are in the public domain, however, and have been published in Emberson-Bain 1994:197. Numerous newspaper articles in Fiji over the years have also discussed the VTA terms.

20. The relevant legislation for the period under review includes the *Income Tax Act* (1974) as amended, *Stamp Duties Act* (1971) as amended, *Mining Act* (1966) as amended, *Customs Act* (1986), and the *Customs Tariff Act* (1986) as amended.

21. The Fiji government is as much subject to the laws of Fiji as anyone else, of course, and may not contract to vary tax obligations imposed by law. Each of the concessions granted or to be granted under the VTA must be referable to the discretionary powers of the finance minister under existing legislation, and the minister's discretionary decision must be expressed in the form required by law. This article neither debates nor considers the legal efficacy of the VTA.

22. Stamp duty is tax imposed on documentation effecting a transaction. Duty is a small percentage of the transaction price with different transactions attracting differing rates of duty. The total price of the asset sale was F$4,200,000.

23. *Income Tax Act,* sect. 11(k).

24. Ibid., sects. 19(1), 23(2)(b).

25. *Stamp Duty Act,* sect. 4(a), empowers the finance minister to waive duty payable on any instrument.

26. Fiji does not tax capital gains. Asset sale monies would be a capital receipt for EGM and thus excluded from taxable income on general principles. Hence this provision benefits EGM with respect only to recouped depreciation included in taxable income under the *Income Tax Act,* sect. 11(k). There is no statutory foundation in the act for this concession.

27. *Income Tax Act,* sect. 23(4), empowers the finance minister to authorize an immediate write-off of acquisition costs.

28. *Income Tax Act,* sect. 16.2(a), empowers the finance minister to grant tax holidays to mining companies. From 1980 income-tax rates for resident companies were 37.5 percent and from June 1992, 35 percent *(ITA,* Fourth Schedule).

29. The VTA does not offer a geological or mining engineering definition of a new mine that would allow Fiji's commissioner of Inland Revenue to make a distinction between a new mine and new shafts.

30. *Income Tax Act,* sect. 23(1).

31. Relying on *Income Tax Act,* sect. 23(4); see note 21.

32. *Mining Act,* sect. 3.

33. Ibid., sect. 54; *Mining Regulations,* sects. 6, 7.

34. *Customs Act,* sect. 6.

35. Under terms of the *Value Added Tax Decree* (1992), export sectors are exempt from the value-added tax.

36. Relying on the *Customs Tariff Act* (1986), sect. 11.

37. Concessionary treatment on customs and fiscal duties is conditional upon plant, equipment, and services being purchased locally if local terms equal or better terms obtainable elsewhere. This standard condition, essentially to promote local industry, incidentally also discourages transfer pricing with regard to items available locally.

38. The tax on dividends is known as "nonresident dividend withholding tax" *(Income Tax Act,* sect. 8). This is a final tax (ibid., sect. 17[39]). The tax on interest is known as "interest withholding tax" and is not technically a final tax (ibid., sects. 9, 104[2]). Know-how payments are subject to the dividend withholding tax (ibid., sect. 8 [2][c]). As regards know-how payments this is technically not a final tax (ibid., sect. 104[1]).

39. Act 25 of 1983.

40. Decree No. 10, 1990.

41. Relying on the *Income Tax Act:* sect. 14(2)(e) regarding interest payments, sect. 8(6)(c) for know-how payments.

42. As of 1992, mining companies in Papua New Guinea have paid additional-profits tax on only one occasion, in 1980 (Commissioner of Revenue, pers. corn., 1992).

43. The new mines of the Tavua Basin Mine, which began operation in 1988, had recovery grades of 36.13 g/t in 1988, falling to 20.36 g/t in 1991. In May 1991 the mine was closed after a fire was set to the surface infrastructure. See Australian Stock Exchange 1993:2. By way of contrast PNG's Porgera gold mine, which is considered one of the richest high-grade deposits in the world, was recovering grades of 17.48 g/t in its underground phase in 1993 (see Papua New Guinea, Department of Minerals, *Quarterly Report,* September 1993, p. 23).

44. *Report of the Auditor General on the Accounts of the Government of Fiji for the Year 1989.* Paper No. 3 of 1991, vol 2:23. The Auditor General had asked the permanent secretary of Finance where the agreement had been gazetted. The authors have also been unable to find the gazettal notice.

45. In its reply to the Auditor General's statement, the Mineral Resources Department defended the decision to grant the VTA on the grounds that the development would not have occurred without the concessions. With recovery grades at 36 g/t, it is difficult to imagine that tax concessions of such magnitude were absolutely necessary. (The response of the Mineral Resources Department was provided by the Fiji Auditor General's Office.)

46. Emperor discusses at length its tax payments to the Fiji government in a full-page advertisement but does not suggest that in the 1987-1991 period it had paid any corporate tax *(Daily Post,* 5 September 1992, 38).

47. Australian Stock Exchange 1993:11. By way of facile comparison, Emperor's $3,000 corporate-tax liability in 1987, the last year in which it made provisions, was less than half the liability of a senior lecturer at the University of the South Pacific.

48. This includes royalties payable to the Mineral Resources Department under the terms of the *Mining Act.* Although no royalty is prescribed under the terms of the *Income Tax Act,* royalties were paid to the Department of Finance, under provisions of the VTA.

49. In December 1987 the government reintroduced a gold export tax, at 2 percent. In November 1989 it was raised to 5 percent and then lowered again to 2 percent in November 1991. This was raised once again, to 3 percent, in 1992.

50. Under the PNG mining tax regime existing in the late 1980s and early 1990s companies paid a royalty of 1.25 percent of FOB value to landowners and the provincial government. They paid 35 percent corporate tax and 70 percent of all positive net cash flow for rates of return above 20 percent or U.S. prime rate plus 12 percent. Companies also had to offer 20 percent equity to the government at historical cost.

51. This period was chosen because it corresponds to the period of the Vatukoula tax agreement under study. It does not include 1992 because in that year Papua New Guinea began to export oil and that sector, while contributing to the MRSF, is subject to a different tax regime than mining companies.

52. There remains one possible criticism of the approach taken above: that comparing EML and Bougainville is unfair as Bougainville is five times larger than Emperor; is open cast, capital intensive, and very profitable; and as a result the returns that the PNG government obtained during this period may reflect economies of scale rather than the mining taxation regime. There are two possible replies to this line of argument. Our estimate is based on MRSF returns divided by exports, which includes both the very large and profitable Bougainville Copper Limited operation and the equally large but totally unprofitable Ok Tedi Mine. Thus, Papua New Guinea's take of mineral exports is a reasonable reflection of what government can expect from both profitable and unprofitable mines. Moreover the lower estimate made above, of 8.55 percent of export revenue, is downward biased by including two years (1990 and 1991) when Bougainville Copper made no profits because its operations were closed. Throughout the period Ok Tedi paid nothing to the government except the 1.25 percent royalty. Therefore we contend that, despite the effect of economies of scale on government revenues, the 8.55 percent figure is not unreasonable for a reasonably profitable mine operating under a normal taxation regime in a developing country. It should be kept in mind that during the 1980s Bougainville Copper paid the PNG government some 20 percent of export earnings on average.

53. This is based on the assumption that the average gross annual wage in the period was F$8,000 per miner and that there was average employment of 1,200 over the period. See Emberson-Bain 1994:211.

54. Following the completion of this article in November 1995, Fiji's minister of Trade and Commerce, the Hon. Jim Ah Koy, announced during the 1996 budget session in Parliament that he had come to an agreement with Placer over a tax agreement for the Namosi mine. While he did not clarify its exact nature, he did say that the tax agreement Placer had been offered was "slightly more than what is offered to the Tax Free Factories and would encourage the company to continue further development work and progress towards a full feasibility study on proposed mine" *(Fiji Times,* 4 December 1995, 7). If this is in fact the case, then Placer will be offered a minimum thirteen-year tax holiday, duty-free imports, and exemption from Fiji's value-added tax.

55. PNG Commissioner of Taxation, pers. corn., 1992.

56. Bougainville Copper's additional-profits tax was not based on a percentage of cash flow. Rather, a 15 percent rate of return on investment was used as the threshold where the additional-profits tax became operative. This created enormous potential for stockpiling and other activities to raise inventory investment and thereby decrease apparent rates of return on investment.

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EDITOR'S FORUM

DO PACIFIC ISLANDERS IN THE UNITED STATES HOLD "GOOD" JOBS?

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In the 1980s, the quality of the jobs held by Pacific Islander householders generally improved, while job quality declined for the average American householder. The factors that improved the economic position of a Pacific Islander household--education, work experience, and good English language skills--were the same as those that improved the position of other households in the United States. Over the decade, Pacific Islander householders generally acquired more of these productive attributes, which helped them to find employment, get access to better jobs, and increase their earnings.

AFTER GROWING STRONGLY in the 1960s and 1970s average real earnings in the United States stagnated in the 1980s and poverty rates, after falling since the mid-1960s began to climb. Income inequality, after improving modestly, also worsened. There was widespread concern that changes in the world economy were destroying "good" jobs, jobs that paid a "living wage" sufficient to support a household at a reasonable standard of living. Further, racial minorities, including Pacific Islanders, were thought to have suffered even greater declines than other groups in the United States.

Many Pacific Islanders in the United States started the decade in a weaker economic position than the average U.S. resident. For example, in 1979 mean household income in the United States, based on the 1980 census, was $20,306, but for Pacific Islanders it was $19,016 (U.S. Bureau of

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the Census 1988). Because of the much larger size of Pacific Islands households, Pacific Islander groups had lower per capita household income: $7,411 for all US. households and $5,153 for Pacific Islander households. As a consequence of their lower incomes and larger family sizes, the incidence of poverty among Pacific Islanders (16.1 percent) was almost twice the U.S. average (9.6 percent; U.S. Bureau of the Census 1988).

Labor-force participation rates and employment rates of Pacific Islanders were similar to those of other U.S. residents; the lower incomes came from lower earnings rather than from lower participation in the workforce. The lower earnings were, in turn, related to a higher incidence of unemployment during the year than for all U.S. workers; holding a job lower on the occupational ladder; lower educational attainment than other U.S. residents; and a weaker command of spoken English among island-born persons compared to all Americans (about a 10 percentage point difference).[1]

Given the weaker economic position of Pacific Islanders in 1980 and the adverse economic developments of the 1980s how did Pacific Islanders fare? This article investigates one dimension of the economic success of Pacific Islanders, namely, the proportion of jobs held by Pacific Islanders that are "good" jobs, and whether this changed over the 1980s. Finally, it will determine the factors that affect whether a Pacific Islands householder holds a-"good" job.[2]

What Is a "Good" Job?

The first task is to define what is meant by a "good' job. Given a choice, a person will prefer a "good" job to a "bad" job. But what do these terms mean? A "good" job is likely to be described as challenging or interesting, as having social importance, as using a person's skills, as having prospects for promotion, as having variety and entailing responsibility, and, most likely, as paying well. A "bad" job is the opposite. The problem for research is how to capture the essence of these characteristics. The task is made more difficult because it is not clear that all individuals value the same characteristics or, if they do, that they value them equally. But most people value money--not necessarily for itself, but for what it allows individuals to provide for themselves or their families--and link high pay to a "good" job and low pay to a "bad" job. It is also likely that other "good" characteristics of a job are positively correlated with pay. There are exceptions to this. One reviewer of this article noted that some Samoans he or she has known have given up well-paid jobs for intermittent jobs at minimum wage, simply to be able to "go home" to family weddings and funerals. For these individuals, a "good" job was one that they could leave frequently and without permission. However,

such behavior does not appear to be widespread. The data in Table 1 below on income and weeks worked per year indicate that most Pacific Islander householders have strong, persistent ties to the labor market. By and large, it is pay that is most often used to differentiate a "good" job from a "bad" job, and for this reason it is used in this study.

The next issue to face is how much pay makes a job a "good" job? Again, there is no agreement, since it is not simply a statistical issue, but involves value judgments. The minimum wage and the poverty level are possible definitions of an income level sufficient for an individual to support a household. In 1993 the minimum wage was $4.25 per hour or $8,840 per year and the poverty level about $7.00 per hour or $14,350 for a single-earner family of four. Many would reject these levels as too low, since poverty-level income is associated with poorer health, less education, greater exposure to social stress and crime, and diminished economic prospects. Further, growing up in a poor family increases the chance that an individual will experience poverty as an adult (Gottschalk, McLanahan, and Sandefur 1994: 100).

In a national study of jobs and earnings carried out for the Urban Institute, Levy and Michel (1991) defined a "good" job as one that would support a "middle-class" living standard or as one that generates sufficient income to place the jobholder in the middle of the earnings distribution. In their study this income was somewhat above 150 percent of the poverty level for a family of four. This definition also makes the current poverty line comparable, in normative terms, to the original threshold established in the 1960s (Ruggles 1990). This value, about 150 percent of the poverty line, has been arrived at independently by other researchers (Schwartz and Volgy 1992; Scott 1993) and appears to represent something of a consensus definition of a "good" job.

In this study four income-based definitions of a "good" job are investigated: the most conservative definition of a "good" job is one that pays wages at least at the poverty line. Higher income requirements are also used to define a "good" job, namely 150 percent, 200 percent, and 300 percent of the poverty line figure. Each of these definitions is based on the poverty-level income. The poverty level is defined by the Office of Management and Budget as that amount of money required to sustain a family of a particular size and age composition. The core of the definition is the amount of money needed to purchase a least-cost nutritionally adequate food plan. Since families spend one-third of their budget on food, the poverty level is roughly three times the value of the core food budget. The poverty level varies for each household depending on its size, the presence of children under the age of eighteen, and the age of the householder. For a household of five in 1979, the poverty level was $9,923; and in 1989 it was $15,702.[3] The poverty line seems to be a reasonable lower cutoff, because if a job is not

able to pay enough to keep a family out of poverty, it is unlikely to be considered by many to be a good job, no matter what its other characteristics.[4] A somewhat higher threshold for defining a "good" job, 150 percent of poverty, indicates a fairly broadly accepted definition of a "good" job; and 200 percent and 300 percent of poverty would be accepted by most investigators as defining a "good" job, or even a "very good" job. These definitions would seem to embrace a reasonable span of income-based definitions of a "good" job.[5]

Pacific Islanders in the United States are part of American society, just as they are part of their home island society. As such, like other residents of the United States, their well-being and that of their family is affected by their income, although not necessarily defined by it. Low income in the United States makes it harder for them to meet the material demands placed on them by family and friends both in the United States and at home, so an income-based definition, although far from perfect, seems a reasonable approach.

Data

The data used in this study are taken from the computerized Public Use Micro Sample (PUMS) of the 1980 and 1990 censuses of the United States: The 1980 PUMS is a 1 percent sample of the U.S. population, and the 1990 PUMS a 5 percent sample. Pacific Islanders are defined in this study on the basis of the race question in the census. Since the focus of the study is on Polynesians, Micronesians, and Melanesians who came to the United States as migrants or are the descendants of migrants, Hawaiians are not included in the analysis. Further, since the economic position of Hawaiians is superior to that of other Pacific Islander groups (their 1989 household income is 10 percent to 30 percent higher and their poverty rate 50 percent to 90 percent of that of most groups) and Hawaiians are the largest Pacific Islander group, the inclusion of Hawaiians would cloud the situation of the other Pacific Islands groups.[6]

According to the U.S. Bureau of the Census (1993), in the 1990 census Hawaiians were 58 percent of the total Pacific Islander population of 365,024; Samoans were 17 percent, Guamanians 14 percent, Tongans 5 percent, and Fijians 2 percent. Palauans, Northern Mariana Islanders, and Tahitians were each less than one-half of 1 percent of Pacific Islanders.[7] The majority of Tongans (61 percent) and Melanesians (78 percent) were born outside the United States and can be considered first-generation migrants. Only 23 percent of Samoans and 18 percent of Micronesians were foreign-born, so most of these individuals are second- or higher order generation migrants. These percentages are biased down for at least two reasons. The first is the ten-

dency to report "born in the United States" to avoid immigration problems, and the second is a Bureau of the Census coding procedure that treats American Samoans and certain groups of Micronesians as born in the United States.[8]

The unit of observation is the householder. Because the focus of the study is "good" jobs, I have limited my investigation to householders between sixteen and sixty-five years of age. The 1980 PUMS contains 138 such households, 108 of which had an employed householder, and the 1990 PUMS contains 1,114 such households, 884 with an employed householder. I focus on the householder because the concern over the disappearance of "good" jobs addresses the question "can a job support a household?" not "can ***all*** of the jobs held by members of a household support that household?" The focus on the earnings of just the householder gives a somewhat grimmer picture of the economic position of Pacific Islander households than would a focus on the income of all earners in the household. But other members of the household may work because the head is unable to earn sufficient income and would choose not to work if the head had a "good" job. There are a number of interesting questions that can be asked about the jobs, pay, income, and standard of living of Pacific Islanders in the United States. Each requires a somewhat different approach or treatment of available data, and some cannot be answered with existing data.

Empirical Results

While I refer to Pacific Islanders collectively, there are sizable differences among groups and also among different locations for a particular group. For example, the poverty rate of Guamanians, Melanesians, and Micronesians in 1989 was only 50 percent of that of Samoans and Tongans, and poverty rates of Pacific Islanders in Hawai'i were about twice those in California. Thus, in the analysis to follow, the situation for Samoans and Tongans will be below the average for all Pacific Islanders, and the situation in Hawai'i will be worse than that in California. The size of the PUMS sample is too small to carry out a separate analysis for each group and each location.

Table 1 reports some descriptive statistics on the samples of employed householders for 1980 and 1990. The sample for 1990 is quite large, but that for 1980 is small. Since there is so little information available about Pacific Islanders in the United States, I have decided to present the information for 1980, recognizing that it may lack precision and that differences between 1980 and 1990 in individual characteristics may reflect sampling bias in the 1980 sample.[9] Between 1980 and 1990 the average age of Pacific Islander householders rose almost a year, work experience rose by a half-year,

TABLE 1. Descriptive Statistics on Pacific Islander Householders: 1980 and 1990

	1980	1990
Age (years)	35.8	36.6
Education (years)	12.2	12.6
Work experience (years)	17.6	18.1
Disability (%)	5.5	4.2
Speak English well or very well (%)	97.2	95.1
Female (%)	9.3	18.2
Occupations (%)		
Service	18.5	16.9
Farm	1.9	4.0
Blue collar	39.8	41.0
White collar	39.8	38.1
Weeks worked	47.0	46.5
Hours per week	40.7	41.0
Annual hours	1,942	1,988
Sample size	108	884

Sources: Calculated by the author from data in the computerized 1980 and 1990 Public Use Micro Samples, U.S. Bureau of the Census, Washington, D.C.

Note: Employed household heads aged sixteen to sixty-five years.

and years of education rose by one-third of a year. The incidence of a work disability dropped nearly one and one-half percentage points, and the percentage who spoke English well or very well dropped by two percentage points. This last change was the only one discussed thus far that would tend to worsen the economic position of the household. Changes in the occupational distribution were slight: almost 40 percent were blue-collar workers (farming, forestry, fisheries, operators, fabricators, laborers, precision production, craft, and repair), 40 percent white-collar workers (managerial, professional, technical, sales, and administrative), and almost 20 percent service workers. Although weeks worked in 1989 were slightly lower than in 1979, hours worked per week were higher, yielding a larger number of hours worked in 1989 than in 1979.

What Proportion of Pacific Islanders Hold a "Good" Job?

The debate over the disappearance of "good" jobs is primarily a debate over the quality of the jobs that exist. As a consequence, the "quality" of the jobs held by people who have them will be discussed first. That is, a distinction is made between jobholders with a "good" job and those with a "bad" job. Often combined with the debate on the quality of jobs is a concern over the disap-

TABLE 2. Percentage of Pacific Islander Householders Who Hold a "Good" Job: 1980 and 1990

	Employed		All	
Earnings	1980	1990	1980	1990
Less than poverty	28	31	44	45
Greater than or equal to poverty but less than 150% poverty	28	23	22	18
Greater than or equal to 150% poverty but less than 200% poverty	19	13	14	11
Greater than or equal to 200% poverty but less than 300% poverty	13	19	11	15
Greater than or equal to 300% poverty	12	14	9	11

Sources: See Table 1.

pearance of jobs no matter what they pay. Thus, data on jobholding among Pacific Islanders will also be discussed. Information on the two concerns--whether a householder has a job and whether that job is a "good" job--will then be combined to estimate the proportion of all householders that hold a "good" job. That is, I distinguish among those that hold a "good" job and those that either hold a low-paying job, are unemployed, or are not in the labor force at all.

The percentages of employed Pacific Islander householders who hold a "good" job are reported in the first two columns of Table 2. There are several things to note. First, almost one-third of Pacific Islander householders are "working poor." That is, the job the householder has pays insufficiently to bring the household above the poverty line. That does not necessarily mean that such a household is in poverty. Other household members may work and earn enough to bring the family above the poverty line. Second, if we accept 150 percent of poverty level as the most widely accepted definition of a "good" job, in 1980 only 44 percent of employed Pacific Islander householders (excluding Hawaiians) held "good" jobs, and in 1990, 46 percent did so. Third, in 1980 about one in eight employed Pacific Islander householders held a job that paid more than 300 percent of poverty level. In 1990 the figure was one in seven. Fourth, the overall picture is one of improvement. Although the percentage of working poor rose by three percentage points, improvement occurred at other levels of the distribution. Especially notable are the declines in the percentage of householders earn-

ing between poverty-level income and 200 percent of poverty level and the gain in the percentage earning above 200 percent of the poverty line. Thus, for most employed Pacific Islander householders, the difficult decade of the 1980s saw economic improvement.

The two-point increase in the percentage of Pacific Islander householders with "good" jobs, paying greater than or equal to 150 percent of poverty level, can be decomposed into a part due to changes in the characteristics of the householders (education, experience, disability, and so on) and a part due to the returns to these characteristics. When this is done, we find that one-half of the increase was due to changes in the characteristics of the householders and one-half to changes in the returns to these characteristics.[10] Increases in education and work experience contributed importantly to the improved situation. If they had not increased over the decade, the percentage of householders with jobs paying at least 150 percent of poverty would have remained at 44 percent.[11]

It is possible that households headed by men and by women fared differently over the 1980s. For the nation as a whole, Levy and Michel (1991) found evidence of a decline in the number of "good" jobs for males but an increase for women. When male- and female-headed Pacific Islander households, are examined separately, it is clear that male-headed households were in a much better economic position than those headed by women: 29 percent of male-headed householders were working poor in 1990, while 42 percent of female-headed households were in this category (Table 3). Over the 1980s female-headed households exhibit the same pattern as males, a larger proportion of working householders earn below poverty but a larger percentage earn more than 150 percent of poverty (not shown). Changes for females need to be handled with caution, since the 1980 sample contains only ten

TABLE 3. Percentage of Pacific Islander Householders Who Hold a "Good" Job by Sex: 1980 and 1990

	1980		1990	
Job Pays	Male	Female	Male	Female
Greater than or equal to poverty	72	70	71	58
Greater than or equal to 150% poverty	46	30	48	37
Greater than or equal to 200% poverty	26	20	35	27
Greater than or equal to 300% poverty	12	10	15	10
Sample size	98	10	723	161

Sources: See Table 1.

households. However, the improvement for Pacific Islands women noted here is consistent with the improvement found for female-headed householders in the United States (see Ahlburg, Song, and Leitz 1995).

While the economic position of Pacific Islander householders improved over the 1980s, it is still below that of other U.S. residents. Although I am not aware of comparable data for the entire United States, a similar study has been carried out for the state of Minnesota (Ahlburg, Song, and Leitz 1995). In 1980 and 1990, only 15 percent of Minnesota householders were working poor, half the percentage of Pacific Islanders, while 30 percent earned more than 300 percent of poverty, more than twice the Pacific Islander figure. The figures for Pacific Islander working poor are better than those for Native Americans in Minnesota but worse than those of "Blacks." Pacific Islanders held "very good" jobs at about the same rate as Native Americans in Minnesota, but at only half the rate of Blacks. The Minnesota figures are a reasonable basis for comparison, since the Minnesota and U.S. data on average annual pay, disposable personal income, and median household income were almost identical in 1990. Poverty was a little higher in the United States, 13 percent, than the 10 percent in Minnesota (U.S. Bureau of the Census 1993).

Who Has a "Good" Job?

Methodological Issues

In this section the characteristics of employed heads of Pacific Islander households that are associated with holding a "good' job are discussed. These determinants are identified by regression analysis. The regression equations that identify variables that are associated with an employed head of household holding a "good" job are reported in the Appendix. These regressions are probit regressions, not ordinary least squares regressions. Because the variable of interest is a zero-or-one variable (you either have a "good" job or you do not), ordinary least squares regression is not appropriate (Griffiths, Hill, and Judge 1993). Table 4 reports the effects of each variable on the probability that a householder will hold a "good" job (these are called "marginal effects" and are calculated from the regression coefficients but are not equal to them, because the probit regressions are nonlinear).

For example, the marginal effect of years of education in 1990 in the first column of results in Table 4 means that for each extra year of education completed, the probability of a householder having a job that paid above the poverty level increased by 3.4 percentage points. A Pacific Islander householder with an eighth-grade education was thus 27 percent less likely to have a good job in 1990 than was a college graduate. The marginal effect of dummy variables such as sex, English language facility, presence of a disabil-

TABLE 4. Percentage Effects of Determinants of Having a "Good" Job: 1990

	Definitions of a "Good" Job			
	Greater than or Equal to Poverty	Greater than or Equal to 150% Poverty	Greater than or Equal to 200% Poverty	Greater than or Equal to 300% Poverty
Years of education	3.4**	5.4**	4.5**	2.3**
Years of work experience	1.8**	1.1	0.1	0.2
Years of experience squared	-0.0**	0.0	0.0	0.0
Sex	-14.4**	-10.6*	-10.2**	-5.3*
Disability	1.5	-13.0	-8.1	3.6
Speak English	16.1*	20.6*	28.4**	10.3
Service occupation	-25.3**	-24.3**	20.0**	-7.9**
Farm occupation	-14.5	-23.6*	-30.7**	-71.0
Blue-collar occupation	-10.0**	-2.4	-6.8	-4.3*

Source: Calculated from regressions in the Appendix.

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

ity, and job type are measured relative to the effect of the category omitted from the regression. For sex, the effect is for being a female householder rather than a male; for English language, it is speaking English well or very well relative to speaking it less well or not at all; for service, farm, and blue-collar occupations, it is being in these occupations rather than in white-collar occupations; and for disability, it is the impact of having a disability that limits work relative to having no disability. For example, in the first column of regression results in Table 4, a female householder of the same experience, education, job sector, disability status, and English language ability as a comparable male householder was 14.4 percentage points less likely to hold a job paying above the poverty line than that comparable male householder. These estimates assume that the factors that affect the probability of holding a "good" job are those shown in the tables. If there are other factors that affect this probability and are correlated with the factors we are considering in Table 4, gender for example, then our estimates are biased. That is, they are either too big or too small.

Regression Results

So what affects the probability that an employed Pacific Islander householder held a "good" job, that is, one paying more than the poverty level?

Table 4 reports the regression results for 1990 only. Few of the variables were statistically significantly related to holding a "good" job in 1980. This result probably reflects the small size of the sample of households. In 1990 each additional year of education was associated with a 3.4 percentage point higher probability of holding a job paying above the poverty level. This finding is consistent with evidence on the importance of education to economic success in the United States, an importance that increased during the 1980s (Levy and Michel 1991:28). Education is even more important in holding a job paying 150 percent of poverty level. Each year of education increases the probability by a little over five percentage points. The impact is somewhat smaller for even better paying jobs. The message of these findings is clear: without adequate education the chances of earning an adequate income are very slim.

Also important to obtaining a good job is a good command of English. A householder who speaks English well or very well is 16 percent more likely to have a job paying above poverty level, 21 percent more likely to have a job paying at least 150 percent of poverty level, and 28 percent more likely to have a job paying at least 200 percent of poverty level than is a householder who speaks English less well. English proficiency is not statistically significant for a definition of a "good" job at 300 percent of poverty-level income. There are a few possible explanations for the latter finding: first, some individuals possess unique skills that are in such great demand that employers ignore language deficiencies; second, these individuals are employed in racial/ethnic labor markets where English is not required; or third, the sample is too small to yield a precise estimate of the effect.

Additional years of work experience increased the probability of having a job that paid above the poverty level by about 2 percent per year of experience but did not significantly affect the probability of holding an even higher paying job. Having a work-limiting disability did not affect the probability of having a "good" job once employed, although it may decrease the probability of being in the labor force. Indeed, this turned out to be the case. In a regression with labor-force participation as the dependent variable, disability reduced the probability of being in the labor force by 23 percent, controlling for other variables that affect participation.

The occupation of the householder matters. Those in services were 25 percent less likely to hold a job paying above poverty level than those in white-collar occupations, while those holding agricultural jobs were 15 percent less likely to do so. Blue-collar workers were 10 percent more likely than comparable white-collar workers to be "working poor." The penalty for being in a service occupation was similar for jobs paying greater than 150 percent or greater than 200 percent of poverty level, and the penalty for being an agri-

cultural worker was about double that for poverty-level jobs. The occupational differences for "very good" jobs are small and those for farm workers imprecisely estimated. It is possible that individuals in high-paying blue-collar or service occupations are well-placed managers or owners of blue-collar businesses.

Gender effects were large and statistically significant (Table 4). Female householders were 14 percent less likely than comparable men to hold a job paying above the poverty level. The gender differentials were smaller for higher-paying jobs: 10 percent for 150 percent and 200 percent of poverty level, and 5 percent for 300 percent of poverty level.

What Proportion of All Householders Hold a "Good" Job?

To determine the proportion of all householders with a "good" job, we need to know the percentage who are employed, that is, the employment ratio, as well as the percentage of the employed who have "good" jobs. The employment ratios are shown in Table 5, along with the percentage of all householders in the labor force. In 1990 most male and two-thirds of female householders were in the labor force, and the vast majority of these were employed. The percentage of males in the labor force was greater than in 1980 but the percentage employed was lower. The small sample size for females in 1980 makes the reliability of the estimates for women doubtful.

The second panel of Table 2 presents data on the percentage of all householders sixteen to sixty-five years of age who hold a "good" job, obtained by multiplying the percentage of employed householders with "good" jobs by the percentage of all householders with a job (the employment ratio). That is, this panel distinguishes those with a "good" job from those who have a "bad" job, are unemployed, or who are out of the labor force. The data show that a slightly larger percentage of Pacific Islander householders had a job paying above poverty in 1990 than in 1980, because although 3 percent of

TABLE 5. Percentage of Pacific Islander Householders in the Labor Force and Employed, by Sex: 1980 and 1990

	1980			1990		
	All	Male	Female	All	Male	Female
Percent in the labor force	80	89	42	84	91	65
Percent employed (employment ratio)	78	88	38	79	86	60

Sources: See Table 1.

jobholders saw earnings slip below poverty, there was a small increase in the percentage of householders who were employed, from 78.3 percent in 1980 to 79.4 percent in 1990. The percentages of all householders with "good" jobs at other definitions are lower than among employed householders, because the data now include nonemployed householders.

In considering these data, it should be kept in mind that the group that does not have a "good" job is diverse. Some members of the group are not in the labor market and so are not even looking for a job. Some of these people may have once looked but have since given up. Many have not. Some are unemployed and are thus actively seeking work. Others are employed, but the jobs they hold do not pay well. Finally, it cannot be assumed that all of these people could acquire "good" jobs even if those jobs became available tomorrow.

In addition to the gains made by the householder, the economic position of the household was further enhanced by an increase in the number-of workers per household: a one point decline in the percentage of families with no workers, a six point decline in the percentage with only one worker, and a commensurate 3.6 point increase for two workers and 3.5 point increase for families with three or more workers (U.S. Bureau of the Census 1988, 1993).

Conclusion

Although the economic position of employed Pacific Islander heads of households still compares unfavorably with that of the average American householder, Pacific Islander householders saw an improvement in their economic position over the 1980s, while the average American householder experienced a decline. While the percentage of workers earning less than the poverty level increased slightly, there were marked increases in the percentage of householders earning well above the poverty level. There was also a slight increase in the percentage of householders who were employed. When data on jobholding and the quality of jobs are combined, the improvement in the economic position of many Pacific Islander households is clear: in 1980 34 percent of all householders earned more than 150 percent of the poverty level; by 1990 37 percent did. Gains were especially marked above 200 percent of poverty: 20 percent of households in 1980 and 26 percent in 1990. Offsetting these gains was the fact that a slightly higher percentage of all householders had income below the poverty line in 1990 than in 1980.[12] As a consequence of these shifts, earnings inequality among Pacific Islanders grew, as it did in the nation as a whole.

The factors that improved the economic position of a household within

all Pacific Islander households--education, work experience, and good English language skills--are the same as those that improve the position of a household in the overall society. Over the 1980s, Pacific Islander householders acquired more of these productive attributes, which helped them to find employment, get access to better jobs, and increase their earnings in the kinds of jobs they had been holding, all of which improved the position of their households. Not only have some productive characteristics increased, but the importance of at least one of these--education--and possibly others increased over the 1980s in the United States. Thus, while many Americans saw their economic position weaken in the 1980s this does not appear to have been the case for many Pacific Islanders.

APPENDIX
Results of Regression Equations for Determinants of "Good" Jobs: 1990

	"Good" Job Paying			
	Greater than or Equal to Poverty	Greater than or Equal to 150% of Poverty	Greater than or Equal to 200% of Poverty	Greater than or Equal to 300% of Poverty
Education	0.099 (0.025)**	0.137 (0.025)**	0.126 (0.025)**	0.139 (0.030)**
Experience	0.052 (0.016)**	0.027 (0.015)	0.003 (0.016)	0.013 (0.019)
Experience squared	-0.001 (0.000)**	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Sex	-0.414 (0.123)**	-0.267 (0.122)*	-0.288 (0.128)*	-0.318 (0.161)*
Disability	0.042 (0.236)	-0.328 (0.234)	-0.230 (0.244)	0.217 (0.216)
English	0.462 (0.210)*	0.521 (0.237)*	0.800 (0.306)**	0.621 (0.438)
Service	-0.727 (0.137)**	-0.615 (0.139)**	-0.562 (0.147)**	-0.473 (0.186)**
Farm	-0.416 (0.243)	-0.596 (0.253)*	-0.866 (0.298)**	-4.258 (35.000)
Blue collar	-0.288 (0.113)**	-0.061 (0.104)	-0.191 (0.106)	-0.258 (0.127)*
Constant	-1.317 (0.414)**	-2.481 (0.424)**	-2.677 (0.474)**	-3.497 (0.627)**
Chi-square	94.5**	100.7**	85.5**	65.3**
Log likelihood	-503	-559	-520	-322

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

NOTES

I am grateful to Yong-Nam Song for dedicated research assistance on this project and to the Center for Urban and Regional Affairs, University of Minnesota, for providing research funding for a larger project of which this study is a part. Thanks also to Michael Levin and five reviewers for their comments. All figures are given in U.S. dollars.

1. See Ahlburg and Levin 1990 for a detailed discussion of the economic position of Pacific Islanders in the United States in 1980.

2. The Bureau of the Census designates one person in each household as the "householder." This terminology has replaced "head of household." The householder is, in most cases, the person or one of the persons in whose name the residence is owned, mortgaged, or rented and whose name is listed as person 1 on the census questionnaire. If there is no such person in the household, any adult household member aged fifteen years or over could be designated the householder. Households are classified by type according to the sex of the householder and the presence of relatives. A family household is composed of persons living together who are related by birth, marriage, or adoption. Nonfamily households are composed of a householder living alone or with nonrelatives only.

3. This definition of poverty has been criticized for overestimating poverty, because it does not include noncash benefits such as food stamps and Medicaid; for underestimating poverty, because families now spend less than one-third of their income on food, indicating that a larger multiplier should be used; and for using the wrong metric: writers such as Amartya Sen have argued that poverty is lacking "capabilities" such as being able to live long, being well nourished, being healthy, being literate, having personal and political freedom, and the like. The official measure of poverty is thus a useful, although imperfect, measure of "poverty" and, by implication, a useful though imperfect measure of a "good" job.

4. A reviewer of this article noted that the definition of poverty is not sensitive to geographic differences in cost of living, leading to poverty being understated in places like Hawai'i, which have high costs of living. However, incomes should, at least to some extent, reflect costs of living. For example, the per capita personal income in Hawai'i in 1992 was 8.3 percent higher than that in the United States as a whole (State of Hawai'i 1994:318). To what extent this difference reflects differences in cost of living is unknown. To the extent that poverty is underestimated in Hawai'i and California, the states with the majority of Pacific Islanders, the estimates in this study of "good" jobs held by Pacific Islanders are too high.

5. One implication of defining a "good" job in this way is that, because Pacific Islander households are larger than other households, for two individuals, one a Pacific Islander and one not, holding the same job paying the same income, the Pacific Islander may not be classified as holding a "good" job while the non-Pacific Islander would be classified as holding a "good" job. This point clearly emphasizes that my definition of a "good" job is based on the adequacy of income from the job. Some readers may find this an unappealing characteristic of such a definition. Whether it is quantitatively important depends on what proportion of households are close to the cutoff for each definition and their size.

6. The economic position of Native Hawaiians is, however, below that of other groups in Hawai'i: incomes are lower, unemployment rates higher, and poverty rates higher.

7. These data are based on the census race question, but some groups so identified, such as Northern Mariana Islander, are not distinct racial groups. Individuals of mixed race are assigned to one of the single-race groups based on self-identification or mother's race.

8. Thanks to Michael Levin for pointing this out to me.

9. It is difficult to compare the sample data to the published census data, because the latter are for all Pacific Islanders, not just for householders, and the published data generally present distributions, not means or medians. Average education of householders is the same as that for Pacific Islanders aged twenty-five years and over. Since education is a critical determinant of earnings, this figure gives some confidence in the 1980 sample despite its small size. The percentage of female-headed households in the 1980 sample is lower than in the published census figures, 9 percent rather than 13 percent, and in 1990 a little higher, 18 percent versus 16.5 percent.

10. This approach takes the regression equations reported in the Appendix and asks the question, 'What would the percentage of 'good' jobs held have been in 1989 if Pacific Islander householders had the same average characteristics as in 1979 but were compensated at the 1989 rates?" The difference between the actual figure and this estimate divided by the change between 1989 and 1979 is the proportion attributable to changes in the rates, and the remainder the proportion due to changes in characteristics. See Acs and Danziger 1993 for a discussion of the methodology.

11. There is some debate over whether increases due to increased education and experience should be counted as an increase in "good" jobs. The debate over "good" jobs tends to be over the wages that jobs pay, not the earnings from jobs. This research uses data on earnings to make inferences about the pay levels of jobs. As Levy and Michel point out, such inferences involve a "big leap of faith," because the distribution of annual earnings can be influenced by a number of factors besides wage rates (1991:17). One is the distribution of the labor force by education and experience. Even if workers with a given level of education and experience in 1990 earn the same amount as their education/experience counterparts did in 1980, the distribution of earnings will be higher in 1990 than in 1980 if average education and experience increase. That is, it will appear that jobs in 1990 are better than in 1980 although wages (for given education and experience) have not changed. Since my concern is primarily with the quality of jobs held by Pacific Islanders, not with the quality of jobs per se, it is significant that education and experience have moved Pacific Islanders into better jobs even though the quality of all jobs may not have improved.

12. However, the percentage of all Pacific Islanders in poverty declined over the 1980s because of offsetting changes in the number of workers per household and average household size. I have estimated that poverty decreased from 30 percent in 1980 to 21 percent in 1990 (Ahlburg 1995). However, the number of Pacific Islanders in poverty increased because of a growth in the number of Pacific Islanders.

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BOOK REVIEW FORUM

Geoffrey Irwin, ***The Prehistoric Exploration and Colonisation of the Pacific.*** Cambridge: Cambridge University Press, 1992. Pp. 240, 83 figs. and maps, photos, references, index. US$54.95 hardcover; $18.95 paperback.

Review: **ROGER C. GREEN**
UNIVERSITY OF AUCKLAND

I FAVORABLY REVIEWED the 1992 hardback edition of this important book within an eight-hundred-word limit in the ***American Anthropologist*** (Green 1993). It is indeed pleasing to now have it issued in a much less expensive form, which means it will reach an expanded audience. It is also useful to have a forum here for more extended discussion of some of the more controversial issues it raises.

My basic views of the book have altered little. Previous discussions of ancient voyaging in the Pacific have covered a whole range of related topics in this general field (kinds of Oceanic watercraft, their sailing abilities and navigation; experimental and modem revival voyages; computer-based simulated map journeys; and often heroic travels that lived in memory or are recorded in historical sources). But until recently only a few works on these topics have been well informed by archaeological knowledge. That is in large part because much of this knowledge has been recovered in the last two to three decades.

Archaeology provides the context and chronological framework that shapes Irwin's book and allows the whole subject to be tackled anew, in a compact, readable, and instructively illustrated book-length form. What is brought together within these pages is firm control over current information on Pacific

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prehistory on the one hand and an adept, critical, and innovative use of two centuries of books, articles, and debate on the subject of its pre-European exploration and colonization on the other. It also draws on the author's and many others' sailing experience within that ocean world and an array of computer-simulated journeys under a variety of conditions frequently encountered there.

In a recent book, coming at things from the opposite perspective but with many of the same themes, Finney writes of "putting voyaging back into Polynesian prehistory" (1994:255-306; see also Finney 1996). In his book Irwin achieves an opposite task over a broader canvas by seeing that theories of Pacific voyaging are firmly grounded within our present archaeological understanding. Thus from the earlier and rather naive theories of deliberate voyaging hither and yon wherever the prevailing hypotheses of settlement and inter-island interaction of that time seemed to require, to the spare and dismissive views of mainly unintentional and accidental voyaging proposed by Andrew Sharp in reaction, the pendulum has finally come to rest on what seems a far more robust and believable version of how the islands of the Pacific were sequentially colonized, and then its ocean utilized thereafter as an interaction pathway.

Gone are the rather romantic accounts evoking ancestral Polynesian "Vikings of the sunrise"; in their place is an explicit model of continuing indigenous exploration, colonization, and subsequent voyaging patterns involving many ethnicities over the millennia from 50,000 years ago to the time of European explorers of the sixteenth through eighteenth centuries. Moreover, the theory of navigation proposed is a developmental one, in which improvements in the strategies practiced by the ancient mariners of the Pacific occur as the watercraft technology improves, the experience and knowledge of their island world increases, and conditions for sailing change as human settlement moves from west to east and thence north and south. The story is complex, subtle, and stretches over centuries for which we still have only the sketchiest details.

Timing and tempo, often partial or missing from most previous studies of Pacific voyaging, structure Irwin's discussion. The first section deals mainly with the theoretical issues of colonization and voyaging, covering the Pleistocene period of Ancient Near Oceania beginning 50,000 years ago, the Lapita horizon of 3,500 to 2,000 years ago, and an outline of the basic strategy and general model for systematic exploration of Remote Oceania within these last few millennia. The book next looks at the archaeological evidence bearing on the settlement within that period of, in turn, eastern Melanesia, West Polynesia, and central East Polynesia, and then of Hawai'i and New Zealand. Micronesia, often ignored as too difficult, gets its own separate treat-

ment. Although a reasonable fit between theory and apparent practice as revealed by archaeology is found, this is further subjected to voyaging by computer simulation, not only to test many of the propositions arrived at in the first section, but also to explore further predictions about what seems to have happened and, importantly, not happened.

While this is what the book does, there are, as is inevitable, possibilities for disputes of interpretation over a number of issues, some of which I wish to raise here. Because they have been the subject of numerous informal discussions between Irwin and myself, both over the years of his books gestation and subsequently, and as colleagues in the same department co-teaching a course on Oceanic prehistory, these differences may prove of wider interest to others wishing to join in our dialogue.

Let me pose the first as a question about the tempo of long-term evolutionary change using the Pacific example. Are the broad outlines of human expansion within the Pacific--a last chapter in world prehistory--able to provide empirical support for either of the two main alternatives in cultural evolution when they are expressed as follows? “Does cultural transformation occur primarily by the accumulation of innumerable slight variations within long established systems analogous to predominant gradualism of Darwinian theory? Or is the principal pattern one of abrupt and episodic change, followed by relatively long periods of stasis, analogous to the theory of ‘punctuated equilibrium’ in evolutionary biology?” (Durham 1990: 195). In a recent paper on the topic Larson, Johnson, and Michaelson state that “opinions by social scientists vary widely on this matter; however it is commonly agreed that empirical research derived from systematical longitudinal studies that focus on the process of cultural change are essential to the evaluation of these important research questions” (1994:285). Their study of historical change among the Californian Chumash during the missionization period focuses on a rather short-term temporal scale and assumes that in this case the tempo of cultural change is most frequently a mixture of the two alternatives (ibid.).

My own focus in two recent essays has been to show that one framework describing human expansion in the Pacific conforms to a step or pause model (Green 1991, 1994), without denying that many other aspects of change within that process, such as the continuous development of voyaging technology and navigational skills, outlined by Irwin (pp. 134, 209), may fit better within the gradualist framework. Thus, a one-page summary of my views on exploration and colonization of the Pacific looks something like that outlined in Table 1. Causation at each step but one seems to have a lot to do with a set of innovative developments in the water-transport mechanisms and sailing techniques and strategies that became available to those

TABLE 1. Human Colonization of the Pacific: Time and Steps

1. Pacific parts of **Sundaland** first settled 800,000-900,000 and maybe several million years ago. 850,000-year temporal delay before **first** step into:
2. **Ancient Near Oceania** first settled 40,000 and perhaps 50,000 years ago, or even perhaps 116,000 years ago (Fullagar, Price, and Head 1996).
3. Development of **Modern Near Oceania,** Island South East Asia, and Australia with additional external cultural inputs in each region 6,000-3,500 years ago. 37,000-year temporal delay before **second** step into:
4. **Remote Oceania** first settled 3,300-3,200 years ago. Either a 500-800-year (early settlement) or 1,300-1,500-year (late settlement) temporal delay before **third** step into:
5. **Central East Polynesia** and **East Micronesia** first settled:
   a. early view 2,200-2,500 years ago
   b. late view 2,000-1,500 years ago

   700-800-year temporal delay before **fourth** step into:
6. **New Zealand (Aotearoa)** first settled:
   a. early view 1,400 years ago
   b. late view 700-800 years ago
7. **Antarctic** first explored 19th century. Settled 20th century.
8. **Space** first explored late 20th century. Settled 21st century?

peoples who, in ever shorter but successive bursts, settled some of the last remaining land spaces in this world before yet others occupied Antarctica and then moved out into space. This view contrasts somewhat with Irwin's claim that "colonisation was a continuous process without systematic cultural pauses. There may have been geographic reasons for the elapsed time of island settlement, but cultural ones have to be confirmed" (p. 134).

Causation at the initial step of movement out into the western Pacific (Ancient Near Oceania) has been explored by a number of writers (Birdsell 1977; White and O'Connell 1982:46; Thiel 1987; Jones 1989; Green 1994: 22-25). But Clark's 1991 discussion of the subject coupled with Irwin's work on Pleistocene developments within the voyaging "nursery" or corridor (1991; pp. 18- 30) seem to me to constitute the most satisfactory accounts presently available. They have to do with the invention of water transport, ecological circumstances, and people's motivation to explore lands they could see on the horizon.

Things do not seem so clear-cut with respect to the next pause and then step into western Remote Oceania. Here matters turn on how one views the issue of additional cultural inputs between 6,000 and 3,500 years ago within what I have called Modem Near Oceania (Green 1994: fig. 1.2). Despite the views of those who steadfastly hold to the notion of almost complete local development of most things Oceanic within New Guinea and adjacent islands

of the Bismarck Archipelago in the mid-Holocene, there was, in my view, a significant Southeast Asian Austronesian cultural input in the Bismarcks at 3,000 to 4,000 years ago that cannot be denied (cf. Bellwood 1992; Spriggs 1995, 1996a, 1996b; Kirch 1995, 1997). One example of this is in the form of improved water-transport mechanisms. On current information this is best associated with the Lapita horizon, where we first get the very long distance component of Lapita systems of trade and exchange, and the incorporation of Lou Island (and Fergusson) obsidian into those networks. It also forms the basis for and the means underpinning the rapid expansion of highly decorated ceramics in the Lapita style associated with a whole complex of nonceramic items and features, which are the foundation cultural assemblages within western Remote Oceania (that is, from the Reef/Santa Cruz group through Vanuatu to New Caledonia and the Loyalties on the one hand, and Fiji-West Polynesia on the other).

The improved water-transport mechanisms introduced to Near Oceania at this time comprise the Austronesian watercraft complex as it is reconstructed linguistically for Proto-Oceanic (Pawley and Pawley 1994; see also Horridge 1987:153-163; Green 1991:498). A seemingly slightly later innovation at this stage was the double canoe (Pawley and Pawley 1994:339-340), restricted to the Eastern Oceanic languages, in large part consistent with its later ethnographic distribution and probable time of first employment (Doran 1974:134-135). These additions provided the power (sail types), large stable platform with sufficient carrying capacity (ocean-going double canoe), and development of out-of-sight-of-land navigational skills and strategies (pp. 42-63) needed in the rapid settlement of Remote Oceania.

The next proposed pause and step is that into Micronesia on the one hand, and central East Polynesia on the other. The proposal here for a shorter but still significant pause has caused continuous controversy (Terrell 1986: 81-87). First there is the matter of its timing, where Irwin seems inclined to the early view of not just exploration, but established settlement, in the zone from the Southern Cook Islands to Tahiti and the Marquesas 2,000 to 2,500 years ago (pp. 80-82, 215, and figs. 24 and 83). A considerable literature has since grown up over this matter of timing in respect of the first established and continuing habitation with obvious impact on the landscape in the Southern Cooks, the Society Islands, and the Marquesas (Spriggs and Anderson 1993; Kirch and Ellison 1994; Anderson 1994, 1995, 1996a; Anderson et al. 1994; Peters 1994; Lepofsky, Kirch, and Lertzman 1996; Kirch 1996).

Certainly on the Tahitian evidence it seems that a period before **A.D.** 600 (1,600 years or more) is reasonably well attested to by means of a lake-based pollen record, presence of directly dated semidomesticated coconuts, and the initiation of a well-dated sequence of human-induced landscape change

(Lepofsky, Kirch, and Lertzman 1996), together with a long and continuous archaeological sequence for human habitation (Green 1996). In the Cook Islands and the Marquesas, one or another of those components are not entirely synchronous or simply not yet adequately documented, so disputes have arisen over how to interpret each of the different lines of evidence (Rolett 1996:535-538). The maximal date range for any line of evidence, however, lies in Irwin's estimate of 2,500 to 2,000 years ago.

All of the evidence adds up to indications of a considerable time interval, if not pause, before human settlement was established in central East Polynesia. Linguistically in Polynesia I can see no way out of the need for a culturally based, as well as perhaps a geographically induced, pause of many centuries in length (Pawley 1996; Marck n.d.). Thus, even allowing 200 to 300 years for the initial Lapita expansion into Remote Oceania of 3,200 to 3,300 years ago, followed by another 400 years of continuing voyaging to explore and find landfalls in central East Polynesia, does not take us much beyond 2800-2600 **B.P.** This leaves some 700 to 800 years at a minimum to be accounted for before a colonization event in the form of established settlement may be reasonably postulated at present for central East Polynesia, even on the most liberal interpretation of the available archaeological and environmental evidence. The conservative view requires an even longer interval (Spriggs and Anderson 1993). For these reasons I too strongly support Pawley's view that the linguistic evidence does indeed indicate "a long delay after Western Polynesia was settled, before the effective colonisation of East Polynesia" (1996:404).

The same timing of circa 2,000 years ago is also suggested by the eastern Micronesia evidence. But here the archaeological evidence of its settlement from western Remote Oceania at that date is rather more convincing (Intoh 1996). It is therefore the parallel delay in the effective settlement of both areas that must be explained. Although Pawley says he is willing "to leave it to others to work out why there should have been such a lengthy pause . . . in West Polynesia" (1996:404), for me that situation will simply not do. Irwin and I have often discussed possible causal factors, being reasonably in agreement that these factors probably did not involve major changes to the then existing transport mechanisms or methods in searching for or exploiting new lands. Moreover, he has continued working on the problem since the publication of his book, and I know he may now be nearing one possible solution. That would be a most welcome development, even if we cannot as yet pin down the exact timing of the event itself, as I think we both find unconvincing proposals such as those of Anderson for an adaptive change in sailing and subsistence strategies (1996a).

Irwin's explanation of the need for a later development in Polynesia of a

further strategy of viable "across and down the wind" voyaging as the reason underpinning the delay in the settlement of Aotearoa (New Zealand) and Hawai'i seems to me particularly well motivated. Exploration of Hawai'i may extend back into the earlier part of the first millennium **A.D.,** but ecologically and archaeologically well-established settlement does not seem to occur until after **A.D.** 600 to 700 (Graves and Addison 1995; Athens and Ward 1993:219 and n. 1). It is just possible that discovery and exploration of New Zealand (from New Caledonia/Fiji or West Polynesia) occurred at about the same time East Polynesia was also first visited (Holdaway 1996; Anderson 1996b), but it seems that truly effective settlement of those temperate, continental-type islands may have been only a few centuries older than the currently archaeologically well-attested dates in the twelfth to thirteenth centuries **A.D.** (Anderson 1991). Going on the pollen, charcoal, and sedimentary record, settlement may be as much as one or two centuries earlier in the warmer northern zone of the North Island (Elliot et al. 1995). The initial ecological adaptations required for such tropical East Polynesian societies to establish themselves in Aotearoa are well attested (Green 1975; Davidson 1984); what is not so often appreciated are the semantic linguistic innovations that also occurred at the time of effective initial settlement (cf. Biggs 1991). In my view this geographically imposed need to adapt also had a major impact on later voyaging technology and the maritime skills and strategies exhibited by the contact-period Maori. Irwin does not really discuss this in his otherwise fine chapter on "voyaging after colonisation and the study of culture change," but such a sketch could have been included with profit (cf. Green 1975: 608-609).

In summary, then, the broad temporal outlines of Pacific settlement are in my view now reasonably established. This was not so a decade ago. Granted, at nearly every step some argument obtains (and probably will continue to do so) over the precise timing of that particular event, but the overall framework outlined in Table 1 now provides a reliable guide to the kind of tempo involved in the colonization process. It is my claim here that one pattern that can be detected within that process is more akin to the punctuated or ramp model of evolutionary development rather than the one of slow, steady change during a gradualist movement eastward from an Asian source. Moreover, the tempo of the overall pattern conforms to a log-linear model where the pauses grow significantly shorter as the distances covered grow longer. At the same time plausible cultural, as well as geographical, explanations for each pause and then further rapid expansion are now being developed and debated. Yet it would be wrong to overlook other aspects in the colonization process that may conform more closely to continuous or gradualist process. Both alternatives are in fact likely to be involved,

differentially displayed by the various domains that make up the full range of processes involved in establishing new migrants in both previously uninhabited as well as already inhabited landscapes.

In this respect, one of the most contentious issues raised by Irwin is that of Lapita (pp. 31-41), to which he devotes a separate short chapter. His view is that the term covers "an uncertain and variable archaeological category," which "does not begin to approach an ethnic category except in just a few archaeological sites where the data are under reasonable control" (p. 34). Such cases are stated to be rare and their connections undefined. Yet Irwin concedes that in western Remote Oceania "Lapita more plausibly approaches an ethnic category because it can be shown to be associated with a maritime tradition in a rapid and integrated burst of colonisation, which is likely to have been the first in the region. But there is still no telling how representative a Lapita 'culture' in the remote Pacific was of Lapita in the region it left behind, and it is too soon to say to what extent the deep-sea Lapita colonists were themselves a homogeneous human group" (p. 34).

In Irwin's view, therefore, the uncertain archaeological category of Lapita has "no precise biological or linguistic identification," though in Remote Oceania, as noted in the quote above, he has certainly long seen it as representing "a largely integrated episode of colonisation" (p. 38). For these reasons he seems to favor Terrell's suggestion that only east from Fiji might it be "safe and historically appropriate to speak, biologically and culturally, of a Lapita people who spoke an Austronesian language" (1989:625), while allowing for the possibility in the Remote Oceanic islands to the west of there.

These statements, of course, contrast rather markedly with the data-rich and carefully argued studies of Bellwood (1989a, 1989b) and of Pawley and Green (1973, 1984) for a linguistic and biological identification of different regional aspects of Lapita with various Oceanic Austronesian language subgroups as well as of Proto-Oceanic with Lapita in general, made well before the first publication of this book. The views also differ substantially from those of Green (1997), presented since Irwin's book was published, for both a fairly precise linguistic and biological affiliation of Western Lapita with an Eastern Oceanic linkage or cluster of languages and with a pre-Polynesian biological population in western Remote Oceania (that is, to the west of the Fiji-West Polynesia region, where such an equation is not currently in dispute). And the statements contrast as well with the most recent views of Pawley and Ross for a reasonable case of linguistic correlation of Lapita with Proto-Oceanic itself (1993, 1995), which would include the well-defined Lapita sites with rich assemblages in Near Oceania as part of the equation. Finally, they stand in almost direct opposition to the position forwarded in Kirch's new book on ***The Lapita Peoples*** (1997:79-117), where the linguistic

and biological cases are argued in some detail from a now-ample literature on these topics. Here a "peoples" concept is adopted for the whole region from Fiji-West Polynesia to the Bismarck Archipelago (cf. Kirch 1997:15-18 and n. 24).

As I currently view the matter, Pacific archaeologists are attracted toward one of two positions over the issue of Lapita and its interpretation. For one group the term has a poor to nil prospect of ever embodying varied ethnogenetic, biological, or linguistic characteristics. Those studies attempting to establish such identifications are deemed speculative or unprofitable, if not impossible. This is especially so when the Lapita constructs with which they are being equated are viewed as not constituting a culture, cultural complex, horizon, tradition, or any other similar archaeological unit in common use. On this point Irwin offers as an alternative the suggestion of Terrell (1989) and Hunt (1989) that Lapita "is made up of what could be seen as the elements of trade, at least in Near Oceania" (p. 34). Usually this group of Pacific archaeologists also holds that the history of a language family may actually tell us little or nothing about the history of the people speaking that set of languages, and neither archaeology (Smith 1995a, 1995b) nor material culture (Welsch, Terrell, and Nadolski 1992; Welsch and Terrell 1994; Welsch 1996) are able to be correlated with or used in testing these linguistics-based constructs. Analytical attempts along these lines are considered neither productive nor informative avenues of inquiry.

A second, fully interdisciplinary position sees the endeavor of establishing correlations as a necessary product of a holistic anthropological approach to history, one that is certainly fraught with difficulty, but one in all its complexity certainly well worth pursuing by employing an abundance of data not usually discussed in any detail by the other group. For Lapita, Kirch's 1997 study exemplifies this second position, but it is possible to find supporting arguments and analyses for correlations of archaeological information, material culture, linguistics, and biological data in the work of Bellwood (1989a, 1989b, 1995, 1996a, 1996b), Moore, Roberts, and Romney (Moore and Romney 1994, 1995, 1996; Roberts, Moore, and Romney 1996), Pawley and Ross (1993, 1995), Spriggs (1995), and Pawley and myself. Along with Sutton, this group takes a position in contrast to the other that it is possible and necessary to deal critically and in depth with the problems involved in employing these other means for making constructions of the past, so that "archaeology can actually test them and must" (Sutton 1996:382).

Irwin, in this excellent book, has dealt critically with the problems involved in making constructions about the history of Pacific voyaging and colonization from a point of view encompassing several decades of historical accounts, ethnographic studies, experimental voyages, and computer simula-

tions. And he has productively tested these constructions against the current archaeological evidence. Here he clearly adopts a strategy in line with the second group's position; on the topic of Lapita it is less certain that this is the case, as the issues involved are not in my view sufficiently explored by him for what we both agree is an important homogeneous voyaging and colonizing event in Pacific history. Where we differ is that I see Lapita as constituting a range of acceptable archaeological categories that function with greater degrees of ethnic, linguistic, and biological salience than he so far has been prepared to allow.

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Geoffrey Irwin has written a splendid book on what is surely the longest-standing and perhaps the most contentious issue in Pacific anthropology: the question of how and why the far-flung Oceanic islands were discovered and settled by the ancestors of the indigenous peoples who now occupy them. Generations of explorers, missionaries, and scholars have formed strongly held opinions on these questions. Cook, Banks, the Forsters, and other late eighteenth-century explorers encountered Oceanic peoples who practiced inter-island and interarchipelago voyaging in impressive watercraft, and these explorers formed the opinion that Pacific colonization had been from the west through a long series of purposive voyages. But indigenous watercraft technology soon succumbed to the impact of Western intrusion, and esoteric navigational lore was also rapidly lost. Nineteenth-century missionaries began to question whether the settlement of the Pacific had not

been accomplished merely by chance voyages of drift. At the end of the nineteenth and in the early twentieth centuries the pendulum swung back, with scholars such as Fornander, Smith, Best, and Buck who drew upon (and sometimes elaborated) Polynesian oral traditions invoking images of impressive migratory feats: the 'Vikings of the sunrise." After World War II, Heyerdahl and Sharp challenged anew the purposive voyaging models with new theories of drift voyaging, from east and west origins.

In the past quarter-century, research on the problem of Oceanic voyaging, discovery, and colonization has been invigorated by new approaches. Modem, stratigraphic archaeology has finally come into its own in the Pacific, providing "hard" settlement dates and direct evidence from initial or early settlement sites in several archipelagoes. High-speed computers have allowed for extensive simulations of voyaging and drift probabilities (Irwin himself extends this approach). Surviving indigenous navigational traditions, in Micronesia and the Polynesian Outliers for example, have been ethnographically documented. Most dramatically, replicated voyaging canoes such as ***Hokule'a*** and ***Hawai'iloa*** have made long-distance voyages navigated without instruments over thousands of kilometers, carefully assessing and documenting the conditions, possibilities, and limitations of this mode of transport.

Irwin's book brings together all of these new approaches for a fresh assessment of this long-standing debate. Irwin combines the perspectives of a seasoned archaeologist and of a deep-water sailor who has experienced, from the perspective of the washstrakes what it is like to cross the long seaways from Fiji to Vanuatu, or to navigate the reefs and cays of the Massim. His basic argument is succinct and elegant: in the Pleistocene "voyaging nursery" extending from Wallacea to the Bismarck-Solomons chain, early Pacific peoples gradually accumulated considerable technological and navigational skills. When they finally "burst out" into Remote Oceania about 3000 **B.P.** (with the Lapita expansion), they had developed a "survival sailing strategy" of upwind searches for new land, assured of a high probability of safe return on the prevailing winds and currents. Only relatively late in the sequence of island colonization did they deviate from this strategy, to attempt the "less safe" across-the-wind voyages that would lead to Hawai'i, for example, or the outright dangerous downwind voyages out of the tropics necessary to find New Zealand and the Chathams. Thus Irwin adds ***method*** to ***intentionality*** in modeling ancient voyaging practice. Irwin's book elaborates and explores this elegant model in considerable detail, and I find his arguments generally compelling. As a prehistorian, however, it is to some of the implications for ***archaeology*** raised by his model that I shall devote the thrust of my remarks.

Archaeologically, the onset of the second great episode in Pacific coloni-

zation--that which would lead to the settlement of virtually every habitable island--is closely associated with the Lapita cultural complex. Irwin calls Lapita an "uncertain archaeological category," although I would argue that recent advances in Lapita archaeology have made it quite firmly definable (Kirch 1997). One issue raised by Irwin is the dating of the first spread of Lapita sites, for this bears on the rate of human dispersal, and in turn on the nature of voyaging and discovery strategies. He suggests that the initial Lapita dispersal out of the Bismarcks as far south as New Caledonia and as far east as Tonga and Samoa might have taken 500 years, or some twenty human generations. Substantial numbers of new radiocarbon dates from Lapita sites as well as advances in calibration now allow a refinement of this chronology (Kirch 1997:57-63). While the earliest Lapita sites in the Bismarcks date to around 1550-1400 B.C. (calibrated), the expansion eastwards almost certainly did not get underway until at least 1200 B.C. Then, within two centuries at most (perhaps as few as eight to ten human generations), Lapita communities were established through the southeastern Solomons, Vanuatu, Loyalty Islands, New Caledonia, Fiji, Tonga, and Samoa. (See, for example, the clarification of the initial settlement date for the Koné Period in New Caledonia, provided by Sand [1995:73-76].) This is a truly remarkable expansion on the scale of world prehistory. As Irwin correctly observes (p. 39), this chronology cannot possibly be explained by "ecological or demographic pressures." A "pull" rather than "push" model of migration is required (Anthony 1990). If anything, this expansion must have required a very high birthrate or some means of recruitment of people (a distinct possibility within the long-occupied Bismarck-Solomons region), or both, simply to "fuel" such a rapid expansion. Most important for the theme of Irwin's book, the Lapita peoples clearly were in possession of both the technology for long-distance open-ocean travel, as well as a sailing strategy for safe and successful upwind exploration.

An issue that Irwin sidesteps to some extent is the cultural origins of that technology and sailing strategy. In the preceding chapter he discusses the importance of the Pleistocene "voyaging nursery" lying north of New Guinea and extending into the Solomons. However, the sudden appearance of the Lapita complex in the mid-second millennium B.C., and its extremely rapid and widespread expansion shortly thereafter, have convinced many archaeologists that it reflects an intrusion into Near Oceania of one or more groups of Austronesian-speaking peoples. In this regard, it is significant that historical linguists have reconstructed a large suite of Proto-Austronesian and Proto-Oceanic terms for a sophisticated outrigger and double-hulled sailing canoe complex (Pawley and Pawley 1994). Irwin's "voyaging nursery" is an important concept with regard to Pleistocene movements in Near

Oceania, but surely it was the introduction to this corridor of the Austronesian outrigger sailing canoe in the second millennium B.C. that rapidly allowed for the subsequent expansion of humans throughout Remote Oceania.

The western Polynesian archipelagoes of Tonga and Samoa, as well as the more isolated islands of Futuna, ʻUvea, and Niuatoputapu, all had Lapita communities established no later than the end of the second millennium B.C. Indeed, recent work by David Burley in the Haʻapai group of Tonga suggests a highly structured colonization pattern of establishing small (hamlet-sized) Lapita settlements on virtually every islet of habitable size (pers. corn., 1996). And although classic dentate-stamped pottery has yet to be recovered there, the Manuʻa group in eastern Samoa was also inhabited by 1000 B.C. Then, in spite of the fact that both the Northern and Southern Cook Islands to the east are closer to Samoa-Tonga than Fiji is to Vanuatu or the Solomons, the Lapita "bullet train" seems to have come to a screeching halt. Or did it?

Nothing has been more contentious in Pacific archaeology in recent years than the debate surrounding the chronology of settlement in central and eastern Polynesia. Archaeologists for some years have had to contend with the problem of a "long pause" between the Lapita colonization of western Polynesia and the expansion into central-eastern Polynesia. But with the application of "chronometric hygiene" to the corpus of available radiocarbon dates from eastern Polynesian habitation sites (Spriggs and Anderson 1993), this pause has been widened to a yawning gap of as much as 1,500 years! Others, including myself (Kirch and Ellison 1994), are extremely dubious that the interval between human settlement of Tonga-Samoa and the initial movement on into central Polynesia (e.g., the Cooks-Societies-Australs region) was much longer than about 500 years. One objection, on strictly theoretical demographic grounds, is that an ultra-long pause (or "short chronology" scenario) would require a huge population burst to fuel such a vast migration in a very short time period, a much greater demographic increase than that driving the documented initial Lapita expansion into Remote Oceania. In archaeological terms, the problems are largely methodological and concern such issues as sampling error, geomorphological constraints on site visibility (e.g., submerged coastlines and substantial valley in-fillings), acceptability and calibration of radiocarbon dates, and the validity of proxy measures of human presence on islands, particularly evidence for vegetation disturbance and anthropogenic fire regimes derived from sediment cores and pollen analysis (Kirch and Ellison 1994).

Irwin's "continuous settlement model" for East Polynesia (see his fig. 30) is critical in this debate, and in my view reflects the closest approximation to what we may expect to find archaeologically. In his model, there was "no sys-

tematic delay" in voyaging eastwards out from the Samoa-Tonga region, and his prediction is that initial Polynesian discovery of the Southern Cook Islands was likely made by about 500 **B.C.** (see his fig. 24). Rather than a sudden "explosion" throughout central-east Polynesia (as required by the Spriggs-Anderson short chronology model), Irwin envisions a continuous but somewhat slower progression down the Austral, Society, and Tuamotu alignments, ultimately reaching as far as Mangareva, Pitcairn, and Easter. Only after this region had been thoroughly explored and settled would the more difficult voyages be made "across the wind," leading to the discovery of the Line Islands and beyond them the great Hawaiian archipelago, probably early in the first millennium **A.D.** The last of the Polynesian lands to be discovered, in Irwin's view, should have been New Zealand and the Chathams, given that they are both downwind and out of the tropics.

There is considerable new archaeological and paleoenvironmental evidence to support Irwin's continuous settlement model, although not all of my colleagues concur. Most notably, in the Southern Cooks our extensive coring on Mangaia demonstrates an unmistakable human presence on the island probably as early as 2400 **B.P.** and certainly no later than 1600 **B.P.** (Kirch 1997). Significantly, the Mangaia dates closely matched Irwin's predictions for the Southern Cooks (see his fig. 24). Mangaia also illustrates one of the problems of detecting early settlements, as the mid-Holocene paleosols of the valley floors (the most desirable locations for house sites) are now buried by up to six meters of clay in-filling. On Mo'orea in the Society Islands, Lepofsky, Kirch, and Lertzman (1996) also document massive in-filling and coastal progradation of precisely the sort pointed to by Kirch (1986) as a likely problem in locating early settlements in that archipelago. In the Mo'orea case, early domesticated coconuts and other signals of human presence date to **A.D.** 600, earlier than any as-yet-discovered actual habitation sites in the Society Islands.

A major problem with archaeologically assessing Irwin's model, however, is the almost complete lack of prehistoric sequences for the Austral, Tuamotu, and Gambier archipelagoes, one of the most likely corridors of upwind exploration. Weisler (1995) has firmly documented Polynesian occupation of remote and ecologically marginal Henderson Island by **A.D.** 900 (if not 100 to 200 years earlier), but we would hardly expect to see a permanent colony on Henderson until some centuries after the main Austral-Gambier chains had been permanently settled. The initial settlement date for Easter Island remains enigmatic, although the dates on a remarkable extinct avifauna recovered from Anakena by Steadman, Vargas, and Cristino make it clear this must have been sometime prior to **A.D.** 900 (1994). Flenley has recently published a new pollen core from Rano Kao on Easter Island (1996),

with a radiocarbon age suggesting anthropogenic disturbance beginning at 1630 ± 130 **B.P. (A.D.** 147- 676), which fits well with the Mangaian evidence. The fundamental point is that the current debate between "long" and "short" chronologies--and the testing of Irwin's continuous settlement model--will never be resolved on the evidence presently at hand. We urgently need much more archaeological exploration in the Australs, Tuamotus, Mangareva, and even the Societies, and such work must be both geomorphologically sophisticated and go hand-in-hand with interdisciplinary paleoenvironmental research.

New Zealand provides an especially intriguing case. Irwin argues that reaching Aotearoa "involved a more complex voyage across the trade winds, through a belt of variables to the latitude of prevailing westerlies" (p. 105). In his overall model of sailing strategies, it should have been the last sector of Polynesia to be colonized, and indeed, the orthodox archaeological view is consistent with this, initial settlement generally being put at **A.D.** 1000 or after (Spriggs and Anderson 1993). Yet in the chapter of his book on computer simulations (certainly one of his most innovative contributions), Irwin also observes that New Zealand could readily have been reached from islands other than the Southern Cooks, the point of departure presumed in most scenarios. Indeed, New Caledonia is its closest large neighbor, and ten simulated canoe voyages originating from La Grande Terre all made it safely through cyberspace to successful landings on the North Island. Irwin is characteristically cautious about the implications of his analysis, saying only that "voyaging considerations suggest a wider range of possibilities" than an exclusively eastern Polynesian origin. In this regard, the recent publication of AMS radiocarbon ages on ***Rattus exulans*** bones from New Zealand (Holdaway 1996), dating to ca. 2000 **B.P.,** must be noted. ***R. exulans*** accompanied Lapita voyagers and later Polynesians throughout the Pacific, and can hardly have swum to New Zealand, although Anderson would have them arriving in drifting canoes whose crews had already perished (1996). While I would agree that the settlement chronology for temperate South Island is unlikely ever to be pushed back beyond **A.D.** 1000, I am still not so certain about the subtropical parts of the North Island. Sutton's (1987) provocative claims for a longer period of occupation there than the current orthodoxy allows may require a rehearing.

An issue that Irwin alludes to in passing but does not take up in any detail is the relationship between archaeological and linguistic models for Polynesian settlement. However, his voyaging models give cause to rethink some of the correlations between archaeological and linguistic scenarios. In the 1960s, the discovery of early sites in the Marquesas Islands was combined with a linguistic subgrouping model that derived the modem East Polynesian lan-

guages from two main stocks (Tahitic and Marquesic) to suggest that the Marquesas were a "primary dispersal center" for eastern Polynesian settlement. But the linguistic evidence in and of itself never provided an intrinsic reason to think that the Marquesas archipelago was the geographic location of Proto-Marquesic! It could just as well have been in Mangareva or in one of the other Austral Islands, most of whose languages or dialects have gone extinct without ever being adequately recorded. (In this regard it is noteworthy that Irwin's voyaging simulations indicate the likelihood of the Marquesas being directly settled from western Polynesia as "a long shot" [p. 151].) Recent work by Marck now suggests that the Polynesian subgrouping model is itself in need of revision (1996; and pers. corn., 1996), with Proto-East Polynesian being derived from Proto-Nuclear Polynesian via a Proto-Ellicean interstage that includes Tokelau and certain Outlier languages as witnesses. The Ellicean-Tokelau-Outlier linkage might lead one to favor a northern (atoll) route into eastern Polynesia. I think we must be cautious, however, of immediately assuming that an exclusively Northern Cooks-Societies route into East Polynesia is required by the linguistic phylogeny. It is entirely conceivable, as Irwin's "close proximity analysis of mutual accessibility" (see his fig. SO) demonstrates, that the exploration and colonization routes into East Polynesia were multiple. From the linguistic viewpoint, I can only urge that the as yet mostly undocumented but clearly distinct language of Mangaia be studied by some enterprising linguist before the opportunity is lost. It is nonsense to continue to take Rarotongan as the exclusive witness for what was clearly a highly diverse linguistic situation in the Southern Cooks.

One of the tantalizing issues in Pacific voyaging also taken up by Irwin is the South American connection. Heyerdahl drew world attention to the matter with the ***Kon Tiki*** voyage, but Irwin rightly turns the question around to ask what was the likely success rate for voyages originating in East Polynesia to arrive on the South American coast, and to return again to their home islands. It turns out that success rates between 50 and 70 percent are achievable, and "controlled Strategy 4 returns to Easter Island and Sala y Gomez brought survival up to 95%" (p. 163). Given the radiocarbon-dated presence of the South American sweet potato ***(Ipomoea batatas)*** in Mangaia at 900 **B.P.** (Hather and Kirch 1991), it is no longer an issue whether contact between East Polynesia and South America was made. Irwin's analyses, however, greatly strengthen the probability that it was Polynesians and not South American Indians who were the transferrers of this important crop plant, as Douglas Yen indeed averred (1974:264-267).

The core of Irwin's book deals with Polynesia, but he does not entirely neglect the western Pacific. A chapter devoted to Micronesia begins by pre-

senting the results of computer voyaging simulations, from which it is clear that relatively high successful rates could be had for reaching the Marshalls-Kiribati chains from an origin point in the southeastern Solomons, such as the Reef Islands. Voyages originating in the central Solomons (e.g., Malaita) could readily reach Kosrae and other central Caroline islands. At the time Irwin was writing, archaeologically based chronologies for Micronesia were either contradictory (as in the case of Palau and the Marianas) or very preliminary (as for Chuuk, Pohnpei, and Kosrae). The last few years have seen an outpouring of new archaeological data for this region, however, and we can now be a bit less tentative and cautious than Irwin was inclined. For the central Caroline high islands, in particular, initial colonization around 2000 **B.P.** is increasingly well established, and the highly probable point of origin is one or more late Lapita communities in the Solomons region (Kirch 1997: 74-77). The earliest ceramics in Pohnpei and Kosrae consist of sand-tempered plainwares that are almost indistinguishable from late Lapita plainwares found at sites ranging from Mussau to Tikopia. Here is a case where the linguistic, archaeological, and voyaging models all converge robustly on a scenario of central-eastern Micronesian settlement out of island Melanesia some two millennia ago. The situation in western Micronesia is more complex, and the probability of multiple origins out of the Philippines or even Taiwan remains high.

Irwin also applies voyaging models to the period following initial colonization, for example to the Polynesian Outliers and the so-called mystery islands of Polynesia. While I concur with his analysis of the Outlier situation, I am more skeptical of his conclusions regarding the abandonment of the roughly twenty small islands that are sometimes labeled "mysteries." Irwin's argument is that they were abandoned primarily because they are "less accessible," and he adduces a graph of distance and target angle (his fig. 67) as supporting evidence. But I fear that Irwin may be mistaking a correlation for causation, for it remains the case (as he rightly notes) that these islands are also characterized by small size and by ecological marginality (limited water, poor soils, or other resource limitations). Irwin argues that when long-distance voyaging began to decline throughout Polynesia, these islands were the first to be affected. Weisler's research on Henderson Island (1995), however, has revealed a continuous human population on that tenuous habitat between **A.D.** 900 and **A.D.** 1650. Weisler believes that eventual abandonment was linked to severe ecological and sociopolitical transformations in Mangareva, to which Henderson was vitally linked by a complex, long-distance interaction network. I suspect that a similar scenario might hold for Nihoa and Necker in the Hawaiian chain; but more work there will be required to test this model. In short, while Irwin's accessibility model adds

to our understanding of the "mystery islands," I find it an insufficient argument.

Finally, Irwin's book also draws attention to the matter of continued interisland and interarchipelago voyages long after initial colonization and therefore to the question of the influence such contacts may have had on culture change and differentiation among the Polynesian societies. Again, this is a complex issue, at the heart of current intellectual debates within anthropology regarding phylogenetic versus reticulate models of cultural change (Bellwood 1996). Of course, such models need not--indeed, should not--be mutually exclusive, and Kirch and Green explicitly included contact between islands as a "mechanism of divergence" among what are nonetheless genetically (that is, phylogenetically) related groups (1987). In providing what he calls an "independent navigational theory of Pacific colonisation and change," Irwin now augments the tools available for investigating the extent to which contact may have acted either to encourage, or in some cases to minimize, divergence.

Geoffrey Irwin's book demonstrates how much more sophisticated and nuanced anthropological discussions of Pacific origins and voyaging have become. In an age of increased subdisciplinary specialization, he also reminds us of the great strength that a truly holistic anthropology commands: an anthropology willing to bring to bear methods, evidence, and theories from archaeology, ethnography, biological anthropology, linguistics, and even from related disciplines. It is in the continued application of such an approach that Irwin's models will be tested, and our ultimate understanding of Pacific history thereby advanced.

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Exploring and Colonizing the Pacific by Logic and Computer

For years histories of seafaring have focused primarily on Ancient Egypt, the Mediterranean, and Western Europe, tracing human maritime capabilities to the point when western European mariners developed the technology and skills to sail continuously from ocean to ocean over the global sea, an achievement that naval historian Perry called the "discovery of the sea" (1974). Yet any truly global account of our species' development of seafaring would also have to record the nautical achievements of other maritime peoples, which, preceded this globe-girdling stage of maritime development. Examples come to mind such as the expeditions of the early Ming Dynasty, when the Chinese sent huge fleets of ships into the Indian Ocean as far as the shores of Africa; the pre-Columbian pioneering of sea routes along the Pacific coast of South America by Native American raft sailors; and above all the expansion into the Pacific that started in Pleistocene times with the crossing to Greater Australia and culminated in the more recent colonization by Austronesian canoe voyagers of the islands of the deep Pacific.

Over the last thirty or so years anthropologists, archaeologists, linguists, and students of indigenous sailing and navigation have made great progress in roughly outlining the sequence and chronology of Pacific expansion and, in the case of the Austronesian phase, in providing insights based upon contemporary ethnographic and experimental research about canoes, navigational methods, and sailing strategies. Irwin's book represents the most ambitious attempt to date to draw from all this material to come up with a comprehensive model for the exploration and colonization of the Pacific. Using logic and computer simulation, Irwin cuts through the jumble of data, hypotheses, and opinions to brilliantly and economically model this Pacific Ocean phase in humanity's spread over the globe.

In the mid-1960s when David Lewis was beginning his remarkable studies of indigenous Pacific navigation reported in ***We, the Navigators*** (1972), and a team of Hawaiian outrigger-canoe paddlers and I were testing

the performance of ***Nalehia,*** the first of the Polynesian double canoes to be reconstructed for experimental purposes, a group led by geographer Gerard Ward started a project to test by computer simulation Andrew Sharp's then-influential thesis that because of the limitations of their primitive sailing and navigational technology, Polynesians could only have "accidentally" settled their islands through a long series of random and for the most part involuntary drift voyages (Sharp 1956). Ward and his colleagues first loaded their computer with a massive compilation of data on wind and current speed and direction in the Pacific, estimates of the drift speed of canoes pushed by the wind and current and how long crews could survive at sea, and then instructions for subjecting canoes to wind and current drawn from the compilation and for plotting their progress until land was reached or the crews perished. Their simulations of thousands of drift voyages clearly showed the poverty of Sharp's thesis: above all it could not really account for the long crossings by which the whole of the Polynesian triangle was settled, in particular the expansion eastward from West Polynesia to the archipelagoes of central East Polynesia, and the subsequent movements from there north to Hawai'i, southeast to Rapa Nui, and southwest to Aotearoa (Levison, Ward, and Webb 1973). Yet, although they concluded that Polynesia must therefore have been settled intentionally by seafarers deliberately exploring unknown seas and intentionally colonizing the islands they found, Ward and his colleagues did not attempt to simulate how they might have accomplished this.

Some data for such a simulation were then available. Our experiments with ***Nalehia*** had indicated that a double canoe could sail to windward up to about 75 degrees off the wind (Finney 1967), a figure supported by observations of the windward capacity of Micronesian outrigger canoes made by Gladwin (1970) and Lewis (1972). We then plotted how a canoe with such a windward capacity could sail from Hawai'i to Tahiti by making a long, curving slant across and slightly into the westward-flowing trade winds and equatorial currents, a model supported by the 1976 voyage of ***Hokule'a,*** our second reconstructed canoe, and the four subsequent crossings she has made between Hawai'i and Tahiti. In addition, published accounts dating back at least to Cooks times indicated that when indigenous seafarers from Indonesia to Polynesia wanted to sail to the east against the trade-wind direction, they waited for those times when the trades were replaced by spells of westerly winds. Had Ward and his associates, or others following immediately in their wake, injected rules for sailing up to 75 degrees off the wind and for waiting for westerly wind shifts into the simulation program, the resultant trials would have undoubtedly demonstrated how gaps unlikely to have been crossed by drifting could have been closed by sailors who had learned to wait for favorable winds before starting out and who could slant moderately

to windward when necessary. Yet, I doubt if a hasty reprogramming with just these two factors would have resulted in a study as sophisticated as the one under review here.

Irwin titles his last chapter "The Rediscovery of Pacific Exploration." I would foreground that process and assert that Pacific archaeologists had to rediscover the seafaring basis of Pacific colonization before so comprehensive a study as this one could have been undertaken. Although when we began experimenting with sailing canoes some archaeologists were vitally interested in our findings, many others were skeptical of their value. The latter seemed wary of the whole idea of intentional voyaging by competent seafarers. A distrust of oral traditions upon which earlier models of voyaging and settlement had been based, a rejection of the culture history approach in favor of focusing on processes of adaptation and change on assumed island isolates, and an ignorance of sailing and navigation were involved in their skepticism. Many seemed content to assume that each island or archipelago had been settled when somehow one or at the most a few canoes chanced upon it, and that the resultant societies developed thereafter in total or near total isolation from all but nearby islands. However, such least-moves modeling could not stand up in the light of the discoveries that followed in the 1970s and 1980s. For example, the archaeologically revealed evidence of the remarkably swift colonization of islands spread from the Bismarcks to Samoa, and of the exchange of materials along at least major portions of this 2,000-mile-plus stretch of ocean and islands, begged for models featuring intentional exploration and colonization as well as interarchipelago, two-way travel. Then, the documentation by Lewis, Gladwin, and others of the superb skills of surviving navigators in the central Caroline Islands of Micronesia and a few other remote places, plus the realistic insights on navigation and sailing strategy provided by sailing ***Hokule'a*** over the long sea routes of Polynesia, served to demonstrate how stone-age sailors could have intentionally explored and colonized the Pacific and then traveled back and forth between distant outposts. Now, intentional voyaging is certainly "in" among Pacific archaeologists, as witness one of their most exciting research frontiers: investigating long-distance interaction in the Pacific by tracing basalt adze blades and other mineral products that bear a unique chemical signature back to the islands where they had been quarried (Weisler 1997).

Irwin, one of the few archaeologists who owns his own ocean-sailing yacht, writes how one of his most fruitful insights into Pacific colonization came in 1985 while sailing his yacht ***Rhumbline*** westward from Fiji to New Guinea, and, as he puts it, "nursing" his "dissatisfaction with the subject of Pacific colonization" (p. 8). As the yacht slipped downwind with the trades, opposite the generally eastward course followed by the pioneering Austronesian sea-

farers, it occurred to him that he was experiencing the other side of the coin of colonization: "It was what any canoe could choose to do, which had sailed upwind, whether it found land or not. The ease of sailing west is what made sailing east possible!" This insight he later transformed into a formal sailing strategy, which in this study he argues best accounts for the eastward expansion of Austronesian seafarers and shows the highest success and survival rates in his computer simulations: explore to the east on westerly wind shifts, then after finding land or failing to do so after a reasonable time, turn around and sail west before the returning trade winds, first sailing to the latitude of the departure island and then running down that latitude to get home safely. Other nautically informed writers had stated that the ability to exploit westerly wind shifts and the subsequent resumption of easterly trades to explore eastward and then run home to spread the word of a new discovery or in order to survive after a fruitless search must have been crucial to this oceanic expansion. But Irwin has elevated this to a comprehensive strategy and tested it against the archaeological record and by means of computer simulations.

The resultant analysis compellingly models the Austronesian expansion eastward and offers stimulating perspectives on the moves off the main line of advance to eastern Micronesia, Hawai'i and Aotearoa, as well as such issues as the abandonment of the marginal islands and the settlement of the outliers. With text, charts, and diagrams Irwin delights in demonstrating how his analysis leads to new and surprising insights. For example, he argues that plotting the eastward progress of Pacific colonization implies that instead of slowing it was "accelerating at a rate approaching exponential" (p. 81) until the seafarers ran out of uninhabited islands and possibly landed on the shores of an already populated South America (p. 80: fig. 24). He also maintains that both his plotting of archaeological dates and his computer simulations rule out the disputed "long pause" in West Polynesia, arguing that progressive improvements in nautical skills more than matched any difficulties met with as these seafarers moved across the Pacific (pp. 88-89, 208-209).

Nevertheless, as in any grand scenario, there is plenty of room for objection and comment. For example, the Austronesian ***drang nach Osten*** does not appear to have smoothly accelerated to the point of going exponential if the starting gate is set back, as many students do, to the Bismarck Archipelago region as opposed to where Irwin models it (p. 80: fig. 24): the Santa Cruz Islands, midway between the Bismarcks and West Polynesia. Take a globe and a piece of string. Place one end of the string at the Bismarck Archipelago and then extend the string eastward to West Polynesia and cut it there, thereby creating a measuring rod representing approximately 2,100 nautical miles. Then stretch the string eastward from West Polynesia, and you will

find that it reaches slightly beyond the Marquesas Islands. If expansion continually accelerated from the Bismarcks, we should therefore expect that it would have taken a significantly shorter time to reach, from West Polynesia, the Marquesas and certainly Tahiti than it took to move from the Bismarcks to West Polynesia. Although Irwin quotes himself on the archaeologically "instantaneous" character of the latter movement--meaning that the time was too short to measure given the uncertainty range in radiocarbon dating, he supposes for purposes of discussion that as much as 500 years might have been involved in moving from the Bismarcks to West Polynesia (p. 39). If, therefore, West Polynesia was reached in 3500 **B.P.** as Irwin charts it in fig. 24 (p. SO), during an accelerating expansion the central East Polynesian islands up to and including the Marquesas should have been reached in significantly less than 500 years, or before 3000 **B.P.** However, this time frame is well before any existing radiocarbon dates, or even Irwin's early settlement estimates graphed in fig. 24, which indicates that at 3000 **B.P.** the frontier of expansion had reached only as far as the Cook Islands, some 700 miles east of Samoa and just halfway to Tahiti. In a perfect archaeological world where the earliest sites are always found and dated without problem, this issue of whether the Austronesian expansion smoothly accelerated throughout its length or was punctuated by bursts and pauses of varying durations could be solved easily. Until then we are stuck with models that depend upon differing estimates of site sampling error, interpretations of radiocarbon dates, and ways of imagining the expansion.

Irwin clearly sets out the computer simulation results in charts, tables, and accompanying text but cautions the reader not to imagine that island navigators ever employed the sailing strategies he tests precisely as he develops them (p. 209). The reader should also be cautious about the virtual environment into which Irwin injects his imagined voyagers, for as he outlines it on pp. 134-136 they sail on a sea without currents, employing wind data from only two months out of the year.

Sailing virtual canoes over a currentless sea may simplify computer simulations but could produce unrealistic results where current counts--such as when, during an eastward passage started with a westerly wind shift, easterly trades come back in strength and require a canoe to tack, or when a canoe makes long slants across steady winds and accompanying currents. A canoe tacking to windward 75 degrees off the wind must travel almost four miles at an angle to make one mile directly to windward. Thus a canoe tacking at four knots would be making only one knot directly to windward, or twenty-four miles in one day. But if that canoe is also sailing directly against a one-half knot current, windward progress would be cut to half a knot, or twelve miles a day. A current of one knot would stop forward progress cold. Even

sailing across the current flow can significantly affect canoe performance, as is evident every time we sail ***Hokule'a*** from Hawai'i to Tahiti. Tahiti lies some 2,250 nautical miles south-southeast of Hawai'i, slightly upwind of the easterly trade winds that flow over most of the route. If there were no currents in the sea, sailing from Hawai'i to Tahiti would be relatively easy. Just put the canoe on a heading sufficiently into trade-wind flow to gain enough miles to the east to reach the longitude of Tahiti, and not end up west of the island. However, except in a narrow band just north of the equator, strong ocean currents generally flow from east to west across the course, requiring us to sail ***Hokule'a*** extra hard into the trades to keep from getting set to the west of Tahiti. Although adding current to computer simulations on this and other routes would not be easy, doing so would enhance the applicability of such virtual voyaging to the conditions actually faced by sailors.

The ways in which Irwin employs wind data in his simulations also raise questions about their application. He appears to use only two months of wind data: those for January and July, to represent midsummer and midwinter conditions. What about sailing in April, November, or other times of the year when conditions are most likely to be favorable in particular regions? Although Irwin gives little detail about his simulation programs, it also appears that for his virtual sailors the computer probabilistically selects a wind for "each new day" (p. 135). Both from ethnographic information and experience we have learned to sail by exploiting favorable spells of wind, such as the brief episodes of westerlies that enabled ***Hokule'a*** to be sailed from Samoa to Tahiti--against the direction from which the trade winds usually, but not always, blow. Does Irwin's program effectively simulate these and other spells of winds that are so crucial to actual sailing strategy? Or, as Irwin's wording would seem to indicate, is the wind for each day a separate probabilistic event?

One of the main strengths of this book is its inclusiveness. Irwin does not confine himself to only one phase of Oceanic expansion but begins the story of Pacific settlement with the very early expansion into Greater Australia and the voyaging conditions around New Guinea, and also includes an interesting chapter on Micronesia, which, since the advent of the Lapita discoveries, has come to be considered off the main line of advance. This inclusive approach stimulates such comparative questions as why the expansion (whether smoothly accelerating or punctuated) eastward by Austronesian voyagers from south of the equator continued so far across the Pacific, in contrast to the apparently truncated eastward thrust to Micronesia by their cousins from north of the equator. During the same era Austronesian sea-

farers were striking out into the Pacific, other Austronesian sailors, perhaps sailing from the Philippines, reached the Marianas, and perhaps Belau and Yap. However, these North Pacific Austronesians apparently did not keep expanding to the east like their South Pacific cousins. Primarily from linguistic evidence, it appears that the Micronesian islands farther to the east were settled first and/or primarily by southern voyagers who split off from the Lapita trail, headed north across the equator, and then to the west to populate the earlier settled islands along Micronesia's western margin. Why didn't the initial thrust to Micronesia from the west carry farther into the ocean?

Might it have been, as Irwin suggests, because 3,000 or so years ago the Carolinian atolls immediately to the east of the western edge of Micronesia were still just below the surface of the sea, or only barely awash, and therefore provided little incentive for expanding eastward? Perhaps, but as Irwin admits, we can't really be sure these atolls were not already above the surface then. Furthermore, even if they were still submerged, it could be argued that seas filled with shoaling atolls would have encouraged truly expansive seafarers to keep probing eastward to find and settle the handful of high islands and upraised atolls beyond, and then perhaps to dip southeastward and cross the equator to the more island-filled seas there. A more fruitful tack to take in trying to explain this contrast between the two eastward movements might be to emphasize those factors explored by Irwin that appear to have been crucial to the advance of Austronesian voyagers through the islands south of the equator. In apparent contrast to the seas north of the equator from whence sailed the colonizers of western Micronesia, the "voyaging corridor" running from northern Indonesian-southern Philippine waters eastward along the northern shore of New Guinea was truly a nursery for adventurous seafaring. Its alternating regime of easterlies and westerlies, lack of tropical cyclones, and abundance of island landfalls seem to have honed the seafaring skills of early Austronesian sailors (who may also have benefited in some degree from the local knowledge of indigenous sailors and farmers descended from the Pleistocene seafarers who first colonized this region). Then the chains of islands to windward with respect to the trades apparently beckoned these fledgling explorers farther and farther to the east, particularly when they reached the point where the islands were empty of other humans. The resultant expansion across the Pacific stands as one of the most remarkable nautical achievements in humanity's spread over the planet. By so clearly and resolutely modeling this expansion, Irwin's book puts archaeologists firmly back on board the endeavor to understand how the open Pacific was first explored and colonized.

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Thirty years have now passed since Robert MacArthur and Edward O. Wilson proposed the theory of island biogeography (MacArthur and Wilson 1967). As Wilson recollects, he had reams of data on ant distributions on Pacific islands, but no process to bring general order to the "exciting chaos." Spreading his notes and graphs on MacArthur's coffee table next to the fireplace, the two speculated on the possible causes for observable patterns. They were struck by the importance of geometrical properties, the spatial relations between islands, as the key elements affecting the species equilibrium on islands (Wilson 1985:455-459).

Armed with predictions about species composition in relation to island

geometry, islands were viewed as laboratories, or natural experiments, in which some dimensions were constant (e.g., distance from a mainland) while others varied (e.g., island size). This quantitative and experimental approach was the foundation for much of evolutionary ecology, and the study of islands provided much of its clearest documentation (MacArthur and Wilson 1967:3).

A decade ago, Jared Diamond and I used the theory of island biogeography as a framework to compare the colonization of islands by humans in all the world's oceans (Keegan and Diamond 1987). We were following in a tradition of interarchipelago comparisons that our colleagues had initiated shortly after the MacArthur and Wilson monograph first appeared. In our article we pointed to a few of the most obvious contradictions to the theory. These contradictions emerged from the fact that the theory does not consider the motives of the colonists or the means by which they reached an island. For the theory, all that matters is that there is a balance between arrivals and extinctions. To a large degree we maintained that perspective in our review. Navigation and the skills needed to reach an island were simply assumed to exist; for if they did not, then an island would not have been colonized.

That view was formalized by proposing "autocatalysis" as the process by which the colonization of Pacific islands progressed. Autocatalysis is simply a positive feedback loop in which voyagers returning to announce the discovery of a new island or islands stimulated the search for additional islands (Keegan and Diamond 1987:67-68). Irwin is critical of this concept (p. 63), yet our objective was to counter two prevailing notions. The first was that having to move to a new island was bad, and the second was that return voyaging was not possible. However, because our suggestion that Polynesians were making return voyages was "not an established view" (p. 63), we were encouraged by several reviewers to include an alternative version of the theory in which return voyaging was not required. Irwin has shown that this alternative is no longer necessary.

At the time it was clear to us that a more comprehensive theory was needed, one that took into account human motives and abilities. This view was also expressed by others (e.g., Cherry 1981; Kirch 1984; Terrell 1986; Watters 1982). Our suggestion was a scalar approach beginning with very general theories, such as island biogeography, incorporating abstract models of process, such as those developed through the application of neo-Darwinian evolutionary principles and predictive models like those in economics, and concluding with a healthy dose of realism, meaning what the world was like when the island was colonized (Keegan 1991).

In this vein Geoff Irwin's book is an exciting addition to the study of island colonization. His theory of navigation fills a major void, and his

emphasis on exploration, the important first stage in a colonization event, opens new vistas. Furthermore, his conclusions regarding "safe sailing" and the notion that explorers traveled into the wind so they could more easily return home if their quest failed are brilliant insights. Lastly, he challenges the sense of isolation that is a basic element of the laboratory model of cultural development on islands. It is clear from Irwin's work that Pacific archaeologists and anthropologists must give greater consideration to inter-island contacts, even over substantial distances.

My contribution to this forum is a view from beyond the Pacific. And although the Caribbean is much more like the Mediterranean than Oceania, research in the West Indies shares a number of elements with the Pacific studies (Watters 1982, 1989). The most important of these concern renewed interest in maritime capabilities and an enhanced appreciation for socio-cultural interactions throughout the region (see Keegan 1994 and 1996 for reviews).

For the West Indies, navigation and seamanship appear to be less critical problems. The islands are arrayed as stepping stones that are largely inter-visible so ocean-going canoes were rarely out of sight of land (Figure 1). In addition, sails were not used until after the arrival of Europeans, probably because travel distances were short. Native West Indians may have paddled against wind and currents to explore new areas ("safe sailing"), but these expeditions would have been of short duration. The absence of archaeological sites on Grand Cayman suggests that the search for new islands was limited in scope (Stokes and Keegan 1996).

Using computer simulations, Callaghan has shown that "direct crossings between Puerto Rico or Hispaniola and the South American mainland were not only possible but faster and safer than following the Lesser Antilles" (1995:186). In addition, one of the widest water gaps in the region is between Cuba and Florida (about 150 kilometers), a distance that recently was crossed by numerous makeshift rafts and in June 1996 was almost breached by a swimmer. Clearly, the pre-Columbian cultures of the Caribbean should not have developed in isolation.

Yet despite the relative ease with which people could have moved around the West Indies, the trend among archaeologists has been to assume that movement between islands, and between islands and mainlands, was difficult (Rouse 1986). For example, the conventional wisdom is that a single culture from the Orinoco River drainage was responsible for peopling the West Indies. Called "Saladoid," for the type site of Saladero, Venezuela, it is assumed that this one group entered the Antilles about 500 B.C. and advanced slowly, island by island, until they reached the Greater Antilles (Rouse 1992).

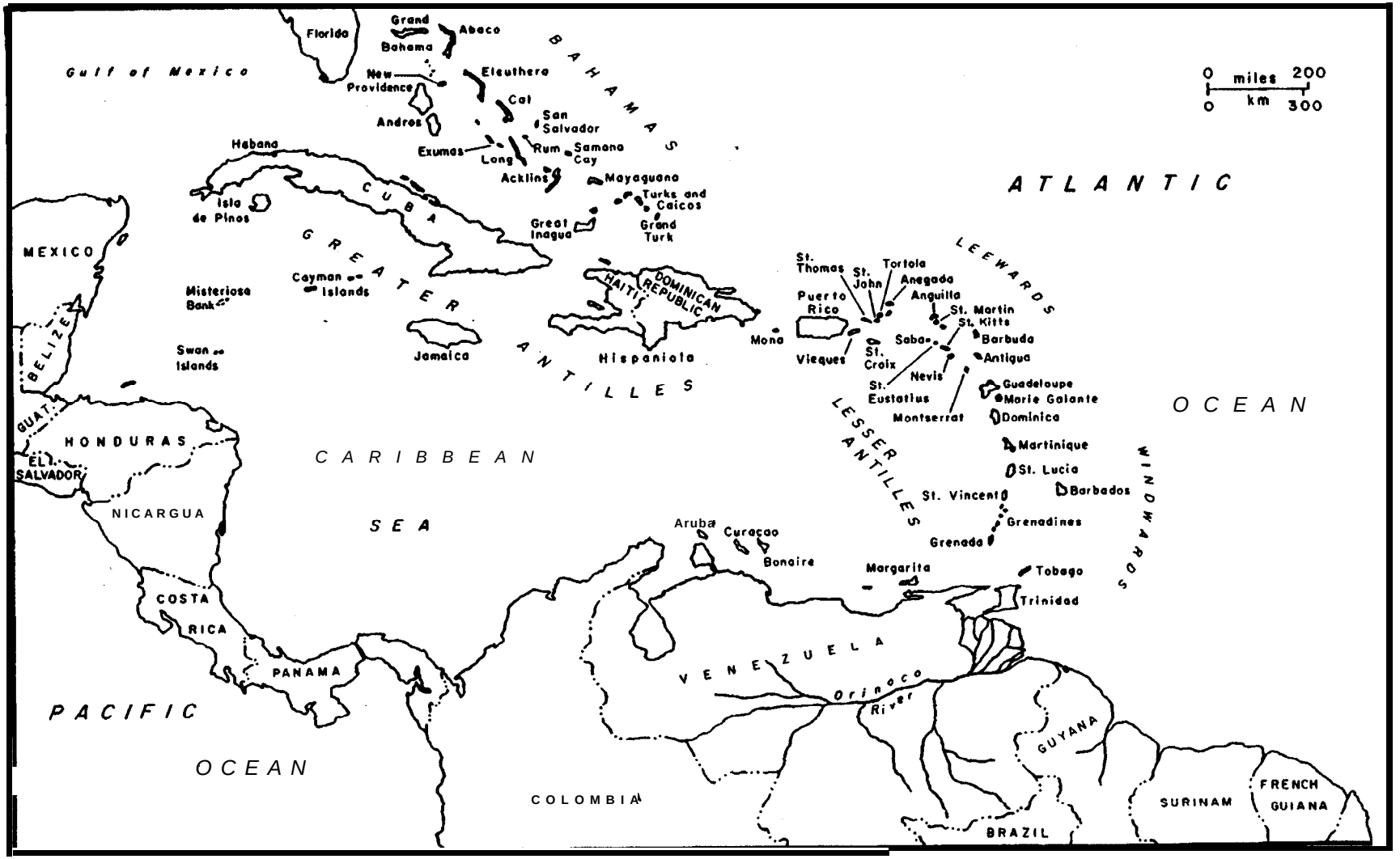

FIGURE 1. Islands of the West Indies (Source: Keegan 1994).

This emphasis on the expansion of a single people or culture emerged to counter efforts to explain every cultural change in the Caribbean as the product of new immigrants (e.g., Chanlatte Baik and Narganes Storde 1990; Rainey 1940; Veloz Maggiolo 1993:77-80; Zucchi 1990, 1991). While others proposed multiple migrations into the West Indies to account for changes in pottery styles, Rouse has maintained a unilineal model in which the Saladoid culture and its descendants have become increasingly impervious to outside influences (Rouse 1986, 1992). All variability and change are attributed to ***in situ*** developments from an initial cultural endowment, though this "development" occurs largely though the loss of variability (Roe 1989:270). The devolution of ceramics is such an important process that it has been suggested that, like Pacific islanders, the natives of the Bahamas were in the process of abandoning pottery when Europeans arrived in the late fifteenth century (Watters 1982:6), a view that I do not share.

In a sense, the Saladoid peoples were viewed as living in cultural isolation. There were mainland Arawaks and there were "Island Arawaks," and once these people moved to the islands there was no going back. Despite remarkably short water passages, communication with South America is thought to have ceased around **A.D.** 500, and exchange with Central and North America apparently never developed (but cf. Lathrap 1987).

Recently, a new view of the ceramic-age colonization of the West Indies has begun to emerge. It is now known that Puerto Rico was settled by 400 **B.C.,** and that the earliest known settlements are clustered in the central Caribbean on the Leeward Islands, Virgin Islands, and Puerto Rico (Haviser 1997). The inescapable conclusion is that most of the Lesser Antilles were bypassed or leapfrogged in a direct jump from Venezuela or Trinidad to Puerto Rico and its neighboring islands (Keegan 1995).

The Windward Islands of the Lesser Antilles were then settled later by peoples moving south from Puerto Rico and north from South America. Until the populations in the Antilles achieved an equilibrium with those in South America, the conditions that stimulated the initial migration continued to fuel emigration from South America. Thus, the Saladoid expansion was not the single-culture migration usually portrayed (Rouse 1992): more likely it involved the movements of a variety of competing ethnic groups from the South American mainland over the next millennium (Roe 1989).

Just as in the Pacific, the sense of isolation and remoteness attributed to Caribbean islands is breaking down. But while Pacific archaeologists are laboring to dispense with the so-called long pause between the colonization of western and eastern Polynesia, an equally long pause has emerged in the West Indies. Ceramic-age colonists settled 'Puerto Rico by 400 **B.C.,** yet there is no evidence for expansion out of Puerto Rico until after **A.D.** 500 (Keegan 1995).

When expansion finally resumed, settlements appeared simultaneously in Hispaniola, Cuba, Jamaica, and the Bahamas. For now, the presence of well-established foraging populations (called "Archaic") on Hispaniola and Cuba are used to account for the development of a frontier between Puerto Rico and the Dominican Republic (Rouse 1986, 1992). However, given the speed with which the second wave of expansion took place, it is likely that the Saladoid invaders melded with indigenous Archaic groups to form the population from which the historic Tainos emerged (Chanlatte Baik and Narganes Storde 1990). Future research will tell.

In this brief commentary I have emphasized questions concerning the ceramic-age colonization of the West Indies because these best fit the frameworks developed in Geoff Irwin's book. As in the Pacific, archaeologists in the Caribbean are moving away from isolationist models to models that recognize the presence of multiple ethnic groups and cultural mosaics or matrices (Hulme 1993; Keegan 1996; Whitehead 1995:9-22; Wilson 1993). It will be especially interesting to compare how the "long pauses" in the Pacific and Caribbean are finally resolved.

New perspectives notwithstanding, the notion of "islands as laboratories" is not dead. The geometrical properties that provided the foundations on which the theory of island biogeography was established remain important structures for evaluating the colonization and development of insular societies. However, because all formal models oversimplify reality, we tend to be disappointed, or critical of their use, when the models fail to provide realistic answers to complex questions. Yet it is exactly these failures of the models to explain observed phenomena that provide our most significant insights.

In sum, the theory of island biogeography is important because it identifies variables that are useful to measure. Its significance, in the sense of a laboratory or natural experiment, derives not from what it explains or predicts, but from those observations that deviate from expectations. It is in these deviations that human choice and cultural norms can be observed.

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Review: **CLIVE GAMBLE**
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Boats are something I am content to let other people steer. All that necessary knowledge about tides, currents, winds, stars, and lee shores, not to mention the robust jargon of ships and shipping, just fills me with admiration for those who have it. Of course I could learn it, like acquiring another foreign language, but thanks to Irwin's book I can now stick with the translation.

But why should I let my ignorance of things nautical lead to my approval of this book? I can think of two reasons that have nothing to do with the sea. First, there is the intellectual economy in the uniformitarian principle that underpins the navigational theory of colonization. Even a landlubber like me can understand the principle of against and across the wind (p. 132). Here is a demonstration of one of those simple bridging arguments that allows us to explore the data of the past as the outcome of human decisions. These decisions were determined but not dominated by the environment that surrounded prehistoric people. The same principle applies to Irwin's studies of inter-visibility, accessibility, and remoteness.

But uniformitarian principles always need testing with independent data. They cannot be assumed to act as explanations. This was Heyerdahl's mistake. He followed a uniformitarian principle--the direction of the currents--to its deterministic but wrong conclusion for human colonization (1950230). Common sense may be cheaper than archaeological fieldwork, but it invariably leads to an impoverished history of past events and a downgrading of human action to natural causes.

Understanding the present requires a historical perspective that involves taking into account human intervention. For example, island biogeographers used diversity as a measure of the constancy of the relationship between time and distance in determining community composition. Consequently they have either ignored or downplayed human impact on island faunas, particularly in the Pacific, as Flannery and White have shown (1991). The problem displayed by biogeographers arises when history is only conceived in the narrow sense of elapsed rather than constructed time (for a full discussion, see Gell 1992; Gosden 1994). Irwin shows instead that knowing the history, or in this case the prehistory as revealed by archaeology, is essential if we are to learn something about Pacific islands. The perspective supplied by Irwin is a human prehistory where choice and contingency played a dominant part in exploration and colonization although tempered by the forces of the environment.

The second reason concerns his insights into human colonization generally. His elegant analysis finally dismisses the idea that humans are random colonizers; rather, they have intention and rationality. I particularly liked his

demonstration of their concern for safety rather than speed (p. 210). The remote Pacific was systematically colonized and although, as he notes, the motives may have been various and remain unknowable (p. 211), the intention can be traced from the outcome. I am in complete agreement with this assessment, having concluded elsewhere that the singular fact of global colonization by modem humans was due to the fact that they had purpose (Gamble 1993). I have discovered however that such a conclusion makes reviewers gibe. For some of them purpose, like walking, is an assumed property of modem humans. Therefore, they say, to claim purpose is to assert nothing. Instead they are happier with human colonization being driven by chance, hunger, population pressure, the wind, or that lucky technological breakthrough. The result is that our prehistoric ancestors are presented as demiurges, driven and determined by external forces. They stand outside history with its contingency and human choices.

An example of this approach is provided by Terrell's ***Prehistory in the Pacific Islands*** (1986). The complexity of the region apparently requires a complex answer and one moreover that only scientific methods can uncover. The message seems to be that if only we could strip away the complexity of Polynesian society, then we might find a complex scientific truth beneath. But first we have to batter the problem into submission. Hence Terrell produces "one possible model of the peopling of the Pacific islands based on . . . five observations, four descriptive variables, two causal variables, and five operating rules" (1986:59). He might have added a partridge-in-a-pear-tree for good measure, but continues, "This is only one of many possible models that could be built with only minor changes in the variables, rules, and assumptions used" (Terrell 1986: caption to fig. 19). The result is a ghastly confusion of possibilities that here, and in the past, has steered the study of human colonization onto the rocks. The contrast with Irwin's navigational theory of colonization could not be greater. The simplicity, but strength, of Irwin's approach establishes exploration and colonization as processes rather than events, and from this baseline any subsequent complexity can be investigated. This is the way to tackle the question of physical, linguistic, and cultural diversity of the region--not by reference to some preferred causal variable. I was therefore pleased to see that population pressure, a favorite causal explanation of colonization, does not even figure in Irwin's index. People don't need babies to give them a reason to colonize. People have other purposes and intentions, and it is those I want to now comment on.

Social Life and Colonization

Purpose is derived from social life. As Irwin comments, colonization was not forced but was, rather, part of the colonizers' structure of ideas (p. 212). It is

this human ability to construct social life that deserves more attention since it returns agency, the act of doing (Giddens 1984:10), to the study of prehistoric societies. It is a necessary step in returning a sense of history to prehistory, where generally scientific explanations involving ecology, laws of thermodynamics, and natural selection dominate to the exclusion of all else. Bradley summed this up in a celebrated aside that archaeologists still stress how "successful farmers have social relations with one another, while hunter-gatherers have ecological relations with hazelnuts" (Bradley 1984:11). The earliest Pacific colonizers may have related more to rats and island size, but the principle is the same. On the contrary, agency implies knowledgeable actors who, rather than entering existing social structures, are instead responsible for their creation and interpretation. Social structure is therefore contained in action. Obviously there are constraints to individual action, but these are not all negative or preordained in establishing the patterns of social life.

Archaeologists still tend to see society as somehow external to the actions of individuals (Johnson and Earle 1987). Societies such as bands or chiefdoms are analogous to the natural environment, exerting selective forces and existing independently of the individual. Although this view is rapidly changing, it still exerts a hold over archaeological approaches to issues such as colonization, where it is more common to dwell on the mechanics rather than the character of the social process (Keegan and Diamond 1987). An interesting comparison is with current approaches to the study of primate societies (Dunbar 1988; Hinde 1987; Strum and Mitchell 1986; Strum and Latour 1987; Waal 1982). The emphasis in these studies is on the relationships defined and maintained by interaction between individuals. Hence society is literally performed into existence through the actions of individuals. This is in contrast to the view that primate society is somehow hard-wired. Instead it is flexible and variable due to the actions of individuals operating within a wide array of constraints.

The link to colonization comes when considering two further aspects of agency, social and system integration (Giddens 1984:142). Social integration is what people do together. It covers the many and varied interactions that take place, face-to-face. System integration is concerned with how the patterns of social integration are extended in absentia. It is this extension, what primatologists Quiatt and Reynolds refer to as the ability "to go beyond" (1993:141), that really marks out human social systems and has particular importance in the context of long Pacific voyages. Colonization therefore provides an opportunity for us to examine how the limitations of performing society face-to-face were overcome in the course of human evolution to allow the stretching of relationships across time and space (Giddens 1984:35). We

know ***when*** this happened with the late arrival, in terms of human prehistory, of people on Australia and soon after in the islands of western Melanesia. Here is very tangible evidence for a major change in the way society was performed and structured. The question ***why*** is more difficult but Irwin's account suggests a direction.

The Pacific focuses attention on navigation as a human skill. The question is to what extent is this a transferable skill or one that had to be learned in the context of colonization. The latter implies strong selection pressure for such a long apprenticeship and the development of appropriate means for cultural transmission. The former suggests that these skills were fundamentally social since they were incorporated in the act of doing, of living in the world (Ingold 1993a, 1993b). I would draw the analogy between navigation and negotiation and where "sailing in a sea mapped in the mind" (p. 1) was comparable to that stretching of social relationships in absentia, that going beyond, with all the implications this held for remembering, using, and reviving knowledge about people and places.

Accordingly, exploration may well have been an unintended consequence of new negotiating skills. These were the dominant factors in the process of going beyond. The direction and pattern of such exploration were then influenced by the principles contained in Irwin's navigational theory of colonization. As an example of this relationship we can see with hindsight what happened when system integration was elaborated: the Pacific was colonized. But as the emerging prehistory of the Pacific islands shows, this was not a steady, relentless colonization. Just as Irwin uses the mystery islands to calibrate the decline of voyaging in parts of the Pacific (p. 194), so the different times at which it started speak to the complexity of the historical tradition that led to the inception of voyaging. Oceanic society, as Irwin concludes (p. 213), was truly wider than its islands.

The Ocean as Landscape

This brings me to my final landlubberly point. For me the ocean is an inconvenience to be crossed. Away from land it is as anonymous as the Channel Tunnel but much more dangerous. It is a wet, featureless space between solid landscapes with only the boat to remember it by. I could learn its jargon and acquire the skills of navigation but I cannot know it in the way I can know a mountain or a city.

Reading Irwin's book I get a different impression. The Pacific becomes a surface inscribed with the tracks of voyages. These paths are themselves features that incorporate the ocean landscape into human action rather than leave it to be treated as a separate, foreign environment to be simply con-

quered or traversed. The concept of landscape (Gamble 1995; Gosden 1994; Ingold 1993a) is increasingly used in archaeology to reunite the multifarious strands of human action that for too long have been chopped up for independent analysis. The study of global colonization, across a conceptually united landscape of land and sea, provides a powerful means by which the future potential of a social archaeology based on interaction and agency can be explored. This may seem ironic in the Pacific, whose island laboratories provided so many of the models for an earlier social archaeology (e.g., Renfrew 1973) but now seem to have little general relevance outside the history of the region.

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Response: GEOFFREY IRWIN

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Colonization is a hot topic in the prehistory of the Pacific and elsewhere. The five reviewers above have made a valuable contribution to the discussion. They enjoy a range of academic backgrounds and have often approached similar issues from different perspectives. This reply tries to draw many of their comments together into a number of themes. These include the need for navigational theory, the tempo of change, the identity of colonists in Remote Oceania, the question of continuity of colonization of the Pacific Plate, "mystery" islands, computer simulations, and future research.

But first let me say that I feel lucky to have had the opportunity to draw together the separate strands of this book. These include a background in

Pacific Islands archaeology that brought the chance to see and ride in traditional canoes large and small. A long interest in sailing came from growing up on the very shore of Auckland Harbour and later sailing in various boats in different places. A particular interest in colonization arose from an obvious need to add some voyaging realities to the subject. In 1985, at the same time as the small chartered sailing ship ***Dick Smith Explorer*** was supporting the Lapita Homeland Project organized by Jim Allen in the Bismarck Archipelago, a group of us took my own yacht ***Rhumbline,*** an 11.2-meter retired ocean-racing sloop built of New Zealand kauri wood, to survey the Louisiade Archipelago. Eventually, this became a round trip that spread over two years while I was a research fellow at the Australian National University. Writing this book afterwards left the ***Rhumbline*** tied up and growing weeds for most of the next three years.

The Need for Navigational Theory

In the centuries-long debate about Pacific colonization, the reputation of traditional navigators reached a low point in about the mid-twentieth century. This reputation was progressively restored beginning about the 1960s and there is now, at the end of the century, a major cultural revival of voyaging. Many scholars and sailors contributed to this development with pieces of fine ethnographic and historical research, which was supported by marvelous experimental voyages in various boats, rafts, replica canoes, and even in computers. All this is well known. But much of this work was concerned with fairly recent voyaging between known islands in an ocean already mapped in the minds of indigenous navigators and the charts of Western ones. Not enough attention had been paid to early exploration, to fixing the position of newly discovered islands without instruments, and to the process of initial colonization. There was a range of views about unknowns such as the rate of loss of life and whether voyages were generally one-way or return (i.e., there and back). More fundamentally, it was unclear whether there were purposeful strategies of exploration or to what extent canoes were transported across the sea by mechanical agency. It was unclear how navigational skills developed as colonization spread and ocean geography changed.

Ben Finney describes in his review how the good news about traditional navigation was rather slow to reach prehistorians. Little notice was taken of the spaces between the islands. For example, William Keegan comments that the early theory of island biogeography did not consider the motives of colonists or the means by which they reached islands. Generally, the sea was regarded as such a barrier that the theme of island isolation has been exaggerated until now. Keegan also discusses how the study of island laboratories

as if they were closed led to underestimating the effects of external interaction on internal change.

A lack of concern for the methods of navigation explains another persistent theme of Pacific prehistory: that water crossing ***happened*** to people rather than was something that they ***chose*** to do. In his review Clive Gamble draws out a number of such issues with admirable clarity: namely, that humans are not random colonizers but have intention and rationality; that they were influenced but not dominated by the environment; and that exploration and colonization were processes rather than events. His book ***Timewalkers*** applies such themes to world colonization (Gamble 1993).

So with those few words as context, my main conclusions are that the exploration of the remote islands of the Pacific was purposeful and rapid but followed the order of safety rather than speed. But once islands were known, interisland voyaging was able to follow the patterns of accessibility. These propositions allow many predictions that can be tested against evidence--as it exists and as it comes to hand. Computer simulation provides another kind of investigation.

Abrupt or Gradualistic Change?

Roger Green suggests that one theoretical framework for colonization might conform to a step or pause model as set out in his Table 1. This can be likened by analogy to biological evolution by punctuated equilibrium. But he also notes that many other aspects of cultural change with respect to voyaging may fit within a gradualistic framework. The world's first water crossings were made from mainland Asia to what Green terms Ancient Near Oceania (1991b) (or to a voyaging corridor, as I put it). In terms of evolutionary biology the initial settlement of this area is just one region implicated in the continuing global debate on the origins and spread of modem humans. Alternatives include both abrupt replacement or long biological continuity in the region. The accepted age of these early crossings is after 40,000 **B.P.** by radiocarbon and perhaps before 50,000 **B.P.** by thermoluminescence, but whether these dating systems are complementary, contradictory, or both, is still at issue (Allen and Holdaway 1995; Chappell, Head, and Magee 1995). A very recent suggestion of an antiquity for Australia beyond 116,000 **B.P.** (Fullager, Price, and Head 1996) might have implied the possibility of archaic ***Homo,*** but these early dates have not been accepted.

With regard to the second step, there was a long period of occupation of the voyaging corridor prior to the first moves beyond. Evidently maritime technology developed until, as Patrick Kirch puts it, people finally "burst out" into Remote Oceania after 3500 **B.P.** By this time deep-sea explorers

had learned how to search offshore and survive. But this was after some tens of thousands of years, which also saw the transition from Pleistocene to Holocene conditions and such changes as the emergence of a center of indigenous plant domestication in New Guinea, perhaps by 9000 **B.P.** (Golson 1991). This may have followed a long period of management of wild plant foods dating back to the Pleistocene settlement of the Bismarcks and northem Solomons (Groube et al. 1986; Loy, Spriggs, and Wickler 1992).

It may be difficult to distinguish between punctuated and continuous models of colonization after colonization began in Remote Oceania. We are dealing with a very different scale of time--now short enough to be relatively more affected by methodological problems including archaeological sampling and the error associated with conventional dating techniques. But certainly the question of whether Remote Oceania was settled continuously or in a series of steps merits close attention, below.

Identifying the Colonists

Messing about in boats may be one thing, but who were the people on board the canoes during significant episodes of colonization? With the passage of time the archaeological evidence fills out and such questions now come within range of what both Kirch and Green refer to as a holistic approach. Interdisciplinary studies allow similar problems to be approached through independent data and analysis.

The last two decades have seen spirited debate on the origins of Lapita. In his review Green refers to the different views held by archaeologists about the contribution to "things Oceanic" by way of Southeast Asian Austronesian input to the Bismarck Archipelago or from local cultural developments in the region (see also Green 1991a). In the matter of Lapita origins, some prehistorians have recently reemphasized a strong association of Austronesian language and aspects of material culture, economy, and settlement as intrusive to the Bismarck Archipelago (Bellwood 1996; Kirch 1997; Spriggs 1996). One aspect especially relevant to colonization is the linguistic reconstruction for Proto-Oceanic (Pawley and Pawley 1994) of what Green calls the Austronesian watercraft complex; Kirch also refers to the introduction to the voyaging corridor of the Austronesian outrigger canoe. But Finney is careful to raise another possible input not to be overlooked: "the local knowledge of indigenous sailors and farmers descended from the Pleistocene seafarers who first colonized this region." It remains to be seen what linguistic indications of this remain in other languages.

Discussions of introductions often proceed geographically from west to east, but while navigation is our main theme, arguing from the other direc-

tion may be more pragmatic. The reason for this reversal is that it may be simpler at this stage to establish an identity for the first people into Remote Oceania than to distinguish them in Near Oceania, which has a long and complex history. First, I am still sure that there was no settlement of Melanesian Remote Oceania significantly before Lapita. In my book I argue there is such a navigational threshold (pp. 20-21; see also Pawley and Green 1973) that a much earlier crossing would have been largely accidental and the possibility of it remains to be demonstrated by archaeology. In navigational terms Lapita was a single integrated expansion in Remote Oceania that followed a general strategy of upwind exploration by return voyaging. It left no evidence for crosswind or downwind voyaging. In this region Lapita represents an archaeological culture, and I have said it probably approaches an ethnic reality as well. However, insofar as Remote Oceania was the extension of a wider communication field that stretched back into Near Oceania, then more diverse elements could have passed through it over time. Certainly information about Lapita pottery decoration was flowing (Summerhayes 1997). Moreover, Lapita colonization may not have been confined to Remote Oceania. By their similar nature, some sites in Near Oceania could be seen as "colonizer" rather than as "homelands" settlements.

Lapita people were not the only ones around. It was to be expected that seagoing skills developed over a wide field, and we note that the first deep-sea settlers of West Micronesia evidently came out of the voyaging corridor west and north of New Guinea. Again, these settlers appear to have been the first people in Micronesia and similar navigational skills and strategies are implied. A similar set of arguments that they were Austronesian speaking can be made, as for Lapita. Dates back to 3500 **B.P.** are reported for the Marianas (Butler 1994), much the same as for the beginning of Lapita in the Bismarcks (Kirch 1997). And the intervening Micronesian island of Yap is now finally beginning to show evidence for comparably early settlement (Dodson 1995), as expected. Early migrants in the Marianas show technological similarity with Lapita. For example, the early pottery was red-slipped and decorated with stamping and fine-line incision. However, the precise archaeological signature is different.

In Island Southeast Asia to the west of New Guinea is another group of sites that are broadly comparable in technology, economy, and settlement to the ones mentioned above. The site of Uattamdi on Koyoa Island on the western coast of Halmahera dates from a time that falls between the range of early dates reported for Lapita settlement in Mussau and the Arawes (Bellwood et al. 1996; Kirch 1997; Summerhayes 1997). Uattamdi has plain red-slipped pottery with similarities to early Lapita, but some differences (Irwin et al. n.d.). Closer affinities may be with contemporary assemblages

at Bukit Tengkorak and Madai Cave in Sabah and Leang Tuwo Mane'a in the Talaud Islands (Bellwood 1976, 1988; Bellwood et al. 1996). Other associations at Uattamdi are of a kind seen by many prehistorians as part of a package of introductions related to the dispersal of Austronesian languages.

The question of precisely how similar these Southeast Asian assemblages are to one another bears on the question of how their occupants may be related historically. The same general issue applies to their similarities with Lapita assemblages in Melanesia and other assemblages from West Micronesia. There is an archaeological heterogeneity that bears on questions of identity and relationship. The questions apply within Lapita at a finer grain and Kirch accommodates this by referring to the Lapita "peoples" (1997:18).

Keegan draws a parallel between the Pacific and the Caribbean that may be relevant here. He says that in spite of the relative ease with which (it is now understood) people could have moved around the West Indies, the trend had been to assume such movement was difficult. Conventional wisdom has been that a single culture from the Orinoco River drainage called "Saladoid" peoples expanded through the area by a gradual advance from the south (Rouse 1992). However, Caribbean archaeologists are now moving away from isolationist models to ones that recognize the presence of multiple ethnic groups and cultural mosaics or matrices.

The relevant question for Near Oceania might be: does our current evidence for the period from 4000 - 3000 **B.P.** capture a suitable scale of heterogeneity among contemporary Austronesian and non-Austronesian groups together with the nature of interactions among them? It was not my purpose to try to answer this question in the colonization book. I am content to take the interim position that a Lapita people sailed into the Melanesian islands of Remote Oceania and, at about the same time, another people sailed into West Micronesia. Communities of the same or similar people may have also settled in islands of Near Oceania. The identity of the groups offshore is indicated by coherent episodes of colonization, and if each was the foundation culture, then there is no one else to confuse them with. Biological evidence may one day indicate more about the genetic composition of these populations. On the other hand, I am not so confident that we have the evidence to interpret a mosaic of identities and interactions that may have remained in the voyaging corridor. There is still no known archaeological site that shows how the elements of Lapita came together, even though the general whereabouts of a "homeland" may be predictable.

Pauses in Remote Oceania

My position is that voyaging in the remote Pacific was an unbroken tradition, that the impetus to colonize was not interrupted, and that any apparent

pauses in settlement should be explained by changes in environmental circumstances, in the first instance, before we seek cultural ones. A related point is that there are cases where intervals of time separate discovery and settlement. Underscoring this latter point, and as Green notes, is the present debate whether the Pacific rat ***(Rattus exulans)*** may have been brought to New Zealand a thousand years before permanent human settlement (Anderson 1996b; Holdaway 1996).

Finney takes up the issue of whether colonization accelerated as it went. Taking a piece of string to represent 2,100 nautical miles, he measures whether colonization traveled as fast over a second length as in the first. My preference would be to begin with a shorter piece to represent the first voyages and lengthen the string with progressive offshore experience (and wider water gaps). However, the key question is whether colonization rate should be measured by linear distance to islands discovered or by area of ocean to explore. A look at the map will show that Lapita sailed within quite a limited range as compared with what was to follow. One thing the computer simulation showed was that there was more land to be found had they sailed towards it--Australia for a start. It seems the Lapita seafarers directed their attention upwind and, not knowing what lay ahead, they knew the direction from which they could most easily return. Compared with their descendants in East Polynesia and East Micronesia, one might even suggest the range of Lapita colonization was modest. I think this indicates that navigational knowledge was not as advanced, at that time, as it was to become. Nor did it need to be. Lapita culture did not leave its mark beyond the large archipelagoes of Island Melanesia and West Polynesia.

But compare the subsequent range of colonization in Polynesia and Micronesia. The ocean expands hugely to capture Hawai‘i, Easter Island, New Zealand, and Micronesia (not to mention South America). Unless, by a miracle, canoes tracked directly to these very distant and isolated islands, then a lot of empty ocean was traversed. The point is that expanding space absorbs more time. But was this continuous or discontinuous time?

Finney asks a very significant question that pertains to the discussion above. Why did colonization apparently stop in West Micronesia when voyagers could have been encouraged onward into the Carolines by the emerging reefs and shoals that would later be occupied as atolls? A delay in the radiocarbon dates occurs in both East Polynesia and East Micronesia. The former is the so-called Polynesian long pause. The issue is also raised by Green, and Kirch suggests that “nothing has been more contentious in Pacific archaeology” than the debate about the chronology of settlement in central and eastern Polynesia. He says that chronometric hygiene as applied to the corpus of Cl4 dates (Spriggs and Anderson 1993) has opened up the pause into a “yawning gap.”

In fact the pause applies to the wider Pacific Plate and must be seen from that perspective. After 3500 **B.P.** deep-sea colonists spread into Remote Oceania to settle the remaining continental islands of the western Pacific. By 3000 **B.P.** a string of settlements with ceramics stretched 6,000 kilometers along the edge of the Pacific Plate from the Marianas to Tonga. Yet almost nowhere is there archaeological evidence to demonstrate that canoes crossed this geological divide that was hidden beneath the sea. The one exception concerns ʻUvea and Samoa where their close proximity to neighboring islands of Fiji and West Polynesia led to the rapid discovery and settlement of all. The speed of settlement among the continental islands has made the subsequent delay more puzzling. However, although there is a geological threshold here, there is no navigational one. The closer islands of East Polynesia, such as the Southern Cook Islands, present feasible targets compared with previous ones. Kirch attributes part of the anomaly to methodological problems that have been raised in the past including archaeological visibility, various sampling issues (Irwin 1981; Kirch 1986), and the acceptability of radiocarbon dates.

Green and Kirch may take rather different views on the length of the pause but we all agree more archaeological exploration is required to resolve it. In my view the case made by Kirch and Ellison (1994) for c. 500 **B.C.** human settlement of Mangaia in the Southern Cooks is in line with the wider context. They describe evidence of declining values for forest pollen and the appearance of charcoal and erosional sediments of similar age. Not all scholars agree with the dates or the anthropogenic cause (e.g., Anderson 1994). However, it seems fair to say that similar evidence located in a less-contentious time and place has been more readily accepted. The archaeological evidence from Mangaia, which includes bird extinctions, currently dates from only c. **A.D.** 1000, but such evidence of human impact on fauna does not necessarily contradict the evidence for earlier impact on vegetation and soils. Indeed, this may prove to be informative about the scale and nature of subsistence and settlement.

Chronological predictions follow from theories of exploration strategy, although such predictions are more closely concerned with the relative order of island settlement than with absolute dates. Graves and Addison maintain that Hawaiʻi had an archaeologically established population by around **A.D.** 600 (1994). If it was settled successively later in time than the Tuamotus/Marquesas and the Societies, then there is a clear need for earlier settlement in the Southern Cooks.

But even a shorter pause of half a millennium requires explanation. Archaeologists are aware that settlement of the volcanic islands of the Pacific Plate may have required various new adaptations. However, while changing

ecological circumstances might have slowed settlement, they would not have prevented discoveries. But we may have underestimated the issue of social adaptation that follows from diminished land area and increasing isolation. How much ocean area progressively expands with distance east on the Pacific Plate is hard to exaggerate. For example, within an area of some 2 million square kilometers surrounding the hundreds of islands of Fiji and West Polynesia is a total land area approaching 24,000 square kilometers. Within the same-sized area of ocean surrounding the Southern Cook Islands are less than a dozen habitable islands, with a total land area of approximately 240 square kilometers. The ratio for the change is in the order of 1:100. Both East Polynesia and East Micronesia were colonized by people living initially in greater isolation. If this represents a settlement threshold, then Lapita never crossed it (Irwin 1998). The circumstances for settlement of the Pacific Plate were expanding ocean area, increasing navigational experience and skill, more elapsed time during colonization, and greater isolation of settlement. The time interval between discovery and effective colonization becomes an issue.

I do not mean to say that people had to learn to live in greater isolation. Rather, they needed to learn to mediate or manage it. This may have involved a greater frequency of long-distance voyaging until a sufficient population density was established. Kirch made a perceptive comment along these lines about the role of exchange in early Lapita settlement. He saw the formal exchange of noncritical resources as a kind of reinforcing lifeline back to parent communities (Kirch 1988).

Gamble refers to "a human prehistory where choice and contingency played a dominant part in exploration and colonization although tempered by the forces of the environment." This may apply to the "long pause," such as it was.

Ocean Landscapes and "Mystery" Islands

It is a moot point whether Pacific island communities could ever live in isolation. I believe island societies were generally wider than their islands, and where they were not the island population was at risk (depending, of course, on island or archipelago size). Archaeologists have recently begun to appreciate the scale of postsettlement voyaging, and a first phase of investigation is finding increasing evidence for inter-island trade. I think this is only the start and a next phase may reveal that some internal social transformations occurred in the context of external contacts and influences. There are many interesting similarities and differences between island prehistories to consider. Oceans, like continents, are continuous fields. Gamble says, "The

Pacific becomes a surface inscribed with the tracks of voyages. These paths are themselves features that incorporate the ocean landscape into human action rather than leave it as a separate, foreign environment to be simply conquered or traversed."

To digress briefly back to the "long pause," acceptance that cultural diversification can continue in the context of continuing voyaging after colonization reduces a perceived need for cultural pauses during colonization in Remote Oceania. In other words, there was less obligation for people to stop and undergo a cultural change before they could proceed.

It was the decreasing frequency of interisland voyaging that stressed small communities in particular during the second millennium **A.D.** Kirch finds increasing isolation an insufficient explanation for the abandonment of the "mystery islands" and he may well be right. However, the "extinction line" defined by accessibility to outside contact (p. 175, fig. 67) unambiguously identifies the empty islands that, in many cases, had prehistories quite independent of one another. And that line was moving with changes in voyaging frequency. So, even if isolation was not the ***cause*** of their abandonment, I would still say that adequate communication was a necessary ***condition*** of living on islands. Measurements of island or archipelago size and accessibility point to the islands that will suffer other problems.

Computer Simulation--Fact or Fiction?

Finney makes a number of pertinent comments about the nuts and bolts of the computer simulation of voyaging (see also pp. 133-173; Irwin, Bickler, and Quirke 1990). As Keegan comments, all models simplify reality in order to represent it. What gets included and what is left out usually depends on the aims of the experiment. One shortcut we took was to simulate only January and July to represent the seasonal weather patterns for summer and winter. Although we actually had twelve months' weather data, we found we could make our points without having to use them all. Including other months would have generated a very large number of intermediate solutions. However, one arbitrary consequence was that some canoes went sailing for a fifty-day month!

The simulation also selected winds on a daily basis, when in fact weather is continuous. Weather patterns such as low-pressure systems with associated fronts build and move and change over a period of days and their progress can be observed and predicted. One supposes that real navigators would have chosen a weather system to set off on and dealt with others as they came. But ours had no memory of yesterday's wind and no forecast for tomorrow. (Actually it is still hard to get weather forecasts at sea in the trop-

ical Pacific.) So some artificial changes of weather occurred in the computer as one day ticked over into the next. But this approach would not have affected the overall outcome of our experiments, because daily winds were selected according to probabilities based on records by month. Even so, new ventures in voyaging simulation will probably use more-realistic data as there are now computer models of global weather containing immense data (although these usually have inbuilt aspects of simulation themselves). Modeling continuous weather data will allow additional sailing strategies to be investigated. I would expect that if simulated sailors are given more weather intelligence they would be even more successful, not less.

Another pertinent question is whether it was unrealistic to sail virtual canoes over a currentless sea? Yes, it probably was. And one way to measure the effect would be to rerun some simulation experiments that include current data. This would not be hard to do, although conventional current data are generally less fine-grained than those for wind. Our reason for omitting current was that wind is by far the major variable in boat speed. On the other hand, current is an important consideration when navigating to a particular island target. Like leeway, it can offset a boat from its course. Leeway is a property of the boat as it moves; it varies with boat direction relative to the wind and it can be observed. Current relates to place, although its velocity and direction may vary. Finney, elsewhere discussing Andrew Sharp's view that current could not be judged accurately except over short journeys (Sharp 1963:35-37), noted that the "equatorial current that sweeps past most Polynesian islands is a fact of life, knowledge of which was essential to coastal fishermen as well as to inter-island voyagers" (Finney 1979:333). Certainly, current had to be taken into account in navigation. But our simulation was mainly about getting to previously unknown islands for the first time. Precise position fixing applied when getting back to them for a second time.

However, as Finney notes in his review here, the case does become more relevant when we are dealing with a canoe trying to make progress into the east against both wind and current. As he explains, windward progress of a canoe sailing at four knots per hour into a half-knot current could be cut to half a knot, or twelve (sea) miles a day. Anderson makes a similar point (1996a). But we all know the wind does not blow from the east all the time. If it did Thor Heyerdahl would have to be right. There is the intermittent effect of the northwesterly monsoon beyond West Polynesia in the summer, and subtropical weather affects winds in the southern reaches of the tropics at whatever latitude they may be found in any season. Captain Cook found out about these "westerlies" at Tahiti on his first voyage (Beaglehole 1968: 137-139). John Williams, head of the London Missionary Society mission at Raiatea (Society Islands) from 1817 to 1839, gave an explicit account of their

Tahitian names, the months they blew, and their duration. He describes his own experiences, including one voyage from Rarotonga to Tahiti with a fair wind and fine weather all the way (Williams 1837:169). To turn to modern times, in July and August 1986, on her only passage from West to East Polynesia, the ***Hokule'a*** canoe took ten days from Samoa to the Southern Cook Islands and another ten on to Tahiti. Over the two legs the canoe averaged around sixty sea miles of eastward progress per day, although she by no means sailed a direct route (Finney, Rhodes, and Thompson 1989). This was in the trade-wind season. It was a brilliant example of strategic use of weather, the use of noninstrument navigation, and of dealing with the contingencies that arise at sea. The computer simulation seems to be in the same ballpark as a number of such real-life passages. For comparison with the voyaging simulation, in one experiment of 325 canoes that set off eastward from Western Samoa for eighteen days, 49 reached the Northern Cook Islands and 44 the Southern Cooks; a further 25 got directly to the Societies and 4 to the Tuamotus; none reached the Marquesas (p. 151). In addition, even without current the simulated voyages along the north and south tracks between Hawai'i and the Tuamotus/Societies are generally similar to the experiences of the ***Hokule'a*** and the remarkable fleet of canoes that traveled north over that route in 1995.

One further comment about sailing east when the wind is in the east: usually it is by no means due east but has a northerly or southerly slant. We could expect canoes to take the best tack, wind shift by wind shift, as real boats do between episodes of other weather.

To turn to more important things, there is little point in getting a bad back crouched over a computer playing with simulations that simply imitate life. We know about real things already. Where simulations help is in making explicit the variables relevant to real life. In this case the simulations did so, by suggesting the kinds of information needed to make the various passages actually made in prehistory and by showing the increasing levels of skill required as colonization extended in space and time. The simulations were also both illuminating and rather subtle in showing that certain voyages were possible that, according to archaeology, were not made. These results offer insights into exploration strategies or, as Gamble says, human choices.

Where to Next?

Kirch comments on how sophisticated anthropological discussion of Pacific origins and voyaging has become. But where do we go next? More archaeology on more islands is needed to produce the data to resolve various issues. Changes to existing models are inevitable. This is probably the most impor-

tant aspect of future work but it will take time. I hope enough new scholars will do their pioneering in the field as well as at the keyboard.

The study of colonization can be expanded to include early subsistence strategies. But we still have to be careful to distinguish means from ends. For instance, I would prefer the motivation for oceanic colonization to lie more in the minds of explorers than in the stomachs.

More computer simulation would be useful and should use a new generation of computer-modeled climatic data. Simulation of water crossing in the voyaging corridor should employ corrections for Pleistocene climate and sea levels. As for colonization of the Remote Pacific, it would be useful to simulate weather patterns over a period of some years. Perturbations such as El Niño need more attention (see Finney 1985). With regard to the different voyaging strategies there could be development of Strategy 5, which involves voyages with indirect returns through intermediate islands rather than return trips from A to B and back. Also, easier targets could be used as waypoints to more distant and difficult targets. However, I doubt that using more-realistic data of wind and current would greatly alter the conclusions to date. But allowing navigators the kind of ability to forecast weather as described by Banks (1962) on Cooks first voyage to Tahiti might produce surprising results. It could make the navigation aspect of inter-island voyages in known ocean even more secure although the passages themselves would be by no means uneventful.

Some informed person might take a modem anthropological approach to voyaging traditions, which often, if not always, have had a hard time in the past. But tradition is a living thing with its own social dynamics. In a similar vein one might consider the structure of social categories, perhaps as reconstructed from proto-language, but I hope we can come up with something more enlightening than two millennia of migrations by ambitious younger brothers.

Motive is an ultimate question, but the extent to which it is an archaeological question is not clear. Just as prehistorians have distinguished ultimate from immediate origins, much the same may be said of motive. At a very basic level colonizing behavior seems to be part of being human. However, there is still a lot more to be said for the Pacific, even if the most useful suggestions made so far have been about what motives or “pressures” to eliminate. Another thing to remember is that voyaging was going on before the colonization of each part of the Pacific and it continued afterwards.

We are so removed from Pleistocene voyaging that its circumstances rather than its events have been perceptible so far. For voyaging in Remote Oceania the evidence is more recent and discernible. As I suggested before, voyaging was an unbroken tradition and colonization was a continuous pro-

cess "embracing a multitude of individuals, events and transformations played out on island after island" (p. 212). And subsequent cultural change should be seen in the context of continuing voyaging although that, too, changed with time.

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VISUAL MEDIA REVIEWS

Mountains of Gold: The People of Porgera. 1993. Video, 52 min., color. Produced and directed by John Davis. New York: Davis Film & Video, distributed by Filmakers Library (124 E. 40th St., New York, N.Y. 10016; http://www.filmaker.com). US$395; $75 rental.

Reviewed by Dan Jorgensen, University of Western Ontario

PAPUA NEW GUINEA HAS AFFORDED a rich field for ethnographic films notable for their attention to the contexts in which people negotiate their often troublesome engagement with modernity. Yet filmmakers have devoted relatively little attention to what, since the Bougainville rebellion and the Ok Tedi lawsuit, has attracted international media attention: the relation between indigenous people and Papua New Guinea's growing number of mining projects. All the more reason, then, to welcome ***Mountains of Gold,*** which focuses on the situation of the Ipili people, landowners at the site of the large Porgera gold mine.

An opening voice-over announces, "It is often fashionable to tell a story like this as the destruction of the noble savage by the wicked ways of capitalism, but the Ipili are a practical and resourceful people." The film thus promises at the outset to go beyond shopworn stereotypes by portraying the Ipili not as victims, but as active players on the contemporary scene. For the most part it makes good on this promise by attending to the hardheadedness with which they manage their affairs in the world that transnational mining has brought to them.

One of the film's strong points is the historical overview it gives of mining in the Porgera area. An aim of the Black and Taylor patrol of the 1930s was to find gold, which John Black sums up by saying that "good colors [were] showing" at Porgera--prospector's parlance for signs that local streams bore traces of gold. Small-scale prospecting began following the Second World War, and in 1984 sluicing got underway. At that point significant numbers of local people were hired as laborers and, more importantly, issues of compensation for landowners came to the forefront, where they have remained since. By 1990 Porgera Joint Venture (PJV) went into operation, opening one of the worlds largest gold mines.

From this point on, much of the footage is devoted to an examination of the tensions and maneuvering that inevitably arise when mining transforms land into a source of royalties and compensation payments on a scale dwarfing subsistence farmers' usual incomes. In the run-up to mine construction, PJV paid out US$12 million in a variety of compensation claims, and continual negotiation was the order of the day. In a scene that must stand for many, a PJV surveyor relocates a road route to avoid an intransigent landowner's demands for compensation, the surveyor's frustration spilling over as he asks, "who else wants to stop the work?" Another company employee complains that as soon as word gets out that PJV is interested in drilling in a particular area, new houses spring up overnight, the better to stake compensation claims. In this part of the story, the Ipili show themselves to be capable and shrewd in their dealings with PJV, much to the latter's chagrin.

The eagerness with which mining was awaited is shown in shots taken in the 1980s when Kule, an Ipili man, complains that there had been too much talk about mining and too little action for his liking. The same man is later shown--after he and his family relocated to housing PJV provided when the mine went into production--with a new wife, who proudly displays their home freezer. The wealth is obvious, as is Kule's satisfaction with his new prosperity.

But the situation is not without its problems. Kule's wife is not Ipili, but comes from a neighboring area, and part of the attractiveness of the match is revealed by her relatives' willingness to accept reduced bridewealth payments in exchange for a diffuse connection to Porgera and its wealth. Such marriages have become something of an issue among Ipili women, who complain that second wives originating outside Porgera are responsible for the spread of sexually transmitted diseases.

Whether "outside women" are a menace to public health or not, the charge exemplifies tensions rooted in disparities that are largely a function

of proximity to the mine. Porgera has acted as a magnet for people from surrounding areas, who often come at the invitation of Ipili hosts who welcome outsiders as allies to bolster their strength. Thus Ekake, an Ipili landowner, has been joined by more than 200 Huli and Enga affines, and he makes it clear that he sees this as a way of underwriting his own position. Such tactics are double-edged, however. Ekake later complains that his allies should be given payments of their own, rather than sharing in those made to him. Here the division between landowners and others manifests itself in demands that PJV pay for "all" the local land (and not just that needed for mining) and help look after the new immigrants.

Ekake's situation registers a larger problem facing the Ipili, for the influx of outsiders--mainly Enga and Huli--has exacerbated local discord. In 1986 Porgera's population stood at 10,000, but by 1993 that figure had grown to 15,000. Although many came in search of work, the mine employs only about 900 local people, leaving the rest to shift for themselves. Enga newcomers number about 2,000 and figure as the film's villains, styled as one of Papua New Guinea's "wildest and strongest" groups. In one memorable scene, closely packed Enga men brandish spears as they dance menacingly forward to claim death compensation from local Ipili. Escalating Enga demands contain an element of intimidation, and the Ipili complain about their relative weakness vis-à-vis the Enga. Further afield, another Enga group blocks roads, threatening to starve the mine and its township out unless their demands for payment are met. Faced with this, the viewer is left to conclude that if the Ipili have been tough and resourceful in dealing with PJV, they have had a harder time holding their own against the pressures of their neighbors.

The film closes on this note, citing an Ipili myth that the world will end when too many outsiders come to Porgera. Declaiming before a group of onlookers, an older man confronts the camera and with a mocking laugh says that in his day people had to fight for their lives; now his children have to fight for their land.

The Ipili may turn out to be victims after all, the film implies, but victims of local politics rather than of transnational mining per se. Critical viewers will find this troubling--the silences on environmental issues and the role of government are deafening, and the mining economy has after all been the source of discord--yet Davis is surely right in emphasizing the intensely local nature of the tangled politics surrounding the mine. Without an edifying moral, the story departs from the tales of victimization and resistance we are accustomed to telling, reason enough to recommend ***Mountains of Gold*** to a wide audience in and out of the classroom.

Lords of the Garden. 1994. 55 min., color. Produced and directed by Judith Hallet for Hearst Entertainment. New York: Arts and Entertainment Television Networks; Paris: Tele-Image. Released in the United States in abridged form as ***Treehouse People: Cannibal Justice,*** VHS videocassette, 50 min.; distributed by A&E Television Networks (l-800-625-9000; http://www.aetv.com). US$24.95.

Reviewed by Stuart Kirsch, University of Michigan

Cannibal Tales: Review Essay

"Rumors about an all female tribe in Irian Jaya turn out to be false," reported the ***Jakarta Post*** (1991) after an Indonesian government expedition visited the Korowai in the interior lowlands of southern Irian Jaya. Initial reports suggested that "the tribe comprised females only and captured males from other tribes for reproductive purposes." These male prisoners and any male offspring were alleged to have been put to death. Instead, the government patrol established that the Korowai were "fully equipped with a supply of males," who "cover their genitals with cone shaped dried leaves, which are more simple than the better known ***koteka*** penis [gourds] worn by many other tribes" in Irian Jaya. They live in "thatch houses built high up on tree tops about 25 meters from the ground" (ibid.).

Such sensationalism commonly accompanies reports about the discovery of "isolated" or "lost" tribes in New Guinea (Kirsch 1997).[1] In 1995, it was announced that another tribe "practicing cannibalism [was] discovered in Irian Jaya" (British Broadcasting Corporation 1995). According to the official Indonesian news agency Antara, "the primitive tribe, living in tree houses, was sighted in the southern regency of Merauke" (ibid.). The members of this "new tribe" were described as "still living naked and still liv[ing] in trees . . . and [possibly] still practicing cannibalism." A month later the international news media reported that "Indonesia [is] preparing contacts with alleged tree cannibals" (Agence France Presse 1995).

Tree Cannibals on T.V.

This is the milieu of the film ***Lords of the Garden,*** which depicts Smithsonian Institution anthropologist Paul Taylor's 1993 visit to the Korowai.[2] The film was originally planned as part of a larger comparative research project on human use of the rain forest, although this broader program had been abandoned by. the time filming began. From the outset, filmmaker Judith Hallet sought to produce a documentary that represented "New Guinea as a

land of mystery, myths [and] headhunting" (cited in Kaupp 1994:9). Actor Mark Harmon introduces the shorter, broadcast version of the film, which was given the title ***Treehouse People: Cannibal Justice.***

The film is organized as a journey of discovery to the territory inhabited by the 4,000 Korowai. It follows Taylor and his entourage as they travel by plane and canoe and on foot through the rain forest. Harmon describes the hazards that the team faced, "clawing through the mud, fighting heat and insects--even chancing death." Their trip took them close to the "edge of the so-called 'pacification line,' . . . beyond which inter-clan warfare is active and outsiders cannot venture" (Kaupp 1994:8).[3] The threat of violence and frequent allusions to cannibalism are used to provide dramatic tension otherwise lacking in the film.

Taylor acknowledged the limitations of the project from the beginning, writing in his diary that the "concept of filming the anthropologist going to 'contact' a previously uncontacted group of people is outdated: 1) 'contact' is not a genre of valid anthropological research, and 2) even if it were, everybody here is already 'in contact' " (Kaupp 1994:9). Yet even if one sets aside the inappropriateness of the discovery paradigm for anthropological research, the Korowai are still presented as though they live in a vacuum. There are only two substantial exceptions: the tabloidlike opening vignette about the head-hunting Asmat and the disappearance of Michael Rockefeller (see Morris 1996:142-143), and brief references to the specter of precipitous change through logging.[4] Although Taylor justifies the project in the name of "science" (Sidharta 1994), the film makes little attempt to place its subject matter in the context of other ethnographic research in New Guinea. Either Taylor failed to do his homework or the structure of the film--an epic voyage into the unknown--prevented him from acknowledging how much is already well known.

In addition to the misguided focus on the researchers' perilous journey, two other themes of the film are the construction of Korowai treehouses and cannibalism, or at least the talk of cannibalism. In his attempt to tie these subjects together, Taylor posits a structural opposition between the "good life" symbolized by the domestic realm of treehouses and the shadow side of Korowai life, including cannibalism, murder, and hatred.[5]

A Room with a View

When Taylor climbs to the top of a Korowai treehouse for the first time, one of the crew members calls up to him, 'What's it like?" "Comfortable," he responds, "Lots of light, lots of ventilation, a penthouse suite." Taylor praises the architectural abilities of the Korowai, who build treehouses that soar as

tall as a six-story building without the use of saw, hammer, or nails, although today steel axes and knives expedite the task. Significantly, this was not the first Smithsonian Institution expedition to document the treehouses of New Guinea. In 1928, E. W. Brandes led a team of scientists who traveled from the Fly River to the Sepik River by plane, seeking disease-resistant varieties of sugarcane endemic to the island. His photographs of treehouses on the Ok Tedi River were published in ***National Geographic Magazine*** with the captions "The Interior of a Pygmy Tree-House Built 50 Feet Above Ground" and "An Upper Fly River Native Perches His House High In Air" (Brandes 1929:291, 309). The film failed to mention this expedition, even though a case full of artifacts collected by Brandes in New Guinea is still on display in the National Museum of Natural History building in which Taylor's office is located.

Treehouses like those depicted in the film were once the predominant architectural form in a wide area across the interior lowlands of southern New Guinea, a fact ignored by the film. One hundred miles east of the Korowai, the Yonggom people--with whom I spent two years conducting ethnographic research in the late 1980s--also had built treehouses, until the region was pacified in the 1940s. Quanchi has shown how images of treehouses figure significantly in the earliest photographs of the region (1993). He argues that Lindt's 1885 photograph of a Koiari treehouse taken near Port Moresby became the signifier "for Papua in the same way that Baldwin Spencer and Frank Gillen's 1900 photograph of a gaunt, desert Arunta" became iconic for all of Aboriginal Australia (ibid.:3). Built into the branches of a tree, the platform shelters of the central coast of Papua were significantly smaller, cruder, and lower in height than the elegant towers of the interior. Photographs of these treehouses were the "opening sign in a narrative of primitiveness . . . which [has] continued to the present" (ibid.:6). Taylor unwittingly recycles this colonial trope in ***Lords of the Garden.***

The most valuable aspect of the film concerns its depiction of interpersonal relations in scenes of the Korowai singing and joking among themselves. While paddling their canoes upstream, the young men burst into spontaneous call-and-response singing. This is later echoed by a group of women pounding sago pith, who sing of the boys they chose not to marry. Their songs exhibit the rhythmic pattern common to music throughout the southern interior of the island (Feld 1991). The enthusiastic shouts accompanying the felling of trees reminded me of the Yonggom, as did their teasing comments about the physical bulk of Taylor and company ("Be careful that they don't get hit by falling trees, we'd never be able to carry their bodies"), about the European aversion to physical labor ("They are people with white skin, they don't have axes"), and among themselves (the teenage

boys building the house wanted to live there unchaperoned, but its owner Yakob quipped that he and his wife planned to live there unchaperoned themselves). Here the film briefly surpasses written descriptions of bonhomie and humor in New Guinea.[6]

Cannibal Mongering

From the outset, the film was intended to emphasize Korowai cannibalism, a choice that Taylor rationalizes in terms of cultural relativism, defining the goal of anthropology as making "other people seem logical and reasonable and rational and understandable. If you can do that in New Guinea [laughs], you can do that anywhere." Proposing an analogy accessible to the film's popular audience, Taylor suggests that Korowai cannibalism functions as part of their "criminal justice system," distinguishing two varieties of cannibalism: "1) the sentence of death followed by cannibalism, given to criminals on an individual basis--in which a clan expels one of its own members to be killed and eaten by a neighboring clan with which it maintains reciprocal arrangements for carrying out such sentences; and 2) the murder and cannibalization that is the consequence of interclan warfare, in which an enemy may be killed and eaten" (Taylor, cited in Kaupp 1994:9). Taylor also compares Korowai cannibalism to capital punishment in the United States (Trescott 1994).

There are two shortcomings in Taylor's analysis. The first problem is that cannibalism is not a punishment meted out against persons convicted of crimes. There is no presumption of a fair trial based on evidence that is evaluated to determine the guilt or innocence of the accused, as the ***Wall Street Journal*** noted (1994). The second problem is that Taylor fails to explain why cannibalism is regarded as an appropriate form of retaliation for certain kinds of objectionable behavior. Cannibalism is the ideal response to acts of ***sorcery,*** which Taylor mentions only in passing, rather than crime. Cannibalism is the chosen form of retribution because this tradition imagines its victims as having been "consumed" by sorcery. (The metaphor is similar to the characterization of the wasting effects of tuberculosis as "consumption," although by a different agent.) Thus the consummate form of revenge for a death attributed to sorcery is for the victim's relatives to kill and eat the sorcerer (see Knauft 1985102-103; Kirsch 1991:112-113).[7] Some metaphors we live by; others we dine by.

Rosalind Morris criticized ***Lords of the Garden*** for its exoticizing emphasis on cannibalism (1996), arguing that the film and its makers naively follow the logic of the consumer age, which reduces all cultural differences to objects of mass marketing and public consumption: feasting on alterity has turned us into cultural cannibals, leading us to seek our mirror image in the

Korowai and their cannibal culture. Yet Morris does not persuade me that cannibalism is any more potent a figure of inverted sociality for contemporary capitalism than it was for nineteenth-century accounts of southern New Guinea (Knauft 1990, 1993), for African discourse about Europeans during the colonial era (e.g., Lewis 1986:63-66), or for that matter, still is for the Korowai. It is difficult to conceptualize a more viscerally compelling way to deny the humanity of "the other" than through the practice of cannibalism, or conversely, through its attribution to others.

The film's attention to cannibalism provoked some minor controversy but no serious debate apart from Morris's examination of "televisual" anthropology's dark side, in which disciplinary capital is exchanged for media sponsorship and exposure. Fearing the wrath of the Congressional Black Caucus, with whom museum officials had already quarreled over what proved to be incorrect references to cannibalism in its African galleries, the Smithsonian Institution delayed the American broadcast premiere until after budget hearings in Washington (Trescott 1994). Decrying Taylor's cultural relativism, the ***Wall Street Journal*** (1994) asserted that the film endorsed cannibalism as "just another life-style choice" (Harrop 1994). Cartoonist Berkeley Breathed gave the affair a final, ironic twist in his Sunday comic strip depicting a "headhunter" who rejects the label "cannibal" in favor of the less-prejudicial "taste bud challenged" (1994).

The problem is not with cannibalism as a legitimate subject of inquiry, nor with cultural relativism as the appropriate starting point in efforts to understand cannibalism. Yet when filmmaker Hallet accompanies a voice-over description of Korowai cannibalism with a scene depicting a group of men--their bows held high and their arrows drawn--circling with clearly ominous intent, ***Lords of the Garden*** descends into farce, subsuming Taylor's relativistic pretensions in the process. Worthwhile considering, in contrast, is what Conklin has described as the more judicious handling of Wari' cannibalism in Brazil:

> Before the contact, rumors of people-eating contributed to images of the Wari' as ruthless savages and intensified public pressures to pacify them. After the contact, the reality of cannibalism and the perceived need to stop it gave moral purpose to the work of the individual missionaries, priests and government agents who were most closely involved in the Wari' contacts. Treatments of Wari' cannibals were far from one-sided, however. Even in the midst of the clamor to pacify the savages, journalists sometimes balanced the stories of Indian savagery with critiques of Brazilian inhumanity.

> One of the notable aspects of the Wari' story is that it was not just intellectuals who treated cannibalism with empathy and relativism. Rather, it was missionaries, priests and government agents--people directly involved in the work of pacifying and "civilizing" the Indians--who tried hardest to keep negative stereotypes out of the public images of the Wari'. . . . Instead of promoting notions of Indian savagery, some of the colonizers purposefully tried to suppress or reframe information about Wari' people-eating in ways that drew a distinction between cultural traditions and innate character, and affirmed the humanity of the cannibals themselves. (Conklin 1997:76)

In contrast, Taylor's desire to present a humanizing portrait of the Korowai remains at odds with the resulting film.

Political Economy and Ethnographic Responsibility

After querying Yakob, the main Korowai figure in the film, about his expectations and aspirations for the future, Taylor wistfully declares that Yakob cannot comprehend what the fates have in store for him, because he is "not being told that this area is a logging concession." Although the film relies on an omniscient narrator throughout, a convention generally eschewed in contemporary ethnographic films (Loizos 1993:12-13), it still comes as a shock when anthropologist Taylor adopts a similar position himself vis-à-vis the Korowai. It is also difficult to reconcile his odd insistence that the "effects we are having [on the Korowai] concern me a great deal" with his assertion that "this may be the last chance to observe a way of life that may very soon disappear" and his knowledge of the state's economic interests in the region.

What influence might this film have should the Korowai try to protect their land rights in response to government-sponsored plans for logging? Would it support or undermine a plea to respect their autonomy? What are the political consequences of representing the Korowai as unknowing innocents, incapable of comprehending their future, or of assuming that the demise of their way of life is inevitable? The film offers no explanation for why this must be their fate, no examination of the political system that is responsible, and no consideration of whether these events have any connection to us, the audience, as potential consumers of tropical hardwoods or in other ways. There are no clear rules to guide anthropologists in conducting research where the economic and political stakes are so great. Given the close working relations that anthropologists establish with the subjects of their research, responsibilities are configured differently for them than for

scholars in the other social sciences. Should anthropologists always align themselves with the subaltern's point of view? Or conversely, do anthropologists tacitly endorse the positions of those in power if they fail to call attention to injustice and inequality?

These questions are fundamental to ethnographic inquiry everywhere, although they have a distinctive valence in Irian Jaya, which is one reason why so little ethnographic research has been carried out there in the last three decades. Is it productive to talk about protecting the traditional land rights of the Korowai when the Indonesian government only recognizes individual land ownership and when land tenure for shifting cultivation, hunting, and gathering, has no legal status? Perhaps there should be an ethnographic equivalent to the efforts of local nongovernmental organizations to address these issues in Paniai, Asmat, and Lorentz (Burnett 1997). These groups are working to demarcate community land boundaries, providing people with new tools to conceptualize and manage their resources. The new systems of land tenure they help create may facilitate legal recognition of local rights.

It is imperative that anthropologists grapple with these issues in their work. Not only does it make for better ethnography, but it also helps anthropologists fulfill their responsibilities to the people represented in the texts and films they produce, people like the Korowai translator Yakob. ***Lords of the Garden*** could not have been made without Yakob's help, although his articulate presence continually undercuts the film's claim that the Korowai have little contact with the world around them. The film would have been far more valuable had the filmmakers paid greater attention to Yakob's experiences in moving between his home atop the forest and the rest of Irian Jaya, as well as to his reactions to the powerful new forces with which the Korowai must now contend in this rapidly changing backwater of Indonesia. We are left to imagine the insights of a film that would take Yakob's journeys of exploration as its focus, rather than Taylor's.

NOTES

1. In Indonesian, these groups are known as *masyarakat terasing,* isolated peoples or tribes, denoting physical seclusion and suggesting foreign or exotic qualities (Gay-nor 1997). The implicit contrast between *masyarakat terasing* and "developed peoples" provides legitimation for development projects that target these communities and their resources.

2. I am grateful to Lorraine Aragon, John Burke Burnett, Lisa Klopfer, and Bruce Knauft for their insightful comments on this review, although they are not responsible for the views expressed here.

3. Steinmetz describes the "pacification line" as the "shifting and often indistinct border between those Korowai who have had direct contact with outsiders and the upstream

clans, known as *betul,* or true Korowai, who refuse it" (1996:37). In other words, the Korowai living in the outer region prefer to keep their distance from the Indonesian state and all that it entails. In this regard, Korowai isolation should be seen as a form of social relations rather than as a natural condition (see Kirsch 1997).

4. The references to the Asmat and Michael Rockefeller's death have more to do with establishing Irian Jaya as the concrete referent for audiences' ideas about head-hunting and violence than with any cultural or linguistic link between the Asmat and the Korowai (Grimes 1996).

5. Knauft identifies a similar opposition in *Good Company and Violence,* his study of Gebusi sorcery: "Perhaps the most striking feature of sorcery in Gebusi culture is the way it both creates and reflects a huge gap between good company and violence. The tension between good company and violence is reflected consistently in Gebusi social life--in ethos, social structure, rituals, narratives, and seances, as well as in sorcery attributions. As Kenelm Burridge (1969: xviii) might put it, the juxtaposition of good company and violence is the primary dialectic of Gebusi society and culture" (Knauft 1985:330).

6. Few films have taken such humorous but revealing exchanges as their subject, although see *Under the Men's Tree* (MacDougall and MacDougall 1973).

7. Such practices have long been outlawed in Papua New Guinea.

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BOOKS NOTED

RECENT PACIFIC ISLANDS PUBLICATIONS: SELECTED ACQUISITIONS, JUNE 1996-MAY 1997

THIS LIST of significant new publications relating to the Pacific Islands was selected from new acquisitions lists received from Brigham Young University-Hawai'i, University of Hawai'i at Manoa, 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 Bureaus 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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