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CODE SWITCHING AND CONFLICT MANAGEMENT IN SAMOAN MULTIPARTY INTERACTION

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Multiparty Interactions in the Field

In studying the details of everyday interaction, in engaging in participant-observation, ethnographers typically alternate between two strategies: We either try to be invisible (by hiding behind our notebooks, pretending to be deaf) or we stumble right into the middle of things (as we clumsily attempt to be "one of them"). In the last few years, scholars have learned to reanalyze the contexts out of which our ethnographies are born. Howe and Sherzer (1986), for instance, have reminded us of how the people we study can create through humor a liminal space where the ethnographer can be located. Our relationships in the field have been reassessed as involving notions of domination, dialogue, and fictionalization (Clifford and Marcus 1986). Ethnograpers have been reexamining mistakes and embarrassing moments in the field in search of those magic transactions where a real understanding can be documented (DeVita 1990). These are all attempts at putting the researcher back into the picture, which is an important enterprise, not necessarily because it makes anthropology closer to literature or to psychoanalysis, but becanse it makes it more honest, more humble, and hence potentially more enlightened. Thus, for instance, when we listen to tapes or transcribe our interactions with or among the people we are studying, we learn an important lesson, namely, that whether we are being talked to or talked about, we are also talked through and around (see Haviland 1986). We, the observers, are often the means through which certain acts of social life get done. In this article, I would like to show that not

only dialogues, as Tedlock (1983) suggested, but also triadic or multiparty exchanges form the thread out of which ethnographies are weaved. Those in search of objective criteria should realize that much can be learned by looking at interactions we might be tempted to ignore because they are contaminated by our presence.

I must stress that my point here is not to encourage introspective analyses of memories from the field or an anthropology of self-reflection. I do not intend to promote the writing of emotional, first-person narratives about ourselves among "the natives" and "the native" in ourselves. My point instead is to show that when we examine the interactions in which we are present, we find recurrent discourse strategies that typically exploit and index the multiparty nature of the interaction, a multiparty framework that we helped to create and sustain. This suggests that when we take ourselves out of the picture (or out of the transcript) to write dialogues without the observer or to create a passage of objective description, we are not simply manipulating the data; we are missing the important point that triangulation, indirection, and multiparty frameworks typically provide the organization for much of human interaction, in particular, for human conflict and reconciliation. It is through the examination of such interactions that we can further refine our methods and test our hypotheses.

The thesis of this article is that our ability to understand or simply describe native strategies for accomplishing face-threatening acts (such as shaming, blaming, complaining, accusing) and, more generally, for dealing with conflict situations requires an understanding of the dynamics of multiparty interactions. In the cases discussed here, the multifunctionality of linguistic forms as realized through situated discourse is largely founded on the possibility of provoking and sustaining multiparty participation frameworks; as often the case in daily verbal interaction, multiple goals may be achieved by addressing, in a differentiated fashion, more than one party at the time (Goffman 1981). This can be done through the strategic use of particular linguistic subsystems such as phonological or lexical registers, affective markers, or pronominal forms (see Ochs 1989). In turn, such subsystems are used and reconstituted precisely through the continuous effort to exploit the varied and choral nature of human communication and public performance, which always implies multiple speakers and multiple audiences (see Duranti and Brenneis 1986; Bogen 1987).

As pointed out by those interested in the details of multiparty conversation, even when speakers seem to be talking to one person, they may in fact make others co-participants, allies, or victims of the social acts

they accomplish through talk (Clark and Carlson 1982; Gumperz 1982; C. Goodwin 1981, 1986; Goodwin and Goodwin 1987a; Haviland 1986). Such strategic multifunctionality of linguistic forms has been discussed with regard to political arenas, where indirection is common (Brenneis 1984, 1987; Myers and Brenneis 1984). Here I look at less formal and less institutionally bound forms of interaction, in particular a conversational exchange carried on at night by a wife, a drunken husband, the ethnographer, and his child. I will discuss three segments from this conversational exchange in which speakers express disagreement.

Disagreement

Students of everyday verbal interaction have argued that in conversation there is a dispreference to correct others and a preference to let others correct themselves (Schegloff, Jefferson, and Sacks 1977). In requests for action, mitigating forms have been said to be more frequent than aggravating ones (Labov and Fanshel 1977:84-86). It has also been shown that in certain conversational contexts, namely after an assessment, there is a preference for agreement over disagreement (Pomerantz 1978; Levinson 1983). According to Brown and Levinson (1987), when speakers decide to engage in acts that potentially threaten the addressee's "face" (Goffman 1955), they have the option of using a number of verbal techniques that mitigate those acts. Such techniques, which reassure addressees that their territory and freedom of action are not being impinged upon, typically violate Grice's (1975) cooperative maxims (for example, be informative, say the truth, say what is relevant). When about to perform a face-threatening act--by going "on record"--social actors can (I) explicitly express disagreement or imposition of their wants on others (what Brown and Levinson call "bald on record') or (2) act in a seemingly "irrational" manner, that is, violate Grice's maxims of cooperative behavior by making false statements, pretending to be unsure, withholding information, and so forth. The second of these two choices is often claimed to be preferred by speakers in most contexts.

Other studies, however, have shown that disagreement and competitive behavior is not only frequent but interactionally searched, achieved, and sustained as a common strategy to negotiate power, exchange and assess culturally important values, and socialize others into accepted and successful patterns of behavior. This is the case, for instance, in children's interactions (M. Goodwin 1982, 1983; Goodwin

and Goodwin 1987b), in verbal dueling among adult African Americans (Abrahams 1976; Kochman 1979, 1983; Labov 1972; Mitchell-Kernan 1972), in Jewish argumentative style (Schiffrin 1984; Katriel 1986), and in institutional contexts such as courts and political arenas where expression of disagreement and confrontative style is realized through special registers and highly scripted roles (Bloch 1975; Brenneis and Myers 1984).

In this article, I suggest that disagreement can be expressed and at the same time temporarily mitigated and negotiated through the linguistic creation and maintenance of multiparty participation frameworks. Particular linguistic features such as phonological and lexical registers evoke or avoid other parties' involvement and thus redirect potential confrontation, Discussion of these themes is based on fifteen months of fieldwork in a traditional village in Western Samoa and many hours of audiorecorded spontaneous interaction in a variety of sociocultural settings. I concentrate on one interaction recorded at night, while walking on the road. Further discussion of ethnographic and linguistic data on the same field experience can be found in Duranti 1981, 1988, l999a, 1990b; Duranti and Ochs 1986; and Ochs 1988. First I illustrate the phenomenon of phonological registers in Samoan and then I discuss their functions within a potentially conflictual situation.

Code Switching between "Good Speech" and "Bad Speech"

The Samoan language has two phonological registers, which Samoans themselves call *tautala lelei* 'good speech' and *tautala leaga* 'bad speech'. The basic linguistic difference between the two registers is that in good speech there is an opposition between alveolar and velar nasals (/n/ and /ŋ/—the latter is written as g in standard Samoan orthography) and alveolar and velar stops (/t/ and /k/). In bad speech these contrasts are neutralized: Only /k/ and /ŋ/ *(g)* are used. Most speakers switch between the two registers from one situation to another or even within the same conversation, as shown below. Table 1 illustrates this contrast with a few examples, including two cases of minimal pairs.

The labels "good" and "bad" for the two registers are potentially misleading. The choice between the two cannot be directly correlated with "proper" versus "improper" behavior or with "formal" versus "casual" speech (although in some cases it may appear so). Equally misleading to the outsider are such categories as "formal" and "colloquial" (Milner 1966) or "literary" and "colloquial" (Hovdhaugen 1986), which have been used and continue to be used in the linguistic literature. Rather, as

TABLE 1. **"Good Speech" and "Bad Speech"**

Good Speech	Translation	Bad Speech
toe	again	*koe*
inu	drink	*igu*
Elenoa	Elinor	*Elegoa*
tilotilo	watch, stare	*kilokilo*
tagata	person	*kagata*
lota	my (inalienable)	*loka*
loka	lock (borrowing)	
fana	gun, shoot	*faga*
faga	bay	

discussed in a number of sources (Shore 1977, 1982; Duranti 1981; Hovdhaugen 1986; Ochs 1988), the opposition between the two registers must be seen in cultural or sociohistorical terms.

Good speech is strongly associated with literacy activities, Christianity, and Western values, whereas bad speech is associated with traditional precontact activities, which include informal household interaction as well as traditional ceremonies and political contests. As in other communities (Blom and Gumperz 1972; Gumperz 1982), Samoans often switch between the two registers within the same interaction, thereby either reflecting or constituting a different interpretive frame for the activity. Thus, for instance, before a meal speakers switch from bad to good speech when they recite a thanksgiving prayer. In example 1 below, speaker Vg. invites an older woman, Vaetolu (Vt.), to perform the prayer. The word *fa'afetai* 'thanks, thanksgiving' is pronounced in bad speech (viz. /faʔafekai/) by Vg. before the prayer starts and in good speech (viz. /faʔafetai/) by Vt. in the prayer, because the interpretive frame changes from "conversation" to "(Christian) religious practice."

(1) ("Women Eating," August 1988. Four women are about to have lunch after having cleaned a communal house.)

Vg; *fai se fa'afekai suga Vaekolu.*[1]
'Say the thanksgiving lady Vaetolu!'
Vt; *fa'afetai.* (.5)
'Thanks.'
fa'afetai Iesū i Lou fa'atasi mai. . . .
'Thanks Jesus for Your joining us. . . .'

In other instances code switching may coincide with change of addressee. Although not everyone complies, Samoans as a rule believe that foreigners should be addressed in good speech, the register used by preachers and teachers during their professional performances. Those Samoan speakers who try to follow this rule are often forced into repeated code switching when an audience includes both Samoans and foreigners. They sustain co-membership with other Samoans by using bad speech while simultaneously paying respect to guests by addressing them in good speech. This situation is shown in example 2. The speaker Tui, a Samoan chief, switches back and forth from bad speech to good speech depending on whether he is addressing the researcher or the other two Samoan chiefs present. Speaker F., on the other hand, uses bad speech regardless of the social identity of the addressee.

(2) ("The Watch," 1979. Chief F. and Tui [T.] engage the researcher A. in a joking sequence on the theme of marriage.)

F; [To A.] *fa'apea a'u le'i fai se ko'alua 'oe.*
'I thought you didn't have a spouse.'

(1.)

T; *'ae, magū e fa'aipoipo ma se si keige i gei.*
'But, otherwise he would marry some other girl from here.'

F; *ke lua fa'aipoipo ma le keige o māk-*
'You marry one of ou- girls'
'ae kia'i le fafige le lā.
'and get rid of that woman (you married).'

A; *hhh!*

T; (Laughing) *hehe-hehehehe!*

(3.)

T; *sā faipoipo 'oulua ma Elenoa?*
'Did you and Elinor get married?'

A; *ioe.*
'Yes.'

T; *oh!*

(1.5)
'ae pe 'ā tu'u nā fa'aipoipo (.5) *'aa?*
'But what about if you drop that marriage, huh?'
'ae toe fai se fa'aipoipo?
'and instead remarry?'

Differently from speaker F., who uses bad speech with everyone, including the researcher A. (see the word /koʔalua/ with the /k/ in the first line), Tui uses bad speech in the second line when addressing F., a Samoan, and good speech later when addressing A., a foreigner.

Finally, example 3 is taken from a long speech where the orator N. reads from a notebook.

(3) ("*Fono* at Sanonu," 1981. The orator N. reads names and amounts of contributions to a money collection.)

	good speech	bad speech
N;	*'o Nonu Tapuvae 'ua iai le* **kālā,**	
	'Nonu Tapuvae one dollar given.'	
	(Lit., 'Nonu Tapuvae there is a dollar.')	

The name of the contributor, Nonu Tapuvae, is read from a notebook N. has in front of him, and is thus given in the good speech variety that is appropriate for writing and reading. But the comment about his contribution is given in bad speech, as shown by the word **kālā** 'dollar', which would be written and pronounced **tālā** in good speech.

I will now discuss how these variations and choices are played off in a potentially conflictual situation. First, I must introduce the setting.

A Samoan Night

When we were in Western Samoa, Elinor, David, and I were accustomed to retiring early.[2] "*Manuia le* **pō!** 'May the night be healthy!' " our friends would shout from the road while we were giving our last glance of the day at village life, before closing the door of our Western-style house, pretending to be going to sleep. That was a special time of day, a time for private talk, for reading novels or writing letters. That was the time for silence, after turning off the gas lamp. It was the night, our night. But what was night like for the other people in the village?

For Samoans, the night is the time when things, especially "bad things," happen, when people can dare, in the dark. It's the time when the *komiti*[3] might not see you when you slip through the banana trees with a bottle of beer, or when you meet your sweetheart behind the old church, or when you decide to confront your opponent on the beach.

There were times when I wished I could be part of the Samoan night or at least get a taste of it. I would then look for an excuse to go out.

Very much in a Samoan fashion, I would then try to find an ally, a supporter, a *tāpuaʻi* (see Duranti and Ochs 1986). One night I convinced David to go out with me and visit our friend Tui.

We left the house and joined Tui's wife, Salu, on the road. She was looking for her husband, visibly upset. The three of us walked together, almost in a line, each of us with our own thoughts. David was carrying a carton of cigarettes I had brought for Tui from Pago; I had with me one of our tape recorders, with a tape inside, switched on--I wanted to capture the voices of the night. Suddenly, Tui came out from behind a bush. He looked unstable on his feet but still fairly in control of his actions. He saw us and joined us, on the way to his house. He was in a good mood. The alcohol in his body made him speak more slowly than usual. He used good speech--the phonological register with t's and n's --as is typical of him in talking to me, his *pālagi* 'foreigner' friend. I was trying to keep up with the conversation, my Samoan still shaky after only three months in the village.

First Case: Complaining about Drinking

The first case I want to analyze is an utterance produced by Salu right after a relatively successful exchange in Samoan between Tui and me. Although, as shown in the more extended transcript in the appendix, there were two earlier attempts by Salu to enter the conversation between Tui and me, it is only after line 60 in the following excerpt that she succeeds in getting some form of recognition from Tui (see lines 62 and 64 of example 7, below).

(4) ("At Night," 1978. Chief Tui [T.], his wife Salu [S.], seven-year-old David [D.], and researcher A. are walking on the main road.)

44 T; *'a fea ia Elenoa?*
'Where is Elinor?'
45 (.3)
46 A; *totonu o le fale.*
'Inside the house.'
47 T; *'ae e lē ō o tātou?*
'So she is not coming with us?'
48 (.5)
49 A; *leai.* (.3) *malōlō Elenoa.*
'No. (.3) Elinor [is going to] rest.'

50 T; *‘ua uma na fai se mea‘ai?*
‘Have [you] finished eating?’
51 A; *‘ua uma.*
‘Finished.’
52 T; *lelei.*
‘Good.’
53 (12.0)
54 T; [Sigh] *huuum.*
55 (2.0)
56 A; *(Tui) ‘ua uma le galuega?*
‘(Tui) is the work finished?’
57 (1.5)
58 T; *toetiti.*
‘Shortly.’
59 A; *toetiti.*
‘Shortly.’
60 S; *e lē koe ‘uma ‘i le igu pia so‘o!*
‘It's not finished because of the drinking of beer all the time!’

Line 60 appears to be a complaint by Salu. The first noticeable feature of the utterance is the fact that it is spoken in bad speech, whereas all the prior turns by Tui and myself are in good speech. This is particularly remarkable given that Salu usually speaks to me in good speech, demonstrated in the following exchange that took place half an hour later the same night, at their house. Here if the chief P., a guest, uses bad speech with everyone, whereas Salu switches to good speech when the topic involves me, perhaps as a way of inviting my participation. The segment here is part of a long sequence in which everyone reminisces about a visit a few months earlier.

(5) (“Later, at Tui and Salu’s House,” 1978. Chief P., Tui [T.], Salu [S.], and researcher A. remember the heavy rain and the problems staring the car when visiting Chief P.’s village.)

P; *kele kimu. kele le kimu i kua. leaga kele ‘ā?*
‘Much rain. Much rain back there. Very bad, isn’t it?’
S; [Laughter] *i:: hihi::!*
‘Yesss! Hehe!’

A; *ʻa e lelei (l-) tagata.*
'But the people were good.'
(.5)
S; [Laughter] *he-he-he-he!*
[
P; *kele kimu. (e) pē ai le kaʻavale. ʻāʔ*
'Much rain. The car was dead for it, wasn't it?'
maʻalili ai le kaʻavale.
'The car was cold because of it.'
S; *maʻalili ai le taʻavale le pē soʻo.*
'The car was cold because of it. Kept being stalled.'
A; *iaʻ ma- maʻalili.*
'Right. So- cold.'

In the penultimate line above, Salu repeats P.'s prior utterance, switching from bad to good speech (from *kaʻavale* to *taʻavale*). Having established that Salu is a speaker who typically code switches from bad to good speech when the interaction involves me, let us return to line 60 of example 4. The question is, How can we make sense of Salu's use of bad speech there?

60 S; *e lē koe ʻuma ʻi le igu pia soʻo!*
TA NEG again finish because ART drink beer repeatedly[4]
'It's not finished because of the drinking of beer all the time!'

From the content of the speech act expressed in line 60, Salu seems to be complaining about her husband's drinking habits. The target as well as the recipient of the complaint, however, is not made explicit. They must be inferred from the context and from certain features of the utterance other than its literal content. The only thing that Salu actually says is that "there has been continuous drinking of beer." But she neither mentions who has been doing the drinking nor does she address her remark to anyone in particular (at least verbally). Similar to the use of verbal dueling in African American English (Labov 1972; Kochman 1983), Salu's speech act is directed to whomever finds it relevant. Her husband's name is not even mentioned.

Let us reconsider the grammatical form of the utterance. The nominalization *(le igu pia soʻo)* focuses on the act of drinking rather

than on the agent of the act, which is left out. It is in this grammatical context that we may make sense of her use of bad speech. From other studies of code switching (Blom and Gumperz 1972; Gumperz 1982), we know that a sudden change of code in the middle of an interaction may carry social meaning, that is, it may convey a (meta)message regarding some aspects of the speech event (Bateson 1972). In Salu's case the change from good to bad speech may be used to generate an inference about the recipient of the utterance, namely, that what is being said is primarily meant for or directed to her husband and not to me or David. Bad speech here would thus be a potential device for signaling the primary "target" of the speech act (Basso 1979; Haviland 1986). At the same time, in a less apparent but still effective way, the utterance is shaped in such a way as to imply that I am a possible secondary recipient. Salu exploits here what Silverstein (1984), following Jakobson, calls the "poetry of prose": By using syntactic and lexical parallelism, Salu's utterance plays off the trope *'ua uma* '[it's] finished' used in lines 50, 51, and 56 of example 4 by embedding it in a negative assertion (line 60: *e lē koe 'uma* 'it's not yet finished') and continuing with a complex nominalized clause *(le igu pia so'o),*[5] which parallels the syntactic structures in earlier utterances but introduces the new topic of drinking.

(6) The parallel structures from example 4:

50 T; *'ua uma na fai se mea'ai?*
PST finish COMP do some food
'Have [you] finished eating?'

51 A; *'ua uma.*
PST finish
'Finished.'

56 A; *(Tui) 'ua uma le galuega?*
'(Tui) is the work finished?'

60 S; *e lē koe 'uma 'i le igu pia*
TA NEG again finish because ART drink beer
so'o!
repeatedly
'It's not finished yet because of the continuous drinking of beer.'

Such parallel structure links the complaint to earlier talk in such a way that the utterance is, or could be, another answer to my question in

line 56 or an expansion of the responses. In this sense, Salu's turn can be seen as evoking my response/involvement. However, it is Tui who speaks next:

(7) ("At Night," continued.)

60 S; *e lē koe ‘uma ‘i le igu pia so‘o!*
'It's not finished yet because of the continuous drinking of beer.'
61 (.5)
62 T; [Laughter] *hhehehehe. he‘.*
63 (1.5)
64 T; *inu pia ananafi.*
'Drink beer yesterday.'
65 (1.0)

Tui first downplays the importance of his wife's complaint by laughing and then admits to having drunk the day before, as if to explain her remark to me.[6]

At this point I get back into the conversation, giving Tui a chance to defend himself:

(8) ("At Night," continued.)

66 A; *inu pia?*
'Drink beer?'
67 T; *ioe.*
'Yes.'
[
68 A; *inu pia aso uma?*
'Drink beer every day?'
69 (.5)
70 T; *leai.*
'No.'
71 A; *hhuh lelei.*
'Huh good.'
72 (.7)

Salu's complaint/accusation is thus dealt with partly through the distribution of roles and functions among three parties. In the end I am the one who discusses with Tui his drinking habits and provides a brief eval-

uation of his behavior and of what is acceptable. The confrontation between Salu and Tui is momentarily concluded by a dialogue between Tui and myself (in front of Salu). I believe Salu's utterance in line 60 to be the rhetorical seed that made this resolution possible.

This pattern of evoking and accomplishing other-involvement and cooperation in performing an accusation or in shaming someone is quite common across contexts. It is found in the village *fono* (Duranti 1981, 1988, 1990a, 1990b) and in the household interaction studied by Ochs (1988).

Another point to note here is that Salu seems to be talking as if Tui is not a full-fledged participant. This is even more apparent in the next example, where Salu talks about Tui in the third person.

Second Case: Complaining about the Husband's Choice of Language

Another multiparty involvement is illustrated in example 9, below. Here Salu's turn comes in the middle of an interaction, involving her husband, David, and me. She attempts to shame her husband for trying to speak English. This exchange starts with Tui expressing in good speech his interest in the packet that David is carrying. Once David is brought into the conversation, Tui appears to decide he should use English, probably because David has been in the village for a short time and his Samoan is minimal.

(9) ("At Night," continued.)

82 T; *'o le ā le pepa a Tavita?*
'What is [in] David's packet?'
83 A; [Whispering to D.] *(? ? don't do anything.)*
[
84 T; *Devi.*
'David.'
85 (1.0)
86 D; *what?*
87 (1.0)
88 A; *'o le: meaalofa mo Tui.*
'A gift for Tui.'
89 T; *oi=oi.*
'Oh oh!'
90 A; *ia.* [To D.] *give it to Tui.*

91 T; *leai.*
'No.'
92 (2.0)
93 *alu i fale. totonu o le fale.*
'Go to the houses. Inside of the house.'
[
94 A; *i le fale=le fale.*
'In the house=the house.'
[
95 T; *alu i fale.*
'Go to the houses.'
[
96 A; *wait.=*
97 *=wait David*
98 T; *David wait.*
99 D; *wait?*
100 T; *yes.*
101 A; *yeah.*
102 T; *in the house, ya,*
103 (8.0)
104 S; *fia gagu ia Kui.*
'Tui really feels like speaking foreign [speech].'

In line 104, Salu uses bad speech again, but this time the third person description of Tui (pronounced /kui/) makes the speech act even more problematic from a dyadic point of view. It is a description of Tui's preferences and actions expressed in the register appropriate to talking to Tui but not said directly to him (cf. Goffman 1981:124-157). At the same time, it is unlike an utterance with me as a primary audience. Furthermore, it is a negative evaluation. The term *gagu* (*nanu* in good speech) is loaded with negative affect: It means 'foreign speech' and also 'incomprehensible speech'. Note in the next example how Tui interprets the statement as an invitation to reassess his ability to speak English and asks me to give an assessment, which he anticipates with the negative *e leaga* '[it's] bad'. This negative evaluation is supported by Salu's comment in line 107, which answers a question directed to me.

(10) ("At Night," continued.)

105 (2.0)
106 T; [To A.] *e lelei le nanu a Tui? e leaga!*
'Is Tui's foreign language good? [It's] bad!'

107 S; *humm valea!*
'Huhu [it's] stupid!'

Dialogue between husband and wife is possible here only through a triadic interaction: It is through talking to me or around me that Tui and Salu communicate.

Third Case: Blaming the Foreigners

The third and last case to examine is one in which Salu uses good speech.

Subsequent to the exchange analyzed previously, Tui asks about David's health. It is a polite question that provokes David's interest in the conversation. David's use of English, however, is used by Tui as an excuse to promote Samoan.

(11) ("At Night," continued.)

144 T; *Tavita,*
145 (1.0)
146 T; *Tavita,*
147 *David?*
148 *you try to speak in- Samoan language.* (.5)
149 *it's a good-* (.3) *one for you.*
150 (2.0)
151 D; *I'm trying.*
152 T; *ia. okay.*
153 (2.5)

At this point I try to get David to show off the few words he has been able to learn in the last few weeks (see appendix, lines 154-189). After such a performance, Tui seems eager to admit David's progress in learning Samoan. His wife, however, disagrees with him once again, this time using good speech (note the words *Tavita* and *nanu*).

(12) ("At Night," continued.)

191 T; *lelei tele*
'Very good.'
[

192 S; *e le lelei le fa'a* ***Sāmoa*** *a- a Tavita*
'David's Samoan is not good'
193 *leaga e nanu iai 'oulua.*
'because you two speak foreign language.'
194 A; *hh!*
195 (1.0)

The switch to good speech this time suggests that the speech act is aimed at me. The pronoun *'oulua* 'you two' in line 193, however, indicates that there are two addressees and hence two people to blame. If I am one, who is the other? Before answering this question, let us examine a few more potentially relevant turns. In line 196 below, Salu tries to elicit David's agreement. When she fails to do so, given that David has probably not understood what she said, Salu answers her own question (line 198). At this point David frankly admits his problems with the language and Tui echoes his remark.

(13) ("At Night," continued.)

196 S; *'a Tavita?*
'Right David?'
197 (1.5)
198 S; ***ī.***
'Yeah.'
[
199 D; *(I forgot all those words . . .)*
200 *I don't understand (? her/them)*
[
201 T; *no don't understand (them).*

It is at this point, right after David's admission of lack of competence (lines 199-200), that Tui intervenes to accept and expand Salu's assessment. Salu will shortly shout at him that he should be hiding the bottles of beer he is carrying because people on the road can see him--the first time that she addresses him directly in the last few minutes of interaction analyzed here--while he is engaged (lines 203 and 206) in explaining to David and me what Salu has just said (lines 192 and 193 of example 12). Half in Samoan and half in English, he tells us that Elinor and I are the cause of David's unfortunate situation.

(14) ("At Night," continued.)

202 (2.0)
203 T; *e e le- te 'oe* ***iā*** *Alesana e-Elenoa*
'Don't- to you from Alessandro [and] Elinor'
[
204 S; *(oi* ***fē****) kilokilo mai* ***ā*** *kagaka*
'(Oh!) people stare [at us]'
205 *i au fagu pia. (oga* ***ū****).*
'to your bottles of beer. (? ?).'
206 T; *speak in- English every time (? ?)*
207 S; ***ē!***

In line 193, the dual pronoun *'oulua* could refer to Tui and me, given that we are the only ones present and we have been using some English in talking to David. However, Tui interprets the pronoun as referring to my wife and me.

With the last exchanges, there seems to be a realignment in the participant structure of the interaction: For the first time, in the last few minutes, Salu and Tui seem to agree on something. In fact, this time it is Tui who takes up the role of co-shamer that I had been clumsily fulfilling earlier.

Conclusions

In the last decade or so, many ethnographers have been questioning the authority of their own categorizations and theoretical assumptions. Instead of unexamined monologic descriptions, Tedlock, Rosaldo, Marcus, Fisher, Clifford, and others have been encouraging their colleagues to question their own methodological biases and admit the inherently dialogical nature of ethnographic experience and description. The invitation is to produce a different kind of "script," one in which our voice does not necessarily overpower those of the people whose minds and actions we are trying to understand. It is in the spirit of this enterprise that I have written this article. I chose a transcript of an interaction that includes me as a naive participant not so much to show, one more time, how inadequate we can be or how truly wise our "subjects" are. I selected a spurious piece of data, with mixed codes and mixed messages, with culturally and linguistically varied participants, with ambiguous messages and even more ambiguous stances, to show how both speech

and ethnographer can become resources in the daily constitution of conflict relations and conflict management. The linguistic and interactional intricacies of multivocal dialogues in which we, more or less reluctantly, more or less competently, participate reveal to a close examination two kinds of orders: the one contained in the available code systems and their oppositions, and the one reproduced and challenged in the live exchanges of everyday life. I hope I have been able to show that to describe such orders we must pay close attention to the form and content of talk.

I have tried to demonstrate how, through various discourse strategies such as code switching and referential ambiguity that involve differentiated participation among the parties present, the speakers succeed in challenging and at the same time recreating alliances within the expected boundaries of the local order. Husband and wife, from being foes over drinking and drunken behavior, become allies--blaming the ethnographers for wrongdoing. The initial conflict between two Samoans has turned into an accusation, however benign, of the outsiders' failure to properly integrate their offspring into the local culture. This is indeed a happy ending for an interaction that could have turned in quite a different direction. From my point of view, incurring blame for not living up to the expectations of our Samoan friends was but a small price for the thrill of being part, even though for a few minutes, of the Samoan night.

APPENDIX

This is the transcript of the first five minutes and sixteen seconds of an hour-long tape recorded in the village of Falefā, Western Samoa, in 1978.

Title: "At Night." Setting: walking on the main road, at night; participants: Chief Tui (T.), his wife Salu (S.), seven-year-old David (D.), and researcher Alessandro Duranti (A.). The names of the Samoan participants, "Tui" and "Salu," are pseudonyms.

1 A; *'o le ā le mea e fai?*
'What are you doing?'
2 (1.0)
3 T; *tafafao*
'Visiting.'
4 A; *tafafao tele.*
'Visiting a lot.'
5 T; *(oi/leai) 'umi. 'umi 'ā*
'(Oh/no) long. Long [visiting], isn't it?
6 A; [
a,
'Isn't it?'

7 T; *ia'. **tātou** te¯ō i le fale?*
'Well. Are we all going to [our] house?
8 (1.0)
9 A; *ia.*
'Right.'
10 (1.0)
11 T; ***mālō** David.*
'Hello [lit., congratulations] David.'
12 S; ***'ā lē ō** i le fale ua **ō** uma kagaka.*
'Why not go home [if] everyone has gone.'
13 (4.0)
14 T; ***kākou ō** i le fale **ā**,*
'Let's go home, okay?'
15 A; *ia.*
'Right.'
16 (1.0)
17 T; *David,* (2.0) *what's happen?*
18 (1.0)
19 A; [Laughter] *heh.*
20 S; [Soft] *e le **mālamalama**.*
'[He] doesn't understand.'
21 T; [Laughter] *humhumhum 'a Davi,* (1.5) *e iai se mea 'ua tupu?*
'Huhuhu so David, (1.5) anything has happened?
22 S; *'o le **'ā-*** (.3) *'o le **ā** le **ō***
'Why- (.3) why aren't [we] going'
23 *oga **ō** aku la'ia iai. 'ae-*
'when (I/we?) come to you but-'
24 (1.0)
25 T; [Sees tape recorder] *oi! fia-* (.3) *pu'e se tautala?*
'Oh! (.3) record some speech?'
26 A; *ioe.*
'Yes.'
27 T; *lelei.*
'Good.'
28 D; [Whispers to A.] *should I give it to him now?*
29 (.5)
30 A; *no.*
31 (2.0)
32 *pu'e se:-*
'record some-'
33 (1.0)
34 T; *talanoa i le **'āiga 'ā**,*
'Talk in the family, right?
[
35 A; *talanoa i le **'āiga**.*
'talk in the family.'
36 (.5)
37 T; ***māgaia** leo 'ua lelei lua fekaui **ā** lea 'ua tele 'upu.*
'Nice voices the time is right for you two there are many words.'

38 A; *ia lelei.*
'Well, good.'
39 (3.0)
40 ***fa'afiafia:-*** (1.0) *Salu.*
'Make Salu happy.'
41 S; (? ?)
42 T; *('a-)*
43 (2.0)
44 T; *'a fea ia Elenoa?*
'Where is Elinor?'
45 (.3)
46 A; *totonu o le fale.*
'Inside the house.'
47 T; *'ae e lē ō tātou?*
'So she is not coming with us?'
48 (.5)
49 A; *leai.* (.3) ***malōlō*** *Elenoa.*
'No. (.3) Elinor [is going to] rest.'
50 T; *'ua uma na fai se mea'ai?*
'Have [you] finished eating?'
51 A; *'ua uma.*
'Finished.'
52 T; *lelei.*
'Good.'
53 (12.0)
54 T; [Sigh] *hu::um.*
55 (2.0)
56 A; *(Tui) 'ua uma le galuega?*
'(Tui) is the work finished?'
57 (1.5)
58 T; *toetiti.*
'Shortly.'
59 A; *toetiti.*
'Shortly.'
60 S; *e* ***lē*** *koe 'uma 'i le igu pia so'o!*
'It's not finished because of the drinking of beer all the time!'
61 (.5)
62 T; [Laughter] *hhehehehe. he'.*
63 (1.5)
64 T; *inu pia ananafi.*
'Drink beer yesterday.'
65 (1.0)
66 A; *inu pia?*
'Drink beer?'
67 T; *ioe.*
'Yes.'
[
68 A; *inu pia aso uma?*
'Drink beer every day?'

69 (.5)
70 T; *leai.*
'No.'
71 A; *hhuh lelei.*
'Huh good.'
72 (.7)
73 T; *yesterday.*
74 (1.0)
75 A; *ananafi ma:- ma:-*
'Yesterday and- and-'
76 T; *ua nei.*
'Just before.'
77 A; *ma gāgei.*
'And later [today].'
78 T; *'ua nei.*
'Just before.'
[
79 A; [Misunderstands] ***nānei. nānei.***
'Later. Later [today].'
80 (20.0) [A truck goes by, children's voices]
82 T; *'o le ā le pepa a Tavita?*
'What is [in] David's packet?
83 A; [Whispering to D.] *(? ? don't do anything.)*
[
84 T; *Devi.*
'David.'
85 (1.0)
86 D; *what?*
87 (1.0)
88 A; *'o le: meaalofa mo Tui.*
'A gift for Tui.'
89 T; *oi=oi.*
'Oh oh!'
90 A; *ia.* [To D.] *give it to Tui.*
91 T; *leai.*
'No.'
92 (2.0)
93 *alu i fale. totonu o le fale.*
'Go to the houses. Inside of the house.'
[
94 A; *i le fale=le fale.*
'In the house=the house.'
[
95 T; *alu i fale.*
'Go to the houses.'
[
96 A; *wait.=*
97 *=wait David*
98 T; *David wait.*

99 D; *wait?*
100 T; *yes.*
101 A; *yeah.*
102 T; *in the house, ya,*
103 (8.0)
104 S; *fia gagu ia Kui.*
'Tui really feels like speaking foreign [speech].'
105 (2.0)
106 T; [To A.] *e lelei le nanu a Tui? e leaga!*
'Is Tui's foreign language good? [It's] bad!'
107 S; *humm valea!*
'Huhu [it's] stupid!'
108 T; *leaga le nanu (gāgei)* (.5) *se'iloga onā.*
'The foreign language is bad (?) (.5) unless
[one] is drunk.'
109 (1.0)
110 A; *ona?*
'Drunk?'
[
111 T; *'a Alesana?*
'Huh, Alessandro? [i.e., What do you think?]'
112 A; *onā laititi?*
'A little bit drunk?
113 T; *onā laititi. lelei Alesana.*
'A bit drunk. Alessandro [is] good [or, Well done, Alessandro].'
114 A; *lelei.*
'Good.'
115 T; *umm.*
116 S; *(? nanu?)*
117 (20.0)
118 [Pig screams]
119 A; *ola!*
120 T; *(pig)*
[
121 D; *pigs!*
122 T; *pigs?* (.5) *huhu. pig, 'ā?*
123 A; *'ua pē le pua'a?*
'Has the pig died [i.e., been killed]?'
124 T; *leai.*
'No.'
125 (2.0)
126 T; *le'i tai* [from English *die*] *l(e) pua'a.*
'The pig has not died.'
127 (5.5)
128 T; *mālosi Tavita,*
'[Is] David healthy [lit., strong]?'
129 A; *mālosi.*
'Healthy.'

130 T; *ioe.*
'Yes.'
131 D; *what does that mean?*
132 T; *fiafia?*
'Happy?'
133 A; *fiafia.*
'[He's] happy.'
[
134 S; ***(kālofa)*** *huhuhum.*
'(Poor thing) huhuhum.
[
135 D; *what does that mean?*
136 A; *[he wants to know] if you feel good.*
[
137 T; *are you happy,*
138 *David?*
139 D; *yeah,*
140 T; [Laughter] *hehehehehe.*
[
141 D; *I don't know.*
142 (12.0)
143 *God the moon is up!* (1.5) *(? ?)*
[
144 T; *Tavita,*
145 (1.0)
146 T; *Tavita,*
147 *David?*
148 *you try to speak in- Samoan language.* (0.5)
149 *it's a good-* (.3) *one for you.*
150 (2.0)
151 D; *I'm tying.*
152 T; *ia. okay.*
153 (2.5)
154 A; *'ua il- 'ua iloa Tavita:-* (0.4)
'David knows- knows' (.4)
155 *'upu* ***fa'aSāmoa*** *e: sefulu.*
'ten Samoan words.'
156 (1.0)
157 T; *oh* ***mānaia.***
'Oh nice.'
158 (1.0)
159 T; *lelei. it's good.*
[
160 A; *ia'.*
161 A; *Da- tel- d- Tui the-*
162 *the words you learned in Samoan.*
163 T; ***tōfā.***
'Bye.'

164 (1.0)
165 A; *heh,*
166 D; *(humm)*
167 (.5)
168 D; *hum:* ***tālofa,***
'Hello.'
169 (.5)
170 T; ***tālofa.***
171 D; *hum: fafetai.*
'Thanks.'
172 T; *fa'afetai.*
'Thanks.'
173 D; *'aua.*
'Don't.'
174 T; *'aua.*
'Don't.'
175 (1.0)
176 *don't.*
177 (2.0)
178 A; *and then what?*
179 (6.0)
180 D; *(and then)*
181 (1.0)
182 A; *how do you say to re(st)?* [*malōlō* 'rest']
[
183 D; *manaia.*
'Nice/ good.'
184 A; ***mānaia.***
'Nice/good.'
185 T; ***mānaia,*** (.4) *good* ***'ā,***
186 (.5)
187 D; *(oden)*
188 (.5)
189 T; *alu?*
'Go?
190 (1.0)
191 T; *lelei tele*
'Very good.'
[
192 S; *e le lelei le* ***fa'aSāmoa*** *a- a Tavita*
'David's Samoan is not good
193 *leaga e nanu iai 'oulua.*
'because you two speak foreign language.'
194 A; *h h !*
195 (1.0)
196 S; *'a Tavita?*
'Right David?
197 (1.5)

198 S; *ī.*
'Yeah.'
[
199 D; *(I forgot all those words . . .)*
200 *I don't understand (?her/them)*
[
201 T; *no don't understand (them).*
202 (2.0)
203 *e e le- te 'oe iā Alesana e- Elenoa*
'Don't- to you from Alessandro [and] Elinor'
[
204 S; *(oi fē) kilokilo mai ā kagaka*
(Oh!) people stare [at us]'
205 *i au fagu pia. (oga ū).*
'to your bottles of beer. (? ?).'
206 T; *speak in- English every time (? ?)*
207 S; *ē!*
208 A; *leaga a'u.*
'It's my fault [lit., I am bad].'
209 T; *hum. ia'.*
'Hum. Right.'

NOTES

Earlier drafts of this article were presented at the Conference on Discourse in Its Sociocultural Context, University of Texas at Austin, April 1987, and at the 1987 American Anthropological Association meeting, Chicago, in the session "Dyadic vs. Multiparty Participation Frameworks." I would like to thank the participants in those two events for their comments and criticism. In particular, I am indebted to Aaron Cicourel, Chuck Goodwin, John Haviland, Joel Sherzer, and Michael Silverstein for their support and insightful comments. Special thanks go to Celso Alvarez and Elinor Ochs for their careful reading of an earlier draft. The research on which this article is based was supported by two grants from the National Science Foundation: Grant 53-482-2480 (Elinor Ochs, principal investigator) and Grant BNS-8608210 (Alessandro Duranti and Elinor Ochs, principal investigators). Special thanks go to the people of Falefā in 'Upolu, Western Samoa, where I conducted my research, for their friendship and cooperation.

Transcription: The transcripts used in this article were prepared by the author with the help of a program ("SCAN") written by John B. Haviland for personal computer. The conventions are basically those introduced by Gail Jefferson (Sacks, Schegloff, and Jefferson 1974) for conversation analysis of English, with a few exceptions such as my use of the semicolon following identification of the speakers (the colon is instead used to mark sound lengthening). A stand-alone bracket--"["--signals the point of overlap; "=" indicates latching; parentheses indicate uncertain hearing or timed pauses; brackets around English words in translations indicate interpolation to ease understanding or to match Samoan idioms with English ones. Samoan is transcribed according to traditional Samoan orthog-

raphy--i.e., the inverted apostrophe (‘) stands for a glottal stop, a macron on a vowel (**ā, ē,** etc.) indicates length--taking into consideration sociolinguistic variation.

1. *Suga* is an informal address term used exclusively with women; Milner (1966) translates it as “lassie.”

2. “Elinor” refers to my wife, Elinor Ochs, who was conducting a longitudinal study of child language acquisition (Ochs 1988). “David” is my stepson, David Keenan, who was seven at the time and had joined us after our first two months of fieldwork.

3. An appointed group of *matai* (chiefs) who enforce respect of the social etiquette.

4. Abbreviations used in the glosses: ART = article; COMP = complimentizer; NEG = negation; PST = past tense; TA = tense/aspect marker.

5. In Samoan the nominalized form found in line 60 (*le igu pia so‘o* ‘the drinking [of] beer repetitively’), contrary to what is said about other languages such as English (Williams 1981), is not more formal than its verbal or sentential counterparts and is in fact quite common in everyday, casual speech. In this context, the important aspect of this utterance is the nonmention of the actor/subject, which gives the addressee(s) more responsibility for deciding whom the speaker is talking about.

6. A linguistic note is necessary at this point. Line 64 does not have a subject expressed: Tui does not explicitly say, “I drank beer yesterday.” His utterance in fact parallels Salu’s nominalized clause, which was also subjectless. This seems to be a good candidate for one of the strategies mentioned by Brown and Levinson (1987:225): Be vague or ambiguous. Assessing the import of such a linguistic choice is difficult, however, given that in Samoan this kind of ellipsis is used more often than in languages such as English and the referent of the “missing” subject is usually understood from the context.

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ASPECTS OF SOCIAL ORGANIZATION IN THREE SAMOAN COMMUNITIES

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The *ʻāiga* (family) has long been recognized as the basic unit of Samoan social structure, and as one of Samoan society's most stable features (e.g., Gilson 1963). Not only has the remarkable conservatism of *faʻaSamoa* (Samoan custom, or the Samoan way) been attributed to the *ʻāiga,* but it has been credited with creating conditions for the successful adaptation of modernizing and migrant Samoans by providing economic, social, and psychological support (Pitt and Macpherson 1974; Macpherson 1978; Higgenbotham and Marsella 1977; Filoialiʻi and Knowles 1983).

Yet modernization and migration have resulted in, even necessitated, changes in the structure, function, and accessibility of the *ʻāiga* (Pitt and Macpherson 1974; Ablon 1971; Kotchek 1978; Filoialiʻi and Knowles 1983; Franco 1978). The modern Samoan family is fragmented. Some members are in the home village, others live in the local urban centers, and still others are scattered about in New Zealand, Australia, Hawaii, and the U.S. mainland. For those who live outside Samoa further fragmentation occurs as a result of housing limitations (Filoialiʻi and Knowles 1983).

Several observers have noted a shift toward nuclear households and a change in the authority structure of both households and *ʻāiga* in migrant communities. They have also noted that Samoans abroad widen their social networks and admit non-Samoans into their intimate circles (Pitt and Macpherson 1974; T. Graves 1978; Lyons 1980). Such changes are perhaps inevitable as individuals leave the confines of

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homogeneous, well-defined communities and migrate to heterogeneous societies where they are exposed to a broad range of new contingencies.

The impact on social organization of changes taking place within Samoa has been subject to less scrutiny. Conventional wisdom has been that rural Western Samoa represents the most conservative end of a continuum, with American Samoa intermediate and Samoan communities abroad as the least conservative, or most "modern" (e.g., Baker, Hanna, and Baker 1986). While many observers have commented on the obvious differences between Western and American Samoa, brought about by dramatic changes in the latter's economy in the past few decades, few systematic comparisons have been made between them. Evidence from studies of changing health patterns suggests that the "modernization" of American Samoa has resulted in significant changes. Thus the studies reported in Baker, Hanna, and Baker (1986) indicate that the difference between American Samoa and Western Samoa vis-à-vis disease patterns is considerably greater than the difference between American Samoa and migrant communities abroad. This implies that the processes differentially affecting the Samoas--commercialization, urbanization, and Americanization--are more significant than migration abroad as far as health and illness patterns are concerned. Our major concern in this article is to explore the relative importance of *in situ* change, as represented by an American Samoan sample, and migration, as represented by a Hawaiian sample, to see if the same pattern holds for key aspects of social organization.

The data on which this article is based were collected during 1986 and 1987 as part of the University of Hawaii Samoan Stress and Health Project. Interviews were conducted in three locations--a rural village on the island of Savai'i in Western Samoa, seven villages on the southern coast of the island of Tutuila in rapidly modernizing American Samoa, and urban Honolulu, Hawaii. Although the research was designed to obtain information from individuals bearing on the health consequences of modernization and migration, data were also collected concerning their social involvement with kinsmen, non-kin, and organizations since social support was hypothesized to be an important variable influencing health status (Caplan 1974; Cassel 1976; Gottlieb 1981).

Research Sites

The Western Samoan sample comes from a village on the southwest coast of Savai'i. Life in this village, which is recognized by Samoans as one of the most traditional in modern Samoa, contrasts greatly with

that in the two other sites. The economy is based on subsistence agriculture, and *matai* (chiefs) continue to play important, relatively traditional roles in everyday family and village life. Most of those employed outside the village work on government plantations or forestry projects where they engage in activities similar to those involved in subsistence farming (i.e., clearing land and pulling weeds). There is no electricity or running water, and most people live in traditional-style houses *(fale).* Data from this village were collected over a period of three months.

The American Samoan sample was drawn from seven villages on Tutuila. American Samoa has experienced extensive *in situ* modernization since World War II, and rapid change continues. Marked differences between rural and urban areas on the island no longer exist (Hecht, Orans, and Janes 1986; Martz 1982). There is ready access to transportation, electricity, and running water in all seven villages. In spite of such physical changes, however, *fa'aSamoa* and family obligations continue to play central roles in people's lives.

All of the Hawaii respondents reside in the city of Honolulu. We chose an urban sample in order to maximize the contrast with the two Samoa sites, but the sample can be considered representative of young Samoan adults in Hawaii insofar as the vast majority reside in Honolulu (Franco 1987). A large portion of them live, or have lived, in public housing. Although unemployment is a problem, most of the men and a significant proportion of the women are wage earners. In Hawaii *matai* do not have the same degree of influence they enjoy in the Samoas. Titles are likely to be of relevance only during Samoan-oriented events and, since these are limited, the authority of *matai* in Hawaii is considerably more restricted than in Samoa.

Methodology

Forty-eight individuals (23 males, 25 females) were interviewed in Western Samoa, 49 (26 males, 23 females) in American Samoa, and 51 (22 males, 29 females) in Hawaii. Respondents in Honolulu have lived in Hawaii for an average of 11 years (range 3-23 years); 51% have also lived on the U.S. mainland or in New Zealand. Although 15% of the Honolulu sample were born in Hawaii, only about half of them had always lived there. All of the respondents were between 18 and 37 years of age, the most common age group for Samoan migrants (Pirie 1976; Harbison 1986; Hecht, Orans, and Janes 1986). The sociodemographic characteristics of the sample are listed in Table 1.[1]

The data on which this article is based derive from two different sets

TABLE 1. **Sociodemographic Characteristics of Samples**

	Western Samoa		American Samoa		Hawaii	
	Men	Women	Men	Women	Men	Women
Respondents	23	25	26	23	22	29
Mean Age (years)	24.2	23.7	24.3	23.0	25.9	24.3
Mean Education(years)	8.9	9.8	12.4	13.0	12.6	12.3
Married (%)	39.1	56.0	21.4	28.6	54.5	51.7
Employed (%)	34.8	32.0	61.5	60.9	72.7	48.3
Mean Annual Income (US$)	642	315	3256	3969	8641	4070

of interviews. The first set aimed at obtaining information directly on each subject's network of relationships and social support system. Respondents were asked to do three things in the following order:

1. On a diagram containing three concentric circles, adapted from Antonucci (1985), they were instructed to write the names of people with whom they "felt close." In the inner circle they were to write the names of people they "feel so close to they could not imagine life without them." In the second circle they were to place the names of people they "feel close to, but not as close as those in the inner circle." The third circle was for people with whom they "feel less close, but who were still important to them." They were instructed to write as many or as few names as they wished.

2. Respondents were asked to provide genealogical information on their household, their siblings, parents, parents' siblings, grandparents, spouse, and children, if any.[2]

3. Finally, for all of the people identified in stages one and two, respondents were asked to locate them on (or add them to) the genealogy form and then to answer a series of questions concerning social interaction. These included questions on the flow of money, food, goods, and services, as well as whom they go to for advice and support concerning health and personal problems.

The second set of interviews focused on life events, attitudes, and routines. It included questions concerning involvement with kinsmen, friends, *matai,* church, and community organizations.

Findings

Households

Traditionally, the *ʻāiga* is defined as an extended family: a group tied together by blood, marriage, and adoption. The basic unit for an *ʻāiga*

is the household, although there is little agreement among Samoan scholars about the Samoan term for this unit.[3] For our purposes here we will use the word " *ʻāiga*" for the extended family and "household' for that group of people sharing a common residential unit, whether an apartment or a multiple-structure compound.

Households in the Samoas are usually described as extended, but there is little data on the ratio of extended to nuclear households. An exception is Shore's description of a rural village on Savaiʻi in Western Samoa (1982:53). From a total of 55 households, Shore identified only 1 as one-generational, 16 as two-generational, and 38 as three-generational. Mead reported that in 1925-1926, on Taʻu, American Samoa, only 12 of 68 households were "qualified biological family" households (1949:172). Franco describes households in Hawaii as having a preponderance of female heads of household (26.8%) and as more likely to have children under age 18 compared to other households in Hawaii (1987:7-8). Oakey's account of Samoan migrants in U.S. gateway cites (e.g., Honolulu and San Francisco) suggests households there are usually composed of parents, children, and two "other adults" (1980: 195). The "other adults" are often siblings of the parents but may be grandparents, in-laws, nieces, nephews, cousins, aunts, uncles, or grandchildren. Pitt and Macpherson indicate that migrant households in New Zealand are generally either nuclear or are composed of "various relatives": siblings, aunts, uncles, nieces, nephews, cousins (1974: passim).

That more ethnographers have not attempted to give precise information on household size and composition for Samoan communities may reflect some distinct methodological problems that we, too, encountered. Several respondents in each sample were members of the same household and each was asked to list co-members. They often did not agree as to who was or was not a member. To some extent these disagreements were the result of changes that took place between interviews.[4] Fluidity is a characteristic of Samoan households--people continually move in and out for longer or shorter periods of time, creating an ambiguity with regard to membership. Most of our respondents included only those people in the household at the time of the interview; others included sojourners and temporary residents.[5] For example, some respondents in Western Samoa included young people away at school but others did not. Some respondents in all three locations included visitors and people who live part-time in their household and part-time in another; others did not. This kind of fluidity accounts for all the household-size discrepancies within our American Samoan sample.

Another source of discrepancy is that Samoans, especially in Western Samoa, often fail to list young children as household members.[6] In four

Western Samoan cases discrepancies occurred because a child was omitted. During one interview the respondent's sister, who had been interviewed a few days earlier, came by and was helping him with the household listing. When he mentioned a particular child she laughed with embarrassment and told the interviewer she had forgotten to report that child during her interview.

Additional discrepancies occurred in Western Samoa and Hawaii because individuals had different ideas about household membership. For example, in Western Samoa one woman included only those people she regularly interacted with while performing household duties; her brother-in-law included people from all the houses in the large compound. In Hawaii one woman included nieces and nephews who had just arrived from Samoa to attend school while her husband did not.

These problems of determining household membership constitute a problem for census takers that has not been sufficiently emphasized in the literature. Where only one person provides data for each household, awareness of the problems may not emerge, leading to unwarranted acceptance of information on household size and composition. Our way of dealing with this issue is to present ranges for household size rather than single figures. The ranges represent the highest and lowest figures given by different respondents (if more than one) for each household.

Discrepancies in household structure were also noted (see Table 2). Households were categorized as nuclear, subnuclear, or extended. Nuclear households are composed of a married couple, with or without their children. Subnuclear households are composed of adults without spouses, with or without children. Extended households contain grandchildren and/or secondary relatives (e.g., aunts and uncles, cousins, nieces and nephews) of the household head. All three-generation households are thus classified as extended, as are households composed of adults, with or without spouses, that contain secondary relatives.

Household data from our study are presented in Table 2. Size varies significantly across the three sites, with Western Samoan households being the largest (averaging 11.5-13.1) and households in Hawaii the smallest (averaging 6.0-6.5). American Samoa is intermediate with an average of 7.7-7.9 persons per household. In Western Samoa the large majority of households were extended (91.7%) and contained three or more generations (79.2%). Although extended-family households also predominate in American Samoa, the percentage is considerably lower (71.1-75.6%) and the proportion of households with three or more generations is less than half (42.2-46.7%). In Hawaii the proportion of extended households drops to 36.8-47.3% and only 15.8-21.1% are

TABLE 2. **Household Characteristics by Site**

	Western Samoa	American Samoa	Hawaii
Number of Households	24	45	38
Household Size[a]			
Mean	11.5-13.1	7.7-7.9	6.0-6.5
Range	3-22	3-17	1-14
Household Type[a]			
Nuclear	2	11	12
Subnuclear	0	0	8
Extended	22	32	14
Discrepancies[b]	0	2	4
Number of Generations per Household[a]			
One	0	0	5
Two	5	24	25
Three	13	19	6
Four	3	0	0
Discrepancies[c]	3	2	2

[a]ANOVA significant ($p < 0.001$) by site.

[b]In American Samoa both discrepancies involve siblings, one of whom includes a secondary kinsman as a household member while the other does not. In Hawaii two instances of discrepancy are the result of a change in household composition over time. In the third case the wife includes a sister of her husband that he does not include. The fourth case is the result of sisters differing on whether to include one of their boyfriends.

[c]All three discrepancies in Western Samoa, and both in American Samoa, result from one or more respondents including a grandparent while the other does not. In Hawaii one difference is explained by a change in household composition over the time between interviews, the other by a disagreement about household membership.

three generational or more. Three Hawaii households include only a mother and her children. In addition to kin-only households, we also found several types that contained non-kin in Hawaii. Four respondents were residing in a household made up of two unrelated sibling-groups. Two others were staying with an unrelated roommate of the same sex; one of these respondents, who lives in a university dormitory, did not consider her roommate part of her household. Three additional respondents were living in households that were kin-based but also included friends (e.g., a boyfriend or girlfriend of a household member).

In Western Samoa respondents not living in their natal or conjugal household have been adopted into the household of close kin (grandparent or parent's sibling). In American Samoa three respondents were residing in the household of a sibling and two with a parent's sibling, but none considered himself or herself adopted. In Hawaii two respon-

dents were living with a sibling and two with a parent's sibling and none had been adopted. These households are similar in structure and hierarchy of authority to those described by Pitt and Macpherson (1974) for sibling-based migrant households in New Zealand. Either the eldest sibling assumes the authority or responsibility is shared among siblings.

Our data are consistent with information provided by previous studies. For example, Shore reported an average household size of 14.8 for another Western Samoan village on Savai'i (1982:53)[7] (see also Macpherson 1975:99). Descriptions of households in American Samoa generally do not include information on the average number of members, emphasizing instead the fluidity of multigenerational extended-family households (i.e., Mead 1930, 1949; Holmes 1958, 1987).[8] The American Samoa census of 1980, however, provides a figure of 7.1 persons per household (U.S. Government 1983: 5). An important factor facilitating the nuclearization of households in American Samoa may be Western-style house construction (Mageo 1988). In Hawaii, reports of average household size range from 5 (Franco 1987:7) to 10.5 (Ala'ilima and Ala'ilima 1965:2). Using information presented by V. Ala'ilima we calculate an average of 7.8 (1966:3). Samoan households in California are described by Ablon as having 6 to 10 members (1971:79). Holmes (1978:208) and Franco (1978:262) give 8 as an average, and DuBois found an average of 7 in her San Diego sample (1988:83). Similarly, Macpherson reports an average of 7.25 among Samoan migrants to New Zealand, with a range from 2-14 (1975: 133).

Differential household size may be misleading if used as an index of life-style changes, however. In Samoa the extended-family household is usually divided among a number of structures within a single compound. Some structures are identified as the house of one member and that person's spouse and children. For example, one structure may be occupied by an older couple while others are occupied by married children. Siblings who share a cookhouse, and by Samoan definition thus constitute a household, generally have separate sleeping *fale.*

In Hawaii, especially in public housing, the family members who in Samoa would share a household compound (or contiguous compounds) frequently live in close proximity to one another, with one of the residential units--usually that of a parent--serving as the locus of activity. For example, one family we worked with has eight siblings living in Hawaii. Four (including one married daughter) stay with the mother in a high-rise apartment building, three live in apartments in the same building or one adjacent, and one lives in military housing just a few miles away. The mother's apartment is clearly the focal point of family activity. All of the siblings, their spouses, and children generally visit

the mother's apartment on a daily basis and most, if not all, family meetings take place there. This same situation is replicated in other family groups within our sample.

Thus, although many people can be identified as living in nuclear or subnuclear households, the situation often closely resembles that of extended households in Samoa. Almost all of our respondents, even in Hawaii, have frequent interaction with members of their available *ʻāiga*.

Networks and Patterns of Interaction: The Circle Diagram

For the purposes of this study we included measures of instrumental aid and *informational aid* as indicators of socially significant relationships. Based on a work by House, Thoits defines these terms as follows: Instrumental aid refers to actions or materials provided by others that enable the fulfillment of ordinary responsibilities, such as household, childrearing, financial, and job-related obligations; informational aid refers to communications of opinion or fact relevant to a person's current difficulties--advice, personal feedback, and notification of job openings, available medical assistance, or other opportunities that might make an individual's life circumstances easier (Thoits 1985:53).

We consider a relationship to be socially significant to an individual if he or she reports transactions involving money, food, or goods, or if the respondent reports having asked for advice or help regarding personal or health-related problems. Since such transactions are generally unidirectional with children, data for children under age twelve in a respondent's social network were not considered.

Clearly people feel close to others for reasons beyond the kind of material support they provide. Data from the circle diagrams suggest, and comments during the interviews support, the idea that people named on the diagrams are those with whom our respondents have a special relationship. They seem to share a sense of identity and feel an emotional bond, which usually come from shared experiences. For the most part the names on the diagrams appear to represent people with whom our respondents feel a desire to spend time.

Immediate family--parents, siblings, spouse, and children--were almost always the first names people wrote down, and the majority of these were located in the inner circle. Next they usually wrote the names of other household members, a few members of their or their spouse's extended family, and one or two friends with whom they felt especially close. Almost all of the circles that contained *ʻāiga* included the names of at least a few aunts, uncles, and cousins. But, while some respondents

included all the aunts, uncles, and cousins they could think of, most had little difficulty distinguishing those with whom they had some special relationship. Often the names were of people they had lived with in a common residence at some time in their lives. Non-kin, the majority of whom appear in the second or third circle, were usually those with whom they shared church, athletic, village, or work activities. One male in American Samoa wrote down the "*Fautasi* crew," a group of young males he was training with for the Flag Day boat race. A young *matai* in Western Samoa included a large number of other village *matai.* A couple of people put God in the inner circle, and one female put the name of her deceased grandfather.[9] Like the American participants in Antonucci's 1985 study (from which the idea of the circle diagram was borrowed), few respondents had difficulty conceptualizing their networks in a hierarchical fashion.

There is a statistically significant difference between Western Samoa and Hawaii in the mean number of names included on the circle diagram (Table 3). But, although our respondents in Hawaii included more names on their circles than those in Western Samoa, they were more likely to include the names of people who lived elsewhere. Over 90% of the people listed on the diagrams in Western Samoa also live in Western Samoa, most within the same village. Once again American Samoa is intermediate between the two others.

The individuals mentioned on the circle diagram can be divided into two groups—*ʻāiga* and non-kin. Non-kin are primarily friends and coworkers. In most cases the pastor of the local church is listed as someone important, and for some the pastor is included as a personal friend.

Respondents rarely included all members of their household on the diagram. Some did not include anyone in the household: In five instances in Western Samoa household members were excluded from circle diagrams; in American Samoa only one was without a household member; and in Hawaii three did not include household members (two of these live with an unrelated roommate).

As noted earlier some members of the *ʻāiga* or special friends may not be immediately available, primarily because of migration. These people may live thousands of miles away, but the miles do not necessarily negate bonds of affection and responsibility. Nevertheless, although feeling close to particular people can contribute to an individual's sense of well-being, those who live far away are unavailable to provide immediate emotional, instrumental, and informational support.

When only *available* people are considered (those resident on the same island), the number of names included on the diagram is still smallest in Western Samoa (11.7)) but there is no difference between

TABLE 3. **Circle Diagram by Site**

	Total Circle			Available Circle		
	Western Samoa	American Samoa	Hawaii	Western Samoa	American Samoa	Hawaii
Sample Size	48	49	51	48	49	51
Number of Persons Named:						
Total						
Mean	12.8	24.9	28.7	11.7	19.1	20.6
sd	8.5	24.6	24.0	7.6	20.1	19.7
ʻĀiga						
Mean	7.3	13.1	18.6	6.9	8.9	11.4
sd	4.8	12.1	13.3	4.4	8.1	9.6
Samoan Non-kin						
Mean	5.5	11.7	8.6	4.8	10.0	7.8
sd	6.2	20.6	15.5	5.7	18.1	14.8
Non-Samoans						
Mean	0.1	0.1	1.5	0.0	0.1	1.5
sd	0.3	0.4	3.1	0.0	0.4	3.1
Social Interaction:						
Total						
Mean	8.4	11.0	13.8	8.1	10.5	10.6
sd	6.7	8.2	13.9	6.5	8.3	10.7
ʻĀiga						
Mean				5.4	7.4	6.6
sd				3.9	7.0	5.2
Samoan Non-kin						
Mean				2.7	3.0	3.7
sd				4.4	3.8	8.7
Non-Samoans						
Mean				0.0	0.0	0.3
sd				0.0	0.3	0.8

sd = Standard Deviation

American Samoa and Hawaii (20.8 in each). The amount of decrease is greatest, however, in Hawaii. In other words, people in Hawaii are more likely to include family and friends even when separated by thousands of miles.

When the number of non-kin are considered, the pattern of across-site differences breaks down. American Samoans include more names of non-kin on their circles, and available non-kin represent a significantly larger portion of their circles than in the other two sites. In fact, the *ʻāiga*/non-kin ratios of available people are quite similar in Western Samoa and Hawaii. In part these differences reflect differences in mari-

tal status (r^2 = 0.10, p = 0.0001): 75% of the American Samoans have never been married compared to 52% of the Western Samoans and 47% of those in Hawaii.

It is also in the proportion of *ʻāiga* to non-kin that we find the only significant difference between the sexes. Male circle diagrams in all three sites tend to have a larger proportion of friends than those of females. Again, this reflects a difference in marital status, as males in all three places are less likely than females to be married. Using a multiple regression model, sex explains 4% (p = <.01) and marital status 10% (p = <.0001) of the variance in the proportion of friends; together they account for 13.6% (p = <.0001) of the variance. To some degree, even within the narrow age range of our sample, life stage influenced who appeared on the circle diagram. The absence of some (or, in certain cases, all) household members and the inclusion of substantial numbers of non-kin probably reflect the importance of peers for the younger, unmarried subjects--those who have not yet assumed family responsibilities and the social behavior of older, more established adults (Mead 1949; Gerber 1975).

Although respondents often provided large lists of people with whom they feel close, they did not always report significant social interaction with them.[10] While the differences are not statistically significant, the proportion of available people listed with whom they had significant social interaction shows a modest decrease across sites. When social interaction with *ʻāiga* is compared to social interaction with non-kin there is a significant difference. In Western and American Samoa *ʻāiga* represent 68% of all those named on the circles with whom there is significant social interaction. In both locations transactions with non-kin are generally of a casual nature, involving the sharing of an occasional meal, small amounts of money, and small gifts. In contrast, *ʻāiga* represent only slightly more than half of all those with whom significant social interaction takes place in Hawaii. Thus, friends represent a larger portion of the available circle in American Samoa, but little significant social interaction takes place with many of them. In Hawaii friends and co-workers appear to represent a more important part of our respondents' social networks. Although the sex difference is significant only at the p = <.06 level, it seems evident that non-kin generally play a somewhat stronger role in the social networks of males than females.

The number of respondents who included the names of non-Samoan friends and co-workers on their circles increases across the sites. Only two respondents in Western Samoa (4.2%) gave the names of non-Samoan friends, and none of these friends lived in or near the village.[11] In American Samoa 10.2% included non-Samoans, but never more

than two persons. The percentage of those with non-Samoan friends and close workmates increases to 35.3% in Hawaii; the majority of these offered two or more names, and four offered ten or more.

Despite this increase the self-identified "close" social networks of all of the respondents are predominantly Samoan, and in many cases exclusively so. A strong link to family is also clear: In most cases family represents roughly two-thirds of the names on the circle. There appears to be a transition from friends to family following marriage and the assumption of adult status that is consistent with Samoan culture and the Samoan focus on family and family responsibilities. In Hawaii non-kin appear to supplement rather than replace ties to *ʻāiga*, and the greater inclusion of non-Samoans enhances rather than replaces a social network focused on Samoans.

It is clear that there is considerable individual variation within each site, not just in Hawaii. In all three sites some respondents wrote only a couple of names on their circles and others wrote until they could find no more room. Some circles contain only the names of kin; others include few, and on rare occasions no kin. Family was obviously important to all participants in the study, but our analyses suggest that sex, age, and marital status must be taken into account when considering the importance of the *ʻāiga* to modernizing and migrant Samoans in this age group.

Monetary Exchanges between Kinsmen

The importance of remittances from relatives abroad has been well documented for Western Samoa (O'Meara 1986; Pitt 1970; Shankman 1976). This is one way kinship reciprocity and ties to home communities are maintained at a distance. For the most part previous studies have focused on the effects of remittances on home communities in Samoa; little has been done on the patterning of monetary flows between individuals or households. Our data shed some light on this issue.

As part of our questionnaire on life experiences we asked about the sending and receiving of money to and from relatives. As expected, significantly more individuals in both Western Samoa (52.1%) and American Samoa (49%) reported receiving money from relatives than individuals in Hawaii (21.6%). However, the amounts received in American Samoa were greater, presumably because of a greater need for cash and/or because their benefactors have more access to cash.[12] An interesting difference also exists between both Samoas and Hawaii regarding which relatives send money. In Western Samoa 73.1% of those receiving money reported siblings as benefactors. Aunts or uncles were reported

as donors by 30.8% of receivers. Only one person (3.8%) receiving money reported the source as a parent. In American Samoa siblings are reported as benefactors by 66.7% of those receiving money, aunts and uncles by 41.7%, and no one reported receiving money from a parent. In Hawaii, however, 7 of the 11 respondents (63.6%) who report receiving money named a parent as sender. Only 2 (18.2%) received money from a sibling and 1 (9.1%) from an aunt or uncle.

When it comes to sending money the Hawaiian and American Samoan samples donated at similar rates (57.1% and 56.9% respectively), while the rate for the Western Samoan sample was about half of that (29.2%). This reflects, of course, the greater access to cash enjoyed by the residents of American Samoa and Hawaii (see Table 1 regarding mean income). The amounts sent also reflect this factor, with the Hawaiian group sending an average of US$609.48 per year, the American Samoans an average of US$271.54 per year, and the Western Samoans US$94.86 per year.[13] The pattern concerning kinsmen again differs between the sites. In Western Samoa siblings (50%) and aunts and uncles (50%) were the prime recipients, followed by parents (21.4%). In American Samoa "other kin" and affines were named most often as recipients (39.3%), followed by siblings (35.7%), aunts and uncles (28.6%), and cousins (17.9%). Two respondents (7.1%) reported sending money to non-relatives, but none reported parents. The Hawaiian sample donated most frequently to "other kin" and affines (51.7%), followed by parents (44.8%) and siblings (27.6%).[14]

These data suggest somewhat different patterns of monetary flow in the three locations. It appears that in Western Samoa, which is primarily a recipient of remittances, the dominant flow is between siblings, supplemented by flows between nieces/nephews and parents' siblings. Monetary flows there are almost entirely between close kinsmen, reflecting the fact that income is relatively low in rural Western Samoa (see Table 1). Providing money may therefore be a way of helping close kinsmen in times of need or difficulty. In American Samoa and Hawaii monetary networks are more expansive and include more distant kin. This may reflect the fact that incomes are substantially higher in these locations, allowing individuals to invest in expanding their social networks on the one hand, and substituting money for more demanding ways (in terms of time and labor) of meeting social obligations on the other. Whereas in Western Samoa helping with *fa'alavelave* (ceremonial events) primarily involves producing goods and providing services, in American Samoa and Hawaii giving cash is an alternative way of meeting obligations. In Hawaii the importance of monetary gifts between parents and children provides a fascinating contrast with both Western

and American Samoa. Most likely the difference is a reflection of the increased nuclearization of families in Hawaii. In the Samoas parents are more apt to be in the same household and to share household resources, including income. This would preclude the necessity for transferring funds through gifts. In Hawaii, on the other hand, parents are far more likely to live in a separate dwelling or to have stayed behind in Samoa, necessitating the transfer of funds between households in order to provide financial support.

Service to Matai

Questions have been raised by several scholars on the response of the *matai* system to the intrusion of a cash economy and its fate in migrant communities (Holmes 1967; Ablon 1971; Pitt and Macpherson 1974; Norton 1984). To gain an understanding of how the *matai* system is adapting to such changes we posed a series of questions to each of our respondents concerning their involvement with, and attitudes toward, *matai*.

All of the men and 91.7% of the women in the Samoas reported serving a *matai,* while 59.1% of the men in Hawaii and 44.8% of the women reported giving service. A striking difference is evident between Western and American Samoa, however, with regard to the location of the *matai* being served. In Western Samoa 91.7% of *matai* served were within the respondent's household, while in American Samoa only 23.9% were in the household. In Hawaii only 11.5% of those serving *matai* were co-resident with him.[15] This seems to reflect differential household size in each location--the smaller the household, the less likely that a *matai* will be present. However, this information also raises some serious questions about the nature and function of chieftainship in the three locations. A comprehensive comparative study of the changing role of *matai* in different Samoan communities remains to be done.[16]

Frequency of service (giving labor or donations) is also revealing. In Western Samoa the vast majority of respondents (89.6%) reported providing daily service to a *matai.* In American Samoa there is a wide distribution with 18.4% reporting service daily, 18.4% weekly, and 22.4% monthly.[17] An additional 16.3% reported only providing service for *fa'alavelave.* In Hawaii 5.9% reported providing service daily, 5.9% weekly, 13.7% monthly, and 19.6% only yearly. One person reported only helping for *fa'alavelave,* and two report serving their *matai* only when they are in Samoa. This indicates a clear progression away from frequent obligatory service across sites. The American Samoan data are perhaps most interesting in this respect since they suggest that the *matai*

system has responded to the imposition of a commercial economy by retaining obligatory service but reducing its demands. It is also interesting that although expressed satisfaction with *matai* is high among those who provide service in all three locations (95.8% in Western Samoa, 80.4% in American Samoa, and 100% in Hawaii), American Samoans are the least satisfied.

Reasons given for being dissatisfied include statements like: *matai* expect too much, they drain resources, and *fa'aSamoa* (of which the *matai* system is an important part) holds people back from becoming modern. General dissatisfaction with *fa'aSamoa* was a common theme in casual conversations with people in American Samoa. A separate, open-ended question addressed the best and worst things about being Samoan. *Fa'aSamoa* was offered as the worst thing by 37.3% of American Samoans compared with only 9.4% of Western Samoans; the Hawaiian sample was intermediate with 15.4%. American Samoans are clearly ambivalent about *fa'aSamoa,* however, for they also frequently responded that it was the best thing about being Samoan (62.7%, compared with 54.7% in Western Samoa and 42.3% in Hawaii).

Church and Organizational Involvement

A number of students of Samoan culture have pointed out the important roles churches play in Samoan communities, both within Samoa and abroad (Ablon 1971; Pitt and Macpherson 1974; Kotchek 1978; Vavae 1979; Sala 1980). Our data lend support to this contention. In Western Samoa 91.6% of those interviewed said that they provided support for a local church; in American Samoa 93.9% and in Hawaii 82.4% responded this way. Involvement in church organizations is, however, considerably less in Hawaii. Only 29.4% of the Hawaii respondents are members of a church organization compared to 72.9% in Western Samoa and 63.2% in American Samoa. It may also be significant that whereas the large majority of respondents in both Samoas belong to the same denomination (Methodist in Western Samoa, London Missionary Society in American Samoa), denominational membership in Hawaii is more varied (see Table 4). Given the importance of church-related activities for community solidarity, this may be an index of increasing fragmentation within the Samoan community in Hawaii.[18]

With regard to traditional village organizations, the differences between Western and American Samoa are rather dramatic: 100% of

TABLE 4. **Religious Affiliation by Site**

Religion	Western Samoa		American Samoa		Hawaii	
	Number	%	Number	%	Number	%
Total	48	100.1	49	99.8	51	99.9
None	0		0		2	3.9
Mormon	3	6.3	1	2.0	5	9.8
Methodist	41	85.4	2	4.1	4	7.8
London Missionary Society	3	6.3	33	67.3	20	39.2
Catholic	1	2.1	8	16.3	8	15.7
Seventh-day Adventist	0		1	2.0	2	3.9
Assembly of God	0		2	4.1	4	7.8
Baptist	0		1	2.0	1	2.0
Jehovah's Witness	0		0		4	7.8
Other	0		1	2.0	1	2.0

the men in Western Samoa belong either to the *fono* (organization of *matai*) or to an *ʻaumāga* (village organization of untitled men) compared to only 30.8% of the men in American Samoa; likewise, 93.8% of the women in Western Samoa belong to a village women's organization compared to only 13.0% of the women in American Samoa. There are no comparable organizations in Hawaii.

Several people in American Samoa and Hawaii (six in each location) belong to school organizations, and five men in Hawaii belong to organizations associated with their employment. In general, however, our data suggest that both men and women in the Samoas belong to more organizations and attend more meetings than their counterparts in Hawaii.

Summary and Conclusion

Data from our study of young men and women in Western Samoa, American Samoa, and Hawaii support many of the conclusions reached by previous researchers about changes in Samoan social organization. When compared with Western Samoa, American Samoa appears to have undergone some rather profound changes in response to the commercialization of its economy, to urbanization (which has affected all of Tutuila; see Hecht, Orans, and Janes 1986:51-53), and to Americanization.

One response has been the nuclearization of households and the

decrease in household size, although the effects of these changes on lifestyle may be exaggerated if relationships between households in the same vicinity occupied by kinsmen are ignored. A second response seems to be an expansion of networks outside the household, as reflected in the greater number of non-household persons named on the circle diagram. While some of those named are kinsmen who might be co-residents in Western Samoa, the American Samoan respondents named significantly more non--kin and non-Samoans. This expansion of networks in American Samoa also involves more individuals who are located elsewhere-not immediately available for social interaction. Data on monetary flows follow the same pattern, with the American Samoan sample reporting more expansive networks, including a greater number of distant kinsmen.

A correlate of household nuclearization in American Samoa is that the *matai* served by individuals are far less likely to be members of the same household unit and the frequency of service is correspondingly significantly less. Our data also suggest a dramatic decrease in participation in traditional men's and women's village organizations in American Samoa and a decreased satisfaction with *matai.*

Data from Hawaii, as expected, show an even stronger shift away from traditional Samoan social organization. Households are less likely to be extended and are of even smaller size than in American Samoa. Social networks are expanded and include even more non-Samoans and distant kin. Ties to *matai,* while still in evidence, are functionally weaker, and traditional men's and women's organizations are not present. Finally, although church membership continues to be important in Hawaii, participation in church organizations is significantly less, and there is evidence of denominational dispersion, which may signal an increasing fragmentation within the Hawaii Samoan community generally.

While none of these findings was unexpected, they help to clarify the extent of change and of continuity with traditional patterns of social organization. The fact that the most dramatic contrast is between Western Samoa and American Samoa is testimony to the effects of commercialization, urbanization, and Americanization on social patterns, independent of migration. Migration simply seems to give further impetus to changes already set in motion by processes operating *in situ.*

NOTES

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1. The base sample from American Samoa is composed of all the willing subjects we were able to locate who participated in an earlier study during 1981 (see Howard 1986:185-186 for details). In Western Samoa respondents were chosen randomly to match the age and sex distribution of the American Samoan sample. The Hawaii group is a convenience sample, matched for age and sex, drawn from a list of people referred to us by members of the community or by other subjects.

2. As each person was named for the genealogy we used a rubber stamp to place a box of answer spaces below the name. The top portion of the answer matrix included spaces for genealogical information (name, date and place of birth, current place of residence [or, if deceased, date, place, and cause of death if known], ethnicity, and, if applicable, adoption information); the bottom part was used to record answers to the social support protocol. If the person was named on the circle diagram, the number of the circle (1 = inner, 2 = middle, 3 = outer) was recorded on the matrix in step three.

3. For example, Shore (1982) uses ***'au'āiga,*** Holmes *(1987)* uses *fua'ifale,* and Orans uses ***umu'āiga*** (Hecht, Orans, and Janes 1986).

4. In some instances more than a month elapsed between the time the first and last respondents in a household were interviewed.

5. Some of our respondents were part of this fluidity, changing their place of residence one or more times during the course of fieldwork. For example, two Hawaii respondents lived in the same house when the first one was interviewed, but by the time the second one participated she had moved to another household and gave information for her new household.

It has also been pointed out to us by an anonymous reviewer that Samoan concepts, such as ***'au'āiga,*** are contingent in nature; inclusion depends on who is currently participating in food production, food preparation, and other relevant activities.

6. Very early in the research we became aware of this tendency and, after respondents completed their lists, made a point of asking if there were any additional children. The fact that people sometimes do not list children is of interest for what it may reveal about Samoan notions of household membership. For some Samoans, at least, membership seems to imply active contribution to household resources and activities.

7. Household size in both our village and the one studied by Shore is larger than the average household size of 11 reported by Hirsh (1958) for an urban Western Samoan village.

8. This fluidity is also seen in all three research sites. There were respondents in all three sites who changed households during the period of data collection. The data presented here represent the primary household on the day we collected the information.

9. Our analyses, however, include only living people.

10. Social interaction was considered significant if it involved more than occasional exchanges of small amounts of food, goods, services, advice, or support.

11. In Western Samoa non-Samoan members of the research team were included on a few circles, but these were excluded from the analyses.

12. Differences between the amounts sent and received may also reflect the age and life stage of many of our respondents. Younger respondents often pointed out that they do not yet have *fa'alavelave* (at least not in the sense of a ceremonial event, as the term is commonly used); it is their parents or families who have *fa'alavelave* (for which they provide assistance, for young people are rarely the focus of such events until after they are married and have children).

The *absolute* amounts reported should not be taken as an accurate index of money exchanges. As one reviewer of this manuscript pointed out, in his research Samoans continually overestimated the value of gifts made and underestimated the value of gifts received. This tendency probably reflects the importance of generosity--being an overall giver rather than receiver--in Samoan culture. However, we have no evidence to suggest that such reporting errors differed across sites; thus, we assume that the relative figures are valid indicators of comparative giving and receiving.

13. These figures do not reflect the monetary value of all goods and services transacted. It may well be the case that if nonmonetary transactions were given dollar values the differences between the sites would dissolve or be greatly lessened.

14. Since several individuals sent money to more than one relative the figures total more than 100%.

15. In two instances the *matai* named lives in Samoa.

16. Lyons (1980) compares attitudes toward the *matai* system in American and Western Samoa, and Stanton (1978) addresses aspects of the issue, but little has been done on structural changes in the institution as it has accommodated to urbanizing and migrant communities. An exception is Miller (1980), who analyzes changes in *matai* roles in New Zealand.

17. Two men report they normally *serve* a *matai* on a regular basis but their *matai* died and the title has not yet been filled. Three claim they have a *matai* but are not currently giving service. In one of these cases the *matai* no longer lived in Samoa.

18. It is also, of course, a reflection of the greater number of options available. For example, there was only one active church within the Western Samoan village.

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THE MICRONESIAN EXECUTIVE: THE FEDERATED STATES OF MICRONESIA, KIRIBATI, AND THE MARSHALL ISLANDS

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For approximately a decade now, three adjacent Pacific Island polities --the Federated States of Micronesia (FSM), Kiribati, and the Republic of the Marshall Islands--have been enjoying the heady experience of exercising self-government under the leadership of their respective executives. Now classed as "independent" nations,[1] they first existed as dispersed individual islands or island clusters under warring chieftains until they came under the colonial rule of different European nations at roughly the same time.[2] Similarly, within the short span of less than a year,[3] they all took the definitive step of severing political ties with their respective administering metropolitan nation.[4] In each, indigenous tradition still exercises an important influence over normal daily activities,[5] and to varying degree custom continues to have a part in government as today practiced. Viewed against the current world panorama of nation-states, all three are minuscule both in population[6] and land area;[7] none possesses enough natural resources or is sufficiently developed economically to enjoy more than a very modest level of living;[8] and all are heavily dependent upon extensive financial aid from external sources to support the governments now functioning.[9] In each, most paid employment is in their public sectors and all are experiencing strong, persistent movements of inhabitants from outer islands to the urbanized centers. Notwithstanding these many comparabilities, when establishing their respective democratic systems of government they opted for distinctive executive forms, each varying in significant ways

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both from those of the other two and also from those of the metropolitan goverments under which they received their political apprenticeship in modern rule.

Constitutional Background

The drafting of a constitution for the Federated States of Micronesia (FSM) was designed to permit the resulting document to be truly autochthonous: Elected delegates freely chosen by the indigenous inhabitants gradually fitted together its contents without the participation of or direction by any metropolitan administrator.[10] The Marshall Islands' constitution was similarly adopted by elected delegates without American administrators involved; however, before the Marshallese Constitutional Convention met, the staff of the Marshall Islands Political Status Commission (MIPSC) "were widely believed to have a constitution with a parliamentary form already drafted. . . . Despite a lengthy convention, the resulting constitution was mostly drafted by outsiders."[11] In the case of Kiribati, some 165 representatives from all islands, major institutions, and interest groups met informally in 1977 to debate the contents of a Kiribati constitution. The colony's House of Assembly then accepted the report of this convention in principle and added its own modifications. A Constitutional Conference was held the following year in London with the British colonial administration, and this conference then agreed upon the terms finally incorporated into the Kiribati Constitution.[12]

During the writing of the three areas' respective constitutions, there was no question but that Kiribati would opt for a parliamentary system of government. Similarly, the convention that met on Saipan in the Trust Territory gave only perfunctory attention to other than a presidential form. It might be generalized that their long colonial experience had helped to condition each for the action ultimately taken, but how to explain the Marshalls? The Marshallese had undergone the same tutelage with the United States as did the other Trust Territory districts: The FSM, the Commonwealth of the Northern Marianas, and the Republic of Belau all installed presidential systems.

Part of the explanation, at least, appears to lie in the personal leadership of the present president of the Marshalls. Decades ago, Amata Kabua expressed to this author his preference for a parliamentary system as more fitting to Marshallese tradition. "President Kabua is the undisputed leader of the Marshalls, moving into his third term as President. . . . Kabua has been a key figure in Marshall Island politics for

more than 30 years. He was elected to the first Congress of Micronesia, representing the islands there until the Marshalls broke away from Micronesia, and then became the first President of the Marshalls in 1979."[13] He chaired the MIPSC whose staff, as noted above, was believed to have a preconvention draft ready: "Prior to the opening of the ConCon there was a workshop for the delegates who studied the basics of parliamentary government. . . . The ConCon staff was, according to resolution, supposed to produce two drafts--one parliamentary and the other presidential. The presidential draft was not written."[14]

> Officially the reasons for adopting a parliamentary form of government (as recommended by the MIPSC) was *[sic]* because: a) the leaders' experience was largely legislative, b) it is more in keeping with the culture, and *[sic]* c) it could work more effectively with the Trust Territory executive branch as represented by a district administrator, and d) it is less expensive than presidential government. All of these propositions were hotly debated.
>
> However there was a compelling reason for a parliamentary form of government which has not generally been discussed publicly. In essence legal advisors said that it would be easier to have an internationally recognized government if the executive could emerge from an already recognized legislative body.[15]

Classificatory categories carry with them the shortcoming of being but gross generalizations, concealing the nuances that differentiate the components. This is particularly true with respect to the three Micronesian polities in the central Pacific. Notwithstanding that parliamentary systems have been installed in two and the FSM system is presidential in form, a chief executive called "president" heads the executive branch of all three. None institutionalizes a separate Head of State, the most common pattern in the Westminster-style systems in the Pacific, which provides ceremonial and umpiring functions for the polity and which can also serve as a brake on action by the prime minister.

To differentiate between these polities all with presidents, Yash Ghai refers to that of the FSM as "executive presidential" and the other as "parliamentary."[16] But there are other interesting permutations not so easily encompassed with a ready choice of procrustean terms: The president in the FSM must have majority support of the Congress to be chosen but thereafter can continue in office without it, while in the

Kiribati parliamentary system the president depends upon majority support in the Maneaba to continue in office but not initially to gain the post. Under the terms of the Marshallese Constitution, as will subsequently be developed, in this aspect the position of the president of that polity falls somewhere in between the other two. "Although the models of the head of state derived from two metropolitan traditions (the Commonwealth and Washington), the modes of appointment and tenures differ in significant ways from those models."[17]

Federated States of Micronesia

While the delegates to the Micronesian Constitutional Convention on Saipan readily agreed that a presidential system was appropriate for the FSM, there was less concurrence over the means to be adopted for selecting that president. Without political parties and with the vast weight of the FSM population located in Chuuk (Truk) and Pohnpei,[18] a primary election to nominate candidates was considered financially, mechanically, and politically unfeasible. The solution was found in directing each state every four years to elect one senator-at-large (all of the state's other representatives would serve from districts and for only two-year terms) and the Congress then to co-opt the president and vice-president from this select group of senators. Presumably, when casting a ballot for an at-large candidate, each voter would also be conscientiously expressing the opinion that the candidate possessed the attributes necessary for occupying the nation's chief executive posts. Later, when the new Congress convened, by simple majority its members would fill the two executive seats; once sworn in, under the separation of powers principle the two senators would vacate their legislative seats and by-elections would fill them for the balance of the four-year term.

The first two elections of an FSM president occurred without incident. Tosiwo Nakayama from Chuuk, the former president of the Trust Territory Senate whom the delegates had chosen as president of the Constitutional Convention, easily was selected as the first FSM president. Four years later the congressional choice was repeated.[19] However, the awkwardness of the reelection process was disclosed by Nakayama's first having to run for his state's at-large seat in the Congress, disregarding the fact that an incumbent had been elected to fill the vacancy, only then once again to surrender the senatorial post after being rechosen as chief executive. At the third election for president, in 1987, not only was Nakayama now ineligible under constitutional prescription (Art. X, Sec. 1) prohibiting a president from serving more than

two consecutive terms (he chose not to stand for any congressional seat), but this provision had now become virtually tantamount to a preclusion of the same state from capturing the presidency for more than two consecutive terms. At the time Nakayama was co-opted to serve his second presidential term, it was tacitly understood that the next president would come from Pohnpei. The members of the new Congress proceeded to choose a president on the premise that no one from Chuuk ought to be considered eligible. However, the at-large member elected by Pohnpei had previously incurred the strong personal antipathies of some Chuuk members of the Congress. The impasse was resolved by allocating both FSM executive posts to the senators-at-large from the federation's two small states.

At the time the Constitution was drafted, the FSM potentially could have been composed of six, and possibly seven, states, although privately the delegates were already discounting the inclusion of the Marianas and probably held grave doubts about the Marshalls. Even without these districts of the Trust Territory, however, they anticipated that every four years there would be a field of at least four and perhaps five senators available from which to select the FSM's two chief executive officers. They failed to anticipate that the language of the Constitution and practical politics would so narrowly constrict the presidential choice.

Kiribati

While fully familiar with the Westminster practice of designating the chief executive through action of the legislature, the Constitutional Convention that met in Kiribati nevertheless recommended popular election. The colony's House of Assembly adopted this recommendation when it sat in 1978 following the election of its new members, and this was ultimately incorporated into the Kiribati Constitution. The Maneaba (the designation of the parliament under the Constitution) would nominate not less than three nor more than four MPs, as the convention desired to have as many candidates as practically possible so that one commanding political support would not be blocked by action of the MPs.[20] Election would be gained by the candidate for Te Beretitenti (President) who received the largest plurality. The present incumbent, Ieremia Tabai, initially assumed office by virtue of serving as the colony's chief minister when the Kiribati Constitution took effect and being "grandfathered" in. Thereafter, he was elected in 1982 and again in 1983 and 1987. The High Court of Kiribati ruled that the Constitution's

prohibition against a person assuming the office of "Beretitenti after election on . . . more than three occasions" (Sec. 32[5]) did not apply to Tabai in 1987, holding that his becoming president in 1979 was not by "election" but by virtue of constitutional succession.[21] Yet to be determined is whether this limitation will disqualify Tabai from ever again seeking the office of president. Ghai believes that unlike in "the FSM, there is no possibility of [the president] coming back once the three terms have been served."[22]

A term of the president in Kiribati may not extend beyond the maximum life of the Maneaba--four years--and the assumption of office by a successor, and can be ended earlier. By a majority vote of all members of the Maneaba, the president can be removed on a vote of no confidence in him or his Government.[23] Similarly, he ceases to be president when he declares that a vote on a matter before the Maneaba raises an issue of confidence, and the matter is then rejected by a majority of all members (Sec. 33[2(b,c)]). With such removal, however, the members of the Maneaba sign their own death warrant, for the Kiribati Constitution mandates election of a new Maneaba. Unlike in the FSM, an MP assuming the office of president does not vacate his legislative seat; however, should he have been elected to the Maneaba from a single-member electoral district, to assure that district adequate representation it is entitled to elect an additional member at a by-election. The latter situation has yet to occur in Kiribati, but in 1982 President Tabai did lose a vote of confidence. Apparently many of the MPs did not appreciate that in voting against the Government's position they were ending the life of the Maneaba and would have to stand for reelection. So traumatic was this experience that it has yet to be repeated. It is somewhat ironic that in the FSM, where the president's continuance in office does not depend upon support in Congress, his selection requires majority congressional support, while in Kiribati majority support of the Maneaba is essential for the president to complete a term of office but is not a requisite for his selection as one of the candidates. Reference to the applicable provisions of the Marshalls Constitution adds further incongruity.

Marshall Islands

When superficially examined, the executive provisions of the Marshalls Constitution appear to fall within the general thrust of the Westminster model. The vast executive authority is vested in a cabinet of not less than seven members, who are collectively responsible to the Nitijela. As

desired by the president, the cabinet may be expanded to include as many as one-third (eleven) of the Nitijela's membership. The president, who is part of the cabinet, is elected by a majority of the total membership of the Nitijela, and he selects and may remove the other cabinet members. A motion of no confidence in the cabinet, brought by four backbenchers and carried by a majority of the total membership of the Nitijela, results in the president's being deemed to have tendered his resignation from office. At this point the Marshalls Constitution diverges from normal parliamentary convention: If the Nitijela then fails to elect a new president within fourteen days, both the no-confidence vote and the resignation lapse, and the president continues to serve as chief executive. Only if a vote of no confidence has twice been carried and lapsed, and no other president has held office in the interval, may the president use the no-confidence vote to dissolve the Nitijela.[24] The Marshalls thus fits somewhere between the FSM, where no power of dissolution is possessed by its president, and Kiribati, where the president can force a dissolution. However, as Ghai notes, the circumstances in which dissolution may occur in the Marshalls or, indeed, Kiribati, are "very restricted and leave the head of state with little or no discretion."[25] On another note, unlike the other two Micronesian constitutions, the Marshalls Constitution carries no limitation on the number of terms a president may serve.

Veto Power

Of the presidents in the three Micronesian polities, that of the FSM nominally possesses the most potent veto powers. Belying the fact that the FSM Constitution makes express provision for the president to exercise the veto--and, as well, allows the Congress to repass such vetoed legislation--the veto power of the FSM president is not as powerful a weapon as it may appear. Part of the explanation is political, as the president in the FSM lacks a constituency of his own as chief executive that he can mobilize to counter the weight of congressional objection. The balance of the explanation lies in an unanticipated structural anomaly of the FSM Constitution: With only four states in the Federation, the three-state vote requisite for passage on final reading (two-thirds of all state delegations, each delegation with one vote [Art. IX, Sec. 20]) also suffices for the Congress to override a presidential veto (three-fourths of all state delegations, each delegation with one vote [Art. IX, Sec. 1(q)]). Since the proponents of an enacted measure have already shown they have the strength to adopt it notwithstanding the

president's objections, he may well be reluctant to undertake the futile gesture of formal veto, thereby exacerbating congressional resentment.

A search of the available *Journals* of the FSM Congresses (1979-1987) indicates that for this entire period the president vetoed about 8 percent of the bills passed by the Congress (see Table 1). Over and above this, the president showed his disapproval by allowing an additional five measures to become law without his signature and appended express reservation to two that he nevertheless felt constrained to sign.

In Kiribati, the president may withhold assent only to a measure believed to be inconsistent with the Constitution and return the disputed legislation to the Maneaba for amendment. Should the latter fail to remove the feature objected to on constitutional grounds, the president can then refer the bill to the Kiribati High Court to rule on the claimed inconsistency. Other than for this reason, the Kiribati president must assent to all proposed legislation, regardless of whether it incorporates egregious technical error or embarks the nation upon a strongly disapproved policy. Of course, in Kiribati the president always has the reserved option of threatening to make the passage of a measure a matter of no confidence, thus invoking the implicit sanction of automatic dissolution of the Maneaba should it fail to heed his objections. While this constitutes a tacit veto, by its very nature it can be used only sparingly; its effectiveness is overshadowed by the real power exercised by the president of the Marshalls.

The Marshalls Constitution makes no provision for its president to play any role in formally assenting to enactments of the Nitijela. Also, the Marshalls president cannot initiate action that would end in dissolu-

TABLE 1. **Vetoed Bills, 1979-1987**

FSM Congress	Bills Passed	Bills Approved	Law Without Signature	Vetoed	Overridden
1st: 1979-81	158	144	3	11[a]	1
2d: 1981-82	86	70	1	15	4
3d: 1983-84	92	87	1	4	1
4th: 1985-86	91	87	0[b]	4	1
5th: 1987- (1st Reg. and Spec. Sessions, only)	14	12	0	2	0[c]

[a]Plus 9 suspended by High Commissioner.

[b]But two signed with reservations.

[C]Veto may have been overridden in subsequent session.

tion of the Nitijela should it refuse to follow his expressed views on pending legislation. Nevertheless, the direction he currently exerts over the Nitijela's actions practically assures that measures he openly opposes will not be adopted. It appears that the incumbent president's registering objections to a bill before final passage in the Nitijela constitutes a more effective veto than resort to the formal negation process available to the FSM president after adoption.

Political Parties and Organized Opposition

In none of the three island polities are there well-organized parties with formal grass-root structures, this notwithstanding that two are parliamentary in form and the classic parliamentary model is premised upon the clash between political parties to maintain the system and promote MPs to head the executive branch of government. It was this absence of parties to conduct campaigns for presidential candidates that helped convince the delegates at the Constitutional Convention on Saipan to reject direct election of the FSM president and opt for selection through action of the Congress. At the time the Kiribati and Marshalls constitutions were drafted, their polities' incipient parties might have metamorphosed in the traditional parliamentary mode, but this was not to occur. Instead, the recognized leader in each of these parliamentary systems at the time of independence has continued on as chief executive, heading a loose coalition of MPs, without benefit of any structured political party to sharpen up policy decision making and mobilize public support.

Toward the end of the Gilbert and Ellice Islands Colony period, a Gilbertese National Party had formed with the objective of bettering the position of the Gilbertese population. It sought separation of the Ellice Islands and independence for the Gilbertese. A counterparty (the Christian Democratic Party) opposed it. Once the objectives of the Gilbertese National Party had been achieved, both parties disappeared from the political scene, for there appeared to be no need of organized parties to mobilize support for the choice of chief minister. Rather, Ieremia Tabai, who had led the Opposition in the old Assembly, was one of the four MPs who were nominated for the chief executive post, and he won an absolute majority of the popular vote cast for all candidates for chief minister.[26] With independence, Tabai automatically became president.

The Tabai Government, installed in the independent Kiribati, entered into a fishing agreement with the Soviet Union that badly

divided the nation, particularly incensing the areas of strong Catholic persuasion. Out of this developed a new party, bearing the same name as the old Christian Democratic Party, composed of Catholic members of the Maneaba and the remnants of a discredited trade-union movement. The party failed to gain national credibility, but within the Maneaba its members have tended to play an opposition role.[27] Meanwhile the Tabai Government has been returned to power.

In the Marshalls, as chairman of its Status Commission, Amata Kabua had led the separation movement that defeated the FSM Constitution. Today, the "Commission" remains as a diffuse political identification with which many Marshallese relate. Similarly, the Aniken Dri-Majol (Voice of the Marshalls) had advocated a unified Micronesia and continued as an unstructured, low-keyed opposition in the Nitijela to Kabua and his "Commission." Discredited, its membership today has lost much of its popular support. "While there are occasional opposition groupings and coalitions, there is no organized opposition party. There are no formal political parties in the Marshalls."[28]

Kabua brings to his position as president a traditional status as an important *iroij* (paramount chief) with extensive control over land, "the indigenous basis for social identity" in the Marshalls.[29] This allows him to wield an extraordinary authority not duplicated at the national executive level of the other two polities. Partially because of this, "the distribution of power in the present system of Marshall Islands governance reflects features of the traditional political order and the democratic parliamentary model."[30] Under these circumstances, a formally organized political party is extraneous to Kabua's remaining in political power as president, while the heavy influence of Marshallese tradition mitigates against open expression of discontent and discourages any attempt to mount a party by the fragmented opposition.

Separation of Powers: Executive-Legislative Linkage

When erecting the proposed new Micronesian federation, the delegates to the 1975 Constitutional Convention on Saipan gave relatively little consideration to their decision to adopt a presidential form of government, replete with full complement of checks and balances. Practically all of their governmental experience had been under a presidential-type system, and undoubtedly the formal limitations they incorporated that were designed to counter unbridled executive power loomed to many as basic as the civil liberties they protected in the Constitution's bill of rights. Previously, in the Micronesians' drive to attain self-government,

the district legislatures and then the Congress of Micronesia had been the fulcrum on which they had rested their effort to modify and eventually terminate American rule. Through the legislative institution they had obtained their introduction to Western-style politics, and it had increasingly served them as a brake on the American executive, as well as the means for gaining Micronesian participation in policy setting as the Trust Territory administration gradually, seemingly grudgingly, saw initiative shifting to the elected Micronesian legislators. The delegates to the ConCon were vaguely aware of the parliamentary system as a potential form to be considered--if nothing else, preliminary orientation by staff had attempted to alert them to this alternative--but they (and the staff) possessed limited knowledge of the conventions that facilitated its implementation. This combined with the absence of advocacy for the adoption of a parliamentary form to deny it any serious attention as the Governmental Structure and the Governmental Functions committees each brought forth their respective blueprints for the future FSM government.[31]

Interviews conducted at the end of 1988 with a number of delegates to the FSM ConCon who subsequent to 1975 had served in the executive and legislative branches of the Federated States government tended to reveal a somewhat amorphous satisfaction with its opting for a presidential system, apparently with some under the misconception that its continuance was necessary to offset Chuuk's predominant weight of population from otherwise controlling both executive and legislative branches of the national government. Most volunteered that they were aware of the existence of difficulties in executive-legislative relations within the FSM during the last decade, and a few former delegates were ready to convert completely to a parliamentary form of government and eliminate the separation between the two branches now built into the federal government. One proposal aimed for change just short of abandoning the presidential system by removing the present constitutional impediment preventing congressmen from concurrently holding posts as heads of departments in the FSM executive branch. The seeming parallelism with a parliamentary cabinet is obvious, but lacking would be the bulwarking conventions that collectively help facilitate the functioning of a parliamentary government. A number of other interviewees believed that adopting some of the other structural devices or practices found in a parliamentary government, but retaining the fundamental separation of powers principle intact, would suffice to reduce those difficulties.

The experience of the Marshalls to date demonstrates that mere adop-

tion of a parliamentary form of government would not of itself provide ready solution to the separation of powers problems encountered in the FSM. Despite the parliamentary structure of the Marshallese government, even its president confirmed that in practice it is not fully parliamentary.[32] Other interviewees elaborated upon that observation, disclosing that members of the Nitijela are inclined to follow the legislative process with which they became accustomed under the Trust Territory administration and that legislative-executive relations in the Marshalls are not as completely dissimilar from those in the FSM as terminological differences would imply. Carried over into the Nitijela's process is a wide-ranging system of subject-matter committees that serve both as gatekeepers determining the measures to be returned to the Nitijela floor and as content refiners of those measures they release for floor action. A considerable number of private members' bills are introduced each session, and some are enacted; many of these may end up "ice boxed" in committee, but so does Government-sponsored legislation. That the Kabua Government has agreed upon adoption of a particular policy is no guaranty that it will receive speedy consideration in the Nitijela, be approved as initially submitted, or, indeed, that it will ever be enacted into law.[33] Members have forced adjournment, leaving the Government with its legislative program incomplete. In short, the Government in the Marshalls does not have the control over Nitijela action, nor is it held to the same accountability, as is typical of a more classical Westminster system, such as in Kiribati. There, private members' bills are few in number, a majority of the Maneaba's meeting days are devoted to considering measures originating in the government, the ability of MPs to defer action thereon is limited, and no subject-matter committees exist to diffuse or counter the Government's thrust.

Although the placing of initiative in the Government for proposing public expenditures and raising governmental revenues is a fundamental tenet of a parliamentary system, private members in the Marshalls have nevertheless continued to introduce money measures, only to be reminded that this is now the prerogative of the Government. Also, there apparently is no appreciation in the Marshalls of the symbolic defeat suffered by a parliamentary Government in power should a reduction in an appropriation or a revenue measure be forced upon it against its will. There is nothing in the Marshalls Constitution that prevents the Nitijela from reducing an appropriation, but lacking public comprehension of its significance, resort to this convention would be an empty gesture. While the Marshalls have adopted the structure of a parliamentary system, the polity fails to observe many of the practices and

political conventions necessary to flesh out the skeletal undergirding. It is not surprising, therefore, that the Nitijela's Accounts Committee has yet to devolve into being a critical watchdog of government, scrutinizing it closely and holding it up to public accountability. In short, it seems that the smoother state of legislative-executive relations that prevails in the Marshalls, as compared with the situation in the FSM, rather than being the result of its parliamentary form of government, can be attributed to the traditional status of the incumbent president and the leadership he exerts.

The bettering of communications in the FSM between executive and legislative branches underlay most of the suggestions encountered that propose the grafting of one or more parliamentary devices onto the FSM presidential system. The ability of the president to place a nonmember spokesman on the floor of the Congress, participating in debate, was advanced by some interviewees in the executive branch as constituting a promising means by which to present the president's case more effectively. They envisioned the spokesman as correcting misconceptions voiced on the floor of the Congress, constituting an advantage akin to that enjoyed by the Government in a parliamentary system.[34] Such an innovation, so long as the president's spokesman did not vote, would not violate the separation of powers principle fundamental to the presidential system, and could be instituted by mere change in the standing rules of the Congress. As an aside, in Kiribati the attorney general, who is not an elected MP, sits in the Maneaba and, along with other members of the cabinet, participates in floor debates.[35]

The collective responsibility imposed upon the cabinet members in a parliamentary system was another principle alluded to by interviewees in the FSM as one that could advantageously be grafted onto their polity's presidential system. Somehow, when before committee or in informal discourse with congressmen, department heads seem to forget the executive policy line and speak their department's own position. Here, again, adaptation of parliamentary device would not violate any fundamental tenet of the FSM presidential system. Rather, it may be difficult for a departmental spokesman to resist congressional blandishments to reveal the department's original appropriation requests before they were trimmed by executive staff to fit within the president's budget, especially when the president is perceived as weak. By strengthening the president's powers, particularly financial, the consequences of any such breaking of collective responsibility may be minimized, if not negated.

Within either a parliamentary or presidential system, ultimate control of the public purse is a treasured legislative prerogative not to be

lightly surrendered. Nevertheless, parliamentary systems do curtail what are relatively freewheeling money powers of legislatures in presidential systems. Denial to FSM congressmen of the right to introduce appropriation or revenue bills not endorsed by the executive would be a direct borrowing of parliamentary practice; so, too, would limiting unilateral legislative ability to increase appropriations in or the revenue take of measures sponsored by the executive. Members of the FSM Congress would be unlikely to volunteer the surrender of either power readily, even though constitutional denial of them to the Congress would not constitute abandonment of the presidential system. The FSM Constitution now precludes the Congress from making any appropriation except for legislative expenses, or on the approval of the executive, until the budget is adopted (Art. XII, Sec. 2[b]). Fully consonant would be a prohibition against passing members' bills carrying appropriations without also providing for raising the revenues necessary to meet the proposed expenditures. Such provisions derive in part from the intent to curb legislative excesses, just as constitutional attention in the FSM might be given to putting an end to the innovation of allocating "pork barrel" moneys among individual congressmen for their direct disbursement to constituents. However, all of these constitutional limitations would also have the immediate effect of altering the legislative-executive balance: An FSM president bulwarked by greater discretion in waiving limitations on the money powers of the FSM Congress would in consequence occupy a stronger bargaining position vis-à-vis the members of the Congress than he now enjoys.

Conclusion

Basically, most Micronesians are uncomfortable with disputatious confrontation and take more kindly to settling differences though discussion and even recourse to indirect means. Because of this, the former metropolitan authorities administering Micronesia in the past did the three polities under study a disservice by introducing governmental forms and processes that capitalize upon conflict, force formal divisions, and reach decision through the arbitrary process of counting bodies. Neither the Westminster nor the presidential system fits well with the area's consensus approach to decision making. Some of the Kiribati MPs serve as independents in the Maneaba because they believe that policy should evolve through consultation and consensus, without forming pro- and anti-Government cleavages. Particularly obnoxious to them is the raucous style of debate practiced in the Australian and other British-style

parliaments that exacerbates such cleavage, currently being introduced into Kiribati. Similarly, a lack of fit with Micronesian ways holds for the very form of the presidential government in the FSM, a form that divides governmental powers and requires each branch to be a check on the other. In such a system, those changes designed to facilitate consultation would be consonant with underlying Micronesian cultural norms, and the borrowing of structural forms and political practices from any governmental system that would tend toward that end would appear to be the path most advantageous to pursue.

NOTES

1. "Independent" is shown in quotation marks as the United Nations Security Council has yet to act on the assertion of the United States that the Trusteeship of the Pacific Islands has been terminated for all but the district of Palau, and that the Republic of the Marshall Islands and the Federated States of Micronesia are now sovereign nations in associated-state relationship with the United States. A number of nations around the world have established diplomatic relations with them, thus recognizing their sovereignty.

The district of the Northern Marianas chose the opposite course of drawing closer to the United States as a U.S. commonwealth. Guam, now an American territory, is moving closer to also becoming a commonwealth. Partially because the remaining Micronesian polity--Nauru--severed colonial ties a decade earlier (1968) and therefore has had a much longer period of political maturation, it also is not included in this comparative survey. (Doubtlessly other major differences counterindicating its incorporation will also suggest themselves to the reader.)

2. The Gilbert Islands became a British protectorate in 1892 and with the Ellice Islands (Tuvalu) were annexed in 1915 as the Gilbert and Ellice Islands Colony. The Germans established a protectorate over the Marshalls in 1886. The balance of Micronesia above the equator was under Spanish rule from about the same time, excepting the Mariana Islands, which Spain had annexed in the sixteenth century.

3. The Kiribati Constitutional Conference held in London at the end of 1978 fixed the terms of the Kiribati Constitution, and independence was declared in 1979. The FSM Constitution was drafted in 1975, but the plebiscite on its ratification was delayed until 1978; the four administrative districts that then approved it thus became integral parts of the federation. Under its provisions the FSM Constitution was to take effect one year after ratification, but this date was pushed up to May 1979. After the Marshall Islands District rejected the FSM Constitution, it drafted its own and then adopted it at a plebiscite in March 1979; constitutional government became effective several months later.

4. At the time of breaking colonial ties, Great Britain was administering the Gilberts, and both the Marshalls and what are now states of the FSM were being administered by the United States.

5. The FSM includes a small indigenous Polynesian population long resident in Pohnpei, and also includes a far greater language and cultural diversity than found in the other two polities.

6. FSM, 90, 407 (1986); Kiribati, 66,000 (1986); Marshalls, 43,335 (1988).

7. FSM, 270 sq. miles; Kiribati, 266 sq. miles (this figure is misleading as the sparsely inhabited Line Islands account for most of the land area); Marshalls, 70 sq. miles.

8. Per capita GDP/GNP (in US$): FSM, $761; Kiribati, $729; Marshalls, $724. From Pacific Basin Network Project Database, relying on Organization for Economic Cooperation and Development, *Development Cooperation, 1983 Review;* found in Norman Meller, "The Pacific Island Microstates," *Journal of International Affairs* 41, no. 1 (Summer/Fall 1987): 123, table 2. These data conceal the relative affluence afforded by U.S. grants to the two polities in free association with the United States.

9. Official development aid (both per capita and in absolute amounts) for the two associated states far exceeds that of Kiribati.

10. Norman Meller, *Constitutionalism in Micronesia* (Honolulu: Institute for Polynesian Studies, Brigham Young University-Hawaii, 1985). It should be added that the Compact of Free Association negotiated with the United States required the political systems of the FSM and the Marshalls to be "consistent with the principles of democracy," so this can be regarded as a qualification on the statement carried in the text.

11. Daniel C. Smith, "Marshall Islands," in *Politics in Micronesia,* ed. Ron Crocombe and Ahmed Ali (Suva: University of the South Pacific, 1980), 60.

12. Roniti Teiwaki, "Kiribati," in *Politics in Micronesia,* ed. Ron Crocombe and Ahmed Ali (Suva: University of the South Pacific, 1980), 18-21.

13. Giff Johnson, "Marshall Islands," in *Micronesian Politics,* ed. Ron Crocombe and Ahmed Ali (Suva: University of the South Pacific, 1988), 83.

14. Smith, "Marshall Islands," 60.

15. Ibid., 59.

16. Yash Ghai, "The Head of State in Pacific Island States," *Warwick Law Working Papers* 9, no. 1 (September 1986): 1. Interestingly, in the only other Micronesian parliamentary polity, Nauru, a "president" also combines Head of State and chief executive functions, while in the closely adjoining Polynesian polity of Tuvalu a separate governor serves as Head of State.

17. Ghai, "Head of State," 9.

18. The state constitutional convention that met in 1988 changed the name from "Truk" to "Chuuk."

19. David Hanlon and William Eperiam, "The Federated States of Micronesia," in *Micronesian Politics,* ed. Ron Crocombe and Ahmed Ali (Suva: University of the South Pacific, 1988), 98.

20. Taomati Iuta and others, "Politics in Kiribati," in *Micronesia Politics,* ed. Ron Crocombe and Ahmed Ali (Suva: University of the South Pacific, 1988), 19.

21. *In the Matter of Interpretation of the Constitution* (High Court Civil Case No. 15/1987).

22. Ghai, "Head of State," 13. Roniti Teiwaki refers to the Constitution barring "from holding office for more than three *consecutive* terms." Teiwaki, "Kiribati," 19 (emphasis added).

23. I use "him," "his," and "he" in this article only for purposes of brevity and clarity; such pronouns are meant to refer to an individual of either sex. All three states under consideration have universal suffrage and females are eligible to hold office, although no female candidate has been put forward for president to date.

24. Although the Marshalls Constitution also declares that the president may dissolve the Nitijela if no cabinet has been appointed within thirty days after the president has been elected (Art. IV, Sec. 13[1(b)]), another section specifies that should the president fail to submit his cabinet nominations within seven days after election "his election to that office shall have no effect, and the Nitijela shall proceed to elect a President" (Art. V, Sec. 4[3]). The only apparent way these two sections may be reconciled would be in the unusual situation where the president submits the nominations but the speaker fails to carry out his constitutional duty to issue the instruments of appointment.

25. Ghai, "Head of State," 22.

26. Teiwaki, "Kiribati," 20.

27. Iuta and others, "Politics in Kiribati," 32-37 *passim.*

28. Johnson, "Marshall Islands," 82.

29. "Traditionally the *iroij* held absolute power over the land and the people living there even though use rights were inherited by the *kajur* (workers) lineages. The latter were expected to provide the *iroij* with goods and services. This system still survives but the *iroij* have had to moderate their demands. . . ." Leonard Mason, "A Marshallese Nation Emerges from the Political Fragmentation of American Micronesia," *Pacific Studies* 13, no. 1 (November 1989): 25.

30. Ibid., 25.

31. One of the reasons for having these separate committees was to permit the delegates in committee to consider the functions to be performed by government, and their allocation, without anything being inferentially predetermined by the way the convention structured the work of the committees. The Governmental Structure Committee initially recommended a plural executive, with powers equivalent to those of a chief executive in a presidential system. Meller, *Constitutionalism,* 295.

32. Interview with President Amata Kabua, Majuro, Marshall Islands, 23 November 1988.

33. Until recently it was not possible to separate objectively those measures that were Government proposals from those being introduced by a minister under his personal sponsorship. New rules now require a Government proposal to be countersigned by two ministers in addition to the minister charged with the subject matter of the bill.

34. Congressmen interviewed on Pohnpei and Moen, Chuuk, in 1988 asserted that most decisions are made in committee, where the administration has full opportunity to state its case; the additional arguments of spokesmen on the floor of the Congress would only be an idle gesture.

35. An attempt to have the Kiribati courts restrain the attorney general from taking part in the proceedings of the Maneaba was struck down. *Pacific Islands Monthly* 59, no. 12 (December 1988): 32.

WALLERSTEIN'S WORLD-SYSTEMS THEORY AND THE COOK ISLANDS: A CRITICAL EXAMINATION

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Wallerstein developed world-systems theory as an attempt to correct the inadequacies he saw in the development orthodoxy of the 1950s and the 1960s--the modernization school (Skocpol 1982: 1075). In common with many other dependency theorists, Wallerstein defined his theory of development (and underdevelopment) in opposition to modernization theory in two critical ways. First, whereas modernization theory tended to examine nations as discrete and independent units, world-systems theory made the interstate context of development its main source of explanation: A nation developed the way it did because of its position on the world-system (Wallerstein 1974:351). Although Wallerstein incorporated national and subnational factors into world-systems theory (particularly his more recent versions), these remained in a subordinate role as intervening variables. Second, Wallerstein stressed the importance of historical factors in development. In his first book on the world-system he said that he wished to avoid the "intellectual dead-end of ahistorical model-building" (presumably a reference to modernization theory) and claimed that it was possible to build a universal theory of development on the analysis of the specific histories of individual nations (Wallerstein 1974:338).

This article examines world-systems theory in the light of Wallerstein's claims for its validity and usefulness by using information collected on the Cook Islands. I will attempt to show that world-systems theory overemphasizes the global context of development, misinterprets

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the historical evidence, and falls far short of an adequate understanding of the forces that shape the development of the nations of the world. I will briefly outline world-systems theory, discuss the history of the Cook Islands since European contact, and provide a critique of world-systems theory both as it relates to a Pacific microstate like the Cook Islands and as a general theory of (under)development.

World-Systems Theory: A Broad Outline

Wallerstein's *Modern World-System I,* published in 1974, is a theoretically ambitious work. According to Wallerstein, world-systems theory, roughly outlined in this book, provides a major breakthrough in the explanation of the origin and the dynamics of the capitalist world-system. Such claims have not gone unchallenged, however, and Wallerstein has been subject to serious criticism from a variety of sources (Skocpol 1982; Janowitz 1982). More recent books and articles published by Wallerstein reflect this to some extent; he develops world-systems theory further--sometimes refining it, sometimes altering it in a more substantial manner. In this section I attempt to broadly sketch an outline of the theory as it stands at the moment.

According to Wallerstein the world-system arose in Europe either in the late fifteenth century or early in the sixteenth century. The system was (and is) held together by exploitation, and had the result of polarizing the nations of the earth into two main groups: the core and the periphery. An intermediate position, the semiperiphery, was also created. Wallerstein's model rests on the assumption that the forces that caused this polarization to occur in Europe in the sixteenth, seventeenth, and eighteenth centuries continue to operate on a global level in the late twentieth century.

Polarization began when initial "edges" in the marketplace were transformed into major advantages. If, "at a given moment in time, because of a series of factors at a previous time, one region had a slight edge over another in terms of one key factor, and there [was] a conjuncture of events which [made] this slight edge of central importance in terms of determining social action, then the slight edge [was] converted into a large disparity and the advantage [held] even after the conjuncture [had] passed" (Wallerstein 1974:98). For a number of reasons advantage could be cumulative: A point could be reached where strength[1] created more strength[2] (Wallerstein 1980a:40, 288; Wallerstein 1974:356). Crucial to the ability of a nation to take advantage of any edge created in the marketplace, and to begin an upward spiral

from strength to strength, was the nature of its internal class structure. If the class structure allowed the development of a strong state, then the state could act on the behalf of its dominant economic groups in the world market to reinforce the nation's advantage. "Within a world-economy, the state structures function as ways for particular groups to affect and distort the functioning of the market. The stronger the state machinery, the more its ability to distort the world market in favour of the interests it represents" (Wallerstein 1979:61).

The creation of a strong state coupled with a national culture enabled core nations to protect the disparities that had arisen in the world-system (Wallerstein 1974:349). They were able to do this either through diplomacy, war, or subversion (Wallerstein 1982:41). A core nation was thus able to dominate a peripheral region and extract surplus from it. According to Wallerstein the polarization of nations and states created by this extraction was and is necessary for the maintenance of the system as a whole (Wallerstein 1974:354-355).

The peripheral regions were unable to prevent the transfer of surplus to the core because they had states that were weak relative to core states --this weakness stemmed from their lack of resources and their inability to unite internal interests (Wallerstein 1982:40). In most cases peripheral states actually facilitated such transfers. While core states had a certain degree of autonomy concerning their internal economic interests (Wallerstein 1974:355), the same could not be said of peripheral states. They may have been nonexistent, as in colonial situations, or barely autonomous at all, as in neocolonial situations (Wallerstein 1974:349). In either case the states in the periphery could be seen as operating in the interests of the core (Wallerstein 1980b:82).

The strength of core states with respect to peripheral states, coupled with the greater efficiency of core producers with respect to peripheral producers, gave rise to a geographical division of labor. Under this division of labor the core nations produced the goods for which labor was most highly rewarded and the peripheral nations produced the goods for which labor was less well rewarded. This division of labor magnified "the ability of some groups within the system to exploit the labour of others, that is, to receive a larger share of the surplus" (Wallerstein 1974:349). Once established the division of labor reinforced the polarization of the nations within the world-system and enabled the core to develop at the expense of the periphery through unequal exchange on the world market.

As noted earlier the semiperiphery lies between the core and the periphery. It is a core with respect to the peripheral zones and a periph-

ery with respect to the core zones. Wallerstein points out that the semi-periphery is not "an artifice of statistical cutting points, nor is it a residual category. The semi-periphery is a necessary structural element in a world-economy." It is necessary because it is able to "partially deflect the political pressure which groups primarily located in peripheral areas might otherwise direct against core-states," thus maintaining the world-system (Wallerstein 1974:349-350). Eventually, however, the contradictions in the world-economy will become too significant to be eased and a radical shift will occur. Wallerstein anticipates the formation of a world socialist government in the twenty-first or twenty-second century (Wallerstein and Hopkins 1982:139).

As the world-system developed it expanded into areas previously external to itself (Wallerstein 1980b:80). As these "external arenas" were incorporated into the system the commodities produced by them, originally part of the "rich trades," became a necessary part of the world-economy. Using Wallerstein's typology they became "peripheral zones" (1980a: 109). Two main changes occurred to regions on incorporation. First, there were changes in the form of government. Where a centralized state was lacking it was created, enabling the conditions necessary for surplus extraction to be guaranteed--for example, the enforcement of contracts (Wallerstein 1980b:81). Second, a transformation occurred in the processes and goals of organization. Production was increased by various means[3] and ceased to focus on goods with a high use-value to the producers, shifting instead to goods with a high exchange-value on the world market. According to Wallerstein and Hopkins this process of incorporation caused in every case the "more or less rapid, more or less extensive decline in the material well-being of the population in the area from what it had been" (Wallerstein and Hopkins 1982:129).

A Brief Political and Economic History of the Cook Islands

The history of the Cook Islands since European contact contains, as we shall see, elements that both support and undermine Wallerstein's theory. European exploration of the Pacific began with the Spanish and the Dutch. Although the Dutch ventured into the South Pacific their sphere of influence centered on the Indonesian archipelago and the neighboring seas. The Spanish focused their attentions on the Americas (Morrell 1963:4). As the fortunes of these nations waned the French and the British began to increase their activities in the South Pacific. Their battle for supremacy in Europe played itself out in the external arenas and the peripheral zones of the world, including the Pacific. As Britain was ris-

ing to fill the position of hegemony left vacant by the United Provinces of Holland in Europe, it became dominant in the Pacific. Following in the footsteps of the explorers were the traders and the whalers. A growing trade in vegetables, fruit, pigs, poultry, and firewood developed (Crocombe 1960:2). Also entering the Pacific at this time were the missionaries, most of them associated with the London Missionary Society. The missionaries had a significant role to play in the Pacific and the Cook Islands was no exception.[4]

According to Gilson the dependency of the Cook Islanders on cash crops and overseas trade that developed during the mid-1800s was mainly due to the establishment of local missionary societies.[5] Although the field staff of the London Missionary Society "were instructed to make the Polynesians as self-reliant as possible within a limited range of new economic needs," the missionary and explorer John Williams and his colleagues did not agree with this policy. As they saw it "not only was trade bound to develop with the whalers and merchant ships already calling at the island, but the people could produce a surplus of marketable goods much more easily than they could spin cotton for example. . . . [Williams] felt that they should be encouraged to produce raw materials to exchange with the mother country for manufactured articles" (Gilson 1980:36).

Whatever the cause, by the mid-1800s European involvement in the Cook Islands was substantial and its society was gradually being incorporated into the world-economy. Howard describes this process as a complex interaction or articulation of the older modes of production with the ever-expanding capitalist mode. He also suggests that the impetus for change and even greater capitalist penetration came from three sources: the Polynesian ruling classes, the missionaries, and the European settlers. "Traditional, or pre-capitalist, elites in particular . . . continued to appropriate economic surplus and resultant political power by mobilizing traditional pre-capitalist productive structures" (Howard 1983:9). Trade was generally organized along lineage lines and controlled by the chiefs, although usually subject to the advice of the missionaries (Crocombe 1960:2). As a result of their control of trade some leading chiefs built large European-style houses furnished in the Victorian style and imported thoroughbred horses and wagons (Crocombe 1960:2; Gilson 1980:51).[6]

By the late nineteenth century the British were consolidating their position in the Pacific. Although reluctant to assume new colonial responsibilities the British declared a protectorate over the southern Cook group in 1888 (Crocombe 1960:2; Gilson 1980:57). In 1891 a Brit-

ish Resident, both appointed and financed by New Zealand, was sent to Rarotonga (Crocombe 1960:2).

There were several reasons for the establishment of a protectorate by the British. In 1865 certain chiefs and British residents of Rarotonga had unsuccessfully attempted to persuade Britain through Sir George Grey to establish a protectorate over the islands (Ross 1964:234). While the benefits for trading relations were of some importance, perhaps of greater significance was the fear that other European powers would attempt to claim the Cook Islands for themselves. The fear of French Catholicism held by the missionaries was also a factor (Douglas and Douglas 1987:37). And in August 1881 the French warship *Hugon* arrived in the Cook Islands. The captain announced that France proposed to establish a protectorate and that all future trade should be with Tahiti and not with Auckland or Sydney (Ross 1964:234). This never came to pass but the incident serves to highlight the nature of European colonial politics in the late nineteenth century. Furthermore, from about 1884 onwards New Zealand politicians began to press for the annexation of the Cook Islands. Prior to this their attention had been focused on other Pacific states. "As part of his programme for countering the expansion of Germany and France in the Pacific, Stout [the New Zealand premier] included Rarotonga in the islands which he held Britain should annex forthwith" (Ross 1964:235). Eventually the Colonial Office sided with New Zealand, and Britain finally declared a protectorate over the Cook Islands in 1888.

European influence on the structure of Cook Islands society became more pronounced as the end of the century approached. Frederick Moss, the British Resident, encouraged the chiefs to form a central government. In this he was successful and "the status of the leading chiefs, which had already been enhanced by their religious and commercial contacts, was now further strengthened by their new political powers and functions" (Crocombe 1960:2).

Other changes in the political system were to be achieved through education. Moss believed that democratic self-government would follow naturally from an education in English. "The chiefs accepted Moss's optimistic views with enthusiasm. English language became the *sine qua non* of progress" (Gilson 1980:74). The colonial administration anticipated that educating the islanders would not only result in democracy but also change the social relations of production, allowing productivity to be increased.[7] A high school was opened in 1893 by the London Missionary Society with the administration's support.

During this period the Cook Islanders displayed considerable economic enterprise. The 1890s saw more development of native enterprise

than the group has ever seen since (Bellam 1980a:13; Crocombe 1960: 2). "Native owned and operated schooners traded throughout the Group as well as up to Tahiti and down to New Zealand" (Crocombe 1960:2).

Incorporation into the global economy was not without its cost, however. Local problems of production were difficult enough, but during the 1890s there were also considerable problems associated with changes in the external market. By 1896 cotton production had virtually ceased and the price of coffee had plummeted as a result of Central American competition (Gilson 1980:79-80).

The role of the New Zealand administration in the economic development of the Cook Islands during this period has been much criticized. Bellam, for example, claims that New Zealand was responsible for the underdevelopment of the Cooks (1980a:5). Strickland is even more critical, arguing that a "thick crust of apathy" was forced on the Cook Islands' people "by the strong arm of direct rule, by discouragement of local initiative, by lack of participation in government and by the pent-up feeling against the dictatorial attitude of the Resident Commissioner's [New Zealand] administration" (Strickland 1979:7).[8]

In spite of these problems, or perhaps because of them, New Zealand politicians continued to push for annexation, which was preferred to an ill-defined protectorate (Ross 1964:252). In 1900 Richard Seddon arrived in Rarotonga on a goodwill tour from New Zealand. It was a success because the *ariki* (the senior chiefs) responded by immediately agreeing to the annexation of the Cook Islands by New Zealand (Gilson 1980:99). As Gilson suggests, the annexation cannot be explained solely in terms of Seddon's or the British Resident's imperialist tendencies (Gilson 1980:104). The trade between the two nations was a factor of considerable importance.[9] Ironically, within five years of annexation, indigenous enterprise had virtually ceased (Bellam 1980a:15). Crocombe (1960) suggests that this was due to the leveling of the leadership system on which such enterprise was based.

At this stage it appears that a strong case can be made that New Zealand caused the underdevelopment of the Cook Islands. As the concept of exploitation and underdevelopment is central to Wallerstein's thesis I shall examine these issues in greater detail.

The Dependency of the Cook Islands

The dependence of the Cook Islands on New Zealand aid has been attributed to various causes. The most commonly cited relate to the land tenure system, the problems associated with irregular and expen-

sive shipping, and, more recently, the "MIRAB" set of phenomena (Watters 1987; Bertram 1985).

Land tenure in the Cook Islands was traditionally organized around lineages.[10] The colonial administration opposed such a system as "discouraging the application of technology and capital and hence . . . the use of land in commercial agricultural enterprises" (Kelly 1984:44). Nevertheless the Cook Islanders almost consistently produced more than they could export (Gilson 1980: 156).

In 1902, soon after annexation, a land court was established with jurisdiction over all land matters. "The colonial administration felt that chiefly exploitation was largely responsible for the lack of productivity of islanders as a whole" (Howard 1983:157). Accordingly the power of the chiefs over land was significantly curtailed by the newly formed land court, and chiefly powers to organize production and marketing were either annulled or brought under the control of the Resident (Crocombe 1960:3). Ironically this resulted in a reduction in indigenous enterprise and had enormous consequences for the economic and social development of the Cook Islands. The entrepreneurial base of the Cook Islanders had been undermined (Fairbairn 1987a; see also Crocombe 1964:84). By "1910 it was stated that the power of the chiefs was 'passing away' and that the chiefs were less able to organise group activities than before, as they could no longer compel people to work without pay, and the people were increasingly selling to the highest bidder. The people were hiding their money from their kinsmen and chiefs, usually by burying it" (Gilson 1980: 153). The social system that relied upon the land tenure system was never to be the same again.

Another consequence of the creation of the land courts was that, contrary to local custom, all of a landowner's children inherited from him an equal but undivided share. This has led to the situation where it is not at all uncommon in Rarotonga today for more than one hundred people to hold rights in a single house site. This state of affairs provides a serious deterrent to the planting of long-term crops (Crocombe 1960:7).

Any discussion of the problems associated with shipping in the Cook Islands inevitably brings to the surface some of the ideological debates surrounding development. On the one hand the unsatisfactory nature of shipping in the Cook Islands can be attributed to its location. The islands are small, scattered, and distant from their nearest markets. On the other hand one could argue that the primary cause of the problem is the exploitation of the Cook Islanders by shipping firms that operate exclusively in their own interests.

As noted earlier substantial indigenous control of shipping existed

prior to 1901. Various policy measures introduced by the New Zealand administration, however, appear to have been responsible for a severe reduction in the strength of such indigenous enterprise. With the onset of World War I, and the subsequent drop in the level of exports, the remaining indigenous fruit companies went out of business as indebtedness increased (Gilson 1980:160). By the end of World War I European merchants dominated the export trade.

Losing control of shipping had disastrous consequences for the Cook Islanders. It was not unusual for between a third and a half of the total marketable fruit crop to be left to rot because the only ship available did not provide sufficient cargo space (Gilson 1980: 159). Even when large quantities of produce were successfully exported the indigenous producers did not gain an equal share of the rewards. The benefits of rising exports after 1921 went mainly to European traders as the trading arrangements were heavily weighted in their favor (D. Stone, cited in Kelly 1984:49).

Throughout the 1920s the growers and their representatives continually pleaded for an improvement in shipping arrangements. In 1924 an official reported that the territory was "capable of growing ten times the fruit and tomatoes it produced, if more and faster steamers were put on the trade"; in 1937 S. J. Smith, secretary for the Cook Islands, estimated that "75% of the orange crop of Rarotonga would be wasted that year through the lack of shipping" (Bellam 1980b:18, 19). The problems associated with the transport of produce to external markets earlier this century can hardly be overstated. A parliamentary inquiry discovered that traders had been operating a "ring" to restrict payments to growers and to increase debt bondage. Growers not in debt had difficulty obtaining cargo space (Bellam 1980b:20-23). Compounding these problems was increasing competition for the New Zealand market from growers elsewhere (Kelly 1984:49).

While private shipping firms were able to operate profitably in the Cook Islands for many decades, according to Bellam this is no longer the case. He suggests that only intervention by the New Zealand and the Australian governments has enabled shipping to continue at all (Bellam 1980b:26).

"MIRAB" is another central feature of the recent history of the Cook Islands. It is an acronym coined to describe national economies dependent on *mi*gration, *r*emittances, *a*id, and large *b*ureaucracies. This description applies particularly well to the Cook Islands (Kelly 1984), as we shall see. There, a MIRAB economy had its beginnings at the turn of the century, soon after annexation.[11]

In 1906, when it was realized that the islands were only going to

export a fraction of the volume of produce hoped for, responsibility for the Cooks was transferred from the minister of trade to the minister of justice (Crocombe 1960:3). Whereas prior to 1909 the policy of the New Zealand government was to keep the administration of the islands self-funding, after 1909 this goal was abandoned. Responsibility for the islands over the course of the next twenty-six years then went to three cabinet ministers, all New Zealand Maori, and this meant that "the emphasis was not on rapid economic change, but on welfare and protection" (Crocombe 1960:3). It was believed that social development and welfare would "modernize" the islands and that this would provide the necessary base for independence (Kelly 1984:74-75). Instead the policy entrenched the Cook Islands' dependence upon New Zealand even further: During the early 1960s the New Zealand financial grant finally exceeded receipts from exports.

The welfare emphasis of the administration required an ever-growing bureaucracy. The bureaucracy was mainly staffed by New Zealanders of European descent, especially in the more senior positions, as the indigenous Cook Islanders had been denied sufficient education by Gudgeon, the British Resident, who had closed the high school in the early 1900s.[12] Not until 1954 was a new high school opened in Rarotonga.

In 1965, when the Cook Islands gained political independence, the islands remained firmly integrated in the New Zealand economy, retaining budgetary support and the New Zealand currency. An official publication of the Island Territories Department of New Zealand in the 1950s described the Cook Islands as a tropical province of New Zealand and according to Britton (1982) little has changed in the ensuing decades.

Cook Islanders also retained New Zealand citizenship and free entry into New Zealand. There have been significant losses of population, most pronounced for males aged between fifteen and twenty-nine, resulting in a labor shortage, particularly of skilled workers and tradesmen (Haas 1977:43). "Ease of entry and employment . . . meant the option of obtaining wages up to four times the rates operating in the Cook Islands" (Kelly 1984:96). Other factors also encouraged high migration levels: the lure of new experiences, access to better educational opportunities in New Zealand, and availability of jobs toward which Western education has molded the aspirations of young Cook Islanders (Kelly 1984:96).[13] Loomis also cites the land tenure system and the limited economic opportunities in the Cook Islands (1986). Associated with such migration is the remittance of money back to the Cook Islands (see Loomis 1986; Kelly 1984).

A decade after "independence," New Zealand's policy on assistance to the Cook Islands underwent considerable change. "Assistance was henceforth intended to foster economic sovereignty as the desirable accompaniment of political self-government" (Kelly 1984: 1). It was realized that previous policies designed to establish the conditions for "modernization" had merely resulted in increased dependence.

There were, however, sizable difficulties to be overcome (Fairbairn 1987a). The most important of these was the declining activity of the productive sector. By 1977 the New Zealand minister of foreign affairs, Brian Talboys, was questioning the Cook Islanders' commitment to achieving self-reliance in light of the continuing decline in agricultural output (Kelly 1984:150). One of the options considered to increase productivity was to reduce the size of the bureaucracy, which by 1984 had become responsible for the employment of 39 percent of the Cook Islands' population active in the cash economy. This, however, would have been more likely to result in increased migration than in increased productivity (Kelly 1984:160). It is unlikely that agricultural production in the Cook Islands will ever rise substantially while the ties of the "special relationship" with New Zealand are maintained.

An alternative to agricultural production as a source of national income was provided by the tourist industry. Tourism had become a major source of revenue for the Cook Islands, second only to aid from New Zealand (Milne 1985:145). Perhaps not surprisingly, "the prime initiators of tourism development were . . . external and local European interests, primarily New Zealand companies and expatriates" (Britton 1982:353), and "foreign and European ownership dominate the key sectors within the tourist industry" (Milne 1985: 114). Obviously tourism is a highly ambiguous development strategy (Britton 1982:333). "The Cook Islands' historical dependence on New Zealand has been . . . continued if not heightened by the growth of tourism" (Milne 1985: 107).

World-Systems Theory and the Cook Islands

Relating this discussion back to Wallerstein's world-systems theory, we would expect the relationship between the Cook Islands (a peripheral nation),[14] Britain (a core nation), and New Zealand (a semiperipheral nation) to be primarily characterized by economic exploitation. While there is evidence that could be used to support this view it can only be sustained by understating the importance of other factors. The New Zealand government may have annexed the Cook Islands with the

expectation of economic gain,[15] but within a few years welfare and other goals increased in importance and began to dominate. Even the British appeared to become interested in the Cook Islands for mainly strategic reasons.

It is the existence of these other factors that undermines the ability of world-systems theory to adequately explain the modern history of the Cook Islands. In this sense world-systems theory suffers from the same problems as all universal theories dominated by a single logic (Rapkin 1981). Wallerstein bases world-systems theory on his understanding of the dynamics of global economic forces. Although political and even cultural factors are referred to on many occasions (Wallerstein 1974: 349-354; Wallerstein 1980b:80-85), Wallerstein's model of development remains economically deterministic. This criticism has been lodged by a number of writers including Skocpol (1982), Levy (1981), and Rapkin (1983).

In some ways Wallerstein's model appears to fit the evidence quite closely. Wallerstein explains the geographical expansion of the world-system that occurred from the 1600s onward as resulting from the need for raw materials and markets (Thompson 1981a:12). And the ability of a nation to explore and incorporate external arenas is assumed to be largely conditioned by its strength with respect to other core states. This certainly appears to have been the case in the Pacific. World-systems theory would also predict the creation of a central state in a peripheral zone where one was previously nonexistent. In the case of the Cook Islands this began with the missionaries and continued under the colonial administration.

But on closer examination the inadequacies are clearly revealed. For example, Wallerstein's assertion that incorporation inevitably causes a decline in the material well-being of the population of an external arena does not appear to have been borne out in the Cook Islands. Neither does the assumption that a peripheral state is unavoidably and permanently weak in relation to a core or semiperipheral state (Short 1987). In fact the Cook Islands has two sources of strength vis-à-vis New Zealand.

First, the Cook Islands are of strategic importance to New Zealand and the Western alliance. The importance of this for aid is discussed by Sevele (1987). Second, the New Zealand government is well aware that any decrease in aid may result in a flood of migrants to New Zealand, all of whom would be eligible for unemployment benefits, subsidized education and health care, and so forth (Kelly 1984: 166). It would be consistent with world-systems theory to suggest that the real reason for the continued support of the Cook Islands by New Zealand is that such

support is functional for the system as a whole, and for New Zealand in particular. According to Wallerstein the peripheral zones are not allowed to go completely under in a time of recession because they will still be needed in the upswing in the world-economy (Wallerstein 1980a:129).

According to world-systems theory the state that developed in the Cook Islands would ideally (for capitalist interests) have been strong enough to ensure conditions favorable for surplus extraction. Wallerstein's model is premised on the assumption that comprador states exist in the periphery, acting on behalf of the core and seeking to maximize their own financial well-being in the process. This aspect of his model fits particularly well with the rule of the chiefs in the second half of the nineteenth century. The state that developed after this, however, whether colonial or "independent," always lacked either the strength to extract significant levels of surplus or the will to do so.[16] To ensure adequate rates of surplus extraction the state would have had to increase productivity. Prior to the emergence of the MIRAB economy this could have been achieved by changing the land tenure system and by allowing ownership of economically viable blocks of land by foreign interests. Neither of these changes occurred to any significant extent (Crocombe 1964:83). It can be argued that the administration did not act forcefully initially because it assumed that the Cook Islanders were dying out and that land would eventually become available for settlement (Crocombe 1964:97, 102). But the point remains that the state was too weak to make anything happen by itself (see Crocombe 1964:83-84, 104; Kelly 1984). In any case, by 1915 the whole approach taken by New Zealand to the Cook Islands had changed. The Cook Islands Government Act of 1915, for example, was "based on the recognition of land as the essential basis of Maori life" (Crocombe 1964:105) and, according to Lewis, the administration created its land tenure laws with the express purpose of protecting indigenous land rights despite the consequences for productivity levels (Lewis 1988:30).

A General Evaluation of World-Systems Theory

A more general critique can also be made. World-systems theory relies for its validity on its analysis of the mechanisms underlying (under)development. For the theory to have any predictive value, one of its central aims (Wallerstein and Hopkins 1982:141), the key processes outlined in the theory must be shown to be essential (or, at the least, highly probable) elements of social life. Failing this the mechanisms relied

upon must at least be demonstrated to have some measure of "fit" with those revealed through empirical research. Wallerstein's model, however, can be shown to be both conceptually flawed and historically inaccurate.

It is conceptually flawed in that its central mechanism collapses if the world is able to continue in a form similar to that it takes now, without polarization on a world scale occurring between strong states and weak states and between rich nations and poor nations. This, as I shall argue, is quite conceivable (see also Warren 1973).

Wallerstein is quite correct in assuming that unequal exchange alone is not a sufficient mechanism for sustaining polarization. If the possibility is allowed that state intervention could prevent the extraction of surplus on a global scale, then the system would collapse (Wallerstein 1974: 344-355). It is necessary for Wallerstein to overcome this difficulty by assuming that political and military power will be used to enforce extraction from the periphery to the core. While this may have been the case prior to the establishment of the League of Nations (and even the importance of such factors prior to that is open to question), it is insufficient to explain developments in the twentieth century.

Wallerstein's theory has survived into the late 1980s in spite of these weaknesses, and this is probably due in no small part to its flexibility. While world-systems theory requires continuing polarization on a global scale, it does allow for individual nations to rise or fall in power. A semiperipheral nation, for example, may become a core nation. Obviously, if these changes were completely random and unrelated, then polarization would cease to occur. And if every nation could develop at the same time, the world socialist government predicted by Wallerstein might never come into existence.[17] Wallerstein circumvents this problem by asserting that for a nation to improve its position in the world-system it must do so at the expense of the other nations, reinforcing the polarity (1979:100-101; 1980a:179). A clear explanation of the necessity of this is not provided.

Another possible reason for the resilience of world-systems theory is its simplicity. As with other functionalist theories it relies on a biological analogy. The social system in question is compared to an organism (Wallerstein 1974:347). The demonstration of relations between the members of a group, however, does not in itself constitute proof of the existence of a "system."[18]

Even if the existence of a world-system can be demonstrated, this is not sufficient reason to believe that the system has needs or goals. One accusation that can be leveled against Wallerstein is that he often builds

his argument on teleological assertions (Skocpol 1982:1078, 1088; Rapkin 1981:259). It would be superfluous to outline the weaknesses of this form of argument (see Saunders 1981:212-215; Pratt 1978: 117-131).

Wallerstein's model also suffers from an inadequate treatment of history, as this critique based on the evidence from the Cook Islands has hopefully shown. It can be claimed that the Pacific microstates, for reasons peculiar to themselves, are exempt from world-systems theory and that this accounts for the theory's weakness in explaining the development of the Pacific Islands. The claim that Wallerstein has misinterpreted history, however, does not rely exclusively on evidence from the Pacific, or even from the twentieth century (Skocpol 1982). The whole basis of his theory--his analysis of Europe in the seventeenth century and beyond--has also been subject to attack. Rapkin accuses materialist historians in general of ransacking history in an *ad hoc* and incomplete manner (1981: 245). Wallerstein has been no exception.

For many people the real strength of world-systems theory lies in its central theme: The development of a nation is significantly determined by its role in the world-economy. While this is undoubtedly true, Roxborough's cautionary comments on the subject are worth noting: "To assert that one cannot study processes of social change without putting them in their context does not imply that the only important factor is the external context itself. Some radical dependency theorists have at times inclined toward a one-sided emphasis on the determining role of the world-market, and have seen developments within Third World countries as mere reflections of, or responses to, exogenous changes" (1979:25).

For others the driving force behind world-systems theory is its portrayal of exploitation. Wallerstein's model allows them to interpret the dependence of the Cooks as a result of underdevelopment and hence exploitation by New Zealand.[19] Exploitation is not the only possible explanation of the lopsided nature of the Cook Islands' economy, however, Bellam to the contrary (1980a:29); other factors must also be examined. The islands comprising the group are small, have limited resources, and for various reasons lack a significant skilled work force (B. Shaw, cited in Lewis 1988:50). These factors have obviously been of major importance. The MIRAB set of phenomena has also had its part to play.[20]

Some would argue that in spite of all its weaknesses world-systems theory has made an important contribution to the debate on development. It has generated research focusing on important issues and pro-

vided a more sophisticated critique of modernization theory than Frank's work, for example. Although I consider Wallerstein's model to be of dubious value it is unlikely to lose favor in the academic institutions of the Pacific for many years. As Ball points out, no "matter how waterlogged [a theory] is a research program will not sink and have to be abandoned until a better, more buoyant one comes along to replace it" (T. Ball, quoted in Higgott 1983:7).

The limitations imposed by the reliance on theories imported from outside of the Pacific to explain developments within it are increasingly being recognized. Meleisea has humorously portrayed this (1987). Lewis (1988) concludes that none of the theories of capitalist transformation, from dependency and conventional Marxist theories to modernization theories, apply particularly well to the part of the Cook Islands he was studying. The same could be said of world-systems theory.

As Packenham says, "It is possible that we are at one of those junctures of political and intellectual history that cries out for the brilliant theoretical innovator with a talent for creative synthesis. In certain respects the time seems ripe for going beyond the rigid established ideologies and paradigms and establishing more fruitful and compelling new ones" (Robert A. Packenham, cited in Higgott 1983:[ix]). Unfortunately, world-systems theory has been unable to meet the challenge and we will have to look elsewhere for the synthesis required.

NOTES

1. Wallerstein defined strength to include both strength vis-à-vis other states within the world-economy and strength vis-à-vis local political units within the boundaries of the state (1974:355).

2. An initial economic advantage could provide the state with sufficient revenues to create an army, an important component in maintaining dominance (Skocpol 1982:1080; Wallerstein 1974:136).

3. In many cases, for example, colonial authorities imposed a tax on the indigenous population, which they required to be paid in cash. The tax was sanctioned by violence where necessary and was the means by which a sufficient labor force was created to operate mines and plantations.

4. According to Morrell, parts of the Cook Islands in the mid-1800s were near-theocracies. While the chiefs still exercised power, John Williams was in practice the real ruler of the islands. A native police under the control of the missionaries was established to enforce certain moral standards--fornication and smoking, for example, were both punishable offenses (Morrell 1960:280; see also Lewis 1988:17).

5. The role of the missionaries was not deliberate in the sense that they were trying to incorporate the Cook Islands into the world-economy. The missionaries were trying to generate funds to support their operations elsewhere in the Pacific. In fact, the policy of the central administration of the London Missionary Society was to prevent the islanders from becoming dependent on trade. This was for several reasons, not least of which was the possibility that contact with the traders might have weakened the influence of the missionaries.

6. The unequal distribution of the rewards of trade did not necessarily generate dissatisfaction with the chiefs among their followers. The literature on conspicuous consumption suggests that it is quite likely that the members of a clan saw the conspicuous consumption of their chief as a public symbol of their clan's prestige.

7. Moss attempted to create a class of yeomen, a society of peasant farmers independent of obligations to their chief and kin (Crocombe 1964:83).

8. Colonel W. E. Gudgeon, for example, who replaced Moss as British Resident in 1898, believed that "stern authority" was essential in dealings with Polynesians (Crocombe 1964:48).

9. Several New Zealand writers during this period viewed the Pacific as a veritable cornucopia for New Zealand. In 1857, Charles Hursthouse published his book *New Zealand, the Britain of the South,* in which he referred to the proximity of "the thousand Polynesian Islands, slumbering in their summer seas . . . needing only the magic touch of steam to open new worlds to [New Zealand's] commerce" (1857:52). The potential for prosperity inspired Martin F. Tupper to pen the immortal words (quoted in Ross 1964: 53-54) :

> Queen of the South! Which the mighty Pacific
> Claims for its Britain in ages to be. . . .

10. The exact nature of the land tenure system varied from island to island (Crocombe 1964:4; Hecht 1987: 188; Crocombe and Marsters 1987:202).

11. The migration component of MIRAB dates back much further than this, however (Douglas and Douglas 1987:39).

12. Gudgeon believed that there were already too many "educated' islanders and that education merely made the young men dissatisfied with work on the land (their "true work"). He claimed that the Cook Islanders would only be capable of using their education once they had obtained a "stiffening of European blood' through race contact (Gilson 1980:164-173).

13. Writing in 1976, a secretary of education from the Pacific region commented that "because of the smallness of [his] country, employment opportunities [had] been very limited and [would] continue to be so and [would] not cope with all [the] school leavers each year. There [was] work in agriculture and other self-employed activities available--but [the] children [had] been educated to expect to work for others in offices, shops, factories on a permanent basis. There [was] plenty of casual work as orange pickers or plantation workers but people [were] unwilling to work on such a basis, considering it below themselves" (quoted in Dickson 1976:41). A similar situation applies in the Cook Islands. There were difficulties encountered in acquiring labor for fruit picking in the large islands ten years earlier than this (Kelly 1984: 114).

14. Some would argue that the Cook Islands cannot meaningfully be described as a nation at all. Whether or not one accepts this point the thesis that Wallerstein's model does not fit well with the reality of the Cook Islands stands.

15. Initial attempts to increase productivity were most likely motivated by the needs of traders and produce marketers in New Zealand. Later attempts were aimed mainly at reducing the levels of aid required by the Cook Islands.

16. We can only speculate on the nature of the Cook Islands state had the rights of the chiefs to control production been maintained in one form or another.

17. Wallerstein claims that the only alternative to the world-system currently in existence (other than a world empire) is a system operated by a global socialist government. Only the reintegration of the political and the economic realms would enable exploitation to cease and the nations of the world to advance together (Wallerstein 1974:348).

18. See Charles Gore's discussion of the use of functionalist analogies in examining spatial systems (1984), especially pp. 200-210.

19. New Zealand is virtually the sole market for Cook Islands goods. From 1976 to 1979 New Zealand accounted for two-thirds of Cook Islands imports (Fairbairn 1984:57).

20. The fact that New Zealand benefited economically from the arrival of Polynesian migrants does not in itself prove exploitation. To the migrants New Zealand represented an opportunity to experience a better life, both in New Zealand and back in the islands.

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EDITOR'S FORUM

THE NONGOVERNMENTAL ORGANIZATION FACTOR IN DEVELOPMENT: A VIEW FROM THE SOLOMON ISLANDS

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Over the past forty years development efforts have focused on national economic growth. In the early years development was conceived of in terms of a nation's achieving rapid industrialization. By the mid-1970s this mainstream argument--that economic growth was at the heart of development-- was being discredited, not on the grounds that economic growth had not occurred but more importantly that it was not alleviating poverty. A World Bank report stated: "It is now clear that more than a decade of rapid growth in underdeveloped countries has been of little or no benefit to perhaps a third of their population. Although the average per capita income of the Third World has increased by 50 percent since 1960, this growth has been very unequally distributed among countries, and socio-economic groups."[1]

A more recent and stronger criticism of conventional development wisdom comes from the Club of Rome's informal grouping of government leaders, scientists, economists, and businessmen who seek to influence national policies by recommending new strategies. A recent report by Bertrand Schneider, the club's secretary-general, focuses on nongovernmental organizations (NGOs) as the new agents of change. His book, *The Barefoot Revolution,* calls upon governments and financial institutions to recognize NGOs as fully committed agents of development and to support them with appropriate funding. It asks the major development decision makers to recognize the NGO presence: "This new trend

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is radically changing the tenets of development that have prevailed until now, for it entails a complete overhaul of 20 years of economic strategy that has not fulfilled its promises."[2]

These authoritative statements say in effect that the past forty years of development efforts patterned on Western industrial society models have been failures and it is clearly time to change, to try less ambitious and more pragmatic approaches. Engineers and water experts now say most of the giant water projects undertaken in the Third World since 1960 have been disasters and have had devastating ecological effects (Egypt's Aswan High Dam is an example). The Tanzania farming project in Arusha was far too intensive for the valley's delicate tropical ecology. Brazil's debt has been aggravated by enormous construction projects. The Club of Rome report says that in Africa, as in Latin America, food self-sufficiency has been undermined by spending scarce cash on huge agroindustrial schemes to grow cash crops for export.

But NGOs ask that the analysis go a step further. They do not see themselves as new agents of change simply by pushing self-help, grassroots projects. It makes little sense to them to seek funds for poultry projects when villagers' very life sources--forests, streams, rivers, reefs--are being destroyed by logging companies. It is not that the motto "Small Is Beautiful!" is wrong, but inadequate. NGOs are becoming more and more interested in "Strong Is Beautiful!" They define development as "empowerment" rather than the now-discredited notion that North/South resource transfers are the solution. Empowerment means increasing villagers' capacity in relation to the surrounding world. In the village-oriented Pacific empowerment takes the form of instructing villagers to use the political, bureaucratic, and economic systems to better their lives. A major goal of the Solomon Islands Development Trust, a local NGO more fully explored later in this essay, is to engage village people in public discussion in which problems, difficulties, and issues they now face become the agenda items of public debate.

The NGO Era

Recent studies recognize that the emerging Pacific era has already burst upon us. Of course their focus is primarily on economics and military might, and on Pacific rim countries, not the island nations. Adapting to this major shift from a North Atlantic preoccupation to a Pacific focus, however, poses unprecedented challenges.

Many Pacific Islands nations have long histories of what is now called

the nongovernmental organization presence. Christian churches in fact sometimes preceded government structures and before them were village and chiefly organizations. Since the advent of national government structures, however, sports clubs, economic organizations, civic groups, and now the nongovernmental organizations (sometimes called people's organizations, citizen groups, or voluntary organizations) have increasingly become part of the public scene.

The most recent players, the NGOs, have jumped into the development scene with both feet. In just a few years some of these groups have moved from a peripheral to a more central role in providing leadership for national and international development. The Solomon Islands Development Trust (SIDT), for example, is an indigenous nongovernmental organization working especially in the village sector of the Solomon Islands since 1982. A recent evaluation team from the International Science and Technology Institute of Washington said that SIDT was "by any standard a resounding success. After only three years of operation, it has an annual budget of about $250,000 from diverse sources, a dedicated, highly respected, and competent director, a well established purpose and program, an effective and appropriate operational methodology, a well-developed and dynamic system of training and re-training over 100 villagers who staff its mobile teams in all the far-flung islands of the Solomons."[3]

The SIDT Story

The winds of political change blew strongly in the Pacific in the decade of the 1970s. Many new nations were born (Fiji, 1970; Papua New Guinea, 1975; Solomon Islands, 1978; Kiribati and Vanuatu, 1980) but few had the foresight to realize that an independent government should also be matched by local, independent developmental groups as well. In early 1980 the first meeting of people interested in beginning a local developmental body in the Solomon Islands met in Honiara, the national capital.[4] SIDT was conceived at that time. It was the second such organization in the Pacific; the first, Nasonal Komuniti Developmen Trust, began in Vanuatu in 1979.

Only in mid-1982, however, did SIDT actually begin to function, after the Foundation for the Peoples of the South Pacific interested the development group Private Agencies Collaborating Together in funding the new organization. In its first two years (1982-1983) the young institution formulated a development philosophy, instituted a training program, and recruited staff. Its Board of Trustees directed SIDT to be

involved in development education and awareness building, not project funding and implementation.

By early 1984 SIDT had grown from handful of people stationed in Honiara to more than seventy-five men and women working in villages in many parts of the country. The newly formed organization welded disparate individuals--five dozen island trainees, ten recently arrived Australian volunteers, and an untested training staff--into a working group. Their initial task was formidable. SIDT worked closely with the government's Rural Water Supply and Sanitation Program, adding an educational component.

The following two years (1985-1986) deepened team members' understanding of development issues. They went out to villagers and worked closely with government personnel in health, education, and water supply. Training, retraining, and training again of Mobile Team members (MTMs) became the critical factor in its village outreach program. SIDT's main goal of strengthening village life and empowering villagers put a special emphasis on recruitment of women and building bridges between and among other NGOs nationally and internationally.

Cyclone Namu in May 1986 became a watershed for SIDT. The organization responded quickly to the government call for a disaster survey, supplying more than half the personnel needed to carry out the nationwide survey. Further government requests--for a nutrition research survey, home gardening program, and housing rehabilitation awareness program--were also carried out. SIDT's strong involvement increased government acceptance of the organization.

The events of 1986 also prepared SIDT for a new, three-year program--Disaster Awareness and Preparation 1987-1989. Development education now included ways to better prepare villagers for natural and man-made disasters. Using the same outreach methodology, the MTMs toured the many scattered, small-population villages. In the first three years of work (1984-1986) these teams had conducted more than twelve hundred village-level workshops. The program had grown: 115 field staff (villagers all) organized into thirty-two teams operating in all seven provinces.

A major event of 1987 was the publishing of *LINK* magazine, a bimonthly dedicated to giving villagers a voice in the decision-making process. Many village problems have direct links to decisions made in Honiara. *LINK*, a magazine for and about village life, is a way for villagers to make known their thoughts, aspirations, and plans for the country's future. Copies of the magazine were also used by the Mobile Teams in their village outreach program. By the end of 1989 thirteen

issues had been published, each having a print run of over three thousand copies.

Theater has also become a highly successful tool for sharing information and bringing fresh perspectives to villagers. In 1988 SIDT, with the help of a CUSO volunteer, began the SEI! Akson Team, a group of young people who toured villages and performed prepared skits about the effects of logging, the value of local rather than imported food, and medical practices such as immunization. It would be hard to exaggerate its power. As one young mother said, "Now I know why my child needs three injections. I always thought that one was enough!"

The success of the theater group led directly to the publishing and use of comics as outreach tools. By the end of 1989 SIDT had published six small comic books in Pijin English, called *KOMIKs.* More than fifty thousand copies have been distributed throughout the country, covering such topics as family planning, logging, life in town, and planning a feast. The nation's secondary-school teachers requested that *KOMIKs* be sent to each school for use as textbooks in the social science curriculum.[5]

SIDT's efforts have not been confined to its internal well-being. The organization has sparked a local umbrella NGO group, the Development Services Exchange (DSE). At present DSE has more than thirty-two members, with twenty-four of them paying members of an organization that acts as an informational clearinghouse for NGO activities as well as a lobbyist for its component members.

On the international scene, SIDT has been in the forefront of NGOs wishing to join forces across national boundaries. At present SIDT functions as Secretariat to the Pacific Islands Association of Non-Government Organisations (PIANGO). Great effort is now focused on helping Pacific Islands NGOs establish national liaison units (NLUs), which could function as a country's NGO representative body at PIANGO and other international meetings. Some countries such as Fiji, the Solomon Islands, and Tuvalu already have strongly functioning NLUs, while other countries are in the process of establishing them. At the October 1989 meeting in Melbourne where PIANGO's Steering Committee met, other country representatives detailed their efforts to establish NLUs in their respective countries.

Has SIDT's Work Changed Anything?

What changes have come about from SIDT's village outreach program? Have the time, effort, and funding been worthwhile? Have the work-

shops and village discussions produced better patterns of living, changed minds to new ways of thinking, strengthened institutions, and rooted beneficial structures? Good changes have come about. Some are only now surfacing and will need years of work to bloom. Other trends are more clearly seen.

The national development debate has been broadened from an economics emphasis to include the social and cultural concerns of villagers. A recent government initiative, for example, focuses on rural human resource development, the first of its kind for the country. Funding worth SI$9.4 million came from the European Economic Community.

Villager participation in the development process has changed from rhetoric to reality. The Constitution and the Provincial Government Review committees recently toured villages to consult and involve villagers in the review process.

Cooperation between government and the private sector has grown appreciably. The participation of NGO personnel in governmental projects such as the nutritional research survey, home gardening project, and so forth are signs of the increasing acceptance and cooperation between the two.

Personal growth of SIDT personnel is a striking feature of a program changing people. SIDT's village personnel are "dropouts" from the school system. Coastal dwellers often judge "bush" people as inferior, second-class citizens. Eddie, a field-worker from North Malaita, recently reported after a trip to Malaita's interior, "We talk about self-reliance but they are doing it." With training, village involvement, and a supportive organization these dropouts become productive and responsible rural citizens.

Enabling villagers to ask the right questions and use the political and bureaucratic systems empowers. The team members' ability to share information and bring new ways of thinking enriches both giver and receiver. In Guadalcanal a Mobile Team conducted a workshop deep in the bush. On the last day of the workshop a participant revealed he had recently chased away a helicopter and its crew doing unauthorized explorations on his land. He had wrestled with his feelings over the last three weeks: Had he acted correctly? Was what he had done right? Would the police come after him? When he shared his feelings and was informed that his actions were entirely within the law and that he had done the correct thing, the weight of his action left him and he became a free person once again.

Certain program results--attitudinal change, skill enhancement, individual accomplishments--are the more obvious fruit of the village

outreach program. Other results such as awareness building are only beginning to root. If allowed to bloom, greater change will occur in the future. Table 1 gives an overview of SIDT's 1988 program of outreach activities and is representative of a year's work.

Financing the Program

To organize, direct, and train personnel for a national outreach program calls on many resources. SIDT's major resource base, however, is the group of rural workers who train, tour villages, and share their development insights with other villagers. Without their frequent and continuous personal contacts, SIDT's work would remain locked up in the capital city. But running a national village-outreach program of more than 130 workers is not a shortcut to development. Creating and sustaining a community education pattern from the ground up is an expensive but necessary step to engage the backbone of the nation, the village person.

Funding sources come mainly from people's organizations in other countries. Some of these nongovernmental organizations are as close as Australia: the Overseas Service Bureau, Australia Freedom from Hunger Campaign, and Community Aid Abroad. A major contributor, however, lies at a distance, Holland's Interchurch Coordinating Com-

TABLE 1. **SIDT Touring Program, 1988**

Province	Disaster Awareness and Development Workshops	Special Workshops[a]	Participants
Central	48	6	2,425
N. Guadalcanal	49	8	2,125
S. Guadalcanal	12	42	1,500
N. Malaita	79	3	1,474
S. Malaita	53	4	2,409
Temotu	62	3	2,419
Makira/Ulawa	42	3	1,732
Western	41	0	1,434
Ysabel	44	6	2,395
Total	430	75	17,913

Source: Adapted from Solomon Islands Development Trust, *Annual Summary 1988* (Honiara, 1989), 3.

[a]Special workshops include: logging and resource reviews; kitchen gardens; women's interests; raising village quality; communal education; leadership courses; and, especially, political education.

mittee for Development. This one group financed almost 29 percent of the village outreach program in 1989.

Despite the great assistance received from overseas, SIDT's own efforts to become more self-reliant have grown. Proceeds from sale of services, publications, and subscriptions supplement the support that comes from more than seven hundred villages housing, feeding and working with the MTMs as well as government contributions in the form of transport, use of facilities, and the work of extension officers. Last year's expenditures and sources of income are detailed in Table 2.

Lessons Being Learned

Development Involvement

Some people balk at the NGOs' having assumed certain leadership roles in development work, roles thought to be those of elected officials. Development is government's prerogative, contends conventional wisdom. Governments are installed by a public, more or less democratic selection process, for which everyone understands the rules. Who gives the NGO its authority to be working in this field? Should not a properly elected government official be worried about the idea that NGOs are and ought to be moving into a more central role in providing development leadership?

This fear relates to the fact that NGOs can be used and manipulated by outside interests. The outsiders may not be the ones who originally conceived and created the local development organization, but their generous support has a great effect in determining which of them will live and which will die. "Indigenous" NGOs may become so dependent on external support that these external agencies become their effective constituencies.

These contrary views of government's and NGOs' developmental roles lie at the heart of the present-day development debate. The two views crystallize the fundamental conceptual problem. Are the significant resource transfers favored by the large donor groups and governments themselves the essential key to stimulating a sustained development process? Or is it the more effective use of personal, physical, and financial resources to develop human and institutional capacity that holds the key to authentic development, as the people's organizations insist?[6]

The population issue comes to mind. The Solomons' dramatic population increase, 3.5 percent annually, is currently a hot subject. Govern-

TABLE 2. **Financing SIDT's Outreach Program: Analysis of Funding and Expenditures, 1989**

Source	Amount (SI$)	Percentage	Activity Funded
Total	$624,358	100	
By Country:			
The Netherlands	195,119	31	
Interchurch Coordinating Committee for Development	178,038		Administration
Dutch Bishops' Lenten Campaign	17,081		MTMs training
Australia	167,587	27	
Overseas Ser. Bureau	75,363		Volunteers
*AIDAB	31,940		Women's program
Community Aid Abroad	26,874		MTMs: S. Malaita
Freedom from Hunger Campaign	22,404		MTMs: N. Malaita
Asia South Pacific Bureau of Adult Ed.	11,006		*KOMIKs,* Travel
European Community		19	
*European Economic Community	115,949		*LINK* magazine, administration
United Kingdom	75,160	12	
Christian Aid	49,446		MTMs: Western & Central
Foundation for the Peoples of South Pacific	14,414		MTMs: Guadalcanal/ Ysabel
Isle of Man	11,300		*LINK* magazine
United States		6	
Catholic Relief Services	35,380		MTMs: Makira & Temotu
Solomon Islands		6	
Solomon Islands Development Trust	35,163		Travel, administration, scholarships
By Sector:			
*Government Sources	147,889	24	
Nongovernment Sources	441,306	70	
Own Sources	35,163	6	

Source: Adapted from Solomon Islands Development Trust, *Summary Report 1989* (Honiara: Provincial Press, 1990).

ment red buttons are flashing and danger flags are flying. Urban planners and decision makers are detailing the additional classrooms, new clinics, and required infrastructure investment needed to cater to the alarming population growth. Yet the villager, the essential decision maker when it comes to having or not having additional children, has yet to be fully brought into the picture. Yes, radio messages about the problem are aired. But these are communiques, not communication--the two-way flow of information so vital to such a subject. Mass media attempts, such as they are, are confined to the Honiara area. No one doubts that the villagers must be involved. If villagers increase their understanding of the major issues bothering them, then there is a reasonable hope that they can begin to address them in a creative manner. Without adequate, timely, and continuous information they are unable to fully define and analyze the problems, much less act on them.[7]

Some evaluators, when examining SIDT's village outreach pattern, ask, "What is the next step once villagers become more aware of their problems?" What the questioner assumes is that once awareness of development issues has come about, the real "meat and potatoes" of development--doing a project--must be the next step. But raising awareness sets loose new energies. SIDT has found that sometimes a project proposal may be the next step. Frequently, however, villagers--who up until this stage had missed out on the information revolution, being the last to be informed of what is happening in their own country --show great interest in grasping and wrestling with the deeper issues: questions of land tenure, natural resources ownership, and the relationship of resource owners to local government councils and the central government.

Social Movements

To bring about the people-centered development we have been speaking to requires people-accountable institutions.[8] People-accountable groups, a fair description of the Pacific NGO movement, help respond to the fears of government officials and politicians about the NGO sector's seeking to become more deeply involved in development leadership roles. But these same people-accountable institutions are responding to other needs as well.

The world economic crisis has reduced the efficiency of, and popular confidence in, the nation and its customary political institutions. The recent Eastern Bloc vaporization presents a sober lesson. The major transformational forces of village lives--modernization, technological

change, cash economy, economic development--were and are processes hardly driven or directed by social movements or state institutions. These processes have reduced popular confidence in the nation and in the ability of its customary political institutions to defend and promote villagers' interests.

In the recent past the unwritten social contract whereby Solomon Islanders felt that the government apparatus was basically on their side has been jolted. They have begun to doubt that their "big-men" in government know what is best for the villager, for the country. In April 1988, for example, the Solomon Islands prime minister was presented with written petitions produced at two public demonstrations; parliamentary no-confidence motions have become endemic in the Solomons (and in Vanuatu and Papua New Guinea as well). An Australian journalist's statement about Papua New Guinea probably summarizes what has happened since independence in all Melanesian countries: "On average, today's national citizen is poorer than in 1975, the year of independence; but in towns affluence is more and more conspicuous."[9]

Given this atmosphere rural and urban villagers are turning to groups, movements, organizations offering new interpretations and solutions to the problems of conventional development efforts. Some people, for instance, turn to strongly religious movements such as charismatic prayer groups. Others prefer organizations like SIDT that carry a nonmonetary message, stress ideals such as self-respect and a spirit of unity, and favor a new type of *wontok* (literally, "one talk"; a blood relative). As a nationwide organization SIDT has made friends among people who were traditionally strangers, if not enemies. Team members from Temotu, for instance, speak at ease about their work with those from Malaita and Western provinces. They have become friends.

Internationally the PIANGO movement seems to strike a responsive chord. NGO communities across the Pacific seek to forge links, if not coalitions and networks, with other Pacific Islands organizations. In 1950 many island national leaders met for the first time under the South Pacific Commission's auspices. Twenty years later, at the first meeting of the South Pacific Forum in 1971, they took a second critical step in Pacific Islands togetherness. In 1988, however, island leaders seem to be breaking up into a Melanesian and a Polynesian camp, but the NGO sector pursues a unification theme. PIANGO strives to create an institution that reflects commonalities rather than accenting differences. The former governor of American Samoa, in a letter asking if it could host PIANGO's inaugural meeting, made the point that "I am also pleased to note that the PIANGO effort has effectively dismantled the concept of

political boundaries in its organizational planning which is truly representative of the NGO sector."[10]

Problems

NGOs write their own scripts for the most part. They represent a complex mosaic of local organizations and groups, each with its own agenda. The bulk of the NGO community remains focused on particular aspects of development: small-project funding, income generation, women's issues. Others have gradually moved to a program mode. The water supply program of Vanuatu's Nasonal Komuniti Developmen Trust, for example, is becoming an entry point for establishing adult and ongoing education patterns. At a third level, a handful of NGOs interpret their role in terms of influencing policy making or acting as a catalyst. They have traveled from projects through programs to playing a role in policy formation. In 1988 and 1989, for instance, SIDT's director, Abraham Baeanisia, was closely involved with senior government personnel in the process of forming government policy on AIDS, population education, and youth.

However, the traditional or even newly created NGO rarely has the strategic competence, organizational forms, or management methods to cope with this new workload. Kortin's words of warning must be heeded: "But when NGOs position themselves to be systems catalysts, their technical weaknesses become more apparent. Some of the most important of the organisations with which they work will be large, influential, and staffed by highly credentialed professionals. Needless to say, the NGO that presumes to help such organizations become more effective must be guided by more than good intentions."[11]

NGOs also have a critical role to play in providing feedback and advice to government officials. With their effective communication links with village groups, they can introduce new ideas and initiate change at the local level. But at the same time they have public and social responsibility. Development education must flow both ways: to the villager as well as to the government policy makers and administrators.

Conclusion

Two awarenesses seem to be emerging at the same moment: a deeper Pacific consciousness and a recognition of the worth of the NGO sector. The Club of Rome's report calls the nongovernmental organizations the

new agents of change. Large and powerful funding groups--AIDAB, the UN family, USAID, and EEC--are increasingly focusing attention on reaching out to village populations. Island governments' growing inability or unwillingness to touch their grass roots in a sustained and creative way has forced these major development agencies to call more frequently on people's organizations. They are looking for convenient and inexpensive alternatives to government for seeing that resource transfers get more reliably to those in need.

But some of the newly formed NGOs see themselves not so much reflecting a "small is beautiful" philosophy but one of "strong is beautiful"--strong not in power over others but by empowering villagers to go from "cannot" to "can," to democratize the development process. They wish to move from a peripheral to a more central role in providing leadership for national and international development. The growing Pacific NGO presence in the development field and in people's movements hastens the day. The NGOs recognize the importance of networking and coalition building in order to continue the dialogue between government and themselves and to insure more informed participation in policy formulation by villagers.

NOTES

1. Hollis Chenery, "Redistribution with Growth," in *Redistribution with Growth,* ed. Hollis Chenery et al. (London: Oxford University Press, 1974).

2. Bertrand Schneider, *The Barefoot Revolution* (London: IT Publications, 1988), xii.

3. Richard Huntington and John Oleson, *PVO Institutional Development Evaluation Series: Field Report 8* (Washington, D.C.: International Science and Technology Institute, 1987), 12-13.

4. The following section draws on information from *Quarterly Reports,* vols. 1-29, Solomon Islands Development Trust, May 1982-June 1989.

5. Social Science Teachers' Curriculum Panel/SIDT, 21 June 1989, Teachers Training College, Panatina Campus, Honiara. Each secondary school has now received the *KOMIKs.*

6. David C. Kortin, "Third Generation NGO Strategies: A Key to People-Centered Development" (MS prepared for *World Development,* Oct. 1987).

7. George Kent, "Empowerment for Children's Survival" (Department of Political Science, University of Hawaii, draft, 3 Dec. 1987).

8. Some of the ideas in this section were drawn from Andre Gunder Frank and Marta Fuentes, "Nine Theses on Social Movements," *IFDA Dossier* (International Foundation for Development Alternatives, Nyon, Switz.) 63 (Jan./Feb. 1988), 27-44.

9. Gabriel Lafitte, "Papua New Guinea: The Politics of Renown," *ABA Newsletter,* Spring 1987:5.

10. Lutali to Abraham Baeanisia, 11 Feb. 1988.

11. Kortin, "Third Generation NGO Strategies," 15.

BOOK REVIEW FORUM

Roger M. Keesing, *Melanesian Pidgin and the Oceanic Substrate.* Stanford, Calif.: Stanford University Press, 1988. Pp. vii, 265, appendix, bibliography, index. US$37.50.

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Melanesian Pidgin English (MP) is best known from the many studies of one of its dialects, Tok Pisin, spoken in Papua New Guinea. Only in recent years have more detailed studies begun to appear of the other two dialects of MP: Pijin, spoken in the Solomon Islands, and Bislama, spoken in Vanuatu (formerly the New Hebrides). Roger Keesing's *Melanesian Pidgin and the Oceanic Substrate* is an important book because not only does it provide detailed information on Solomons Pijin (or Pidgin) but also it looks at the development of MP in general from new perspectives. The book is also assured popularity because it provides more fuel for the "substrata versus universals" debate that has been raging for years in pidgin and creole linguistics (see Muysken and Smith 1986).

For those unfamiliar with this debate, it basically concerns the origin of some grammatical features common to many pidgin and creole languages. The "substratist" position is that these features come from the mother tongues of the speakers who were instrumental in the development of the pidgin--that is, from the "substrate languages." The "universalist" position, on the other hand, is that these common features represent basic, perhaps inborn, properties of human language or common patterns of second-language acquisition.

In the first of fourteen chapters, the author outlines the major themes

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of the book and his position in this debate. His main argument is that Eastern Oceanic languages of the Austronesian family have heavily influenced the structure of all three MP dialects, thus supporting the substratist position. But Keesing does not take a one-sided viewpoint in describing the development of MP; he also considers universals of human language and language acquisition as having played an important role as well as the "superstrate" language or the "lexifier language," that is, the language that provides the bulk of the vocabulary (in this case, English).

Keesing's other major themes are concerned with the chronology for the development of pidgin in the Pacific and with the relationship of the three dialects. In most of the literature (e.g., Clark 1979), it is assumed that there was an unstable (or highly variable) precursor to MP, called South Seas Jargon, which was spoken around the Pacific from the early part of the nineteenth century. This supposedly became a less variable variety, or a stable pidgin, sometimes called "Early Melanesian Pidgin," in the 1860s and 1870s when it was used among Pacific Islanders recruited to work in plantations in Samoa and Queensland. Later, when repatriated laborers from New Guinea, the Solomons, and the New Hebrides took this pidgin back to their own countries, it expanded in both its functions and grammatical structure and diverged into the current three dialects. Keesing, however, claims that a stable pidgin developed not on the plantations but years earlier in trading enclaves in the central Pacific and on trading ships. He argues that the most important stabilization and expansion of the Pacific pidgin that became MP took place prior to the plantation era, and therefore prior to the separation of MP into different dialects. Thus, he believes, the differences between the dialects are not as great as some linguists have claimed.

In chapter 2, Keesing expands on the theme that a stable pidgin developed in the Pacific much earlier than the prevailing wisdom holds. He begins by describing the "crucial phase" in the formation of a Pacific-wide prepidgin or jargon. This was associated with the whaling and trading ships that frequented the central Pacific in the 1840s, at first mainly in Pohnpei and Kosrae and later in the Gilbert Islands and Rotuma. Keesing shows the sociolinguistic conditions on these islands to have been perfect for the development of a pidgin language, with contact between many different ethnic groups using English as lingua franca. But he also claims that similar contact took place on the ships as well, since a great number of Pacific Islanders were aboard working as crew. He says that because these ships crisscrossed the Pacific there was a great deal of contact between various islands and, therefore, the

emerging pidgin was more homogeneous and stabilized than is usually thought.

According to Keesing, this emerging pidgin was later brought by these ships to the southwest Pacific, especially to the New Hebrides and New Caledonia, during the sandalwood trading period, described in chapter 3. During this period, beginning in the 1840s, men from the Loyalty Islands (near New Caledonia) also played an important role in working on trading ships. Several examples of the "Sandalwood English" spoken around the central and southwest Pacific from 1850 to the early 1860s are given in this chapter. The author points out the linguistic features of modern MP present in these examples and concludes that this lingua franca should be called a pidgin rather than an unstable jargon. He speculates that "incipient creolization" may also have been occurring during this period, as children of islanders involved in trading and shipboard work learned this emerging pidgin either as a first language or as a coordinate first language (along with another).

In chapter 4, the author continues to develop his thesis, saying that "sophisticated islanders" (those who had worked on ships or taken part in trading) continued to be involved in the Pacific trade as ships' crew or as recruiters, brokers, or plantation foremen. Since these people already knew the "emerging pidgin," they were responsible for teaching it to the laborers and spreading it to the plantations. Keesing mentions the Gilbert Islands as an important source of plantation laborers as well as crew members, but he gives figures showing that the most important source of laborers was the New Hebrides and the southeast Solomon Islands. Then he moves to the linguistic evidence. Following provisos about the reliability of the data (all from European sources) and a suggestion that Pacific Islanders and Europeans spoke different "registers," he presents several pages of examples of the pidgin spoken in the first decades of the Labor Trade, 1863-1885. Again, he extracts many linguistic features from the examples that are present in modern MP and concludes that the "essential patterns" of all three dialects of MP were established by 1885.

This argument is expanded in chapter 5, which is basically an attack on the view that Tok Pisin developed separately from the other two MP varieties, a view that Keesing attributes to Peter Mühlhäusler (1976, 1978). Keesing reiterates his point that an already stabilized and homogeneous pidgin was introduced into the plantations in Queensland and Samoa during the beginning of the Labor Trade. He also says that these areas were connected until the end of the 1880s by common recruiting grounds and by laborers who reenlisted and moved from one location to

the other. Thus, he claims that, except for a few lexical differences, the varieties of pidgin spoken in Samoa and Queensland were identical. Finally, he suggests that when the pidgin was brought from Samoa to the New Guinea islands by repatriated laborers, it was relexified--that is, a large number of English words were replaced by words from local languages. Because of this large Melanesian-derived content, the author says, Tok Pisin is wrongly considered as the "genuine and canonical dialect."

In chapter 6, the author moves on to discuss the substrate languages that "shaped the structure" of the pidgin that was the forerunner of MP. Because of the preceding arguments, he discounts the influence of the linguistically diverse New Guinea languages. Instead he looks at the relatively homogeneous Eastern Oceanic (EO) subgroup of Austronesian languages, spoken in the areas of the central and southwest Pacific where he says the pidgin originally developed.

After giving some details about historical reconstruction and the subgrouping of EO languages, the author describes what he calls their "core syntactic structures" (p. 69) and "global syntax" (p. 83)--in other words, their common grammatical features. These include subject noun phrase-verb phrase-object noun phrase (SVO) word order, the same as in English. Also like English the subject and object noun phrases include a noun or pronoun, but unlike English these phrases are optional. An EO sentence may consist of only a verb phrase but a very complex verb phrase, differing greatly from English. The verb is preceded by a pronominal particle that refers to the subject, called the "subject-referencing pronoun" (SRP) (p. 70). This is opposed to the pronoun that may act as the subject in the noun phrase, called the "focal pronoun" (FP). If the verb is transitive, it usually has a special suffix added to show this. There may also be another marker added to refer to the object. The chapter includes examples of these structures from a wide variety of languages. It ends with examples of Solomons Pidgin compared to Kwaio, an EO language spoken on the island of Malaita, showing a direct correspondence in words and parts of words between the two languages.

In chapter 7, the author stresses the interplay between substrate and superstrate languages and universal tendencies in the development of MP. He suggests that because of the similarities among the EO languages of the Pacific Islanders in contact and the superficial similarities (especially in word order) between these languages and English, there was no necessity for extreme "bending, simplifying, and rearranging" in order to reach linguistic accommodation and the compromise pidgin language (p. 91). In situations where typologically diverse languages

come in contact (as in the development of Chinook Jargon in North America), the result may be a variety stripped down to the basic universal properties of language. But Keesing argues this was not the case for MP, for which basic EO structures were the common denominator.

Another major point of this chapter concerns the superstrate or target language to which Pacific Islanders were exposed. The author suggests two facets to this exposure. Before the 1850s and to a lesser extent in later years, islanders were exposed to English spoken by white men but heavily influenced by "foreigner talk" or nautical pidgin. From the 1860s onward, however, many islanders were fluent in the stabilized pidgin they used to communicate among themselves. This pidgin, rather than English, provided the target language for other islanders. The result was what Keesing calls "two registers" of pidgin English: one used between some islanders and whites, and one used among islanders themselves. He illustrates that communication between whites and islanders may in fact have been accomplished with whites using grammatical rules of English and islanders using rules of basic EO.

In chapter 8, Keesing lays down the ground rules for determining substrate models for MP: (1) They must have linguistic features found in all dialects, (2) their influence must be possible historically, and (3) their features cannot be merely the same as those of other possible superstrate or substrate influences. Again, the author stresses that general statements about MP should not be made only on the basis of data from Tok Pisin. Then he lists ten grammatical features common to all three dialects in the 1880s. He concludes that the elaboration (or grammatical expansion) of MP is a late-nineteenth-century, rather than a twentieth-century, phenomenon, and that when taken to New Guinea, MP "underwent a considerable withering of its syntactic resources" (p. 115). The chapter continues with more detailed descriptions of the EO sources for features of MP syntax including possession, prepositions, plural marking, and transitive verbal suffixes. It is stressed that simplification toward a more natural or universal pattern has sometimes been involved as well as reinforcement by similar features in the superstrate language, English.

The next two chapters present other elements of EO "core" sentence structure present in MP. Chapter 9 describes the first and most important of these: the pronoun system. It is well known that the semantic categories of the MP pronoun system correspond to those of most Austronesian languages--with dual, and sometimes trial (or paucal), as well as plural pronouns, and with inclusive and exclusive first-person pronouns. Thus, the MP pronoun system clearly shows the influence of

the substrate languages rather than the superstrate or any linguistic universals. Chapter 10 presents detailed arguments to show that what has been called the "predicate marker" in MP (for example in Tok Pisin, the *i* in "*Em i go*") is really a subject-referencing pronoun (SRP), as in EO languages.

The next three chapters deal more specifically with Solomons Pidgin and show how Malaitan languages have influenced its development. Chapter 11 describes "the bending of Queensland Pidgin in a Southeast Solomonic direction" (p. 176) and three directions of change that have distinguished Solomons Pidgin from the other varieties. Chapter 12 returns to a discussion of pronouns, this time showing that "the changes in Solomons Pidgin partly entailed a reanalysis or selection among existing patterns so as to approximate more closely to the patterns of substrate languages" (p. 189). Chapter 13 shows how Solomons Pidgin is actually used in discourse, particularly by speakers of the Kwaio language of central Malaita. It also illustrates with many examples how speakers of EO languages "could calque pervasively and systematically onto their native languages" (p. 210)--in other words, speak pidgin as if it was a word-for-word translation of their own languages.

The final part of the book contains a short conclusion (chapter 14) and some end matter, including an appendix with comparable texts in Kwaio and Solomons Pidgin, the list of references, and a short index. The conclusion consists of three questions. The first is concerned with whether or not there was a group of native speakers of the Pacific pidgin in the 1850s and 1860s and, if so, what their role was in the elaboration and spread of the pidgin. The second question is about how speakers of MP who "calque so closely on their diverse native languages" can communicate with one another (p. 228). The author says that these questions remain unanswered and require further research. The answer to the third question, however, has been the major theme of the book: "How could a pidgin have evolved that, despite its almost total lexification from English as superstrate language, has a structure so close to that of Southeast Solomonic Oceanic languages?" (p. 227).

With regard to this theme, I believe that Keesing is quite successful in illustrating that the Oceanic substrate was influential in the development of Melanesian Pidgin. From his evidence, it seems clear that the basic pronominal systems present in all three dialects, and the "subject-referencing pronouns" in at least the Solomons, are all derived from nearly identical features of Eastern Oceanic languages. I agree wholeheartedly with his conclusion, as would any but the most diehard universalist, that substrate languages, the superstrate language, and

universal properties of human languages and second-language learning all usually play a part in the development of pidgins and creoles.

Keesing's study also seems to reinforce the idea that the more homogeneous the substrate, the greater its influence in pidgin/creole development (Singler 1988). One major problem, however, is pointed out in the book (p. 65) but not dealt with satisfactorily. This is the highly debatable question of including the southern New Hebridean languages in the Eastern Oceanic subgroup. These "less conservative" languages do not have many of the EO features described in this account and the author may be stretching things a bit in trying to show that they do. This is an important factor because in the first twenty years of the Labor Trade (the real formative years of MP, as discussed below) a large proportion of the plantation laborers were from the southern New Hebrides (as demonstrated by the figures given by Keesing [p. 40]).

I find it hard to agree, however, with several of the other major arguments in the book, especially concerning the timeframe for the stabilization of a Pacific pidgin, its separation into the three MP dialects, and the key period of substrate influence. As is common in sociolinguistics, though, some of our differences in opinion may be the result of different interpretations of terminology.

First of all, the term "jargon" usually refers to an individual's imperfect productions of the superstrate language, as defined by Mühlhäusler: "Jargons . . . are individual solutions to the problem of cross-linguistic communication and hence subject to individual strategies, the principal ones being lexicalization or holophrastic talking; pragmatic structuring; grammaticalization by transfer; and universals" (1986: 135-136). The first phase of pidgin development, called the "jargon" stage, is thus characterized by a high degree of variation due to concurrent use of numerous individuals' versions of the superstrate language, the various "jargons." At this stage, however, certain conventions do emerge and, although not used consistently, are found in many individual jargons. Some of these are "salient linguistic features" (Siegel 1987: 15), features that differ from any in the superstrate language and give the impression that all the various jargons make up a distinct variety. The term "pidgin" is usually reserved for the next stage of development, when there is less variation--that is, when "autonomy as a norm" has been achieved (Hymes 1971:84) and when a higher degree of "conventionalization" is displayed (Sankoff 1980: 140).

SO it seems to me, as shown in the following passage, that Keesing may misunderstand Mühlhäusler's use of the term "jargon" to characterize the precursor of Pacific pidgin in the 1850s:

> But I see no strong evidence, linguistic or historical, supporting Mühlhäusler's contention that there were a multitude of different jargons. To be sure, speakers of different Pacific languages brought to an emerging lingua franca different phonological repertoires; and they probably bent the constructions of a developing jargon/pidgin to their own grammatical patterns.
>
> And no doubt local media of interlingual communication incorporated indigenous lexical items and usages. (P. 24)

Also, I'm not sure if Keesing is using the terms "stable" and "stability" in the usual sense. When he applies these terms to certain features of the early Pacific lingua franca, he appears to mean "recognizable" rather than "consistently used." From the data he presents, his definition of stabilization is clearly far from that of Hancock: "the establishment of linguistic conventions . . . whose manifestations will be predictable for *at least* 90 per cent of any speaker's performance" (1980:65).

It is true that certain salient linguistic features had emerged by the 1850s that are still present in MP. These were mainly lexical items, illustrated in the examples such as "savvy," "too much," "plenty," "by and by," and "all same" (pp. 31-32). But even these are not used consistently --for example, compare "too much bad" with "very good' (p. 31). Furthermore, many other items that are not features of MP are found in the examples, such as "that," "see," and "speak."

Keesing also claims that this lingua franca was an "already quite grammatically developed pidgin" by this period (p. 25), but the fragmentary evidence does not confirm this statement. The samples show some constructions that on the surface seem to match grammatical features of current MP. For certain of these, though, only one or two examples can be found, and there is no proof that they are not simply features of English rather than grammatical developments. For instance, there is nothing to prove that "by and by" is not being used only as an adverb, just as in English. And it seems to be stretching things to say that "come worship" illustrates verb serialization and "go and kill every man" shows "go" was used as an auxiliary (p. 32). Similarly, one or two examples such as "steal little thing he no want" do not necessarily illustrate embedded relative clauses rather than merely juxtaposed sentences. In addition, it seems presumptuous to include "belong" for possessives (p. 33) as a grammatical feature of this period on the basis of one example, "man belongen noder place" (p. 22), which other writers have questioned (Clark 1979:22) and which could simply be derived from the English *belonging* (see Crowley 1989). In addition, we again have

many other grammatical features illustrated in the data that are not characteristic of any form of Pacific pidgin: the *'s* possessive marker, the *-ing* verbal suffix, *can't* as a negativizer, and attribute plus noun ("Uea man" rather than "man Uea"). Thus, it certainly does not appear that stabilization had occurred.

Of course, these features just mentioned could be a result of Europeans' inaccurate renditions of the way Pacific Islanders spoke the lingua franca. Keesing repeatedly points out (pp. 32, 33, 41, 101, 120, 141) the problems of interpreting our only source of information about earlier forms of MP--representations given by Europeans, mainly in travelogues and court records. These problems have also been discussed by Clark (1979:23-24), who concludes that basically the data are reliable. Keesing's view, however, is that "almost all observers have heavily anglicized their renderings of pidgin" (p. 41) and thus, in some cases, the data are not reliable. But one of my major criticisms of this book is the inconsistency with which the data are accepted. For example, Keesing notes that "my," which occurs three times in texts on pp. 42 and 43, is "highly suspect" as a genuine feature while, as mentioned above, one occurrence in the pre-1860 literature of what may be "belong" is accepted as hard evidence. Also, at times the author seems to reinterpret the data to better fit his arguments--for example, "want to" as in "want to get" as the present Solomons Pidgin auxiliary *wande* (p. 43) and "make a paper" as *mek-em pepa,* showing the transitive suffix *-em* (p. 125). (See also Crowley 1989 concerning this latter example.) It is also interesting to note that the author says "it would be unwise, when our fragments of recorded speech come from Europeans with a highly imperfect command of the pidgin being spoken by the islanders themselves, to make assumptions about its grammatical impoverishment" (p. 33). Yet earlier in the chapter he uses precisely the same data to make assumptions about its grammatical complexity.

Returning to the topic of the timeframe for the development of MP, I feel that just as there is not enough linguistic evidence to support Keesing's claim that a stable and developed pidgin rather than a jargon was spoken in the Pacific before 1860, there is not enough sociohistorical evidence for the existence of a Pacific-wide "linguistic community" in this period (p. 35). The book contains a great deal of evidence that islanders from all over the Pacific were being exposed to English early in the 1800s, but this does not say anything about the development of a homogeneous linguistic community. The author's descriptions of the trading centers at Pohnpei and Kosrae make an important contribution to the study of the history of Pacific pidgin, and perhaps support the idea of a

Micronesian Pidgin English, as suggested by Wurm (1971a). But again, Keesing provides only one piece of evidence to show that the same islanders who worked in the central Pacific later moved on to the southwest Pacific. Therefore, it is certainly possible that "Sandalwood English" developed separately from Micronesian Pidgin.

In fact, during the 1840s and 1850s, whaling was going on in the central Pacific while the sandalwood and bêche-de-mer trades were proceeding in the New Hebrides, New Caledonia, and the Loyalty Islands. Between 1841 and 1855, for example, approximately 195 voyages were made as part of the triangular sandalwood trade between Port Jackson (Sydney) and the South China coast (Shineberg 1967). Therefore, it is also likely that during this period in the southwest Pacific there were two other important influences on the developing pidgin: Chinese Pidgin English (CPE) and New South Wales Pidgin English (NSWPE). Clark notes that the first attestations of several features of MP are from Australia rather than the Pacific, for example, "belong" used in possessives and "fellow" used to mark adjectives (1979:43). In a more recent study Baker writes, "In the Southwest Pacific, I have suggested that NSWPE was the most important influence on the way in which varieties of Pidgin English developed in the islands prior to the start of the labor trade" (1987: 199). He also shows that the transitive suffix *-im,* which Keesing says developed in the Pacific due to EO substrate influence, is actually attested first in New South Wales in 1816 and in German New Guinea in 1834. Other salient linguistic features of the early jargon stage, such as "too much," "all same," and "what for," are also attested first in New South Wales in the 1820s. These are also features of Chinese Pidgin English. So it appears that from the start both CPE and NSWPE have had an influence on Pacific pidgin and that the influence was especially significant in the southwest Pacific.

Thus, with at least three different varieties of pidgin English in contact, Keesing's idea of a homogeneous Pacific pidgin before 1860 appears unfounded. Consequently, his ideas about the early nativization of a Pacific pidgin (p. 14) and "incipient creolization" before the 1860s in such a community in Micronesia (p. 21) and the Loyalty Islands (p. 33) seem highly speculative, especially considering the lack of sociohistorical evidence to support these ideas. Also, I have shown that the linguistic evidence used by the author (the apparently well-developed grammatical features of this period) may not be really acceptable.

As a result, I feel Keesing is unsuccessful in proving that a homogeneous, stable, and well-developed pidgin was used when the Pacific Labor Trade began in early 1860s. It seems rather that it was a still

unstable, "developing" contact language, influenced not only by the first languages of its speakers but also by at least two more-established pidgins. The usually accepted view (e.g., Clark 1979) then stands: Development into a stable Early Melanesian Pidgin occurred at the start of the plantation era--from the early to mid 1860s (and not from the 1870s, the date given by Keesing [p. 13])--when recruited Pacific Islands laborers began working on European-owned plantations in Queensland, Samoa, Fiji, and New Caledonia.

Keesing gives sixteen "essential syntactic and lexical/semantic patterns of Melanesian Pidgin" (pp. 48-50), represented in texts from the 1870s and 1880s (although some come from the late 1860s). Again, several are based on either flimsy evidence or broad interpretations of the data, as shown below (with Keesing's numbering) :

2. Relative clauses. Only one other example is given in addition to the dubious one mentioned above. It too may simply be juxtaposed sentences: "That big fellow wind, man Sandwich make him; he broke ship" (p. 42). But note that when it is discussed (p. 48), the punctuation from the original example is omitted, making it look more like one sentence.

4. *Wande* as a modal. Two examples of "want" plus verb and three of "want to" plus verb are all interpreted as *wande* plus verb.

10. Use of *ol* as plural marker. In all the examples given with the word "all," it could be used literally to mean "all" rather than showing the plural, for example, "Me think all the boy want to kill me" (p. 45).

15. The use of "say" *(se)* as a complementizer. There is only one example, "He say, canoe come. . . . He say, long time before he no kaikai man" (p. 42). Here "say" could be used as a main verb.

Keesing, however, does give some solid evidence that certain structures were in use earlier than has previously been suspected, for example, "fellow" as a suffix in regular grammatical slots (p. 49).

So it seems that many, but certainly not all, of the grammatical features of modern MP emerged during the first decades of the plantation era. This brings us to another of Keesing's themes: that the essential linguistic features of MP were in place and used consistently before the divergence into separate dialects. First, I have just shown that not all features were in place. Second, it is clear that, while many features can be identified in the data, they may not have been used consistently by all speakers and there was still a good deal of variation. For example, in Keesing's data, we see "like" being used as well as "want (to)": "he like spik you" (p. 42). We also find several transitive verbs used without the *-im* suffix: "man Sandwich make big wind" (p. 42) and "he bin give me

small fellow boks" (p. 43). There is also the use of pronouns not found in MP, such as "I," "we," and "they," mentioned by the author himself (p. 49).

On the topic of the separation of Tok Pisin from the other two MP dialects, I feel some of Keesing's criticisms of Mühlhäusler's position are largely unjustified. First of all, nowhere do I find Mühlhäusler saying that Samoan Plantation Pidgin (SPP) had "a substantially separate linguistic history from 1870 onward" (p. 51). The main purpose of his work on SPP (1978, 1979) was to show that the forerunner of Tok Pisin was brought back by returned New Guinean laborers not from Queensland plantations, as was previously believed, but from Samoa. From 1879 to 1912 about six thousand laborers went from German New Guinea to work in Samoa, whereas perhaps less than a hundred went to Queensland (Mühlhäusler 1978:69, 79). Laborers from the New Hebrides and the Solomons also worked in Samoa, but only from 1878 to 1885. Thus, as Clark points out, "New Guinea's connection with the Melanesian pidgin network thus lasted no more than seven years" (1979:39-40, quoted by Keesing on p. 52). These two important facts help explain why the New Guinea dialect of MP differs from the other two more closely related ones spoken in the New Hebrides and the Solomons.

By giving the label Samoan Plantation Pidgin, Mühlhäusler is not saying that it was a separate language from Queensland Plantation Pidgin, as Keesing implies (p. 54). In fact, Mühlhäusler agrees with Keesing about there being one early Melanesian Pidgin language in the 1880s and the reasons for this: "Because of a number of factors, including common recruiting grounds for most Pacific plantations and a number of linguistic conventions that had emerged in Pacific Jargon English, this early form of SPP did not differ greatly from the plantations pidgins found in Queensland or New Caledonia" (Mühlhäusler 1978:81).

Where the two scholars differ is that Keesing says that "Samoan Plantation Pidgin was essentially the same dialect as the pidgin of the Queensland/Fiji/New Caledonia Labor Trade" (p. SS), while Mühlhäusler says it was a different "variety." Again, what we may have here is a terminological muddle. According to most linguists, separate varieties or dialects of the same language share most of their grammatical features but are distinguished by a few phonological, lexical, and minor grammatical differences. Thus, one wonders what point Keesing is making when he says: "Scant wonder, then, that the dialect of pidgin Mühlhäusler characterizes as Samoan Plantation Pidgin was essentially

identical (judging by the limited linguistic evidence available) to the pidgin being spoken in Queensland in, say, 1890. No doubt there were Samoan-derived lexical items, just as there was a French lexical component to the pidgin spoken in New Caledonia" (p. 57).

On the other hand, Keesing does make a good point about Mühlhäusler's discussions of structural expansion in Tok Pisin (1980, 1981, 1985b). The dates Mühlhäusler gives for the stages of development of Tok Pisin are pre-1880 for the jargon stage and 1880-1920 for the stabilization stage. As Keesing notes (p. 52), the simplicity of grammar at the jargon stage would rule out the development before 1880 of most of the grammatical constructions described by Mühlhäusler for SPP that later came into Tok Pisin. Clearly, though, many of these constructions were attested for other varieties of early MP before 1880. Thus, Keesing says the only way Mühlhäusler could explain the presence of the same features in all three dialects would be by separate parallel development. However, I think that Mühlhäusler simply has his dates wrong here and, in fact, earlier he says in a discussion of Samoa between 1867 and 1879 that "a relatively stable form of pidgin had emerged during this period" (1978:81). (In a more recent work he gives the dates for stabilization as 1860-1883 [Mühlhäusler 1985a:39]).

This brings us back to the issue of the key period of substrate influence in the development of MP, and the major substrate influences. As already noted, Keesing maintains that the essential grammatical features of MP are derived from EO languages and are found in all three dialects. Thus, in setting forth his "ground rules," Keesing restricts possible candidates for substrate influence to languages that could have influenced all three dialects and restricts the period of influence to before the laborers from New Guinea, the New Hebrides, and the Solomons took early MP back to their home islands. Since Keesing's argument is that the main features of MP are due to EO substrate influence, these ground rules eliminate the non-EO New Guinean languages, which could have influenced only Tok Pisin, and set the key period of substrate influence clearly during the stabilization stage or before.

Keesing also maintains that the grammatical expansion of MP, previously described for Tok Pisin by linguists such as Mühlhäusler and Sankoff and sometimes attributed to substrate influence, actually occurred not in New Guinea but in Queensland and Samoa during the plantation era. According to the author, when the "extensively elaborated pidgin" was transplanted to "alien linguistic soil"--that is, to the non-EO New Guinea islands--some of these features "withered" (p. 115). By implication, when transplanted to the EO areas of the New

Hebrides and the Solomons, they thrived. Therefore, during the period usually thought of as the expansion stage, the substrate languages are seen as providing either negative or positive reinforcement for already existing, well-developed features.

I have two criticisms of this point of view. First, there seems to be no reason to eliminate the possibility of substrate influence of New Guinea languages during the stabilization stage in the plantations. Large numbers of New Guineans began to work in Samoa in the early 1880s and, since MP was still at a formative stage before 1885, their substrate languages could have had an influence on the developing pidgin. Because New Guineans worked alongside New Hebrideans and Solomon Islanders and because of the networks between Samoa and Queensland, described by Keesing, I don't see why these influences could not have affected MP in general.

Second, once again it is not clear that all the basic grammatical features found in modern dialects of MP were firmly in place as early as Keesing maintains. True, some evidence of their existence is found in the data, but there was still a great deal of variation. Keesing's list of syntactic features common to all three dialects in place by the late 1880s (pp. 112-113) contains features that were not used systematically at that time, as already shown, such as the pronouns, the *-im* transitive suffix, and the *se* (from "say") complementizer. Some features listed are also used in different ways in the different modern dialects. For example, Crowley (1989) shows that Bislama differs from other dialects in which transitive verbs take the *-im* suffix. Also, although the three dialects use the suffix *-fela* (from "fellow") in some of the ways described by Keesing, there are major differences. For instance, its use in demonstratives (e.g., *dis-fela* 'this') is not found in Bislama and while the suffix is obligatory on certain adjectives in Tok Pisin (e.g., *gut-pela* 'good') and on quantifiers (e.g., *tu-pela* 'two'), it is optional in Bislama. And Keesing himself shows how the "predicate marker" *i* is used differently in the three dialects.

When Keesing says throughout the book that Tok Pisin is considered the "canonical" dialect of MP, he is being critical of what he sees as people making generalizations about MP based only on data from one dialect. Yet it appears to me that he similarly may be making Solomons Pidgin the "canonical" dialect for his arguments. His descriptions of the important MP features he is focusing on (transitive suffixes on verbs and prepositions, "subject-referencing pronouns," etc.) are based on data from modern Solomons Pidgin, which has most obviously been influenced by EO languages. In fact, he even breaks his own "ground rules"

by discussing "prepositional verbs" (p. 122), present in Solomons Pidgin and Bislama but not in Tok Pisin. This leads to his assumption that such features were present in earlier versions of all three dialects and the ones that are not present must have been lost or later modified by the substrate languages.

What I see is a slightly different scenario in the development of the modern MP dialects, one that would better explain differences between the individual dialects as well as the similarities. During the plantation era, the early MP spoken by the laborers was still highly variable. It did contain many characteristic features of later MP, some clearly resulting from substrate influence (both EO and non-EO), others from superstrate influence (Standard English), and still others from what may be called universals of human language or second-language acquisition. It also contained many features not found in later MP, such as the pronouns "I," "my," and "we," the use of "fellow" as a subject and with "him," and others that Keesing explains away as recording errors or filtering through English. This pool of features was taken back to the laborers' different islands. There, under the influence of substrate languages that were more homogeneous, and out from under the influence of the superstrate language, the use of some of these features died out while the use of others was reinforced. Some were also reanalyzed or "bent" according to substrate patterns, as Keesing himself describes for the Solomons and New Hebrides.

Some strong evidence of such a scenario is given in Crowley (1990) for the development of the prepositions in Bislama. Through the records of testimony in an official inquiry held in Queensland in 1882, he shows that along with the typical MP prepositions "along," "all same," and "belong," others such as "on," "in," "at," "with," "of," "for," "from," "without," "through," "alongside," and "like" were also used as in Standard English a large percentage of the time. Crowley demonstrates that the surface forms of the five basic prepositions of current Bislama are derived from the English "along," "belong," "from," "with," and "all same," but that their semantic roles and grammatical behavior are very different from English, matching patterns in the substrate languages, specifically in Paamese.

Of course, if there are similarities between the groups of substrate languages, certain features may similarly be reinforced. This would account for the nearly identical pronominal systems in all three dialects, which all developed in areas where Austronesian languages with the same system are spoken. Also, similarities in pre-verbal tense and aspect marking in the languages of the three areas led to similarities in the MP

dialects. However, there are some features not found in all the substrate language areas that are found in all three dialects. How can these be explained if we disagree with Keesing's claim that these features were already fully elaborated and systematic in early MP? I think the answer is that although these structures were not well developed in early MP, the "seeds" of these structures were planted at the time. In other words, certain embryonic structures had emerged in early MP that then grew and developed in each of the three environments into structures looking quite similar in their mature states. This would account for the parallel developments that Keesing says would have been unlikely.

Along these lines, Mühlhäusler (1981) has described the developments of the expansion stage (that is, for MP, post-separation into the three dialects) as being mainly a continuation of those started at the preceding stabilization stage (that is, pre-separation). He gives the example of the development of the *ol* plural marker in Tok Pisin (putting *ol* before nouns to show the plural, as in *ol dok* 'dogs'). This is not the way the plural is marked in the New Guinea substrate languages, and Mühlhäusler attributes the development instead to natural internal growth or "universal principles governing expansion" (see Romaine 1988: 134). However, Keesing shows convincingly that the origin of this type of plural marking could have again been EO languages (pp. 128-129) and that there is no need to resort to universals to explain it. But again he is talking about the origin of the "seed' planted in early MP. It appears from the data that this method of plural marking was not well developed or systematized in the 1880s or until much later in any of the three dialects. We can easily see why this feature grew to maturity in the New Hebrides and Solomons, within the supportive environment of the substrate languages with a similar feature. And we may want to call on the universal principles mentioned by Mühlhäusler to explain not the origin of this feature but why it also grew and developed in New Guinea without such a supportive substrate environment.

For me, this book does not only stir up some controversy, provide valuable information on Solomons Pidgin, and make us relook at the history of MP. It also indirectly reconfirms two misgivings I have had about accepted notions of the development of pidgins in general. First, if we go by the usually accepted stages of pidgin/creole development--jargon, stable pidgin, expanded pidgin, creole--it is difficult to say at which stage substrate influence is most likely to occur. Mühlhäusler says that transfer from the speaker's first language is unlikely in the jargon stage and that substrate influence is more likely to occur in the stabilization and especially the expansion stages (1980, 1985b, 1986). It is often

not clear what stage Keesing has in mind in his discussion of substrate influence in MP. In some parts of the book it appears that he is talking mainly about the influence of EO in initial stages (before 1860), but he is also claiming that stabilization had occurred by then. He moves on to discuss the substrate influence of the languages in the southwest Pacific in further stabilization during the Labor Trade. But later, in setting forth his "ground rules," he restricts possible candidates for substrate influence to those that could have historically influenced all three dialects. This implies that substrate influence is not relevant in what is usually considered the expansion stage in MP--when laborers took early MP from the plantations back to their home islands. But the final four chapters of the book are devoted to showing how, in the development of the Solomons dialect, features of MP have been "reanalyzed' and bent to patterns of speakers' native languages, especially those of Malaita.

It seems to me that substrate influence can occur at any stage of development, except perhaps for the jargon stage for the reasons given by Mühlhäusler (1985b: 77). The importance of transfer of features from the first language in second-language acquisition is not so great as originally thought, but still significant. As Keesing has shown, at nearly all stages of development some Pacific Islanders were attempting to learn not English but a form of Pacific pidgin as a second language. Thus, we could expect some substrate influence at these stages. It is also clear that when a pidgin is learned by a new group of people with different substrate languages, it is affected by these languages. Some examples are the loss of the predicate marker *i* in Tok Pisin spoken by New Guinea Highlanders (Wurm 1971b:13-17) and the change in word order to possessor preceding possessed and to SOV in Pidgin Fijian spoken by Fiji Indians (Siegel 1987:242, 246). I see no reason why a fully developed pidgin should behave any differently from other languages with regard to substrate influence. For example, there is a wealth of recent information on how substrate languages (as well as universals of second-language acquisition) have affected English transplanted to India, Singapore, the Philippines, and other former British and American colonies so that "New Englishes" have emerged (Platt, Weber, and Ho 1984).

The second misgiving concerns the stages of pidgin development themselves. As I have pointed out, the data given in this book show a great deal of variation, even after so-called stabilization is supposed to have occurred. Certainly, in the data from the 1880s, after "stabilization" on the plantations, there is nothing near the 90 percent consistency mentioned by Hancock (1980). It seems stabilization is a continuing process, beginning with the emergence of the recognized salient

linguistic features of the jargon stage, increasing dramatically during what is now called the "stable" pidgin stage, and continuing to an even greater degree during the expansion stage. It may be only after expansion, then, that the 90 percent mark is approached. The same is true for expansion; it also seems to be a continuing process that starts earlier than the "expansion" stage and continues on in creolization. Perhaps pidgin/creole studies would be better off talking not about distinct stages of development but rather about different developmental continua, such as stabilization, grammatical expansion, functional expansion, and nativization. And perhaps clarification of these terms would prevent disagreements between linguists on how they are applied to the data.

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This book undisputably shows the crucial importance of the substrate in the formation of diverging pidgins, from the earliest jargon stage till now. It makes a valuable contribution both to the general theory of

pidgin/creole formation and to the history of English-based pidgins of the South Pacific. Its concentration on Solomons Pidgin fills many gaps in sociohistorical and synchronic grammatical descriptions of this language. Since the book is quite consistent with my own ideas on the subject, I will not challenge any of its main theses but simply make some minor suggestions.

The skeleton of the historical scenario for the formation of Pacific pidgins is the same among all the authorities: From the early nineteenth century on in many areas of the Pacific, a lingua franca based on a kind of nautical English has been used. Its incipient formation dates back to the sandalwood period and the language(s) was (were) enriched on sugar plantations beginning in the late 1860s, gradually became the main means of interethnic communication in various parts of Melanesia, then undoubtedly diverged at this stage; the process of creolization, mainly in an urban context, slowly began only in the last decades.

The crucial points of disagreement among researchers are as follows:

1. Was it generally the same idiom throughout the South Pacific or was it a set of idioms emerging rather independently at different points? In other words, who was responsible for its incipient formation--diverse Oceanic peoples or sailors?

2. When did this idiom (or each of the idioms) turn from the jargon stage into a pidgin, reaching stability in lexicon and grammatical devices?

3. When did it (or they) gain enough vocabulary to serve as the means of everyday communication?

4. What is the ratio and correlation between different sources of the grammatical repertory of modern pidgins: substrate languages, English, and universal tendencies?

All these questions are interconnected.

Before Keesing's book, the prevailing scenario in the field was that of P. Mühlhäusler, summarized in his *Pidgin and Creole Linguistics* (1986). Leaving aside many details, the scenario can be shortly represented in the following way. Before the plantation era there was a set of loosely connected, unstable jargons in Melanesia; based on these jargons two pidgins formed, one in the canefields of Queensland, the other in Samoa. The former is a predecessor of modern Solomons Pidgin and Bislama (which were lexically anglicized during the expansion stage). The latter finally resulted in Tok Pisin, which became a separate stabilized and expanded entity enriched by a Tolai substrate and a German (later English) superstrate.

Keesing effectively challenges many points of this scenario. I think that even half of the documentary evidence given in the book would be

enough to support his concept of the emergence of a rather stable pidgin aboard ships during the first half of the nineteenth century. According to Keesing, in the early sandalwood period "the key area for the development of a Pacific lingua franca" (p. 9) was the central Pacific: the eastern Carolines, the Gilberts, Rotuma, and Fiji. This territory was frequented by European ships and many inhabitants became seamen. In 1840s the main center of pidgin formation transferred into southern Melanesia (p. 27): Keesing puts special emphasis on the role of Loyalties' natives.[1] The language became fully developed during the first years of the Labor Trade period. "By the first half of the 1880s, and in many cases well before 1880, the essential patterns of Melanesian Pidgin --syntactic, semantic, and lexical--were thus well established" (p. 50). At this period it already began to be used as a lingua franca by the islanders returning from the sugar plantations (p. 44).

At the beginning of Melanesian Pidgin's formation the majority of its speakers were natives of the central Pacific. Keesing gives a lot of thoroughly documented evidence of substrate influence via morpheme-by-morpheme correspondences. It was not Europeans but Pacific Islanders who were the most fluent speakers of the language--even in the middle of the nineteenth century many of them knew it from their childhood (p. 14) and they were responsible for its standardization and dissemination. The pidgin of the Pacific Islanders was a target variant for the Europeans. These two groups of pidgin speakers interpreted the same surface strings differently (pp. 100-101).[2] The stigmata of the inadequate European filter greatly degrades the quality of written representations of the language, leading, in particular, to the usual underestimation of its stability and grammatical richness (pp. 100-101, 149).

This scenario looks quite appealing but has, to my mind, weak points.

First, during the sandalwood period Polynesians, especially Hawaiians, were by no means less numerous among the seamen than Micronesians, Fijians, and Rotumans. In the early 1840s a thousand natives left the Hawaiian Islands each year (Simpson 1847:15); in the 1850s the figure was not less than five hundred per year (Day 1955:134). The number of the Hawaiians scattered in the Pacific ports and on the ships can be estimated for the period as three to five thousand. Some early jargon examples from Hawaii, Tahiti, the Marquesas, and New Zealand can be found in Clark 1979. Eastern Polynesian languages have no morphologically marked transitives, no preposed subject pronouns, and the standard word order is VSO. Some Polynesian vocabulary items have a universal distribution in the Melanesian pidgins (e.g., *kanaka* is undoubtedly of Hawaiian origin). So why did not Polynesians "bend' the

grammatical structure in the direction of their own substrate languages?

Second, according to Keesing, during the late sandalwood period the "pidgin became rich enough lexically and syntactically to serve as a primary language of daily communication in the 'communities,' mainly shipboard settings, where it was used, in what may have been a phase of at least incipient and partial nativization" (p. 94).

There is no doubt that the ship variant of South Seas Jargon quickly became grammatically enriched and lexically expanded in some semantic fields, but it could not be stabilized. Each ship "community" was small and mixed, with a large proportion of fluent English speakers. I think that in this situation the only imaginable interpretation would be a "post-jargon continuum" with newcomers as basilectal speakers, each bending the jargon to his own substrate language, and with an acrolect approaching substandard English. Keesing himself gives evidence for this supposition (pp. 41, 95, 151): Some acrolectal speakers could read and write English (p. 34). It is hard to believe that sailors with Oceanic background used a kind of "foreigner talk' to communicate with their colleagues of European origin (p. 212). Even in case it were true, they should do more the same interacting with the Pacific Islanders on the shore.

The Pacific pidgin of the nineteenth century was a second language to practically everybody. Its idiolects can be grouped, to my mind, into sociolects functionally similar to those of modern Pacific pidgins (bush pidgin, rural pidgin, urban pidgin, Tok Masta); independently of the previous classification the same idiolects can be grouped into classes with the same substrate. These substrate classes can be arranged according to typological similarity of the substrate languages. The whole story was complicated by a quick and individual evolution of each idiolect.

The supposition that at the end of 1880s "there was no room or need . . . to expand its [pidgin's] syntactic possibilities" (p. 39) seems too optimistic. Keesing's own story of the constant bending of Solomons Pidgin to the direction of the Kwaian substrate suggests that such "room" exists even now, especially taking into account the additional substrate bending of "standard" Solomon Islands Pidgin by the western islanders, described in one of his later papers (1988).

A neatly connected question is the lexical richness of the pidgin before separation of the Tok Pisin lineage. I have not found in the book the direct evidence for the claim that the Vanuatu-Solomons variant is lexically more archaic and the "historically aberrant New Guinea offshoot" (p. 61) was relexified by Tolai.

Unquestionably Tok Pisin was "Tolai-ized" and "Bismarck-ized"--but was it a *re*lexification? The attempt to measure the degree of lexical similarity between modern Melanesian pidgins with the standard instrument of lexicostatistics gives unexpectedly low results (Belikov 1987, 1988). The main reason is the absence of some items of the Swadesh list in the "protopidgin." It is but natural that some semantic fields are poorly represented in a language of this kind.

The only fundamental attempt to reconstruct the lexicon of the nineteenth-century pidgin is that of Clark (1988). According to Clark, "five hundred words would have been a bare minimum vocabulary for a competent speaker of Early Melanesian Pidgin circa 1880 . . . , a lexicon of a thousand words would not have been uncommon" (1988:8). Previous estimations have been much lower, even for a later period. Mühlhäusler, for example, gives a figure of about three hundred words for Samoan Plantation Pidgin (1983:51). The list of some six hundred vocabulary items appended to Clark's paper does not confirm the thesis of the later relexification in Tok Pisin.

Sometimes Tok Pisin has a circumlocution corresponding to an English-derived item in Bislama and Solomons Pidgin (cf. *sit bilong paia* vs. *asis* 'ashes', *skru bilong lek* vs. *ni* 'knee'). In some cases two or more specialized words in Bislama and Solomons Pidgin correspond to one general term in Tok Pisin (cf. *rip* vs. *rif* 'reef and *korel* 'coral'; *kaikai* vs. *kaikai* 'meal', *dina* 'dinner', and *sapa* 'supper'; *papa* vs. *papa* 'father' and *angkel* 'uncle'). The easiest way to qualify these cases is to consider the Tok Pisin variant a retention; hence the counterpart would be an English borrowing.

Sometimes it is not clear whether the semantic item was present in the "protolanguage" (cf. *abus* vs. *mit* 'meat',[3] *meme* vs. *nani* 'goat'). Some of these words, but not many, of course replaced previously existing words of English origin.

The final category--not numerous but significant--contains words definitely archaic in Tok Pisin and not used in the other pidgins. For example, *pato* 'duck', unmistakably of Ibero-Romance origin, also is attested in the Samoan *pato* borrowed from Pidgin (there were no contacts with Spaniards or Portuguese), so Bislama and Solomons Pidgin *dakdak* should be considered as an innovative loan after the separation of Tok Pisin.

One more point should be mentioned about calquing the most characteristic substrate features.[4] "In SIP [Solomons Pidgin], as in Melanesian languages, reduplicating a verb . . . implies continuation or repetition of an action" (Keesing n.d.:20). Vanuatu Bislama has the same

feature. This salient morphological device is not mentioned in the book under review. Was it absent in the pidgin or plantation period? If so, why did it emerge later on? Did the substrate influence become more intense?

Finally, I should like to make a metalinguistic complaint about the instability of the terminology. In some cases it can be a real obstacle to understanding one other. In Keesing's conclusion he puts the question that "allows of no easy answer" (p. 227): How can syntactic complexity of Solomons Pidgin "be reconciled with the view linguistic theorists have consistently taken of pidgins as radically simplified and syntactically limited?" The question is not in reconciliation but in unification of terminology. Pidgin in Bicker-ton's (1981) sense is a mere jargon in Mühlhäusler's (1986) sense! The existence of regional dialects in Bislama is a sign of its instability for Mühlhäusler (1986: 19) but not for Keesing, I suppose. We should be accurate in labeling natural phenomena and in understanding each other's labels. This will solve some problems.

It is always a pleasure to conclude a review with the statement that the only vexing points of the book are misprints. I have found only two: The work of J. Chignell mentioned on p. 165 is missing from the bibliography and the citation of J. Charpentier (1979:310) on p. 161 should be read "it is not common to say *olketa i* + verb."

NOTES

1. The number of languages in the Loyalties is only four, not a dozen (p. 33).

2. This is equally true for many other pidgins; in Russenorsk, speakers of Russian and Norwegian ascribe different deep structures to the similar surface strings (Belikov 1989).

3. Animals were not numerous in Oceanic context. Some Oceanic languages use a general term "fish" for both fish and animal flesh. Newly introduced European animals had special labels, which could be used for different types of meat.

4. The author gives sufficient examples to demonstrate the typological similarity of the Oceanic languages on the points under discussion. So his appeal to Proto-Oceanic is not necessary. Moreover, methodologically it is a weak argument: The typology of a protolanguage often has nothing to do with that of similar modern languages.

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Review: DEREK BICKERTON
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Was it Don Laycock or Chris Corne who once complained bitterly about the " 'Podean bias" in pidgin and creole studies? Whoever it was, they will have less cause for complaint following the appearance of this handsome volume, which goes a long way toward redressing the balance between the northern and the southern hemispheres in that field.

Keesing is an anthropologist whose modesty about his linguistic attainments is uncalled-for: He may not be up on the latest jargon, but

his account of linguistic structures in Oceanic and pidgin/creole languages is admirably thorough, clear, and convincing. His work represents a fascinating detective story that reveals, with a wealth of detail, the growth and development of Melanesian Pidgin (MP) from its earliest stages.

For linguists directly involved in the study of MP, a major interest of the volume will be Keesing's stance on the relationship between Tok Pisin and other MP varieties. Briefly, he claims that Melanesian Pidgin was developed during the early to middle years of the nineteenth century mainly by Pacific Islanders who worked on sailing ships as members of English-speaking crews. Most of these sailors were from central Pacific islands where nowadays no form of pidgin is spoken, for example, Pohnpei, Kosrae, Mokil, Rotuma, and the Gilbert Islands. Accordingly, the nascent pidgin was strongly influenced by the languages of those islands, which fall into the group referred to by Pawley (1977) as "Remote Oceanic." Keesing's first few chapters trace these early contacts in considerable detail and contain much that should be of interest to historians of the Pacific, as well as to anyone who is interested in the relationships among nineteenth-century Europeans, Micronesians, and Polynesians.

Keesing believes that this mid-Pacific pidgin stabilized during the second half of the century and spread to New Guinea, the New Hebrides, and the Solomon Islands as well as to the plantations of Queensland and Samoa, thus serving as the ancestor of all the pidgin varieties subsequently spoken in Melanesia. His thesis thus stands in direct opposition to that proposed by Mühlhäusler (1976, 1978), which claims that Tok Pisin evolved on Samoan and Australian plantations and hence is of a different lineage to the other pidgins of Melanesia.

Keesing establishes very clearly, with a range of data extending widely over time and space, his claim that MP had stabilized and (to some extent) complexified by the last quarter of the nineteenth century, and that many constructions that were already characteristic of it (for instance, what he refers to as "subject-referencing pronouns," perhaps better regarded as subject-agreement markers, and "transitive suffixes") were later adopted both by Tok Pisin and by the pidgins of the Solomons and Vanuatu. As he points out, any alternative would have to hypothesize convergent evolution in all three places, a development the improbability of which casts strong doubts on Mühlhäusler's scenario. Keesing also provides evidence for supposing that MP was able to stabilize and complexify more rapidly than most recorded pidgins because its original substrate was highly homogeneous.

The only weak link in this argument is the paucity of Keesing's citations from the Remote Oceanic languages that are supposed to have formed the original substrate of MP, as opposed to the indigenous languages of the areas in which varieties of MP are currently spoken. For instance, no Gilbertese or Rotuman sentences are cited, while there is only one from Mokilese and four from Pohnpeian; however, Kwaio, a language of the Solomon Islands in which Keesing is fluent, is cited constantly, and we are periodically reassured that the Remote Oceanic languages pattern in a similar way to it. To clinch his argument, he should have paralleled his citations of mid and late nineteenth-century pidgin with citations of similar structures in the indigenous languages of those islands in which he claims that MP originally developed. I leave to experts in the field of Oceanic languages the task of determining whether the grammatical structures of Remote Oceanic languages are as similar to those of the indigenous languages of the Solomons and Vanuatu as Keesing claims.

The remainder of Keesing's work consists in explaining the data that gave rise to the Mühlhäusler position: the differences that nowadays exist between varieties of MP in Papua New Guinea, the Solomons, and Vanuatu. Keesing claims that a single original MP developed distinct varieties through the influence of idiosyncratic features drawn from indigenous languages in the three regions concerned. Naturally, given his own research interests and experience, this process is most thoroughly documented where it deals with the Solomon Islands variety (but see also Camden 1979 for a similar operation on a New Hebridean variety). Again, the argument might have benefited by some direct comparisons between the three varieties, but one mustn't expect too much: Comparative MP studies is just one of the new research fields that Keesing's work both suggests and provides initial data for.

It is inevitable that this book, touching as it does on important issues of language contact and language genesis, will have an impact that extends beyond the field of Pacific studies, and will be invoked in a number of ongoing controversies surrounding those issues. It seems desirable, therefore, to discuss at least two such aspects of Keesing's work: its relation to substratum theory (which claims that the grammatical structures of creole languages are derived from the languages spoken by the parents of the original creole speakers) and its relation to the origins of Hawaiian Pidgin/Creole.

Keesing himself wisely refrains from any attempt to extrapolate from MP studies to studies of pidgin and creole languages generally. Others, however, will be less cautious (see, already, Mufwene 1989). If one

pidgin/creole can be shown to have developed by relexifying substrate structures (that is, by keeping the grammars of the speakers' original languages intact but substituting an alternative, in this case an English, vocabulary), then perhaps all other pidgins and creoles can be shown to have developed in a similar way. Moreover, since the major, "universalist" alternative to this theory (that creole languages more or less directly reflect a biologically determined language-creating capacity: see Bickerton 1981, 1984) is still ideologically repugnant to many, it may seem a tantalizingly easy step from "could have" to "must have."

As Keesing's work clearly shows, however, the circumstances under which MP emerged were vastly different from those that produced the plantation creoles. In the Pacific, pidgin was built by several generations of adult speakers; Keesing notes that the process began in the latter part of the eighteenth century and that the pidgin was not fully stabilized until the 1880s. Throughout this period, and indeed until much later in most cases, pidgin speakers retained their ancestral languages, permitting a transfer of features between substrate and pidgin. Moreover, the extreme homogeneity of that substrate reinforced a set of shared patterns.

On plantations in the Caribbean and elsewhere the story was very different. Almost everywhere the substratum languages were much less homogeneous in structure and seldom persisted beyond the first generation. Long before there was time for a stable pidgin to develop, children somehow managed to generate languages of their own--languages that share with one another a wide variety of structures but conspicuously lack most of the structures characteristic of MP. In other words, the linguistic and sociolingustic circumstances surrounding the birth of MP differed radically from those surrounding the birth of the plantation creoles. Accordingly, it is at best highly unlikely that identical language-forming processes could have operated in the two cases.

Nothing, perhaps, shows this more clearly than Keesing's contribution to the second issue: the origins of Hawaiian Pidgin/Creole. The earliest pages of his book might seem to offer support for the thesis of Goodman (1985), Holm (1986), and others that Hawaiian Pidgin derived from some external model that spread across the Pacific *and* the Atlantic: When Keesing points out that Hawaiian sailors were probably among the first speakers of Pacific pidgin, one can almost feel the hot breath of diffusionists on the back of one's neck. Alas for them, he subsequently provides an inventory of the "syntactic and semantic/lexical patterns of [MP] . . . represented in the texts from the 1870s and 1880s (and the earlier texts we have seen)" (pp. 48-50). Of the sixteen patterns

he lists, only two were fully shared by Hawaiian Pidgin (and one of these, svo order, is shared by pidgins and creoles generally) while three semantic/lexical patterns were partially shared. The remaining eleven patterns seem to have played no part in the development of Hawaiian Pidgin--at least, they are not to be found either among the few surviving pidgin speakers or among speakers of creole varieties.

In other words, even where their speakers may have been in partial contact, plantation pidgins and maritime pidgins remained two quite separate ball games. Nor should this come as a surprise: While Hawaiians worked on English-speaking ships and Hawaiians worked on sugar plantations, there is no evidence that these were the *same* Hawaiians, and good reason for supposing--since men used to the variety of a sailor's life would be unlikely to accept the monotony of sugar cultivation--that the two populations overlapped little, if at all. Hawaiian Pidgin's few lexical similarities with Pacific pidgins--*sapos, baimbai, save,* and so on--may have come via Pacific Islanders employed on Hawaiian plantations prior to 1876, but these islanders were too few and left too early to have had any effects on the *structures* of Hawaiian Pidgin or Hawaiian Creole.

Only a synopsis of Keesing's book could be used to support a general substratist or diffusionist position. The text itself, admirably balanced and thorough, affords no such comfort. Keesing is concerned simply to chronicle a process that, as he himself implicitly recognizes, may have been unique in linguistic history, and, unlike some other scholars, he does not attempt to make his findings carry more theoretical weight than they will readily bear. If the book has a defect, it is the complete absence of maps: Even those familiar with the Pacific will find it by no means easy to follow the tangled trail Keesing pursues in his hunt for origins. If this book enters a second edition, as it surely should, this deficiency should be removed.

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By his own admission Keesing writes as "an amateur linguist who is an equally amateur historian" (p. vi). There seem to be at least two different arguments in *Melanesian Pidgin and the Oceanic Substrate.* One concerns the history of the three varieties of Melanesian Pidgin and relationships among them. The other deals with substratum influence in Solomon Islands Pijin, and by implication the role of substratum in pidginogenesis in the Pacific. This is an important book, particularly given the centrality of data from Solomons Pijin to Keesing's arguments. Once Crowley's forthcoming study of Bislama appears (1990), we will have solid studies of the three major varieties of Melanesian Pidgin. I will concentrate my remarks here on the issues of substratum, stabilization, and grammaticalization and I will show how they are interrelated by analyzing one particular grammatical feature that plays a large role in Keesing's argument.

Keesing claims that many of the most important developments in the expansion and stabilization of Melanesian Pidgin took place in the central Pacific prior to separation into regional dialects in Melanesia (p. 3). In particular he argues that Mühlhäusler overestimates the separateness of New Guinea pidgin English and that only after 1880, when Melane-

sian Pidgin had stabilized, did the New Guinea variety develop into the distinctive variety known today as Tok Pisin. According to Keesing, stabilization took place on ships rather than on plantations, Loyalty Islanders played a crucial role in spreading this pidgin, and the distinctive developments that characterized Tok Pisin came about through drastic relexification, which involved the replacement of English forms with forms from indigenous languages, particularly Tolai. I think Keesing is probably right that there was considerable stabilization at an earlier stage than most scholars have thought possible, but I am dubious about the extent to which substratum determined the structure of the Pacific pidgins.

Keesing also says that he avoids giving labels such as Jargon English, Beach-la-Mar, and so forth to the speech used in the Pacific at various historical stages because labeling would convey a spuriously discontinuous development and imply that we know more than we do about the linguistic characteristics of the codes in use at various stages (p. 92). Unfortunately, we will probably never be able to uncover sufficient historical and other data to untangle the threads of the various linguistic traditions.

Much obviously hinges on the interpretation of earlier, fragmentary historical accounts--for example, attestations by travelers, missionaries, and the like--about the English spoken by the "natives" in various Pacific islands. Keesing bases many of his claims on the early appearance in these records of certain constructions that later become "grammaticalized" (grammaticalization is discussed further below) in the Melanesian pidgins (e.g., the transitive suffix *-im,* the predicate marker *i,* and the use of *baebae* to mark futurity). The reasons why these historical records may not be accurate renditions of the languages are well known. Keesing is aware of them, too, but nonetheless relies on the records when it suits him and ignores them when it doesn't. For instance, he dismisses statistical counts of features in texts as useless, given the "overall filters of anglicization" and "internal variations in the corpora" (p. 151). Thus Keesing rejects Mühlhäusler's counts for the use of *he* as the predicate marker but insists that his own from a set of 1908 texts are reliable (p. 195). This is but one example of a number of inconsistencies in method and argumentation.

In chapter 6 Keesing discusses patterns in the Oceanic languages that were calqued into Melanesian Pidgin. He seems, however, to indulge too frequently in the "cafeteria principle"--a random picking out and attribution of features to substratum influence without regard for how they might have been borrowed or incorporated into the pidgin or cre-

ole in question (see Dillard 1970). While Mühlhäusler has attempted to work out implicational patterns for borrowing and the incorporation of particular features at particular stages of pidgin development, Keesing cites patterns that suit him from a range of different languages. He justifies his strategy--that "it is possible to take any Oceanic language of the southwestern Pacific and . . . make a case of substrate influence" (p. 106)--by claiming that these languages share a common core of constructions.

The relative diversity or homogeneity of the substrate is an issue that has finally begun to receive serious attention (e.g., Singler 1988). But in this case we are asked to believe that Oceanic speakers simplified "down to common denominators deriving from a common ancestral language" (p. 91), incorporating core structures of Oceanic grammar (in some cases at a relatively abstract level) (p. 96). Here, however, we run into problems because both Oceanic and English speakers were "analyzing and producing mutually acceptable sentences using different grammars" (p. 91). Thus, while ostensibly making a big bid for substratum influence, Keesing also admits that the syntax of the Oceanic Austronesian languages quite closely resembles the grammar of English when considered at this abstract level (p. 107). Chomsky, of course, would argue that at a certain quite abstract level the global syntax of all human languages should resemble one another.

So how can we separate substratum from superstratum influence? The simple answer is that in many cases we cannot (see Romaine 1988: ch. 3). Although Keesing recognizes this, he nevertheless pursues his substratum line. He suggests that these common denominators in the Oceanic pattern reflect unmarked and maximally natural constructions (p. 110). Then he says that in some cases this abstract Oceanic did not correspond to universal "default grammar" and that there are therefore two sorts of simplification processes that do not coincide (p. 116). In cases where the Oceanic pattern would have been opaque to English speakers, Keesing says they did not rely on it.

However, this in itself raises questions. For instance, the inclusive/exclusive distinction, which is a clear-cut case of Oceanic substratum in Melanesian Pidgin, is relatively opaque to English speakers. Most English speakers of Tok Pisin whom I know consistently fail to make it adequately. It is also probably not that transparent to some younger speakers of Tok Pisin, among whom its use is declining. So why was it incorporated in the first place? Another problem is that some features Keesing would like to attribute to Oceanic substrate are also found in other pidgins and creoles. One such is the use of a comparative con-

struction whose main characteristic is that the noun serving as the standard of comparison is the direct object of a transitive verb meaning "surpass" or "exceed" (e.g., in Cameroon Pidgin English *pas mi fo big* 'He is bigger than I'). Clearly modeled on serial verb constructions, this type of comparative is found in many pidgins and creoles where it is possible to argue African substrate. Nevertheless it would not be surprising to find this construction type occurring independently of substratum influence because it represents a weakly grammaticalized and transparent means of expressing the notion of comparison (see Romaine 1988: 56-57). The prepositional verbs like *agensim* and *raonem*, which Keesing says are a striking feature of Oceanic grammar that probably evolved from serial verb constructions (p. 181), are also found in Tok Pisin. (A new one I have heard is *afterim*, 'to be after someone'.) However, they are also found in English: "to up the price," "down a beer," and so forth.

This brings me to one of Keesing's central claims, to which I will devote the rest of my discussion: namely, that certain features either were present earlier than previously thought and are therefore common to Melanesian Pidgin or were "grammaticalized" earlier. The latter is crucial to Keesing's wish to push back the date for stabilization, but what he means by grammaticalization is unclear. Since Mühlhäusler's specific arguments against Keesing rest mainly on the analysis of the predicate marker, I will focus my own on Keesing's interpretation of the data for future marking.

Keesing says that the regularity of *bambae* (from English *by and by*) as a future/irrealis marker in texts of the 1870s and 1880s suggests that it was already becoming grammaticalized during the Labor Trade period and was not merely a "temporal adverb" (p. 48). I know of no way to distinguish clearly when *bambae* (and its related variants) is used as a temporal adverb as distinct from a grammaticalized future marker, which is also used with other adverbs that indicate relations of time or discourse sequencing and in certain contexts with an implication of causality or hypotheticality. Keesing, however, argues that by the late nineteenth century *bambae* seems to have been a grammaticalized form and not simply an adverb temporally framing the clause (p. 187). He notes that what happened to *bambae* is "theoretically important because . . . the transformation of what was until recent decades a temporal adverb in sentence- (or clause-) initial position to a grammaticalized preverbal particle is supposed to reflect a late phase in Melanesian Pidgin development, particularly associated with incipient creolization" (p. 182). This suggests that for him syntactic position of the

marker is the criterion for deciding whether we are dealing with a grammaticalized form.

The data and issues raised by them are actually more complex than Keesing, or for that matter Sankoff and Laberge (1980), are aware. On the basis of research done on Tok Pisin in the late 1960s and early 1970s, Sankoff and Laberge claimed that the temporal adverb *baimbai* was becoming a marker of future tense. Moreover, they linked this change to "the passage of Tok Pisin from a second language lingua franca to the first language of a generation of urban New Guineans" (Sankoff and Laberge 1980). They identified three stages in this process, as in examples 1-3.

(1) *Baimbai mi go.* 'By and by I'll go.'

(2) *Bai mi go.* 'I'll go.'

(3) *Mi bai go.* 'I'll go.'

The first stage is accomplished when *bai* results from the morphophonological reduction of the full adverbial *baimbai.* This is accompanied by a loss in stress. A later stage is reached when *bai* is placed in preverbal position next to the main verb and following the subject rather than at the beginning of the sentence or clause.

This general sequence of grammaticalization of tense markers is taken by many to be a significant hallmark of creolization. Pidgin languages normally use adverbial expressions to express tense, whereas creoles use particles that are usually preverbal (e.g., Markey 1982; Mühlhäusler 1986: 156-157; also Kay and Sankoff 1974:64, who cite the use of sentence external propositional qualifiers as a major typological characteristic of pidgins). From a cross-linguistic perspective, however, it is clear that neither the process nor specific chain of grammaticalization transforming a sentence initial temporal adverb into a preverbal tense particle is unique or necessary to pidgin and creole languages. Marchese, for instance, notes the development of tense auxiliaries from time adverbs in Kru languages (1986:254-257). Some Kru tense markers are clearly reduced forms of time adverbs and now have the distributional properties of auxiliaries rather than adverbs and can even occur in the same clause with the corresponding adverb; for example, a general past tense is derived from the corresponding adverb meaning "yesterday" by semantic extension.

All three stages of this alleged grammaticalization are represented synchronically in data from both children and adults that I collected in

1986-1987 in Papua New Guinea. Even at the time when Sankoff and Laberge obtained their data, however, *baimbai* was a recessive feature. It is clear from my data that the use of *baimbai* is not entirely exclusive to rural or second-language speakers, or to adults.

At least three issues need to be discussed to clarify the sequence of steps identified by Sankoff and Laberge as part of the grammaticalization process and to address Keesing's claims. First, do these stages reflect true diachronic ordering? Second, what is the connection between syntactic positioning and phonological reduction? Third, is this sequence coincident with creolization? I will dismiss the third issue straightaway by saying that it does not appear to be, despite Sankoff and Laberge's statement (1980:195). My claim is based mainly on the finding that younger rural speakers are more frequent users of preverbal *bai* than urban speakers. Since creolization is mainly an urban phenomenon, and creolization is the trigger for grammaticalization, then we would have expected to find urban speakers to lead this development (Romaine 1989).

To illuminate all three questions I will consider further diachronic and comparative evidence. As far as the history of *by and by* in Pacific Pidgin English is concerned, Schuchardt notes its occurrence in Chinese Pidgin English and quotes example 4 below (1883), which interestingly contains a preverbal usage.

(4) *My by'mby catchee he.* 'I will get it.'

Although Baker has attested the earliest occurrence of *by and by* in the Pacific in Chinese Pidgin English (1807) and early examples in other Pacific pidgin Englishes (Hawaii 1820, New South Wales 1826, Queensland 1855, New Hebrides 1865, Solomons 1874, Papua 1885, and German New Guinea 1883) (1987:179), he does not note any preverbal occurrences.[1]

None of the earlier historical sources mentions the possibility of preverbal *baimbai,* and Sankoff and Laberge have overlooked it too. Keesing cites an 1883 occurrence (again from Schuchardt) to justify his claim for early grammaticalization. Thus he claims that until the 1880s *bambae* was being used as a temporal adverb, but that Schuchardt's example shows it as a "grammatical tense marker" (p. 184). I have, however, collected some thirty attestations of preverbal *bambae* both diachronically and synchronically in speech and writing. The earliest attestations for this feature in Pacific Pidgin English occur in New South Wales (1844; see Troy 1985) and Queensland (1858) and both predate the Labor Trade.

This means that syntactic positioning of *bai* and phonological reduction must be seen as separate issues. Their conflation by Sankoff and Laberge fails to accommodate a number of competing developments, which have made the grammaticalization process messier than it appears. Phonological reduction is neither a necessary nor a sufficient precondition for preverbal placement of the marker. Example 5, which I recorded from a teenage boy in rural Papua New Guinea, may also provide evidence for the possibility of yet another variant, namely, a clause-initial reduced form of *baimbai.*

(5) *Barn yu go stap?* 'Will you go and stay there?'

My examples are important because they indicate that the incorporation of the full form *baimbai* within the verb phrase probably existed as a grammatical option long before creolization or indeed the existence of a community of fluent second-language speakers. This option has apparently been available in Tok Pisin for at least a century, and in Australian Pidgin English since 1844, and it still exists today in the speech and writing of fluent Tok Pisin users. It is possible, of course, that the synchronic examples I have recorded are not survivals in any direct sense of the earlier attestations and, therefore, are not genuine reflexes of this construction. They may be simply analogical reformations patterned on preverbal *bai.* In fact Sankoff and Laberge note a personal communication from Anne Chowning, who claims that "in areas of New Britain in the 1950s, *bai* was the exclusively used form, with *baimbai* appearing later as a novel introduction" (1980:201). It is dangerous to assume, although it is commonly done, that older speakers preserve an earlier stage of the language and do not change their speech over the course of their lifetimes.

Some of this evidence could be seen as consistent with Keesing's claims for early stabilization. In my view, however, it is inconclusive. The most we can say is that preverbal position was a potential slot for the positioning of grammatical markers long before creolization or extensive phonological reduction. At the moment no unequivocal criteria exist for determining when a form has become grammaticalized, though a number of scholars have cited category shift, phonological reduction, and semantic bleaching as concomitant processes of grammaticalization. It is difficult to tell at what stage we are dealing with a form that is no longer a temporal adverb.

As far as meaning is concerned, the comments made by a rural Tok Pisin speaker in example 6 are interesting. When questioned about the

variation between clause-initial and preverbal *bai,* he mentions preverbal *baimbai* as a possible variant. He does not attach any important difference in meaning to the variants, though.

> (6) *Baimbai, nogat, em nau liklik. Baimbai em i go, baimbai i kam. Em baimbai i kam. Baimbai em. klostu bai. I gat kainkain mining i stap. Bai baimbai ating wankain olsem.*
>
> 'You only hear *baimbai* a little bit now, as in *baimbai em i go, baimbai i kam, em baimbai i kam.* That's *baimbai.* It's just about the same as *bai.* They have a similar sort of meaning. *Bai* and *baimbai* are almost the same thing.'

It should not be surprising that the same structural innovations arise at different stages in the development of a language and either spread or fade away. The possibility that convergent etymology is important in determining lexicalization in pidgin and creole languages is now widely acknowledged, and it seems plausible to assume that structures compete for grammaticalization too. The more potential sources for grammaticalization of a construction, the more likely that construction is to be incorporated, though different speakers may pull the language in different directions. Keesing argues for substratum influence in the form of a common Oceanic pattern for incorporation of the future marker within the verb phrase (p. 184). The early attestations in New South Wales Pidgin English and Chinese Pidgin English make superstrate influence more likely, since the Australian and Chinese substratum would have been different from each other and each would have been different from the Oceanic substrate.

There is, however, another structural possibility that could have paved the way for the use of *bai* in preverbal position. Some speakers use the form *em bai,* as in example 7. Here the third-person pronoun *em* is not syntactically integrated as a clause argument. It is easy to see how speakers might have regarded this as a short form of *baimbai* if we look at example 8, where we have a case in which *bai* appears on both sides of the third-person singular pronoun *em.* In rapid speech the sequence of *bai em bai* is almost identical with *baimbai.* The full form *baimbai* might have been first reanalyzed in this position to the sequence *bai em bai,* which would then have set the precedent for the reduced form *bai* to occur both clause initially and preverbally. This also fits in with the finding that it is the third-person pronoun that provides the point of departure for the diffusion of preverbal *bai* throughout the pronominal

paradigm. If this is true, then phonological reduction of *baimbai* is not the only source for the short form *bai.*

(7) *Em bai tupela sindaun.* 'The two of them will sit down.'

(8) *Em bai makim yu, bai yu no laik long en, bai em bai bagarapim yu disla kain olsem.*
'If you didn't like him, he'd mark you and rape you or something like that.'

There are also many examples where *bai* appears both preverbally and clause initially with a repetition of the same verb, as in example 9.

(9) *Nau bai kau bai go.* 'Now the cow will go.'

There are also cases where preverbal and clause-initial uses are juxtaposed within the same utterance, which suggests that for some speakers the two are optional variants, possibly with some stylistic or pragmatic significance. It seems to me likely that we have to acknowledge that there is more than one route to grammaticalization of *bai* in its present meanings and functions.

Keesing's book is a useful and important starting point for further debate about the historical and present-day affinities among varieties of Melanesian Pidgin. Future work should address, in particular, the reasons why the syntax of Solomons Pijin and Bislama is considerably more elaborated than that of Tok Pisin.

NOTE

1. I am grateful to Philip Baker for providing me with some of the examples from German New Guinea and Australia.

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Review: PETER MÜHLHÄUSLER
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Of the many issues that this book raises I shall address only two: the question of continuity and the problem of time.

Regarding the question of identity over time and place and what

Keesing refers to as "spurious discontinuity": Languages are brought into being by all sorts of forces, for instance by acts of identity of members of a speech community, by acts of non-identity (as was the case between the speakers of Chinese Pidgin English), by forced official decree, and so forth. The crystallization (or gelling in Philip Baker's terminology) of a language would seem to be crucially dependent on something like a moderately closed social network. I feel that Keesing misinterprets the nature of the whaling, sandalwood, and bêche-de-mer trades by arguing that they led to "a single early-Pidgin speech community" or "the linguistic community" (pp. 34, 35). Rather, contact between the members of this postulated speech community was tenuous and often only indirect (via visiting Europeans). Structural and lexical identity of a language over time and space depends on reliable and homogeneous patterns of transmission. As Keesing himself argues, the nature of transmission of early Pacific Pidgin differed considerably from place to place and time to time: on board whaling and other vessels (p. 33), adults learning from other adults in the plantations (pp. 56-59), children learning pidgin from returning adults as a second language (p. 55), in the early mixed beach communities (pp. 15-21). That such different modes of transmission and crystallization are signs of a single speech community or likely indices of a shared core grammar seems implausible. My logic leads me to conclusions quite different from those of Keesing.

I am aware of the fact that Keesing rejects the use of Tok Pisin as a canonical language, whatever that may mean. Nevertheless, certain observations made during my own fieldwork in this language seem pertinent here. My first observation is that varieties of very different degrees of sophistication can coexist quite happily within a small area. The existence of a highly developed creolized Tok Pisin in Malabang village in Manus Island or Urip village in the West Sepik Province of Papua New Guinea did not influence the second-language Tok Pisin of surrounding areas to any great extent. This suggests that the early presence of centers of creolization in the Pacific (for which Keesing can present no firm evidence) may have had much less influence on the development of Melanesian Pidgin than he suggests. A second observation relates to Aitchison's study of variation in creolized Tok Pisin (1984). She found the speech of small social groups within a larger social network differed significantly from that of other such groups. Moreover, there was evidence of differences even between same-generation members of the same family. Keesing's view that the use of *we(a)* as a relativizer was common to all Melanesian pidgins in the late 1880s and

his dismissal of my own evidence from Tok Pisin to the contrary (p. 112) contrasts with Aitchison's finding that the "Goroka girls had fully developed relative clauses introduced by the marker *we*, whereas the Lae girls did not" (1984: 17). One needs to add that Tok Pisin was introduced to the Goroka area in the 1950s whereas it has been spoken around Lae since the turn of the century. Continuity of grammatical tradition would seem to have a rather shaky empirical basis.

Let us assume with Keesing, however, that the first occurrence of certain diagnostic constructions was followed by continuous diffusion and transmission. Of the ten constructions that Keesing claims to have been Common to southwestern Pacific pidgins in the late 1880s (listed on p. 112-113), the majority turns out not to have originated among speakers of Oceanic languages. Philip Baker's provisional analysis of the corpus of data collected for the Atlas of Languages of Intercultural Communication in the Pacific Area project (1989) suggests that the majority of them originated in Australia before Melanesian immigration to that country. For instance (using Keesing's numbering):

1. The basic pronouns were first documented as follows:

me	'I'	New South Wales 1817
yumi	'we' (incl.)	Queensland 1814
yufela	'you' (pl.)	Queensland 1880
alltogether	'they'	Queensland 1858

3. The systematic use of the transitive affix *-im* is first documented for New South Wales in 1826.

7. *-Fela* as a suffix for quantifiers occurs in Queensland in 1848, with attributive adjectives in New South Wales in 1842 and with demonstratives in Queensland in 1842. Contrary to Keesing's assumption (p. 113), *-fela* was not introduced from China Coast Pidgin.

8. Phrasal interrogatives of the "what name" type are first documented in Queensland in 1868.

9. The marking of possession by means of *bilong* first appears in New South Wales in 1826.

I am not suggesting a continuous transmission of these or other features. Rather, I would like to point out that such constructions could arise even where Oceanic substratum is absent. Given the quite considerable typological differences between Australian Aboriginal and Oceanic languages, the similarities of the Pidgin English used by their speakers will have to be explained in terms of linguistic universals or shared superstrate influence and not, as Keesing wants us to believe, substratum languages.

In the above discussion, chronological time features prominently, per-

haps too prominently. The date of first occurrence is not a sufficiently reliable indicator even when culled from a vast corpus such as the one compiled by Baker and myself over many years. What is needed is an approach that distinguishes chronological from relative time. The former concept is needed to answer such questions as: When did Tok Pisin become severed from the Melanesian Pidgin tradition--1880? 1890? 1900? When is a construction first documented for a particular area? When were the first Pacific Islanders repatriated from Queensland?

Separate from these issues is that of relative time. Underlying my entire body of writings on the development of Pidgin English in the Pacific is the implicational or quantum-linguistic model that asks (a) in what order do constructions (rules or rule environments) emerge in a pidgin language and (b) does the presence of C imply that of B and A for a given lect? Thus, with the third-person plural pronoun, for instance, the question is not so much the chronological one of When is it first documented? but rather queries such as, If speakers use the third-person plural pronoun, will they also use the second-person plural and the first-person plural pronouns? If speakers use plural pronouns, will they also use dual pronouns? Will plural pronouns be used to refer to animates before they refer to inanimates?--and so on.

Along these lines, in my 1981 article quoted by Keesing, I looked at such implicational patterns and found that, for speakers of different ages in the same location, one could establish patterns such as D implies C implies B implies A, but that, at the same time, not all speakers have reached stages D or C (Mühlhäusler 1981:80). The importance of the implicational argument for the universals versus substratum debate is considerable.

The claim is that such implicational patterns as the animacy or accessibility hierarchies provide principled limitations on what can be transferred from another language in what order (not, as Keesing interpreted it, "that substratum models will have an impact on a developing pidgin only at certain crucial points in its development" [p. 171]). They can thus provide an answer to a problem that neither Keesing nor any other substratophile can answer: Why is it that many constructions, rules, or rule environments found in the substratum languages are not borrowed by pidgins, and why do those that are adopted get borrowed in a particular sequence rather than all at once?

Let us briefly return to the pronoun system that Keesing suggests had been established by 1890 (pp. 133-142). Ignoring the observable fact that the distinction between inclusive and exclusive first-person plural pronouns (a typical feature of Oceanic languages) could not be found in

most Melanesian pidgins by that date, it is also quite striking that there appear to be stages in the development of the pidgin pronoun systems prior to 1890. Thus, there is an earlier system consisting of *me* and *yumi,* and a later system where *he, you, youfela,* and *alltogether* were added. These stages were postulated in an article I published long before Baker's data had become available (Mühlhäusler 1986). Inasmuch as pidgins change in complexity over time, any comparison with a static substratum grammar that does not change in complexity must remain unsatisfactory.

I have discussed these matters in much more detail in a forthcoming review article to appear in *Studies in Language.* Baker's analysis, made available subsequent to my writing this review, seems to further confirm the fallibility of Keesing's substratist position.

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First, I want to thank the distinguished scholars who took the time and trouble to review my book so carefully and constructively.[1] Responding to their comments provides an opportunity to clarify my views on some issues, to restate parts of my argument that have been misunderstood, and to add some new and important pieces of evidence that have come to light since *Melanesian Pidgin and the Oceanic Substrate (MPOS)* was written.

Let me begin with Siegel's commentary, since it sets out a useful summary of the argument of *MPOS.* This will allow me to note some points on which Siegel's reading differs from the one I had intended and to clarify some terminological differences.

An important starting point is the question of how various lexical items, and what became grammatical forms, got into the Pacific in the first place. I entirely agree with Siegel (and Baker and Clark and Troy and now Mühlhäusler) that many of the lexical forms that became established in Pacific Pidgin, and some incipiently grammatical forms (such as *-fela* and *-im*), came into the Pacific Islands by way of New South Wales English or Chinese Pidgin English or both.[2] I have developed the theme further in my unpublished paper on *-fela* (Keesing n.d.*b*). I have compared notes closely with Jakelin Troy on her research on New South Wales English for the last five years and its relation to my own. Both the eastern coast of Australia and the Pacific Islands were connected into a worldwide network of whaling and commerce;[3] and the Australian frontier inherited the same traditions of military/maritime English and "native talk' as other margins of the expanding British empire. Elements of this tradition were, I argue, *reanalyzed* by Pacific Islanders to fit grammatical patterns broadly common to their own languages. Despite Mühlhäusler's comment, the early use of particular pronoun forms outside the Pacific Islands is irrelevant to my argument (although the ones he lists, citing Baker, may well have been introduced into Australia). I am talking about a developing linguistic code, not the lexical bits and pieces incorporated into it.

Siegel and I obviously are talking past one another regarding certain terms and issues. I see no point in arguing about whether a developing medium of intercultural communication in the Pacific at a particular point of time (say, 1855 or 1860 or 1865) was a "pidgin" or a "jargon," given the different senses assigned to these terms in the literature. In *MPOS,* no theoretical weight of any kind is hung on the distinction between "jargon" and "pidgin" (although I agree that greater theoretical clarity and consensus regarding this issue is needed).

Since Siegel misunderstands what I was trying to say about "stabilization," I need to clarify that. I used "stabilize" in an intentionally general sense to refer to the progressive regularization of linguistic patterns. One sort of regularization is grammaticalization of forms. The verb ending *-im* discussed by Siegel will serve to illustrate. I infer that in the 1840s *-im* was being used sporadically both by Europeans (drawing on their tradition of "native talk') and by Pacific Islanders, emulating the speech of whites and finding in *-im* an analogue to transitive suffixes

pervasive in their native languages. During the next forty years, *-im* progressively changed into a fully grammaticalized transitive suffix. Perhaps Siegel and I differ in our guesses (and that is all they can be, given the thin evidence) about how far *-im* had gone down this track by 1860, 1870, or 1880. A second sort of regularization is standardization of constructional patterns (such as the use of *blong [mi]* for possessives); a third sort is standardization of particular lexical forms.

When I talk about the degree of regularization or expansion of a developing Pacific pidgin at any particular point in time, it is with reference to those speakers who had the most fluent command of the code and whose speech served as the target language in its further diffusion. In a zone where a pidgin is used, there will always be participants who speak it badly (that remained true of most Europeans through most of the nineteenth century and was true of Pacific Islanders on the frontiers of labor recruiting as they expanded). It should come as no surprise that in the 1870s we find in historical texts many instances of verbs used transitively without *-im*. I infer that the form was well on its way toward regularization in the 1870s, but had not become fully grammaticalized across the range of transitive verbs until the late 1880s. (Within the past century, further fine-tuning of the marking of transitivity has continued, a point Siegel mentions and one I illustrate with regard to a text below.)

A consequence of the differential command of a developing pidgin is that the discovery of historical texts (such as that I set out below) showing a more fully developed pidgin at a particular date than prevailing theories lead us to expect can force us to revise the time scale *backward,* but we cannot (contra Mühlhäusler) be similarly forced to revise the record forward by finding fragments of less developed pidgin, unless they constitute an extended corpus of the speech of ships' crews or other sophisticated speakers. Since my argument in *MPOS* hinges heavily on the most fluent speakers of a developing pidgin and their role in its expansion linguistically and its diffusion geographically (and since this seems to have been misinterpreted), let me again summarize my claims:

1. From 1855 (or so) onward, the most fluent speakers of a developing pidgin were *Pacific Islanders,* not native speakers of English (some of these islanders may also have commanded a register much closer to standard English, used when talking to Europeans).

2. They were primarily speakers of Oceanic Austronesian languages (speakers of Gilbertese, various Loyalties languages, Pohnpeian, Rotuman, and Fijian were prominently represented).

3. These most-fluent-speakers worked in key positions, notably on

ships but also on shore bases and as foremen in plantation settings, where they were the primary agents of diffusion of the code, both to Europeans and to fellow Pacific Islanders.[4]

Siegel errs in attributing to me the claim that "stabilization had occurred by 1860." Stabilization (in the sense in which I use the term) was a gradual process that was certainly still going on (in the New Hebrides, the Solomons, and New Guinea) in the early years of this century. As I show in examining pronominal forms, stabilization/regularization was further along in some parts of the developing grammatical system than in others (and I advance arguments about why this should have been so). I do argue that an important phase in the expansion and regularization of a developing Pacific pidgin probably had taken place by the time the Labor Trade began in 1865, an inference I base as much on sociolinguistic as linguistic grounds. I make very few specific linguistic claims regarding pidgin as of 1865, precisely because the textual evidence is so thin. What I suggest is that the code had expanded enough that by the 1860s it apparently could be a primary medium of ongoing quotidian social life on the ships, rather than a stripped-down medium for sporadic communication about work-related tasks. Although I speculate that there were probably fluent childhood speakers for whom this was a coordinate first language, this is only a guess, and one on which no argument in *MPOS* hinges.[5] Whatever the linguistic nature of this code, I hypothesize that it constituted the initial medium of the Labor Trade, disseminated by the Pacific Islanders who acted as its key agents (hence, contrary to some widely held views, pidgin did not have to be invented by islanders thrown together as recruits and plantation workers).

My general claim is that, decade by decade, the ongoing process of stabilization/regularization/grammaticalization had advanced considerably further than most authorities have asserted. I argue that the entire time frame for the development of pidgin needs to be pushed back by ten or fifteen years from the timetable most specialists have posited, whatever index of that development we choose to use (i.e., whether we use regularization of usages or expansion of constructional possibilities or global syntactic complexity). Siegel himself comments that "Mühlhäusler simply has his dates wrong" (something the latter has yet to concede despite his professed reverence for "chronology"). I believe that Siegel (and Clark) and I differ more in our discursive practices than in substantive questions of who-was-saying-what-when. It is clear, however, that we read the same texts in different ways and that the texts presently available allow of such alternative readings. I believe

that additional new evidence (such as that set out below) will require them progressively to accept the general picture I present. Time will tell.

I believe Siegel misrepresents my position on a number of other points, although it would be tiresome to detail all of them. For example, my sketch of Oceanic subgrouping (pp. 65, 68) indicates quite clearly that I do *not* include Loyalties/New Caledonia languages or South Hebridean in a putative Eastern Oceanic subgroup. What I do claim, citing data from Iai (Uvea) and Sie (Erromanga),[6] is that these languages incorporate all key grammatical elements of what I call the "core" Oceanic pattern (notably, pronominal elements referencing implied subject and object noun phrases), but that these elements are heavily cliticized, operating as bound particles marked on verbs and aspect markers. I leave to syntactic theorists the question of whether speakers of such languages could have been primary agents in the creation of an interlingual code in which the equivalents of such cliticized forms were free morphemes (as they would have had to be to be intelligible to superstrate speakers);[7] but they certainly would have found such a code congruent with their native languages. Siegel appears to misunderstand my argument with regard to "core grammatical patterns" and a putative Eastern Oceanic subgroup. What I claim to be the "core" pattern for marking subject-reference and transitivity has been reconstructed for *Proto-Oceanic* (not only Proto-Eastern Oceanic). Elements of it are manifest in *all* Oceanic Austronesian languages I have examined. Some of these languages (including Southeast Solomonic, Nuclear Micronesian, North Central Hebridean, and Fijian-Rotuman, which are very provisionally and problematically subgrouped as Eastern Oceanic) are extremely conservative in preserving the "core" Proto-Oceanic pattern; others are considerably less conservative (including the New Guinea Oceanic languages, South Hebridean, and those of New Caledonia and the Loyalties, but also in some respects including Polynesian languages, as Belikov notes, although they are Eastern Oceanic).

On the question of separation between a putative Micronesian Pidgin and a Melanesian Pidgin, I believe Siegel overlooks the crucial linkages between the German plantations and colonial centers in the Marshalls and the plantations of Samoa and Neu Guinea. Hernsheim came from Jaluit in the Marshalls to Neu Guinea; I note his observations, as cited by Schuchardt, that clearly indicate that the same pidgin was in use in both areas (pp. 58-59; Governor Solf implies the same). Hernsheim told Schuchardt that this regional pidgin was represented only to a limited

degree in New Britain when he arrived in 1876, but that it had spread rapidly in his early years there.

I would not rule out the possibility that speakers of Bismarcks languages could have had some minor influence on the pidgin developing in Samoa and Queensland. However, these were fellow speakers of Oceanic languages (albeit ones that have substantially modified the "core" Oceanic syntactic pattern) and, demographically speaking, they were a drop in the linguistic bucket--a clear minority even on Samoan plantations until well into the 1880s. I remain convinced that the pidgin originally introduced into Samoa was the one spoken by the linguistic brokers I discuss above and that linguistic changes taking place in Queensland and its major recruiting zones were continuously fed into the Samoan plantations (for reasons I make clear in chapter 5). I have yet to see any clear evidence that "Samoan Plantation Pidgin" was a separate dialect, as Mühlhäusler claims quite unambiguously in several places. (Mühlhäusler's argument regarding the separate origin of Tok Pisin is ambiguous on several points but I cannot accept Siegel's reading of it, in the light of Mühlhäusler's repeated assertion that Tolai is Tok Pisin's only significant substrate language.)

I agree with Siegel that my interpretation of the early texts is colored by a theoretical argument; so, inevitably, is any counterinterpretation. That is why I rely so heavily on distributional evidence as a basis for inferring a stratigraphy of Pidgin development. Since Siegel mistakenly imagines that I break my own "ground rules" in discussing "prepositional verbs," let me state these "rules" yet again, and as clearly as I can.

1. Where we find a syntactic pattern or lexical usage in Tok Pisin, Bislama, and Solomons Pidgin, we can assume that it was present (although not necessarily fully regularized) in Southwestern Pacific Pidgin as early as 1885.[8]

2. Where we find a syntactic pattern or lexical usage in Bislama and Solomons Pidgin and *not* in Tok Pisin, we can assume that it emerged (although it was not necessarily fully regularized) after 1885 and prior to 1905.

3. Where we find a syntactic pattern or lexical usage in Bislama and not in Solomons Pidgin, or vice versa, we can assume that it emerged after 1905.

Some qualifications need to be made to these guidelines. One is that once crucial elements of a paradigm or pattern have emerged within a speech community, it is quite plausible that after separation of daughter dialects the pattern might be augmented or completed in similar ways in these dialects. A grammaticalization process set in train has a kind of

internal logic that can be realized in parallel among separated languages (that, I think, is what Siegel means by "embryonic structures," "planted seeds," and "mature forms"). I illustrate at some length how pronominal paradigms have inner logics, whereby a set of slots is filled in. Clearly the process of paradigm stabilization had not run its full course as of 1885, when Tok Pisin split off. Thus, while we find a close correspondence between the present pronominal paradigms, some interesting differences remain (such as the Tok Pisin use of *entupela* for "they two," where Solomons Pidgin has simply *tufala*).

It is also plausible that a *very few* similarities in grammar or lexicon between Tok Pisin and Bislama/Solomons Pidgin (or between the latter two) have emerged after their geographical separation, either by chance or by diffusion (through continuing contact between plantation communities). What is not plausible is a massively long list of such similarities, such as those connecting Tok Pisin and the other two dialects. A further qualification is that it is quite possible that a form or pattern that was present in the regional pidgin as of 1885 subsequently disappeared in Tok Pisin (perhaps because of the lack of substrate support, a possibility Siegel notes). It is *possible* that "prepositional verbs" were being used in Queensland prior to 1885, but that they disappeared in Tok Pisin.[9] But, in fact, I follow very strictly my own "ground rules" in accepting as more probable the emergence of prepositional verbs in Queensland and its recruiting areas during the period 1885-1905.

Talk of "embryonic forms" could distort the picture if it suggests that the Pidgin of the late nineteenth century was too limited in its syntactic resources or insufficiently regularized to permit connected and elaborated narrative discourse. I cite below a Pidgin text I recently discovered, recorded in the Solomons in 1893. It shows compellingly that by a century ago Pidgin had achieved virtually all the syntactic richness characteristic of contemporary "bush" Pidgin in the Solomons or Vanuatu and many of its present forms.

I believe that Siegel underestimates both the degree and historical importance of regional variation *within* modern Pidgin dialects. Siegel claims, for example, that in Bislama *-fela* is not used with demonstratives *(dis-fela).* Tryon's and Charpentier's evidence,[10] however, shows that in different parts of Vanuatu three different demonstrative patterns occur (seemingly corresponding to different patterns in the substrate languages): *ples ia* 'this/that place', *dis-fela ples* 'this place', and *dis-fela ples ia* 'this place'. In this case and a number of others, Tryon shows patterns supposed to distinguish Tok Pisin from Bislama are found in some regional dialects of Bislama and attested in nineteenth-century texts.[11]

Belikov raises several interesting points. One is the issue of why speakers of Polynesian languages, which have gone off in some syntactic directions that deviate from the ancestral Eastern Oceanic patterns, did not leave a stronger imprint on a developing Pacific Pidgin. The Polynesian presence in ships' crews and beachcomber colonies was marked prior to 1850, declining sharply after that. My reading of the evidence is that such elements as transitive suffixes and resumptive pronouns as equivalent to Oceanic subject-referencing pronouns (SRP'S) were only beginning to be used in the 1840s and 1850s. By the time the generation of fluent speakers of a developing pidgin emerged in the period 1850-1860, Polynesian speakers had become a marginal presence.

A second point is the question of relexification. I infer that lexical items in Tok Pisin derived from Tolai and other Bismarcks languages were introduced into New Guinea Pidgin after its separation from the regional pidgin from 1885 onward. Such forms as *diwai* and *liklik* replaced *tri* 'tree' and *smol* 'small', and so forth, which we know were present in the regional pidgin of the 1880s. (If it is accepted that Tok Pisin is historically derived from a regional pidgin used in Queensland and its recruiting areas as well as in Samoa, I don't see how this can be disputed.) The evidence Belikov gives does not address these forms,[12] but simply indicates that all three dialects have continued to add (inevitably different) lexical resources from English in the century since Tok Pisin separated from the others.

What I mean by my claim that by the end of the 1880s "there was no room or need to expand [pidgin's] syntactic possibilities" should be clear from Pionnier's early 1890s texts and the text from Solomons Pidgin quoted below, from 1893. That is, by a century ago, Melanesian Pidgin incorporated syntactic structures that allowed complex, multiclausal sentences and extended, rich narratives. Obviously, room existed for further expansion, syntactically as well as lexically (and a good deal of that has occurred in the recent creolization of Pidgin dialects, as documented by such scholars as Sankoff, Romaine, and [for the Solomons] Jourdan 1985b).

Bickerton's comments are positive and helpful. As he notes, *MPOS* is by no means a blanket argument for substratomania. Rather, I am arguing (as he himself has) that Melanesian Pidgin is a very special historical case. First, the substratum languages are relatively homogeneous and their speakers had ample sociolinguistic room (in the context of shipboard and later plantation communication, with limited exposure to standard English) to leave a strong impress on the developing pidgin. Second, Pacific pidgin remained a second language, mainly learned by

adults, over the span of four or five crucial formative generations, without (in the New Hebrides, the Solomons, and New Guinea) either the hegemonic presence of a superstrate language or the breakdown of the plantation system. In these special circumstances, I claim (1) a pidgin can become much richer syntactically than pidgins are ever supposed to get, without turning into creoles; and (2) a pidgin can incorporate global patterns broadly common to substratum languages, where these are sufficiently congruent with superstrate patterns and compatible with general strategies of language learning/simplification. I had not intended to extend my argument to Atlantic creoles or to pidgins elsewhere in the world, or to enter into debates regarding the special Hawaiian case to which Bickerton refers.

Bickerton is right, and constructive, in suggesting that I should have given more examples from Gilbertese, Rotuman, and so forth (the data I have on these languages indicate that they manifest the Oceanic patterns I discuss, but further examples of this would have been helpful); and in pleading for maps. The difficulty with maps of the Pacific is that there is so much water and so little land. Perhaps a foldout map will be possible if a second edition proves feasible.

Romaine asks, "How can we separate substratum from superstratum influence?" I argue in *MPOS* that in many cases we cannot and need not. That is, where there is convergence between substrate and superstrate or congruence with universal patterns of minimal markedness, speakers of the different languages involved in multilingual interaction can get to the same place by different routes. However, in chapters 7, 8, and 9 I cite a series of morphological and syntactic constructions where Melanesian Pidgin incorporates patterns that are *unmistakably* modeled on Oceanic (rather than English or universal) grammar. Romaine asks why, if the inclusive/exclusive distinction is opaque to New Guinea speakers, "it was incorporated in the first place." I thought I answered that question. It was incorporated *somewhere else*, by speakers of languages where such a distinction is natural and obligatory (cf. Mühlhäusler's comments above; also in 1987a and 1989). Having been transplanted to "alien linguistic soil," as I put it, Tok Pisin is being pushed in the direction of radically different substrate languages. Scant wonder that such semantic distinctions, and the so-called predicate marker, are in some jeopardy.

Romaine devotes most of her attention to future marking. I have published a long article on future marking in historical perspective (Keesing 1985), which Romaine does not mention, so here I will be brief. First, I agree with her (on the basis of my textual evidence and

Solomons data) that two separate issues have become entagled in the literature: the reduction of *baebae* to *bae*[13] and the incorporation of the form into the verb phrase as a grammatical element. My data on *bae* in rural and older forms of Solomons Pidgin and Jourdan's data on urban speakers (1985a) indicate that the use of the long or short form carries no grammatical weight.[14] The short form is more common in the urban dialect, however, following a general pattern of streamlining and phonological reduction (Jourdan 1985a:76-78).

The question of grammaticalization is complicated, as Romaine indicates. I show (1985) that deciding what is "preverbal" and hence grammaticalized is by no means straightforward (because of a verb phrase pattern in which pronominal subject markers intervene between tense-aspect markers and verbs). I have argued that there was a continuous pull by Oceanic speakers to grammaticalize *baebae* within the verb phrase, Oceanic-style (and assign it an irrealis as well as time reference), and a countervailing continuous pull by English speakers to keep it in clause-initial position as "by and by." Of such linguistic tugs-of-war are pidgins fashioned. In short, I agree with practically all of Romaine's argument: with her separation of the *baebae* → *bae* shift from the question of grammaticalization, with her conclusion that "preverbal position was a potential slot for the positioning of grammatical markers long before creolization or extensive phonological reduction," and with her observation that this is "consistent with [my] claims for early stabilization."

Romaine misrepresents my disagreements with Mühlhäusler about counting of forms. She refers to a controversy about whether (as I claim) the Eastern Oceanic cast of the Solomons Pidgin pronominal system dates from the beginning of this century or whether (as Mühlhäusler has claimed in several papers) it represents a more recent and conscious linguistic change, an attempt by Malaitans (in the 1920s or 1930s) to distance themselves from Europeans. In demonstrating that he is wrong (Keesing 1988, 1991), I challenge the appropriateness of the statistical measures he proposes, but also show that even if we use them the numbers prove him wrong. The 1893 text I give below further establishes that I am right and Mühlhäusler is wrong (readers can count the "resumptive" pronouns and *him he's* if they like). As with *bae,* counting is valuable and useful provided you know how to count and what the results mean, both of which require an adequate grammatical analysis.[15]

Let me turn to Mühlhäusler's comments. First, as I have reiterated,

the ultimate origin of lexical forms and incipiently grammatical elements is irrelevant to the patterns into which they are constructed. Mühlhäusler writes, "Of the ten constructions that Keesing claims to have been common to southwestern Pacific pidgins in the late 1880s . . . the majority turns out not to have originated among speakers of Oceanic languages." But my argument had nothing to do with where the labeling bits and pieces came from but rather with their development into a highly expanded pidgin, which had attained much of its present complexity a hundred years ago.

Mühlhäusler does not confront the fact that the ten patterns I show to have been established in the pidgin of the southwestern Pacific more than a century ago have a far-reaching significance in terms of the historical interpretations he himself has proposed. He has previously argued that prior to 1880 Pidgin had such a simple "one- and two-part grammar" that complex constructions (such as periphrastic causatives) were impossible (Mühlhäusler 1980). I show that periphrastic causatives were recorded by 1869. He claims that *-fela* was used quite unsystematically until long after the 1880s. I document that *-fela* was being used by the 1880s in seven stable, interconnected grammatical slots. He has claimed the "they" pronoun was used as a plural marker only with human nouns until the end of the century.[16] I show that *olgeta* was being used to pluralize inanimate nouns as early as 1880 (p. 129). To say, as Siegel does, that "Mühlhäusler simply has his dates wrong" will not suffice when so much theoretical weight has been assigned in the literature to the developmental sequences he has proposed.[17]

Mühlhäusler offers no evidence for the "observable fact that the distinction between inclusive and exclusive first-person plural pronouns (a typical feature of Oceanic languages) could not be found in most Melanesian pidgins by [1890]." We have texts showing both *yumi* and *mifela* in use by the 1890s (although it is true that no observers explicitly tell us that an inclusive/exclusive semantic distinction is being used). But does Mühlhäusler really expect us to believe that Tok Pisin and the Solomons and Vanuatu dialects separately evolved not only the same semantic distinction (which, as Romaine notes, is as opaque to many Papua New Guineans as it is to English speakers) but also exactly the same pronominal forms to fill these slots? He apparently would have us believe the same thing about *hem i.* The 1893 text below shows that *hem i* was thoroughly regularized in the Solomons by the early 1890s.

This text, which is of considerable importance in reinforcing the general argument of *MPOS,* was recorded in 1893 by the British naval offi-

cer Lieutenant B. T. Somerville in New Georgia, in the western Solomons. I employ the English-based orthography he used (Somerville 1897:450-451).

Long time before, one fellow man, name belong him he Tasa, him he go along Tomba, along canoe catch him fish along spear. By and bye one fellow makasi he come, him he catch him, him he put him along canoe. Close up another fellow makasi he come, he put him head belong him out of salt-water, he sing out, "What name you shoot him woman-makasi belong me? by and bye altogether picaninny belong me he die suppose he no catch him kaikai belong him."

Tasa, him he talk, "What name you talk him, suppose picaninny belong me he no kaikai makasi, he all o'same picaninny belong you, altogether him finish, he die." Man-makasi he sing out: "All right, you look out, me go talk him shark, by and bye he kaikai along you." Him he go away along salt-water.

Tasa he go, he shoot him plenty fish, sun he go down, he put him up sail, he go quick along Mungeri. Big fellow wind he come, rain he come, plenty thunder and lightning he come, canoe he capsize, canoe he broke, Tasa he swim, he swim along. Shark he come, crocodile he come, Man-makasi he come, shark he catch him Tasa along head, crocodile he take him along leg, he pull, he pull plenty hard. Tasa he sing out, no man he come, by and bye he broke, he finish.

Makasi he laugh: him he go place belong him, he catch him another fellow woman: picaninny belong him he no die.

[Translation: Long ago, a man named Tasa went to Tomba in a canoe to spear fish. After a while a *makasi* (fish) came and he caught it, and put it in the canoe. Then another *makasi* came and put its head out of the water and called out: "Why did you spear my *makasi*-wife? All my children will die if she doesn't get their food." Tasa said, "But (in relation to what you said) if my children don't eat *makasi,* they're just like your children, they'll die." The *makasi*-husband shouted: "Well, watch out then, because I'm going to go and tell a shark, and he'll eat you." He disappeared into the sea. Tasa went on and caught a lot of fish, and when evening came he raised the sail to get back to Mungeri quickly. A strong wind came up, it started to rain, there was lots of thunder and lightning, and the canoe capsized

and broke, so Tasa had to swim for his life. The *makasi*-husband came, bringing a shark and a crocodile; the shark seized Tasa by the head, the crocodile seized him by the leg and pulled, pulled really strongly. Tasa called out, but before anyone could come he was torn in half, and that was the end of him. *Makasi* laughed and went to his place; he took another wife; so his children didn't die after all.]

If we transpose this text to a plausible Melanesian phonology,[18] only three minor changes distinguish this 1893 Solomons Pidgin from what one might record from older bush speakers in New Georgia or Malaita in 1989. In 1893 *wanem* (from English "what name") was being used as an all-purpose "wh" question marker. This is attested from other turn-of-the-century texts as well. In the twentieth century, a distinction emerged between *waswe* 'why?' and *wanem* 'what?'. In this text, *kaikai* 'eat' is used transitively with *long.* In this century, this has come to be expressed using the transitive suffix *(kaikai-em).* Finally, *tok-im* has been replaced by *tal-em.*

The commonalities with modern Solomons Pidgin vastly outweigh these minor contrasts. The Oceanic pronominal pattern, with the pronominal *i* following noun subjects and in *hem i* sequences, is exactly the one shown in my texts from older bush speakers (and which Mühlhäusler claims was not incorporated in Solomons Pidgin until the 1920s). The semantics of forms such as *kas-em* (to acquire s.t., to catch s.t., to reach a place) exactly correspond to contemporary usage. Forms such as *putimap* (and *leftemap*) can still be recorded from Solomons bush speakers. Note that by this time *olketa* was regularized both as plural marker and as third-person pronoun.[19] The use of *-fala* as a suffix to quantifiers and demonstratives *(wan-fala, nara-fala)* and some common attributive statives *(big-fala)* in this text exactly corresponds to present usage.

This text[20] confronts us inescapably with a key fact that underlies *MPOS:* The major expansions and stabilizations of Melanesian Pidgin had occurred by about a century ago. To account for this, we have to postulate either an extraordinarily rapid crystallization and stabilization of Melanesian Pidgin at the end of the 1880s or the sort of progressive development through the 1860s and 1870s I have proposed. If, as Siegel seems to recommend, we take the most conservative and skeptical readings of the texts from these earlier periods, the highly expanded Pidgin syntax of the early 1890s has to be viewed as having emerged almost overnight.

The other key thesis of *MPOS* is that the close grammatical parallels between Melanesian Pidgin grammar and the core syntax of Oceanic Austronesian languages suggest that substrate patterns--interacting with superstrate patterns and universal faculties of language simplification and second-language learning--had a strong historical impress on the development of Pidgin. This thesis remains unproven; but I see no compelling challenges to it in these reviews and no alternative answers to the puzzle with which *MPOS* begins, of how and why Solomon Islanders are able to calque Pidgin morpheme by morpheme onto their native languages.[21]

NOTES

1. And to thank Christine Jourdan for helpful comments.

2. I do not think the evidence yet allows us to dismiss, as Mühlhäusler does, the possibility of significant inputs from Chinese Pidgin English into the Pacific (or, indeed, into Australia).

3. There were substantial numbers of Pacific Islanders in the ports of eastern Australia from the early nineteenth century onward (p. 14). We cannot date the first salient linguistic connections between Australia and the Pacific Islands to the onset of the Labor Trade.

4. I agree with Siegel that we know much less than we would like to about the pidgin being used by ships' crews at the onset of the Labor Trade (which, I made clear in *MPOS,* was in 1865, not 1870). Not least of all, this is because most accounts by Europeans quoting fragments of Pidgin deal with their interaction with Pacific Islanders on shore; their renderings of Pidgin were usually included for their exotic cast or amusement value. The everyday Pidgin of the ships is almost completely absent in the archival records.

5. My speculations about this, as I note in *MPOS,* were largely a response to Bickerton's suggestion that there might have been an early creolization and subsequent repidginization. My conclusion is that while there may have been some nativization, this probably would have had few linguistic consequences (p. 228). I find it hard, having just reread pp. 33-34 of *MPOS,* to understand how I could be misread on this point.

6. These interpretations could be reinforced by further data from Anejom (Aneityum) and other Loyalties languages such as D(r)ehu.

7. I assign them no such role in *MPOS;* see p. 29.

8. I assign that date to a separation that in fact took several years to run its course; 1884-1889 would be more precise.

9. I must say, though, that I have always suspected that prepositional verbs do occur in Tok Pisin, a point strengthened by Romaine's observations. The crucial diagnostic prepositional verb, however, is "with"; and despite considerable searching I have found no sign of *weit-im* or equivalent in Tok Pisin, where *wantaim long* seems pervasively established.

10. Set out by Tryon in a paper presented at the 1988 Fifth International Conference on Austronesian Linguistics in Auckland.

11. In Keesing n.d.*b*, I note the recorded occurrence in the New Hebrides in the 1880s of *dis-fela* and *dat-fela* as demonstratives preceding nouns and as demonstrative pronouns.

12. *Pato* 'duck' was probably introduced into New Guinea, along with the waterfowl, by Polynesian missionaries.

13. For many Solomons speakers, as in Vanuatu, the long form is *babae.* I use Solomons/Vanuatu (Oceanic) phonology as further commentary on Tok Pisin hegemony.

14. I have many texts in which the same speaker is using long and short forms interchangeably, in the same slots.

15. It is also necessary to be extremely cautious regarding what constitutes a corpus. The transcripts of the various Queensland inquiries are particularly treacherous documents for counting and other purposes (moreso, I think, than Clark acknowledges in his interpretation of prepositions cited here). As I note in *MPOS* (pp. 151, 157), a number of different voices and registers are discernible within the transcripts; moreover, the court recorders were evidently trying to represent roughly what was being said in Pidgin while producing a legal document intelligible to English speakers. The compromises they reached seem to represent a very unevenly anglicized text, misleading with regard to prepositions and almost everything else.

16. He is not quite precise about dates for this.

17. Theoretical weight has also been assigned to his claims that use of a "they" pronoun as plural marker and use of transitive suffixes to form causatives have no motivation in substrate languages. In *MPOS,* I show that both patterns are pervasive in Eastern Oceanic languages (pp. 124-126, 127-130).

18. In using the conventional contemporary orthography here, I am making no claim that --either in 1893 or 1989--native speakers of Roviana who learned Pidgin as young adults used exactly the pronunciations represented in the now-conventional Solomons Pidgin orthography: only that the Pidgin forms Somerville writes in a way modeled on English orthography would have been bent to follow Roviana phonology in a way probably not very different from the Pidgin phonology of older speakers in New Georgia villages in 1989. Edvard Hviding (personal communication, 1989), who has been working on the use of Pidgin by contemporary New Georgians, has provided helpful information on this.

19. In the Solomons, *olketa* is recorded as a plural marker for inanimate nouns as early as 1880 (p. 129).

20. Unlike Pionnier's almost exactly contemporaneous texts from Malekula, Somerville's has the virtue of representing a Melanesian story told by a Solomon Islander, rather than a Catholic religious text.

21. In forthcoming papers (Keesing n.d.*a,* n.d.*c*), I have carried this argument further, showing that Solomons Pidgin as spoken in the western Solomons is bent so as to follow quite different substrate patterns and showing how calquing has historically shaped (and shortcut) the processes of grammaticalization being uncovered in "natural" languages.

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REVIEWS

John L. Culliney, *Islands in a Far Sea: Nature and Man in Hawaii.* San Francisco: Sierra Club Books, 1988. Pp. xiv, 410, maps, figures, bibliography, index. US$25.00.

Reviewed by Mark Merlin, University of Hawaii at Manoa

The ecology and evolutionary biology of the Hawaiian Islands have been recognized by scientists as extraordinarily interesting and valuable since Darwin's time. However, not since the appearance of Sherwin Carlquist's useful and widely read *Hawaii: A Natural History* (New York: Natural History Press, 1970; 2d ed., Lawai, Kauai: Pacific Tropical Botanical Garden, 1980) has anyone attempted to produce a single-volume, detailed survey on this broad, fascinating topic. Carlquist's landmark book focused primarily on terrestrial organisms and their habitats. An updated survey that included discussion of both the marine and land biota and ecosystems has long been needed.

The remote geographical isolation, environmental diversity, and relatively small habitat size help explain the remarkable and instructive degree of endemism, adaptive radiation, and adaptive shifts that characterize the native biota of Hawaii. Unfortunately, much of the rich and diverse endemic biota is lost forever or is severely threatened by a variety of human activities. Utilizing his extensive review of the literature and numerous personal communications with experts in various fields, Culliney has produced a sweeping survey of Hawaiian natural history. His "evocative" book celebrates the special natural heritage of Hawaii and chronicles the devastation that has occurred since humans first arrived in Hawaii. Culliney repeatedly emphasizes the importance of Hawaii as a living museum of biological adaptation and evolution in his examination of the many types of environments, from the deep

ocean surrounding Hawaii to the upper reaches of the high volcanic mountains. For each of the many Hawaiian marine and terrestrial ecosystems described, Culliney focuses on prominent species and key environmental variables, comparing them to related organisms or habitats elsewhere.

Islands in a Far Sea has a strong conservation theme and is aimed at an undergraduate audience. Both amateur and professional readers will benefit, however, from the wealth of information that the author has marshaled together here. For many, this comprehensive overview will serve as a thoughtful, challenging introduction into the unique natural history of Hawaii and the severe ecological changes and loss of biodiversity that humans have caused in the islands. For others, Culliney's opinionated commentary may seem unfair and perhaps inappropriate. Although by no means an objective treatise, this book informs the reader and challenges him or her to consider the consequences of human impact on the environment--both past and present.

Although this book is crammed with information derived from a broad archival investigation of nineteenth-century literature and an up-to-date, thorough review of relevant scientific journals, the referencing of sources is frustrating. Notes are grouped by chapter at the end of the text and one has to keep returning to the back of the book to determine where Culliney found his data or who shared their personal expertise on Hawaiian natural history with him. Even though the book is generally well edited, there are a few typographical mistakes. For example, the genus of large native tree ferns, *hapuu,* is misspelled as *Cybotium* on pages 199 and 340. There are also some informational errors. For example, Culliney refers to *pili* grass *(Heteropogon contortus)* as an "endemic species widely used for thatching houses" (p. 328). This important grass in prehistoric Hawaii is certainly not endemic, possibly not even native, to the Hawaiian Islands. Culliney might also have noted that this herbaceous species was used for mulching purposes, especially in the cultivation of the sweet potatoes. Hence, some prehistorians and ethnobotanists assume that fire was used to encourage an increase in the availability of this useful, perhaps indigenous, grass--at the expense of many endemic trees, shrubs, and other plants.

Another, more discouraging, problem in Culliney's book involves the quantity and quality of the illustrations. He does offer the disclaimer that "budgetary limitations and structural constraints" precluded the inclusion of sufficient figures that could have further enhanced the impact of this commendable survey. To his credit, Culliney refers the reader to a variety of other "sources" where adequate photographs and

line drawings of the native and alien plants and animals of Hawaii can be found. These are listed in his "Supplementary Sources for Illustrations" near the end of the book. Nevertheless, if there is to be a second edition, the author should try to convince his publisher that more and better illustrations will significantly increase the value of his splendid effort.

In spite of the reference and illustration problems, I strongly recommend this book to those who wish to learn more about the truly special natural history of Hawaii and the regrettable aspects of human use of land and water in the archipelago.

Bob Krauss, *Keneti: South Seas Adventures of Kenneth Emory*. Honolulu: University of Hawaii Press, 1988. Pp. 472, illus., index. US$35.00.

Reviewed by Bengt Danielsson, Papehue, Tahiti

Having previously never written any book reviews for this journal, I was quite surprised when I received a request a little while ago to put down on paper what I think about Bob Krauss's biography of the grand old man of Pacific science, professor Kenneth Emory. But it soon appeared to me that the book review editor of *Pacific Studies* must be aware of my close association with Keneti, as he is called by Pacific islanders, over the past forty years.

In 1949-1951, when I was doing my first fieldwork in Polynesia, he became my faithful correspondent and mentor. While preparing my notes for publication in Honolulu in 1952-1953, he kindly gave me free access to his own field notes from the Tuamotu islands, where he had undertaken pioneering studies twenty years earlier. Subsequently he saw to it that I was appointed a British Museum honorary associate in anthropology, which meant that my link with him became permanent. After my French wife, Marie-Thérèse, and I had settled for good in Tahiti in 1954, Kenneth and his French-Tahitian wife, Marguerite, were frequent guests in our Papehue home. Alternatively, we stayed with them in their home in historic Nuuanu Valley on many occasions, during which they told us fascinating stories about their life. From 1962 on, I often assisted Kenneth and his favorite pupil, Yosi Sinoto, during their annual diggings in the Society Islands, which resulted in a long series of epoch-making archaeological discoveries. Last but not least, I spent much time in 1967 gathering material and interviewing people

about Kenneth's career to comply with a request by Bishop Museum trustees to write the introductory biographical chapter for a festschrift presented to Emory on his seventieth birthday.

These must be the reasons, I conclude, for asking me to pass judgment on this book, and I must confess that I feel quite qualified to do so. My first reaction is to say that since my friend Kenneth has never felt compelled to write down his own memoirs, despite much encouragement to do so, we must all be extremely grateful to Bob Krauss for having undertaken this task in close cooperation with him and in such a congenial manner that the reader almost has the feeling that this is an autobiography. Nevertheless, it is better, I think, that the narrator is not Kenneth, for his extraordinary modesty would have made him reluctant, if he had been the author, to give himself full credit for all his pioneering work and to appear as the hero he often was in the numerous battles he has been involved in. Incidentally, it is not at all surprising to us, his friends, who have always admired him for his almost saintly devotion to the cause of science, that in his youth he toyed with the idea of becoming a missionary--in the Pacific, of course.

As all authors and readers of biographies will admit, it is practically impossible to write a four hundred-page book that contains no factual errors, but this is what Bob Krauss has managed to do. His achievement is the more remarkable considering that the story he is telling spans ninety years and the whole Pacific. Once more I base this judgment on personal experience, that is, the way in which Krauss used the information I supplied during several long interviews. Most important, however, is the skill with which he has managed throughout to concentrate on and emphasize the crucial events and facts, without neglecting to sprinkle his narrative, as a good writer should, with amusing anecdotes. To sum up, the author has produced a splendid book of great documentary value that will be used and read for pleasure by professionals and amateurs alike interested in the Pacific islanders, their history, and their culture. It is moreover illustrated with well-chosen old photographs.

If we want to assess Emory's achievements correctly, the best approach in my opinion--obviously shared by Krauss--is to compare them to those of his contemporary colleagues Peter Buck, Raymond Firth, Craighill Handy, and Herbert Gregory. What makes him *primus inter pares* is no doubt his enormous versatility, since he has been in turn or at the same time an ethnohistorian, a social anthropologist, a linguist, an artifact specialist, and an archaeologist. His most amazing achievement is, of course, his metamorphosis in 1950 into a first-rate archaeologist, who discovered in the Kuliouou cave on Oahu rich

deposits that yielded the first Polynesian carbon date. This gave the impetus to the numerous later diggings elsewhere in the Pacific, which made it possible to reconstruct the prehistory of the islanders with an accuracy never attained by the sort of comparative studies based on the diffusion of cultural elements that had been prevalent during the first half of this century. Because Emory has constantly been the precursor who has set the trend, his life story offers the additional attraction that it is likewise a general history of all scientific work undertaken in the cultural field by the whole body of Pacific scholars.

My last remarks, this time highly critical, concern the peculiar lack of appreciation Emory has sometimes encountered. For instance, how does it come, we must ask, that an eminent American scientist like him has had to endure all his life such an incredible poverty and spend so much of his valuable time trying to raise money for his epoch-making field trips? Having often met him in remote islands in the South Pacific, I have each time been appalled by the sort of beachcomber or Robinson Crusoe life he has been forced to lead for lack of funds--of course, without ever complaining. Strangely enough, Keneti has not been more gallantly treated by the government officials in the *patrie* of his wife, French Polynesia, where he has carried out over a period of fifty years the major anthropological, linguistic, and archaeological investigations that form the basis for the present educational work, tourist shows, and politically motivated search for their roots by the lost generation of Polynesian youths. Yet, Kenneth Emory's tremendous contributions are never praised or even mentioned in official speeches and publications, no street in Papeete is named after him and no commemorative plaques have been installed. Let us hope that this unfair neglect will, to some extent, be remedied by the prompt translation of Krauss's outstanding and highly readable biography into French.

BOOKS NOTED

"Books Noted" is a new section starting in this issue. It is designed to acquaint scholars and others with the most significant new Pacific Islands publications that are accessible through major libraries and institutions, selected for their interest to readers of *Pacific Studies.* The list for this issue was produced from acquisitions lists of Pacific Islands materials from the University of Hawaii at Manoa, Brigham Young University--Hawaii, and the Bernice P. Bishop Museum for 1988 and early 1989. Other libraries are invited to submit current acquisitions lists for future installments. Contributions and queries should be addressed to the Books Noted Editor, Riley Moffat, Joseph F. Smith Library, Box 1966, Brigham Young University--Hawaii, Laie, Hawaii 96762.

Following library conventions, this short list includes only the primary author in cases of multiple authorship and is alphabetized by title in cases of an edited or compiled work.

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