PACIFIC STUDIES

a journal devoted to the study of the Pacific--
its islands and adjacent countries

SPRING 1984

Anthropology
Archaeology
Art History
Ethnomusicology
Folklore
Geography
History
Sociolinguistics
Literary Criticism
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Sociology

Published by

INSTITUTE FOR POLYNESIAN STUDIES
(Brigham Young University--Hawaii Campus)

Funded by the
Polynesian Cultural Center

EDITORIAL BOARD

PACIFIC STUDIES

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ISSN 0275-3596

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PACIFIC STUDIES

Vol. VII, No. 2 Spring 1984

CENTRAL CAROLINIAN ORAL NARRATIVES: INDIGENOUS MIGRATION THEORIES AND PRINCIPLES OF ORDER AND RANK*

by William H. Alkire

Within anthropology two competing theories have been proposed concerning the original dispersal of Austronesian peoples in Micronesia and Polynesia. The first is the so-called "northern route theory" put forward by Peter Buck (1938a:47) and revived in a more complex form by William Howells (1973).[1] This theory is so named because it proposes that the original inhabitants of Micronesia entered the area from the west (i.e. Indonesia and/or the Philippines) proceeding to occupy the various islands of the region as they moved on to the east. Eventually such migrants entered Polynesia from the eastern extremity of Micronesia thus bypassing most of Melanesia which lay to the south. In contrast is the "southern route theory" delineated by George Grace (1961) and supported by Shutler and Marck (1975). This hypothesizes that the Austronesian peoples entered Oceania via the islands of Melanesia. If so, the ancestral populations I hat settled Micronesia most likely arrived from the southeast and then occupied the islands of the region from east to west.

In earlier years traditional narratives were frequently used, albeit with caution, as one type of supporting evidence for these theories.[2] More recently they have not played an important role in such studies largely because the tales contradict the accepted models of settlement derived from linguistics and archaeology.[3] However, it should be noted that the linguistic/archaeology model *is* the southern route theory. Therefore with the recent revision and revitalization of a northern route theory, the oral narratives might be taken as support for this hypothesis which otherwise has depended primarily on physical anthropological evidence for confirmation.[4]

This paper will reexamine those tales of the central Carolines that include migration motifs and demonstrate that such stories cannot easily be utilized for such purposes. I reached this conclusion because the narratives appear to follow a consistent pattern of development that suggests a degree of "tailoring" to conform to important principles of order and rank that obtain between the societies of the region. As a general conclusion, of course, this is not unique. Raymond Firth (1961:168-83) clearly detailed how traditional narratives on Tikopia were used to validate the social order on that Polynesian island. The central and western Carolinian narratives discussed in this paper demonstrate that parallel processes operate in Micronesia and that the "tailoring" follows some very specific lines.

Linguistic and Archaeological Evidence of Settlement

To date linguists have provided the most complete model of hypothesized population movements within this region of Oceania (Matthews 1951; Grace 1961; Dyen 1965; Shutler and Marck 1975). They have concluded that the nuclear Micronesian languages are most clearly related to the Austronesian dialects of eastern Melanesia.[5] Thus migrations into Micronesia, as stated above, probably came from the east moving to the west. Within the area of immediate concern, an easily identifiable linguistic chain exists linking all islands from Ulithi to Woleai to Lamotrek and on to Truk (Quackenbush 1968). The languages of western Micronesia--Palauan, Chamorro, and Yapese--are not closely related to nuclear Micronesian nor, for that matter, to each other. Given these relationships it seems most likely that the coral islands between Yap and Truk were settled by migrants moving from the region of Truk.

Very little archaeology has been done in the central Carolines and that which has occurred is quite recent (see Craib 1983). Until additional work fills in some important gaps and resolves a number of inconsistencies, the evidence that has been accumulated is subject to varying interpretations. Nevertheless, work on Truk indicates that those islands were settled by 2000 B.C. (Shutler, Sinoto, and Takayama 1978:97). The earliest reliable date from Lamotrek demonstrates that the island was inhabited by 1000 A.D., and possibly as early as 300 A.D., although we are not convinced of the accuracy of this latter date.[6] Radiocarbon dates from Mogmog, Ulithi, indicate that this atoll was settled by 400 A.D. (Craib 1980:198). The earliest radiocarbon date relating to human settlements on Yap is 176 A.D., obtained by Gifford and Gifford (1959:200). Given the significantly older Trukese dates, the archaeological work thus far com-

pleted does not contradict the above linguistically derived model.[7] The remainder of this paper will try to determine if the traditional narratives have anything to contribute to this question.

The Oral Traditions: Settlement and Resettlement[8]

Of the sixty-six narratives published by Lessa only two make reference to the original settlement of islands. The first (no. 5) "implies that Fais was populated from Ulithi," after that island was fished from the sea (Lessa 1961:35).[9] The second (no. 9) tells how Ngulu was settled. In this narrative a Ulithian man, Halengloi, visits Yap and renders outstanding service first to his Yapese patron and then to his host in Guror. Consequently, he is rewarded with the gift of a wife. Halengloi expresses a desire to his new wife on four separate occasions to eat a particular type of fish *(likh).* He is told that if he wants this type of fish he should go out fishing for it. He then sets out with his wife and several other men:

> Now Halengloi was a pelu, or navigator. He had never before been to the island of Ngulu but he knew about it. The people from Yap, however, did not. Halengloi . . . sailed far to the south. . . . Suddenly, all the people on the canoe shouted, saying there was something in the distance. Halengloi told them it was the island of Ngulu. . . .

After returning to Yap, the island is given to the Guror chief (Halengloi's host) by the Gatchepar chief (Halengloi's patron) and the former then permits Halengloi and his wife to return to Ngulu as settlers.

> This is why Ngulu belongs to the chief of Guror in the district of Galiman on Yap. And this is why the people of Ngulu have the customs of both Ulithi and Yap and also speak these languages. For their ancestors came from there.

In the Woleai, Ifaluk, and Lamotrek region oral narratives gathered by Sarfert, Hambruch (in Damm 1938), Burrows (1949, 1963; with Spiro 1953) and Spiro (1951) have been published. Several of those gathered by Burrows are most relevant to the topic under discussion in this paper.

Burrows (1949:151-53; also in Burrows and Spiro 1953:7) provides an account of the first settlement of Ifaluk. It is of such importance as to warrant reproducing here:

> Long, long ago, a chief of Garpar (Gatschapar) village, in Gagil district, Yap, ordered some of his people to go out and colonize the outer islands to the east. He himself remained in Yap. In charge of the expedition was a man named Tatar, who was accompanied by his sister, Iau. They went first to Mogmog on Ulithi; then to Wetegau in Woleai, then to Ifaluk and the other islands--Faraulep, Elato, Lamotrek, Satawal, and so on to Puluwat and Truk. The chain of command, ever since, is from Yap to Mogmog, from Mogmog to Wetegau, Wetegau to Ifaluk, from Ifaluk to the other islands.
>
> On Ifaluk Tatar left one man and one woman from each of the eight clans. The two from each clan were brother and sister.

Safert apparently makes reference to this same narrative, gathered during his work on Ifaluk in 1909 (Damm 1938:79, 83-85). In his account, however, the first settlers on Ifaluk were a man named Modj and his wife. Modj (Mosh or Maur) is an important legendary figure who appears in several other tales, most of which deal with interisland warfare (Burrows and Spiro, 1953:10-16; Burrows 1963:72-77). I shall discuss these later.

The most important points to emphasize from the three tales thus far cited are that they specify that the outer islands--Ngulu, Ulithi, Woleai, Ifaluk, Faraulep, Elato, Lamotrek, Satawal, and Puluwat--were settled from Yap; and secondly, owing to this, these outer islands are subservient and owe allegiance to Yap. This political charter and the derivative status ranking prevailed in the region until recent years (actively until ca. 1910).

There are no other published narratives dealing with the settlement of these islands, but there are a number concerned with resettlement (those relating to warfare mentioned above) that have relevance to this discussion. Burrows (1963:72-77; Burrows and Spiro 1953:10-18) again has published the most extensive tales dealing with the topic. In these narratives, gathered on Ifaluk, the recurrent theme is one of neighboring islands conquered by, and resettled from, Ifaluk. For example, one tells of Mosh (Maur) who travels to Woleai and marries there. The Woleai men are jealous of his success with a local woman and therefore beat Mosh and leave him for dead; but Mosh revives and makes his way back to Ifaluk. The Ifaluk men then set out to avenge this beating:

> They went to Woleai in many canoes--two hundred, three hundred.... Everywhere they attacked the Woleai people and speared them, men, women and children.

After a prolonged chase between islands of the atoll and the use of various strategies:

> The Ifaluk men caught the rest of the Woleai men during a bonito drive, attacked them and killed them. Then they went on to Falalus and killed the women and children too. The canoes returned to Ifaluk, leaving only Ilimeng and her boy on Woleai (the Woleai wife and son of Mosh, who had been spared). The people had a great feast at home . . . then . . . Mosh told them it would be too bad to have no people on Woleai. Then he sent people from Ifaluk to settle in Woleai:
>
> > One man and his sister from the clan Kovalu.
> > One man and his sister from the clan Sauvelarik.
> > One man and his sister from the clan Mangaulevar.
> > One man and his sister from the clan Rapevelu.
> > One man and his sister from the clan Sauvel.
> > One man and his sister from the clan Bwel.
> > One man and his sister from the clan Kailangailuk.
> > One man and his sister from the clan Kailangalualea.
>
> Each of the men became a chief in Woleai. Little boys went too with the women, their mothers. These people with Mosh's wife and son, repopulated Woleai.

Ifaluk also had a war with Lamotrek. According to that narrative the war was precipitated by provocations and depredations on the part of Lamotrek, so:

> Mosh called his people together. . . . He told them to make ready, for they were going to make war on Lamotrek. . . . [The Ifaluk men] came first to Elato, but the Elato people, when they saw the Ifaluk fleet coming . . . fled to Lamotrek.

The Ifaluk warriors then attacked Lamotrek using a strategy of dividing forces and attacking from behind a smoke screen:

> The Lamotrek men fought bravely against the party that had come in behind a smoke screen. But they were at such a disadvantage with the smoke in their eyes, that they were driven back and at last fled into the bush. Here the other Ifaluk party was lying in wait. They fell upon the disordered defenders and

> killed every last one of them, then went into the houses, killing, killing, until not a man, woman, or child was left alive. So the fleet returned to Ifaluk and reported to Mosh that Lamotrek has been depopulated, and . . . Ifaluk . . . avenged.
>
> Then. . . [Mosh] told them it would not be good for Lamotrek to remain without people. He ordered each clan to send one man and his sister. All these could take their families along if they wished. So Lamotrek was repopulated from Ifaluk.

During my own work on Lamotrek (1962-1963) and Woleai (1965), I collected these same two tales, but my versions were less detailed and did not include the passages concerning the sibling-sets that putatively resettled the conquered islands. My versions did state that the Ifaluk conquerors killed the original inhabitants, albeit in a less traumatic fashion. And, importantly, I believe, my versions emphasized that the first of the new settlers represented the historically important chiefly clans of the island.

The Recurring Themes

In several earlier publications I have noted that central Carolinians commonly structure various domains according to a few organizing principles (Alkire 1970, 1972, 1982). One of the more frequently encountered of these principles is that of *quadripartite divisions.* This emphasis on units of four appears in many narratives including those tales collected by Lessa quoted earlier.

A second principle important to central Carolinians is that of the *solidarity of siblings* (Marshall 1981; Alkire 1978b). Certainly the tales collected by Burrows on Ifaluk emphasize this. In each case of settlement and resettlement Burrows' informant stated that sibling-sets were involved. From these examples it seems clear that central Carolinians *do* make an effort to incorporate important cultural themes or organizational principles into their oral narratives.

It is my contention that a third theme is also emphasized in these narratives, a principle I would label *priority of settlement.* Furthermore, the inclusion of this theme has led to the tailoring of narratives so that they are of limited use in reconstructing cultural history.

Status and rank are of great importance in most of Oceania. In the central Carolines, seniority (e.g., chronological age, generation standing, lineage, and subclan seniority) and control of land are two interrelated

variables that are used to establish relative rank (Alkire 1965:32; Alkire 1978a:117). These variables are interrelated because those social groups that first settle on an island or in a particular locality are the ones that first invoke ownership rights to surrounding lands.[10] For example, in recording the individual histories of clans on both Lamotrek and Woleai, I found that those clans of highest rank invariably were the ones that either controlled the most land or claimed to have once done so. Given the apparent importance of this principle, it seems quite likely that social groups of current high rank could seek to validate or legitimize their claims to such rank by emphasizing some form of historical priority and seniority. The oral narratives cited in this paper contain elements that support this hypothesis.

For example, the narratives collected from Ifaluk informants were clearly constructed to emphasize the status-superiority of Ifaluk. Not only was Ifaluk described as having been settled earlier than neighboring islands, but in addition present-day populations on such neighboring islands were said to have derived *from* Ifaluk. Of importance in this regard is the fact that the tales of conquest and resettlement always emphasized that the original population of the conquered island was completely annihilated; thus no present-day residents could claim descent from settlers who had some other historical priority on the island. In those versions collected on the conquered islands, conquest and annihilation were not denied but priority in resettlement was given to currently high ranking clans (and the clans of informants).

The importance of a principle of priority of settlement is further illustrated by the *sawei* exchange system that linked Yap to all of the outer islands (Alkire 1965). The islands of this system were roughly ranked according to their distance from Yap and, therefore, their presumed order of settlement from Yap--the legendary homeland of the original migrants to the outer islands (see the first of Burrows' legends). In other words, if a priority of settlement principle is not to be contradicted by sociopolitical realities, then a legendary charter of the system would have to hold that the outer islands were settleld from Yap, the highest ranking island of the system. In this context it is interesting to note that on Lamotrek one also finds "contradictory" narratives relating to the origins of some clans. In these stories such clans are described as having come from the east (frequently Kusaie). On Puluwat (to the east of Lamotrek and on the periphery of traditional Yapese control) Gladwin (1970:4) implies that all islanders trace their origins to Truk. The principle seems to be that narratives gathered on those islands closer to Yap and Yapese control are both more consistent and more detailed in identifying Yap as the ancestral homeland.

Historical Evidence: The Saipan Case

One final body of data can be drawn upon to underscore the importance of priority of settlement. These are data relating to the Carolinian community settled on Saipan in the Mariana Islands.

Historical and archaeological evidence have established that Saipan was inhabited at the time of first European contact by the Chamorro people who dwelt throughout the Marianas (Thompson 1945; Spoehr 1954, 1957; Craib 1983:923). Following a series of Spanish-Chamorro conflicts those Chamorro who survived the bloody wars were removed to Guam in the early 1700s. Saipan was thus without any permanent residents from that time until 1815 when a group of Carolinians was given permission by the Spanish authorities to settle on the island. It was not until some fifty years later that Chamorros began to return to the island in any number. It is of interest to this paper that members of the present-day Carolinian community of Saipan now emphasize a number of legends that give their community historical priority on the island.[12]

Today Carolinians on Saipan are outnumbered three to one by Chamorro residents. There is resentment within the Carolinian community about their lack of political and economic power on the island that derives from their minority status. Members of the community have additional reasons for dissatisfaction at this state of affairs for, in their minds, it is contradicted by the priority of settlement principle. According to their oral traditions the Carolinians were the *first settlers* on the island. A manifestation of this history, made tangible, is a concrete marker erected on Managaha islet at the entrance to Saipan's Tanapag Harbour:

> This marker commemorates King Agurup c. 1785-1850, founder of the first permanent colony on Saipan after the Spanish conquest. The colony was founded in 1815 by settlers from Satawal and was named Seipon. King Agurup's body was laid to rest on this island.
>
> Erected by the clan of King Agurup and friends Sept. 18, 1970.

(The inscription is then followed by a list of Carolinian sponsors and subscribers to the project.)

One should note that the inscription states that Saipan itself was named Seipon by the Carolinians, and this is explained as a Satawalese compound whose formal meaning is empty place or empty container, i.e. an uninhabited place. This contention of Carolinian priority is further em-

phasized by informants who cite the numerous Carolinian place names on the island: Tanapag, Oleai, Garapan, and Halahal (Managaha), all Carolinian labels for important locations on the island. Furthermore, Carolinians expressed bitter resentment when the Saipan municipal government (dominated by Chamorros) changed the name of Oleai Village (named after Woleai Island) to San Jose Village.

The monument, of course, makes reference to the Carolinians only as the "first permanent colony on Saipan after the Spanish conquest," but more recently informants have projected the Carolinian presence further into the past by listing place names on other Marianas islands that suggest a "Carolinian origin" predating 1815. On Guam, Umatac "blazing oven" and Mongmong "arrowroot" are such examples while on Tinian the name of the island itself, like Seipon, is interpreted as a Carolinian word, meaning "rising sun".[13] One informant had even worked out an extensive migration itinerary whereby the Carolinians arrived en masse in the Marianas from Jerusalem via Pakistan, Malaya, Java, Kusaie, and the central Carolines.

Conclusions

In conclusion, there is a body of evidence that demonstrates that Carolinian oral narratives and traditions are frequently structured so that they incorporate important cultural themes, elements, and principles. Examples of these include a focus on sibling-sets and on quadripartite divisions or tetradic groupings. Most important to this paper, however, are the narrative consequences of a cultural emphasis on status and rank and the direct relationship these variables have to seniority and priority of settlement. Regardless of the actual historical order of settlement on these islands, when a socioeconomic system linking various islands became established and differential rank within and among the islands became an important part of that system, then it became obligatory for the traditional charters--i.e., myths and legends--to be tailored to conform to the principle that populations of lower ranking islands were derived from the islands of higher rank and that clans of lower rank must have arrived after clans of higher rank. This suggests that oral narratives from this area of Micronesia can only be used with extreme caution in reconstructing cultural history. This conclusion agrees with those of writers who have examined similar problems in other areas of Oceania.

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NOTES

*An earlier version of this paper was read at the 35th Annual Northwest Anthropological Conference held at Simon Fraser University, Burnaby, British Columbia, April 9, 1982. I want to thank both the participants in that conference and the anonymous reviewers for *Pacific Studies* who made suggestions regarding revisions of this article. I also want to acknowledge that my research in the Yap and Saipan areas of Micronesia has been supported over the years by grants from NIMH, NSF, and the Canada Council, the Social Sciences and Humanities Research Council of Canada, and the University of Victoria.

1. Whereas Buck described the migration through the Carolines as a relatively simple west to east movement, Howells (1971:260-61) suggests that during the initial west to east movement only the larger high islands of the archipelago were settled. His contention is that the low coral islands were settled later by migrants moving out in various directions--some east to west--from these older, high island population centers. Given the fact that the high islands of the Carolines are quite scattered and the coral islands more numerous, I find it difficult to accept that early migrants would have missed or bypassed all of the coral ones, especially since they had no way of knowing they would find any more suitable volcanic ones.

2. Some examples of early ethnographies, besides Buck 1938a, that place considerable emphasis on traditional narratives include Buck 1938b:14-96; Buck 1958:1-73; Burrows 1936:27-56; Burrows 1937:17-41. In addition, of course, Thor Heyerdahl (1952:709-763) emphasized traditional narratives in his discussion of the settlement of Easter Island. His "tendency to see myths as texts possessing strict historical validity" is criticized by Metraux (1957:225).

3. In addition, central Carolinian oral traditions emphasize mythology rather than legend (cf., Spiro 1951:289). In myths one generally is presented with characters interacting without reference to specific localities other than heaven and earth. Those tales that are specific, about locations generally focus on the adventurous and/or amorous escapades of mythical beings and the need for filial or sibling loyalty.

4. In Buck's (1938:47) earlier discussions he emphasized phenotypic similarities between Micronesians and Polynesians. Howells (1973:79) bases some of his conclusions on blood types and enzyme similarities. See Simmons et al. (1965:152) for a different interpretation of blood type and genetic evidence.

5. "Nuclear Micronesian" refers to those Micronesian languages that show close resemblance in phonology and lexical items. These include the languages of the Gilberts (Kiribati), Marshalls, and the Carolines, but not Chamorro, Palauan, Yapese, or Nauruan, all of which are significantly different.

6. Similarly we do not have confidence in the 3310± 85 B.P. date of specimen N-3125 for the reasons detailed in Fujimura and Alkire 1979:72-77.

7. If one discounts those dates from Truk, and eliminates the questionable early dates from Lamotrek, the archaeology can still be interpreted as suggesting that the outer islands were settled from Yap. Consequently, much more work remains to be done before archaeology provides complete answers to these questions.

8. Oral narratives on Ulithi have been published by William Lessa (1961, 1980). E.G. Burrows (1949; with Spiro 1953) and M.E. Spiro (1951) gathered material in fieldwork on Ifaluk.

The only other collections on this region were published by A. Krämer (1937), E. Sarfert, and P. Hambruch (in Damm 1938) who were ethnographers on the German Southseas Expedition of 1908-1910. This writer has gathered a number of myths and legends on Woleai, Lamotrek, and Faraulep, but they remain unpublished.

9. The core of this legend involves three brothers who go out from Ulithi fishing. On three Successive days the two older brothers catch fish while the youngest (Motikitik) catches baskets of food:

> On the *fourth* day, the older brothers caught fish as usual but Motikitik caught something else. . . . Motikitik pulled up an island to the surface of the water and their canoe was right in the middle of the island, which was Fais. . . . *Motikitik said the middle of the island belonged to him, but he told his brothers that one could live on each end of the island.* (Lessa 1961:36. Emphasis added--relevant to subsequent discussions and note 10.)

10. The legend mentioned in note 9 above makes this point in the italicized passage.

11. It is interesting to note that on at least three occasions during the last twenty-five years some of the inhabitants of the Lamotrek/Elato/Satawal area have talked about trying to cut their administrative ties with Yap in order to amalgamate either with Truk or some other unit of the Trust Territory.

An anonymous reviewer for *Pacific Studies* has also made the interesting point that, in contrast to the outer islanders, Yapese and Palauans emphasize an autochthonous rather than a migratory origin for their peoples.

12. I have discussed some political and cultural aspects of this situation in "The Carolinians of Saipan and the Commonwealth of the Northern Mariana Islands," in press (revision of Alkire 1983).

13. These issues are also discussed in Alkire 1983.

LITERATURE CITED

Alkire, William H.

1965 *Lamotrek Atoll and Inter-island Socioeconomic Ties.* University of Illinois Press, Urbana.

1970 "Systems of Measurement on Woleai Atoll, Caroline Islands," *Anthropos* 65: 1-73.

1972 "Concepts of Order in Southeast Asia and Micronesia," *Comparative Studies in Society and History* 14:484-93.

1977 *An Introduction to the Peoples and Cultures of Micronesia.* 2nd Edition. (1st Edition, 1972.) Cummings Publishing Co., Menlo Park,

1978a *Coral Islanders.* AHM Publishing Corp., Arlington Heights, Ill.

1978b "Siblingship and Lines of Authority in Central Carolinian Atoll Society." Unpublished paper read at ASAO Annual meetings, Asilomar, Calif.

1982 "The Traditional Classification and Treatment of Illness on Woleai and Lamotrek in the Caroline Islands, Micronesia," *Culture* 2:29-41.

1983 "Carolinian Ethnic Identity and Saipan Political Realities." Unpublished paper read at the XV Pacific Science Congress, Dunedin, New Zealand, Feb. 2, 1983.

Buck, Peter

1938a *Vikings of the Sunrise.* Lippincott, Philadelphia.

1938b *Ethnology of Mangareva.* Bernice P. Bishop Museum Bulletin 157. Bishop Museum Press, Honolulu.

1958 *The Coming of the Maori.* Whitcombe and Tombs, Wellington, N.Z.

Burrows, E.G.

1936 *Ethnology of Futuna.* Bernice P. Bishop Museum Bulletin 138. Bishop Museum Press, Honolulu.

1937 *Ethnology of Uvea.* Bernice P. Bishop Museum Bulletin 145. Bishop Museum Press, Honolulu.

1949 *The People of Ifalik: A Little-Disturbed Atoll Culture.* Final Report, CIMA, Washington, D.C. Pacific Science Board, National Research Council. (Also found in HRAF files.)

1963 *Flower in My Ear: Arts and Ethos of Ifaluk Atoll.* University of Washington Press, Seattle.

Burrows, E.G. and M.E. Spiro

1953 *An Atoll Culture: Ethnography of Ifaluk in the central Carolines.* HRAF Press, New Haven.

Craib, John L.

1980 *Archaeological Survey of Ulithi Atoll, Western Caroline Islands.* Pacific Studies Institute Monograph No. 1, Agana, Guam.

1983 "Micronesian Prehistory: An Archaeological Overview," *Science* 219:922-27.

Damm, Hans

1938 *Zentralkarolinen.* Ergebnisse der Südsee Expedition 1908-1910. Halbband II. Friederichsen de Gruyter, Hamburg.

Dyen, Isadore

1965 *A Lexicostatistical Classification of the Austronesian Languages.* International Journal of American Linguistics, Memoir No. 19. Indiana University Publications in Anthropology and Linguistics, Bloomington.

Firth, Raymond

1961 *History and Traditions of Tikopia.* Memoir no. 32. The Polynesian Society, Wellington, N.Z.

Fujimura, Keiko and W.H. Alkire

1977 "Recent Excavations on Three Atolls in the Caroline Islands: A note," *Journal of the Polynesian Society* 86:413-14.

1979 *Archaeological Test Excavations on Faraulep, Woleai and Lamotrek in the Caroline Islands of Micronesia.* Final Report, Historic Preservation Office, Trust Territory of the Pacific Islands, Saipan. (Also in press, Bishop Museum.)

Gifford, E.W. and D.S. Gifford

1959 *Archaeological Excavations in Yap.* Anthropological Records, vol. 18, no. 2 University of California Press, Berkeley.

Gladwin, Thomas

1970 *East Is a Big Bird.* Harvard University Press, Cambridge, Mass.

Grace, George
1961 "Austronesian Linguistics and Culture History," *American Anthropologist* 63:359-68.

Heyerdahl, Thor
1952 *American Indians in the pacific.* George Allen and Unwin, London.

Howells, William
1973 *The Pacific Islanders.* Scribner's, New York.

Krämer, Augustin
1937 *Zentralkarolinen.* Ergebnisse der Sudsee-Expedition, 1908-1910. Halbband I. De Gruyter, Hamburg.

Lessa, William A.
1961 *Tales from Ulithi Atoll: A Comparative Study in Oceanic Folklore.* Folklore Studies, no. 13. University of California Press, Berkeley.
1980 *More Tales from Ulithi Atoll: A Content Analysis.* Folklore Studies, no. 32. University of California Press, Berkeley and Los Angeles.

Marshall, Mac
1981 "Sibling Sets as Building Blocks in Greater Trukese Society." In *Siblingship in Oceania: Studies in the Meaning of Kin Relations,* ed. Mac Marshall, chapter 7. ASAO Monograph no. 8. University of Michigan Press, Ann Arbor.

Matthews, W.K.
1951 "Characteristics of Micronesian," *Lingua* 2:419-37.

Metraux, Alfred
1957 *Easter Island.* Trans. M. Bullock. Andre Deutsch, London.

Quackenbush, E.M.
1968 "From Sonsoral to Trnk: A Dialect Chain." Unpublished Ph.D. thesis, University of Michigan, Ann Arbor.

Shutler, Richard, Jr., and J.C. Marck
1975 "On the Dispersal of the Austronesian Horticulturalists," *Archaeology and Physical Anthropology in Oceania* 10 (2):81-113.

Shutler, Richard, Jr., Y.H. Sinoto, and J. Takayama
1978 *Fefan Island Survey and Mitigation Project.* National Park Service Report. National Technical Information Service, Washington, D.C.

Simmons, R.T., J.J. Graydon, D.C. Gadjusek. and P. Brown.
1965 "Blood Group Genetic: Variations in Natives in the Caroline Islands and Other Parts of Micronesia," *Oceania* 36:132-70.

Spiro, M .E.
1951 "Some Ifaluk Myths and Folk Tales," *Journal of American Folklore* 64:289-302.

Spoehr, Alexander
1954 *Saipan: The Ethnology of a War-Devastated Island.* Chicago Natural History Museum, Fieldiana Anthropology, 41.
1957 *Marianas Prehistory: Archaeological Survey and Excavations on Saipan, Tinian, and Rota.* Chicago Natural History Museum, Fieldiana Anthropology, 48.

Takayama, J. and M. Intoh
1978 *Archaeological Excavation at Chukienu Shell Midden on Tol, Truk.* Reports of Pacific Archaeological Survey, No. 5. Tezukayama University, Nara, Japan.

Thompson, Laura
1945 *The Native Culture of the Mariana Islands.* Bishop Museum Bulletin 185, Honolulu.

THE IMPACT OF MODERNIZATION ON THE AGED IN AMERICAN SAMOA

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ABSTRACT.Research was conducted in American Samoa in 1976 to investigate the effects of modernization on the aged in recent years. Interviews with eighty-five aged Samoans plus participant observation revealed that their status remains relatively high, although there are signs of potential problems. A major consideration in planning services for Samoan elders should be recognition of the effective support network of the Samoan family and its role in the retention of this high status by the aged.

Prior to the 1970s anthropological research on aging was very limited (Clark 1967; Holmes 1976): but in recent years evidence of anthropological interest in the study of aging and the aged has become increasingly visible. Several major volumes have appeared (Amoss and Harrell 1981; Cowgill and Holmes 1972; Fry 1980; Holmes 1983; Keith 1982; Sokolovsky 1983) reflecting a wide variety of research interests--status and role, community studies, networks, homes for the aged, retirement, disengagement, ethnicity, longevity, culture, and personality, for example. Holmes (1983) provides a comprehensive review of this growing body of data.

A theory which has emerged from some of this cross-cultural research is the aging and modernization hypothesis, which states that as societal modernization increases the status of the aged will decline (Cowgill and Holmes 1972). Contributions to our knowledge on the effects of such change continue to accumulate (Amoss 1981; Cherry and Magnuson-Martinson 1981; Goldstein and Beall 1981; Bhoads, in press; Van Arsdale 1981). Since the islands of American Samoa have experienced a great deal of change in the direction of modernization in recent years and the traditional status of elders has been reported as very good by anthropologists who investigated the society in earlier years (Holmes 1958, 1972; Keesing 1934; Mead 1928, 1930), field research was conducted there in 1976 to investigate the status of the aged under the changing social conditions. This paper is based on that research.

Although the term "modernization" is widely used by social scientists, there is no general consensus as to its definition. Usually such characteristics as urbanization, industrialization, and increased education are included, with various scholars emphasizing different aspects of the process as significant (Cowgill 1974; Cowgill and Holmes 1972; Inkeles and Smith 1974; Lerner 1958; Palmore and Manton 1974) Cowgill's conceptualization seems most appropriate:

> Modernization is the transformation of a total society from a relatively rural way of life based on animate power, limited technology, relatively undifferentiated institutions, parochial and traditional outlook and values, toward a predominantly urban way of life based on inanimate sources of power, highly developed scientific technology, highly differentiated institutions matched by segmented individual roles, and a cosmopolitan outlook which emphasizes efficiency and progress. (Cowgill 1974: 127)

Research on Samoan aging emphasized the four aspects of modernization designated by Cowgill (1974) as most relevant to the aged--health technology, modem economic technology, urbanization, and education.

When compared to the world's most highly modernized societies, American Samoa is not very modern at all. But when placed in context with other Polynesian island areas, specifically Hawaii, Society Islands, Cook Islands, and Tonga, it seems to occupy an intermediate position on a continuum of least modern to most modern. It is difficult to make precise comparisons since reliable, comparable, statistical data are not readily available from all of these areas. We can, however, make approximate comparisons on the basis of general levels of industrial development, the extent of dependence on wage employment, communication and transportation developments, education and medical services available, and so on.

Hawaii, of course, is unquestionably the most modern of these Polynesian islands by any of these measures. It is also the largest in both land area and population. The Society Islands are next in modern development, especially the island of Tahiti. This represents a typical pattern throughout most of Polynesia: major economic development and urban population concentrations tend to aggregate around the government center and major port town. American Samoa is third among these islands in terms of modernization, followed by Cook Islands and Tonga, respectively. Industrial development in these three groups, especially the latter two, has been limited either by lack of natural resources, lack of capital to de-

velop resources into viable market products, or by geographical isolation. American Samoa does have a successful fishing industry with sizeable exports of fish products. Fairbairn (1971) has noted this industry as one of the most highly mechanized in the South Pacific outside of the major industrial areas such as Hawaii and New Zealand.

Tourism is an important source of revenue in all of the island groups considered here, although Hawaii and Tahiti have been most successful in this respect. Again, American Samoa has more tourist traffic than either Cook Islands or Tonga. This is due in part to better access to international transportation and more hotel facilities in Samoa (Inder 1977). Wages are also higher in American Samoa than in the Cooks or Tonga, and in 1974 there was one automobile for every ten Samoans compared to one for every seventy-six Tongans.

Of the five island groups American Samoa is the smallest with its seventy-six square miles (Inder 1977) and has the second smallest population (Crocombe 1973). It is also important to realize that the Samoan islands have very steep, mountainous terrain, which restricts the area available for habitation and business/industrial development. Given these conditions, the degree of modernization in Samoa becomes significant relative to these other Polynesian islands; and as will be described in more detail below, American Samoa has become much more modern relative to its position just fifteen to twenty years ago.

Historical Overview of Contact And Change: Pre-1960

The islands of the Samoan archipelago lie in the west-central portion of the area of the Pacific Ocean known as the Polynesian Triangle, which is bounded by Hawaii to the north, New Zealand to the southwest, and Easter Island to the southeast. Linguistic and archaeological evidence suggest that Samoa, along with Tonga, was one of the earliest settled areas in Polynesia, about three thousand years ago, with initial movement of populations to other Polynesian islands beginning from these island groups (Bellwood 1979; Davidson 1979).

Contact with Europeans reportedly began with the Dutch explorer Roggeveen, when he sailed near Manu'a in 1722. Bougainville, LaPerouse, Kotzebue, and Wilkes also made early contact in Samoa, and these various explorers' descriptions are quoted by Keesing:

> These early visitors found a group of islands which they were moved to describe variously as "l'Eldorado de la Polynesie," "One of the finest countries under heaven," even "the most beau-

> tiful in the Southern Ocean, and consequently in the whole world;" found a people of "colossal" physique, living in villages scattered along the coastline; the degree to which they were at home in their canoes caused the group to be known as "l'Archepel des Navigateurs." (Keesing 1934:24)

Extended contact between Samoans and Europeans began with the arrival of John Williams, a representative of the London Missionary Society (L.M.S.) in 1830. He introduced Christianity which has remained the dominant religious denomination in Samoa ever since. The Church was also involved with promoting education, translating and printing the Bible in English, and establishing schools. These first schools were "Pastor's *(faifeau)* schools" in which children were taught reading, writing, and arithmetic (Gray 1960), and Keesing (1934) reports that by 1900 Samoa was approximately 99 percent literate.

In late 1899 there was a partition of the Samoan islands, with control of the four westernmost islands being given to Germany; at the outbreak of World War I in Europe, New Zealand seized control of Western Samoa. The seven eastern islands--Tutuila, Aunu'u, Ofu, Olesega, Ta'u, Swain's Island, and Rose Island--became territorial possessions of the United States, with the Department of the Navy placed in administrative control in 1900. The policy of the Navy in its relationship with the Samoans was one of peaceful coexistence and relative noninterference in Samoan affairs so long as there were no obvious conflicts with U.S. laws. It was supportive of education and facilitated establishment of the public schools, although it was unsuccessful for many years in getting funds from the U.S. government for the Samoan educational system. Public schools had to be supported by local money and thus could not serve all children.

The Naval administration did introduce health services in American Samoa. The Navy surgeon established a dispensary, and the first medical efforts were directed toward inoculation for smallpox and improvement of sanitation in the villages. A hospital was opened in 1912 and a nursing school added at that facility in 1914.

World War II brought increased defense measures and more military personnel to the territory, and for the first time significant numbers of Samoans had opportunities for wage employment. The economic "boom" was temporary, however; when the war ended so did most of these jobs. A few years later, in 1951, American Samoa was transferred into the jurisdiction of the U.S. Department of the Interior and a civilian government with an American governor was established.

When Holmes (1958) conducted research in Samoa in 1954, he assessed cultural change relative to earlier observers' recorded reports (Keesing 1934; Mead 1928, 1930) and found that after more than a century of European contact and presence in the islands, Samoan culture had remained relatively stable. The majority of changes had been material items adopted from European culture, but the acceptance of Christianity and the introduction of formal education had been the most significant elements of change through the mid-1950s. This conservatism in Samoa has been noted often by students of Polynesian culture. Holmes (1980) has attributed this stability to the Samoan system of social organization, the *matai* system, particularly the equal opportunity for achievement available to any individual under this system. Hanson suggests that a significant contributing factor in Samoa's stability was the "relatively long period of independence followed by colonial policies of indirect rule . . ." (1973:9), which allowed the traditional system to continue.

Modernization Efforts

In 1960 the U.S. Senate authorized a "study mission" to American Samoa to evaluate and make recommendations regarding the economic, educational, and health systems as well as the general status of the people of the territory. The results of this investigation were published in mid-1961 as a report to the Committee on Interior and Insular Affairs (Long and Gruening 1961). About the same time, however, another report appeared in a more widely available source, *The Reader's Digest*, and caught the attention of the American public with its title "Samoa: America's Shame in the South Seas" (Hall 1961). The writer, Clarence Hall, had recently visited American Samoa and sounded the alarm on what he saw as evidence of sixty years of United States' "neglect and apathy" in a fabled paradise. He described wretched sanitary conditions, poor roads, and an inadequate water system, and lamented the Samoans' increased dependence on imported foods. The U.S. government was also chided for inadequacies of the medical system and the schools, in which "a largely untrained and poorly paid teaching force struggles to teach some 5500 eager pupils on the lowest budget (less than $50 per pupil) of any U.S. state or territory in the world" (Hall 1961:112). Although the Senate Committee report had already put the process of change in motion, Hall's article and the subsequent wave of protest from citizens must have encouraged prompt action.

The Senate investigative committee made a number of recommendations for general improvement of the standard of living in Samoa: in-

creased opportunity for wage employment, agricultural improvements, measures for promoting tourism, consolidation of schools, and so on. What followed in the next few years is perhaps best described as an "attack" designed to eradicate all the ills, real or imagined, in American Samoa. Extensive changes were imposed on Samoan society in rapid order. In short, the modern era began in American Samoa about 1962. A number of projects were targeted for completion by mid-1962 when the Fifth South Pacific Conference was to be held in the territory. These first-priority projects included expansion of the airport facilities with a 9,000-foot runway capable of handling large jet aircraft, an auditorium with a seating capacity of 800 persons, a paved road from town to the airport, expanded electric power facilities, and three new high schools (American Samoa, *Annual Report,* 1962).

Over the next few years other changes were implemented. The school system was consolidated as recommended, and twenty-six new grade schools were constructed. Governor H. Rex Lee, who was charged with implementing this vast program of change, also decided that introduction of an educational television system was the best means for rapidly improving educational standards for both teachers and students. This ETV program initially involved only grades 1-8, but soon expanded to include the high schools. Within a few years television programming included more and more taped, entertainment-oriented shows from the U.S., and families acquired their own sets for viewing at home.

Health and sanitation standards were also important aspects of the rehabilitation campaign, with major efforts directed toward eradication of filiriasis (American Samoa, *Annual Report,* 1966). Other health measures introduced were dental care education and a family-planning program; and in 1968 the new 200-bed Lyndon B. Johnson Tropical Medicine Center opened on Tutuila with U.S. Public Health physicians in charge.

The most dramatic changes, however, were in the economic sphere. While a tuna cannery had been established by Van Camp in 1954, it provided employment for only about 350-400 Samoans (Long and Gruening 1961). During fiscal year 1964, a second cannery was built by Star Kist. These industries plus various government jobs have become major elements in the Samoan economy. The completion of a large hotel and improved transportation between Samoa and other major South Pacific cities have also made tourism a more viable industry in American Samoa.

By 1976 when I conducted research in Samoa, these and other changes were evident. Samoans were working primarily in the wage-labor market, where the minimum wage level was $1.05-$1.42 per hour. With the increased availability of wage employment opportunities, fewer people

were engaged in agricultural activities. Imports of food had become increasingly important as had demands for other products such as automobiles, television sets, clothing, and so on. A few specialty shops selling ready-made clothing, shoes, or sports equipment have opened in recent years, and there are now a variety of recreational activities on which Samoans can spend their money--movie theaters, night clubs, a bowling alley, a golf course, and tennis courts.

The educational system now ranges from early childhood education for preschoolers through two years of college, with many young Samoans also attending U.S. colleges on scholarships. Housing has become increasingly western; when a major hurricane damaged many of the traditional *fale* (houses) in 1966, the government offered free replacement housing constructed of concrete blocks, and this trend has continued. The population in 1974 was about 30,000, which represented an increase of 45% since 1960 (U.S. Bureau of the Census, 1983; Marcus et al. 1974). This increase seems largely due to improved health services and reduction of infant mortality.

Telephone service is available on Tutuila and in the Manu'a group (Ta'u, Ofu, and Olosega), as is television. Improvements in communication and transportation have made many Samoans more aware of happenings within and beyond their own locale and no doubt encourage some of the changes within it. This seems especially true of television (Kaser 1965; Siegel 1979).

Aging In Traditional Samoa

Traditionally, the Samoan concept of age was functional rather than chronological. A person was considered an adult or an old person by virtue of abilities and activities, not because one had attained a certain age. Failing physical strength or other incapacities which precluded active, continued participation in normal activities would result in a person being classified as old. Holmes (1972) found that in 1962 most Samoans thought of a person about age fifty or over as "old."

The old person in Samoa lived in a household that was typically a three-generational unit including at least one married adult child with his or her spouse and offspring. These units varied in size, but in 1954 the average household in Ta'u village had ten to twelve residents (Holmes 1958). The household does not usually coincide with the *aiga*, or Samoan extended family, which is a much larger group with members in several households and villages. The aiga, however, is the most significant social

grouping in Samoan culture. Defining its membership is far more complex than understanding its function, and there is no more apt description of the meaning of the aiga than Margaret Mead's:

> An *aiga* member is always one's ally against other groups, bound to give one food, shelter and assistance. An *aiga* may ask for any of one's possessions and refuse to take "no" for an answer; usually an *aiga* may take: one's possessions without asking. . . . Under the shadow of these far-flung recognized relationships children wander in safety, criminals find a haven, fleeing lovers take shelter, the traveler is housed, fed, and his failing resources reinforced, property is collected for a house building or a marriage; a whole island is converted into a series of cities of refuge from poverty, embarassment, or local retribution. (Mead 1930:40)

The aiga is headed by the matai (chief), who holds the hereditary title of the descent group. Matai are elected by their aiga, and candidates may be related to that body by either blood, adoption, or marriage (Mead 1930; Grattan 1948; Holmes 1958). A matai has power in his family and in the village, but he: also has a great deal of responsibility. He control+, the communally owned family land and, as Shore (1982:68) explains, "is usually the object of a continual stream of requests from family members who look to him as a source of material, moral, and political support," demands which the matai is obliged to respond to. He is also responsible for the behavior of his family because individual acts, either shameful or prestigious, reflect on the entire aiga, including its matai (Shore 1982).

Traditionally it was expected that children would care for their aging parents by housing them, feeding them, and providing for any other needs. Preparation of favorite foods, assistance with bathing, giving a back rub, or bringing; a coal from the fire to light a pipe were typical of the traditional behavior patterns toward the aged. Samoans would comment that old age was a good time of life, when one received better food and better care. It also was a time when one was "less constrained to maintain a dignified image on ceremonial and social occasions" (Holmes 1972:77).

Old age was a time when one should be treated with great respect. The principle of respect for age in Samoa is clearly exemplified in Fay Ala'ilima's comments:

> In a good Samoan family those below treated those above with deference and respect. Children did not talk loudly in the pres-

> ence of elders. Girls did not speak rudely to the wife of the *matai.* A *matai* held his mother in great regard. A low chief let a high chief have the final speech. . . . The spirit of respect for those above was equally matched with a deep feeling of obligation towards those below. (Ala'ilima 1961:28-29)

Age has also been linked with authority in Samoan culture. Keesing (1934:30-31), in discussing Samoan cultural ideals, mentioned that "age and rank should be respected and obeyed." Margaret Mead stated that, "Within the household, age rather than relationship gives disciplinary authority. . . . The newest baby born into such a household is subject to every individual in it, and his position improves no whit with age until a younger child appears upon the scene" (1928:40). Even beyond their own household an aged person's requests or complaints were expected to be heeded by younger family members in other households (Mead 1928).

Retirement, in the Western sense, was a nonexistent concept in traditional Samoa. Work was neither demanded nor discouraged from those who wished and were able to assist with household tasks. Older Samoans believed that continued participation in some work helped them maintain strength. The activities of the aged have always been valued contributions to their household and village. The role of advisor to the family was an important function of both men and women and they were always consulted when there were decisions to be made concerning the family.

Old men were usually matai (chief), and thus in an authoritative position in the family as well as being involved in the village-wide affairs of the *fono* (village council). Although a matai title could be held for life, some voluntarily resigned in old age but continued to function as advisors to their successors and still attended fono meetings. Braiding sennit, a twine made from coconut-husk fibers, occupied a large portion of an old man's time. This product was needed in great quantity for building traditional Samoan houses, and was also used for fish nets and the fly switch, ceremonial trademark of the talking chief. Their knowledge of legends and customs made old men the recognized authoritative sources of traditional knowledge, history, and genealogy (Holmes 1972).

Weaving of the numerous mats required in the household for everyday use and the delicate fine mats that were essential elements in ceremonial exchanges was a primary role of old women. Some were skilled as midwives and in the use of massage as a treatment technique. Pulling weeds around the house was a familiar task for elderly Samoan women (Holmes 1972). And supervision of the young children while the parents worked

was an important responsibility for older women in the household. When tattooing was more common, they also prepared the pigment used in this process (Holmes 1958).

Margaret Mead (1928) contended that the lives of Samoan women were characterized by more continuity than men's. This opinion is based on the fact that adult women experienced little dramatic change in roles or activities. They raised large families and worked very hard through the middle years. "Then," according to Mead, "as age approaches, she settles down to performing the skilled tasks in the household, to weaving and tapa making" (Mead 1928:193). The skills were not new to her, but she could now devote more time to them and to teaching others. Being homebound would therefore be less threatening to an old woman, in Mead's view, since the household was always her primary domain of authority. She felt that there was more discontinuity for men in the transition to old age since more of their activities were focused outside the home. Consequently, being homebound in later years might seem more restrictive. According to Holmes (1958), however, while "old men have less to do than old women . . . they may often be seen assisting the older women in household tasks. . . . It is not unusual to find an elderly grandfather taking a turn at caring for the small children" (Holmes 1958:57).

Old Age in the Samoa of the 1970s

Since 1962 the American Samoan world has been bombarded with extensive modern changes--increased educational opportunities, improved health care, and especially economic development. These kinds of changes, along with urbanization, have been suggested as those most likely to negatively affect the status of the aged (Cowgill 1974). Most of the change has occurred on the main island of Tutuila, with much less direct change evident in the Manu'a island group, which consists of three much smaller, less populous islands isolated from Tutuila by some sixty miles of ocean. Since there is no industry in Manu'a, subsistence agriculture and some cash-cropping are still important aspects of life there. Retail business consists of a few "bush" stores, which offer a limited range of canned goods and household supplies. Although transportation is somewhat improved in recent years, few outsiders go to Manu'a. In 1976 there were only four non-Samoan residents on Ta'u island.

In order to assess possible differences in status of the aged who are exposed to different levels of modern development, the research reported here focused on the port town and government center around Pago Pago Bay on Tutuila, and on Ta'u island in the Manu'a group. Interviews were

conducted with eighty-five Samoans aged sixty or over--fifty individuals in the Pago Pago area and thirty-five on Ta'u. In Ta'u 54 percent were women and 46 percent men, whereas the urban sample was 58 percent women and 42 percent men. With the exception of one couple in the urban sample, all of those interviewed were living in extended family households with some of their children and grandchildren.

Observations in Ta'u revealed much more involvement with traditional tasks by all residents. The aged still braid sennit and a man was even observed carving oars for use with the village longboat. Pandanus leaves can be seen drying in the sun for later use by the old women in weaving mats. In fact, they make mats to ship to Tutuila as well as for their own household use. Household activities and routines vary little from those reported by Mead (1928) and Holmes (1958). Traditional activities such as weaving and carving were observed in the Pago Pago area only at the craft *fale* (houses), where production of craft items by the aged is part of the Territorial Administration on Aging program. Men and women of sixty years of age or older were being paid $1.25 per hour (in 1976) for making mats, jewelry, or carvings; the items were then sold in a retail shop at the center. Bingo games are now a popular (and sometimes costly) new pastime for some of the aged on Tutuila. Both the old people of Ta'u and Pago Pago function as babysitters, and this role is perhaps of increased importance in the latter setting where parents work long hours away from home even more than in the past.

Some of the questions asked of elderly Samoans in the interviews were based on traditional ideals concerning care and respect associated with old age. Their responses give us some insight into their perceptions of their own current status. In both samples the aged agree that they are more knowledgeable than younger people about ceremonial matters (94 percent, Ta'u; 95 percent, Pago Pago), and that they are respected for this knowledge (97 percent, Ta'u; 96 percent, Pago Pago). They are still consulted (91 percent, Ta'u; 95 percent, Pago Pago) about decisions concerning family problems, weddings, funerals, titles, and so on, although many (97 percent, Ta'u; 75 percent, Pago Pago) think perhaps the young do not take their advice as much as in the past. There is strong agreement (100 percent Ta'u; 98 percent, Pago Pago) that the old are respected and obeyed by younger members of the community.

An interesting difference in the two groups of aged Samoans appeared when 91 percent of the aged in Ta'u said they received better food and care than other family members, whereas only 52 percent in Pago Pago responded positively to this question. When asked if special things are done for them, again it is the elders of Ta'u (97 percent) who answer in

the affirmative, while only 55 percent of those in Pago Pago agree. The contrast here may reflect the more dramatically changed lifestyle in the urban area, where more activities outside the household compete for the time of younger family members.

These are but a few of the indicators of the status of Samoan elders in 1976. Extensive observation and participation in many aspects of life in the two research sites, as well as interviews with younger family members contributed to the total data base. From the standpoint of the cultural ideals regarding old age in Samoa, the aged in both Ta'u and the Pago Pago area still seemed at that time to hold a position of high status relative to the pre-1962 period.

Cultural Support For Status of the Aged

There are several aspects of traditional Samoan culture which continue as viable principles and institutions today and which explain how Samoan elders have managed to retain this status. These are the Samoan kinship or family system (often referred to as the matai system) with its key components the *aiga* (extended family) and the matai; ideas about dependency; and the reciprocity which characterizes Samoan social relationships.

Robert Maxwell (1970) has said that young men of Samoa are less interested in acquiring matai titles than formerly because they feel the system is outdated, ineffective, and interferes too much in their lives. He concluded that the authority of the aged was thereby in jeopardy. Holmes (1967:9) who investigated attitudes of both untitled and titled men at about the same time as Maxwell's study, found that "the *matai* system is changing but not degenerating." David Pitt (1970), who studied the economic system in Western Samoa, found that traditional institutions, which include the matai system, have proven quite adaptable to new economic conditions. Informants in both Samoa and California (Samoan migrants) verified that people do return to the islands to vie for matai titles, and if they are successful in acquiring a title, remain in Samoa even though they may have lived in the U. S. for a long time.

The continued strength of the matai system as an organizing principle in Samoan society is probably largely responsible for the fact that Samoans have retained so much of their traditional culture. This was recognized by the Keesings (1956) years ago and emphasized in 1980 by the Governor of American Samoa, Peter Tali Coleman, who was quoted in a news story as saying, "We cannot open the land to outside ownership, because we protect our culture through the family system and that system is

tied to the land" (Macdougall 1980). Since most Samoan land has thus far been retained by Samoans, and the matai controls aiga land for the family group, this reinforces mutual interdependence of aiga members and perpetuates the extended type of family. The aiga is still very important to Samoans; repeatedly during the research it was observed that almost nothing takes precedence over the needs of the aiga. In some instances, for example, the needs of the aged result in significant alteration of plans by adult children. In one case, a family felt that a daughter, who lived in the U. S. with her husband and children and who planned to remain there, was needed in Samoa to help care for one of her aging parents. The daughter was persuaded to return to the islands to live. The continuation of the family system that commands this kind of loyalty tends to protect the status of the aged.

Another cultural factor that is an advantage for Samoan elders is the Samoan view of dependency. Dependency of the aged does not have the negative connotations in Samoan society that we see in the United States. Americans tend to emphasize independence and self-reliance of individuals, traits which may well create value conflict for those older people who can no longer maintain independence (Clark and Anderson 1967; Hsu 1972). Samoans, on the other hand, expect the old person to be dependent on others, which perhaps eases the transition into old age.

The concept of reciprocity is fundamental in Samoan culture, and it also helps support the status of the aged. Intergenerational age relationships are characterized to some extent by reciprocal obligations. I found that contemporary Samoan high school students, in writing essays on aging, continue to acknowledge the long-standing cultural ideals about these responsibilities of the young for the old. They emphasized that when one is young and totally dependent, the parents and grandparents provide whatever is needed. This is interpreted by these young people as producing a debt to the elders which must be repaid by caring for them in their old age. Their comments reflect the special status of the elderly: "A person who is old is the most important person in the whole family." They also indicate that being old means not having to work unless one wants to, and perhaps having the family do special things like sending an elder to America to visit a migrant child.

There is a recognition of the authoritative aspect of aging, but this too implies reciprocity in that they know those who serve while young will be rewarded for their efforts when they reach old age. Just as family members are obligated to contribute to the matai for a funeral or wedding in the aiga, they are assured by their participation of the matai's concern when the need is theirs. The aiga supports its members, and ultimately all

can expect to receive their share of benefits. Observations in Samoan households confirmed that the comments in the teenagers' essays are not merely lip serve to tradition; the beliefs are still being practiced. The value of this system has also been demonstrated by its continuation among migrant Samoans in the mainland U.S. (Ablon 1970) and in New Zealand (Pitt & MacPherson 1974).

Implications for the Future

Modernization has not come without impact on the aged. There are some indications of change and a few potential problems. Some of the skills of the elders are in less demand today. For example, with changes in house construction there is much less need for old men to make sennit (braided twine). One hears also of matai titles being acquired by younger men somewhat more frequently, usually because of educational achievements. Educational disparities between young and old could conceivably jeopardize the effective advisor role of the aged, especially former matai. Few old women are needed as midwives now; over 90 percent of the births in American Samoa. were hospital deliveries by the mid-1970s (American Samoa Health Coordinating Council, 1978). On the other hand, in 1976 there was an obvious resurgence of interest in ensuring that young Samoans learn more of their own cultural history. The aged are the most likely resource in that area; knowledge of tradition and ritual has proven beneficial to elders in other societies that have experienced cultural change (Amoss 1981; Cool 1980).

A clue to a potential problem area was revealed in the results of the Psychosomatic Symptoms test (PT). This instrument has been used by Alex Inkeles and David Smith (1970) to assess mental health of individuals in six developing countries. The PT test is similar to the Health Opinion Survey (HOS), and the questions concern symptoms such as nervousness, shortness of breath, sleeping difficulties, headaches, heart palpitations, and so on. It is believed that such items "serve as very good indicators of psychoneurotic and psychophysiologic disturbances--that is, those types of disorder which are prevalent in communities at large" (Murphy and Leighton 1965:110-11). Respondents who report more than half of the symptoms are judged to be suffering from psychic stress.

Since it is often stated that urbanization and industrialization have detrimental effects on mental health, we would have expected the symptom reporting level to be higher in the Pago Pago Bay area aged. It was surprising, then, to find that 66 percent of the Ta'u elders reported six or more of the eleven possible symptoms, while only 38 percent did so in

Pago Pago. The average number of symptoms was 6.3 in Ta'u and 4.1 in Pago Pago. Even though Ta'uans continue to live in the most traditional setting available to American Samoans, with all the stability that normally implies, the results of the PT test suggest that the aged of Ta'u are under more stress than their counterparts in the more modern milieu.

It seems possible that the primary stress-producing factor in this instance is the increased rate of emigration of young people from Ta'u to either the main island or to the United States. Census data show that 28 percent of the Ta'u population is between the ages of twenty and fifty-nine, whereas 46 percent of the Pago Pago area population is in this age bracket (Marcus et al. 1974). Observations in Ta'u village confirm a conspicuous absence of young adults past high school age.

The security of the aged inherent in the traditional culture has depended in large measure on the continued support by the younger generations, including their physical presence to do the harder work. It is perhaps becoming more difficult to cope with changes that occur now that the population has been reduced so disproportionately. Major events like weddings and funerals still draw the migrants back temporarily, but minor crises and the routine activities of subsistence must be borne without the characteristic support groups of the past.

The isolation of Ta'u, which has served to protect it from the incursions of drastic change, has become in some ways a handicap. The major economic development in Samoa has largely bypassed Ta'u, with the consequence that most of the young people who want nontraditional, wage-earning jobs must go elsewhere. This situation appears unlikely to change soon, if ever. The high level of psychosomatic symptoms in Ta'u may stem from uneasiness about the future, which is less predictable now, and an apparent lack of viable alternatives to the current situation.

It should be emphasized that these effects of change are not being suggested as serious problems for the aged now, but they may well be signs of possible trouble ahead. Reports of more recent visitors to American Samoa indicate ever-increasing acquisition of American goods and demands for more individual freedom (James Bindon, personal communication, 1983). In my opinion, the more Samoans lean toward American value orientations, the more threatened the status of elderly Samoans will become (Rhoads, in press).

As a territorial possession of the United States, American Samoa is eligible to receive assistance through various government-funded programs, including those for the aged. It must be remembered that these programs have been planned to meet American needs and operate under regulations perhaps more workable on the mainland than elsewhere. In more than

one instance these transplanted programs have been difficult to implement and of little or no value in Pacific island populations. For example, nutrition programs for the aged have posed problems in both Micronesia and Polynesia. Borthwick has reported that

> only in very rare instances do elderly citizens of the Trust Territory live with so little support from their relatives that they require special governmental feeding assistance. In cases of genuine hardship, the entire family not just its older members, who alone would be served by Title VII, would need support. (Borthwick 1979:4)

The same situation would seem to apply in Samoa. And in 1976 the Territorial Administration on Aging in American Samoa was experiencing difficulty in implementing a nutrition program due to conflicts between program regulations and cultural values.

Intergenerational family relationships can be a strong source of support for the elderly. Johnson (1983) describes such a system for Italian-Americans as does Simić (1983) for Yugoslavians. According to Simić (1983:88), even in urban Yugoslavia "intergenerational relationships are also characterized by high levels of continuity and reciprocity." In American Samoa, the strength of the family network and the integration of the aged in that network should be recognized. The reciprocal social relationships still appear to be working well. Whether Samoans, who have been noted for their conservatism (Hanson 1973; Holmes 1980), can continue to maintain these positive support features of their culture in spite of the onslaught of contemporary forces of change remains to be seen.

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REFERENCES CITED

Ablon, Joan. 1970. The Samoan funeral in urban America. *Ethnology* 10:209-27.

Ala'ilima, Fay C. 1961. *A Samoan family*. Islands Education Division of the Department of Education, for the Education Department of Western Samoa. Wellington, New Zealand.

American Samoa *Annual Report, 1962.* The Governor of American Samoa to the Secretary of the Interior. U.S. Government Printing Office, Washington.

American Samoa *Annual Report, 1966.* The Governor of American Samoa to the Secretary of the Interior. U.S. Government Printing Office, Washington.

American Samoa Health Coordinating Council. 1978. *American Samoa plan for health 1978-83.* Government of American Samoa.

Amoss, Pamela T. 1981. Coast Salish elders. In P. Amoss and S. Harrell, eds., *Other ways of growing old: Anthropological perspectives.* Stanford University Press, Stanford, California.

Amoss, Pamela T., and Stevan Harrell, eds. 1981. *Other ways of growing old: Anthropological perspectives.* Stanford University Press; Stanford, California.

Bellwood, Peter S. 1979. The oceanic context. In Jesse D. Jennings, ed., *The prehistory of Oceania.* Harvard University Press, Cambridge.

Borthwick, Mark. 1979. Cultural factors affecting the Truk district aging program. Paper presented at conference on U.S. Federal Programs in Micronesia, Kolonia, Ponape, March 1979.

Cherry, Ralph L., and Scott Magnuson-Martinson.1981. Modernization and the status of the aged in China: Decline or equalization? *Sociological Quarterly* 22:253-61.

Clark, Margaret. 1973. Contributions of cultural anthropology to the study of the aged. In L. Nader and T. W. Maretzki, eds., *Cultural illness and health.* American Anthropological Association, Washington.

Clark, Margaret and Barbara Anderson. 1967. *Culture and aging: An anthropological study of older Americans.* Charles C. Thomas, Publisher, Springfield, Illinois.

Cool, Linda E. 1980. Ethnicity and aging: Continuity through change for elderly Corsicans. In C. Fry, ed., *Aging in culture and society: Comparative viewpoints and strateqies.* J. F. Bergin, Publishers, Brooklyn.

Cowgill, Donald O. 1974. Aging and modernization: A revision of the theory. In J.F. Gubrium, ed., *Late life: Communities and environmental policy.* Charles C. Thomas, Publisher, Springfield, Illinois.

Cowgill, Donald O., and Lowell D. Holmes. 1972. *Aging and modernization.* Appleton-Century-crofts, New York.

Crocombe, R. G. 1973. *The new South Pacific.* Reed Education, Auckland.

Davidson, Janet M. 1979. Samoa and Tonga. In Jesse D. Jennings, ed., *The prehistory of Oceania.* Harvard University Press, Cambridge.

Fairbairn, Ian. 1971. Pacific island economies. *Journal of the Polynesian Society* 80:74-118.

Fry, Christine L. 1980. *Aging in culture and society: Comparative viewpoints and strategies.* J. F. Bergin Publishers, Brooklyn.

Goldstein, Melvyn C., and Cynthia M. Beall. 1981. Modernization and aging in the third and fourth world: Views from the hinterland in Nepal. *Human Organization* 40:48-55.

Grattan, F.J.H. 1948. *An introduction to Samoan custom.* Samoa Printing and Publishing Company, Apia, Western Samoa.

Gray, J.A.C. 1960. *Amerika Samoa.* United States Naval Institute, Annapolis.

Hall, Clarence W. 1961. Samoa: America's shame in the South Seas. *Reader's Digest,* July 1961, 111-16.

Hanson, F. Allan. 1973. Political change in Tahiti and Samoa: An exercise in experimental anthropology. *Ethnology* 12:1-13.

Holmes, Lowell D. 1958. *Ta'u: Stability and change in a Samoan village.* The Polynesian Society, Wellington, New Zealand.

——— 1967. *The modern Samoan family.* Wichita State University Bulletin, University Studies no. 71. Wichita State University, Wichita, Kansas.

——— 1972. The role and status of the aged in a changing Samoa. In D. Cowgill and L. Holmes, eds., *Aging and modernization.* Appleton-Century-Crofts, New York.

——— 1976. Trends in anthropological gerontology: From Simmons to the seventies. *Aging and Human Development* 7:211-20.

——— 1980. Factors contributing to the cultural stability of Samoa. *Anthropological Quarterly* 53:188-97.

——— 1983. *Other cultures, elder years.* Burgess Publishing Company, Minneapolis.

Hsu, Francis L. K. 1972. American core value and national character. In F.L.K. Hsu, ed., *Psychological Anthropology*. Schenkman Publishing Company, Cambridge, Massachusetts.

Inder, Stuart, ed. 1977. *Pacific islands yearbook,* 12th edition. Pacific Publications, Sydney.

Inkeles, Alex, and David H. Smith. 1970. The fate of personal adjustment in the process of modernization. *International Journal of Comparative Sociology* 11:81-114.

——— 1974. Becoming modern: Individual change in six developing countries. Harvard University Press, Cambridge.

Johnson, Colleen L. 1983. Interdependence and aging in Italian families. In Jay Sokolovsky, ed., *Growing old in different societies: Cross-cultural perspectives.* Wadsworth Publishing Company, Belmont, California.

Kaser, Tom. 1965. Classroom TV comes to Samoa. *Saturday Review,* June 19, 1965, 58-59. 72-73.

Keesing, Felix. 1934. *Modern Samoa.* Allen and Unwin, London.

Keesing, Felix, and Marie Keesing. 1956. *Elite communication in Samoa: A study of leadership.* Stanford University Press, Stanford, California.

Keith, Jennie. 1982. *Old people as people: Social and cultural influences on aging and old age.* Little, Brown and Company, New York.

Lerner, Daniel. 1958. *The passing of traditional society.* The Free Press, Glencoe, Illinois.

Long, Oren E., and Ernest Gruening. 1961. *Study mission to Eastern (American) Samoa.* Report to the Committee on Interior and Insular Affairs, United States Senate. U.S. Government Printing Office, Washington.

Macdougall, William L. 1980. Samoa: More American than America. *U.S. News and World Report,* June 16, 1980, 83-84.

Marcus, E.S., E. Gebauer, M.J. Levin, and P.N.D. Pirie. 1974. *Report on the 1974 census of American Samoa.* East-West Center, Honolulu.

Maxwell, Robert. 1970. The changing status of the elders in a Polynesian society. *Aging and Human Development* 1:137-46.

Mead, Margaret. 1928. *Coming of age in Samoa.* William Morrow and Company, New York.

—— 1930. *Social organization of Manu'a.* Bulletin No. 76, Bishop Museum, Honolulu.

Murphy, Jane M., and Alexander H. Leighton. 1965. *Approaches to cross-cultural psychiatry.* Cornell University Press, Ithaca, New York.

Palmore, Erdman, and Kenneth Manton. 1974. Modernization and status of the aged: International correlations. *Journal of Gerontology* 29:205-10.

Pitt, David. 1970. *Tradition and economic progress in Samoa.* Oxford University Press, Oxford.

Pitt, David, and Cluny MacPherson. 1974. *Emerging pluralism: The Samoan community in New Zealand.* Longman, Paul, Auckland, New Zealand.

Rhoads, Ellen C. Reevaluation of the aging and modernization theory: The Samoan evidence. *The Gerontologist.* In press.

Shore, Bradd. 1982. *Sala'ilua: A Samoan mystery.* Columbia University Press, New York.

Siegel, Barry. 1979. How Samoa got hooked on TV. *San Francisco Sunday Examiner and Chronicle, Sunday Punch,* July 22, 1979.

Simić, Andrei. 1983. Aging in the United States and Yugoslavia: Contrasting models of intergenerational relationships. In Jay Sokolovsky, ed., *Growing old in different societies: Cross-cultural perspectives.* Wadsworth Publishing Company, Belmont, California.

Sokolovsky, Jay. 1983. *Growing old in different societies: Cross-cultural perspectives.* Wadsworth Publishing Company, Belmont, California.

Van Arsdale, Peter W. 1983. The elderly Asmat of New Guinea. In Jay Sokolovsky, ed., *Growing old in different societies: Cross-cultural perspectives.* Wadsworth Publishing Company, Belmont, California.

U. S. Bureau of the Census. 1963. *U. S. census of population: 1960.* Vol 1, *Characteristics of the population,* Parts 54-57, Oultlying areas. U.S. Government Printing Office, Washington, D.C.

THE WESTERN BREAKAWAY MOVEMENT IN THE SOLOMON ISLANDS

by Ralph Premdas, Jeff Steeves, and Peter Larmour

For many Third World countries independence is an occasion marked by united, nationwide jubilation: the colonial master is at last evicted, and the people now control their own political destiny. But for the Solomon Islands, as much uncertainty as joy attended the independence celebrations on 7 July 1978. The country's Western Province, with about 20 percent of the total population and 30 percent of the land area, boycotted the official festivities. On Independence Day, an attempt to raise the Solomon Islands national flag at the police station in the provincial headquarters of Gizo led to a confrontation between Western people and migrants from Malaita, the home island of the prime minister. Three planeloads of police were flown in to reinforce the police station. The next day, members of the British royal family arrived, fresh from the independence celebrations in Honiara. In welcoming them, the president of the Western Council was careful to limit the symbolism:

> Your visit here is being acknowledged by our people as strictly a case of the British royal family visiting the Western people. (*News Drum,* 21 July 1978 [hereafter *ND*])

Union Jacks still flew in Gizo. A Western flag had been produced, but it was not flown in place of the new national flag: Western Province was boycotting the Solomon Islands' independence, not declaring its own.

Western Council leaders were unhappy with the failure of the Independence Constitution to guarantee the devolution of powers to the provinces. The Western Council wanted a more federal political structure; it feared "internal colonialism" since its population constituted a minority in the country's multiethnic setting. Similar arguments were advanced by secessionists on Bougainville (renamed the North Solomons Province in 1976) which is adjacent to the Western Province but legally part of Papua New Guinea (Hannett 1975:286-93). The North Solomons, like the Western Province in the Solomon Islands, is in many ways the richest region of the state of which it is a part. Secessionist claims by leaders of the North

Solomons movement at times pointed to the ethnic unity between residents of North Solomons and the Western Province, alleging that indiscriminate juggling of colonial boundaries had separated the two parts (Hannett 1975).

The so-called breakaway movement of the West did not display the same overt passions, articulate arguments, and mass mobilization of the Bougainville secessionist movement (Premdas 1978). While some consultation occurred between leaders in the two movements (Hastings 1976), the activities of the Western movement were almost always undertaken by legal means. Some Westerners wanted more powers within the larger Solomon Islands state. Some, particularly New Georgia MPs, called for separation up until early 1978.

In this study we examine first "the West," as a unit. Second, we identify and analyze the factors that led to the emergence of the breakaway struggle, and third, describe the organization of that movement. Finally, we set forth the government's response to the demands and tactics of the movement. As far as possible, we will approach the materials by using guidelines derived from the patterns displayed by other breakaway movements around the world.

The West

The West is one of seven provinces in the Solomon Islands. It comprises the island groups of Choiseul, Shortlands, and New Georgia, and has a land area of 8,660 square kilometers, making it the largest in the Solomon Islands. The islands are widely. dispersed, making communications difficult. While the most western group, the Shortlands, is only a few kilometers from the North Solomons province of Papua New Guinea, it is 150 kilometers from Gizo and over 500 kilometers from Honiara, the national capital. The population of the West is about 40,000. Its economy is the most monetized in the Solomon Islands, providing a large number of employment opportunities in timber extraction, fishing, and plantation agriculture.[1]

The western extent of the British Solomon Islands Protectorate was not settled until 1899, when claims over the Shortland Islands, Choiseul, Santa Isabel, and Ontong Java were transferred from Germany to Britain (Scarr 1967). The physical presence of colonial government was limited beyond the establishment of district offices in Gizo by 1899, and at Faisi in the Shortland Islands in 1906.

The sense of "the west" as a unit, linked to Bougainville, and frequently contending with its own distant central goverment, has many of its

roots in missionary activity. The Methodists were the first mission in the Western Solomons in 1902. Apart from an excursion to Guadalcanal, they had little influence elsewhere in the protectorate. From their base on New Georgia, at Kokengolo near what is now Munda at the western end of the Roviana Lagoon, they evangelized the Shortland Islands, Choiseul, and then Bougainville.[2]

In the 1976 census half the population of the Western Province was reported to be Methodist: 19,500 were members of the United Church, which also has congregations in PNG, and 4,600 belonged to the Christian Fellowship Church, which had broken away from the Methodist mission in 1959-1961, and is now centered on northwest New Georgia.[3]

The spread of Methodism may account for some of the Western breakaway movement's separatism and identification with Bougainville. The movement's style of challenge to central authority also follows precedents set by the Reverend Goldie, who led the mission from 1902 until 1951, and became a member of the government's Advisory Council. Goldie was a political and commercial entrepreneur on behalf of the mission, and a relentless advocate of what he felt were the interests of Westerners against the arbitrary exercise of authority by central government officials (who called him, ironically, "King" Goldie).

Before 1972 the Western district was administered as a whole, but was divided into subunits for local court and local council purposes. British colonial policy, according to Healey (1966), was to create subdistrict courts and councils that "were regarded as preparatory to larger councils which were to be introduced when social change had broken down 'excessive parochialism'."

Events in Malaita related to Maasina Ruru in the late 1940s and early 1950s were to overtake this gradual approach (Keesing 1978). The leaders of Maasina Ruru established a federal council that extended its government throughout the island of Malaita. As a result, British administrators in 1953 used the Malaitan case as a precedent to integrate smaller local subdistrict units into larger operational entities throughout the protectorate.

By 1963 the Western district included five local councils: Shortlands, Roviana (covering southern New Georgia), Marovo (northern New Georgia), Choiseul, and Vella (at the western end of the New Georgia group). In 1972 these five councils agreed to amalgamate into a single Western Council (see Campbell 1974). The political significance of this voluntary unification has perhaps been overshadowed by the subsequent integration of the other island councils under the *Local Government Plan of Operations,* between 1973 and 1977.

The test for Western unity was Choiseul, for it had earlier had its own local council. As a large separate island, Choiseul could have expected its council to survive the amalgamations of 1974-1977, emerging as a separate province--an expectation that Shortlands could not have had. Early in 1978 the Western Council successfully asserted its claim to be sole representative of the West in its dealings with the Kausimae Committee which had been set up to make recommendations about the form of provincial government. Thus Choiseul's views could not be directly tested. The committee accepted this assertion of semi-sovereignty, and went to Gizo to meet the council in May 1978. Choiseul, however, wavered as its two MPs (Zoleveke and Dorovolomo) came out more or less equivocally for separate provincial status for Choiseul. The Kausimae Committee recommended no change in provincial boundaries, but gave the central government the right of review if there were "serious demands" for separation within a province.[4]

Factors Related to the Emergence of the Western Movement

The Western breakaway movement emerged from a complex interrelationship of "fundamental" and "facilitating" factors. The fundamental factors[5] are relatively permanent characteristics such as common color, language, values, territory, and history, which support the underlying cohesiveness of the movement. They may not be altogether as factual as those who invoke them would have us believe. For example, although Western nationalists may claim that Roviana is the lingua franca for Westerners, in fact it is as much a divisive influence as a unifying one because of the diversity of languages spoken. However, the significant point is less the objective fact than the belief among a movement's participants that they share a characteristic in common. The sense of being a single family with a common identity is a psychological phenomenon (see Connor 1973:1-21). Claims to shared characteristics perform the critical role of suffusing a population with a collective spirit. We may refer to this collective psychological condition as "nationalism," "ethnonationalism," or "subnationalism" (Connor 1973). Myths may be created and shared to aid the development of a common identity.

The facilitating factors refer to complaints such as an economic injustice. While the facilitating causes may be solved, the fundamental ones usually persist and may be used in the establishment of new movements in the future. Further, the facilitating factors tend to be rational items while the fundamental factors tend to be irrational. When a government seeks

to solve the former, it cannot delude itself that the latter can be eliminated simultaneously.[6] In the following discussion, we examine first the fundamental factors, then the facilitating ones.

The Fundamental Factors

The fundamental factors discussed here are as follows: territory; language, ethnicity, and values; color; and history.

Territory. The boundaries of the Western Province, as mentioned, were demarcated by colonial and missionary practice, and not every subunit or island unit is comfortable within this territorial unit. Choiseul leaders have from time to time expressed the desire for a separate province. Nevertheless, a discrete and separate territory associated with the Western Province has emerged in the consciousness of most Westerners. In a submission to the Kausimae Committee their spokesman argued:

> This means that the way our islands have been arranged by the creator has been such that the geographical locations have in numerous ways determined how far and with which island groupings the majority of our people have identified themselves and have a growing emotional attachment. Geography tends also to have demarcated the territorial extent of such attachment.[7]

Once attachment is developed around a territorial unit, the inhabitants define themselves partly in relation to that entity (see Enloe 1975 and Barth 1969). Hence, a "Westerner" is associated with a specific block of land, with claims to it to the exclusion of other parcels of territory associated with other groups. "Territory" and "land" are interrelated concepts. Hence, any unpermitted intrusion or alienation is an assault on the entire West. We take up the issues and complaints related to land later. Suffice it to note here that territory and land are fundamental characteristics of all breakaway movements. Their pivotal place in the consciousness of Westerners united them against migrants, resettlement schemes; and government land allocated for large projects in their midst.

Language, Common Ethnicity, and Values. Language, common ethnicity, and values are fundamental traits that help forge a movement such as that in the West (see Emerson 1964). Although there are eighteen dialects and five main languages, Roviana, the vernacular used by the Methodist church, is claimed as the lingua franca among Westerners. Pidgin may be used to unify all Solomon Islanders, but Roviana is the language that separates Westerners from non-Westerners.

The Western Submission (1975:1) noted how a sense of common ethnicity arose from contact with other people:

> When the people of the Solomons were still uneducated and restricted in travelling from place to place, the problems of ethnic pride, identity and different value systems were not as prized as they are today. The clolonial government and the national government think that people from different islands are uniting, but the people are not the same people; they are not homogeneous. . . . This is a dynamic reality because it involves human feeling, wantok system, ethnic pride and values.

Arguments may be advanced to show that Westerners are either divided into cultural and linguistic subunits or united by a common Melanesian value system despite their regional variations. In either case, this does not negate the *belief* among Westerners that they are different.

Color. The color of the typical Westerner is jet black. Most other Solomon Islanders are of a lighter pigmentation. This fact unites Westerners as a separate group, in part because their blackness may be a source of ridicule from other Solomon Islanders. In turn, Westerners may regard other Solomon Islanders with similar racist ridicule. The color factor contributes to a process of in.-group solidarity and out-group stereotyping. The black pigmentation is a ready symbolic instigator of real and imagined interests separate from those of other groups. For example, in a speech in August 1977 calling for Western separation, MP Geoffrey Beti asked rhetorically:

> Is it because we are black as compared to other people in Solomon Islands that the government does not want to meet our wishes? (*ND,* 28 August 1977)

It took the publication of the infamous "Ode to the West Wind" poem in 1978 to bring dramatically to the surface the anti-Western prejudices imputed to non-Westerners. Parts of this poem refer directly to color in a racist and offensive way:

> Ode to the West wind, you carry in
> Your bowels the Westerners
> Black and ugly, proud and lazy
> Manpower they have none.
> (*ND,* 9 June 1978)

The poem was translated into Roviana and other Western languages by the leaders of the movement and was widely distributed throughout the West. The incident created a national scandal, but underscored the point that color prejudice was harbored by other Solomon Islanders and helped to justify the separatist claims of the West.

History. The Solomon Islands, as a country and a state in the international system, is a European artifact created by the needs of colonialism and decolonization. The 'West, as a separate and distinct political subunit, is also a creation of colonial and mission history. It benefitted first and most from the introduction of European educational, health, and economic facilities. The West is now one of the more thoroughly monetized and economically developed regions in the Solomon Islands. Westerners take pride in their preeminence, whether strictly factual or not. However, in an independent Solomon Islands, they would be a minority with only eight of the thirty-eight seats in Parliament. A central government dominated by non-Westerners, especially Malaitans, could direct revenue from the West to develop less advantaged parts of the nation.

The threatened loss of status in the economic sphere was a fundamental cause of the West's demand for autonomy:

> Without a form of government which could create a united nation through respecting the regional differences, the effects of the present government structures, powers, functions could only lead to the overrunning of the numerically weaker regions by the numerically stronger regions.
>
> (Western Submission 1975:3)

The Submission called for "proper constitutional guarantees for the numerically weak" (1975:2). The fear of "internal colonialism," the domination of one group by another, was interwoven with the fear that the West would be eclipsed as the most economically developed region.

The Facilitating Factors

The facilitating factors will be discussed in the following order: political-administrative demands, land, migration, and revenue. A fifth item, "a foreign factor," is termed facilitating since it might have encouraged, the claims of Westerners.

Political and Administrative Demands. The Western Council did not demand a separate sovereign nation although particular Western MPs expressed such a desire.[8] Certainly if the council's demands had met a com-

pletely negative response, then the road to nationhood would have been open. The timing of their demands was also important. Constitutional changes were being proposed for a new independent country and pressures for independence were coming as much from the British as from local leaders. Lest internal colonialism be institutionalized in the fundamental laws, the threatened units had to agitate without delay for special protection. It would have been futile for the West to try to change the constitution after independence. In the words of the Western Council, "the establishment of a state government for the West should not be allowed to come after independence" (Western Submission 1975:2), but should be set up before independence. Unable to gain the votes needed to change the constitution after independence, the West could always try to secede. But it could do this more legitimately before independence, particularly if it could persuade Britain to accept its claims, as Tuvalu did in 1974-1975. The West's bargaining power with Britain was increased by the fact that the pressure for independence was more a push from Britain, than a pull from the Solomon Islands. There was certainly a fear within the Solomon Islands' government that Britain might do anything to get out. The Western movement therefore was caught in a triangular relationship with the Kenilorea government and the retiring colonial government.

The West stated that it wanted a politico-administrative arrangement that would extend to it maximum autonomy and permanent safeguards for its interests. It pointed to the diversity of the land, the people, and their customs as a basis for its claims. It argued that a single centralized apparatus would not adequately recognize the diversity:

> The Solomon Islands' structure and principles of government should, where possible, reflect the different cultures, respect the ethnic divergencies, take into account the geo-political factors, and above all, answer the wishes of the people.
>
> (Western Submission 1975:3)

The West argued that centralizing forces were stifling local initiative. Local governments were merely carrying out decisions made at the center:

> The present [i.e., 1975] local government councils act only as local coordinators, overseers and agents of the central government's plans and policies.
>
> (Western Submission 1975:2)

The Westerners wanted a system of government that returned initiative to them for local development. They wanted a division of powers entrenched in a constitution. They were aware of their minority status in relation to the rest of the nation. When maximum local powers were assigned to them, they wanted to ensure that the concession could not easily be revoked at the convenience of the central government. In effect, they demanded a federal arrangement in which separate spheres of exclusive powers would be created. The coordinate units in this arrangement would not be permitted to overstep their powers, while simultaneously each would be in no doubt about its realm of responsibility. The national or federal government would relate not to subordinate regional units, but to coequal units called provinces and sometimes states:

> It is envisaged that the desired structure of provincial government for the West would be one which follows, with drastic modifications, Federal principles and system of government. . . . It is required that when the provincial government system is established, the national constitution should define the areas of responsibilities, functions and powers which would regulate and justify the existence and activities of the central and provincial governments,, and in so doing, the central government and the provincial governments should be self-ruling coordinate bodies rather than subordinate to each other as is the case under the present system of government.
>
> (Western Submission 1975:4)

The allocation of such sweeping spheres of protected powers free from interference by the national government was bound to create suspicions about the ultimate intentions of the Westerners. Such powers, if extended, might be but one short step to full autonomy and independence. What was equally alarming was the encouragement such proposals might give to other groups in the Solomon Islands. The accommodation of diversity in a federal arrangement could drive the various linguistic and island groups further apart. The Western movement, however, posited that a system of government that accommodated legitimate regional differences would no doubt cultivate provincial or state subnationalism, but that this was a first indispensable step in nurturing a sense of nationalism for the larger federal unit:

> Unity is indeed desirable, but mutual unity and respect can only be grown, not imposed. There are cases where the organic

> growth of unity has emerged. Take the formation of the Western Council. The move of the different local government councils in the Western District to form the Western Council was initiated by local leaders with government encouragement. These leaders, through the increased awareness of their people in knowing that the Western district is in fact for them all and not for the Choiseul man or Morovo man only, came to realize that having one council would be better than having several. So the process of unity gradually grew. Many other factors also contributed to the Western Council, but *the process of unity and identity has spread from a tribe to a village, to a locality, to a whole island, to a district.* A *further process of growth should bring us state and national unity.*
>
> (Western Submission 1975:3; emphasis added)

This theory of the development of unity was, of course, that proposed by the colonial government in the 1940s and overtaken by Maasina Ruru, as pointed out earlier. In summary, then, the politico-administrative system that the West prescribed sought to solve problems relating to regional diversity, majority domination, and the stifling of local initiative. By having full control over the legislative, executive, and judicial arms of government related to all internal issues, they hoped to give their loyalty to the larger nation.

Land. Land in subsistence societies is an integral part of a community's social system. The identity of a people is as much linked to the land as to the language. Land as a concept may include resources on and under it, such as timber and minerals. In the Western Province, complaints had for many years been raised about the arbitrary alienation of land and its resources for plantations, resettlement schemes, timber, and other projects. The sea and seabed as an extension of land also raised controversy. The Western Provincial Assembly was already involved in licensing bait fishing and mangrove timber extraction (used for smoking fish) with Solomon Taiyo, the Japanese joint-venture fishing company based in Noro, New Georgia.

There are two kinds of land tenure systems in the Solomon Islands--customary and statutory (Larmour 1979). There are many different customary systems in the Western Province, and about 15 percent of the land is held under the statutory system, that is, "alienated." This has occurred in various ways, particularly by (a) sales to Europeans in the late nineteenth and early twentieth centuries, (b) government declarations of

"wasteland" between 1902 and 1914, and (c) sales and leases to the government, particularly for forestry purposes in the late 1960s and early 1970s.

During the early 1970s plantation land alienated by sale to Europeans was gradually being returned to customary ownership through a plantation purchase program which involved government loans and technical assistance to groups buying back their land. This took the pressure off several severely affected areas, particularly Vella. However, the category referred to as "wasteland" remained an issue. The area of land alienated under the wasteland declarations had been reduced in the 1920s but the problem remained highly contentious, particularly on Kolombangara where two thirds of the island was alientated in this way.

In 1977 the Parliament passed an act converting the remaining 60,000 hectares (ha) of land owned by non-Solomon Islanders into leases from the national government. While asserting national sovereignty, this act undermined provincial autonomy. Government by then owned 1,073 ha of land in the Western Province, including land alienated under wasteland declarations, or about 12 percent of all land in the province. This was a higher percentage than in any other province except Central and Eastern Islands (and Honiara). In terms of area, it was three times larger than the next province, Isabel. Of the 1,073 ha of government land in Western Province, 828 ha were held for forestry use and another 43 ha for mining purposes. The Western provincial government itself owned hardly any land. The capital, Gizo, was on central government land, and the province only had title to land for some public services like airports. Acquisition of land remained a central government responsibility until a 1979 private member's bill in Parliament provided for provinces to acquire land.

Particular areas of conflict between the central and Western provincial governments on land issues included:

(a) Kolombangara, a problem area since the early part of the century, when Western people had been successful in rolling back at least some of the area declared as wasteland. Opposition to the wasteland declarations, expressed to the Phillips Land Commission in the 1920s, was an important precedent for Western political consciousness as the different landowner groups presented themselves as "one people" against the government (Heath 1979:202-7). Kolombangara re-emerged as an issue in the late 1960s when Levers began to cut timber on land they had never used before, and from the mild-1970s on when government timber replanting began.[8]

(b) North New Georgia, where the government had been trying unsuccessfully since the early 1970s to lease land for timber cutting. Then, in-

cidentally, the Christian Fellowship Church was opposed to the government.[9]

(c) The Shortlands, were timber leases and sales of land were successfully negotiated in the late 1960s, but where pressure had since emerged for renegotiation.

(d) Resettlement schemes, where government land was subdivided on lease for settlement by individual Solomon Islanders. By the mid-1970s the Western provincial government had the power to recommend allocation of this land and could give preference to Westerners.

(e) The Gilbertese settlements at Wagina and in Gizo where the government granted freehold rights to Gilbertese resettled between 1955 and 1971 (Bobai 1979). While saying they were not hostile to the Gilbertese as such, Western leaders resented the fact that their province took all the burden of Gilbertese resettlement.

(f) Land still owned by expatriates or mixed-race planters, a declining number since the establishment of the plantation purchase program, but an issue when the government continued to protect the rights of Solomon Islanders who had bought alienated land (especially if they were not the true customary owners), or of the mixed-race descendants of planters.

(g) Management of urban land in Gizo that was owned by the central government but allocated by the province. This divided responsibility led to confusion and delay as cases were referred back to Honiara.

Migration. Twelve percent of the population of the Western Province was not born there, the largest non-Western grouping being 1,686 people from Malaita. Guadalcanal and Central Islands both had higher percentages of inward migration (19 percent and 23 percent). Nineteen percent of the total employment in the country was in the Western Province, the largest percentage after Honiara (34 percent). Yet in spite of the migration toward the West, 69 percent of the men employed in the Western Province were born there, a higher proportion than in any other province.[11]

Linked with the land issue is the question of free movement. The Constitution protects the right of movement in s.14. Yet migration was felt to have led to a dramatic rise in crime in many communities: to illegal occupation of traditional land and the creation of squatter settlements around the towns; to competition for scarce local jobs; and to ugly rivalries for women. It was argued that the social and cultural impact of uncontrolled migration could offset the abstract or economic gains attached to the fundamental right of freedom of movement. That, implicitly, was the case submitted by the Western and Guadalcanal Provinces.

More people migrated into the Western Province than left it. But the issue was complicated by the fact that a particular regional group, Malaitans, was the target of attempts to limit or control migration. To them were attributed the propensity to crime, arrogance, squatting, job rivalry, and fights for women and, through marriage, land acquisition. Malaita Province is more densely populated than other provinces, and parts of the province have never accepted the Christian church, thereby providing a religious rationalization for hostility toward their alleged antisocial behavior. At the same time, Malaitans are reputed to be dynamic and hard workers. They have tended to displace local residents in the competition for jobs, not least because they do not have access to land.

Western fears and prejudices were expressed particularly toward Malaitans for perhaps two related reasons. Malaitans were living within the West, and they also came from the largest province within the Solomon Islands. So the Malaitan presence in the Western Province both symbolized and aggravated Western fears of political and economic domination within the wider national unit.

Revenue. When in August 1977 the Honorable Geoffrey Beti, member for Roviana and North New Georgia, moved in the Legislative Assembly that the central government "amicably agree" to the West becoming a separate nation, he argued that Western development was being neglected, although the district contributed most to the national economy (*ND*, 28 Aug. 1977). The Finance Minister successfully appealed to him to withdraw the motion before it came to a vote.

Measuring the West's contribution to the national economy depends on judgments about the relative importance of land, labor, and capital in production. These judgments are by no means settled in the Solomon Islands and are reflected in still unresolved debates about "benefit sharing" with landowners and investors in capital projects, expressed in arguments about appropriate rents. The Solomon Islands is moving away from colonial cheap land policies, but the new principles are still not settled and will, of course, depend much on bargaining with big companies. In relation to Western secession, the issue is to what extent the West's "contribution" to national revenue is produced by Western land, or by migrant labor from other parts of the country, particularly Malaita. Similarly, even more difficult questions of principle arise over control of marine resources, another part of the West's case.

Quantifying neglect and the contribution of particular units in a territorial economy is difficult both in practice and principle. Statistics may not be kept at district level and some items of revenue and expenditure may either be particularly "lumpy" or difficult to evaluate except on a per capita basis. Figures produced in the mid-1970s, when colonial "ne-

glect" (shared by all provinces) was to some extent being remedied, do not necessarily satisfy leaders articulating a more historical sense of injustice. Even if the relevant figures can be produced, it is unclear with what they should be compared. Western leaders consistently rejected population as a criterion for comparison, and a movement intent on separation is unlikely to be persuaded by arguments that some parts of the country are in the same position or, indeed, worse off.

Statistics never played a big part in Western arguments, no doubt because Western leaders felt that this was a style of argument that central bureaucrats would always win. There was never any Western attempt to calculate statistically their contribution to national production. The figures produced for the Kausimae Committee certainly do not show the West at any great disadvantage in relation to its population, though they show that it produced a disproportionate amount of some commodities (e.g., timber, but not palm oil or rice). It also did relatively well in terms of services, except perhaps roads.[12]

Production in the Western Province (as % of national)

Product	%	Date	Comment
Cattle	66%	1976	based on number of cattle
Copra	26%	1977	36% of small holder production
Cocoa	29%	1977	
Timber	33%	approx	all from Levers Pacific Timbers
Palm oil	0		Guadalcanal only
Rice	0		Guadalcanal only
Minerals	0		only large prospect was on Choiseul
Fish			difficult to attribute: one of two Taivo bases is in Western Province

Government Services in the Western Province (as % of national)

Service	%	Date	Comment
Primary schools	31%	1977	
Secondary schools	14%	1977	2 of 14
Clinics	25%	1976	
Hospitals	33%	1977	2 of 6
Public road mileage	14%	1979	but 44% if Levers logging roads are included

Attribution of Government Revenue and Expenditures (1979 estimates)

Revenue from West	%	Expenditures to West	%
Taxes and Duties		Transfers and Expenditures	
Income tax	23%	Direct recurrent transfer to Provincial Government	23%
Import and excise	17%	Other government recurrent expenditure	17%
Export duties	36%	Capital transfer to Provincial Government	34%
Other	14%	Other government capital expenditure	n a

The estimates of government revenue and expenditure come from the Finance submission to the Kausimae Committee which noted:

- The relative importance of Honiara to government revenue.
- The interdependence of provinces. For example, while Malaita yielded only 9.9 percent of revenues from a population of 30.5 percent, Malaita people in the rest of the Solomons were paying taxes elsewhere.
- The "lumpiness" of sources of revenue. Over half of Western's contribution to export duties (36 percent) came from one company (Levers Pacific Timbers), employing people from all over the Solomons. If the company moved, the West would score only 22 percent.
- The figures for government expenditure other than through provincial government could only be computed on a per capita basis. Capital expenditures other than through provincial budgets were also particularly "lumpy."

Western's position in the national economy was similar to that of Guadalcanal and Central Islands provinces. Each produced a disproportionate amount of export revenue from a few big projects on alienated land, and each employed people largely from other provinces, particularly Malaita.

These figures provide only a snapshot image of the late 1970s; they do not indicate the rapid changes taking place in the economy, and the West's position in it. The movement's concerns originated in the early 1970s and reflect a sense of both the past and the future--looking backward to the loss of preeminence in the more static economy of the 1960s, and forward to concerns about the future, even if absolute prosperity increased. Thus rapid economic change, whatever its outcome, may be as important a source of unease as the province's particular place in the economy at any one time. This unease might also be increased by the "lumpiness" of sources of revenue. The establishment or closure of one or two projects could greatly change a particular province's relative and absolute position, but this may be outside provincial control.

Rapid changes took place around independence, the period of the Western movement's greatest leverage. In the mid-1970s, the large projects set up at the start of the sixth development plan[13] were gearing up at the same time that independence had given the Solomon Islands access both to new sources of aid funds, and the provisions of the "financial settlement" with the United Kingdom.[14] Commodity prices (particularly copra following the first oil crisis) were also high in the mid-1970s. This

economic buoyancy gave the central government the capacity to buy off secessionist pressure, but simultaneously provided an economic incentive to secede for a copra-rich province such as the West. The Western movement might have been much more bitter in a stagnant economy. Economic growth tends to soften conflicts over redistribution.

Population. Rather than argue about figures, the Western movement took issue with the principle of comparison and attribution, particularly the use of population as a criterion. The West, in fact, was the second largest province in terms of population. The population argument was articulated particularly in relation to central grants to councils, later called provinces. For example, Jerry Buare's July 1978 radio interview listed the population principle with the West's "six points" (control of land, finance, internal migration, legislation, staff, and natural resources), and its claim for revenue sharing, as the sources of Western grievance (*ND,* 28 July 1978).

Population in Allocating Grants. Between 1975-1977, central government grants to councils for recurrent expenditure were allocated according to the cost of running services transferred to the councils, and the amount of revenue the councils had raised the year before.[15] The services grant caused friction between ministries and councils over whether the grant covered actual costs, particularly if the council wanted to improve on often poor central government performance.

The allocation of a relatively untied capital grant called General Development Assistance (GDA) provided the opportunity for discontent about distribution. The guidelines used to divide up the total available were a mixture of relative population and relative land area. These were objective variables and easily quantified, but having been made explicit, their appropriateness became a matter of political debate. Some of the heat was taken out of the argument with the increase of the absolute level of grants to each council as services were transferred, as the size of the national budget increased, and as a greater percentage of the budget was spent through councils. Growth made redistribution easier.

Population became such a contentious criterion that the formula proposed by the Kausimae Committee recommended that it be dropped as a criterion for the distribution of funds between provinces. However, it crept back in as "manpower" to appease Malaita representatives.[16] The committee's hostility to population was not simply an alliance against the most populous province, but an attempt to inject a dynamic and developmental principle into the allocation process. The principles it proposed were a mixture of rewards for enterprise, and compensation for its ill effects, or lack of resource endowment. Population as such was felt to be too static.

The preference of Western representatives for a "production" rather than "consumption" principle had two elements: a straightforward regional preference for a criterion that it felt would be to its advantage; and a more forward-looking attempt to stimulate production in the province, particularly by the release of land. Western's representatives acknowledged that the province's land resources were relatively underused (4.5 percent relative to other provinces, the lowest), and wanted to put pressure on landowners to release more. Suppression of population as a factor appealed also to other members of the committee on these self-reliant grounds. In any event, the central government moved ahead of the Kausimae Committee to meet Western Province's argument about its contribution to national revenue. By 1977 the *Plan of Operations* was almost complete. The Minister of Finance announced in February 1978, in the midst of the Western crisis, that he was changing the principles on which grants were allocated to provinces, as well as increasing the amounts (*ND* 7 April 1980). The service grant remained but was frozen at its 1977 level. The grant that had been related to local revenue raised, and the additional funds made available were reorganized and reallocated according to principles of derivation--that is, from where the central government obtained the revenue that paid for the grants. By this device, 90 percent of vehicle licensing and drivers' revenues, 5 percent of import and direct tax revenues, and 10 percent of estimated export duties would be returned to the provinces in which they were collected.

Population did not appear directly in these principles; it was brought into operation as a means for calculating the division of national revenues from fish exports that were to be divided between provinces. But population had not, in fact, previously appeared in the principles for recurrent grant allocation, and it remained in the formula for allocation of GDA. Nevertheless, the central government had now represented its principles of allocation in derivation terms much more thoroughly than, for example, Papua New Guinea produced to meet Bougainville's demands. And it softened any consequent redistribution by general increases for all provinces.

The principles were decided by the Ministry of Home Affairs within global sums set by the Ministry of Finance. The Kausimae Committee recommended that allocation be taken out of the hands of officials and government ministers. Further, the principles must be fixed and be relatively permanent, a move in the direction of institutions like Papua New Guinea's National Fiscal Commission, or the French Territories' *Fonds Intercommunal de Perequation*.[17] The outcome of this reorganization, redescription, and increase in recurrent grants is shown in Table 14.1.[18]

TABLE 14.1: Changes in Grants 1977-1978

Council	1977 grant ($)	% total	1978 grant	% total	% increase 1977-1978
Western	308	24	400	24	30
Eastern Islands	67	5	92	5	36
Guadalcanal	121	10	240	14	78
Honiara Town	90	7	157	9	74
Makira/Ulawa	141	11	173	10	22
Malaita	384	30	412	24	7
Isabel	70	6	103	6	46
Central Islands	86	7	124	7	44
Total					

The new derivation principles left the West with almost exactly the share it had before (in fact, a slight drop from 24.31 percent to 23.56 percent), though its grants increased 30 percent. Five other councils did better in percentage increase terms.

Foreign Models. By "the foreign factor" we mean the direct or indirect role that a foreign country or its citizens might have played in either instigating or sustaining the Western movement. Breakaway movements tend to be "inviting waters in which foreign powers fish" (Duchacek 1974:68). The proximity of Bougainville to the West has had at least a demonstration effect. Bougainville is geographically and culturally related to the West, particularly the Shortland Islands. Leo Hannett was the ideological guide of the Bougainville secessionist attempt (see Hannett 1975). In the late 1960s, when Hannett was a student at the University of Papua New Guinea, several Solomon Islands students were there also, and sympathized with Hannett's position on Bougainville. Indeed, the Mungkas Society, consisting of Bougainville students, regarded the Western Solomon Islands students at UPNG as part of their group. One observer suggestively noted the frequent visits of Father John Momis to Honiara in 1975 (Hastings 1976).

In the early 1970s both Papua New Guinea and the Solomon Islands were moving rapidly toward independence. Both countries encountered claims for greater regional or provincial autonomy, and the claimants enunciated similar arguments for their cause. Indeed, the similarities and connections between the two cases are striking. Papua New Guinea gained independence first. This was followed by the dissolution of the Bougainville provincial government, and a dramatic confrontation with the central government. The Solomon Islands was an observer to these events, although its interest was recognized in a minor provision of the Bougainville Agreement requiring consultation with the Solomon Islands' chief minister about the new name for the North Solomons Province.[19]

The Bougainville secessionist leaders clearly stated that following their independence, should they succeed, they expected to amalgamate with part of the Western Province. Hannett was insistent on rectifying what he regarded as an arbitrary separation of the geographical and cultural unity of the Solomon Islands.

In a slip of the tongue, Australia's Prime Minister Whitlam had publicly suggested that Papua New Guinea and the Solomon Islands unite as a single independent country (Griffin 1973:319). Further, the departing British government seems to have wanted to regard the Solomon Islands as part of Australia's regional responsibility. While these visions differ from Hannett's, the idea that new entities might be created at independence was an obvious encouragement to the secessionists, as was demonstrated by British acquiescence in Tuvalu's separation from the Gilbert and Ellice Islands by referendum in 1974.

Organization of the Movement

The movement was centered at the Western Council headquarters in Gizo. The Western Submission, discussed above and presented to the Kenilorea government and the Kausimae Committee in May 1980, was originally circulated under the signature of the president of the Western Council in August 1975. Again in 1978, it was the council that provided the movement's focus and minimum "six-point" program. After a long discussion, with six of the West's eight members of the Legislative Assembly present, the council agreed on a motion in early 1978.

> The Solomon Islands Government should give serious consideration in the forthcoming Legislative Assembly meeting to granting State Government to the Western Solomons with full control over finance, natural resources, internal migration, land, legislation and administration *before Independence*, and if this is not granted, the Western Solomons will not be participating in the national Independence celebrations, and may possibly declare eventual unilateral independence.
>
> (Western Council minutes 20/78; emphasis added)

The infrastructure of the council--offices, vehicles, stationery, and staff--provided a means through which the movement could be articulated and organized. The Western Council also provided the major source of direction and leadership. Early in 1978 a political committee was established to coordinate council, national, parliamentary, and agitational

activities. While no Western political party was formed at the national level, one of the movement's leaders, John Talasasa, identified with the parliamentary opposition, while the six Western MPs outside the cabinet acted together to walk out of the Legislative Assembly in April 1978 (*ND,* 14 April 1978). A showdown with the central government would probably have led to the official dissolution or suspension of the council, as had occurred on Bougainville.

Support for the movement is hard to prove but seems not to be in doubt. Western MPs called for a referendum on separation, but the Western Council would not allow the Kausimae Committee to tour the region. The Western people seemed to support the drive to have their list of grievances met before independence. No serious official charge was ever made that the movement had only minority support. An effort was made to raise funds among the village people, but the main resources in support of the movement were the council facilities.

The leadership of the movement was collective, if sometimes divided. No single leader stood above the others for any length of time. For example, John Talasasa was influential during late 1977 and early 1978, particularly with an inflammatory speech made at Munda on 26 January 1978. Talasasa had become Member of the Legislative Assembly for Vonavona, Rendova, and Tetepari in a by-election following the death of his brother, Francis Aqoroau, late in 1978. Talasasa called for "breakaway,"but by February 1978 he was reported to have moved behind the Western Council in its demand for state government "because it would be unwise to go against each other for the sake of personality and politics" (*ND,* 17 Feb. 1978).

The collective nature of the leadership was partly dictated by the internal diversity of the Western Province. Different islands and communities had their own recognized leaders and there were clear differences of style and emphasis between Choiseul, New Georgia, and the Shortlands. Even the Western Council president, Jerry Buare, lacked grassroots support outside his own constituency. He was elected by the councillors who, in turn, were individually elected from separate council electorates. National parliamentarians from the West were also similarly elected from wider constituencies with loyalty to specific leaders. No leader commandcd the loyalty of the West as a whole. Collective leadership included not only national parliamentarians and council leaders, but chiefs and various community and opinion leaders throughout the Western Province. In this dispersion of leadership, the grassroots strength of the movement was affirmed. But a collective leadership with so many centers--national, council, island, village, community, etc.--had the inherent problem of main-

taining unity and coordination. Activist leadership tended to come from the Shortlands, particularly from Peter Salaka with his experience of street politics. But not all parts of the leadership were similarly activist or highly committed. There were also so-called moderates and alleged fence-sitters. It was felt that the West's parliamentary representatives failed to utilize that forum effectively. Overall, one of the weaker aspects of the movement was its leadership.

A number of voluntary associations supported the demands of the West. These included the Christian Fellowship Church (which financed an Australian lawyer to draft the West's proposed constitutional amendments) and the Kolombangara Association. The Christian Fellowship Church, in particular, as an indigenous social and religious movement that had broken with the Methodist church in 1961, provided a model for successful separatist activities as well as a source of funds. The opposition party, NADEPA, supported the movement, at least to the extent of criticizing the government's handling of it. NADEPA's platform called for a federal system of government. In addition, other local governments such as Guadalcanal Council supported the movement's demands for substantial devolution of powers.

The Prunsvick Association became a focus for the movement during 1978. It originated in a football club, and its name was formed by taking the first letters of the names of Western island groups. Prunsvick opened an office in Gizo early in 1978 and published one issue of a newsletter before its closure in October. The association also had a branch on the University of Papua New Guinea campus (*ND,* 20 Oct. 1978).

The movement's methods of communicating its demands to the central government were mainly nonviolent and legal. They ranged from resolutions and submissions issued from the Western Council to speeches and veiled threats by Western national parliamentarians. Record of only one demonstration in Gizo exists.[20] When the movement became organized in 1974, certain leaders talked of seceding and utilizing force if necessary, but this aspect of the movement remained in the shadows.

Lively interest in the movement was sustained by certain outside events. Apart from the Bougainville secessionist struggle and Tuvalu's separation in 1975, the ongoing constitutional debates in the Solomons kept the demands of the West on the agenda. As steps towards self-government and independence were gradually taken, the issue of decentralization had to be resolved. Certain unplanned incidents accelerated the movement's drive to attain its goals. One such incident was the publication, already mentioned, of the "Ode to the West Wind" in the government newspaper, *News Drum* (9 June 1978). The poem ridiculed the

Westerner's aspirations for greater control of his destiny. At first, it was widely but erroneously believed that the poem was written by the prime minister's special political secretary.[21] Since both the prime minister and his political aide were Malaitans, the poem became a highly inflammatory issue. It provided further proof that the West needed special constitutional guarantees lest it become a victim of the prejudices of more powerful ethnic or regional groups. The minister of Home Affairs, Francis Billy Hilly, a Westerner, resigned from the cabinet and later became the president of the Western Council.

Up until independence, the West continued to press its demands. When the country's Constitution came into effect without specification of provincial powers, the West refused to celebrate independence. The issue of decentralization was still 'being discussed in the Kausimae Committee, which did not report until March 1979. The government responded only in October 1979 in the form of a White Paper.[22] In July 1979 the Western Province, however, did celebrate independence although the decentralization argument was now unlikely to be resolved until after the 1980 elections. But the mood was conciliatory. *News Drum* quoted Oliver Zapo as saying on behalf of the Provincial Assembly that "the recent compromise on the West Wind poem had reestablished a mutual trust and understanding between the central government and the Western Provincial Assembly" (*ND,* 20 Aug. 1979). According to the prime minister, "the celebrations marked the end of an era which really tested the patience, endurance and forbearance of both the government and the Western leaders . . . these celebrations are political achievements" (*ND,* 20 Aug. 1979). In December 1979 the Western Provincial Assembly elections were held despite the Kausimae Committee's draft recommendation for a delay. Peter Salaka defeated Jerry Buare for the Inner Shortlands, and Billy Hilly won in South Ranongga, going on to defeat Salaka for council president. *News Drum* quoted Billy Hilly saying that he "favoured a gradual approach to devolution, as more provincial power could not only be a blessing but also a curse" (*ND,* 1 Feb. 1980). In the July 1980 national elections, John Talasasa was defeated. Zoleveke did not stand, and Billy Hilly won and accepted the position of deputy prime minister in a new coalition government led by Malaitan, Peter Kenilorea.

The Central Goverment Response

The central government's response to the demands of the Western movement took the form of limited concessions and rational bargaining. Certainly there was bargaining over the payment of compensation for the

West Wind poem. The central government offered $7,000, while the council demanded more but compromised on $9,000. But it was a highly symbolic process, appropriate to the emotional appeals of "Westernism" and for redress of a wrong committed against ethnic dignity. By being prepared to discuss compensation, both sides indicated their willingness to transform a complex emotional issue into a single calculus of cash. Compensation for injury is a familiar part of Melanesian conflict resolution.

However, a rational. bargaining model does not fit easily into the early stages of the conflict. To bargain, you need clear sides. Yet at least until early 1978, neither side was distinct or in control of its supporters. The West's council, MPs, and other leaders spoke with different voices. The central government cabinet and parliament lacked unanimity, partly because their numbers included Western leaders. The publication of the West Wind poem suggests also that the central government could not control its day-to-day response to the movement. Only with the formation of the Western political committee, the walkout of the six Legislative Assembly members, and the resignation of Billy Hilly, did the two sides begin to take shape. The polarization at least created the possibility of negotiation.

For a long time it seemed that the central government wanted to avoid direct negotiation. The idea of negotiation might have implied that both sides were of equal status. Simultaneously, the central government might have reasoned that negotiations themselves conceded a large part of the Western Council's claim to represent its supporters and the West generally. It was very important to the Solomon Island decision-makers to avoid the kind of substantive horsetrading, bargaining, and agreement that led to the resolution of Papua New Guinea's Bougainville crisis. The Bougainville Agreement resembled a process of treaty-making between sovereign states.

Several purposes can be deduced from the government pattern of response: to control the definition of the situation; to co-opt its potential leadership; to resolve the issue within the context of parliamentary institutions; to play for time; and to avoid crises.

Definition of the Situation. The Western movement was a complex mixture of rational and irrational demands, fears, and grievances. The Western leaders articulated these demands into a claim for Western uniqueness and a program for Western separation. The central government responded by treating it as a case (albeit a special case) of a problem between central and local government throughout the Solomons. If Westernism could be treated as a symptom of a national problem, then it became possible to conceive of solutions within a national context by reform

rather than separation or coerced unity. The risk in generalizing was that it might encourage separatist sentiments elsewhere, for example, in Guadalcanal or in the Eastern Islands. In that case, the general national solutions devised to deal with specific Western grievances would be counterproductive.

The switch to derivation principles for grants to councils in 1978 was a general response to Western claims that affected all provinces. The national government seems to have been fairly successful in broadening the Western issue to practical issues related to the diversity of the state. Hence, it indirectly extended claims of Western uniqueness to all provinces. By the same strategy, it persuaded the Western delegation to present its constitutional proposals to the Kausimae Committee where its demands were treated equally with other council submissions. It also avoided giving any directions to the committee to deal with them differently from those of the other provinces. The committee recommended a system of extensive decentralized powers to provinces short of a federal structure. However, each province had control over the pace of its application. It remains to be seen if a general system of decentralization stretched to fit Western demands could cause problems for other provinces. A similar generalizing effect took place in Papua New Guinea, where the "treaty" with Bougainville became the basis for an organic law applicable to all provinces, and after the McKinsey report, applied at the same time to all of them.[23]

Finally, the central government's purpose in treating the West province like the other was helped by the Western leadership's willingness to put its demands in a form that was, at least in principle, amenable to national political and legislative action, that is, the Western Submission.

To Co-opt Its Leadership. If a united nation was to be preserved, the Kenilorea government had to recruit prominent Western leaders to the highest national offices. The resignation of Francis Billy Hilly diminished Western representation at the cabinet level. The key positions of prime minister, leader of the opposition, and speaker were all held by Malaitans. If some of the prestigious new offices created by the advent of independence could be assigned to Westerners, it could diffuse the conflict. The cabinet proposed a Westerner for the position of governor-general, but failure to maintain cohesiveness among government supporters during the voting in Parliament frustrated this objective (*ND,* 28 April 1978). This failure was cited by Talasasa as a main factor in his motion in September 1978 for a vote of no-confidence in the prime minister (*ND,* 15 Sept. 1978). After that experience the cabinet was more successful in supporting the election of a Westerner, Maepeza Gina, as speaker of the House.

Within the public service, concessions were made to two prominent Westerners. In early 1978 Isaac Qoloni was appointed secretary to the prime minister and Milton Sibisopere became clerk of the Western provincial government. Together their appointments, as well as those of two cabinet ministers (Zoleveke and Ghemu), the speaker of Parliament, and other prominent posts in the public service, indicated a measure of success in co-opting Western leadership.

Resolution of the Conflict Within Parliamentary Institutions. Proponents of the Western movement may argue that its foundations as well as its first public expression preceded the rapid constitutional advances of the 1970s. But the movement reached its greatest prominence between the period 1974-1979. This is a most significant period since the constitution to be adopted would determine for a long time the relationship between regional units in the political system.

The lengthy process of constitutional discussion provided an opportunity for Western demands to be both raised and resolved in a national context. Throughout this period the language of decentralization changed, shifting from local governments to island councils, then "provincial assemblies." The Kausimae Committee took this decentralizing tendency further by resurrecting the term "local government" to describe government below the provincial level.

The report of the Constitutional Committee recommended that the power to establish councils, and to devolve and transfer powers and functions, be tranferred from the area of executive discretion to Parliament.[24] A two-thirds majority should be required to resume devolved powers. However, the committee did not include council autonomy among the entrenched clauses of the Constitution, requiring a three-quarters majority to change. The *Constitutional Conference, 1977 Principles,* the Solomon Islands' position paper, elaborated on these recommendations, particularly in relation to suspension and dissolution, and reaffirmed the previous committee's recommendation for a review committee.[25] The report of the constitutional conference provided for local governments to be renamed provincial assemblies and for the establishment of a review committee within a year of independence.[26]The final national Constitution, however, included only two paragraphs on provincial government (s. 114), leaving to Parliament the task of elaboration after it had considered the report of the Kausimae Committee:

> Solomon Islands shall be divided into provinces, the number and boundaries of which shall be prescribed by Parliament after considering the advice of the Constituency Boundaries Commission.

> Parliament shall make provision for the government of the provinces established under this section and consider the role of traditional chiefs therein.

The government moved quickly to set up the Kausimae Committee, but as independence drew closer, Western representatives pushed for its report before independence. The committee, meeting the Western Council in early June 1978, said it would be unable to report before Independence Day but would produce an interim report as soon as it could. This became the draft report issued in December 1978 and circulated to the provinces for comment. The committee's final report avoided the West's usage of "state" but recommended additions to the constitution. The government's White Paper, in response to the Kausimae Report, however, rejected further constitutional entrenchment of provincial government. It did so on grounds of principle as well as the practical difficulty of gaining endorsement by a two-thirds majority in the Parliament. Overall, the central government seems to have successfully contained Western demands within parliamentary institutions, the council, and ad hoc committees.

The Western issue was raised in several public demonstrations but in each case the government's response was that the issue could be, and was being, dealt with through "normal channels." Again, the existence of these channels suggested that the issue was a part of a general national problem, rather than one requiring a specific government commitment to Western aspirations. By the device of committee and parliamentary procedures, the government successfully channelled the contentious energies of the West to arenas where they could be handled rationally and procedurally.

Playing for Time. Dividing a country into two or more sovereign parts after independence tends to be more difficult than before. After independence, the new govermnent has at its disposal the full use of its coercive machinery, while in the pre-independence period this apparatus is under the control of the colonial authorities. It is usually easier for a subnational group to engage in a liberal use of threats and force than a departing colonial power that may want to avoid charges of racism attending an attempt to sort out an interethnic conflict. During the first wave of the Western movement, and until self-government on 2 January 1976, central authority was still divided between the Council of Ministers and the colonial governor. There is no evidence that the British government considered decolonization by partition. But to the extent that the British and Solomon Islands governments had different interests in the process of decolonization, Western leaders had an opportunity to play one off

against the other. For the Solomon Islands ministers, it was important to present the Western issue as an internal matter, solvable by an internal political process, rather than in terms of entrenched constitutional protections for a minority group imposed by a retiring colonial power.

Tuvalu's separation from the Gilbert and Ellice Islands colony by referendum in 1974 offered some Western leaders a model for peaceful partition (MacDonald 1982), It was achieved against British wishes, but with the agreement of the Tarawa government. Banaba's bid for autonomy and association was resisted by both Tarawa and London, though Kiribati's constitution contained specific protection of Banaban interests, unlike the general statutory protection for provincial autonomy in the Solomon Islands.[27] With independence, the Banaban opportunity was lost. Throughout the entire complex exercise involving the expressing and exchanging of views, the Solomon Islands government was successful in buying time.

Avoiding Crises. The West Wind poem published in June 1978, less than a month before independence, inadvertently created a crisis for the central government. Its appearance in a government newspaper necessitated an official response. The following week the paper published an apology directed specifically to the West and a police inquiry was appointed to establish authorship. This tactic brought the problem back to "normal channels" for resolution. The prime minister and his special secretary visited the West and apologized for the accidental misuse of the government press against Western dignity.

Ad Hoc Responses. Finally, the government made a series of ad hoc and specific responses to the West, particularly in relation to land early in 1978. These included the following:

- The return of 6,000 ha of government land in South Choiseul to customary ownership. This was a housekeeping operation since there were no immediate plans. to use the land. The transfer was presented by the minister as "a gesture of good faith and goodwill towards the Western people" (Hon. Waita Ben, quoted in *ND*, 25 April 1978).
- Consultation with the Western Council before bringing to Parliament the bill protecting existing Gilbertese freehold land rights in the Western Province. The legal protection was linked to a policy change allowing original customary owners to obtain freehold rights in government land that had previously only been available as leasehold, thus meeting Western complaints about the inequity of Gilbertese freehold rights.
- Abandoning in 1977 the attempt to acquire registered title to land on New Georgia in order to lease timber rights to Levers Pacific Timbers. Later in 1978, negotiations (chaired by the province) began to establish a special purpose corporation to allow customary owners to deal di-

rectly with Levers Pacific Timbers. This was a clear case of legislative action specifically directed at Western problems, after attempts to deal with them in the context of general land and forestry legislation had failed (see Larmour 1980).

Conclusion

A comprehensive history of the Western movement still has to be written; it is to be hoped that this will be undertaken by Western people themselves. In this study we have tried to outline ways in which the movement might be understood in relation to other separatist, autonomist, or breakaway movements. Certainly these comparisons were present in the minds of Western leaders, particularly around the time of the Bougainville secession attempt.

We have tried to outline some of the internal dynamics of the movement. This required indentifying the different interests and leadership in different parts of the West including the Shortlands, New Georgia, and Choiseul. We have pointed to the division between the Western Council and Western MPs, describing the way the council embodied and symbolized the movement. We have also noted the international dimension: the model of and links with Bougainville, the University of Papua New Guinea campus, and the use of legal consultants in Australia. Throughout much of the paper, we also discussed the fundamental and facilitating bases of the movement.

The impulse to secession may not be related to a rational assessment that a Western government would do things better and more responsively than a government based in Honiara. Rationally, Westernism called for a government in Gizo substantially like the one in Honiara. The differences would be in the composition of the government, its location, and its territorial jurisdiction. Behind the movement for autonomy was a deeper reaction against centralized government and in particular, the legal, bureaucratic, and career structures that were imposed by colonial rule. These anticentralist sentiments were, contrary to expectation, reinvigorated by independence. While an autonomous Western government, by its smaller scale and increased dependence on fewer big companies, might seem to provide greater opportunities for neocolonialism, it offered a chance to the West to create a government of its own making. However, as a separate country the Western government would probably follow policies similar to the government in Honiara.

There may be some similarities between Westernism and other resistance and cultist movements in relation to colonial rule. In this regard, the

Christian Fellowship Church's support for the movement is certainly relevant. The West mobilized its forces against the transition to an "independent" economy mapped out by the colonial admninistration and the British government in the early 1970s. From this perspective, it was a resistance movement with potential secessionist aspirations rather than an oppositionist group with a blueprint for an alternative government. Looked at in this way, NADEPA's support for Westernism becomes clearer. Objectively, a Malaita-led party representing the interests of wage-earners would have little in common with the Western breakaway movement, which derived much of its backing from fear of "Malaita domination" and the assertion of rights of landowners. But NADEPA certainly had good tactical reasons to support Westernism in order to embarrass the government. Both NADEPA and the West seemed to share the feeling that the transition to independence was being made just a bit too smoothly and quickly, and was thereby denying citizens the opportunity to discuss major issues such as the distribution of power in the state, the role of foreign investment, and so on. These were passed over too perfunctorily in the interests of stability, investor confidence, and the reduction of British aid. Neither group (NADEPA or the Western movement) provided very convincing alternatives, however. Impatience with the government could be explained from the fact that both NADEPA and the West found themselves in the paradoxical position of resisting the imposition of independence until matters were resolved. In particular, we are referring to NADEPA's demonstration against internal self-government in 1976, and the West's final demand that colonial rule continue in the West after independence. Apart from these points of similarity, NADEPA's aims must be seen as intrinsically different from those of the West. NADEPA was not in favor of dismembering the state if its objectives were not met. In its submission the Western movement gave veiled threats as to the course it would take if frustrated.

The sweeping nature of Western demands made it easier for the central government to reject them. The central government discovered that it could absorb the urgency of the demands by submitting them to committee work. Although the fundamental conditions for a breakaway movement remain, by achieving independence without dismantling the unitary nature of the state, the suggestion is conveyed that the central government has temporarily won. The issues remain albeit unresolved. Whether a national government could ever have satisfied the impulses behind Westernism remains unknown.

A rational argument for secession is that the unit asserting its rights to self-determination gets less out of unity than it would as an independent

entity, Government revenue and expenditure are not the only measure of the advantages and disadvantages of participation in a large unit. In terms of foreign aid, a separate government of a small country could probably expect more than it would get proportionately as part of a larger one. In small, open economies, the size of internal markets is likely to be a determining factor in attracting foreign investment in extractive industries, such as timber. In effect, small size may succeed in sponsoring externally stimulated economic development if resources are present and government policies are attractive. But, by the same token, the autonomy sought by the secessionists could be severely compromised by overdependence on multinational corporations.

A particular argument in favor of secession in small countries concerns "viability." To the extent that it is appropriate in an increasingly integrated world economy, the argument can equally be applied to the national as well as the secessionist unit. The West, with a population of 20,000 people, would have been larger than eight members of the South Pacific Commission and, in terms of its geographical dispersion and resources, a more viable unit than say Kiribati, or the Cooks (with 18,000 people). In any event, pragmatic concerns are only part of the reasons for seeking autonomy. Political sovereignty, ethnic solidarity, a vote in the United Nations, and so on, are probably the most valuable resources available to a small group of people in their dealings with the rest of the world (particularly for islands because of the recognition of their right over the seabed). The U.N. countries' acceptance of the principle that size should not be related to voting rights made secession a more practical option. The move, however, from the status of province or district to that of sovereign state is rarely without violence. Tuvalu's case was an exception.

Often the case for secession is put partly in terms of a calculus of economic cost/gain. This was part of the Western case. Arguments about "contribution" may overlook the overhead costs of running an independent state, particularly when, until independence, these costs have been met directly by the colonial government and have not appeared in the country's budget. Overheads for a new government would include not only foreign offices and salaries for the head of state, but also the budgets of central departments like a public service office, and functional ministries. The overhead costs of an independent government also tend to be overlooked because they do not fit easily into the framework of the regional development plans, which focus on rural development. Put differently, the cost of running a provincial government as part of a country is not the same as that of running a separate country.

Many of the conditions that led to the Western movement no longer prevail. Independence raised issues that happened only once. The constitutional planning process created possibilities of political change that would henceforth be considerably more limited. The presence of the British offered Western provincial leaders the possibility of an alternative source of redress for their grievances. After independence it would have been difficult for them to look abroad to countries that already recognized the Solomon Islands government as the legitimate representative of all the people. In addition, independence created new offices, money, and jobs, out of which some Western demands could be satisfied without the pain of redistributing resources from the rest of the country.

But many of the fundamental and secondary factors remain. The fundamental factors (race, history, etc.) are, by definition, unchangeable, and so continuous central government efforts will be required to ensure that they are not activated again. The question here is whether the central government will be able to accommodate the Western issue within a national framework of provincial government under the rubric of "unity in diversity." It may be that a national system designed to cope with the West's particular claims will impose too great a strain on other provinces and the central government may have to continue to deal with the West as a special case, a de facto "state" within a country of provinces.

Finally, a few general points need some attention. First, if the Western movement ever achieves full nationhood, it would be unlikely to occur without violence. The history of nearly all other bona fide breakaway movements attests to this general proposition. Second, as a matter of fairness to general theory, one is left to speculate about the prospect of a subunit such as Choiseul demanding full autonomy on the same grounds advanced by the West vis-à-vis Honiara. Would Gizo cooperate? Third, the issue of decentralization in relation to national unity remains to be addressed. Can decentralization be undertaken or conceded without fear that it may be the prelude to demands for full, separate independence? Perhaps the outcome of the ongoing dispute between the West and Honiara will provide a partial answer.

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NOTES

1. See *The Solomon Islands: An Introductory Economic Report* (World Bank Report 2553-501, 18 December 1979); see also *Draft Solomon Islands National Development Plan 1980-1984*, Honiara, 1980.

2. For descriptive accounts of the Methodist mission see Luxton (1955) and Garrett (1982:300-1). For an analysis of the mission's style and the origins of the Christian Fellowship Church, see Tippett (1976: chaps. 5 and 15).

3. *Solomon Islands 1981 Statistical Yearbook,* Statistics Office, Ministry of Finance, Honiara, December 1981, Table 2.7, p. 24.

4. See *Report of Special Committee* of *Provincial Government* (Kausimae Report), Honiara, 1979, par. 2.15-2.17.

5. In the literature on the subject, these factors are described as "fundamental" or "primordial" (see Geertz 1963:105-57).

6. For a discussion of some of these categories in the study of breakaway movements, see Premdas (1977a and 1977b).

7. Submission of Western Council, August 1975, Special Committee on Provincial Government Background Paper No. 28, mimeo. (Hereafter referred to as Western Submission), p. 1.

8. See *Kolombangara Land Use Planning: Working Party Report,* Honiara, 1977.

9. See Rence in Larmour (1979) and Tippett (1976) for the Christian

10. Ministry of Finance Submission to Special Committee on Provincial Government, 1978, Special Committee on Provincial Government Background Paper No. 27, Honiara, June, p. 5 (hereafter referred to as The Finance Submission).

11. *Draft National Development Plan* 1980-1984, vol. 1, par. 398, Honiara, 1980.

12. Special Committee on Provincial Government Background Paper No. 31, Statistics Office, Ministry of Finance, Honiara, 1979.

13. *Solomon Islands Sixth Development Plan* 1971 to 1973, Honiara, 1971.

14. See *Report of Solomon Islands Constitutional Conference,* HMSO, London, 1978, Annex C, for details of the financial settlement.

15. See "Local Government in Solomon Islands," Ministry of Home Affairs, Honiara, 1975.

16. See Kausimae Report, 1979, par. 5.14.

17. See Kausimae Report, 1979, par. 5.12.

18. Ministry of Home Affairs, Mimeo, 1978.

19. Agreement between the National Government and the Province of Bougainville, dated 7 August 1976, Port Moresby, 1976, p. 3.

20. See *Iumi Nao,* the film celebrating Solomon Islands independence, produced by Film Australia, 1979.

21. The author was a government agriculture officer who was charged with sedition, see *News Drum,* 8 September 1978.

22. White Paper on Provincial Government, National Parliament Paper No. 4479.

23. *Making Decentralization Work,* 1977, McKinsey and Co., Port Moresby.

24. *Report of the Constitutional Committee,* March 1976, Honiara.

25. *Constitutional Conference, 1977 Principles* March 1977, Honiara.

26. Report of Solomon Islands Constitutional Conference, *op. cit.*

27. Kiribati *Constitution,* chap. 12.

REFERENCES

Barth, F. 1969. *Ethnic Groups and Boundaries.* London.

Bobai, T. 1979. "Gilbertese Resettlement." In *Land in Solomon Islands.* Ed. P. Larmour. Institute of Pacific Studies, USP, Suva.

Campbell, M. 1974. "The Development of a New Local Government Image." Mimeo, UPNG, 22 Nov.

Connor, W. 1973. "The Politics of Ethnonationalism." *Journal of International Affairs,* pp. 1-21.

Duchacek, I. 1974. *Nations and Men.* Boston.

Emerson, R. 1964. *From Empire to Nation.* Cambridge.

Enloe, C. 1975. *Ethnicity and Political Development.* Boston.

Garrett, J. 1982. *To Live among the Stars: Christian Origins in Oceania.* World Council of Churches in Association with University of the South Pacific, Suva.

Geertz, C. 1963. "The Integrative Revolution: Primordial Sentiments and Civic Politics in the New States." In *New States and Old Societies.* Ed. C. Geertz. New York.

Griffin, J. 1973. "Papua New Guinea and the British Solomon Islands Protectorate: Fusion or Transfusion." *Australian Outlook* 27(3):319.

Hannett, L. 1975. "The Case for Bougainville Secession." *Meanjin Quarterly.* Spring, pp. 286-93.

Hastings, P. 1976. "Bougainville and the Solomons: An Inevitable Union?" *New Guinea* (Jan.) 33-41.

Healey, A.M. 1966. "Administration in the British Solomon Islands Protectorate." *Journal of Administration Overseas.* July.

Heath, I. 1979. "Land Policy in Solomon Islands." Unpublished Ph.D. thesis, Department of History, La Trobe University, Melbourne.

Keesing, R. 1978. "Politico-Religious Movements and Anticolonialism on Malaita: Maasina Rule in Historical Perspective." *Oceania.* June.

Larmour, P. 1979. *Land in Solomon Islands.* Institute of Pacific Studies, USP, Suva.

_____ 1980. "The North New Georgia Timber Corporation." In *Land, People, and Government: Public Lands Policy in the South Pacific.* Institute of Pacific Studies, USP, Suva.

Local Government Plan of Operations. 1973. Government Printery, Honiara. 55.

Luxton, C.T.J. 1955. *Isles of Sobmon: A Tale of Missionary Adventure.* Methodist Foreign Missionary Society of New Zealand, Auckland.

MacDonald, B. 1982. *Cinderellas of the Empire.* ANU Press, Canberra.

Premdas, R. 1977a. "Secession and Political Change: The Case of Papua Besena." *Oceania* (June): 265-83.

_____ 1977b. "Secessionist Politics in Papua New Guinea." *Pacific Affairs.* Spring.

_____ 1978. "Ethnonationalism and Copper." *Canadian Review of Studies in Nationalism.* Spring.

Scarr, D. 1967. *Fragments of Empire: A History of the Western Pacific High Commission, 1877-1914.* ANU Press, Canberra.

Tippett, A.R. 1976. *Solomon Islands Christianity: A Study in Growth and Obstruction.* Lutterworth.

EDITOR'S FORUM

JOHN FRUM: AN INDIGENOUS STRATEGY OF REACTION TO MISSION RULE AND THE COLONIAL ORDER[1]

by Robert J. Gregory and Janet E. Gregory

Introduction

In 1940, British District Agent James M. Nicol on Tanna, Vanuatu,[2] in the Southwest Pacific, discovered that a spirit-man, John Frum, was competing with him for authority in governing the Tannese people. Prior and subsequent events have been the subject of many articles and books, descriptions and interpretations. But the existing social order on Tanna and its impact on the Western institutions that attempted to dominate the Tannese have not been sufficiently considered. Evidence demonstrates that the dissatisfaction and eventual disillusionment of the Tannese led first to strategic planning, then to the emergence of the John Frum cult, and subsequently to the forced redirection of the strength of Western influences on Tanna from mission rule to governmental control. An indigenous reaction to Western influences, the John Frum movement was and is a culmination of efforts by the Tannese to reclaim, and revitalize, their culture.

The topics in this paper include examinations of 1) contact with the West, 2) the existing social order, 3) dissatisfaction and disillusionment, 4) strategic planning, 5) the John Frum movement, and 6) recent events.

Contact with the West

Contact with Western culture developed in Tanna not through programs, administrative procedures, or complex organizations, but through individuals and technology. Contact began in 1774 with the arrival of Captain James Cook, and whalers and sandalwood traders soon followed. From 1840 to 1865 the Tannese participated as crew members on boats involved in the sandalwood trade, receiving in exchange tobacco, tools, fishhooks, knives, and various European manufactured goods. Early missionary contacts were rejected, including a major attempt in 1858 by John G. Paton, Joseph Copeland, and John Mathieson.

By 1865 the island had been stripped of much of its sandalwood, but the Queensland labor trade was flourishing. By 1869, probably twelve hundred Tannese adults were working on Queensland plantations. Recruiters enlisted some men by simply offering their headmen a musket, supposedly to balance their economic and political loss. Other young men went to find adventure or to escape, for one reason or another. In any case, because of the muskets, traditional tribal rivalry on Tanna escalated into open warfare.

Cotton plantations, which operated in the 1860s and early 1870s, were shut down by 1875 because of the hostility of the Tannese and the decline in cotton prices. Missionary efforts continued, however, with Rev. William Watt baptizing converts in 1881 and thereafter. He was joined by Rev. Macmillan, and the two men and their supporters brought about 1) a permanent colony of Europeans on Tanna, 2) the beginning of the end of active warfare, and 3) deepseated changes in many aspects of Tannese society.

The influence of Dr. J. Campbell Nicholson, a medical missionary who arrived in 1903, was felt on Tanna for many years. Descriptions portray him as a hyperactive workaholic: "His horse on Tanna had to go full speed to succor those in emergency. His method of dealing with disease-infected native houses was unhesitating destruction. Dilatory natives braced themselves to energy when he was around.[3] The influence this man had on Tanna can be felt from the letter by the Resident Commissioner in Vila to the High Commissioner for the New Hebrides:

> Dr. Nicholson is a well meaning man . . . but he is of a very hasty and violent temper, and when that temper has command of him, I do not think he is always conscious of what he says . . . he has for so long exercised almost undisputed sway over the greater part of Tanna and the Tannese that he feels, and resents, the loss of influence and authority resulting from the establishment of a representative of Government in the island.[4]

Nicholson's two Tannese helpers, Lomai and Yawis, were able to increase their status through their association with him--and to control the Tannese by "Tanna Law," which had been implemented by the Presbyterians on the West side. Tanna Law included restrictions against many traditional practices such as kava drinking, dances, and exchanges of women between villages for marriages. It was enforced for converts and for the so-called heathen. At the same time, Kowkarey, a war chief on the East side, teamed up with Rev. Macmillan to increase his power. These

Tannese men skillfully used their positions to advance themselves, as well as the interests of the mission. Non-Christian informants, even in 1976 and 1977, were able to recite many almost legendary grievances about these mission helpers.They were feared and renowned for their punishments of those who would not join the missions. Severe punishments for Christians who did not do as the leadership wanted, as well as the harsh treatment of non-Christians, gave the Tanna Law era a bad name, although little documentation is now available as to the excesses. According to informants, the missionaries themselves were not always aware of these happenings. Their interest was in extending the influence of the Christian belief, not with the methods used. The social hierarchy of Tanna in the early 1900s was built by the men who convinced Nicholson and Macmillan of their fitness to control and convert the Tannese.

Macmillan and Nicholson were in charge when the Condominium government appointed its first agent to Tanna. Mr. Wilkes arrived in 1912 and took up his post with initially favorable reviews from all. The non-Christian Tannese were quick to ask for help in preserving their customs against the Presbyterian Tanna Law regime. Wilkes supported them only to a limited extent, but even that was enough to provoke the anger of the missionaries. He resigned under pressure in 1914 to volunteer for service in World War I. As the first representative of the governing powers on Tanna, Wilkes left his mark as an opponent of the missions and as a man who helped preserve customs, including the use of kava.

In March 1916, James M. Nicol was appointed as the Condominium agent. Nicol was a marine engineer, a man with an orderly, administrative orientation. His caution kept him from clashing with the opposition as Wilkes had. He accepted the system set up by the missionaries and worked within it. Nicol played an important role in Tannese affairs because he remained in nominal command for the next twenty-eight years until his accidental death in 1944. Yet his impact was not as forceful as that of the missionaries, Nicholson, Macmillan, and Watt. Nicol was determined to make things work without questioning the status quo. In effect, he introduced an administrative government to Tanna.

At Lenakel Hospital, Dr. William Armstrong reigned from 1925 through 1929, when Dr. Daniel McLeod arrived; he would remain until 1937. Dr. and Mrs. MacLeod used the Oxford Group method, which sought to produce a type of spiritual rebirth. Their tenure at Lenakel Hospital included small intensive groups, peer pressure, confessions, conversions, and spiritual changes. Armstrong later took over again, working until the 1950s. Armstrong and his wife were, like Nicholson, hard working and dedicated. Armstrong described his time as

> occupied with the patients who need skilled attention, occasional surgery, dispensing and general supervision. Part of the day is always taken up with some general handyman work or other . . . The mid-week service and a weekly elders prayer meeting occupy most of two mornings.

Armstrong found that his help was unable to make bread, so he took the responsibility. When he got a horse, he spent at least three days a month traveling.

In summary to this turning point, individuals appointed by the missions and the colonial government, who had power by virtue of their affiliation with the Western world, entered Tanna, told the people they must follow certain ways of living and belief, set up an institutional structure different from the Tannese way of doing things, managed administrative affairs (with a district agent carrying out the wishes of the mission), and kept busy subjugating the heathen. The heathen as might be expected, resisted. It was not until John Frum emerged though, that the resistance spread to the converted, like an epidemic.

The Existing Social Order

By 420 B.C. Tanna was settled by people practicing shifting agriculture, raising pigs, dogs and fowls, and gathering shellfish from the sea.[6] For about 2400 years then, a social system has evolved with occasional intrusions from elsewhere in Melanesia, Polynesia, and more recently, the West.

Several themes provide bases upon which Tannese culture revolves, including the cult of the ancestors, stone magic, and use of kava. The Tannese are strongly oriented to the past, and the present/future is only an approximation and transformation of the past. The ancestors were and are prominent, as the origin of all that now exists.[6] Names of ancestors are given to children, and the children grow up to assume the role of the ancestor within the social system.

Stone magic is fundamental to understanding the beliefs and practices of the traditional society. In the beginning, it is said, the different foods were men, but after darkness was created the foods became wood and then solidified into stone. Now these stones are "like a brother to men" and certain stones can be "worked" to insure that particular foods grow. Men can work other stones to influence the weather, thereby creating rain, sun, and wind. A man named for an ancestor who performs such

magic is obligated to play that ancestor's role in order to assure a bountiful harvest. The spirits residing in stones are powerful and potentially evil as when used in sorcery.

Kava provides a slightly narcotic drink that gives relief to males from social tensions and constant political competition. A muscle relaxant, kava is nonaddicting. It is domesticated and most adult males have at least two kava gardens. Kava is a central mechanism of social exchange and symbols. Perhaps more important, however, kava enables men to communicate with the spirit world. Every evening nearly every adult male drinks kava and then meditates on the past, the gods and spirit world, the right paths to follow, and the correct and appropriate behavioral patterns.

Further insight into Tannese culture can be gained by looking at its institutions: political, social, economic, legal, educational, and medical. Political power revolves around older men, the past, and conservative individualism. Self-reliance and independence give the Tannese strength, and strong egos abound among the men who are the leaders of relatively small villages. Village organization revolves around these men and the *nakamal*, the village meeting place, a clearing in the jungle used by the men for meetings, dances, and kava drinking. Intervillage meetings, traditional exchanges of gifts, and celebrations are frequent and are recorded in memories or songs assigning future responsibilities and obligations to repay past trades. There is however, no operational, islandwide, traditional organization, only individual events join people together.

One important social phenomenon has been organized: competition. Competition can be intense among individuals within a village, but intervillage competition is almost always present, and alliances, intrigues, and disputes are constant. The *nekowiar*, for example, is an intervillage competition in which one man challenges another from a different village. Each then gathers as many allies as possible, attempting to demonstrate greater power. The show of strength consists of a series of dances and preparations, then a killing of pigs, and ritual ceremonies and dances. The side that kills the most pigs is the acknowledged victor, though an approximate balance is desired and reached. The man and village winning the competition are highly respected.

Tanna's economy once-depended on subsistence agriculture, with rich and well-worked gardens. Now, the marketing of copra, beef, vegetables, and other items is well established. Extra work is generally rewarded with crops and/or pigs suitable for exchanges with others to assure increasingly important roles and obligations. Land is inherited by individuals patrilineally, and heirs protect their land jealously.

The production of copra is dependent on world and local prices. Copra provides cash to purchase material goods from the trading stores or cooperatives. Clothing, bush knives, western foods, soft drinks, beer, payment for children's schooling, and travel to Port Vila consume much of the money.

Village Big Men play an important role as mediators in marriage exchanges. The Tannese kinship system, Dravidian Iroquois, classes adults in four categories: brother, sister, brother-in-law (husband), and sister-in-law (wife). Sons in the patrilineage marry women from nearby villages, with the preferred marriage being between cross-cousins.[7] Women are exchanged between certain nearby villages for marriage to appropriate men. The Big Men make the arrangements and enforce the exchanges.

Intervillage meetings settle disputes and serve as courts of law. Disputes are argued before neutral witnesses, and the decisions made are sanctified by a kava ceremony. Arguments most frequently take place over females, female exchanges, land, and the supernatural realms (sorcery). Penalties are fines of pigs and kava. The district agents now try major cases but usually support the decisions made in the village meetings.

Traditional education, involving the learning of lifelong roles, plays an integral part in the indigenous culture. Among the Tannese, certain names are passed down through generations, and a child is named after some late relative such as a grandparent. This cycle of names is also a cycle of roles: as a child grows older, his role approximates that held by the former carrier of the name. Taking the place of one's same-name relative involves gradual assumption of land inheritance, knowledge and practice of magic, social and political status, and even character. Given this situation, men and women take time to see that the cycle goes well and make sure the child properly learns the role he or she is to fulfill. If living, the person teaches his or her *mind* to their young namesake. If deceased, relatives are quick to describe the character, habits, and ways of the departed person to the child. Such formal orientations are still maintained and followed by people who retain the traditional customs, though they are less important in more Westernized villages. By participation, modeling, and imitation, the children of Tanna learn to take their part in the culture. Specialized roles involving songmaking, medicine, magic, and so on, are further developed by apprenticeship with the persons in the lineage who know the skills.

The missions had different concepts of education. Missionaries spreading their beliefs operated not only through their churches but through stations serving villages in the bush. The stations were called schools, and the men of the village came to hear a teacher tell the words of the Presby-

terians. Rev. Macmillan, probably more than anyone, entwined education with religion.[8] The struggle to control the young and to break up the families was real. "I look forward to the time when the power of the Big Men shall be broken" proclaimed Dr. McLeod in 1947.[9] "Whenever I am able, I encourage work for the children in the hope of breaking the power of the Big Men."

Compared with other districts in Vanuatu, Tanna is far behind in formal education. As a result of the John Frum movement many people left the church schools, creating a generation of young people who cannot read or write.

A wide range of traditional medical techniques are available to islanders, but some methods are known only to specialists. One of the most important techniques is the use of plants and leaves. A fresh-cut bamboo stalk makes deep, surgical cuts and may be used when broken bones are set. A piece of wild cane is used to make a custom-cut to the forehead or other body parts to reduce fever, headache, or pain. Another practice is psychotherapy; when feeling poorly, a person may be helped by talking with a Big Man or close friend. Usual payments for such "custom-medicine" treatments may be a chicken or a pig and a kava plant.

When the balance of daily life is upset by some inappropriate action, another type of custom medicine is employed. The masticating and spitting of a certain leaf is a remedy to restore the proper balance. The leaf is medicinal, and the sound of spitting is a way to let the spirits know that something is being done to atone for possible waywardness.

There is constant fear of disease-makers, the people who practice *nahakw*, or sorcery. A man upset with someone might obtain something belonging to that person--a strand of hair, a banana peel, a fingernail, a coconut husk, or any other such possession. The man takes the token to the disease-maker whom he pays to make his enemy sick. Word is then sent to the victim that nahakw is being worked against him. That person although in good health, will soon become ill, perhaps even dying unless able to find a way to counteract the sorcery.

On Tanna, anxiety about cyclones and weather (and consequently food) is great. This anxiety is dealt with by ascribing power to certain individuals who thus become taboo. Unless exact rituals are observed (the past providing the guide) it is believed that retribution from the forces of nature may follow--through the taboo individuals. The spirits and supernatural world are placated only if the instructions of the taboo men are precisely carried out. This gives immense power to the men who make magic and who have contact with the supernatural.

The greatest problem evident in the health care provided by the two governments is the pervasive duplication, which creates confusion and competition. Rather than coordinate medical programs, the French and British and missions establish clinics or dispensaries or hospitals on the basis of politics. This results in some regions having a great deal of medical care and others having virtually none, depending on strategies. Critics of this political gamesmanship have advocated a policy that controls and directs the various services or at least organizes the seven medical services in operation.[10] Other aspects of the health care system are similarly beset with political problems.

Dissatisfaction and Disillusionment

The John Frum cult and social movement was not a sudden irrational outbreak on Tanna in 1941. The appearance of John Frum, the spirit-man, was an expression of certain needs of the people that resulted from slow permutations and ultimately created the social movement.

The Presbyterian missionaries taught a fundamentalist brand of Christianity full of fire and brimstone. In 1915 near the volcano, a fall of ice, probably hail, was recorded.[11] The people nearby became fearful and began shouting that "the last day had come."[12] Their reaction was not surprising as unusual natural events were interpreted by Christian converts in the new, strict and rigid framework of the missionaries. The missionaries' God was a frightening God. In 1915 a man claimed a revelatory dream in which a spirit flew him above Tanna, like a bird. He said this spirit, the true god, gave him two signs: a big white stone and a bottle of the water of life. He drank the water, after which the bottle shrunk. He showed the people the stone, a marble, and the bottle, a hospital vial. The man told people that he had learned where a large amount of money was buried. To his loyal followers he promised rewards that seemed much like those the missionaries offered. Many people came to listen as he described these happenings and gave him gifts. He developed an origin story, with different names for Adam and Eve, but Nicholson noted that the man did not talk of Jesus or of a new Jesus. Soon his followers were building a house of worship for him near a tree with marks in it that, he claimed, were made by God and understood only by himself. But the money did not appear, and interest dissipated even before the temple was completed. This 1915 incident and its aftermath lacked only sufficient active followers to inspire a religious cult.[13]

In 1922, resistance to the government was reported. A man named Iahua prevented two men from going to report two cases to the Con-

dominium government agent. At his court hearing at Lonegi, 15 September 1922, Iahua asserted "that he was the door through which all cases had to come and also he had the power to cover up all cases." He was found guilty and sentenced to six months hard labor on the roads.[14] These and other signs of resistance to the missions and the government show a focusing of energies toward establishments viewed as oppressive.

Also in 1922, a drill show and demonstration were staged by Tannese and Aniwan former policemen and headed by a former sergeant giving orders. A later event planned for Christmas day also was to include a drill team, but it did not perform well and no performance was given. The Tannese quickly learned such interesting and useful behaviors, and were fully capable of putting them to their own use. Some seeds of resistance were present, though not readily apparent. After World War II, military-style drill performances surfaced as a method of demonstrating resistance to colonial power, particularly at Sulphur Bay.

For the Tannese, songs are like books. It is through songs that history is preserved, the future foretold, and ideas communicated. Songs, again like books, are one of the few things that cross over the linguistic, geographical, and political divisions that exist on Tanna. During the 1920s and 1930s many songs of prophecy circulated. Yeru, perhaps the most popular songwriter, or song creator, foretold the decline of the Presbyterians and even prophesied that one day a man would come to help the people maintain their customs and return to the good ways of the past. This, though of little note to the white population, was a matter of significance to Tannese and foreshadowed the coming of John Frum. More than specific events, though, the political climate of the 1920s and 1930s helped to generate interest in alternatives to the Presbyterian domination.

The impact of the missionaries on their followers was great. Given power and sanctioning by the missionaries, the Christian converts used tactics to gain new adherents that led to Tanna Law and the prohibition of many past customs. Social, economic, political, and other changes were occurring; the result was a non-Christian group competing with the more powerful Presbyterian-backed people. The advancing stations of the mission and their strictures against customs--the banning of traditional dances, the disparaging of the grass-skirt and the *nambas*, or penis sheath, and the prohibition on kava--became well known to the people who simply tried to live their lives and to follow the customs of the past. There was fear that their traditions would be lost, that their children would never know the old customs. During this time, traditional-minded Tannese developed a strategy, indeed, a rich philosophy for dealing with encroachments by outsiders.

The climate of Tanna Law was set by Yawis of Sitni Village on the west side of Tanna, and Kowkarey of White Sands on the east side. These men used their close relationships with the missions skillfully, relying on the missionaries, their Christian god, and the promises of eternal life for power in the eyes of the Tannese. They also relied on other Tannese converts for muscle in executing the tasks of the church--Yawis and Kowkarey were in charge of the police and thus the law. When men were arrested, they were put to work breaking stone to build roads and fences. According to informants, the arresting process sometimes involved beatings, while the work on the roads was sufficiently rough for some men to die from exhaustion.

Informants also relate that the traditionalists met to discuss strategies for dealing with the harassment and arrests. An agreement was reached to defend their customs with muskets, bows and arrows, axes, clubs, and other weapons. One time, a large group put on a big dance at one of the villages. That night a man from a neighboring village was sent to tell Yawis that if he came to the dance or tried to arrest people from the villages he would surely die. After the dance, the villagers, heavily armed, waited.

Many of the police and Tanna Law people wanted to break up the dance, harass the supporters of native customs, and arrest their leaders. Some sixty men waited for Yawis to begin the move, which needed the approval of both Yawis and Kowkarey. Yawis, as leader of the west side, had to provide the leadership. Instead, Yawis claimed he had had a dream that things would go badly if his men attacked, and he ordered them all to stay back. The men were almost angry enough to fight Yawis, but he persisted in his refusal to attack. It was a decisive victory for supporters of custom.

Later, many men came and arrested Yasu, possibly for having two wives although this is not clear. He was confined to a different village and ordered to work on the roads. But Yasu left and walked home. Fearful of being beaten and harassed., he ordered preparations for defense. On 23 September, 1923 British District Agent Nicol[15] sent three policemen and twenty-nine appointed police helpers to arrest Yasu. After surrounding Yasu's house the three policemen approached. Yasu fired and killed Ielkuaien, a man appointed by Nicol. He tried to fire again, but his double-barrelled weapon jammed. Yasu then tried to escape but was caught and taken to White Sands on the other side of the island.

At White Sands, where the police wanted to kill Yasu, an assessor, Tom Koat, made a fervent plea for the prisoner's life. He was concerned that if Yasu died, custom would end, either literally or symbolically. The

British district agent had. him sent to Vila for trial.[16] The British resident commissioner in Vila informed the French of this fact in January 1924, and it was proposed to try him before the Mixed Court. Eventually, however, Yasu was returned, first to Aniwa and then to Tanna, serving in all less than two years. Again this was a victory of sorts for the non-Christian Tannese.

Another tense situation arose when the Seventh Day Adventists (SDA) started a mission on territory formerly sacred to the Presbyterians. Nicol[17] wrote to the resident commissioner that some two hundred demonstrators from the Presbyterian mission had walked to Port Resolution to ask Mr. Perry of the SDA to leave. Threats were made on both sides, and Nicol felt the irony that in addition to the normal differences between Christians and heathens there should now be problems between two branches of Christianity. A series of letters was exchanged. Nicol concluded that at the root of the conflict was the reaction of older Presbyterians to the young men joining the SDA.

Other new influences were being felt. A French district agent came to Tanna, and soon afterward the Roman Catholics began a mission. The worldwide depression led to a decline in copra prices, and the European population on Tanna sometimes owed money to the Tannese, even becoming dependent on them for food. Various cases of illegal liquor importation were tried in the Native Court.

Just prior to the advent of John Frum, the churches and the hospital were active. A census in early 1939 found the East Tanna people included 2,281 Presbyterians, 278 Seventh Day Adventists, and 222 non-Christians. The West Tanna figures were 1,100 Presbyterians, 378 Seventh Day Adventists, and 72 Roman Catholics, with 1,437 non-Christians.[18] Armstrong attributed the differences to the long-term presence of Macmillan and Watt on the east side, in contrast to the relatively short tenures of the seven different men who worked on the west side. Further, though the hospital provided a point of contact, it consumed so much time and energy that little evangelism could be conducted. Interestingly, Armstrong pointed out that there was no real educational policy, nor even a means for education. He described the hospital as inefficient and the financial and staff support of the mission as low, claiming that many of the natives were far from Christianized.

Strategic Planning

Determining whether strategy was involved in the John Frum movement can be done by 1) reviewing evidence regarding the development of

the John Frum cult directly, and 2) examining examples of strategic planning encountered during fieldwork. This latter, indirect approach may offer some insight into the cultural style of political and social planning in Tanna. Two examples encountered during fieldwork in 1976-1977 are cited below, then evidence about the development of the cult is examined.

Example 1. The British resident commissioner (BRC) sent a message to the district agent (BDA) to the effect that he would visit Tanna for two or three days. During that time, he wanted opportunities to visit some of the people in various areas of the island to inform them that independence was likely to occur in the near future and to hear their concerns. The BDA then sent a message to the people in the area we were living that such a visit was planned.

The men of several villages called a meeting. There was a concensus that a meeting with the British officials would be good, but it was acknowledged that the district agent and the resident commissioner represented two different levels of authority. They understood that the BRC was the leader and the BDA was a subordinate assigned to Tanna. They discussed ways in which to communicate with the BRC, but not the BDA, about a specific concern.

When the meeting took place a few days later, we sat with the men as they listened respectfully to the speech by the BRC. Afterwards, he indicated his desire for informal discussion and the chance to hear their views. The spokesman for the villages addressed the BRC and the BDA. He proceeded to phrase a. question in Bislama. The BRC did not speak Bislama, so the BDA had to interpret, but he was unable to translate the question into clear idiomatic English. A literal translation was made and both men were left trying to understand its meaning. The question was difficult, mystical, and symbolic. Essentially, the spokesman stated that in the past they had given kava and pigs to the British visitors, that they shared certain experiences, that it was good that the British were leaving if that was what they wanted, but that the British should think carefully about what they had contributed to Tanna. "Man-Tanna" had given pigs and kava to a resident commissioner at a previous time. Did the present resident commissioner even know about the previous gifts? Further, did he care enough to find out specifics or to consider the matter generally?

The BDA was indifferent. He did not attempt to figure out the puzzle posed so carefully. He presumably knew little about the import of balanced exchanges between parties, requiring that gifts received should later be reciprocated. It mattered little to him that "accounts" were not balanced.

On the other hand, the BRC was concerned with the philosophical question or riddle. He attempted to figure out the general intent, and tried to respond then. Later, the BRC questioned us, a trader on Tanna, and others seeking further meaning and significance in the question. At the time, the Tannese men expressed their satisfaction that the commissioner had at least a glimmer of the intent of the question and demonstrated enough interest to pursue an answer. The eventual response by the BRC involved his return in 1978 to offer a gift of five clay pipes and a signed photo of Prince Philip, which was an appropriate return.

Strategic planning had resulted in a communication through a subordinate who ended up with little idea as to what information he had conveyed. The personality of the visitors had been appropriately judged. The "accounts" were balanced, and symbolically, at least, the British were cleared of "debts."

Example 2. The French district agent (FDA) arranged a tour of the various areas of the island for his superior, the French high commissioner (FHC). The Commissioner was interested in traditional customs and dances, so a traditional village (Village C) was contacted. The FDA agreed to provide beef and rice as an exchange if the people would demonstrate traditional dances.

The big men in Village C initially agreed to perform dances at a village close to the sea (Village A). This was a convenient location because of the frequent inclement weather, which caused difficulties on the narrow and slippery roads. Later, at a small meeting, relations with a neighboring village (Village B) loyal to the French, were discussed. The big men of Village C had some minor disputes with the French village (B) and therefore wanted to demonstrate their superiority over them. Two different routes were available to get to Village C. Road two had recently been improved by the French because of their school in Village B. The discussion that followed reviewed the merits of various sites, and the big men chose Village C as the proper site for a dance. It was the home of the big men of the traditionalist movement, and its choice as the site would clearly demonstrate the importance of Village C. Whether the visitors came on Road 1 or Road 2, the people of Village B would know that the high commissioner was visiting Village C. If the dances were held at Village A, this demonstration of superiority would not take place.

A delegation returned to the office of the FDA and declared that the people would dance only at Village C. The FDA was upset but eventually agreed. When the visitors came a few days later, they used Road 2, and drove by Village B on their way to Village C. The big men of Village C commented cheerfully on how they had been able to make the high com-

missioner and the FDA come on their terms and thereby demonstrate the importance of their village. After the dance, the high commissioner, the FDA, and other guests went to the school at Village B for a meeting, but this did not detract from the significance of the event. The administrators had no idea of the importance attached to these maneuverings.

In both these examples, relationships with Western political leaders gave the Tannese an opportunity to compete. The competition is not necessarily apparent to the Western leaders, and is not concerned with power and control per se. Rather, events are used to enhance the personal and village status of Tannese men. The in-group versus out-group competition is prevalent in historical records, contemporary political organization, and indeed, in Melanesian culture. For the Tannese every event, no matter how small or insignificant, has within it the opportunity to outsmart and outmaneuver others.

The John Frum Movement

In 1939 vague talk and rumors surfaced about a spirit in the southwest part of Tanna. An apparition would appear in a clearing at night, sometimes praising the government and the mission. According to informants, the being was described as human but in spirit form. It would appear at kava time at dusk at Green Point, usually dressed as a man although sometimes shrouded. People looking at the being's face did not see a human face, but something strange and unknown. The being would accept kava and food and would make pronouncements about many things. The language would vary, and informants said the spirit could speak all languages. Sometimes the being wore European clothing including "flip-flops," the now relatively common shoe.

Those who attended the kava ceremonies became convinced that the being was superhuman and began to invite others to attend. Invitations were sent to the missionary villages selectively to insure that *nimrukwen* and *koyometa* big men were invited simultaneously--a precaution to prevent a recurrence of the ancient schism that has long existed on Tanna.

In the area of Tanna where we were located, informants recalled that an invitation had been sent to them to attend a kava ceremony and meet the apparition. They had discussed the matter and decided that it was not essential to attend; instead, they sent a messenger with a *wulbie,* a small very sweet finger banana that is regarded as a food of the ancestors. This symbol was followed by a return message from the apparition indicating its understanding that the people of the area were in full accord with the goals set forward by the apparition. Attendance was not necessary.

Kava drinking and traditional dancing, prohibited by Tanna Law, spread rapidly in 1940. A tour of the island by Bell, a Presbyterian minister, and one of his pastors in early 1941 revealed that a return to traditional ways was taking place.[19] In April and May 1941 men began to drink kava openly, and many also began to spend all of their European money, abandon their gardens, and stop working. On 11 May, 1941, virtually no one attended the Presbyterian church at Lenakel. The churches everywhere were empty. While Bell reflected about the susceptibility of the people to superstition, District Agent Nicol tried to stop the escalating events. After returning from a trip to Aneityum, Nicol sent police to burn the houses used by the John Frum followers at Green Point and arrest the men. The men were taken in handcuffs to the Isangel prison where one, Manahewey, was chained to a tree without clothing as an example. He and another man were sent to prison at Vila, followed by many more over the next year. Any John Frum activities were interpreted as resistance to the government and led to incarceration in Vila. Those imprisoned included all of the big men from Green Point and later the Sulphur Bay big men from the east side of the island.

The Tannese men imprisoned in Vila were forced to work at hard labor on the roads. But John Frum came in spirit form to the imprisoned men and told them to watch for a symbol. They felt secure and confident that John Frum would provide for them and the Tannese.

The symbol that John Frum promised did come--in the form of American troopships. Airplanes, ships, soldiers (including some blacks), and war material were an impressive and magical sight to the Tannese. Since the Americans needed manpower to build roads, airfields, and supply depots for the drive against the Japanese, the Tannese prisoners in Vila were released to help. They joined two boatloads of Tannese men brought to Vila to work for about four months. Their payment, although low (six dollars per month), was more than they had ever received, and it was more than the French and British wanted the Americans to pay. The awesome amount of war material, the wartime experiences, the black American soldiers, and, perhaps most of all, the gregarious American style of mingling removed the blindfolds imposed on the Tannese by their isolation, by the mission, and by the colonial power of the British and the French.

During this period the Tannese worked for the Americans always believing that John Frum had been instrumental in bringing America to their island. The war kept everyone busy and arrests for John Frum activities were secondary to larger concerns. Many of the wartime experiences in Vila were understood within the scope of the supernatural world, and many were related to John Frum. It was a magical time for this group of

men, awakened from a colonial backwater, suddenly sharing--without preparation--a wider view of the world. Because they interpreted the new sights and scenes within the only framework they had, their understanding was limited. But the missionaries and the government administration had also shown only marginal comprehension, not recognizing how limiting their role had been and still remained.

Who was John Frum, the man? Evidence from fieldwork and knowledgeable sources points strongly to Jack Kohu. Kohu grew up at Green Point, a highly traditionalist area. During the mid-1930s, according to informants, he had been a policeman in Vila for the British. Not long after his return, John Frum first appeared. Kohu was a tall, muscular, well-built man. Many stories today relate Jack Kohu to the spirit world. In later life, a stroke left him partially crippled. Kohu developed a fistula, and it was reputed he did not excrete or urinate for seven years before his death. Supernatural events are said to have occurred after his death, and his body disappeared from its grave.

It seems probable that Kohu created an initial deception, then developed a plan, and in time skillfully led the opposition to the mission and the government. One of the intriguing events was the initial involvement of Nicol, who was investigating the disappearance of a number of goats. Some men in the district disclosed knowledge of the disappearance, claiming that a mysterious entity had asked for goat meat to eat after drinking kava. Given the Presbyterian prohibition against kava use, the activities were secret. Under some pressure the mysterious entity was identified as John Frum, and Nicol subsequently pursued him, though with little success.

Devising spoofs and exploiting situations to ones own advantage are popular among the Tannese. It may well have been that Kohu appeared to drink kava and disguised his identity purposefully. Perhaps through his disguise he was able to add a chicken or two to his diet, and then turned to the goats accumulated by a man of another village. The disguise and spoof led gradually to the emergence of John Frum as a mystical figure making pronouncements of a return to traditional ways. Converts and disciples soon followed, and--inevitably, given the two factions prevalent in the late 1930s and since--an east-coast version appeared. Eventually it developed into a cult of resistance to the existing social situation. This movement was skillfully directed by Kohu and others on the west side and by Nampas and others in a splinter group on the east side to restore Tanna for the Tannese. The deception, due to ripeness of the social conditions, matured quickly into a social movement, following the phases artic-

ulated by Burridge: 1) an awareness of being disenfranchised, 2) the development of a "new man," and 3) the use of new organizational skills and sect development.[20]

The prophetic role of Kohu had ample precedent on Tanna, and indeed throughout Melanesia, in myths and legends of spirit men from the past.[21] Kohu fulfilled the role described by Burridge, who claimed that "a prophet . . . must articulate thoughts and aspirations and emotions that are imminent in the community to which he speaks if he is to be acceptable." The Melanesian culture provided a framework within which the specific manifestation of John Frum appeared, in the form of Jack Kohu.

Another point of interest is the ensuing reorganization among the Tannese. Those on the west side sought a return to more traditional social patterns, previous land tenure rights and obligations, and ritual behavior. The east-coast sect practiced worship of a red cross, experimented with a confrontationalist stance toward the colonial powers, and sought island-wide political power. In keeping with Lawrence's delineation of motivation and means,[22] the Tannese sought to regain control of their own culture and society. The means used on the west coast involved a reversion to tradition, while the more acculturated east-coast Tannese turned to political activities. These changes had (as in Lawrence) a conservative impact on both groups. The confrontations of the east-coast group prevented the introduction of information, ideas, and change processes. The west-coast group developed a rich philosophy justifying their rejection of the Western way of life, but given the problems inherent in the larger world, this retreat to their own culture can also be claimed an excellent choice.

Another factor to be considered is the role of kava. John Frum advocated its use, and when men, including those from Christian villages, came to see him, they were invited to drink kava. Actually many of the men in the traditionalist area had been drinking kava actively since Yasu's stand against the Presbyterians, or even earlier. Whether the social ties to the Presbyterian mission loosened before or after kava use resumed is hard to ascertain. Perhaps they occurred simultaneously. Once the emotional conflict began, kava became a symbol of allegiance for both sides: a kava drinker was a John Frum supporter, and a nondrinker was a mission follower. Interestingly, kava was drunk at all hours of the day, by younger as well as older men. Kava roots circulated from village to village as gifts and symbols of belonging; to the movement. The mission continued with little success to try to stop the use of kava.[23]

The larger political situation must also be considered, particularly as a cause for the continuation of the John Frum cult. The British and French

administration were ineffective during World War II. "Actual administrative technique has occupied so much time and energy that almost nothing has been done for the unfortunate native--no education, no medical service, and very little oversight," wrote Frater.[24] Although there were dedicated administrative officers in Vila and Tanna, the system of government prevented effective services to the people.

During the height of John Frum activities on Tanna, some Europeans armed themselves. Such weapons as hand grenades, machine guns, pistols, and rifles were available. People were fearful in an atmosphere they considered threatening, but their concern was more for personal safety than for repression.

The war raged nearby from 1941 through 1945, and thousands of American, British, and Australian troops fought against thousands of Japanese. It seems possible, although it is difficult to substantiate, that at least one reason for the repression of John Frum was the unstable wartime situation, and the tenuous position of the British and French as governors of Tanna. Sir Harry Luke was the ranking senior British official in command of the New Hebrides from his station on Fiji. He resigned in 1942. Frenchman Henri Sautot had been in charge of the New Hebrides, but when France fell he took over the New Caledonia headquarters and proudly declared himself and his followers for De Gaulle. As the first French official to declare against the Vichy government he should have been honored, but it was not long before he was unceremoniously fired. It appears that the policy of repressing the John Frum cult was made by high-level military men unfamiliar with the New Hebrides (Vanuatu) and Tanna, during a time when there was some question about the ability of the governing forces to remain in power. The lack of experienced leadership may have contributed to subsequent policies and practices.

After the war the government sought to repress cult activities (and therefore the Tannese people) through harsh measures. There were repeated instances of John Frum outbreaks in locations scattered throughout Tanna. The government responded to each outbreak with the arrest and imprisonment of leaders and followers.

The repression of John Frum activities kept the courts busy, often over events that seem trivial in retrospect.[25] For example, six men planted another man's pipe in a garden to make it appear that he had stolen yams (14 October 1941). They conspired to do this because the man would not join John Frum. Nicol sentenced them to ninety days each at hard labor. A court case at White Sands (20 August 1943) concerned a man who claimed the mountains on Tanna were full of soldiers who would one day come forth to help John Frum. He received a three-month sentence. An-

other case on the same day involved three men who spread rumors that American troops would land at Sulphur Bay to help John Frum. The men also told people to keep John Frum strong by holding dances and drinking kava. Each was given three months in prison. Court Case 562 (7 May 1945) charged a man with sedition. He told people it was not good to work for Europeans and that John Frum would return to Tanna at the end of May. John Frum had given the people new laws, so there was no reason to follow the old ones. He received six months. Court Case 563 (18 May 1945) involved a man who claimed he was for John Frum and that people should drink kava and avoid the mission schools. He received one month for inciting people against the law and order of the Condominium.

The reaction of the government was also harsh in cases involving family relationships. One Native Court Case (1 March 1946) involved a woman convicted of adultery while her husband was in a Vila prison for John Frum activities. She received three months. A similar court case occurred the following year (18 July 1947). Another situation arose near Sulphur Bay, where two villages had won over their neighboring villages to Christianity some years before. In 1941, however, these neighboring villages opted for John Frum. By 1947, the two Christian villages sought again to convert their neighbors, and harsh government action followed this "John Frum outbreak." For their talk and activities, fourteen men were banished from Tanna, and the two villages were broken. The remaining people were sent to other villages and the wives of the fourteen men were told to remarry as they would never again see their husbands.

Repression of John Frum activities was achieved not only by using the courts to imprison offenders for relatively trivial offenses but also by sending leaders to Vila without benefit of court sentencing. Another special government policy[26] effected at the resident-commissioner level, was the establishment of "a regime of silence on the part of the European population in so far as concerns John Frum and all his acts past or conjectured most particularly in conversation with natives."

Why this repression occurred is a most important question. The Presbyterians faced their conflict with the John Frum movement with rigid and inflexible behavior,, not allowing their few remaining followers to associate with the "heathen" or the John Frum people, even when threatened. The Presbyterians felt defeated and discouraged for a time, but gradually they renewed. their struggle by working with a few children and hoping for a better future. The mission took an active role in again trying to outlaw kava and so may have been one cause of the political repression. This is another story, however.[27]

Recent Events

In October 1956 a new policy was set in motion after a meeting of the resident commissioners and the district agents. Strict adherence to the law was to be required, but the strange beliefs of John Frum were to be regarded as religion rather than an intent to disrupt. The new policy allowed both rumors and activities that honored John Frum. It seems that John Frum had united the Tannese people, and the repression had helped maintain the unity. When they no longer had to unite against the government, factions developed within the cult. Divisions that have long existed on Tanna arose again. This was to be expected, given the geographic separation of groups, the language differences, the split between *koyometa* and *nimrukwen,* the divisions caused by the British and French, and the differences found in the various contemporary religious groups. Because of these many factions, little unity exists in contemporary Tanna.

Some resistance continued and was dealt with by the government. Much of the resistance appeared due not just to hostility but to different ways of handling things. The Tannese traditional culture is viable, although atomistic, with workable methods of governing, a religion or set of beliefs, approved social behaviors, a sound economy, and so on. The Tannese enjoy their relationships with various governmental officials but do not usually accept subservient roles. When the government tries to install survey markers, arms permits, or road taxes, the Tannese block the attempt and go about their business. When the government attempts to treat yaws, take a census, or do something about the economy, the Tannese may or may not cooperate. They have a choice because their customs are strong, and they can survive and even live well by following traditional customs.

Given the power of the intrusion from the West and the rapid conversion of many Tannese, there is no doubt that the shock waves through the existing atomistic structure caused concern. This anxiety created conditions ripe for cult development, which, when it came, served to reduce the anxiety and to provide opportunity for a different type of competition. The Tannese had a chance to defend themselves against the influences of the West, and they chose this alternative. The strategy of a prophetic man, and then a number of followers, led the Tannese to greater control over their island.

An increasing orientation toward France, Great Britain, Australia, New Zealand, and Port Vila is now evident among the Tannese. Much attention is given to the two major political parties and the fight they wage for political control in Vanuatu.[28] The fragmentation of the few Eu-

ropeans among very different reference groups located elsewhere, fits the style of the Tannese people. Frequently one or several villages joins a particular European faction to provide an ideology, to act as a go-between, or as an access to resources. An islandwide allegiance to a particular set of institutions does not exist. Fragmentation and atomistic social structure characterize not only the Tannese but also the European community. Yet allegiance to belief in John Frum persists among many of the Tannese. Indeed, there are sons of John Frum now who are spiritual descendents. Their future depends on the political situations that emerge, particularly the developments that take place because of independence, gained 30 July 1980.

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NOTES

We would like to thank Jean-Marc Philibert, John G. Peck, and Monty Lindstrom, for comments and reviews of early versions of this paper. All errors are, of course, those of the authors.

This paper was completed prior to the publication of the excellent book *Vanuatu: Politics, Economics and Ritual in Island Melanesia,* edited by Michael Allen (Sydney: Academic Press, 1981). A chapter by Ron Bruuton, "The Origins of the John Frum Movement" is of particular relevance.

1. Fieldwork was conducted on Tanna, Vanuatu in 1976 and part of 1977 under U.S. Public Health Service Grant NIDA DA 01129. At the time, Robert Gregory was a Research Associate in the Department of Anthropology at Duke University, Durham, North Carolina and Janet Gregory was a doctoral candidate in the Department of Special Education at the Univeristy of North Carolina at Chapel Hill, North Carolina. The study was designed to examine possible relationships between kava use and cult formation. The final report was titled "The Relationship of Kava to a Cultural Revitalization Movement." Part of this article is derived from the report. Its new title was suggested by Jean-Marc Philibert of the Department of Anthropology at the University of Western Ontario in early 1979.

2. Vanuatu was known as the New Hebrides until it gained independence from both British and French colonial rule on 30 July 1980.

3. Editor, "Death of J. Campbell Nicholson," *Quarterly Jottings* 167 (1935):4.

4. W. Wilkes, Letter, 24 November 1913, Folder 6, File 79/1913, Western Pacific Archives, Suva, Fiji. (The Archives have subsequently been moved from Fiji to the United Kingdom.)

5. M.E. Shutler and R. Shutler, Jr., "Origins of the Melanesians," *Archaeology and Physical Anthropology in Oceania* 2 (1967):91-99.

6. J. Guiart, *Un Siècle et demi de contacts culturels à Tanna, Novelles-Hebrides* (Paris, 1956).

7. R. W. Casson and R. J. Gregory, "Kinship in Tanna, Southern New Hebrides: Marriage Rules and Equivalence Rules," *Anthropological Linguistics* 18 (1976)168-82.

8. M. Frater, "Dr. Thomson Macmillan: Notes," *Quarterly Jottings* 198 (1942):4-5.

9. C. McLeod, "News from the Field," *Quarterly Jottings* 215 (1947):6-7.

10. B. Paul, *New Hebrides Advisory Council Minutes,* 6th Session. (1962):51.

11. W. Wilkes, Letter to the British Resident Commissioner, 10 June 1915, Folder 79/1913-1915, Western Pacific Archives, Suva, Fiji.

12. J. C. Nicholson, "From Our First Mission Station," *Quarterly Jottings* 91 (1916):8.

13. J. C. Nicholson, "From Our First Mission Station," *Quarterly Jottings* 91 (1916):9-11.

14. J.M. Nicol, Letters to the British Resident Commissioner, 7 November 1923, 13 November 1932, 23 November 1933, and 8 January 1934, Western Pacific Archives, Suva, Fiji.

15. See note 14 above.

16. Missuaren, Letter, 14 Novemer 1923, Folder 29/1913-2 of the Western Pacific Archives, Suva, Fiji.

17. See note 14 above.

18. W. Armstrong, "Letter," *Quarterly Jottings* 186 (1939):6-8.

19. H. M. Bell, "Letter," *Quarterly Jottings* 194 (1941):8-11; and Bell, "Nalau Keeps the Faith," *Quarterly Jottings* 209 (1946):9. In addition, we are indebted to Rev. Ken Calvert for use of a tape made by Rev. Bell.

20. K. Burridge, *New Heaven, New Earth: A Study of Millenarian Activities* (New York, 1969):155; Brunton, R., "The Origins of the John Frum Movement: A Sociological Explanation," in *Vanuatu: Politics, Economics and Ritual in Island Melanesia,* edited by M. Allen (Sydney, 1981).

21. Writing about the foremost deity in Fiji, C. Wilkes stated:

> No one pretends to know the origin of Ndengie, but many assert that he has been seen by mortals. Thus, he is reported to have appeared under the form of a man, dressed in masi (white tapa), after the fashion of the natives, on the beach . . .

Wilkes also found:

> Among other forms of this superstition regarding spirits, is that of transmigration. Those who hold it, think that spirits wander about the villages in various shapes, and can make themselves visible or invisible at pleasure; that there are particular places to which they resort, and in passing these they are accustomed to make a propitiatory offer of food or cloth.

See C. Wilkes, *U. S. Exploring Expedition* (Philadelphia, 1845), vol. 3, 82-84.

22. B. Lawrence, *Road Belong Cargo: A Study of the Cargo Movement in the Southern Madang District, New Guinea* (Manchester, 1964).

23. For a more detailed explanation of the role of kava, see R. J. Gregory, J. E. Gregory, and J. G. Peck, "Kava and Prohibition in Tanna, Vanuatu," *The British Journal of Addiction* 76 (1981):299-313.

24. M. Frater, "The New Hebrides Today: Dr. Frater's Broadcast," *Quarterly Jottings* 201 (1943): 1-6.

25. Court Cases, Files of the British District Agency, Isangel, Tanna, New Hebrides, and Files of the Western Pacific Archives, Suva, Fiji. This time period is discussed in J. Guiart, *Un Siècle et demi de contacts;* and in P. O'Reilly, "Prophetisme aux Nouvelles Hebredais: Le Mouvement Jon Frum à Tanna 1940-1947," *Le Monde Aux Chretian,* New Series 10 (1949):192-208.

26. Blackwell, 9 May 1947, Files of the Western Pacific Archives, Suva, Fiji.

27. See note 23 above.

28. J. Jupp, "The Development of Party Politics in the New Hebrides," *Journal of Commonwealth and Comparative Politics* 17 (1979):263-82; W. Lini, *Beyond Pandemonium: From the New Hebrides to Vanuatu* (Wellington, 1980); and M. Lindstrom, "Cult and Culture: American Dreams in Vanuatu,"*Pacific Studies* 4 (1981): 101-23.

BOOK REVIEW FORUM

Derek Freeman, *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth.* Cambridge, Massachusetts: Harvard University Press, 1983. Pp. xvii, 379, illustrations, notes, orthography, glossary, index. $20.00.

Review: Fay Ala'ilima
Leeward Community College

In this 1983 book Derek Freeman attacks the validity of Margaret Mead's 1928 thesis that Samoan adolescents are relieved of storm and stress because of the easy and permissive nature of their society. But that is not all. By "unmaking her myth" he also hopes to shake the very foundations of American anthropology, which he claims has been misled by her Samoan research into an era of blind cultural determinism.

It is a crusade for which he shows considerable enthusiasm. He marshalls an impressive array of historical, statistical, and psychological evidence to show that far from being pleasant, easygoing people, Samoans are involved in more murder, rape, child-abuse, and general mayhem than almost any society in the world. He attributes this tendency toward violence to their authoritarian ranking system, puritanically enforced by chiefs and now Jehovah as well.

This sounds for a moment as if he too is about to reach a culturally-determined conclusion. But no. He summarizes his efforts as follows: "The time is now conspicuously due" for us to recognize "the radical importance of both the genetic and the exogenetic and their interaction."

Most people I know came to that conclusion long ago. The book does not seem to add much to our actual knowledge of this topic. What it does seem to document thoroughly is the darker side of the Samoan character, and for that, he claims, they are tremendously grateful.

I am not an anthropologist, only an American wife who has been living in a Samoan family for thirty years. I have seen days (and nights) like Margaret Mead's and moments of mayhem like Freeman's. No one who has lived in Samoa long could doubt the existence of both. My only problem is with people who, like the blind men and the elephant, feel for one aspect or another and draw conclusions about what Samoans really "are."

Freeman says Margaret Mead buried the deviations from her mild, permissive norm into one chapter and forgot about them. He himself seems to have put the deviations from his repressed-agressive norm into one paragraph at the bottom of page 278. Why doesn't he include a chapter on Samoan hospitality, based on his own experience with Lauvi who has welcomed him into his household for many years? And on the chiefs of Sa'anapu who not only honored him with a title but trusted him with access to their deliberations? He might even have included a chapter on Samoan forebearance. Despite their "violent tendencies" the chiefs of Sa'anapu have not banished him for revealing to the world only their darker tendencies. Would a town in Australia have shown such tolerance?

Freeman may feel that a one-sided study is warranted in order to unmake Margaret Mead's myth and save anthropology from cultural determinism. Others may claim it is more related to the making of Freeman. But in the relentless pursuit of knowledge and each other, don't anthropologists consider what their prounouncements do to the people they study?

Coming of Age in Samoa lured many starry-eyed young Americans (like myself) to Samoan shores. The "unmaking" of the myth makes me wonder how I escaped alive! Derek Freeman's book may bring him fame and fortune but it will hardly make life easier for thousands of Samoans struggling to gain acceptance in Honolulu, Auckland, and Carson City. For them it may operate more like a stereotype than a great intellectual discovery. I am not sure they will continue to thank him.

Review: Tuaopepe Felix S. Wendt
Western Samoa

I have no claim to expertise in the realm of anthropology that would necessarily qualify me to comment on this publication by Professor Derek Freeman. Therefore my contribution to the current debate will be made simply as a Samoan and a participant in the culture about which these renowned anthropologists, Mead and Freeman, have written.

I was thirteen years old when I first came across *Coming of Age in Samoa* by Margaret Mead. That was in 1951, some twenty-six years after Mead had spent nine months on Taū in American Samoa doing the fieldwork that became the basis of her book. Prior to that, I had never heard of Margaret Mead (and I do not think very many of my peers at that time had either), nor had I been aware of any controversy concerning her work in Samoa.

As a young intermediate-school student at the time, my initial interest in the book centered on that fact that it was, supposedly, about Samoa.

However, as I read through the book I found myself asking the questions, "Is this really true?" "Where is Taū, this place that Mead is talking about?" I was having great difficulty recognizing the Samoa Mead wrote about.

Up to age thirteen I had lived in Apia (the, village, not the town) where my family *(aiga)* had been one of the principal families to found the Congregational Church of Jesus in Samoa, a breakaway group from the LMS church. My grandfather, my father, several uncles, and other family members were lay pastors for the church. We later lived at Malie where my father was for many years the lay pastor for the church, and in Lefaga where my father held the family title Tuaopepe. Growing up for me was, therefore, very much in the "*faa*-Samoa," in a rather strict religious environment.

For me, Margaret Mead's idyllic, romantic description of Samoa, with its easy life, free love, and uncomplicated adolescence, was always a myth, a dreamworld. It was nothing like the real world I grew up in. In fact, if anything, the romanticism of Meads writings prompted in me a wishful yearning to be in that paradise with all that free love and carefree life.

There was no doubt in my mind, nor in the minds of my classmates, that Dr. Mead was describing something in her own mind. and imagination. As we used to say at the time "Manaia tele mafaufauga ole teine. Maimau pe ana moni" (The girl's thoughts are nice but if only they were true). But then it was not too difficult to guess why Margaret Mead got carried away in 1925--a young woman of twenty-three, unattached, raised with the attitudes about young people and sex no doubt typical of the eastern United States--and it seems very likely that a lot of her findings and conclusions reflected her own wishful yearning to be part of a society where life was not complicated by the social turmoil and "hang-ups" found in her own society.

By the time I first left Samoa at age seventeen in 1956 for New Zealand to continue my education, I had read Mead's book three or four times. By then I had gotten over my own fantasies and wishful thinking. Like many other young, educated Samoans at the time, I was firm in my assessment that many of the things Margaret Mead said about Samoans were incorrect. However, in New Zealand and elsewhere, what we knew and felt was unimportant and mattered little to the intellectually knowledgeable.

My training in the sciences has given me a great appreciation of the scientific method and its use as a research tool. I wholeheartedly agree that Dr. Mead's findings were based on superficial and shoddy research

techniques, and I have, over the years, been unable to excuse her on the grounds of lack of knowledge of the scientific method. I have great admiration for her achievements and acknowledge her to be the intellectual giant of social science that she came to be; but because she did not have the courage to reassess and recheck her work in Samoa, I can only reaffirm to myself that she was completely misled about Samoa.

Like most Samoans who travelled and went to school overseas, I have had to live with Margaret Mead's Samoa, the way in which, invariably, I was perceived by most European *(palagi)* people. Over the years I have developed a thick skin and the ability to simply ignore or explain away the questions on free love. And as one of my friends expressed it while we were attending school in New Zealand, if some people refuse to believe otherwise and expect me to be "with it with the ladies then why shouldn't I capitalize on it? After all the lie is not mine."

A good half-century has lapsed since *Coming of Age in Samoa* was first published. While our outrage in the early years against our portrayal as a joyously promiscuous society mattered little to the intellectual world. the damage Margaret Mead did has, with time, healed. We learned to live with it. As advances in communication made the world smaller, Samoa opened up to the world at large. Through actual experience many people found out for themselves that Margaret Mead's Samoa was, for the most part, a myth.

Now in 1983 comes Professor Derek Freeman with his book, *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth.* Professor Freeman, whose acquaintance with Samoa spans some forty years, and who regards himself as an authority on Samoans and things Samoan, set out. to refute Dr. Mead. This he has very ably done. His meticulous presentation of historical and statistical evidence is overwhelming. His use of the scientific method, step by step, breaking down each of Mead's findings, shows why they cannot be accepted as valid. Logically, he arrives at the conclusion that Margaret Mead's findings "are fundamentally in error and some of them are preposterously false." There is no doubt in my mind that Freeman's book is a major achievement in research and scholarship, and will add significantly to the body of knowledge of Pacific societies and their cultures. I am, to a certain degree, thankful to Professor Freeman that his work has finally produced the documented evidence considered credible enough by the intellectual world to substantiate the doubt we Samoans have always had regarding the truthfulness and accuracy of Mead's findings.

However, some fifty-five years after the fact, when Samoans have learned to live with Mead's myth (and in a way risen above it), do we

really need such a refutation? What good does it do in 1983 to finally have Mead's 1928 findings proved false? Is it possible that Freeman has created another image, possibly another myth, of the Samoans? That "violent, competitive, extremely puritanical, delinquent, rape and suicide prone, Jehovah dominated, and rank-bound people"? Even if I am to concede that Freeman is correct in his findings, the important question now is what will this description do to us?

This is where I have great difficulty crediting the motives said to lie behind this study. Some people have stated that Freeman's "love" for Samoa and Samoans motivated him to "champion" their cause, and he set out to undo the damage Mead had done. Unfortunately, I do not think very many Samoans (myself included) are applauding a champion who has made them appear like the gang of hoods in Charles Bronson's "Death Wish II." In the words of one of my lawyer friends who doubted there were many Samoans of either Mead's or Freeman's types, if these were the only choices, he "was sure to God" he did not want to be the latter.

At that, I began to wonder out loud whether Freeman's labor was one of love (based on his own personal "feeling" for Samoa), or one directed more by the intense emotions of some of his principal collaborators and informants, staunch advocates of Samoan puritanism as adhered to and preached by many of the Protestant congregational denominations in Samoa. This strong sense of evangelical purity comes through loud and clear arid has greatly influenced Freeman's analysis and reconstruction of the so-called "Samoan Ethos." A "Thou shalt not, Moses complex" pervades his conclusion that "for most Samoans, there is no escape from the insistent demands of their society, one of its fundamental principles being that anyone who disobeys the instructons of those in authority should be duly punished." Further, "while Samoans frequently talk of the boundless love of Jehovah, they also view him as a God who may become 'full of anger for sinful people', and who will strike down, in infirmity or death, those who have broken his commandments."

In other words, "Jehovah is believed by Samoans to be a punishing God, and the punishment he metes out, while it is greatly feared, is also looked upon as being God's chosen and just way of dealing with the willfully disobedient." The evidence, as Freeman interprets it, suggests that the "punitive regime has long been' endemic among Samoans ever since their conversion to Christianity, and has been justified in terms of the principles by which Jehovah Himself is believed to rule Samoa--punishment having become culturally established as the sovereign way of dealing with those (including children) who do not heed the dictates of authority."

Freeman duly provides an impressive array of evidence to support his interpretation that Samoans give preeminence to the Jehovah concept of authority and punishment. However, it must also be stressed that this same Samoan ethos recognizes that Jehovah is a God of great and unending love. Though Freeman duly acknowledges this, it is almost as an afterthought: "The Samoans do indeed have a 'dark side' to-their lives. . . . And, as with all human societies, they also have their shining virtues" (p. 278). Thus his conclusions and findings are overstated, biased, and weighted very much with this "thou shalt not, Moses complex." The evidence, the statistics are too exact, too much like a scientific experiment designed to prove that Samoans are Jehovah-ridden, violent, competitive, prone to assault, manslaughter, rape, and jealousy, rather than to prove Margaret Mead's nature-nurture and negative instance theories wrong.

I contend that the (overriding characteristic of the Samoan ethos is *alofa* (love); alofa is the foundation of the total *fa'a* 'Samoa (the Samoan way of life). Alofa in the Samoan ethos is not just "shining virtues" as Freeman portrays it. I find Freeman patronizing and paternalistic, in spite of his claim to be an authority on Samoa, as he quotes other palagi authorities on the shining virtues of Samoans: "lively, jocose, kind people" (Wilhams); "a people more prepossessing in appearance and manner" (Erskine); "the most polite of Pacific peoples" (Sabatier); "Samoans are wonderfully hospitable and generous. . . . [They] can display great magnanimity"; "[There is] no more memorable instance of the kindliness of Samoans than the road that a group of high ranking chiefs built for Robert Louis Stevenson . . ." (Freeman). In making this contention, I am well aware that some quarters of the academic world will demand proof and corroborative evidence. I have none, other than the fact that I am a Samoan.

Alofa is the principal component of the Samoan ethos. That concept is instilled from birth, as Samoans are taught about the importance of every bond: *Alofa i lou matua* (love your parents); *Alofa i lou aiga* (love your family); *Alofa i lou nu'u* (love your village); *Alofa i lou itumalo*, (love your district); *Alofa i lou Atunuu* (love your country). Alofa is sharing, giving, helping, responding, and contributing to the needs of others. It is willing participation in ones family, village, and community affairs. It is love expressed physically in the giving and receiving of material goods and services, the confirmation of being a part of the social group. Alofa is not, extolled simply as an ideal when the chiefs meet in village council, as Freeman makes it out to be. It permeates all levels of the social life of Samoa. It is the essence of the "bright side" of Samoan life, which far outshines the "darker side" that Freeman has dwelt on with such excess. To

ensure a balanced perception of Samoans with regard to the darker and brighter sides (and more so, to be fair to us), Freeman should have analyzed in depth and discussed in equal detail those facets of Samoan life that relate to and are based on alofa. These include the sharing, giving and receiving of goods and help in *faalavelave,* be it a birth, wedding, funeral, house-, church-, or school-building, or plantation work, fishing, travelling, care of the young, the elderly, and the sick; the social norms regarding illegitimacy and the treatment of unwed mothers and illegitimate children; and the *ifoga.*

The *fa'atamālii* that Freeman describes as "conduct characteristic of an aristocrat," refers to chiefly rank, but the form of behavior it engenders--*tu fa'atamālii* (behaving like an aristocrat), *amioga fa'atamālii* (behavior becoming a person of good breeding)--is sought after, taught to, encouraged and inculcated in every Samoan from birth. Thus Samoan generosity is not mere face-saving, superficial, impassive hospitality, motivated by gentle, passive obedience, but *alofa fa'atamālii*—love extended by giving and sharing the best there is available. I am certain that Freeman himself, in his forty year acquaintance with Samoa, can vouch for alofa fa'atamālii through his own personal experiences. The fa'atamālii of Tamasese Lealofi III, when he admonished that peace must be maintained at any price, was not prompted by authority, rank, or supremacy, but by the greater feeling of *alofa ile atunuu* (love for his people).

Freeman seems to have spent the best part of some forty years working on his refutation of Margaret Mead. During that same period, he studied Samoans and Samoa: he adopted (or was adopted by) a Samoan family at Sa'anapu (a village adjacent to my own at Legaga) where he eventually acquired a *matai* title; he learned the language and speaks it well (so I am told); he has lived "like a Samoan" for lengthy periods; he understands and knows Samoan custom and the faaSamoa; and he loves and respects Samoa and things Samoan. But in spite of all this, what I found missing from the book was a "feeling" for things Samoan as a Samoan. In this respect; Freeman, like many other palagi I know of, learned Samoan, lived, dressed and behaved as a Samoan, respected and loved Samoa, but is still "pseudo-Samoan" in that he did not and could not feel as a Samoan does. Most of these people (and Freeman, it appears to me, is no exception) had their own "hang-ups" just as Mead did. They came to Samoa (ironically many lured and attracted 'by what Mead wrote) hoping to find some thing, some place, some people to identify with and to belong to. They probably felt lost, alienated in their own countries, expecially those in which material wealth and affluence have sapped much of the humaneness of society. Freeman found something in Samoa he had lacked else-

where. But while he might have become a Samoan in nearly every way possible, he was still a palagi inside. No true Samoan who "feels" things Samoan would cultivate the confidence, the trust, and faith of his people in order to become privy to their "secrets" and intimate personal lives, then turn around and expose these "secrets" publicly to the world, and with much exaggeration, imagination, idealization, perhaps even to the extent of purposeful misinterpretation.

Freeman readily dismisses the "reaction of some Samoans" that "Mead lied," that her Samoan '"informants must have been telling lies in order to tease her." He states that Samoans themselves have offered the explanation in the form of behavior called *tau fa'ase'e,* which literally means deliberately duping someone. Freeman, from his knowledge of Samoa, knows very well that tau fa'ase'e does involve deliberate telling of half-truths or lies. It generally occurs when the respondent gives the answer that he/she knows is what the questioner wants to hear, even though the respondent knows it is not the truth. Thus the example he quotes from Milne, "e fa'ase'e gofie le teine, the girl is easily duped," describes that form of behavior in Samoan boyfriend-girlfriend, man-woman relationships wherein the boy/man tells the girl/woman what she wants to hear, even though he knows (in fact, most times she knows it too) that it is a half-truth or a lie. It is to be noted that the opposite also happens just as frequently, "e fa'ase'e gofie le tama, the boy is easily duped." It is a form of flattery and sex play.

Freeman, however, goes on to state that there, is. no detailed corroborative evidence to confirm the truth of this Samoan claim that Mead was "mischievously duped" by her adolescent. informants. Be that as it may, if Mead was not the victim of tau fa'ase'e (I personally believe, as do most Samoans of my generation, that Mead was duped), then she must have purposely, deliberately, and knowingly given incorrect information on Samoa, thereby misleading the intellectual world. This in Samoan is *tau fa'asesē,* the deliberate action of telling falsehoods to mislead other people. In other words, as Freeman himself concludes, Mead's findings "are fundamentally in error and some of them are preposterously false." If this is so, then to my layman's mind Margaret Mead lied about Samoa. But oddly enough, Freeman does not agree with this. Why? Clearly, either Mead was the victim of tau fa'ase'e or she was the perpetrator of tau fa'aseē or he has some doubt about the integrity of his own informants--might they be duping him too? Or is he himself behaving tau fa'asesē?

Some people have expressed the view that Freeman has done us (Samoans) a good turn by finally dispelling Mead's illusion of Samoa. Unfor-

tunately, the more I re-read Freeman's book, the more difficulty I have identifying what constitutes this "good turn." Granted he has to a large extent succeeded in refuting Mead. But he has at the same time, contributed significantly to confirming another stereotype of Samoans--that they are temperamental and violent. While Mead's work was tainted very much by romantic notions and the wishful thinking of a young woman seeking her ideal society, it is possible that Freeman's conclusions reflect the disenchantment of another palagi academic who, in his search for his ideal society, in living and trying to become a Samoan, has, over a forty-year period, grown old and disillusioned with the changing faces of Samoa?

Samoans are human like everyone else, and always have been, even way back in the early 1920s. We have always been and still are a sexually tolerant and gentle people. As with people the world over, we have our joys and our sorrows, we can cry, get angry, and sometimes have fights. But our darker side is no darker than that of any other people. If we are so much more prone to assault, manslaughter, and forceful rape as Freeman has made us out to be, how many times during his forty years of experience in Samoa was he abused, sworn at, or even punched in the face by a Samoan? Was his wife or daughter ever sexually molested?

If as a matai he participated in village *fono* at Sa'anapu, and their collective experience was "much given to extolling obedience as the essential basis of virtue and concord.," and if as a matai he himself "condemned freedom of action as the source of sin and social disorder," then in such a setting, even I would be disillusioned. In that respect, I am glad my village fono (as with most other village fono's in Samoa) has much more important things to discuss than merely insisting on obedience and curtailing people's freedom to act.

Review: Nancy McDowell
Franklin and Marshall College

Derek Freeman has two related goals in this book. The first (xii-xiii) is the narrow aim of refuting Margaret Mead's ethnographic descriptions and general conclusions about Samoa and thereby discrediting her assertion that Samoa provides a "negative instance" for the universal presence of storm and stress in adolescence. His second goal is less particularistic and more relevant to general theoretical concerns.

> This book, then, while primarily given to the refutation of the general conclusion that Mead drew from her Samoan researches,

> is also concerned with examining related aspects of the wider myth of absolute cultural determinism, and with arguing that this now antiquated doctrine should be abandoned in favor of a more scientific anthropological paradigm. (xvi)

To this latter end, Freeman examines the rise of modern American anthropology. It is primarily to Boas that he attributes the blame for "absolute cultural determinism" and the supposed denial of the role of biological factors in human behavior which still, according to Freeman, characterizes "many anthropologists" (294).

These, then, are Freeman's stated goals. However, his historical analyses and theoretical discussions are so biased and inadequate that the more general framework quickly collapses to expose what this book really is--an attack on Mead that has almost no general or constructive relevance to contemporary anthropology. I begin my remarks by assessing the adequacy of his presentation of history and theory in anthropology, pause briefly to examine his scholarly methods, and finally address more specific questions and doubts pertaining to Mead and her research.

History and Theory: Boas and Absolute Cultural Determinism

Freeman is certainly correct in describing absolute cultural determinism as an "antiquated doctrine," so antiquated in fact that I doubt many take it seriously (today. While it is true that many anthropologists specialize in and stress culture more than biology in their research (and vice versa), I can think of no anthropologist who would unequivocally deny that human behavior is the result of an extremely complex interplay between both spheres.[1] Freeman cites no contemporary examples and is tilting at nonexistent windmills. He never grapples with the crucial problem of explaining behavioral diversity between populations without at least recognizing a primacy for cultural factors.

By denying us an ethnography, Freeman denies us the context(s) in which his facts and data exist. Since most of the evidence he offers is itself cultural and not biological (a striking fact discussed below), he presents us with cultural facts out of context, and such data are easily distorted and sometimes quite meaningless. He would have come closer to achieving his aim of correcting what he perceives to be Mead's inaccurate picture of Samoan life if he had presented us with an accurate one.

Freeman's entire argument rests on his assumption that in refuting Mead's negative instance, he demolishes forever the idea that adolescence is not necessarily a period of stress. Nowhere does he consider the possi-

bility that other negative instances might have been put forward since 1928; it is as if his ethnographic clock had stopped over fifty years ago and nothing had been published since. In fact, the ethnographic literature is full of examples of societies in which adolescence is not what it was in the 1920s in the United States, including many in which stress is not a necessary concomitant.[2] Further, Freeman himself cites Katchadourian, from a book entitled *The Biology of Adolescence*, who said that "research on ordinary adolescents has generally failed to substantiate claims of the inevitability and universality of adolescent stress" (Katchadourian in Freeman: 255).

Freeman clearly calls for a "more scientific anthropological paradigm" than absolute cultural determinism, and surely no one can fail to be in sympathy with this goal of recognizing the interaction between cultural and biological factors in understanding human behavior. However, his call is hollow for three reasons. First, although the refutation of Mead's data does not logically require that Freeman present biological evidence, it is highly significant that he himself presents predominantly cultural data. Second, his final conclusions seem to be mere lip service to the idea of cultural-biological interaction in human behavior--facile generalizations without discussion or extended example. Third, he ignores the existence of biological anthropology in the United States, both its history and contemporary liveliness. I will amplify each of these statements before going on to explore his version of the history of anthropology.

Freeman's own data are overwhelmingly cultural--in one case so cultural that the mind boggles.[3] He continually asserts that Samoans are not as Mead portrayed them (they have a different ethos, character, etc.), and these differences are the basis for his refutation of Mead. What is ironic is an implicit contradiction Freeman fails to address or even to understand. Why, according to Freeman, are the Samoans not gentle and unassertive as Mead described them? It is not because of biological factors, as one might suspect; it is because of Samoan childrearing practices! Freeman does not seem to realize that his basic approach to the issue of why Samoans are as they are is completely consistent with Mead's major tenets. Freeman is unambiguous: 'Samoan character . . . is very much the product of the way in which discipline is imposed upon young children" (216). He also states that "this behavior, in ethological terms, is a form of redirected aggression, and its prevalence among Samoans is evidence of the tension generated within individuals by the mode of discipline imposed upon them from childhood onward" (219). This second quote is especially interesting because it indicates an attempt to incorporate biological ("ethological") considerations. The behaviors he refers to here are "manner-

isms" such as "the agitated moving of the fingers of the hand in states of frustration. . ." (219). But instead of linking the behavior firmly to any kind of biological factor, he clearly terms it the end result of childrearing, or "mode of discipline."

Freeman does not provide any significant examples of the biological factors that influence human behavior, or how they seriously interact with cultural ones. He does attempt to incorporate biological evidence for aggression, but fails to make the point because he still asserts that childrearing patterns are the genesis of Samoan character.

> Within Samoan society there is very frequent resort to punishment, and I would argue that it is in particular a pervasive dependence on the physical punishment of children that makes Samoans so disturbingly prone to interpersonal aggression. [Several studies] . . . have clearly shown that punishment enhances rather than inhibits the expression of aggression. And this conclusion has been corroborated by D. D. Woodman's finding that physical punishment is allied to aggression outside the home. Woodman's researches also suggest a biochemical component in interpersonal aggression, with an increase in noradrenaline being linked with increasing aggression in personality. It seems likely that it is the regime of physical punishment, and especially of children, that generates the "air of violence on a tight rein" reported by Mackenzie, and that results in Samoans flying "from feathers to iron" at the slightest provocation, to engage in the physical violence that they have come to accept as customary. (275)

The first section of the book is an examination of what Freeman perceives to be the rise of the paradigm of absolute cultural determinism, and the second is a point-by-point refutation of Mead. Only in the last chapter does he finally directly address the interaction of biology and culture. Readers who expect a synthesis with significant exemplary examples will be disappointed.

> We have before us then, a view of human evolution in which *the genetic and exogenetic are distinct but interacting parts of a single system*. If the working of this system is to be comprehended it is imperative . . . that a clear distinction be made between the genetic and the cultural, for only in this way is it possible to understand "the causes and mechanisms of change in

> any organism capable of both cultural and genetical change." This requirement, furthermore, holds not only for the study of mote evolutionary history of the human species, but equally for the analysis and interpretation of cultural behavior in recent historical settings. In other words, specific cultural behaviors, to be understood adequately, need to be related to the phylogenetically given impulses in reference to which they have been evolved, and in apposition to which they survive as shared modes of socially inherited adaptation. (299-300; emphasis in original)

I have quoted this passage at length because it appears to be Freeman's final conclusion although it receives minimal discussion. He does go on to cite a brief example involving Samoan respect language. He argues that this elaborate convention developed as a way to "avoid potentially damaging situations" (300) in which there might be "an extremely rapid regression from conventional to impulsive behavior" (300).[4] (Incidentally, this example, a short one that demeans the complexities of Samoan respect language, is not the only place where Freeman seems to take a Hobbesian view of human nature with more than a dash of Malinowski added.) The example fails to illustrate the point because we see a "cultural convention" (300) responding to presumably innate aggressive impulses--we do not see a complex interaction, only a cultural response to supposed biology.

> It is thus evident that if we are to understand the Samoan respect language, which is central to their culture, we must relate it to the disruptive emotions generated by the tensions of social dominance and rank, *with which this special language has been developed to deal.* (301; emphasis added)

It is important to note two things about these passages. After presenting an example purporting to support the basic theoretical stance of the first, he associates the respect language not with biology at all but with social dominance and rank, which are cultural conventions themselves (he does not try to associate Samoan hierarchy with pecking orders and biological manifestations of ranking). Second, he begins with the interaction of biology and culture but subtly shifts away from interaction to understanding behaviors as evolutionary responses to "phylogentically given impulses" and as things that exist "in apposition" (not in interaction). There is no interaction between biology and culture here at all. By adopting this ap-

proach, Freeman cannot help but find himself (as others have before) without a theoretical means of accounting for the diversity of behavior apparent between human groups.

Freeman was trained at Cambridge and therefore is unfamiliar with American physical or biological anthropology. However, he blames Mead's errors on her adoption of the anthropological paradigm generated by Boas, and his theoretical attack is predominantly against Boas and other early American anthropologists (Mead, Kroeber, Lowie, Benedict). Boas was certainly the prime mover in establishing the importance of culture, but it is odd that Freeman's attack is almost exclusively on American anthropology, which even today maintains basically a four-field approach, which considers biological anthropology essential to the discipline and in which the interaction between biology and culture has always been important.[5] Nowhere does Freeman even mention the existence of biological anthropology--it is as if it did not exist. Considering the reading he did on the history of the discipline, there is no excuse for ignoring the fact that biological factors have always played a role in American anthropology. He could have cited numerous excellent studies to support his position concerning the importance of biological factors, yet curiously he does not. If he had, I think two things would have happened: (1) his call for the new paradigm would have looked foolish because many anthropologists are already doing this kind of research and have been for some time; and (2) he would have been forced to recognize the significant and positive role of Boas himself in establishing the importance of biology in the discipline.

Freeman argues strongly that Boas was an "absolute cultural determinist" but this portrayal of Boas does not adequately represent the complexity of Boas' thinking. Freeman (47, 282) accuses Mead, especially, of "suppressing" evidence contradictory to her main ideas[6] and yet it is Freeman himself who suppresses and distorts, who fails to present all of the significant information concerning Boas and this "absolute cultural determinism." As he seems to have consulted most of the relevant sources, the picture he gives surely involves a conscious manipulation to bolster the weak theoretical framework of the book. The simple truth is that although Boas stressed cultural factors, he was far from being an absolute cultural determinist. Boas' foremost concern was for rigorous science. He himself conducted research on biological factors, especially those affecting human growth, as well as culture. To some extent his own work documented the interaction between heredity and environment despite the fact that he remained skeptical that biology would ever help explain historical process or cultural diversity.

Freeman's discussion begins, as it must, with a consideration of events in biology during the nineteenth and twentieth centuries. He writes at length about Galton and the eugenics movement in the early chapters and makes it clear that Boas reacted against the racist nature of this opposing paradigm. Freeman states that early in his career Boas called for cooperation between biology and anthropology, but argues that Boas later became more extreme and denied the relevance of biology altogether (e.g., p. 5). There are enough half-truths in this presentation to entice an unwary reader into believing that it is the whole picture, but it is not.

In reality, Boas made significant contributions to physical anthropology which were recognized by his contemporaries as well as later anthropologists (Boas' pioneering; work in biological anthropology is still being cited; see, for example, Frisch 1975; Johnston, Borden, and MacVan 1975). Hrdlicka (1919:102), a proponent of eugenics and hardly free of racism himself, "regretted" that Boas did not devote all of his time to physical anthropology and wrote that "the published contributions of Professor Boas to physical anthropology are both *numerous as well as important*. They cover a wide range and in general are characterized by a distinct leaning towards a mathematical rather than anatomical treatment of the subject matter" (1919:102; emphasis added). (Others have commented on Boas' statistical contributions to biological anthropology; see, for example, Harris 1968:317.) There follows a list 'of publications in physical anthropology by Boas that Hrdlicka himself thought significant and important, and although the list ends in 1916, it numbers sixty-five entries (102-105).

Krogman, an eminent physical anthropologist, also recognized Boas' significant contributions to biological anthropology, especially in his own field of human growth and development:

> his early work in growth and development was not only well known to me, but it was inherent in Todd's growth-research design as well. Todd felt that Boas's work in the field of growth was incisively innovative. I have never surrendered my opinion that Boas was the "compleat anthropologist". . . . (1976:6)

Krogman summarizes Boas' contributions to physical anthropology by saying that Boas "stresses three major themes: 1. the nature of the physical and behavioral differences between people (races); 2. the physical growth and development of the child; 3. the role of biometrics in areas 1 and 2" (7). Krogman goes on to list Boas as one of the "giants" (11).[7]

Before looking at Boas himself, it is beneficial to examine a contemporary theorist for his perspective on Boas. Marvin Harris, a strong proponent of cultural materialism, is not in accord with any kind of absolute cultural determinism of the kind Freeman attributes to Boas, and he is critical of Boas because Boas never constructed a coherent theoretical framework, was too particularistic, and did not adopt a materialistic approach. However, Harris' interpretation of Boas differs radically from Freeman's. First, he recognizes "Boas' renaissancelike involvement with all four fields of anthropology . . ." (1968:255),[8] and gives Boas substantial credit for having higher scientific standards than his contemporaries (253ff.). Harris firmly maintains that Boas was not an absolute cultural determinist but an ardent advocate of including all potential and possible explanatory factors. Thus he states that "Boas systematically rejected almost every conceivable form of cultural determinism" (283) and refers to Boas' rejection of all monocausal determinisms (284). Harris criticizes Boas because he "rejected *all* coherent (i.e., noneclectic) explanations of sociocultural differences that made any appeal to any deterministic principle whatsoever, with rather marked indifference to whether they were inspired by materialist, idealist, or theistic doctrines" (296; emphasis in original). Harris is quite specific about Boas and evolution: "Boas did not reject evolutionism in any degree whatsoever. What he rejected was (1) biological reductionism; (2) cultural parallelism; and (3) universal standards of progress" (295). Harris certainly does not agree that Boas was an absolute cultural determinist, and yet that is Freeman's main point. Freeman cites Harris' book in other contexts but never mentions these ideas; his interpretation of Boas is clearly at odds with that of Hrdlicka, Krogman, and Harris. In order to evaluate which interpretation of Boas is more appropriate, we must now turn to the words of Boas himself.[9]

As mentioned earlier., Freeman does give Boas credit for initially calling for cooperation between biology and anthropology, but claims that Boas later changed in response to the extremes of the eugenicists:

> in December 1907, Boas had given it as his view that a separation of anthropological methods from the methods of biology and psychology was impossible, and then gone on to express the hope that "the safe methods of biological and psychological anthropometry and anthropology" would help to remove the problems of "race-mixture" and eugenics from heated political discussion and make them subjects of calm scientific investigation. By 1916, however, his attitude had decisively changed. During the intervening years the eugenics movement had effloresced into a

> pseudo-scientific cult, and Boas had come to see both eugenics and the racial interpretation of history as irremediably dangerous. The extreme doctrines of the hereditarians, Boas pointed out, had set anthropologists and biologists at odds, and so much so that a "parting of the ways" had been reached. (5)

It is important to note here that Boas' reaction was to the extreme deterministic position and racist implications of the eugenics movement, not to biology in general. Freeman continues:

> These were portentous words. Within the space of a few months, two of the most able and active of Boas' former students, Alfred Kroeber and Robert Lowie, had published intellectual manifestos that conceptually dissociated cultural anthropology from biology. Their solution was the propounding of a doctrine of absolute cultural determinism that totally excluded biological variables. (6)

Apart from the fact that it was Kroeber and Lowie to whom Freeman refers here, his later argument rests on the assertion that this conversion by Boas to absolute cultural determinism was extreme and profound. However, there are significant problems with Freeman's interpretation: (1) he fails to distinguish among several central concepts, such as biological determinism, biological influences, individual innate characteristics, racism, and evolution, despite the fact that the differences among these are not very subtle; (2) he ignores what Boas meant by environment; (3) he exaggerates Boas' position and bolsters his extreme interpretation with quotes wildly out of context.

That Boas was an ardent opponent of biological determinism and racism (two central elements in the eugenics stance), no one can doubt. But Freeman seems to believe that this is the same thing as opposing the inclusion of all biological factors, and it is not. Boas was consistent throughout his life in calling for scientific research on the role of both biological factors and cultural influences, and the interaction between the two; it was his attack on biological determinism and racism that intensified. That Boas was not just paying lip service to an ideal is evident from his own research in physical anthropology. In his classic study, "Changes in Bodily Form of Descendants of Immigrants" (1940:60-75), first published in 1912,[10] Boas documented physical changes within a generation or two and rightly concluded that hereditary factors played little role. However, he did not advance the notion that explaining these changes was a cultural

problem: “These observations, however, merely set us a biological problem that can be solved only by biological methods. No statistics will tell us what may be the disturbing elements in intra-uterine or later growth that result in changes of form” (1940:70). He underscored the biological nature of the problem again in a follow-up article published in 1916 (1940:76-81). In 1913, he published a paper entitled “Remarks on the Anthropological Study of Children” (1940:94-102) in which he compared the development of racial traits among children and concluded that “here we have undoubtedly traits that are determined by a long line of ancestors, not by environment” (102).

Admittedly, these studies are all before the supposed conversion to absolute cultural determinism took place, but they are not uncharacteristic of Boas’ later work. Two articles first published in 1935 provide conclusive evidence of Boas’ eminently reasonable position and open mind. The first, “Conditions Controlling the Tempo of Development and Decay” (1940:89-93) investigated factors affecting the human life span. Boas argued here that *both* heredity and environment were important factors. He cautions that “the importance of hereditary determinants may not be neglected” (92) and concludes that “each individual has by heredity a certain tempo of development that may be modified by outer conditions” (93). (What these “outer conditions” are concerns his conception of environment, described below.) Finally, in a classic study still cited (see, for example, Frisch 1975) on “The Tempo of Growth of Fraternities” (1940:86-88), also first published in 1935, Boas investigated the rate of growth of children and. concluded, “since the conditions under which these children live are unusually uniform, we may conclude that proof for the heredity of the tempo of growth has been given” (87). Thus based on his own research Boas concluded that the rate of growth was determined by heredity, not environment. Although Boas did not look to biology for an explanation of cultural and historical processes, Freeman’s assertion that Boas was not “disposed to explore, in a constructive way, the coexistence and interaction of genetic and exogenetic processes” (32) is simply wrong.

Freeman ignores these contributions of Boas. Instead, he takes Boas’ opposition to racism and biological determinism as evidence that Boas was opposed to all consideration of biological or hereditary factors, and even incorporates evolution. Freeman is often tricky, discussing Boas’ antipathy to biological determinism or reductionism in one sentence but sliding into his total opposition to biology in the next. Sometimes this jump is obvious. For example:

> the theories of the doctrine of cultural determinism were (in Melford Spiro's words) "developed in the first instance as alternatives to and refutations of *biological determinism*." *Thus* the Boasians had an antipathy to *biology*, and to *genetics* and *evolutionary biology* in particular. (295; emphasis added)

In other instances, however, the leap between opposition to biological determinism and a denial of biology altogether is more subtle and deceptive. A longer example is warranted here because Freeman relies on this kind of specious reasoning frequently. On page 32, he presents Boas' views fairly accurately:

> Boas went on to examine the assumption that "racial descent determines cultural life" and to conclude that not the slightest successful attempt had been made "to establish causes for the behavior of a people other than historical and social conditions." An unbiased review of the facts, he asserted, showed that "belief in hereditary racial characteristics and the jealous care for purity of race is based on the assumption of non-existing conditions." . . . The whole thrust of Boas' thought, as Stocking has observed, was "to distinguish the concepts of race and culture, to separate biological and cultural heredity, to focus attention on cultural process, to free the concept of culture from its heritage of evolutionary and racial assumption, so that it could subsequently become . . . completely independent of biological determinism. (32; first ellipsis added; second in original)

Note here that Stocking says nothing about denying biology, only biological determinism and racism.[11] Freeman goes on in the next paragraph to describe briefly the rise and basic tenets of the eugenicists (e.g., Davenport) and the "hereditarian" cause, concluding

> there, then, in 1911 [not 1916], were two antithetical intellectual and scientific schools--that of Boas and that of Davenport--with neither disposed to explore, in a constructive way, the coexistence and interaction of genetic and exogenetic processes. (32)

The leap between his (and Stocking's) description of Boas and this conclusion is simply unwarranted. [12]

Boas and his students did deny biological determinism and argue against racism, but Freeman fails to understand that in doing so they were

not denying hereditary influences nor did they reject innate individual differences. In fact, both Mead (1963:292) and Benedict (Mead 1972:195) suggested that innate individual characteristics could partially explain deviance from cultural norms. [13] Kroeber wrote that

> often they [biology and culture] are even intertwined in one and the same phenomenon, as when a person is born with hereditary musical capacity and develops this further by study and training. They are not always easy to disentangle; but they must be separated if the processes at work are to be understood. (1948:2)

Neither Boas nor his students completely denied that individuals were affected by heredity. They argued that biological determinism could not explain sociocultural and historical processes.[14]

An appreciation of what Boas meant by environment is important in understanding his work, especially in relation to Freeman's claim that Boas believed that "environment has an important effect upon the anatomical structure and physiological functions of man" (28). That Boas was naive in today's terms about evolution, genetics, and Lamarckianism is true, but in failing to describe Boas' inclusive concept of environment, Freeman warps Boas' position out of proportion. For Boas, environment was not just social or cultural but also had physical, geographical, and biological aspects. As the environmental conditions that affect growth and physical form and function, Boas lists malnutrition and pathology and disease (1940:36) and "habitual uses to which groups of muscles are put" (1940:370). He includes the prenatal environment as well: "constitutional changes in the body of the mother may bring about modifications in prenatal growth. . ." (1940:37). In his article on the life span, he elaborates:

> even here certain allowances have to be made, for we may distinguish between an hereditary, purely biologically determined element and another one that depends upon conditions of life. Ample or deficient nutrition, more or less exhausting daily labor, abuse of the body, greater or lesser nervous strains are elements that modify the life span as it may be determined by heredity. Even geographical conditions may have their influence. (1940:89)

This is a far cry from the position of absolute cultural determinism Freeman claims for Boas in regard to the effects of environment over heredity, and although in some ways Boas was behind his time in relation to biology, it is clear that in others he was ahead.

Freeman's picture of Boas is distorted despite the fact that it contains partial truths. One of the most flawed aspects of his whole presentation is the ease with which he takes quotes out of context and presents them as complete and unambiguous truths, with no concern for even the most blatant misrepresentations. In one astounding case, Freeman takes Boas' words and uses them in almost complete opposition to their original meaning. This travesty of scholarship occurs on page 295 where Freeman says:

> Boas, for example, was opposed to research in human genetics and thought, even as late as 1939, that in respect of the human body, "a search for genes would not be advisable," there being some danger that the number of genes would "depend rather upon the number of investigators than upon their actual existence."

This is indeed a remarkable passage. First, Boas never opposed true scientific research on human heredity. The quotes from Boas do not come from the 1939 article mentioned but incredibly from the classic article in which Boas asserted that the tempo of growth was determined by heredity. What Boas argued here was not that genetic factors were irrelevant (in fact, his whole point was just the opposite) but that the whole phenomenon was very complicated and not likely to be governed by any single or simple set of genetic factors. Here is what Boas really said, and a comparison of his words with Freeman's distortion well illustrates my point about selective and decontextualized quotes:

> It is obvious that a phenomenon of such complexity as length of body and tempo of development must be governed by many hereditary factors and that we are dealing with a phenomenon of general organization of the body and that a search for genes would not be advisable. Is not there some danger anyway, that the number of genes will depend rather upon the number of investigators than upon their actual existence? (Boas 1940:88; originally published 1935)

What Freeman did here is so obvious that further comment is unnecessary. It is important to stress that this kind of distortion is common in the book, that this example is not an isolated one.[15]

Although space does not permit more extensive discussion, it is necessary to mention that those scholars Freeman labels "the Boasians" (Kroe-

ber, Lowie, Benedict, Mead) receive the same treatment in his hands. He never considers the complexities of their thought, giving only one-sided pictures, and presenting their ideas out of context. He lumps them together and seems to imply that they (and Boas) were very much alike in all ways (e.g., if Kroeber said something, it must have come from Boas). Again, there are enough partial truths to entice an unwary reader into believing in the veracity of the whole picture.[16] Freeman fails to stress that they too were reacting to eugenics and racism and trying to provide an alternative theoretical explanation for behavioral differences between human populations.

On Scholarship

Before turning to the real heart of this book, the attack on Mead, I want to pause to discuss its scholarly style. The book's citation form is unprofessional and surely has caused faces to redden among editors at Harvard Univerity Press (and if it hasn't, it should have). There is no bibliography or list of references cited. Nowhere can one learn the name of the publisher of a book cited. Inclusive page numbers are not given for articles in journals or chapters in books. Sometimes the specific page number for a quote is not precisely indicated at all, the note merely saying that material was taken from some page and "ff."[17] When Freeman uses the same quotation more than once, especially phrases, he frequently does not repeat the reference; this is acceptable as long as they are in close proximity but unacceptable when they are separated by as much as one or two entire chapters.[18] Works mentioned by name or author in the text are sometimes not cited in the note at all (and readers who flip between text and notes will be surprised at how often he mentions *Corning of Age in Samoa* but cites some other work).[19] Much that should be footnoted is not.[20] Freeman frequently deletes words and phrases from direct quotations without indicating to the reader with ellipses that he has done so.[21] He walks across the line between adequate citation or paraphrasing and appropriating the words of others as his own.[22] His notes sometimes contain references to specific pages that are not relevant to the text or do not contain the quotes used.[23] In sum, he violates almost every canon of good scholarship.

Most frustrating is that in almost all cases Freeman provides a single note listing all of the references pertaining to an entire paragraph. Thus although sometimes it is clear where he is getting his material, it frequently is not. He strings together several quotes from a variety of sources (not only in the same paragraph but also in the same sentence) and pro-

vides a single note which can include as many as a dozen or more different sources; and these references are not necessarily in the order relevant to their use in the text. It is frequently impossible for a reader to be certain what came from where, or who said what, even after looking up the note. Here is one typical example that illustrates the problem well:

> elsewhere in her writings, Mead elaborates this picture of the background that, for Samoans, "makes growing up so easy," the leitmotif of her depiction being the notion of *ease.* Samoan life, she claims, is above all else "characterized by ease"; Samoan society is "replete with easy solution for all conflicts." She remarks, for example, on "the ease with which personality differences can be adjusted by change of residence," on "the easy acceptance of innovation," and on a prevailing "ease in sex relations." Adolescence is "the age of maximum ease," and Samoans develop into "easy, balanced human beings" in a society that "emphasizes a graceful, easy, diffuse emotional life, a relaxed dependence upon reliable social forms." (84; emphasis in original)

Putting these quotes together to make a point is certainly acceptable, but lumping all of the sources together is not. Note that there are nine quotations in this paragraph, and a reader who does not turn to the single note will never know that they come from five different 'sources (published, of course, at different times). The note cites seven different specific page numbers, so one must assume that some (which?) of these quotes occur together. The most significant thing here is that even after referring to the note, the reader still has absolutely no way of knowing which passage was taken from which source.[24]

I chose the above example because it is relatively straightforward (and short) and clearly shows the inherent difficulties of the style used.[25] These quotes were at least all from Mead; however, the style also lends itself to misuse in Freeman's hands because it allows him to obscure the author of particular quotes. For example, on page 20 there is a paragraph about Boas and the "relationship between culture and biology." The note pertaining to the paragraph (which appears on page 308) lists only two sources, one by Boas and a secondary source by Spier. There is one quotation in the paragraph: is it from Boas or Spier? It is impossible for the reader to know.

One of the worst aspects of this style is that it does not allow the reader to differentiate what Freeman says from what his sources say. For ex-

ample, the paragraph that begins on page 99 and continues on page 100 has only one source cited--Stocking (1968). It begins,

> as George Stocking has shown, "the working out of all the anti-biological [sic] tendencies in behavioral science and the complete dissemination of Boasian thinking were not accomplished until after 1930." In this working out, such as it was, Mead's assertion of the absolute sovereignty of culture, in answer to the problem that Boas had sent her to Samoa to investigate, was of quite pivotal importance. (99)

Freeman's paragraph goes on about the significance of Mead's research, and one might think that: it was Stocking who argued that Mead's Samoan research was "of quite pivotal importance." In fact, Stocking suggests no such thing; nowhere in the book does he discuss Mead's importance. Stocking's next sentence even denies Freeman's point about the significance of Mead's work, since most of the separation between the social and biological sciences had occurred before Mead even went to Samoa.

> In short, the working out of all the antibiological tendencies in behavioral science and the complete dissemination of Boasian thinking were not accomplished until after 1930. Nevertheless, as Kroeber implied in retrospect, the emancipation of the social from the biological sciences, in principle if not in all areas of practice, had been accomplished by 1917. (Stocking 1968; 267)

A reader who does not go beyond the note to the original source here might easily conclude that Stocking implied or said things which he simply did not.[26]

Although most of the notes refer to an entire paragraph, some appear internally within a paragraph. A few of these contain additional information (not references) and thus seem to make sense. However, Freeman is not consistent: others contain only references, and often additional information is merely included in the encompassing paragraph note. This is an unnecessary source of added confusion.

In the above discussion of Freeman's treatment of Boas, I mentioned that he took quotes out ad context, using them without regard for their intended meaning. This kind of scholarship, aided by the ambiguous citation style, characterizes the entire book. Some of the cases are serious misrepresentations while others involve subtle changes of tone and tenor.[27] One of the more deceptive ones occurs on page 282 where Freeman writes:

> it was of this attitude of mind [cultural determinism] that Mead became a leading proponent, with (as Marvin Harris has observed) her anthropological mission, set for her by Boas, being to defeat the notion of a "panhuman hereditary human nature." She pursued this objective by tirelessly stressing, in publication after publication, "the absence of maturational regularities."

The first thing to note here is that both quotations come from Harris; Freeman never cites Mead herself (much less in "publication after publication") as saying that there are no "maturational regularities." These are the words of Harris writing about Mead. It is also critical to note the context in which Harris was writing: Mead's relationship to orthodox Freudianism.[28] After outlining Freud's basic "biopsychological" approach, Harris goes on to say that

> everything in Mead's approach weighed against such compulsory psychic freight. It was her mission, set for her by Boas, to defeat the notion of a narrowly fixed racial or panhuman hereditary human nature. And it was for this reason that she tirelessly stressed the absence of maturational regularities: adolescence is not always a time of stress; children are not necessarily more imaginative than adults; women are not necessarily more passive than men. (Harris 1968:427)

(Note here again that Freeman uses Harris' words without including them as quotations.)

Far more examples of the unacceptable scholarship in this book could be provided (more are included below), but surely these are sufficient to induce readers to treat the work with caution. Freeman's treatment of the history of anthropology, especially Boas and the culture-biology issue, is riddled with half-truths and distorted arguments and should not be taken seriously.

Freeman, Mead, and Samoa

It is neither my intention nor my role to defend Mead against Freeman's attack. There is no doubt that she made some mistakes, especially in interpreting the data she recorded.[29] But Freeman's book is a profound disappointment because anthropology needs professional, unprejudiced, and critical analyses of Mead and her contemporaries, things this book does not provide.[30] Mead was not 100 percent right but neither, as Free-

man would have us believe, was she 100 percent wrong. We get only a one-sided view from Freeman, a view that does not do justice to Mead, to Samoa, and ironically enough to Freeman's own work. That his data on Samoa do not totally agree with hers is not simply a case of Freeman being right (because he considers biological influences or whatever) and Mead being wrong (because she is a cultural determinist or whatever). Specifically, in this section I will discuss five major points: (1) there are significant questions about the comparability of the data, questions that Freeman glosses over; (2) there are serious flaws in his own methodology and reasoning; (3) he does to Mead what he did to Boas, that is, quotes her out of context, ignores evidence that contradicts his position, and exaggerates; (4) he fails to see that Samoans are neither precisely as Mead portrayed them nor exactly as he pictures them, but both; (5) in those instances where Mead was wrong, he suggests simple and naive reasons for her errors, whereas in reality the causes were more profound (and have little if anything to do with a dichotomy between culture and biology).

Mead studied a village in American Samoa in 1925-1926 while Freeman did research in Western Samoa beginning in the 1940s.[31] One cannot help but wonder about the comparability of data gathered from different places at different times That Western Samoa and American Samoa have had different colonial histories is obvious, and surely there was at least some sociocultural change between the 1920s and the 1940s. Freeman is not troubled by this issue. Informants assured him (xiv) that things were the same in both places, not only where he was there but in the 1920s as well: "As one of my informants remarked [in 1967], the happenings of the mid 1920s were still fresh in their memories" (xv). "Fresh" memories after more than forty years are cause for skepticism, but Freeman has none. However he does cite other sources for the cultural and historical uniformity throughout Samoa.

> As George Turner notes [writing in 1861 (Freeman 327)], the Samoans have but one dialect and have long been in free communication from island to island; in Bradd Shore's words, "culturally and linguistically, the entire Samoan archipelago reveals a remarkably unified identity and striking homogeneity" (117).

He continues on the same page with a statement from a Samoan chief to a United States Congressional Investigation Commission in 1930, a statement that stressed the uniformity.[32] But he does not consider that linguistic and cultural identity do not necessarily mean complete social unifor-

mity. Shore (1983:3) indeed says that Samoa is characterized by uniformity, but he also underscores the difference between cultural uniformity and social diversity:

> the nature of this homogeneity and the variations within it is an important issue in understanding Samoa. By cultural homogeneity, I refer to a shared commitment to a large number of political and kinship institutions, a common consciousness among Samoans of . . . a set of understandings and categories which serve as common premises for interpreting and orienting behavior. This sort of homogeneity does not preclude wide divergences in specific practices and beliefs between villages, members of different descent groups, and different individuals, or within the same individual on two different occasions. (1983:303)

(See also Shore 1983:128.) Furthermore, in Shore's excellent analysis the fact that the community he studied possessed an unusual title configuration for Samoa had significant explanatory power. He also notes that "different degrees of colonization" (3) had occurred and that Manu'a (where Mead worked) historically had an important degree of political autonomy from the rest of Samoa (4). Moreover, Freeman himself cites a source that contradicts his point--Manu'ans were different from the rest of Samoa in the fierceness of their warfare (Freeman 169). In sum, although Samoa does exhibit cultural and linguistic uniformity, Freeman does not adequately establish that his data are completely comparable to those of Mead; there is ample room for doubt and he should have addressed the issue more carefully.

Freeman's own reasoning and methodology are not always rigorous and precise. In his fervor he fails to make significant analytical distinctions (just as he did when he neglected to note the differences between biological determinism, racism, evolution, and a denial of the relevance of biology altogether). His analysis contains subtle shifts in which he begins discussing one thing and finishes with a conclusion about something else. The following passage, which contains an implicit and unwarranted assumption that adolescent stress and delinquency are somehow the same, aptly illustrates this problem.

> Is it in fact true, as Mead claimed, that the behavior of Samoan adolescents is untroubled and unstressed and lacks the conflicts that are so often characteristic of this period of development? As Herant Katchadourian notes, "research on ordinary adolescents

> has generally failed to substantiate claims of the inevitability and universality of adolescent stress." Nonetheless, the findings . . . [of several investigators] have clearly shown that the years of adolescence are hazardous for many, with delinquency in the United States and elsewhere reaching a peak at about age 16. To what extent, then, is adolescent delinquency present in Samoa? (255)

(Note here Freeman's flagrant disregard for the conclusion of his own sources.)

The problem of distinguishing between statements of cultural ideal and the reality of human behavior is also prevalent in this book. Freeman cites rules and verbalized ideals as evidence that Mead's descriptions of behavior were totally in error (e.g., p. 188). He is especially prone to citing the moral lessons of myth and legend to substantiate his points without providing behavioral evidence (e.g., pp. 182-83, 191-92, 235). Surely the difference between the ideal and the actuality has at least some relevance to the issue of premarital sex in traditional Samoa. Mead reported that it was common and expected, but Freeman offers evidence for the reverse. He seems to be correct that it was not ideal and approved behavior, but that does not necessarily mean that it did not occur, a possibility he never seriously considers. In fact, despite her interpretation, Mead herself provides some clues that this might be the case. She described love affairs as "hazardous adventures" (1961:61), "clandestine" (77), and "strictly *sub rosa*" (80). She also wrote that conditions were very different in traditional Samoa: "aboriginal Samoa was harder on the girl sex delinquent than is present-day Samoa" (199).

> Deviations from chastity were formerly punished in the case of girls by a very severe beating and a stigmatising shaving of the head. Missionaries have discouraged the beating and head shaving, but failed to substitute as forceful an inducement to circumspect conduct. The girl whose sex activities are frowned upon by her family is in a far better position than that of her great-grandmother. (Mead 1961:200)[33]

Freeman himself cites a source that claims that "virginity was 'a social asset rather than a moral virtue' " (230), and notes that although virginity was valued by all, it was a value "especially characteristic of the higher levels of the rank structure. . ." (236). He also says that "in many cases the defloration that precedes an avaga [elopement] is the culmination of a seduction that the girl herself has actively encouraged" (240). Neither does

Freeman recognize that his emphasis on a Samoan "cult of virginity"[34] (e.g., pp. 234, 239) and a "puritanical Christian sexual morality" (239) is not completely and wholly consistent with his assertion that "while in all the Samoan communities I have studied a few girls remained virgins until they married in a religious ceremony, most of them lost the status of virgin by eloping from their families with the man who succeeded in deflowering them" (240).

Freeman's own methodology is frequently questionable. He presents a table in which he claims to report the incidence of virginity among adolescent girls in one village (239). Ages ranged from fourteen (100 percent virgins in a population of four) to nineteen (40 percent virgins in a population of five), with a total of thirty of forty-one girls (73 percent) who were virgins. He never quite tells us how he gathered these data, however. In the text he says that "we collected information on whether these girls and young women were virgins and whether they were members of the Ekalesia [communicants in the church]" (239). In the note that pertains to this paragraph, it is suggested that membership in the Ekalesia (which theoretically required that unmarried girls be virgins) was his prime determining factor: "Membership in the Ekalesia by an unmarried adolescent girl is based on acceptance by other members, who exercise a very strict surveillance in this matter, that she is a virgin. The classing of a girl as a virgin is based on this and all other available relevant evidence" (350). He never specifies what "all other available relevant evidence" might be. In a matter as sensitive as virginity in contemporary Samoa, these data are clearly unreliable and Freeman is revealed as very naive. On page 290 he notes that "because of their strict morality, Samoans show a decided reluctance to discuss sexual matters with outsiders or those in authority, a reticence that is especially marked among female adolescents:" He goes on to question how Mead could have gotten her data, given this reticence, but never really answers the question of how he himself overcame it and acquired data on virginity.[35] After presenting the table, he goes on in the next paragraph (239) to calculate age at first conception, as if to suggest that these data corroborate his material on virginity. However, if Freeman really knew his biology, he would surely have mentioned that the phenomenon of adolescent infertility might have some relevance here.

In an appendix to *Coming of Age in Samoa* (1961:190) Mead reminds her readers that the numerical data she gathered were not suitable for quantitative analysis;[36] but Freeman has no such qualms about his or her data, and the result in one case is absurd. He correctly reports (237-38) that in Mead's sample of twenty-five adolescent girls, thirteen were vir-

gins. He concludes by saying that "this situation . . . is obviously incongruent with her generalizations about Samoan female adolescents: more than half of the adolescent girls about whom she wrote in *Coming of Age in Samoa* were in fact virgins. . ." (238). He is right--thirteen out of twenty-five is certainly "more than half"; but it is also true that almost half (twelve out of twenty-five) were not virgins. One could easily ignore Mead's warning about performing quantitative operations on these data and do all sorts of interesting things with them. For example, of the eleven girls in the sample (Mead 1961:209) who were three or more years past puberty, eight of them (a full 72 percent) were not virgins.

This is not the end of Freeman's statistical shenanigans with Mead's data, for from an even smaller corpus of material (still based on these twenty-five adolescent girls) he extracts rates of juvenile delinquency. He analyzes the qualitative data Mead presents on deviance and goes on to suggest that "if we assume, conservatively, on the basis of Mead's reports, that among the twenty-five adolescents she studied there was *one* delinquent act per annum, this is equivalent to a rate of forty such acts per thousand" (257; emphasis in original). Then he ignores the definitional differences by which the two sets of data were generated and compares this rate with juvenile delinquency rates in England and Wales. He concludes that delinquency among Samoan girls in 1928 was "about ten times higher than that which existed among female adolescents in England and Wales in 1965" (258). He recognizes that "this comparison is only approximate" (258) but nevertheless immediately asserts that "it does, however, indicate that among the girls studied by Mead in 1925-6 delinquency was in fact at quite a high level" (258). And in, the very same paragraph, Freeman has the hubris to criticize *Mead* for making what he claims was a "decidedly unscientific maneuver"!

Freeman himself presents little reliable quantitative data. There are some data about juvenile offenses drawn from his own observations and Western Samoan police: records (see pp. 259-68), but he never adequately describes his research design or methodology (especially his "samples") and thus his study is not replicable (and violates his own standards for scientific research).[37] After reading all of the prepublication hoopla in the press, I expected statistical or at least quantitative data from those High Court Records of American Samoa, to which he was supposedly denied access until 1981. It was said, for example, in the New York Times that

> although he published a number of papers about Samoa on technical subjects, Professor Freeman said, not until 1981 was he fi-

> nally granted access to the archives of the High Court of American Samoa. "I had tried in the 1960s but was refused, and when I was finally allowed in, the evidence was conclusive," he said, referring to the statistics about rape, assault and other crimes that appear in both the text and in the book's 55 pages of notes. (E. McDowell 1983: C21)

Those High Court records loomed large in why he waited so many years to publish his account, why he published almost five years after Mead's death (xvi). As I began to read the book my expectations were high that he would present documented statistical evidence of violence, rape, murder, and so on in American Samoa in the 1920s, evidence he waited years to acquire. It is not there. In fact, there are only four references in the entire book to data from these court records, all tangential and none quantitative.[38] It is certainly conceivable that he waited to get access to these records, thinking that they would be more useful than they turned out to be. But to suggest that they warranted the wait or provided "conclusive" evidence for anything is nonsense.[39]

In his analysis of rape (which he argues was common as opposed to Mead's stance that it was almost unknown), Freeman again fails to make significant definitional distinctions before comparing data and violates good scientific methods. He presents some very dubious statistics for rape in Western Samoa[40] and then. compares them with data from other countries (e.g., 248). He does not consider that what he defines as rape is not the same as the definition of rape used to generate, for example, the U.S. statistics. The Samoan concept of rape is simply not the same as the American legal definition and the data are not comparable.[41] Again, in disputing Mead's portrait he takes what might have been a relevant point to extremes, and even asserts that "both surreptitious and forcible rape have long been intrinsic to the sexual mores of Samoan men and are major elements in their sexual behavior" (249-50) and describes rape as a "recognized social practice" (352). It is interesting to note that Shore describes rape as thought to be: "evil" (1983:112) and classified along with murder as a serious offense by Samoans (115).

I do not intend to belabor Freeman's now-familiar habit of quoting others out of context, but this practice very much affects the adequacy of his portrayal of Mead's work and some examples are required to illustrate its seriousness. On page 74 he discusses the basic approach of Benedict and correctly points out the similarities between Mead and Benedict. He includes some phrases Mead used to describe Samoans, then continues: "these descriptions could well have been applied by Benedict to the Zuni,

and indeed Mead, on a later occasion, specifically noted that in both Zuni and Samoa it was 'the individual endowed with a capacity to feel strongly' who was 'maladjusted' " (Freeman 74). This quote comes not from a major source on Samoa but from *Sex and Temperament in Three Primitive Societies* (1963; originally published in 1935) and is very much out of context. Mead is discussing deviants and argues against the then-current psychiatric lumping of what she perceived to be two different kinds of deviants. She continues in what is surely an odd vein for an absolute cultural determinist.

> Modern psychiatric thought tends to attribute all of his [he who deviates from cultural norms] maladjustment to early conditioning and so places him in the invidious category of the psychically maimed. A study of primitive conditions does not bear out such a simple explanation. It does not account for the fact that it is always those individuals who show marked temperamental proclivities in opposition to the cultural emphases who are in each society the maladjusted person. . . . It does not explain why . . . it is the individual endowed with a capacity to feel strongly who is maladjusted in Zuni and Samoa. Such material suggests that there is another type of unadjusted person, whose failure to adjust should be referred not to his own weakness and defect, not to accident or to disease, but to a fundamental discrepancy between his *innate* disposition and his society's standards. (Mead 1963:291-92; emphasis added)

Freeman might be excused in this example because he was trying to make a different point (although how he could ignore an absolute cultural determinist attributing deviation to innate differences is difficult to understand). But examples exist as well which indicate that he could not have been other than conscious that he was changing the meaning to suit himself. I offer an example here without comment (it is so blatant that it needs little explanation) but will refer to it in later discussion. One of Freeman's main points is that in characterizing the Samoans as gentle, easy, unassertive, etc., Mead failed to note that in actuality Samoans are assertive, competitive, violent, and so forth. On page 88, he says:

> just as Samoan culture has eliminated strong emotion, so also it has eliminated any interest in competition. Samoan social organization, claims Mead, places "each individual, each household, each village, even (in Western Samoa) each district, in a hier-

> archy, wherein each is dignified only by its relationship to the whole," each performing tasks that "contribute to the honor and well-being of the whole," so that "competition is completely impossible."

Here is what Mead really says:

> these illustrations will show the *two tendencies* in Samoan social organizations, the tendency to place each individual, each household, each village, even (in Western Samoa) each district in a hierarchy, wherein each is dignified only by its relationship to the whole, each performs tasks which contribute to the honor and well-being of the whole, and competition is completely impossible. *The opposite tendency, the rebellion of individuals within the units against this subordination to a plan and their use of a place in a component unit to foment trouble and rivalry with other units, while not so strong, is always present.* (Mead 1976a:301-2; emphasis added)[42]

These examples are not isolated.[43] I provide just one more because it bears directly on Mead's depiction of Samoan adolescence. On pages 93-94, Freeman writes that

> adolescence in Samoa, according to Mead, is thus "peculiarly free of all those characteristics which make it a period dreaded by adults and perilous for young people in more complex--and often also, in more primitive societies." What is the most difficult age in American society becomes in Samoa the age of maximum ease, "perhaps the pleasantest time the Samoan girl will ever know."

The first sentence in this passage is essentially correct, although Freeman does not indicate to the reader that he deleted the first part of the quote and neglects to insert a dash between "primitive" and "societies." The second sentence comes from two sources, *Cooperation and Competition among Primitive Peoples* (Mead 1976a; first published in 1937) and another article by Mead. I have no quarrel with the quote in this second sentence, presumably from the article, but Freeman does not present Mead's more complex notion from *Cooperation and Competition* (and again note the way in which he appropriates her words).

> In understanding the role which age plays in the life of a Samoan child, it is necessary to bear in mind the large households in

> which . . . no child for long has a fixed status as the oldest, the only child, or the youngest. . . . The pressure of the children whose births soon after his own rapidly rob him of the position of youngest, push him slowly upward in the relative scale, until at adolescence a girl or boy is near the center of pressure, with as many younger people who can be ordered about and disciplined as there are older people who can order him about and discipline him. What is the most difficult age in our society becomes in Samoa, *because of this point of relativity,* the age of maximum ease partly because it is the age of most equal pressure. (Mead 1976a:308; emphasis added)

Freeman does not adequately differentiate between Mead's own data and her interpretations of that data; and because she did overgeneralize, especially in later work, there is a significant difference between the two. What made Mead a good ethnographer was her capacity to see and record what was happening around her (and although they sometimes missed the mark, her intuitive leaps were often insightful and sometimes brilliant). The important point is that Mead recorded and published data that did not fit precisely with her interpretation--but the data are there. even for Freeman to use in support of his own interpretation.[44] Although he could almost evade quoting Mead about the "two opposing tendencies," he frequently cites her work as evidence for his own position.[45] One need only read carefully either of her two books on Samoa to find evidence of conflict, hostility, competition, the centrality of ranking, etc.; after all, she did report and support two opposing tendencies even if her interpretation stressed one more than the other.

Freeman's main point in this book is that Mead got her Samoan ethnographic facts all wrong, that Samoan society and Samoans were not and are not as she depicted them. There is no question that Mead did overgeneralize and neglect some aspects although she did consistently mention contradictions and did not ignore or suppress all evidence contrary to her interpretation. In his fervent attack and frequent claims that Mead was all wrong, Freeman goes too far and neglects his own evidence that Samoans in fact seem to be both as Mead and Freeman describe them. I have already cited Mead's comment about the "two opposing tendencies" in Samoa. The point I wish to make here is that Freeman is forced to recognize that Mead was right, there are these two aspects even if he too believes one to be more important than the other. The following passages from Freeman all substantiate Mead. (1) "However, while this system of punctilious social intercourse operates effectively most of the time, it does

on occasion fail to prevent the tensions generated by the Samoan rank system from breaking out into violent conflict" (123); (2) "the elaborate conventions of the rank system are usually sufficient to contain its tensions" (137); and (3) because of this system of child rearing and the stringent demands that those in authority make upon the growing individual, Samoan character . . . has two marked sides to it, with an outer affability and respectfulness masking an inner susceptibility to choler and violence" (276).[46] By Freeman's own admission, then, Samoans are indeed both. He can quarrel with particular ethnographic facts or argue that Mead's emphasis was incorrect, but he cannot legitimately claim that she was all wrong. That both Mead and Freeman are both right and wrong, that Samoans are a wonderfully complex mixture of both, is one of the main points Shore makes in his recently published and superb book, *Sala'ilua: A Samoan Mystery* (1983). Shore offers a dispassionate and critical evaluation of Mead's Samoan research in the context of his ethnographic analysis, giving credit for her insights and criticizing her errors. What is most relevant here is his very good discussion of the duality in Samoan character (especially pp. 150-53). One comment is germane here:

> While some observers appear to have viewed Samoan personality with one eye shut, observing half truths, whether polite passivity without aggression or an aggressiveness lacking reserve and control, other observers have noted the strongly contradictory tendencies of Samoan personality. (152)

Readers who want to learn about Samoans should read Shore's book, not Freeman's.

The final section of Freeman's discussion of Mead is a consideration of why she made the errors he: thinks she did. Although I do not agree with him that Mead was as wrong as he maintains, it is a legitimate question.[47] Some errors, as he suggests., may have occurred because she was young, inexperienced, not fluent in Samoan, and lived with an expatriate family. No one can deny these facts.[48] However, they are hardly significant theoretical points today and not worthy of much attention. That is why, I suggest, Freeman was forced to construct his biology-culture framework, so that his attack on Mead would have a semblance of contemporary significance. In doing so, Freeman's fervor led him to miss the chance to make two very significant contributions, both of which he is unaware but which are inherent in the issues he discusses. Both also relate to the complexity of culture--in general as well as in Samoa.

The first point is the simple fact that Mead was a woman and Freeman a man. Recent anthropological literature abounds with discussion and illustration of the fact that male and female fieldworkers often focus on different kinds of data, or theories, or have access to different sources for data (see especially Ardener 1975a, 1975b). (Mead, of course, always believed this and only worked without a male counterpart once--in Samoa.) The fact that Mead went to study a specific problem and spent most of her time with adolescent girls was bound to have some impact on the data she gathered., just as Freeman's attendance at courts and predominantly male events had to influence his perspective. I am not surprised that her data on female adolescent sexuality differ from his, nor is it any wonder that her views of the ranking system are different (adolescent girls surely did not perceive it in the same way as adult men). Mead astutely considers this possibility. She notes that "village rank hardly affects the young children" (1961:48) but later states "nevertheless, rank not of birth but of title is very important in Samoa" (49). Mead also notes that the typical (or at least not highest ranking) Samoan female

> treats the lore of the village, the genealogies of the titles, the origin myths and local tales, the intricacies of the social organisation with supreme indifference. It is an exceptional girl who can give her great-grandfather's name, the exceptional boy who cannot give his genealogy in traditional form for several generations. (1961:71)

Freeman had a chance to recognize the significance of male-female differences (e.g., pp. 269, 288), but in the end it truly eludes him and he fails to realize what might be one aspect of the true complexity of Samoan culture. At least Mead admits that she views Samoa through the lens of only one of its segments (how many ethnographers admit to using an adult male lens, or accord any other lens equal validity?), and Freeman's suggestion, that her informants tricked her (289-90), does not adequately resolve the numerous contradictions. If Freeman had seriously considered the issue, or if he had even been aware that culture is not necessarily a single whole possessed uniformly by every individual, that different groups or segments in a society might see things in different ways, he could have made a significant contribution. But he did not.

The second point Freeman could have made but failed to perceive also has to do with the complexity of human sociocultural behavior. Shore (1983) brilliantly illustrates how Samoan life contains paradox, contradiction, and opposition, and he subtly and deftly resolves the dis-

crepancies between Mead's point of view and Freeman's. If Freeman had been less anxious to discredit Mead and more concerned with a legitimate understanding of her errors and the realities of Samoan life, he might have made a similar contribution. He attributed what he perceived to be her errors partly to the factors listed above but also to the theoretical paradigm in which she worked--supposed absolute cultural determinism and a denial of biological influences in human behavior. In truth, biology has nothing to do with it and Freeman himself presents a cultural analysis. The errors in Mead's interpretation have nothing to do with Boas or cultural determinism. They derive from a more far-reaching problem in the wider anthropological paradigm that characterized most of our work until recently: an inability to recognize that cultures and social organizations are not necessarily consistent, in equilibrium, and perfectly logically integrated in Western terms. Mead saw the contradictions, the "opposing tendencies," but anthropology had not at that time theoretically incorporated the notion of contradiction. Explaining deviation from the pattern was Mead's problem, in the same way that finding functions for witchcraft was a problem for British social anthropologists and seeking positive roles for conflict was a problem for social science (particularly sociology and anthropology) in general. If Freeman had been conversant with recent anthropological recognition of the nature of paradox and apparent contradiction in human sociocultural systems, he might have seen what the deeper issues here are and made a significant contribution to anthropological theory. But he did not, and the book remains little more than a curiosity.

NOTES

1. Introductory textbooks recently published in the United States are a good barometer of this trend. See, for example, Greenwood and Stini 1977; Johnston and Selby 1978.

2. As Murphy, Alland, and Skinner wrote in a letter published by the New York Times on February 6, 1983,

> whatever may be the Samoan facts, subsequent research in other parts of the world has substantiated her essential theoretical stance. . . . There are hundreds of societies in which girls are married shortly after menarche. Virginity, repression and teen-age problems are hardly important issues in female socialization in such groups, for they have liquidated adolescence, making this a moot question.

3. For additional examples of the basically cultural argument Freeman presents, see pp. 130, 208, 211, 216-17, 220, 225, 249, and 276.

4. The entire passage is relevant here:

> By intently observing their [Samoan chiefs] physiological states [during assemblies], and especially their redirection and displacement activities, I was able, as their anger mounted, to monitor the behavior of these chiefs in relation to their use of respect language. From repeated observations it became evident that as chiefs became angry they tended to become *more and more polite,* with ever-increasing use of deferential words and phrases. Thus, by resort to cultural convention they could usually avoid potentially damaging situations. Occasionally, however, the conventions of culture would fail completely, and incensed chiefs, having attained to pinnacles of elaborately patterned politeness, would suddenly lapse into violent aggression. . . . In such cases there was an extremely rapid regression from conventional to impulsive behavior. For our present purpose the significance of such incidents is that when the cultural conventions that ordinarily operate within chiefly assemblies fail, activity does *not* suddenly come to an end, but rather the conventional behavior is replaced, in an instant, by highly emotional and impulsive behavior that is animal-like in its ferocity. It is thus evident that if we are to understand the Samoan respect language, which is central to their culture, we must relate it to the disruptive emotions generated by the tensions of social dominance and rank, with which this special language has been developed to deal. In this case, as in other domains of their society, impulses and emotions underlie cultural convention to make up the dual inheritance that is to be found among the Samoans, as in all human populations. It is evident, therefore, that the cultural cannot be adequately comprehended except in relation to the much older phylogenetically given structures in relation to which it has been formed by nongenetic processes. Further, it is plain that the attempt to explain human behavior in purely cultural terms, is, by the anthropological nature of things, irremediably deficient. (300-301; emphasis in original)

5. In a footnote, Freeman does admit that

> inasmuch as it has, in accordance with Durkheimian precept, totally excluded biological variables, social anthropology in Great Britain and elsewhere, despite various differences in emphasis, has operated within the same basic paradigm as American cultural anthropology. (313)

Thus one must assume that Freeman's critique is as applicable to British social anthropology as it is to American cultural anthropology, if not more so. At least American anthropology in general included a biological subfield parallel to cultural anthropology.

6. Freeman is unequivocal in his accusations:

> it [the paradigm of absolute cultural determinism] was, indeed, essentially a *system of belief,* which, in claiming to represent something like revealed truth, required the suppression of whatever did not conform with its central dogma. And it was to such suppression . . . that the principal conclusion of Mead's Samoan researches was directed. (47; emphasis in original)

(Note the evasive use of the passive here, however.) Also, see pp. 282-83.

7. Krogman also notes that "in my student days at Chicago I was literally weaned on 'The Mind of Primitive Man' " (1976) "and describes Boas' research on human growth as "pioneer" (7). He added that Boas' "height-weight tables of Worcester children were for many years accepted as physical growth standards" (7).

8. Stocking (1974:14) also gives Boas credit for maintaining the four-field approach.

9. For another excellent discussion of Boas, one that places him firmly in historical context and that analyzes his work fairly, see Stocking 1968.

10. When Freeman cites this reference on page 28, he gives its publication date as 1911 (and place as Washington). However, Boas' professional publication of the results was not until 1912 in the *American Anthropologist* (n.s. 14, no. 3). Freeman must be referring here to (and quoting from) Boas' report to the government, which Stocking (1968:342) lists as Senate Document 208, 61st Congress, 2nd Session. The actual report was submitted to Congress in 1909 (Boas 1940 n. 60). Stocking (1968:176-80) discusses this report in detail and notes that Boas became more cautious about his conclusions and suggestions over time. Certainly, the line Freeman (28) quotes about "financial panics" is not to be found in the version of the article Boas chose to include in *Race, Language and Culture* (1940), which was based on the 1912 *Anthropologist* version. I have not been able to locate a copy of the 1911 report in the time available to write this review and therefore cannot comment on the context in which Boas made the statement about physical changes possibly being affected by "financial panics." Stocking discusses the report in some detail but does not mention the statement.

11. What is missing from the Stocking quote here is not insignificant. It should read:

> the whole thrust of his [Boas'] thought was in fact to distinguish the concepts of race and culture, to separate biological and cultural heredity, to focus attention on cultural process, to free the concept of culture from its heritage of evolutionary and racial assumption, so that it could subsequently become the *cornerstone of social scientific disciplines* completely independent of biological determinism. (Stocking 1968:264; emphasis added)

It is also interesting that Stocking immediately continues by saying that "this is not to suggest that Boas was solely responsible for this process, or even that he was fully conscious of it" (264-65).

12. Here is another example in which Freeman goes from evolution (whether biological or social is not clear) to a demeaning of biological factors in general:

> it is evident that much of the disdain that Virchow had for evolutionary thought was communicated to Boas, for, as Boas' student Paul Radin has noted, Boas "always took a prevailingly antagonistic position" to the theory of evolution. This antagonism was undoubtedly Boas' great shortcoming as an anthropologist, for while it spurred him to oppose the unwarranted application of biological principles to cultural phenomena, it also caused him to underestimate the importance of biology in human life, and to impede the emergence of a scientifically adequate anthropological paradigm based on recognition of the pervasive interaction of biological and cultural processes. (26)

13. "Even earlier, she [Benedict] had made the point that it is those individuals whose innate characteristics are too far removed from the norm of their culture who find their culture deeply uncongenial" (Mead 1972:195). Also see later discussion.

14. Freeman sometimes uses the tactic of implying guilt by association. On pages 98-99 he writes,

> in 1924, when the nature-nurture controversy was at its height, J. B. Watson had baldly asserted that there was "no such thing as an inheritance of capacity, talent, temperament, mental constitution and characteristics," and in subsequent years he had repeatedly spoken of human nature as having "limitless plasticity." However, as the hereditarians were quick to point out, Watson's sweeping assertions were unsupported by any . . .evidence, and in this highly insecure situation Mead's depiction of Samoa became of fundamental significance, not only for the proponents of cultural determinism but equally for the wider environmentalist movement. . . .

Although it is left unsaid here (not elsewhere), the implication is that Boas and his students, because their work was cited by extremists such as Watson, were somehow identical in approach to Watson. But the passages I have cited (and will cite) indicate that Boas, Benedict, Mead, and Kroeber would not agree.

15. Although it is not as theoretically significant, Freeman is also inaccurate in quoting Boas on page 24 (and takes some of Boas' words for his own). Here is Freeman:

> after noting that among these Eskimo, as among the rest of mankind, the fear of traditions and old customs was deeply implanted, he [Boas] added the revealing comment that it was "a difficult struggle for every individual and every people to give up tradition and follow the path of truth."

Here is Boas:

> The fear of tradition and old customs is deeply implanted in mankind, and in the same way as it regulates life here, it halts all progress for us. I believe it is a difficult struggle for every individual and every people to give up tradition and follow the path to truth. (Boas in Stocking 1968:148)

(Note the change Freeman made from "to truth" to "of truth.") Another example is relevant here. On page 28 Freeman makes much of Boas' purported Lamarckianism and cites Stocking. What he does not tell a reader is that although Stocking (1968:184) does say that "there is much in Boas' work to tie him to the tradition of neo-Lamarckian direct environmentalism . . . ," he also says that Boas was neither a "committed Lamarckian" nor Darwinian. Freeman never mentions that Stocking (186) notes that Boas changed his mind about the inheritance of acquired characteristics.

16. Freeman recognizes that Kroeber was more cultural than Boas yet asserts that the difference between them was only of a "slight degree" (46). He fails to note that Kroeber too was reacting to the excesses of the social evolutionists and eugenicists, and that Kroeber did not completely exclude biological factors. In the 1948 edition of his textbook, Kroeber wrote that

> the drift of this discussion may seem to be an unavowed argument in favor of race equality. It is not that. As a matter of fact, the anatomical differences between races would appear to render it likely that at least some corresponding congenital differences of psychological quality exist. These differences might not be profound, compared with the sum total of common human faculties, much as the physical variations of mankind fall within the limits of a single species. Yet they would preclude identity. (Kroeber 1948:204)

And here is how Kroeber differentiates anthropology from other disciplines:

> could it be that the specific subject of anthropology is the interrelation of what is biological in man and what is social and historical to him? The answer is Yes. Or, more broadly, anthropology does at least concern itself with both organic and social factors in man, whereas nearly all other sciences and studies deal with one or the other. Anthropology concerns itself with both factors because these come associated in human beings in nature. (1948)

Kroeber undoubtedly stressed the significance of cultural factors, especially in his own work; no one would deny that. But it is not possible to assert with impunity that Kroeber was completely and unalterably opposed to the inclusion of biological factors in understanding human beings. Kroeber's "intellectual manifesto" of 1917 is nowhere near as simple as Freeman indicates. In this article, Kroeber makes the following statements:

> Everyone is aware that we are born with certain powers and that we acquire others. There is no need of argument to prove that we derive some things in our lives and make-up from nature through heredity, and that other things come to us through agencies with which heredity has nothing to do. (165)

> That heredity operates in the domain of mind as well as that of the body, is one thing; that therefore heredity is the mainspring of civilization, is an entirely different proposition, without any necessary connection, and certainly withont any established connection, with the former conclusion. (192)

Freeman's portrayal of Lowie is equally one-sided. Lowie diverged significantly from Benedict and Mead, and although he did partly follow Kroeber's idea that culture was "a thing *sui generis*" (Lowie in Freeman:45), again Freeman fails to accord any real significance to the fact that Lowie was arguing against eugenics and racism (as well as geographical determinism). See, especially, Eggan's Introduction (1966) to Lowie's *Culture and Ethnology* (originally published in 1917) as well as Lowie (1966) himself.

17. For example, pp. 67-68 n. 6, 74 n. 24, 118-19 n. 11.

18. For example, see pp. 59-60, 104, 119, 249-50.

19. For example, see pp. 24-25 n. 12, 26-27 n. 15, 84-85 n. 4. This practice of mentioning a specific work in the text is especially devious because it gives the reader the impression that information and even specific quotes come from one source when in reality they come from another. For example, on page 74 Freeman writes the following sentence:

> It thus transpired that the first written application of Benedict's new theory appeared in Mead's account, in *Social Organization of Manu'a,* of the "dominant cultural attitudes" of the Samoans, "every detail of the phrasing" of which was "thrashed out" by Benedict and Mead, as they "discussed at length the kind of personality that had been institutionalized in Samoan culture."

None of these quotations come from *Social Organization of Manu'a.*

20. See the paragraphs on these pages for examples: 30-31, 31, 38-39, 67, 80-81, 85-86, 164-65.

21. The examples of this practice are so numerous that it is fair to characterize it as typical and to say that when Freeman does so indicate, it is exceptional. The practice facilititates taking quotes out of context without arousing the reader's suspicion. Some examples have already been given and more will be provided in the discussion of Mead's work and in further notes.

22. Several examples of this practice have been mentioned, but additional examples are required. If one compares passages from Freeman with his original sources, it is easy to see that although a citation is usually present, he uses words and phrases that are not his. Sometimes he changes a tense or rearranges word or phrase order, but this is hardly good scholarship.

Example # 1

> On the morning of 31 August 1925, "remembering Stevenson's rhapsodies," Mead was up early for her Matson liner's arrival in the romantically remote islands of Samoa. The "whole picture," alack, was badly skewed by the presence of numerous battleships of the American Pacific fleet, with airplanes screaming overhead, and a naval band playing ragtime. (Freeman 61)

(Note here that Mead never said "remembering Stevenson's rhapsodies"--at least not in the source Freeman cites. The paragraph mentions W. Somerset Maugham, and cites him, so perhaps it belongs to him; see Freeman 316.)

> The presence of the fleet today skews the whole picture badly. There are numerous battleships in the harbor and on all sides of the island, mostly not in the harbor because they make the water oily and spoil the governor's bathing. Airplanes scream overhead; the band of some ship is constantly playing ragtime. (Mead 1977:23)

Example #2

> Again, although the Christian church required chastity for church membership, in actual practice, according to Mead, no one became a church member until after marriage. . . . (Freeman 91)

> Indeed, the sterner tenets of protestant Christianity had been so "remoulded," according to Mead, that there was "passive acceptance" by religious authorities of the premarital promiscuity which was, so she claimed, customary among female adolescents, with the result that, as she asserted in 1929 "no one" became a church member "until after marriage." (Freeman 185)

> No one becomes a church member until after marriage, and young widows and widowers whose bereaved state is protracted usually fall from membership. (Mead 1929:269)

> This passive acceptance by the religious authorities themselves of pre-marital irregularities went a long way towards minimising the girls' sense of guilt., (Mead 1961:123)

Example #3

> Occasionally, adults will "vent their full irritation upon the heads of troublesome children" by soundly lashing them with palm leaves or dispersing them with a shower of small stones. . . . (Freeman 88)

> If a crowd of children were near enough, pressing in curiously to watch some spectacle at which they are not wanted, they are soundly lashed with palm leaves, or dispersed with a shower of small stones. . . . (Mead 1961:33)

Example #4

> "Love, hate, jealousy and revenge, sorrow and bereavement," we are told, are all matters of weeks. . . . (Freeman 212)
>
> Love and hate, jealousy and revenge, sorrow and bereavement, are all matters of weeks. (Mead 1961:146)

23. See, for example, pp. 87 n. 7; 87-88 n. 10; 88-92 (n. 18, 19, 20, and 21); 131 n. 1; and 188 n. 25.

24. The full text of Freeman's note, which appears on page 321, is as follows:

> 3. M. Mead, *Male and Female* (Harmondsworth, 1962), 100, 201; idem, *Coming of Age* 122, 170; idem, "The Role of the Individual in Samoan Culture," *Journal of the Royal Anthropological Institute* 58 (1928): 418; idem, "The Samoans," in M. Mead, ed., *Cooperation and Competition among Primitive Peoples* (New York, 1937), 308; idem, "1925-1939," in *From the South Seas* (New York, 1939), xxvi.

25. Except when a whole paragraph comes from one source, or in the few cases of clarity, almost any paragraph could illustrate the ambiguity inherent in this style.

26. There are other instances of this. See, for example, pp. 229 n. 5 and 80 n. 36.

27. Examples of the way in which Freeman takes quotes out of context are more than numerous. In order to substantiate that it is a frequent occurrence and not restricted to the examples given in my text, I will provide a few further instances.

Example # 1

> As we have seen, Mead's Samoan researches gave apparently decisive support to the movement that (in George Stocking's words) sought "an explanation of human behavior in purely cultural terms,"and so sustained the antibiological orientation of the Boasian paradigm. (Freeman 294-95)
>
> At numerous points, I have emphasized that internal (although to a large extent parallel) developments within *each* of the social sciences conditioned the movement toward an explanation of human behavior in purely cultural terms, and that these developments in turn must be viewed in the context of changes in scientific disciplines *outside* the social sciences. (Stocking 1968:303; emphasis in original)

Example # 2

> Not only is competition muted and covert within village communities, but also, Mead claims,"competitiveness between villages usually does not reach important heights of intervillage aggression *[sic]*." (Freeman 89)
>
> Pride, in Samoa, is permitted an outlet through the high valuation which is put upon the village, its honor, its prestige. Because of the intricate kin ties between villages, and the freedom of choice which makes membership in each cooperative group ultimately voluntary, competitiveness between villages usually does not reach important heights of intervillage aggressiveness. But intervillage warfare was a possibility which increased with the number of people who lived within easy reach of each other. (Mead 1976b:474)

Example #3

> These were the attitudes that the young Margaret Mead came to adopt, and which led her, when embarking on her inquiries in Manu'a, to assume that human nature, being "the rawest, most undifferentiated . . . raw material," could be shaped by culture into any form. (Freeman 295)
>
> At the beginning of the decade, using language quite as extreme as that of Watson [see above], she advanced the view, on the basis of her researches in Samoa and New Guinea, that human nature was "the rawest, most undifferentiated of raw material". (Freeman 101)
>
> There are theorists to-day [1930] who, proceeding upon the assumption that all children are naturally good, kind, intelligent, unselfish and discriminating, deprecate any discipline or direction from adults. Still others base their disapproval of disciplinary measures upon the plea that all discipline inhibits the child. . . . All of these educators base their theories on the belief that there is something called Human Nature which would blossom in beauty were it not distorted by the limited points of view of the adults. It is, however, a more tenable attitude to regard human nature as the rawest, most undifferentiated of raw material, which must be moulded into shape by its society, which will have no form worthy of recognition unless it is shaped and formed by cultural tradition. (Mead 1975:211-12)

Example #4

> In 1961 she wrote of "the absoluteness of monographs of primitive societies," which "like well-painted portraits of the famous dead . . . would stand forever for the edification and enjoyment of future generations, forever true because no truer picture could be made of that which is gone." *Coming of Age*, she indicated, was just such a monograph, and she dwelt on "the historical caprice which had selected a handful of young girls on a tiny island to stand forever like the lovers on Keats' Grecian urn." (Freeman 106)
>
> This was the period [pre-World War II] when we emphasized the absoluteness of monographs on primitive societies, valuable precisely because they were the records of an order which would soon vanish never to return. Like well-painted portraits of the famous dead, these monographs would stand forever for the edification and enjoyment of future generations, forever true because no truer picture could be made of that which was gone, We were conscious of the historical caprice which had selected a handful of young girls on a tiny island to stand forever like the lovers on Keats' Grecian urn. There was, in spite of the dynamic content of our subject matter, a certain static quality about our approach. (Mead 1961: 13-14)

28. Freeman's position with regard to Freud is an interesting one. He actually appears to be somewhat Freudian himself. He asserts in a note (320) that "my own researches in Samoa, during the years 1966-1967, revealed the Oedipus situation to be decidedly present." His discussion on pp. 208-11 also reveals a Freudian bent. He seems to take the position that orthodox psychoanalytic theory is basically grounded in biology and innate impulses, and thus anyone who debates traditional Freud is taking an antibiological stance.

29. Freeman is very careless about noting changes in Mead's presentation of her Samoan materials, although he does take the opportunity to note exaggerations (e.g., 102). He quotes

freely from all of her works without concern for when they were published or who the intended audience was. If he were seriously intent on establishing the fact that Mead's work on Samoa had a definitive impact on the nature-nurture debate of the 1920s and early 1930s, he should have predominately restricted himself to a consideration of her early work only, especially *Coming of Age in Samoa* (first published in 1928) and *Social Organization of Manu'a* (first published in 1930). But he does not. For example, the paragraph (200-201) that introduces chapter 14, "Childrearing" contains eleven quotes from Mead, none of which is from these two central sources. This is not unusual. In the critical chapter 6, "Mead's Depiction of the Samoans," there are twenty-seven references to *Coming of Age,* only eight to *Social Organization of Manu'a,* and more than seventy-five references to other works by Mead--about two-thirds of which were published in 1935 or later.

30. See Shore (1983) for a more insightful evaluation of Mead's work in Samoa.

31. Freeman did visit American Samoa in 1967 (xv), but does not state the duration of the visit.

32. The full quote from the Samoan chief is as follows:

> All the Samoan people are of one race. Our customs, genealogies, legends and languages are the same. The chiefs and village maids *(taupou)* of American Samoa when they visit British Samoa are recognized as chiefs and *taupous* of certain villages in accordance with their genealogies. Their visitors from British Samoa are likewise recognized in the chief councils of Tutuila and Manu'a. (Freeman 117)

33. There seems to be a constant refrain coming from all Samoan informants: things used to be stricter than they are today. Mead's informants said it, Freeman's informants said it, Gerber's informants (Freeman 108) said it, but nowhere does Freeman consider that this consistency might be a clue to something; he just accepts it at face value as truth unaffected by contemporary Samoan morality and worldview. Gerber (Freeman 108) saw that indeed people see their past from the perspective of the present, but Freeman, despite describing her work as "excellent ethnography" (326), just ignores her.

> She [Gerber] construed the unequivocal statements of her Samoan informants as a "rewriting of history," so accepting Mead's fanciful account of Samoan sexual behavior in preference to the unanimous and direct testimony of the Samoans. . . . Could any myth, one wonders, have acquired, within the confines of a scientific discipline and during the second half of the twentieth century, a greater potency? (108)

34. Freeman never addresses the issue of the nature and definition of "cult," and his usage of the term is clearly at odds with accepted anthropological practice.

35. One must also wonder, given this reticence, about Freeman's explanation of Mead's "error." He claims her informants tricked her and just told her stories (e.g., 290), but why invent sexy stories if they were so reticent about sexual matters?

36. Mead (1961:190) says that "as there were only sixty-eight girls between the ages of nine and twenty, quantitative statements are practically valueless for obvious reasons: the probable error of the group is too large; the age classes are too small, etc."

37. Freeman holds scientific standards such as replicability apparently in high regard. He makes this especially clear on page 291 where he chastises Boas for not subjecting Mead's

finding to a thorough comparison with other sources. While it is more than legitimate to criticize Mead if she inadequately prepared herself with background and comparative reading, it is not appropriate to criticize her for not writing a book on Samoan history. Nor is it appropriate to begin with the assumption that if Mead differs from another source, it is Mead who is necessarily in error. Freeman goes to ridiculous lengths here, claiming that she returned from the field "with tales running directly counter to all other ethnographic accounts of Samoa. . ." (291). As Freeman himself notes many times, other ethnographers (including Freeman himself) have at least on occasion agreed with her. In evaluating ethnography, Freeman's criteria are not necessarily scientific or consistent, and he is incapable of other than black-or-white reasoning on this issue. For example, Lowell Holmes is treated shabbily by Freeman despite his excellent ethnography which disagreed with Meads depiction of Samoa; Holmes' error was to say that "the reliability of Mead's account of Samoa was 'remarkably high' " (Freeman 105; see also 103-5). Freeman chides Holmes for having what Freeman thought was irrefutable evidence against Mead but not arguing that "the central conclusion she had reached in *Coming of Age in Samoa* about the sovereignty of nurture over nature was false" (105). Freeman seems unable to understand that Holmes could present solid evidence that contradicts Mead's data and yet still believe her account was reliable and not argue against nurture over nature.

38. In the first reference to these High Court data, he states "that in the prudish Christian society of Samoa in the 1920s, sexual intercourse between unmarried persons was held to be both a sin and a crime is confirmed by cases in the archives of the high court of American Samoa" (238). Despite his use of the word "cases" here, he gives only a single example. The second reference does not even appear in the body of the text but in a footnote in which he gives a single example of fines being imposed for adultery (351). The third reference is again to a single example, this time of surreptitious rape (246). The final reference to these court records offers promise but gives us no numbers.

> The court records of American Samoa, which begin in 1900, note numerous cases of rape having been committed by Samoans during the first three decades of this century, and the jail statistics included in the exhibits attched to the hearings of the congressional commission on American Samoa of 1930 show that at the end of the 1920s rape was the third most common offense after assault and larceny. . . . (249)

Note that the only reference to the High Court records here is that there were "numerous cases"--the other material is from another source.

39. Freeman (xvi) finished an early draft of this manuscript in August of 1978, obviously without these High Court data. He clearly thought that this early manuscript was substantial because he offered to send a copy to Mead. He claims that he received no reply to this offer, but of course neglects to mention that although not hospitalized, Mead was dying at the time. The innuendo was picked up and made much of by the popular press (see, for example, Rensberger 1983:37).

40. Here in detail is what Freeman does.

> In 1966, when the total population of Western Samoa was about 131,000, the number of forcible and attempted rapes reported to the police . . . was thirty-eight, which is equal to a rate of about sixty rapes per 100,000 females per an-

> num, a rate twice as high as that of the United States and twenty times as high as that of England. Further, if cases of surreptitious rape, or indecent assault, reported to the police be included, then the Western Samoan rate becomes approximately 160 rapes per 100,000 females per annum. These figures, while only very approximate (for in Western Samoa a very considerable proportion of forcible and surreptitious rapes are, in fact, not reported to the police), do indicate that rape is unusually common in Samoa; the Samoan rape rate is certainly one of the highest to be found anywhere in the world. (248-49)

Note the curious generation of these statistics. Also note that in order to get the rate up to 160, he includes "surreptitious rape, or indecent assault"--clearly he has no idea what would happen to the U.S. rates if indecent assault were included. His parenthetical comment about the underreporting of rape in Samoa also of course applies to the United States where it is estimated that only one in five to only one in twenty *actual* rapes is ever reported (Brownmiller 1975:175).

41. Here Freeman describes surreptitious rape:

> Surreptitious rape, or *moetotolo* (literally "sleep crawling") is a peculiarly Samoan custom in which a man, having crept into a house under cover of darkness, sexually assults a sleeping woman. (244)

> The intention of the sleep crawler is, in fact, to creep into a house in which a female virgin is sleeping, and before she has awoken to rape her manually by inserting one or two of his fingers in her vagina, an action patterned on the ceremonial defloration of a taupou. (245)

Freeman never addressses the significant problem here: how does this occur without waking up the girl or one of the many other people sleeping in the partitionless house? Mead (Freeman 245) thought that the girl was expecting him or another lover and therefore did not raise a hue and cry, but Freeman dismisses this possibility entirely by saying that "as anyone who had studied the phenomenology of rape will know, successful personation by a rapist is an extremely rare event . . ." (245). He cites no source here on the "phenomenology of rape.) Freeman describes forcible rape this way:

> . . . 60 percent of the victims were virgins. In the typical case . . . a girl of from 15 to 19 is alone and away from the settled parts of her village when accosted by a male of from 19 to 23 years of age. Often he is known to the girl, and he believes her to be a virgin. When she tries to escape, her assailant commonly resorts to the culturally standardized strategem of knocking her unconscious with a heavy punch to her solar plexus. After inserting one or two fingers into his victim's vagina, the rapist usually also attempts penile intromission. . . . (248)

As anyone familiar with the literature on rape ought to know, these descriptions do not fit with the American legal definition of rape (see Brownmiller 1975). One of the most significant differences, of course, is that in raping, Samoan men are trying to acquire wives. "Many Samoans aver that the principal aim of a male who engages in either surreptitious or forcible rape is to obtain for himself a virgin wife" (Freeman 247).

42. Freeman does, in a later context (142), reluctantly acknowledge that Mead presents both sides.

43. One example is of some historical interest. On page 79, Freeman tells us that "in Sydney, Australia, in October 1928 she [Mead] proudly told A. R. Radcliffe-Brown that it was Boas who had planned her work in Samoa. . . ." What Freeman doesn't relate is how the subject arose.

> [Radcliffe-]Brown is awfully funny. He approves of my work and today, after he had been reading the monograph [Social Organization of Manu'a], he began tea by saying he didn't see how I had gotten away with doing work which was so awfully contrary to the spirit of the American school, the kind of work which Boas didn't believe was possible. I retaliated by saying that Boas had planned my work. (Mead 1959:309)

44. See, for example, Mead 1961:32, 33, 34, 39, 40, 49, and Mead 1976b:310 for just a sampling.

45. For example, see Freeman pp. 84 n. 2, 88, 89, 93, 100, 172, 186, 191, 212-13, 222, 238, 244-45, 255ff., 268.

46. See also pp. 142, 147, 166, 216ff., 273, 278. Freeman also cites authors who support this as well; for examples, see pp. 163, 234, 376.

47. Mead recognized that perspectives on old data can and do change. In the 1962 introduction to *Male and Female: A Study of the Sexes in a Changing World,* she wrote:

> also, there have been many developments in anthropological theory since this book was completed. Fifteen years have elapsed within which the vivid interaction between cultural theory and observations and experiments on other living creatures, primates, ungulates, and birds, have given us new insights into biologically given behavior and possible types of more specifically instinctive behavior in man. (Mead 1967:xiii)

48. Mead herself recognized her own inexperience and youth (e.g., 1961:15) and argued strongly for more pre-fieldwork training and preparation (e.g., 1972:141-44).

REFERENCES CITED

Ardener, Edwin

1975a Belief and the Problem of Women. In *Perceiving Women.* Shirley Ardener, ed. London: Malaby Press, pp. 1-17 (first published in 1972).

1975b The "Problem" Revisited. In *Perceiving Women.* Shirley Ardener, ed. London: Malaby Press, pp. 19-27.

Boas, Franz

1940 *Race, Language and Culture.* New York: The Free Press.

Brownmiller, Susan

1975 *Against Our Will: Men, Women and Rape.* New York: Simon and Schuster.

Eggan, Fred

1966 Introduction. In *Culture and Ethnology,* by Robert Lowie. New York: Basic Books, pp, v-xiv.

Frisch, Rose

1975 Critical Weights, a Critical Body Composition, Menarche, and the Maintenance of Menstrual Cycles. In *Biosocial Interrelations in Population Adaptation.* Elizabeth S. Watts, Francis E. Johnston, and Gabriel E. Lasker, eds. Paris: Mouton, pp. 319-52.

Greenwood, Davydd J., and William A. Stini
1977 *Nature, Culture, and Human History: A Bio-Cultural Introduction to Anthropology.* New York: Harper and Row.

Harris, Marvin
1968 *The Rise of Anthropological Theory.* New York: Thomas Y. Crowell Company.

Hrdlicka, Ales
1919 *Physical Anthropology: Its Scope; Its History and Present Status in the United States.* Philadelphia: The Wistar Institute of Anatomy and Biology.

Johnston, Francis E., Michael Borden, and Robert B. MacVean
1975 The Effects of Genetic and Environmental Factors upon the Growth of Children in Guatemala City. In *Biosocial Interrelations in Population Adaptation.* Elizabeth S. Watts, Francis E. Johnston, and Gabriel E. Lasker, eds. Paris: Mouton, pp. 377-88.

Johnston, Francis E., and Henry Selby
1978 *Anthropology: The Biocultural View.* Dubuque, Iowa: William C. Brown Company, Publishers.

Kroeber, Alfred
1917 The Superorganic. *American Anthropologist* 19(2):163-213.
1948 *Anthropology.* New York: Harcourt, Brace and Company (first published in 1923).

Krogman, Wilton M.
1976 Fifty Years of Physical Anthropology: The Men, the Material, the Concepts, the Methods. In *Annual Review of Anthropology.* Bernard J. Siegel, ed. Palo Alto: Annual Reviews Inc., pp. 1-14.

Lowie, Robert
1966 *Culture and Ethnology.* New York: Basic Books, Inc. (first published in 1917).

McDowell, Edwin
1983 New Book Challenges Margaret Mead's Conclusions. *The New York Times,* January 31, 1983, pp. A1 and C1.

Mead, Margaret
1929 Americanization in Samoa. *American Mercury* 16:264-70.
1930 *Social Organization of Manu'a.* Honolulu: Bernice P. Bishop Museum Bulletin No. 76.
1959 *An Anthropologist at Work: Writings of Ruth Benedict.* New York: Avon Books.
1961 *Coming of Age in Samoa.* New York: Dell Publishing Co. Inc. (first published in 1928).
1963 *Sex and Temperament in Three Primitive Societies.* New York: William Morrow and Company (first published in 1935).
1967 *Male and Female: A Study of the Sexes in a Changing World.* New York: William Morrow and Company, Inc. (first published in 1949).
1972 *Blackberry Winter: My Earlier Years.* New York: William Morrow and Company, Inc.
1975 *Growing Up in New Guinea.* New York: William Morrow and Company, Inc. (first published in 1930).

1976a The Samoans. In *Cooperation and Competition among Primitive Peoples.* Margaret Mead, ed. Gloucester, Mass.: Peter Smith, pp. 282-312 (first published in 1937).

1976b Interpretive Statement. In *Cooperation and Competition among Primitive Peoples.* Margaret Mead, ed. Gloucester, Mass.: Peter Smith, pp. 458-511 (first published in 1937).

1977 *Letters from the Field 1925-1975.* New York: Harper and Row.

Murphy, Robert F., Alexander Alland, and Elliot P. Skinner

1983 An Abortive Attack on "Coming of Age." Letter to the Editor, *New York Times.* February 6, 1983.

Rensberger, Boyce

1983 The Nature-Nurture Debate I. *Science* 83, (April): 28-37.

Shore, Bradd

1983 *Sala'ilua: A Samoan Mystery.* New York: Columbia University Press.

Stocking, George

1968 *Race, Culture and Evolution: Essays in the History of Anthropology.* New York: The Free Press.

1974 ed., *The Shaping of American Anthropology 1883-1911: A Franz Boas Reader.* New York: Basic Books.

Response: Derek Freeman
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I shall comment first on the remarks of Fay Ala'ilima and Felix Wendt, both of whom know Samoa, and then on those of Nancy McDowell, who does not.

On the remarks of Fay Ala'ilima. Rather than dealing with the scientific significance of my book, Fay Ala'ilima has chosen to dwell on other matters. I shall, then, do no more than comment very briefly on her essentially personal remarks.

By admitting that the facts I have marshalled in my book refer to realities, Ala'ilima is, logically, also admitting that Mead's extreme conclusion of 1928--that biological variables are of no significance in the etiology of adolescent behavior--is in error.[1] This, for anthropology, is a crucially important recognition, and is, in fact, the principal objective of my book.

However, a refutation, as Ala'ilima fails to understand, must perforce concern itself with the systematic testing of those propositions that its author supposes to be in error, for only in this way can error be exposed and eliminated from the formulations of a scientific discipline. It is therefore pointlessly digressive for Ala'ilima to inquire why, instead of constructing a refutation of Mead's errors, I did not write an account of my personal experiences in the early 1940's as a member of the household of Lauvī

Vainu'u, who was at that time the leading talking chief of Sa'anapu village on the then remote south coast of the island of Upolu. My reply is that this is something I may eventually do. At this juncture I shall merely note that Lauvī Vainu'u came to mean more to me, in some ways, than my own father, and that I have both warm affection and deep respect for the people of Sa'anapu, some of whom I have now known for more than forty years. My association with Sa'anapu is something that means much to me, and I only hope that I shall be able to continue to make whatever contributions I can to the welfare of its people.

The people of Sa'anapu, like other Samoans (as Ala'ilima must know), are devout Christians, and as such they value the truth. All that I have done in my book, in the interests both of Samoan studies and of anthropology, is to speak a modicum of the unvarnished truth. I have not yet heard of anybody being banished from a Samoan village, an Australian town, or, for that matter, a scientific society, for disinterestedly speaking the truth. Indeed, in my view, the speaking of the truth, when it bears on issues of great intellectual and scientific importance as in my book, is a prime responsibility of any scientist or man of good will, and he should be prepared to honor this responsibility at whatever the personal cost.

Ala'ilima's suggestion that my book was written for self-aggrandizement is sheer aspersion, wholly untrue, and quite unworthy of her. I also reject her equally darksome suggestion that my book will act to the disadvantage of individual Samoans. Because some Samoans engage in, say, aggressive behavior, this in no way means that all Samoans engage in such behavior, and there is certainly no warrant in my book for the formation of stereotypes, as Ala'ilima suggests. As for the Samoans of Honolulu, Auckland, and Carson City, people will continue, as in the past, to take them as they find them. This, moreover, is something that these and other Samoans well understand, which is why the great majority of them are so well mannered and so well behaved.

I do not claim, nor have I ever claimed, that Samoans are "tremendously grateful" for my documentation of the "darker side" that exists in Samoan behavior just as it does in the behavior of all peoples. What I do know is that a number of Samoans of my acquaintance fully appreciate the importance, for the future of Samoan studies, of the refutation of the errors in Mead's account of Samoa, even if this involves, as it necessarily does, the facing of the realities of Samoan existence. The Samoans themselves are, of course, no strangers to these realities. They are brought to the attention of the *matai* of all Samoan villages in the courts, or *fono manu*, in which they all sit: from time to time. Further, they are realities dealt with by these chiefs, and the great majority of Samoans, with both firmness and justice.

On the remarks of Felix Wendt. Although he freely acknowledges that "many of the things Margaret Mead said about Samoans were incorrect," which means that her general conclusion based on these incorrect statements is in error, Felix Wendt objects to the evidence contained in my book. This is despite the fact that it is largely verified evidence from official sources, and solely on the ground that it may create a bad "image."

This is an attitude that, given Wendt's avowal of both Christianity and science, I cannot, in all reason, understand. God, on whom the Samoans aver their country is founded, is (Deuteronomy 32:4) above all concerned with the truth. In science too the truth is all-important and is approached, as Sir Karl Popper has shown, by the elimination of error. And so, if as Wendt acknowledges, Mead's *Coming of Age in Samoa* contains numerous incorrect statements, it clearly becomes one's scientific duty, as a serious student of Samoa, to refute those, errors.

Again, it is a cardinal mistake to suppose that because unlawful behavior by Samoans has been recorded at certain rates, that this, in any rational sense, creates an "image" of the Samoan people at large. As Wendt rightly notes, the "darker side" of the Samoans is "no darker than that of any other people." And further, as I emphasize in my book, the Samoans most definitely have their "shining virtues," being, as I note,, devoted to the ethics of Christianity and the ideal of mutual love, or *fealofani.*

This does not mean, however, that unlawful behavior is absent from Samoa, and the facing of this fact without anger or fear is to be desired on both scientific and humane grounds. This, I would note, is something that Professor Albert Wendt has acknowledged in writing of my book: "Derek Freeman's insights into us and our way of life reveal that he has a deep love of Samoa. He sees us *honestly;* he does not try to hide the disturbing side. His work is a major contribution to understanding who and what we Samoans are; in fact, to understanding what people are like everywhere."

I would add that knowing them as I do, in all their human complexity, I indeed do have love and admiration for the people of Samoa, and that it is my belief that if only we Westerners can understand the Samoans in all their human complexity, then we shall also be able to understand ourselves.

Felix Wendt's aspersions that I have, in my researches in Samoa, betrayed secrets and engaged in "purposeful misinterpretation," are quite capricious and wholly untrue. I have throughout striven to behave as a responsible scientist should, and my regard for the people of Sa'anapu is of a kind that will not fade.

Wendt's further aspersion that, after having known them for over forty years, I have "grown old and disillusioned with the changing faces of Samoa," is also entirely unjustified and totally untrue. Indeed, I am now more hopeful about the future of Western Samoa than I have ever been. In particular, I have been impressed by the progress that is being made within the University of Samoa in the field of Samoan studies.

As a number of eminent Samoans, whose views I deeply respect, have remarked to me, my refutation of Mead's depiction of Samoa has been an essential step in the establishment of a serious discipline of Samoan studies. It was, therefore, a source of great satisfaction to me, during my visit to Western Samoa in August 1983, to be able to set up within the University of Samoa, with an initial donation of W.S. $3,000 from the royalties of the German language edition of my book, a special research fund to enable Samoan scholars to do research on the history and culture of Samoa.

As I remarked in my address at the first graduation ceremony of the University of Samoa, in the capacity of Academic Pro-Chancellor, I very much hope that as the field of Samoan studies develops, it will be possible to communicate to the outside world some of the humanly valuable aspects of the *fa'aSamoa.* Some examples are, the dignified *amio fa'aaloalo* of Samoans, the custom of *tapua'i,* the enlightened way in which Samoans deal with convicted criminals, and, perhaps most importantly of all, their expert techniques of achieving, when necessary, *fa'aleleiga,* or reconciliation, between warring social factions.

It is thus very much my view that although Samoa is a small country, it has great significance for the science of anthropology, and that Samoan studies as they develop will contribute greatly to human studies in general. It is to this process that my book is an essential contribution, as I hope all thoughtful Samoans and *papalagi* will come to realize.

On Nancy McDowell and Margaret Mead. Before commenting on Nancy McDowell's defense of Margaret Mead's Samoan researches let me remind my readers of Mead's extreme conclusion of 1928. In Mead's own description (1977: 19) she went to Samoa in 1925 "to carry out the task" that had been "given" to her by her professor, Franz Boas, "to investigate to what extent the storm and stress of adolescence" is "biologically determined and to what extent it is modified by the culture within which adolescents are reared." In 1928, in the fourth paragraph of the thirteenth chapter of *Coming of Age in Samoa* (1961, orig. 1928:197), she came to the scientifically preposterous conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior. I say scientifically preposterous because in the light of modern knowledge it is evident that all human behavior is characterized by the interaction of cul-

tural and biological variables. Thus, as Konner (1982:80) has recently expressed it, "any analysis of the causes of human nature that tends to ignore *either* the genes *or* environmental factors may safely be discarded."

On this basis alone any knowledgeable behavioral scientist, then as now, would reject Mead's extreme conclusion of 1928. Yet, as I document in my book, the preposterous conclusion of Mead's *Coming of Age in Samoa* rapidly became pivotal to the doctrine of cultural determinism and, having been recorded in countless textbooks and repeated in university lecture rooms throughout the world, has long been integral to the belief systems of many cultural anthropologists and especially of devoted admirers and associates of Margaret Mead. Nancy McDowell, it is important to realize, is one of these.

In 1980, for example, she published in the *American Anthropologist* a paper entitled "The Oceanic Ethnography of Margaret Mead," which contained, among other things, her evaluations of Mead's *Coming of Age in Samoa* and *Social Organization of Manu'a.* By that time there had been serious questioning of Mead's extreme conclusion of 1928, ranging from Raum's observation (1967, orig. 1940:293) that Mead's assertions were "often contradicted by her own evidence" to Barnouw's critique in the third edition of his *Culture and Personality* (1979:89-94), in the course of which he pointedly cites Jane van Lawick-Goodall's observation (1971:160), "Adolescence is a difficult time for some chimpanzees just as it is for some humans." Again, Mead herself, in the "reflections" she included in the 1969 edition of *Social Organization of Manu'a,* had admitted (1969:227) the "serious problem" of "reconciling" the "contradictions" between her account of Manu'a and "other records of historical and contemporary behavior." And finally, in 1972, in the *Journal of the Polynesian Society,* I had published a detailed study of Mead's far from proficient use of the Samoan language (1972:74ff.) listing over 180 errors (some of them egregious) that occur in the Samoan sections of the text of *Social Organization of Manu'a.*

Yet, in her laudatory appraisal of 1980, McDowell, ignoring entirely all of this substantive criticism, dwelt on Mead's "concern for the precision and accuracy of her data," claiming that the fact that *Social Organization of Manu'a* might have been written in 1980, was "a telling statement" about the "standards and brilliance"' of Mead's work, with Mead's "fieldwork and published reports" still standing as "models for any beginning fieldworker to follow" (1980:278).

These statements by McDowell, given the numerous errors that had by then been shown to exist in Mead's *Social Organization of Manu'a,* can only be classed as examples of uncritical adulation.

In the same paper of 1980, McDowell went on to tell her professional colleagues, without critical comment or qualification of any kind, that in *Coming of Age in Samoa,* Mead had shown that the "storm and stress" adolescence was "a cultural creation." Anyone who could repeat this extreme proposition as late as 1980 is very obviously a cultural determinist, and it is very much to professional believers in Mead's preposterous conclusion of 1928 (like Nancy McDowell) that my book is addressed.

It is very evident however that the ungainsayable evidence I have adduced to demonstrate that, at least with reference to Samoa, Mead's conclusion of 1928 cannot be sustained has greatly agitated Dr. McDowell. In their classic study *When Prophecy Fails,* Festinger, Riecken, and Schacter have remarked on "the variety of ingenious defences with which people protect their convictions" (1964, orig. 1956:3). As an ardent admirer of Margaret Mead and a leading proponent of her views, McDowell is in the position of one for whom prophecy has failed. As an apologist for the scientifically preposterous conclusion that the young Margaret Mead reached in her enormously influential *Coming of Age in Samoa,* McDowell has, at inordinate length and with the fervor of a fundamentalist, mustered every conceivable argument in an attempt to save something from the wreckage of one of her fondest beliefs.

Because McDowell knows nothing in particular about Samoa and, as is obvious from her remarks, lacks detailed knowledge of the histories of both anthropology and biology, her arguments are, except in quite minor matters, entirely ineffectual and in no sense amount to a counter-refutation. Indeed, I am grateful to Dr. McDowell who, by her very detailed defense of Mead's views, has given me an opportunity to demonstrate in even greater detail than in my book the scientific inadequacy of the doctrines propounded by Boas and Mead in the 1920s and 1930s, as well as those of the latter-day cultural determinists like Bradd Shore, whose formulations about Samoa McDowell, in her defense of Mead, has also extolled with uninformed enthusiasm.

My personal relationship with Margaret Mead. To divert attention from the major scientific issues with which my book is concerned, McDowell has, quite inaccurately, claimed that it is really an "attack on Mead." In fact, in the preface to my book, in emphasizing my "high regard" for "many of the personal achievements of Margaret Mead," I specifically note that my concern is with the scientific import of her Samoan researches, and "not with Margaret Mead personally, or with any aspect of her ideas or activities that lie beyond the ambit of her writings on Samoa."[2]

In a scientific refutation of the kind I have essayed one has, perforce, to deal with the statements of another individual. One is, however, dealing only with these statements, and *not* with the individual who originated them. Blame is thus in no way involved.

In science, the efficacy of a refutation depends solely on the authenticity, relevance, and cogency of the evidence adduced. Thus in my book (the structure of which McDowell has failed to comprehend) my refutation of Mead's depiction of Samoa and of her conclusion of 1928 precedes and is *logically quite separate from* my subsequent discussion of the likely causes of Mead's misconstruction of Samoa. This, I would emphasize, is because any discussion of the likely causes of an error, while potentially of heuristic value, has no direct bearing on a successful refutation because it adds no relevant evidence.

And here again blame is not an issue, nor can a refutation be justly construed as a personal attack, as McDowell would have it. Indeed, in science, as Popper has emphasized, an individual whose conclusions have been refuted has, by virtue of this fact, contributed in a fundamentally important way to the course of scientific progress.

To exemplify this fact and to rebut McDowell's unwarranted aspersion, let me briefly record the course of my personal dealings with Dr. Mead.

I first met Dr. Mead in 1964 when, during a long and formal private conversation in the Research School of Pacific Studies of the Australian National University, I placed before her the evidence that had led me, as early as 1943, to reject the conclusion she had reached in *Coming of Age in Samoa.*

Immediately after this meeting I wrote to Dr. Mead as follows:

> It is plain to me that our conclusions about the realities of adolescent and sexual behavior in Samoa are fundmentally at variance. For my part I propose (as in the past) to proceed with my researches with as meticulous an objectivity as I can muster. This, I would suppose, is going to lead to the publication of conclusions different from those reached by you, but I would very much hope that, however we may disagree, there should be no bad feeling between us. You have my assurance that I shall strive towards this end.

Dr. Mead replied in a letter dated New York, 2 December 1964, that ended with the exemplary words "what is important is the work." During our subsequent correspondence, which extended from 1964 to 1978, Dr.

Mead continued to behave in this exemplary manner. In a letter to the *New York Times* of 13 February 1983, Mary Catherine Bateson observed that although her mother "was vehement in defense of her views, she did not descend to 'the clangorous exchange of insult' precisely because she believed that anthropology was evolving in her lifetime toward an increasingly exact science and that science is everywhere the cumulative work of many minds."

In my judgment it is precisely because Margaret Mead held these views and because she grappled, throughout her life, with anthropological problems of fundamental importance that she is assured an honored and secure place in the history of anthropology.

On the characteristics of an interactionist approach. In the final chapter of my book, having indicated at least as far as Samoa is concerned the inadequacy of the extreme form of cultural determinism that was adopted by Kroeber, Lowie, Boas, Benedict, Mead, and others, I adumbrate the essentials of a more scientific, anthropological paradigm based on the recognition of both cultural and biological variables and their interaction. Unlike the paradigm with which cultural determinists have long operated, in which all biological variables are totally excluded from consideration by arbitrary fiat, an interactionist paradigm makes no such unscientific, a priori assumption, but recognizes, in any particular case, *all demonstrably determining variables,* be they cultural or biological, without any prior assumption as to their relative importance.

This is a scientific point of view that McDowell, as an inured cultural anthropologist, quite fails to appreciate. It is, for example, a complete non sequitur to suppose that recognition of the biological dimensions of human behavior, as in, say, Richard Passingham's *The Human Primate*, in any way involves what McDowell calls "a Hobbesian view of human nature."[3]

Again, she unthinkingly dismisses the revealing instance of the respect language of the Samoans that I give in my book (1983:300) by claiming that "social dominance" has nothing to do with biology, whereas anyone with the most casual acquaintance with the relevant scientific literature (as, for example, D.R. Omark, F.F. Strayer, and D.G. Freedman, eds., Dominance Relations, New York and London, 1980) will know that social dominance is very much a part of human ethology. And, most extraordinarily of all, while fully admitting that "although the refutation of Mead's data does not logically require that Freeman present biological evidence," she nonetheless quite illogically complains that my refutation (based as it is on an interactionist approach) relies, in part, on "cultural data." In fact, it is precisely because Mead's conclusion of 1928 was based

on highly inexact and incomplete cultural data that the use of such data is directly relevant to the refutation of her erroneous conclusion. As an interactionist it is wholly in order for me to adduce whatever evidence I choose in my refutation of Mead as long as it is both authentic and relevant. Further, McDowell is also mistaken in asserting that, as an interactionist, I am "without a theoretical means of accounting for the diversity of behavior apparent between human groups." I shall return to this crucially important issue in the concluding section of this rejoinder.

On non sequiturs and American physical anthropology. I next come to McDowell's ill-informed assertions about what she mistakenly supposes to be my ignorance of American physical anthropology. She begins with the breathtaking non sequitur that because I was trained in Cambridge, I am "therefore" unfamiliar with American physical anthropology. The fact of being trained in Cambridge (or, for that matter, anywhere else) in no way necessarily involves an unfamiliarity with American physical anthropology. In fact, before I undertook my doctoral studies at King's College, Cambridge, I had been trained in anthropology--first, in the late 1930s, at Victoria University College in the University of New Zealand, and then from 1946 to 1948 at the University of London, when, in the course of my other training, I studied biological anthropology with Dr. N. Barnicot at University College, London.

In New Zealand, my principal adviser was Dr. Ernest Beaglehole, who had studied anthropology at Yale University under Sapir, himself a student of Franz Boas. As Gladwin (1961:148) has noted, the emphasis of Beaglehole's anthropology was "in many ways similar to that of Mead." I was thus exposed to the Boasian approach to anthropology from the very outset of my anthropological career. Indeed, in Samoa in the early 1940s I had with me, and systematically studied, the 1938 first edition of *General Anthropology*, a textbook edited by Boas and containing chapters by Boas himself as well as by Benedict, Bunzel, Lowie, and other Boasians.

Since that time I have taken a close interest in all aspects of American anthropology, including American physical anthropology, a field I have been familiar with (quite contrary to McDowell's ill-informed assertions) for more than forty years.

On Boas and cultural determinism. McDowell, in her ignorance of what is involved in interactional thinking, claims that because physical anthropology is represented in what she calls the "four-field approach" of American anthropology, "the interaction between biology and culture has always been important." This is a false and misleading claim. As Stocking (1968:264) has documented, the "whole thrust" of Boas' thought was to "separate biological and cultural heredity."[4] It was this separation in de-

scription, analysis, and explanation that Kroeber and Lowie, by arbitrary fiat, made the basic assumption of cultural anthropology in 1917. This has meant that while American cultural anthropologists may, in the course of their preliminary training, have had some elementary instruction in physical anthropology, they thereafter operate, with but few exceptions, with the assumptions of the Boasian paradigm and, lacking any training in human ethology, actively ignore the possibility that ethological variables might well be among the determining variables of the phenomena they purport to explain. In other words, they have no theory of human nature and unscientifically assume that human behavior can be fully explained in cultural terms.

In my book I document that it was Franz Boas and his disciples who, during the first four decades of this century, established and actively promulgated the doctrine of cultural determinism. McDowell asserts that in so doing I have failed to recognize "the significant and positive role" of Boas in "establishing the importance of biology" in anthropology. This belief that Boas, during the very decades that saw the formation of the doctrine of cultural determinism, was, at the same time, a positive proponent of biology is, as I shall show, yet another myth.

On baseless accusation. First, however, let me deal with McDowell's baseless accusation that I have knowingly distorted certain of Boas' words. In a supposedly "astounding"' culmination to her opposition to my depiction of Boas, McDowell draws attention to a passage (1983295) in which I note that even as late as 1939, Boas thought that in regard to the human body "a search for genes would not be advisable," as there was some danger that the number of genes would "depend rather upon the number of investigators than upon their actual existence." McDowell then reveals that my quotations of Boas' words do not come from Boas' paper of 1939 "Genetics and Environmental Factors in Anthropology," but "incredibly" from an article entitled "The Tempo of Growth of Fraternities," originally published by Boas in 1935.

Why this situation should be considered incredible I am at a loss to understand. In the note referring to the paragraph in question (1983:359) I cite both Boas' paper of 1939 (in support of my comment on page 95 that he was "opposed to research in human genetics" (an interpretation I shall presently substantiate further) and also the version of his article, "The Tempo of Growth of Fraternities," that was republished in 1940 in a volume entitled *Race, Language and Culture,* while noting it had originally appeared in 1935. The preface to this volume by Boas is dated Columbia University, 29 November 1939. It is thus evident from this fact (as well as from everything else known of Boas' attitude toward genetics from

1935 onward), that in 1939 he still stood by the views expressed some four years earlier in the concluding paragraph of his paper on "The Tempo of Growth of Fraternities." Thus I am *fully justified* in claiming that Boas held the view I have attributed to him about genetics "as late as 1939."

McDowell also accuses me of what she calls a "travesty of scholarship," saying that I have deliberately distorted Boas' attitude toward genetics because Boas recognized in his paper of 1935 that heredity was a variable in the tempo of growth. This accusation I completely reject. If only McDowell had made an informed study of Boas' ideas and attitudes during the last ten years of his life she would know that Boas' notions of heredity were (as I shall presently document) of a decidedly peculiar nature, and that throughout these years, in clinging obdurately to a belief in Lamarckian inheritance, Boas also maintained his long-standing prejudice against both evolutionary biology and the science of genetics.

It remains my view then that the passage in which Boas expressed his scepticism as to the "actual existence" of genes is a clear example of his general prejudice against genetics. Dr. McDowell's far from well-informed defense of Boas affords me an opportunity--which I shall now take--to discuss Boas' attitudes toward genetics and evolutionary biology more fully than was possible in my book. As we shall see, McDowell's wild accusations of deliberate distortion and poor scholarship are groundless and without justification.

On biological research during the last four decades of Boas' life. In support of her claim that Boas played a "significant and important role" in "establishing the importance of biology" in anthropology, McDowell cites Hrdlicka and Krogman on Boas' contributions to physical anthropology. As Krogman notes (cf. Herskovits 1943:39), Boas made notable contributions to the study of race, growth, and development, and to biometrics. These fields, however, are very much peripheral to biology proper and to assess McDowell's claims it is necessary to view Boas and his beliefs in the context of the history of biology, especially during the scientifically momentous first four decades of the twentieth century.

Before he went to America Boas had gained, mainly from Waitz, a belief in Lamarckian inheritance and, from Virchow, a marked disbelief in and antipathy to the theory of biological evolution (see Freeman 1983: ch. 2). These then were the attitudes toward the great biological issues of the day that Boas had firmly espoused by the time he became professor of anthropology at Columbia University in 1899.

The following year three different biologists, de Vries, Correns, and von Tschermak (all of them engaged in studies of plant hybridization) stumbled on Mendel's classic paper of 1866 and what Garland Allen

(1972:v) has called "the age of genetics" began. Boas was to live until December 1942, and so the last forty-two years of his professional life saw both the formation of the science of genetics and the emergence from the early 1930s onward of the evolutionary synthesis (cf. Mayr 1982:567), both of which are central to modern biology. It thus becomes possible to assess Boas' attitudes in relation to these historic events.

As early as 1902 Sutton (Allen 1979:56) had pointed to "the strong similarity between Mendel's hypothesis of segregation and the microscopically observable separation of homologous chromosomes during meiosis." By 1910 it had become evident that "chromosomes were cell structures that acted as the vehicles of heredity," and over the next five years T.H. Morgan and his associates, working in the same university as Boas, in a series of brilliant experiments laid the foundations of modern genetics. In particular, in their book *The Mechanism of Mendelian Heredity* (1915), Morgan and his associates developed the idea that "factors," in Mendel's sense, were physical units (or genes) located at definite positions (or loci) on chromosomes, and by 1920, as Allen notes (1979:65), these discoveries were "almost fully accepted throughout the biological community."

In his 1926 work *The Theory of the Gene,* Morgan presented further evidence to show that the gene represented "an organic entity" (1926:321). In reviewing this book Jennings (1927:184) noted that the day had passed when with respect to heredity "one man's fancies seemed as good as another's"; Dunn (1927:24) remarked that "the theory of the gene or of inheritance by discrete units" was as secure as any was likely to be and was "ready to take its place as one of the major generalizations of biology."

These major advances in genetics also had a profound effect on the theory of biological evolution so that as Huxley (1949: 12) has noted, "about 1920 biologists began to be interested in how natural selection would operate on organisms with Mendelian (particulate) inheritance, and started applying mathematical methods to the problem." This problem was effectively solved with the publication in 1930 of Fisher's *The Genetical Theory of Natural Selection,* which was followed in 1937 by Dobzhansky's *Genetics and the Origin of Species,* a book which, as Mayr (1982:569) records, signalled the decisive emergence of the synthetic theory of biological evolution.

All of these crucial advances within biology had occurred *before* the appearance in 1938 of the textbook *General Anthropology* (which was edited by Boas, and contained a section written by him on the "biological premises"of anthropology) and of the second edition of Boas' *The Mind*

of Primitive Man, originally published in 1911. It is thus possible to gauge with some precision the way in which Boas reacted to the seminal developments within biology that had taken place during the years of his professorship at Columbia University between 1899 and 1937.

The scientific status of Lamarckian theory in the 1930s. First, however, let me refer briefly to the way in which the attitude of informed biologists toward Lamarckian inheritance (in which Boas had long believed) had changed during these same years. Although belief in Lamarckian inheritance had not been uncommon during the first decade of the twentieth century and lingered on in some quarters into the 1920s and beyond, among the vast majority of biologists it did not survive the epoch-making researches of T.H. Morgan and his associates to which I have already referred. Thus, in an article on Lamarckism; published in the fourteenth edition of the *Encyclopaedia Britannica,* Morgan (1929:609) noted that "the most complete disproof of the inheritance of somatic influence is demonstrated in almost every experiment in genetics," and concluded "the facts are positive and unquestioned and contradict thoroughly the claim that germ cells are affected specifically by the character of the individual." And the following year, H.S. Jennings, the Henry Walters Professor of Zoology at Johns Hopkins University, in his book *The Biological Basis of Human Nature,*[5] referred (1930:342) to the fact that by that time an experimenter who put forward a claim that he had "proof of the inheritance of acquired characters" was classified "in the 'lunatic fringe' of biology." It was to this "lunatic fringe," as I shall show, that Boas belonged throughout the 1930s and until his death in 1942.

On Boas' attitudes toward biology. In deploring my depiction of Boas' doctrines, McDowell asserts that I take "Boas' opposition to racism and biological determinism as evidence that Boas was opposed to all consideration of biological or hereditary factors and even incorporates evolution." She then accuses me of trickiness for claiming that the Boasians had "an antipathy to biology and to genetics and evolutionary biology in particular."[6] In this assertion and this accusation McDowell is quite mistaken, and, as one who has (unlike herself) seriously studied the relevant historical evidence, I reject as baseless her accusation of trickiness.

Kroeber, who at the time he was formulating his doctrine of absolute cultural determinism went so far as to refer to those "infected with biological methods of thought" (1916:34), knew Boas well and has recorded that Boas "was not much interested in biological evolution or in genetics both of which he used or related to his own work very little" (1956:156).[7] This is an understatement, for although Boas must have had some inkling of the momentous advances that took place in the theory of biological ev-

olution during the first four decades of the twentieth century, he was antipathetic toward this theory and to evolutionary theory generally throughout the period that he exerted such a decisive formative influence over American anthropology. Thus Radin, another of Boas' students, has recorded (1939:305) that Boas "always took a prevailingly antagonistic position" to the theory of evolution, while Stocking (1968:184), having made a study of the relevant historical evidence, states that Boas was "quite skeptical of natural selection"--the central mechanism of biological evolution discovered by Charles Darwin.

Another measure of Boas' attitude toward biology is his virtually total neglect of the writings of Charles Darwin. Kluckhohn and Prufer (1959:22), in their study of "influences" on Boas during his "formative years," noted that the only citation from Darwin that they had discovered in all of Boas' writings was to *The Voyage of the Beagle.* This, furthermore, was only a reference (Boas 1963, orig. 1911:134) to how a Fuegian, after a sojourn in England, had fallen back "into the ways of his primitive countrymen." There is, in fact, a brief mention of Darwin in Boas' chapter on race in *General Anthropology* (1938:116), though only in the course of a dismissive discussion of natural selection.[8] This book appeared after the publication of Dobzhansky's *Genetics and Origin of Species,* at a time when Boas, had he been in touch with biology, would have had to take an altogether different stance. Again, Boas' *The Mind of Primitive Man,* a second edition of which appeared in 1938, contains a long chapter on "The Emotional Associations of Primitives" in which there is no mention at all of Darwin's classic work of 1872, *The Expression of the Emotions in Man and Animals.*

Boas' pronounced lack of interest in Darwin and the theory of evolution by means of natural selection was actively communicated to others, for Boas was, as Vidich wrote, "personally a powerful figure who did not tolerate theoretical or ideological differences in his students" (1966:xxv). Indeed, Mead herself, in a vivid phrase, described how Boas' influence "spread through American anthropology like an animated veto" (1969:345). And part of this influence, as is evident from his writings, as from other sources, was most certainly an antagonism toward both biological evolution and evolutionary theory in general. Thus Kluckhohn and Prufer (1959:22) record that Boas' students reported that he "did not discuss biological evolution in his seminars," and so marked was his influence that as Professor J.J. Williams noted in 1936, Boas had by that time succeeded in "suppressing the classical theory of evolution among practically the entire group of leading American ethnologists."

Boas' prejudice against genetics. Boas' "prevailingly antagonistic position" toward evolutionary theory on which Radin remarked in 1939 was joined by what Kluckhohn and Prufer (1959:22) have called "a skepticism about Mendelian heredity." Again, in recording that "a relative lack of interest in experiment remained with Boas all his life, and seems to have been a deep-seated quality of his mind," Kroeber (1943:7) also noted that Boas was "long inclined to be suspicious of Mendelian heredity, evidently trusting more in statistical analysis than in experimental findings on selected characters."

These deeply seated attitudes, it is important to realize, were retained by Boas as long as he lived and in the face of decisive scientific evidence to the contrary. As I have already indicated, by about the mid 1930s the science of genetics had, through a series of elegant and precise experiments conducted during the previous two to three decades, decisively illumined the problem of heredity, and Morgan, in recognition of his work in establishing the chromosome theory of heredity, had in 1933 been awarded the Nobel Prize in physiology.

In his book *The Physical Basis of Heredity* Morgan had demonstrated that the presence of genes in chromosomes was "directly deducible" from his experimental results (1919:237), a conclusion (as I have already noted) he further explicated in 1926 in *The Theory of the Gene.* Thus by 1930 Jennings, in surveying the progress of genetics during the previous three decades, could write "positive and inescapable experimental evidence proves that the chromosome is a structure composed of many diverse parts, each part, or gene, having a definite effect on development, and therefore a definite effect on the characteristics of the individual produced" (1930:73).

However, despite the "positive and inescapable experimental evidence" that had been widely published by the 1930s there were still obscurantists, Boas among them (most of them idealists who were opposed to the materialistic implications of genetic research), who, in defense of their own antiquated beliefs, argued that genes were no more than figments. It was to this supposition that Boas gave voice in 1935 in suggesting that if genetic methods were applied to the study of human growth there was a danger that "the number of genes" would "depend rather upon the number of investigators than upon their actual existence." When it is understood in historical context, this remark by Boas in a serious scientific paper is the clearest evidence of that antipathy to genetics which, as we know from other evidence, colored his thinking throughout the 1930s.

By 1930, through the researches of Landsteiner and others, it had become apparent, as Jennings put it, that humans have "the same genetic system, operating in the same manner, as have other higher organisms," and further, that for many human characteristics, there was "no doubt of the applicability of modern genetic science" with these characteristics "being inherited in the same way as are the characteristics of other organisms" (1930:154ff.).

It was to these propositions (that have been fully substantiated by subsequent research) that Boas was most rootedly opposed, as is evident in a brief article he contributed to the November 1939 issue of the journal of the New York Association of Biology Teachers. According to Boas (1939:17ff.), although the study of genetics had "attracted so much attention in recent times," the subject received "perhaps more attention" in the school curriculum than a "well rounded presentation of the facts of biology" justified. There was little doubt, Boas thought, that as time went on and the novelty of the study of genetics wore down, other aspects of "the problems presented by life" would receive "greater attention." It was "particularly unfortunate," Boas felt, that "the data of genetics obtained from the study of lower forms are too readily applied to man." "The application of genetic data to man," Boas declared, should, on account of its social implications, "be made most guardedly."

These statements are direct expressions of the suspicions about Mendelian heredity and the "actual existence of genes" that ruled Boas' thinking throughout the 1930s. They are evidence, in my judgment, both of an antipathy to genetics in general and of opposition to research on humans based on Mendelian principles.

Professor Boas and the woodpeckers. Among the principal arguments that Boas advanced against the "application of genetic data to man" was the supposition that "man cannot be compared to wild animals," as man is a "domesticated form" who has undergone modification in the "process of domestication." As I show in chapter two of my book, Boas was much influenced in his anthropological thinking by Theodore Waitz, an out-and-out Lamarckian. Further, as Kluckhohn and Prufer (1959:22) show, Boas persisted in his belief that Lamarck "was still to be reckoned with" as long as he lived, even though by the late 1930s the evidence of experimental biology had shown Lamarckism to be an unscientific doctrine.

In 1932, in the course of his presidential address to the American Association for the Advancement of Science, Boas (1940, orig. 1932:246) asserted with reference to humans as well as to some other animals, "one series of changes brought about by external conditions are undoubtedly hereditary . . . those developing in domestication." And some years later,

in 1938, in the second edition of *The Mind of Primitive Man*, in arguing for his essentially Lamarckian theory of domestication, Boas (1963, orig. 1911:87) stated that while this process can be studied "in its results only," the "direct influence of environment may be investigated experimentally and statistically." He then went on to quote at some length from a paper by O.F. Cook, originally published in 1907, an action that reveals convincingly just how extreme an environmentalist Boas was and how much out of touch he was with the biological thought of the late 1930s, which is epitomized in Dobzhansky's *Genetics and the Origin of Species* of 1937.

Cook, said Boas, quoting from Cook's paper of 1907, had made "observations" as follows:

> Zoologists speculate on such questions as whether the eggs of Vancouver woodpeckers, if transferred to Arizona would hatch Arizona woodpeckers or whether the transferred individuals would gain Arizona characteristics in a few generations. What the woodpeckers might or might not do depends on the amount of organic elasticity which they may happen to possess, but the experiment is unnecessary for answering the general question, since plants show a high development of these powers of prompt adjustment to diverse conditions. It is not even necessary that the eggs be hatched in Arizona.

Boas then proceeded, in a way that one would not have thought possible as late as 1938, to assert that Cook's ludicrously unscientific speculation "shows" that the "form" of a "species" is "determined by environmental causes."

The Arizona woodpecker, I am informed by Ernst Mayr (1969:pers. comm.), is now considered a subspecies of *Dendrocapos stricklandii*, while the Vancouver woodpecker (referred to by Cook) is probably a subspecies of the hairy woodpecker. By having in 1938, placed the credence he did in Cook's "observations" as scientific evidence, Boas has given us a telling glimpse of the quality of his biological thought, for to suppose that one subspecies of woodpecker might be transformed into another (in the way suggested by Cook and accepted as possible by Boas) is, in Ernst Mayr's words, "total nonsense." Indeed, in 1969 Ernst Mayr informed me that the paper by O.F. Cook relied on by Boas as proof of the environmental determination of the form of a species was "the weirdest, and most abstruse nonsense" he had ever read.

Such then was the quality of the biological understanding of Franz Boas. When reading the pages of *The Mind of Primitive Man* to which I

have just referred, one is impressed anew with the significance of Kroeber's testimony that Boas "was not much interested in biological evolution or in genetics, both of which he used or related to his own work very little." Indeed I am puzzled beyond measure as to what Dr. McDowell, in her secret heart, imagines "the significant and positive role" of Boas--the Lamarckian and extreme environmentalist--might conceivably have been in "establishing the importance of biology" in anthropology.

On Boas' extreme environmentalism. In her ignorance of the paucity of his biological knowledge McDowell asserts that I am "simply wrong" in stating that at the time Boas published the first edition of *The Mind of Primitive Man* in 1911 he was not "disposed to explore, in a constructive way the coexistence and interaction of genetic and exogenetic processes." In fact, the complete absence of any such exploration from the first edition of *The Mind of Primitive Man* fully substantiates my statement. Nor, I would add, was there any trace of such an exploration twenty-seven years later in the second edition of this most influential of Boas' books. Indeed, by 1938, as I have just shown, Boas' extreme environmentalist beliefs had hardened and had 'become even more extreme than they were in 1911. Moreover, because of his lack of knowledge of both genetics and evolutionary biology, Boas was in no position to undertake, at any point in his career, any constructive exploration of the "coexistence and interaction of genetic and exogenetic processes."

Boas and cultural determinism. Yet another of McDowell's errors is her mistaken notion that the argument of my book rests on the supposition that following the propounding in 1917 by Kroeber and Lowie of "a doctrine of absolute cultural determinism that totally excluded biological variables," Boas underwent a "conversion" to this doctrine that was "extreme and profound." This is by no means the case.

As I document in my book (1983:47), in his address on "The Mind of Primitive Man" given during the year following his 1899 appointment to the chair of anthropology at Columbia University, Boas explicitly argued for culture as a construct to which the laws of biology did not apply. He adhered to this view for the rest of his career. During the year before Kroeber and Lowie made their doctrinaire pronouncements, Boas himself (1916:473ff.) declared that it had to be assumed that "all complex activities are socially determined," and that "in the great mass of a healthy population, the social stimulus is infinitely more potent than the biological mechanism." Boas is here directly comparing exogenetic and genetic variables, and his belief that, in general, the first of these two sets of variables is "infinitely more potent" than the second, is but a very short step from the absolute cultural determinism of Kroeber and Lowie with its to-

tal exclusion of biological variables. There was thus no occasion for any "extreme and profound" conversion, for Kroeber and Lowie, who were Boas' former students and admiring disciples, had merely taken to its apogee the extreme environmentalism of which Boas had long been a leading advocate.

What I would next emphasize is that the conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior, reached by Mead in *Coming of Age in Samoa*, is completely in accord with the doctrine of absolute cultural determinism, with its total exclusion of biological variables, that Kroeber and Lowie had propounded in 1917. Furthermore, it is known from Mead's own testimony that Boas accepted Mead's extreme conclusion without question.

As I have argued earlier in this rejoinder, this conclusion of Mead's is, in scientific terms, preposterous, and the fact that it was fully accepted by Boas is the clearest possible evidence that in this crucial instance Boas was indeed a proponent of absolute cultural determinism. Further, his unqualified acceptance of Mead's extreme conclusion is equally an indication of how little Boas appreciated the biological bases of behavior, a fact that is fully confirmed by the analysis of his other attitudes toward biology (as, for example, his citing in 1938 of O.F. Cook's ludicrous flummery about the woodpeckers of Vancouver and Arizona).

McDowell on "good scholarship." I do not propose discussing in any great detail the section of her review that Dr. McDowell has called "on scholarship." Here, with unmitigated pedantry, she has piled Pelion on Ossa in expressing her disapproval of such scientifically momentous issues as the "citation style" that has been followed in my book, as though this, in some magical way, might lessen the cogency of my refutation of Mead.

I have, naturally, referred Dr. McDowell's criticisms to individuals at Harvard University Press of whose scholarly judgment and editorial skills I have the highest regard. Although their very definite advice to me was *not* to reply to Dr. McDowell's exaggerated criticism, I have decided, because this criticism is to appear in a scholarly journal, to comment briefly on the pedantic stance Dr. McDowell has adopted, beginning with part of the advice I received in this matter from Harvard University Press. My advisers write:

> We are very aware (and Ms. McDowell should be) that no form of citation is perfect, and that any decision to use one form rather than another entails both gains and losses. The forms of citation suggested to you as most appropriate for your book are ones that we and other university presses often use, for example,

> choosing not to have a separate bibliography when the sources are all included in the notes, consolidating notes where possible, not repeating citations for frequently used phrases, and reducing the number of quotation marks for phrases so brief that their use in sentences essentially constitutes a paraphrase rather than a quotation.

It is to these well established and widely accepted editorial practices, all of which I personally accept and for which I take full responsibility, that Dr. McDowell has pedantically objected, as though the laborious procedures to which she has become inured should be obligatory for all.

We reject her pedantic strictures both because we disagree with them in principle and because, when examined in detail, they are seen to be utterly trivial or to have no substance whatsoever.

Some examples of pedantry. McDowell complains that in the second paragraph on page 20 there is only one quotation while in the note to this paragraph on page 308 two sources are listed. The reader is thus left in doubt, so McDowell would have it, as to which is the source of the quotation In fact, the first source listed is Boas' paper "The Mind of Primitive Man," published in *The Journal of American Folklore* in 1901; being listed first, it obviously refers to my reference to Boas' presidential address to the American Folk-Lore Society in December 1900, mentioned in lines six and seven of the paragraph under discussion. In contrast, the quotation appearing in lines twelve and thirteen of this paragraph is obviously from the second source listed, Spier's paper of 1959 in volume 89 of the *Memoirs of the American Anthropological Association.*

Even readers of but middling intelligence would be able to work this out for themselves, but, if they found it beyond their capabilities, they could readily solve what is really no problem at all by consulting the sources listed. I do not recollect, in five decades of academic life, having come across a more trivial complaint than that which has been ponderously elaborated in this instance by Dr. McDowell, nor shall I, I would hope, ever hear a more preposterous accusation than that I am guilty of deliberately obscuring the author of a particular quotation. Of such stuff is the "scholarship" of Dr. McDowell.

McDowell also cites a paragraph from page 99 in which I quote Stocking on the dissemination of Boasian thinking and then give my own views on the significance of Meads assertion, on the basis of her researches in Samoa, about the sovereignty of culture. No intelligent reader could suppose, as McDowell suggests, that this opinion was that of Stocking for there is no continuation of quotation marks.

In a similar fashion, McDowell, in her note 19, criticizes the passage from page 74 of my book, but it is an outright non sequitur to suppose that the quotations in this passage come from *Social Organization of Manu'a;* anyone inclined to make this illogical inference would at once be apprised of his error if he consulted note 24 on page 319.[9]

I had not imagined that anyone could be so pedantic as to enumerate these and other trivialities as examples, as Dr. McDowell would have it, of the violation of the canons of "good scholarship."

Again, to suggest that I should not have cited Mead's clearly stated view of human nature as "the rawest, most undifferentiated of raw material," without also citing the long passage of over 120 words cited by McDowell herself, is, in my judgment, quite exorbitant pedantry. I might as well censure McDowell on the grounds that the 124 words she cites do not adequately convey Mead's meaning in that they are arbitrarily taken from a single paragraph of some 297 words in the course of which Mead states her view of human nature. If one were to behave with the extreme pedantry that Dr. McDowell advocates, the writing of readable books would be impossible. Readers if they wish, may check the sources for my citations themselves and reject the construction I have put on the words in question if they consider this warranted.

I can only say that I have written a book about anthropological issues of great moment that, while it may not, despite my best efforts, be entirely free from minor literal errors,[10] is based on painstaking and honest research. This being so, I reject as unprincipled McDowell's repeated resort to aspersion, as in her use of such epithets as "devious" and "deceptive," which being merely her peculiar personal opinion and entirely unsubstantiated, are examples of the *odium scholasticum* which is both out of place and of no probative consequence in scholarly and scientific controversy.

What does matter, however, in both scholarship and science, is the prefering of evidence over dogma and assumption. If only Dr. McDowell, in 1980, had given attention to the then well-known evidence of the errors in Mead's Samoan ethnography rather than uncritically extolling Meads concern for "precision," "accuracy," and "exactness," I might now have greater regard for her present pontificating on the canons of "good scholarship."

McDowell on Samoa. As is apparent from her apologia for Boas, McDowell has never made a detailed study of the relevant sources, relying instead for her "conclusions" on such secondary sources as Marvin Harris, who himself has no adequate appreciation of Boas' standing in relation to the biological theories of the early twentieth century. When it comes to Samoa, a complex world of which McDowell has no firsthand

knowledge, the case is even worse. Yet she has not hesitated to lay down the law about intricate matters of which she knows nothing in particular.

I must now, therefore, deal with the arguments she has put forward in an attempt to evade my refutation, the logic of which she quite fails to understand. For example, it is in no sense my objective, as McDowell mistakenly claims, to have my readers "believe" that Mead was "100 percent wrong" in her account of Samoa. I have simply offered evidence to demonstrate that Mead was not justified in categorizing Samoa as a "negative instance"--and this, as I shall presently show, I had not the slightest difficulty in doing, on either purely internal evidence, or on the contemporary historical evidence for those parts of Samoa in which Mead worked.

The Samoan archipelago. McDowell begins her defense of Mead by wondering about "the comparability of data gathered from different places" in the Samoan archipelago. Although Mead's investigations in 1925-1926 were confined to the islands of eastern Samoa, she fully recognized (Mead 1937:282) that these islands were part of the Samoan archipelago, which prior to European contact was a "closed universe" whose inhabitants conceived of "the Samoan people as all members of one organization." Furthermore, in *Coming of Age in Samoa* (1961:11) Mead specifically notes that "in an uncomplex, uniform culture like Samoa" she felt "justified in generalizing." So, as Richard Goodman (1983:9) has pointed out in his critical study, *Mead's Coming of Age in Samoa* : *A Dissenting View*, Mead's book contains more than 150 generalizations that are all explicitly about, as Mead puts it without qualification, "Samoans."[11]

That Mead should have generalized about all Samoans in this way is understandable, for although she worked only in eastern Samoa, she had repeated contact with native residents of the western region of Samoa whom she encountered in Tutuila and Manu'a. Indeed, the talking chief Lolo who (as Mead notes in her acknowledgements in *Coming of Age in Samoa*) taught her "the rudiments of the graceful pattern of social relations which is so characteristic of the Samoans," came from Salani, a settlement on the south coast of Upolu in western Samoa. Talala, whom Mead (1977:48) saw a great deal of in Manu'a during the first few months of 1926, came from Mulivai, a village of the Safata district of Upolu to which Sa'anapu, the main site of my own researches, belongs. In these circumstances, one is certainly justified in drawing on appropriately relevant evidence from anywhere in the Samoan archipelago.[12]

The time factor. While she has obviously made no significant study of Samoan history, McDowell raises the issue of the "comparability of data" gathered "at different times," arguing, in defense of Mead, that "surely

there was at least some sociocultural change" between the time of the completion of Mead's researches in 1926 and the beginning of mine in the early 1940s. According to McDowell I do "not trouble with this issue."

This is completely untrue. I have studied the history of the Samoans for over forty years, consulting the primary sources wherever possible and giving special attention to the history of the 1920s and 1930s. Thus, when I state that "there is no reason to suppose that Samoan society and behavior changed in any fundamental way during the fourteen years between 1926, the year of the completion of Mead's inquiries, and 1940, when I began my own observations of Samoan behavior" (1983:120), this judgment is based on the most detailed historical research.

Mead herself (1961:273), writing in 1927, considered that "given no additional outside stimulus or attempt to modify conditions, Samoan culture might remain very much the same for two hundred years." No such "stimulus or attempt" was effective during the 1930s, and in November 1937, Roger Duff, of the Canterbury Museum, New Zealand (an expert witness in this matter, who had just returned from two years spent in the Native Affairs Department in Western Samoa), was reported in the Christchurch *Press* as stating that "the outstanding characteristic of the Samoans had been their ethnic resistence to the intrusion of white civilization." "Europeans," said Duff, "had been about the islands for many years but there was no fundamental change in the Samoans principal economic and social customs." Again, Holmes (1957:230) concluded from his comparison of western Samoan culture in the mid-nineteenth century and from his own inquiries in eastern Samoa in 1954 that "cultural change" had been "relatively minimal over a period of a century." So, while considerable sociocultural change has taken place, particularly in American Samoa, during the second half of the twentieth century, my own researches in the early 1940s were conducted (although there had been a higher level of political activity in the 1920s) under conditions that were, in general, very similar to those experienced by Mead only fourteen years or so previously.

The evidence on which my refutation of Mead primarily depends. It is important to realize, however, that my refutation of Mead depends primarily not on my observations in either the 1940s or the 1960s, but on (i) internal evidence, i.e. evidence provided by Mead herself, and (ii) on historical evidence from the 1920s.

The internal evidence, especially that referring specifically to female adolescent behavior, I shall review later in this rejoinder.

My historical evidence for the 1920s is drawn from such unimpeachable sources as the reports of the Royal Commission of 1927 on Western

Samoa and the United States Congressional Commission of 1929-1930 on American Samoa, both of which are concerned specifically with the 1920s, including the period of Mead's researches; from court archives; from contemporary newspapers like *The Samoa Times;* and from the observations of scientists and other investigators like A.F. Judd, Dr. Peter Buck, Francis Flaherty, B. Cartwright, and N.A. Rowe, who were in Samoa (including Manu'a in the cases of Judd and Buck), either at exactly the same time as Margaret Mead or within a few years of her brief stay there. Any reader who has given this historical evidence the attention it deserves will have discerned that it decisively refutes numerous aspects of Mead's romantic depiction of Samoa.

Again, because Mead made unqualified pronouncements on major aspects of Samoan behavior, such as their "unaggressiveness" as she would have it, I have also included in my book a range of historical evidence so that readers can place her pronouncements in historical perspective. Moreover, some of Mead's pronouncements, I would emphasize, were of a historical kind and therefore have to be tested in the light of the relevant historical evidence.[13] So, when Mead, in support of her depiction of the "unaggressiveness" of the Samoans, states without qualification that formerly in Manu'a the "casualties were low" in warfare with "only one or two individuals" being killed, I refute this by showing that, on the contrary, warfare in Manu'a, as elsewhere in Samoa, commonly resulted in a heavy loss of life. For example, in the second half of the nineteenth century, between 1866 and 1871, in a war in Manu'a for which verified evidence is available, some fifty-five men were killed. In comparative terms, this is a very severe loss, representing 11.7 percent of the adult male population of Manu'a at this period.

As this example shows, a consideration of the relevant historical facts is crucially important to my refutation, for, as in this instance, it demonstrates conclusively the extreme inexactitude of some of Mead's statements.

On the memory of things past. As part of her defense of Mead, McDowell asserts that " 'fresh' memories after more than forty years is cause for skepticism." She is here referring to the testimony I collected in Manu'a in 1967 about conditions there in the mid-1920s.

The information I collected, in the Samoan language, was both detailed and specific and came from individuals who, like Mead, in the mid-1920s were in their early adulthood. Some of it was sworn testimony, which had been carefully cross-checked, and is thus of a kind that could be submitted in a court of law. One of my informants described the mid-1920s as being *lata mai nei,* or still close, and I have no hesitation in de-

scribing the memories I recorded of the events of that period as "fresh" in the sense that they were still vivid and circumstantial. This, however, should be no surprise to Dr. McDowell, for Margaret Mead in her seventies often wrote and spoke of events that she remembered having taken place in Samoa and elsewhere well over forty years previously.

On the sexual morality of the Samoans. I am, of course, thoroughly familiar with the distinction McDowell makes between ideal and actual behavior, and obviously this distinction is of critical importance in any discussion of the sexual morality of the Samoans.

The gravest defect of Mead's account of this aspect of Samoan life is her failure to report correctly the strictness of the sexual morality of the Samoans, particularly in respect to adolescent girls.[14] In this matter all other observers of Samoa are, to the best of my knowledge, in agreement. In Professor Albert Wendt's words (1983:4), for example, the Samoans in their public morality "forbid premarital and extra-marital sex and promiscuity."

This, it will be noted, is the antithesis of Mead's depiction of Samoa as being (Mead 1959:74) one of those societies that "permit an easy expression of sexuality at puberty," for "permit" is undoubtedly a term with moral connotations and is antithetical in meaning to "prohibit."[15]

That the prohibition of premarital and extramarital sexual intercourse was also the public morality of Samoa in the 1920s (the period to which Mead's writings on the Samoans specifically refer), with sexual intercourse between unmarried persons being held as both a sin and a crime, is demonstrated by cases in the archives of the courts of American Samoa. For example, on 6 May 1929 in the District Court at Fagatogo on the island of Tutuila, Lafitaga, a male, having admitted that he knew it was wrong for a man and woman to have "intercourse with each other unless they were married," was accused of committing "the crime of fornication" by "lewdly and lasciviously co-habiting" with a woman while not being "legally married to her."

Further, this severe sexual morality means that "if an unmarried girl is discovered by her brother in an illicit sexual relationship, he will beat her" (Schoeffel 1979: 168). This is a far cry from the condoned permissiveness that Mead erroneously reported.

Let me at once go on to say, however, that the existence in Samoa of this strict sexual morality does *not* mean that departures from it do not occur, as in the example I have just cited from Tutuila in 1929. In my book I give cases of adultery, surreptitious rape, and the like, in addition to presenting the results of an inquiry into the sexual experience of sixty-seven Samoan girls.

On virginity and adolescent girls. In reporting this inquiry I am said by McDowell to have been "very naive," and its results she has not hesitated to describe as "clearly unreliable." These comments I reject for they have been made in virtually complete ignorance of the issues involved.

Mead herself (as I mention on page 237) reported that in her sample of twenty-five adolescent girls, thirteen, or 52 percent, had had no "heterosexual experience." In order to test Mead's depiction of Samoan sexual mores and behavior, it was obviously important to repeat the kind of inquiry she had undertaken in 1925-1926.

I did this toward the end of 1967 for a sample of sixty-seven girls varying in age from twelve to twenty-two years, and all members of a village in Upolu, Western Samoa. At the time I conducted this inquiry I had been studying the village community in question over a period of some twenty-five years and had recorded and analyzed the family and kinship relationships of its members, many of whom had become my close friends. Furthermore, during that particular period of field research, I had been continuously resident in the village in question for over twenty months, and had numerous sources of information, young and old, male and female, with all of whom I was able to communicate freely in the Samoan language. My method was to make separate, discreet, and repeated inquiries about each of the sixty-seven individuals in my sample, and if, from any of my diverse sources of information, there was *any* indication of "heterosexual experience," the girl in question was listed as a nonvirgin. In other words, I took fully into account not only the overt status of the girls in question as members or nonmembers of the Ekalesia, but also all other reports, including rumors.

In any such inquiry, as in all investigations of intimate sexual behavior, there is obviously an ever present possibility of error, for no one can be privy to the clandestine behavior of others, and it is always open to individuals to lie about that which they wish to conceal. These, however, are possibilities of which I was well aware and did all that I could to circumvent.

Samoan society takes an intense and widespread interest in the virginity of adolescent girls, so that if there is the slightest evidence that a girl has had sexual contact with a male this very swiftly becomes public knowledge. Further, if a rumor of such contact is maliciously false it is commonly contested, also in public. For example, R. B. Lowe, who was the governor of American Samoa from October 1953 to October 1956, has reported a case in which am argument developed between two families regarding the virginity of a girl belonging to one of them. "The father of the girl," Lowe reports (1967:72), insisted that the Attorney General

make a statement to the effect that the girl was virgin." "This the Attorney General could not without more evidence than that brought to him by the statements of the father and the girl, so that the girl was sent," according to Lowe, "to hospital where it was established that she still retained her maidenhead., and thus she was able to become a certified virgin."

This example, like that which I give on page 233 of my book, is evidence supporting the statement made at the constitutional convention of Western Samoa in 1954 that, compared with Samoa there is "no country under the sun," where "the question of virgins" is "so upheld." This concern with virginity, and especially with the virginity of adolescent girls, is very much connected with the prohibition on premarital sexual intercourse remarked on by Professor Albert Wendt and is further evidence that Mead, somehow or other, fundamentally misreported the realities of Samoan sexual mores and behavior.[16]

It can be fairly stated that my inquiry of 1967 was conducted with both systematic care and a keen awareness of methodological and other difficulties, and that the results (while they do, as in the case of all such inquiries, contain the possibility of some degree of error) are pertinent as an approximate indication of the likely parameters of the phenomena under investigation.

The sexual experience of adolescent girls. I would particularly note that the inquiry just discussed also produced (as shown in the table on page 239 of my book) information on the extent to which adolescent girls break the prohibition against premarital intercourse, information McDowell mistakenly asserts I "never" consider. In fact, my inquiry indicates that premarital intercourse has been engaged in by about 20 percent of fifteen-year-old girls, about 30 percent of sixteen-year-old girls, and about 40 percent of seventeen-year-old girls. Thus, while in Wendt's words the sexual morality of the Samoans prohibits premarital sex and promiscuity, it is evident that departures from this strict morality do occur and to a far from inconsiderable extent. It is, however, crucial to realize that these departures are viewed--in terms of the public morality of the Samoans--as *illicit*, and are liable, if detected, to *social disapproval and punishment*, a situation that most certainly generates "storm and stress" in the lives of numerous Samoan adolescents. Samoa is thus very far from being, as Margaret Mead erroneously reported, a libertarian sexual paradise where dalliance is all.

On the value of quantitative statements. McDowell tells us that in the appendix to *Coming of Age in Samoa* Mead took the view that the "nu-

merical data" garnered from her sample of adolescent girls "were not suitable for quantitative analysis."

This is an opinion that I totally reject. Mead's conclusion in *Coming of Age in Samoa* that biological variables are of no significance in the etiology of adolescent behavior turns on what is plainly a quantitative statement: the assertion that "storm and stress" is, for the effects and purposes of Mead's inquiry, *absent* from the behavior of Samoan adolescent girls. However, as will be apparent to any percipient reader of *Coming of Age in Samoa,* this assertion is achieved by Mead's having relegated those cases in which disturbance did occur to a separate chapter and by then totally failing to make any quantitative statement about the rates of disturbance and delinquency in the sample she was studying.

This extraordinary maneuver must surely rank as one of the most unscientific to be found anywhere in the literature of the behavioral sciences, and its exposure makes it clear that Mead's main conclusion can be refuted on purely internal evidence.

It is, therefore, understandable that McDowell, in her defense of Mead, should once again attempt to deflect attention from this reality by accusing me of what she quaintly calls "statistical shenanigans." In fact, I have simply posed that most pertinent of questions: How does the rate of delinquency existing in Mead's own sample of twenty-five adolescent girls compare with the delinquency rates for adolescent girls in other countries? All that we are interested in is an approximate comparison as a test of Mead's claim that adolescence in Samoa is free from "storm and stress"; to achieve this I extrapolated a rate from Mead's sample of cases. This procedure is certainly preferable to a merely qualitative comparison and is justified because no precise conclusion is based on it, only a very general comparison.

Such extrapolations are, moreover, a standard procedure. For example, in a paper entitled "The Alleged Lack of Mental Diseases among Primitive Groups," that was published in the *American Anthropologist* in 1934 and based on information contained in *Coming of Age in Samoa,* Ellen Winston (1934:236) wrote: "Considering the five definite cases for Manu'a in terms of a population but little in excess of two thousand individuals, we arrive at a rate of mental disorder of between 225 and 250 per 100,000 of population." Winston then went on to note that in the rural U.S.A. there was a rate of mental disorder of "approximately not more than 100 per 100,000 or about the same as that of Manu'a."

When delinquency rates based on Mead's own data are compared in this same general way (as in chapter 17 of my book) it is at once revealed that adolescence in Samoa is quite as disturbed, on this criterion, as ado-

lescence in Western society, and Meads improbable assertion that in respect of adolescent behavior Samoa was a "negative instance" is seen to be unfactual.[17]

Lowell D. Holmes and Margaret Mead. In her note 37 castigating my attitude toward science, namely that scientific knowledge progresses as we succeed in eliminating error from our formulations, McDowell comes to the defence of Lowell D. Holmes. As a graduate student in anthropology from Northwestern University, Holmes did fieldwork in Manu'a in 1954, and in his Ph.D dissertation of 1957 asserted that the reliability of Mead's account of Samoa was "remarkably high." McDowell would have it that I treated Holmes "shabbily" in my book. I reject this accusation. As I thoroughly document (1983:104), Holmes' ethnographic reports, based on his fieldwork in 1954, provide "substantial grounds" for seriously questioning the validity of Mead's classing Samoa as a "negative instance." Indeed, I would argue that the evidence reported by Holmes in the 1950s indicates clearly that Samoa was definitely not a "negative instance" in the sense that Mead claimed.

In 1961, Professor Donald Campbell (1961:340) of Northwestern University observed that Holmes' findings were in "complete disagreement" with several of the broader aspects of Mead's account of Samoa. These differences, in Campbell's judgment, could not be explained by cultural change between 1926 and 1954 but had to be interpreted as "disagreement in the description of 'the same' culture."

Here then was a scientific issue of major importance. In 1967, having made a detailed study of Holmes' Ph.D dissertation, I drew his attention to a long list of the facts (reported by him) that were markedly at variance with Mead and inquired how, given these facts, he could possibly assert that the "reliability" of Mead's account of Samoa was "remarkably high."

Holmes replied (1967, pers. comm.) that while he disagreed with Mead on "many points of interpretation," he did believe that "the majority of her facts were correct." He then went on to state (these being his exact words): "I think it is quite true that Margaret finds pretty much what she wants to find. While I was quite critical of many of her ideas and observations I do not believe that a thesis is quite the place to expound them. I was forced by my faculty adviser to soften my criticisms." To which he added: "The only tragedy about Mead is that she still refuses to accept the idea that she might have been wrong on her first field trip."

We are here concerned with anthropological issues of quite fundamental importance.

Being one who believes,with Bronowski (1956:66) that in science "the test of truth is the known factual evidence" and that in respect of this most crucial of all scientific values "no glib expediency" can justify "the smallest self-deception,"I was then and am now appalled by Holmes' extraordinary admission. Indeed, his admission made it crystal clear to me in 1967 that both for the sake of Samoan studies and of anthropology it was vitally important for me to continue with my investigation of the whole context of Mead's Samoan researches. And I felt it was equally important to publish my findings when they were complete whatever might be the opprobrium and vilification from those for whom prophecy would have failed. In fact, the opprobrium and vilification on the part of some cultural anthropologists has indeed been intense, but my integrity, I would hope, remains intact, and I in no way regret behaving in this whole affair as I have behaved.

The archives of the High Court of American Samoa. As one who makes such a fuss about scholarship, McDowell should know that newspaper reports cannot be relied upon *unless independently verified.* Yet she does not hesitate to place reliance (for the purpose of impugning my veracity), on a report in the *New York Times* that is, in fact, garbled to the point of being completely false.[18]

If McDowell had referred to the preface to my book rather than such newspaper reports she would have discovered that the only claim I make there is that the researches on which my book is based "were not completed until 1981, when I finally gained access to the archives of the High Courts of American Samoa for the 1920s."

Because of the unorganized state of these archives there was no prospect in the time available to me of extracting statistical information, nor was this my objective. I was primarily seeking cases relevant to various of Mead's assertions about the sexual mores and behavior of the Samoans in the 1920s. These I did find, and they were by no means "all tangential" as McDowell, in her ignorance of things Samoan, has asserted, but rather of crucial importance in refuting certain of Mead's ethnographic errors, as will become apparent in the ensuing sections of this rejoinder.

On rape. McDowell's discussion of rape in Samoa is a particularly revealing illustration of the rhetorical devices of denial and prevarication with which she has sought to evade the cogency of my refutation. I therefore propose to discuss the issue of rape in some detail, citing empirical evidence that provides clear proof of the nature and scale of one of the most glaring errors in Mead's depiction of Samoa.

Mead's stance on rape in Samoa. According to McDowell, Mead held that in Samoa rape was "almost unknown." This is a highly misleading re-

porting of Mead's actual stance on rape in Samoa. In *Coming of Age in Samoa* (1961:93) Mead, it is true, states: "Ever since the first contact with white civilization, rape, in the form of violent assault, has occurred occasionally in Samoa." Here, however, Mead is specifically associating such rape as may have occurred in Samoa with the presence there of European males. That this was her view is confirmed by the quite unequivocal generalization she made in the very year *Coming of Age in Samoa* was published (Mead 1928:487): "The idea of forceful rape or of any sexual act to which both participants do not give themselves freely is *completely* foreign to the Samoan mind" (emphasis added).

Mead repeated this generalization in 1950 in *Male and Female,* asserting of the Samoans (Mead 1962, orig. 1950:220): "Male sexuality was *never* defined as aggressiveness that must be curbed, but simply as a pleasure that might be indulged in, at appropriate times, with appropriate partners" (emphasis added).

I would add that this unequivocal view that aggressiveness and rape were completely absent from the sexual behavior of Samoan males was also affirmed by Mead in her conversation with me in 1964, and again in correspondence in 1967. Here then we have a definite case of an unambiguous assertion by Mead, of a supposedly factual kind, that is central to her depiction of Samoa as a "negative instance" and so basic to her general conclusion of 1928.

It is a view that I myself, giving credence to Meads account, accepted as factually correct at the outset of my own researches in Samoa. Very soon, however, I became aware from newspaper reports of convictions for rape in the High Court of Western Samoa (as, for example, in the *Western Samoan Mail* of 28 September 1940 and 18 January 1941) that rape *was* indeed part of the behavior of Samoan males, and when I began to attend courts *(fono manu)* in Samoan villages, I quickly discovered that rape--both forcible and surreptitious--was, in fact, quite common. Moreover, it was apparent from reports of the proceedings of the High Court in the newspapers of those years, that cases of rape had occurred in Western Samoa throughout the 1920s. I therefore sought out Samoans who had lived in American Samoa, including Manu'a, to inquire if rape had occurred there in the 1930s and 1920s. I was assured that it had, and this assurance has been fully substantiated by all of my subsequent research, including my investigations in the archives of the High Court of American Samoa to which I shall presently refer.

On the nature of rape in Samoa. McDowell complains that I do not, in my discussion of rape, make "definitional distinctions." In fact, I cite J. M. Macdonald's *Rape Offenders and their Victims,* where any interested

reader may find that "rape is usually defined as unlawful carnal knowledge of a woman by force and without her consent" (1975:24). A somewhat fuller definition may be found in Amir's well known *Patterns in Forcible Rape* in which he states: "as a general rule the term 'forcible rape' means the carnal knowledge of a woman by a man, carried out against her will and without her consent, extorted by threat or fraudulence" (1971:17).

This definition certainly applies to the cases of forcible rape by Samoan males that I discuss in chapter 16 and is in close accord with the definition of rape under Samoan law. For example, in the Criminal Laws of 1892 of the Malietoa Government that related specifically to "offences of Samoans, not those of foreigners," it is laid down that "if any man goes by force to a woman or deceives her that she may go with him, but the woman is not thoroughly consenting, this is rape."

Although there are no statistics available for the nineteenth century, this law of 1892, applying only to Samoans, is evidence that rape was, in the nineteenth century (in contradiction of Mead's assertion of 1928) clearly recognized as existing in Samoan society. The penalties for forcible rape in 1892 were imprisonment for "not less that four nor more than eight years, with or without hard labour," or, if the body of the woman was "injured," imprisonment for "lifetime or ten years." As these penalties indicate, rape is widely regarded with abhorrence in Samoa.

In a vain attempt to defend the inaccuracies of Mead's account of Samoan sexual behavior, McDowell (note 41) has gone so far as to argue that rape in Samoa is different from rape elsewhere because "Samoan men are trying to acquire wives." As Amir (1971:131) has remarked "rape has many motives but only one intent," and the fact that some Samoan rapists have, as their motive, so they say, the acquiring of a wife, does *not* mean that the rapes they commit are not rapes in the full sense of Amir's definition.

Thus, my study of a sample of thirty-two cases of forcible rape and attempted rape showed that while threat is very occasionally sufficient to enable a Samoan rapist to carry out his criminal intent, there is, in over 90 percent of the cases, a bodily attack on the female victim. In not one case in this sample, let me add, did forcible rape result in the acquiring of a wife. Not infrequently a rapist's attack results in the infliction of major bodily injury, as I well know from having read, to my distaste, the medical reports on Samoan women who have been the victims of rape. I do not propose to cite any of these distressing reports here as evidence that forcible rape in Samoa is indeed forcible rape, but if Dr. McDowell

would like me to send to her a sample report, together with a sworn statement by the victim describing the brutal attack made upon her, I shall at once accede to her request.

I would add that Mead's totally erroneous statements about the absence of male sexual aggressiveness in Samoa have in significant ways impeded the liberation of Samoan women from male sexual violence. In these unfortunate human circumstances I regard it as deplorable that a female cultural anthropologist, as in the case of Nancy McDowell, should in an intellectually and morally frivolous way seek to condone Mead's dangerously misleading errors by a denial of the realities with which Samoan women and girls have to live.

I would further note that the prevalence of rape is a major cause of stress among unmarried Samoan women and especially among Samoan girls. An unmarried Samoan nurse, then about twenty-five years old, with whom I discussed this matter at great length in 1943, said that she could never sleep soundly when staying in a strange village out of fear of surreptitious rape, even when sleeping in a pastor's house. Also in 1943, a girl of fifteen stated that because of her fear of being raped she would never leave the immediate precincts of her village, except in the company of another girl. Indeed, all of the Samoan girls with whom I have discussed this mater have confessed to considerable anxiety at the possibility that they might be raped, and I have observed this *fefe i le toso* (fear of being raped) in Samoan girls as young as eight years of age.

On the incidence of rape in Samoa. When it came to presenting an estimate of the incidence of forcible rape in Samoa I might well have cited the judgment of Sir Charles Marsack, Chief Justice of Western Samoa during the 1950s and early 1960s. In 1964 he wrote of Samoa: "Cases of rape and attempted rape are very numerous, much more so in proportion to the population than in any country of which I have seen the criminal statistics" (1964:91). By the mid-1960s however, criminal statistics on rape had become available in the annual reports of the Police and Prisons Department of the Government of Western Samoa, and I decided to make use of these statistics to give a more exact measure of the incidence of rape in Samoa than Sir Charles Marsack's estimate of 1964. McDowell, who has no firsthand experience of Samoa, has, in her purblind defense of Mead, asserted that the statistics on which I have relied are "very dubious." This can only be described as a gratuitous insult to the Police and Prisons Department of the Government of Western Samoa, in whose methods (which in this case I have studied at close quarters) I have a high degree of confidence.

McDowell also asserts that my comparison of rape rates in Samoa with those in some other countries "violates good scientific methods" as the data I use are "not comparable." This assertion I also reject. In fact, as I have indicated, the definitions of rape in the countries concerned are genuinely comparable, and further, the comparisons I make are only of the most general kind and are intended to do no more than demonstrate that rape behavior exists in Samoa at what is unquestionably a high rate (the figure I cite for Western Samoa in 1966 is that of 60 forcible rapes per 100,000 females per annum) and is not, as Mead erroneously reported, "completely foreign to the Samoan mind."

Surreptitious rape, or moetotolo. In Samoa forcible rape is termed *tosogāfafine* (woman dragging). There is also, however, as I explain in my book (cf. p. 244ff.), a form of rape in Samoa known as *moetotolo* (sleep crawling), often called surreptitious rape in English and classed as indecent assault and a serious criminal offence by the police.

When caught, a surreptitious rapist is severely beaten by the male kin of the female he has raped and then heavily fined by the *fono* of his village. Should he be taken to the government court he is often imprisoned for several years.

This form of rape--which Mead, in her ignorance of the realities of Samoan existence, totally misconstrued, claiming that it "involved no force, only deceit"--in fact involves the forceful manual penetration by a male of a female's vagina without her consent. Thus, moetotolo, or surreptitious rape, like tosogāfafine, or forcible rape, involves what is termed *fa'amalosi* in the Samoan language or the use of force. This fact was noted by the Chief Prosecutor of the Independent State of Western Samoa when I discussed the matter with him in 1978. Furthermore, moetotolo is accompanied about 25 percent of the time by a bodily attack on the female victim, as is shown by my detailed study of a series of cases.

Moetotolo, when carried out in the way I have described, is a conspicuous example of male sexual aggressiveness, and it is thus directly relevant to my refutation of Mead's erroneous account of Samoan sexual behavior to note the annual incidence of this form of rape. Because moetotolo is peculiarly Samoan, no comparisons with other countries are, in this case, possible. However, it is certainly pertinent to note that the two forms of rape found in Samoa produced in 1966 a rate of 160 rapes--either forcible or surreptitious--per 100,000 females per annum, for this is further evidence of the gross inaccuracy of Mead's account, which is part of her fanciful depiction of Samoans as being given to "free love-making."

Rape in Samoa in the 1920s. The figures on rape just noted are from the mid-1960s when the first criminal statistics became available. What

was the situation in Samoa during the 1920s--the period to which Mead's assertions about the complete absence of male sexual aggressiveness specifically refer?

Here, as already noted, I have had also to test (by the relevant documentary evidence) Mead's supposition that there may have been during the 1920s a period markedly different from the rest of Samoan history. Prior to 1981 I had established, through my study of reports of the proceedings of the High Court of Western Samoa in *The Samoa Times*, that rape behavior occurred in Western Samoa in the 1920s. I also had obtained a number of statements from Samoan informants that this was also the case in American Samoa. This did not, however, amount to documentary evidence, which I was unable to obtain until October 1981 when I gained access to the archives for the 1920s of the High Court of American Samoa.

Because of the unorganized state of these archives, I was not interested in attempting to compile statistics, but in locating verified evidence bearing on Mead's depiction of Samoan sexual behavior in the 1920s. Nonetheless, the cases I did locate in these archives (which constitute only a small sample of the total number of cases) do prove quite conclusively that--contrary to Mead's claim--rape behavior did occur among Samoans in American Samoa in the 1920s, just as it did in Western Samoa.

For example, my investigation of the proceedings of the high courts of American and Western Samoa established that during the years 1920 to 1929, twelve Samoan males (five of them in American Samoa and seven of them in Western Samoa) were tried and convicted for forcible rape, or (in two cases) of attempted rape. I would add that my study of the pertinent court records for the 1920s is far from complete, and as the majority of rape cases are settled within the villages in which they happen, these totals of rape and attempted rape are certainly only a minor proportion of the cases that occurred in Samoa in the 1920s.

Additionally in the 1920s, in the reports of the proceedings of the High Court of Western Samoa alone, there are instances of some forty-three cases of surreptitious rape and sexual abduction and some ten cases of carnal knowledge and indecent assault.[19]

Surreptitious rape, or moetotolo in American Samoa in the 1920s. In the archives of the High Court of American Samoa I also discovered a detailed report on a case of moetotolo that occurred in the 1920s, which demonstrates conclusively the inaccuracy of Mead's account of this form of behavior.

The case, heard before the District Court at Fagatogo, Tutuila, American Samoa, 27 September, 1922, concerned the surreptitious rape of Se-

lesa, of the Lesina District of northwestern Tutuila. At the time she was raped, she had held the *taupou* title of Fuiamaono for about one year. On the night of 31 August, 1922, Selesa, who was still a virgin, retired to sleep in her father's house with an old woman of the family to guard her. At about midnight Selesa awoke to find, to her distress, that a man named Teleti, who was holding her down, had with his fingers forcibly ruptured her hymen. When asked in court whether it was not possible to "scream or shove" Teleti off of her, Selesa replied "it was impossible because my mouth was blocked by him." Selesa then described how, knowing that she had been raped and was no longer a *taupou*, or ceremonial virgin, she "sat up and weeped." She also explained that in shame at her plight she agreed to *avaga,* or elope, with her assailant by going "with him to his family."

This verified evidence from the 1920s in American Samoa demonstrates yet again how erroneous is Mead's statement that moetotolo, or surreptitious rape in Samoa "involved no force, only deceit," with a man counting on "a girl's waiting for her lover" and slipping in ahead under cover of darkness, to take "advantage of her receptivity" (Mead 1963:20).[20] Rather, as the case of Selesa indicates, moetotolo involves the unlawful and forced penetration of a female's vagina entirely without her consent and is, therefore, in all such instances, a form of rape and an undoubted instance of male sexual aggressiveness. This revealing case of surreptitious rape in American Samoa in the 1920s taken together with the totals of rape and attempted rape in both American and Western Samoa and with the cognate evidence contained in my book, are certainly more than sufficient to refute conclusively Mead's unfactual assertion that "the idea of forceful rape is completely foreign to the Samoan mind," and to demolish McDowell's ineffectual attempt to defend the validity of Mead's defective ethnography.

On rape as a social practice. Dr. McDowell, with no firsthand experience of Samoa, has nonetheless had the effrontery to charge me, a student of Samoa for more than forty years, with having gone "to extremes" in reporting that "both surreptitious and forcible rape have long been intrinsic to the sexual mores of Samoan men" and are "major elements in their sexual behavior," and for describing rape, as it exists in Samoa, as a "recognized social practice."

Here, as elsewhere in her review, McDowell is, by sheer fiat, generating her own reality by denying the pertinence of well established facts. When the adherents of a belief system let themselves fall into this state they cease to be either scientists or scholars.

As verified facts in the proceedings of the High Courts of both American and Western Samoa demonstrate, both surreptitious and forcible rape are major elements in the sexual behavior of Samoan males. A culture, as Margaret Mead wrote in 1959, "shapes the lives of those who live in it," and this process involves the social transmission of information. In 1943 when, having had its *manaia* title conferred upon me, I became a member of the *aumaga* of Sa'anapu (which was then comprised of virtually all the untitled men in that village), I was systematically instructed in many things; prominent among them were the techniques used by Samoan males in both surreptitious and forcible rape. Again, I have, in all-male groups in Samoa, on several occasions witnessed the giving of instruction in these techniques by one male to another. It is therefore a fact that while regarded with abhorrence by women and older men, rape was, nonetheless, among some Samoan males a "recognized social practice." And this remains a fact, as does the presence of both surreptitious and forcible rape in Samoan society both today and in the 1920s and earlier, however much Dr. McDowell, in her ignorance of things Samoan, seeks to deny it.

On adultery in Samoa in the 1920s. Yet another behavior in respect of which Mead's ethnograpy of Samoa is markedly at error is adultery. For example, Mead (1969, orig. 1930:84) states of Manu'a in the mid-1920s: "A man who seduces his neighbour's wife will simply have to settle with his neighbour. The society is not interested." Statements like this led Bertrand Russell in his *Marriage and Morals,* after reading *Coming of Age in Samoa*, to state quite erroneously that Samoan husbands "when they have to go on a journey, fully expect their wives to console themselves for their absence" (1961, orig. 1928: 108).

As I show in my book (cf. 1983:241-43), in Samoa adultery is regarded as a most serious offence and one about which society at large is most definitely concerned. For Dr. McDowell's information, Mead's erroneous statements about adultery were among those I tested in my investigation in October 1981 of the archives of the High Court of American Samoa.

Prior to that time I knew that Section 23, Adultery, of the *Codification of the Regulations and Orders for the Government of American Samoa* (Noble and Evans 1921) that was in force during the time Mead was in Samoa, stated: "If any man and woman not being married to each other, shall lewdly and lasciviously associate, bed and cohabit together, they shall be fined not more than one hundred dollars, or imprisoned not more than twelve months, or both."

My examination of the archives of the High Court of American Samoa showed that, during the 1920s, Section 23 was regularly enforced. For ex-

ample, in 1927, in the District Court, Fagatogo, Tutuila, it was charged than on 18 January of that year Peresetene did, in violation of Section 23, sleep with Ta‘e, the wife of Patolo. For this offence Peresetene was fined $25 and Ta’e $15. Such cases fully confirm the testimony of my informants in Manu’a who stated that in the 1920s all those found guilty of adultery were heavily fined, with in some cases “the land of an offender being taken from him.” Similar regulations concerning adultery also existed in Western Samoa, and reports in *The Samoa Times* record some forty-three convictions of Samoans for adultery during the 1920s in the High Court of Western Samoa.

It should now be obvious to Dr. McDowell that her ill-informed assertion--that it is “nonsense” for me to claim that my researches in the archives of the High Court of American Samoa provided me with conclusive evidence of anything--is quite wide of the mark. What these researches in fact provided me with, in respect of the behaviors of rape, adultery, and fornication, is verified evidence that Mead’s depiction of Samoan sexual behavior in the *1920s,* in *Coming of Age in Samoa* and her other writings, is made up of a series of flagrant errors.[21]

The Duping Issue. It is the presence of *these* errors in Mead’s writings that has, in my view, led many Samoans to give credence to the claim emanating from Manu’a that Mead must, in these matters, have been duped by her informants. Other Samoans, as has been reported by Shore (1982:213 n.2), insist with anger “that Mead lied” in her account of their sexual mores and behavior. In note 35 McDowell asserts of me: “he claims” that Mead’s informants “tricked her.” An accurate reading of my book will show that I make no such claim. In fact, after having dismissed the Samoan view reported by Shore, I discuss the report of another American cultural anthropologist, Elenor Gerber (1975:126); she was told by Samoans in American Samoa in the early 1970s that Mead’s informants “must have been telling lies in order to tease her.” Gerber’s informants, I explain, were referring to the common Samoan pastime of *taufa’ase’e* in which someone, including on occasions a visiting European, is deliberately duped.

Let me now go on to say that since my book was published another American research scientist has recorded the same kind of information as did Gerber in 1975. Thus P.A. Cox, of the Department of Botany at the University of California, Berkeley, writing in the *American Scientist* (1983:407) states:

> Several years ago during an ethnobotanical survey in Ta’ū, I asked several older Samoans for their opinions on the Samoan

> studies of Margaret Mead. They told me she could not speak Samoan; this, coupled with, "teasing," *(taufa'ase'e)* on the part of her informants, had led her into serious error in her characterization of Samoan culture. They resented some of the implications of her studies and wished that the record could be set straight.

That Samoans hold these views cannot then be doubted, and this certainly deserves to be reported and discussed. What can be said is that the claim that Mead was duped into mistakenly believing that Samoa was a paradise of freelove is highly plausible to the Samoans themselves.

However, I state (1983:291) that while it may be likely that some of the adolescent girls on whom Mead relied for information resorted to taufa'ase'e (as has been suggested in the reports of Gerber, Cox, and others), "we cannot, in the absence of detailed corroborative evidence, be sure about the truth of this Samoan claim that Mead was mischieviously duped by her adolescent informants. Moreover, because this "detailed corroborative evidence" is lacking, I completely reject Felix Wendt's complaint that I ought to accept his view that Mead was "duped," and that "she must have purposely, deliberately, and knowingly given incorrect information on Samoa."

I would emphasize then that the claim that Mead was duped is *not* a claim that I myself make, nor does it have any bearing on my refutation of Mead's depiction of Samoa, which depends on quite other evidence.

On quotation and context. Having been unable to deal effectively with the substantive content of my refutation of Mead's depiction of Samoa, McDowell has belabored what she claims is my "habit of quoting others out of context." Thus, she has given great emphasis to my allegedly "blatant" practice of citing Mead out of context on competition in Samoan society. Let us then examine this particular accusation to see if, in any significant way, it invalidates my refutation of Mead's depiction of the uncompetitiveness of the Samoans.

The instance about which McDowell so expostulates occurs on page 88 in the chapter entitled "Mead's depiction of the Samoans." In this chapter, I attempt to provide those readers unfamiliar with Mead's writings with a general summary of her depiction of Samoa before essaying, as I do in chapters 8 to 18, a detailed refutation of Mead's actual statements.

As everyone who has read the volume entitled *Cooperation and Competition among Primitive Peoples* (Mead 1937) will know, Mead classified Samoa as a markedly uncompetitive society. Mead (1937:301) refers to "two tendencies in Samoan social organizations," the first of which is "the

tendency to place each individual, each household, each village, even (in Western Samoa) each district in a hierarchy, wherein each is signified only by its relation to the whole, each performs tasks which contribute to the honor and well-being of the whole, and competition is completely impossible."

It is from this passage that I quote on page 88, and I do so because it is to this supposed tendency that Mead herself gives *markedly predominant emphasis* in her general characterization of Samoan society. For example, in 1931, in discussing the possibilities of "if not eliminating" jealousy, "at least of excluding it more and more from human life," Mead (1931:45) asserted, without mention of any countervailing tendency, that "Samoa has taken one road, by *eliminating* strong emotion, high stakes, emphasis on personality, *interest in competition*" (emphases added). This unqualified assertion by Mead that Samoan society has taken the road of "eliminating" interest in "competition," *fully justifies* my having mentioned, as I have on page 88, Mead's statement of 1937 that one of the chief characteristics of Samoan society is a form of organization that makes competition "completely impossible."

In chapter 10, entitled "Cooperation and Competition," I adduce evidence to show that there is, in fact, in Samoan social organization (in which competition is explicitly present at all levels) *no tendency,* as Mead erroneously claimed, either toward "eliminating" an "interest in competition," or toward making competition "completely impossible."

If, after having directed the reader's attention to this crucial issue in my general summary of Mead's depiction of Samoa, I had then in my detailed discussion of competition in chapter 10 failed to mention the countervailing tendency toward "rebellion of individuals" of which Mead made specific mention in 1937, I would indeed have been remiss, and McDowell would have had genuine cause for complaint. However, as readers of this rejoinder can establish for themselves by turning to page 142 of my book, I do there make *specific mention* of the other tendency noted by Mead in 1937. Rather than admitting this openly and honestly in the main text of her review, where it would have invalidated her insubstantial accusation, McDowell has relegated admission of this fact to an obscure note; in it she makes the further, and totally untrue accusation, that my full citation of Mead in chapter 10 was made "reluctantly"!

Here then, instead of concerning herself with substantive issues, McDowell is making unwarranted accusations in a futile attempt to deflect attention from the grave errors in Mead's account of competition in Samoa.[22]

As Holmes (in what McDowell has termed his "excellent ethnography") has noted of Samoa, "the whole pattern of oratory is based upon a competition in order to win prestige both for the orator himself and for the village or family he represents," and "competitive behavior and efforts to gain praise through excelling one's peers is believed to be one of the traditional aspects of Samoan culture" (1957:225-26).

That the young Margaret Mead, living in Ta'ū with an expatriate American family and relying for her information mainly on adolescent girls, should have failed to comprehend the centrality of competition in Samoan society is understandable however, for as Mead (1972:151) has noted (and as was fully confirmed, in statements to me by the chiefs of Ta'ū in 1967) she did not have, for the whole of the brief time she was in Manu'a, "any political participation in village life."

On the mistaken supposition that I claim that "Mead was all wrong." Having attempted to deflect attention from substantive issues by asserting quite falsely--as I have shown--that I do not give adequate mention to Mead's views of 1937 about competition, McDowell goes on to make the entirely false assertion that the "main point" of my book is that "Mead got her Samoan ethnographic facts all wrong." Nowhere in my book, or for that matter anywhere else, have I made such an absurd claim, for to many of the ethnographic facts reported by Mead there cannot possibly be any reasonable objection.

What I have done in my book is to present evidence showing that Mead's account of Samoa contains *sufficiently substantial and numerous mistakes and inaccuracies* to demonstrate conclusively that her extreme conclusion in respect of the etiology of adolescent behavior is *in error,* and cannot be sustained.

On male and female fieldworkers. In attempting to dismiss the significance for contemporary anthropology of my refutation of Mead and my advocacy of a more scientific anthropological paradigm, McDowell argues that my book has two main shortcomings. I shall deal with each of these in turn, beginning with McDowell's argument that because I am a man, and, as she would have it, have participated in "predominantly male events," this "had to influence" my "perspective on Samoa" and has prevented me from making a "significant contribution."

This woefully unfactual argument has been advanced by Dr. McDowell from a state of gross ignorance about the nature of my experiences in Samoa from the 1940s onwards. This ignorance has, however, in no way deterred her, for, as is the case with cultural anthropologists of her persuasion, the detailed investigation of the relevant facts is just not a consideration. A set of theoretical assumptions (as in the case of Mead in Samoa

in the 1920s, and of Bradd Shore in the 1970s, as we shall presently see) tells them *in advance of any investigation* what an answer is going to be.

In Mead's case it is known from her own statements that her information was derived mainly from adolescent girls, and that, as Mead has specifically stated (1969:228) it was from the "vantage point" of the adolescent girl that she "saw" Samoan society. This was because of the problem Boas had given her to study, and if Mead had stayed on in Manu'a for more than just a few months, she certainly could have widened her perspective and learned more than she did of the preoccupations of men.

In particular, it is a complete non sequitur to suppose, as has McDowell, that because a fieldworker is a man he thus participates in "predominantly male events"; nor does it remotely follow that being a male cuts one off from contact with females.

In my own case, as a young man in Western Samoa in the early 1940s, having had a *manaia* title conferred on me (cf. 1983:235), I was afforded contact with many young Samoan women, some of whom, as I was able to speak their language fluently, became my very close friends. Indeed, it was through my firsthand experiences with some of these young women that I first became aware of the facts that demonstrate the errors of Mead's account of Samoan sexual behavior and values.

During my years in Samoa, from the early 1940s onward I have observed firsthand on numerous occasions the activities of the *aualuma,* and of all the other women's groups in Samoan society. And, because I have found them such intelligent and knowledgeable informants, much of my time has been spent in the company of middle-aged women like the forty-four year old daughter of a titular chief whom I mention on page 219. Again, during the years 1966-1967, I spent much time in detailed studies of the psychology of young girls, using techniques learned at the London Institute of Psychoanalysis from Dr. D.W. Winnicott and others.

Given these facts, which I can substantiate in detail if required, I can only dismiss as unfactual, ideological, and sexist the extraordinary argument to which McDowell has resorted, while deploring that loose thinking of this kind, which goes under the rubric of "the sociology of knowledge," has become quite common in cultural anthropology in recent years. In most instances, as in the present case, it is, rather, "the sociology of ignorance."

On complexity and Dr. B. Shore. McDowell's second argument is also the product of her great ignorance of things Samoan. I have failed, she claims, to comprehend "the complexity of human sociocultural behavior." This is a ludicrous claim for my whole refutation of Mead depends on my

having documented numerous aspects of Samoan behavior that were ignored by Mead in creating her romantically fanciful picture of Samoa.

Yet, neglecting this fundamental point, McDowell at once goes on to argue that the "complexity" that eludes me has been grasped by an American cultural anthropologist, Dr. Bradd Shore, whose book, *Sala'ilua: A Samoan Mystery*, is, in McDowell's judgment, "superb": "readers who want to learn about Samoans should read Shore's book not Freeman's." According to McDowell, Shore has "subtly and deftly" resolved the "discrepancies" between "Mead's point of view and Freeman's."

Nothing, in fact, could be further from the truth, for Shore's book--which is, if anything, a more extreme exemplification of cultural determinism than was Mead's *Coming of Age in Samoa*--contains at its center an egregious error and quite fails to come to terms with the "mystery" it purports to explain.

According to Shore, most of what he finds "valuable in anthropology" he has derived from Professor David Schneider, and in the main analytical section of *Sala'ilua* we are in fact dealing with Schneider's notion (1980, orig. 1968:1) of "culture as a symbolic system purely in its own terms," and with what Shore calls "the power of cultural templates to guide action and shape experience."

This concept of culture as a "template" is another version of the notion, on which Benedict and Mead so relied, of culture as a "mold." Shore prefaces his analysis of what he calls the "fundamental Samoan categories of action" by a series of citations from Mead, on whose shoulders he has recently described himself as standing (Shore 1983), and by suggesting (1982b:153) that by her interpretation of Samoan culture Mead was committed to a "paradigm" essentially similar to that which he himself has adopted. In fact, Shore goes well beyond Mead in his avowal of "the power of cultural templates" by purporting to explain an impetuous and violent murder by one drunken chief of another in terms of "cultural structures."

Shore's view of culture, like Schneider's (1980:135), is emphatically dualistic; and central to his whole argument is an analysis of what, so he claims, is "really a kind of Samoan ideology distinguishing human nature and culture." Indeed, Shore asserts that the "nature/culture distinction, which Lévi-Strauss has made famous in anthropology as a basic intellectual problem underlying many social institutions is an important Samoan assumption."

According to Shore this assumption, which is evinced in "a fundamental cultural template . . . for ordering contexts," is expressed in two basi-

cally important "categories," the Samoan terms for which are *āmio* and *aga,* with *āmio* referring to nature, and *aga* to culture. It is in these terms that Shore's whole analysis proceeds.

This, no doubt, sounds entirely convincing to someone like Dr. McDowell who has no specific knowledge of Samoa. However, as I demonstrate in detail in a review article entitled "The Burthen of a Mystery" (soon to appear in the journal *Oceania*) Shore has the basic connotations of āmio and aga, in terms of which his whole anlaysis is couched, completely reversed. I do not know of another error of this magnitude in the entire ethnographic literature on Samoa, or indeed, in the ethnographic literature at large.

This means, ineluctably, that Shore's account of the "dual structure" on which he predicates his "distinctively anthropological solution" of the murder he is trying to explain is, by being based on erroneous information, fundamentally flawed. Further, Shore's account of this murder, as I show in my review article, is in various respects seriously defective.

We thus have in Shore's *Sala'ilua: A Samoan Mystery* a telling example of how a cultural anthropologist with an enthusiasm for a particular doctrine (as, for instance, a form of dualism "popular with anthropologists") joined with a belief in "the power of cultural templates," is apt to find exactly what he, or she, is hoping to find--as has happened before in the history of the beguiling islands of Samoa.

Sir Edmund Leach (1983:478), in his review of my book, found it a pity that I was "so solemnly committed to the revelation of the scientific truth," and suggested that I might have "made my points just as well by writing "a satire on the frailty of academic researchers." This is a possibility I did at one stage consider before deciding that such an approach would bring down on me too great a measure of ire. I must, however, confess that the spectacle of the worthy Dr. McDowell, in her impassioned defense of Mead, extolling as an object lesson to me this "superb" book that is both culturally deterministic to the hilt and flawed by a quite egregious error, is truly comic and a fit subject for the kind of satire that Edmund Leach had in mind.

On "the errors in Mead's interpretation" of Samoa. According to McDowell "the errors in Mead's interpretation have nothing to do with Boas or cultural determinism" but with an inability by Mead to incorporate theoretically "the notion of contradiction." This is both a breathtaking denial and a demonstrably false special pleading.

As I have documented earlier in this rejoinder, we know from Mead's own testimony, which she repeated several times,[23] that she went to Samoa in 1925 to investigate at the behest of Franz Boas "to what extent

the storm and stress of adolescence" is "biologically determined and to what extent it is modified by the culture within which adolescents are reared." And we also know from Mead's own testimony (1977: 19) that she regarded Samoa as "a most felicitous choice" for the investigation of this particular problem, and that in chapter 13 of *Coming of Age in Samoa* she reached the extreme conclusion that biological variables are of no significance whatsoever in the etiology of adolescent behavior.

Boas accepted this conclusion without question, and in his *Anthropology and Modern Life,* published in 1928 a few months after Mead's *Coming of Age in Samoa,* stated without qualification that in Samoa "the adolescent crisis disappears" (p. 186). A few years later in *Patterns of Culture* (1945, orig. 1934:21) Ruth Benedict, Mead's other mentor at Columbia University, declared that among Samoan girls the adolescent period was "quite without turmoil."

As I have already remarked and as Raum long ago noted, these assertions are contradicted by Mead's own account, for at least four of her sample of twenty-five adolescent girls were delinquents. This means that in the mid 1920s, delinquency, with its attendant storm and stress, was present among Samoan adolescent girls at about as high a level as has been established for Samoa in later decades and for other twentieth century societies for which delinquency rates are available such as the United States and Australia.

In *Coming of Age in Samoa* (1961:157) Mead, as we have already seen, diverts attention from this reality by relegating her delinquent girls to a separate chapter and by arbitrarily excluding them from her theoretically all-important generalization that in Samoa adolescence represents "no period of crisis or stress." This, however, is a conspicuously unscientific maneuver, for the delinquent girls described by Mead are obviously *as much the products of the Samoan social environment* as are the other members of her sample.

It is, then, a matter for continuing astonishment, in view of the evidence provided by Mead (1961: chapter 11), that Benedict could have asserted without qualification that among Samoan girls the adolescent period was "quite without turmoil," and that virtually the entire anthropological establishment, following the lead of Boas and Benedict, came to give credence to this demonstrably erroneous conclusion. Its fatal appeal was that it so advantageously confirmed a pre-existing assumption.

As I point out (p. 86) Mead was obliged by the logic of her central argument "to depict the whole social life of Samoa as being free of happenings that might generate tension and conflict"--and it is from this situation that her erroneous depiction of Samoa really stemmed.

In some instances this involved (as can be demonstrated, once again, by internal evidence) a neglect of known facts that amounts to the suppression of crucially significant data, as the following examples show.

On affrays and the like. In her depiction of the ease and casualness of their society, Mead, as I have already noted, gave special emphasis to the "unaggressiveness" of the Samoans, describing them as "one of the most amiable, least contentious and most peaceful peoples in the world." As I have also indicated earlier in this rejoinder, an invaluable source of information on American Samoa in the mid 1920s is the manuscript journal of A. F. Judd, who, with an expedition from the Bernice P. Bishop Museum, did research in Manu'a early in 1926 at the same time as Mead was carrying out her own inquiries there. In the course of these researches Judd made a brief visit to the island of Ofu, which had long been in a state of emnity with the people of the island of Ta'ū, among whom Mead was then living.

In his journal, Judd (1926:78) describes a recent incident in which a new pastor, Iakopo, arriving in Ofu was "literally stoned" out of the village by the people, who resented the treatment to which their former pastor had been subjected.

Such an incident, involving a violent attack on a Christian pastor, is most serious for Samoans. It is also the clearest possible evidence of contentiousness and aggression. That Mead knew of this affray is obvious from her letter dated Ta'ū, 16 January 1926 in which she mentions (Mead 1977:47) as proof of the fact that she was "becoming a part of the community," that she had argued with members of the *aumaga* of Ta'ū about the advisability of "burning down" what was left of Ofu after the devasting hurricane of 1 January, 1926, "because the people of Ofu stoned the meddlesome pastor of Ta'ū" out of their village. Further, Mead was acquainted with the pastor who had been stoned, for she mentions him as the Samoan pastor Iakopo in the acknowledgements in *Coming of Age in Samoa.* Finally, in March 1926, Mead visited Ofu and so was in a fully favorable position to investigate and report in detail on the affray of which she had prior knowledge.

That this was not done is characteristic of Mead's whole approach to the problem she had been given by Boas, which was to make of Samoa, as she has admitted, a "negative instance." Yet as must be obvious to even the most doctrinaire cultural anthropologist, if Mead had fully reported the affray that, from Judd's evidence, we know took place in Ofu while she was in Manu'a, together with the history of the severe conflict between the people of Ta'ū and those of Olosega and Ofu (cf. Freeman 1983:169), she could not possibly have made the erroneous statements she

did about the "unaggressiveness" of the Samoans, and her so-called "negative instance" would have been revealed as no negative instance at all.

On suffering for one's convictions. Another of the ingredients of Meads depiction of Samoa as a "negative instance" was her claim (1961:198) that Samoa was a place where "no one suffers for his convictions." This claim, as I demonstrate (1983:270ff.), is directly contradicted by the facts of Samoan history, including the history of the 1920s. Thus not long before the period of Mead's fieldwork, when a number of chiefs of Ta'ū defied the naval government by conferring the august title of Tui Manu'a on Christopher Taliutafa Young, they told Governor Kellogg--after he forcibly quashed what they had done--that they were "dissatisfied to the death" with his interference in their affairs. The acting district governor of Manu'a at this time was Sotoa, a high chief of Lumā on the island of Ta'ū, and Governor Kellogg, holding him (Gray 1960:208) to be "primarily at fault," suspended Sotoa from office. This action by Governor Kellogg, which Sotoa considered to be unjust and which he deeply resented, was borne by him with dignity. Some six years later when the American Samoa Congressional Commission of 1929-1930 visited Manu'a (1931:217), Sotoa, when giving evidence on 2 October 1930, reiterated that it had been "unanimously agreed" by himself and the other chiefs of Ta'ū to confer the Tui Manu'a title on Christopher Taliutafa Young and roundly criticized, Governor Kellogg for his action in interfering in the affairs of Manu'a and banishing Christopher Taliutafa Young to the island of Tutuila.

Here then we have a clear instance of a major political confrontation, which was still in progress at the time of Mead's researches, and in which the high chief Sotoa most certainly suffered for his convictions, as did Christopher Taliutafa Young. Furthermore, there is certain evidence that Mead was aware of what had befallen Sotoa, whom she knew well; in *Social Organization of Manu'a* (1969:167) she refers to a dream reported to her by Sotoa, that he had had "before the political trouble resulting from the attempt to reinstate the Tui Manu'a." Once again, if Mead had investigated and reported the nature of this "political trouble," she could never have included as one of the ingredients of her depiction of Samoa as a "negative instance" the quite erroneous generalization that Samoa is a place where "no one suffers for his convictions."[24]

Thus, however much McDowell might wish to deny it, there exists the clearest evidence that the errors of Meads depiction of Samoa are indeed associated both with the problem she had been set by Boas and with the doctrine of cultural determinism of which she, like Boas, was a principal proponent.

Concluding remarks. As we have now seen McDowell has failed to make substantive points about Samoa that, in any significant way, weaken the cogency of my refutation of Mead. Rather, I have presented decisive new evidence to strengthen this refutation.

Again, even McDowell's most emphatic allegations of misquotation (as in the cases of Boas on genetics and Mead on competition) turn out, when factually analyzed, to have been misconstrued or misrepresented by McDowell herself.[25]

What then of her more general comments occurring at both the outset and conclusion of her review?

According to McDowell, my book "has almost no general or constructive relevance to contemporary anthropology." This is woefully to misunderstand the significance of refutation in the progress of a science. As Sir Karl Popper has shown, a science progresses by the elimination of error from its formulations, so that, as Charles Darwin remarked in 1879, "to kill an error is as good a service as, and sometimes even better than, the establishing of a new truth or fact" (1903:II,422).

The extreme conclusion reached by Mead in 1928, to which McDowell and many other cultural anthropologists have long given uncritical credence, is, as I have noted, scientifically preposterous, and the *formal* refutation of this conclusion is, therefore, of fundamental anthropological importance.

McDowell is entirely in error, furthermore, in asserting that my "entire argument" rests on the assumption that "in refuting Mead's negative instance" I have demolished "forever the idea that adolescence is not necessarily a period of stress." As McDowell for some reason fails to record, Mead's "negative instance" was in fact, as I have documented, used to support the more specific and extreme conclusion that biological variables are of no significance in the etiology of adolescent behavior. All that my refutation of Mead's erroneous depiction of Samoa does is to demonstrate that the case of Samoa can no longer be advanced, as it has for so long by cultural anthropologists, to justify the doctrine of cultural determinism.

There is, however, as McDowell has failed to mention, *no* logical connection between my refutation of Mead's classing of Samoa as a negative instance, and my advocacy, on general scientific grounds, of an interactionist paradigm for anthropology. As McDowell correctly notes it is certainly open to any cultural anthropologist to advance some other "negative instance" in proof of the assertion that biological variables are of no significance in the etiology of adolescent behavior and in support of the extreme doctrine of cultural determinism in which Mead and others believed in the 1930s.[26]

This, however, is a most unlikely happening for today, as Stephen Jay Gould has recently remarked, "Every scientist, indeed every intelligent person knows that human social behavior is a complex and indivisible mix of biological and social influences" (1983:6). And if this be true--and all the relevant evidence indicates it is--then the day of the "negative instance," which has proved in the case of Samoa to be entirely nugatory, as well as the day of the cultural determinism that Mead and others once championed, is indeed over.

In this situation, given the present state of scientific knowledge, anthropology has really no rational alternative but to move toward a fully interactionist paradigm of the kind adumbrated in the final chapter of my book.

So decisive has been the advance in knowledge during recent decades that, as Ashley Montagu indicated in 1979, there is no longer any rational justification for belief in "the *tabula rasa* myth." We are indeed evolved primates, and the time has come when it is incumbent on all behavioral scientists--including cultural anthropologists--to acquaint themselves with and to take fully into account all of the phylogentically given elements in our behavior of the kind that is summarized in, say, Richard Passingham's *The Human Primate* (1982).

When this is done, within an interactionist paradigm, it then becomes possible to analyze and explain cultures in a much more scientific way than is open to doctrinaire cultural anthropologists. For when it is realized that cultures are the products of human choice, it becomes possible to relate particular cultural choices to the evolved primate nature of those who have enacted them, and in this way quite new light is cast on other phenomena of cultural differences.

In his classic book of 1937 Dobzhansky declared: "It is a demonstrable fact that human biology and human culture are parts of a single system, unique and unprecedented in the history of life" (1937:304). Since that time the truth of this declaration has become ever more apparent, and it is, in the words of Peter Corning (1983:151), "increasingly evident that the life sciences and the social sciences must converge on an Interactional Paradigm."

It is to the realization of this most important of objectives for all the human sciences that my book is a contribution, and I have no doubt at all, knowing what I do of the progression of science, that the convergence of which Corning writes will indeed eventually occur.

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NOTES

1. In her book *My Samoan Chief* (1975:18) Fay Ala'ilima records that her Samoan husband, who was born in Western Samoa and lived from the age of twelve onward in American Samoa, completely disagrees that Samoan adolescence is not "a period of 'sturm and drang'."

2. In the preface to my book I also note that in August 1978 I offered to send Dr. Mead an early draft of my refutation of the conclusions she had reached in *Coming of Age in Samoa,* but that "I received no reply to this offer before Dr. Mead's death in November of that year." McDowell has chosen to see in this an "innuendo." She is quite mistaken. Let me for Dr. McDowell's information describe the circumstances in further detail. In August 1978, as soon as I had completed a first draft of a chapter of my book (i.e. the present chapter 16), I wrote to Dr. Mead asking if she would like to see this draft. In reply I received a letter dated New York, 14 September 1978, in which I was informed by an assistant, Amy Bard, that Dr. Mead had "been ill," and that if Dr. Mead had "an opportunity to read and comment on my manuscript" I would be notified. I heard no more from Dr. Mead's office before her death in November 1978. Early in 1979 in a letter to Amy Bard I wrote of Dr. Mead's "unfortunate death," that I considered it "a true loss" to have her "wise and challenging voice forever stilled." I had not then, nor have I now, any knowledge of the course of Dr. Mead's final illness.

3. As Barash (1982:160) has noted: "When we describe and seek to understand the natural world, we are not seeking to condone it." Again, as Barash also points out (p. 161), an understanding of "the biological factors that influence our behavior" may "even provide us with greater 'free will', by making us more aware of our own hidden tendencies, so that we may seek to resist them, if we wish."

4. McDowell asserts it to be "odd" that I should, in my book, have concentrated "almost exclusively on American anthropology." This is by no means odd, as I am concerned with the work of American anthropologists. However, as I note on page 313: "Inasmuch as it has, in accordance with Durkheimian precept, totally excluded biological variables, social anthropology in Great Britain and elsewhere, despite various differences in emphasis, has operated within the same basic paradigm as American cultural anthropology."

5. Jenning's classic book of 1930, *The Biological Basis of Human Nature,* had the utmost relevance to the issues discussed by Boas both in the 1938 edition of his *The Mind of Primitive Man* and in the section entitled "Biological Premises" in his *General Anthropology,* also of 1938. It is mentioned in neither place.

6. McDowell, quoting from page 32 of my book, claims that I have drawn an unwarranted conclusion from a passage I cite from Stocking (1968:264). This is by no means the case. Stocking correctly refers to the whole thrust of Boas' thought as being to "separate biological and cultural heredity." This means that McDowell is wrong in asserting Stocking says "nothing" about Boas "denying biology." I must insist that he does, for in separating biological and cultural heredity, Boas (as anyone who has studied his thought will know) was denying the relevance of biological variables in large areas of human behavior where, in fact, they undoubtedly do operate.

7. Boas' lack of exact knowledge of genetics is apparent in his use of the terms genotype and phenotype, as in his paper of 1925 in *The Nation,* entitled "What is a Race?" In this pa-

per Boas follows the erroneous definitions of these terms that appeared in the 1914 edition of Funk and Wagnall's *New Standard Dictionary of the English Language.* They were subsequently corrected by G. II. Shull (1915:56ff.) in the *American Naturalist.*

8. There is a similar dismissive discussion of natural selection by Boas (1963, orig. 1911:97 in chapter five of his *The Mind of Primitive Man.*

9. Mead says "remembering Stevenson's rhapsodies" on p. 147 of *Blackberry Winter.*

10. I thank Dr. McDowell for having pointed out the following errata in my book: p. 24 (line 12 from bottom of page), *for* path of truth *read* path to truth; p. 89 (line 10 from bottom of page), *for* aggression *read* aggressiveness; p. 93 (line 2 from bottom of page), there should be a dash (--) between "more" and "primitive." These are all transcription errors, none of which, fortunately, alters in any significant way the sense of the excerpts being quoted.

I would also draw attention to these other errata, all of which will be corrected in future editions of my book: p. xiv (line 15 from top of page), *for* forgathered *read* foregathered; p. 121 (line 8 from bottom of page), *for* of constitution *read* or constitution; p. 176 (line 10 from bottom of page), *for* comonly *read* commonly; p. 227 (line 2 from top of page), *for* permarital *read* premarital; p. 238 (line 2 from bottom of page), *for* obseve *read* observe; p. 246 (line 9 from bottom of page), *for* bisucits *read* biscuits; p. 249 (line five from bottom of page), *for* 1938 *read* 1928. On pp. 219, 346, and 374, *for* Fenichal *read* Fenichel.

11. I would add that Richard Goodman *in an entirely independent inquiry* reaches conclusions about Mead's erroneous depiction of Samoan behavior that are virtually identical with my own.

12. In his Ph.D dissertation, "A Restudy of Manu'an Culture," Holmes (1957:15) did not hesitate to use accounts of 'Western Samoa in the mid-nineteenth century as "an early base line" for the analysis of "Manu'an culture."

13. I do not criticize Mead, as McDowell claims, "for not writing a book on Samoan his tory." I would, however, criticize her for making assertions about Samoan history (as, for example, about warfare) without ever having consulted the relevant historical manuscripts.

14. In note 33, McDowell refers to the statement of Gerber's Samoan informants in the early 1970s that "things used to be stricter than they are today." McDowell is mistaken in asserting that I just accept this statement "at its face value." I well know, from intensive firsthand experience, that things were considerably stricter in the early 1940s than in the mid-1960s, and all the relevant historical evidence confirms this.

15. I might mention in this context that when, on 17 September 1967 I interviewed Fa'alaulā, of Ta'ū, Manu'a, who was then seventy-seven years of age and who had been a close associate of Mead in 1925-1926, she claimed that she told Mead of *moetotolo,* or surreptious rape: *"E sā, sā lava ona faia se mea fa'apēnā"* (It is forbidden, most forbidden, to do a thing like that).

16. McDowell's complaint is unjustified that in referring (as on p. 234 of my book) to "the cult of virginity" in Samoa, I do not address "the issue of the nature and definition of 'cult' " and that my usage of this term is "clearly at odds with accepted anthropological practice." I am using the term "cult" in one of its accepted dictionary meanings (cf. *Random House Dictionary)* to refer to "an instance of great veneration of a person, ideal of thing; esp. as manifested by a body of admirers."

17. McDowell's claim that I have a "flagrant disregard" for my source Katchadourian (1977) is a complete non sequitur for I do not suppose, nor do I anywhere argue, that adolescent stress is either inevitable or universal.

18. The fact that McDowell's note 38 is based on a garbled newspaper report, containing totally erroneous information, makes this note entirely misconceived and irrelevant.

19. These cases are taken from reports of the proceedings of the High Court of Western Samoa that appeared during the 1920s in *The Samoa Times.* These reports are by no means complete. Further, there is a marked diminution in the cases of sexual abduction and carnal knowledge reported from 1927 onward during the political disaffection that was then rife in Western Samoa. For example, while twenty-three cases of sexual abduction were reported as having been before the High Court during the years 1924-1926, only nine such cases were so reported during the years 1927-1929. In Samoa sexual abduction, as explained on page 246 of my book, follows a *moetotolo,* or surreptitious rape, and is so referred to in court actions.

20. Successful personation by a rapist is indeed an extremely rare event. The only case I have come across in my researches on rape is reported on pages 148-149 of *Crime in New Zealand* (Department of Justice, Wellington, New Zealand, 1968).

21. McDowell, in note 37, asserts that I go to "ridiculous lengths" in stating that Mead returned from the field "with tales running directly counter to all other ethnographic accounts of Samoa," yet she gives no examples to falsify my statement. Let me then repeat that, to the best of my knowledge, the tales that Mead brought back to New York in 1926 about aggression, warfare, competition, premarital sexuality, adultery, rape, and not a few other aspects of Samoan existence, indeed did run counter to all other then existing ethnographic accounts of Samoa.

22. I also reject McDowell's argument that because Mead's comment--that a Samoan who "feels strongly" is maladjusted--was taken from Mead's *Sex and Temperament in Thee Primitive Societies* (1935) it is very much out of context. As I know from my conversation with Mead in 1964 and from other sources, her views about Samoa did not change over the years, for after June 1926 she did no further substantial research on Samoa. Thus her reference of 1935 is a repetition of the view she had expressed in *Coming of Age in Samoa* in 1928.

Again, the summary I give (pp. 93-94) of Mead's depiction of adolescence in Samoa, to which McDowell has objected, is, in my judgment, both reasonable and fair. The crucial issue here is Mead's assertion that in Samoa adolescence is "the age of maximum ease." Further, the fact to which McDowell directs attention, namely that Mead justifies her assertion by claiming that an adolescent is near some supposed "center of pressure," is not worthy of mention because it is an unwarranted and false supposition.

23. For example, in *Coming of Age in Samoa* (1961, orig. 1928:11), Mead described the "question" that "sent" her to Samoa as: "Are the disturbances which vex our adolescents due to the nature of adolescence itself or to the civilization?" In her paper "Cultural Contexts of Puberty and Adolescence" (1959:60) Mead wrote: "The problem which he [Boas] sent me to Samoa to study concerned the extent to which the well-known vicissitudes of adolescents in our society were dependent upon the physical changes through which they were passing or upon other nature of the culture in which they grew up." And, in her paper "Retrospects and Prospects" (1962:122) she records that Boas persuaded her to "undertake the study of the relative strength of biological puberty and cultural pattern."

24. Again, if Mead had inquired at the High Court when she was in Pago Pago, as she was during both September and October 1925 and again in May and June 1926, she could readily have established that rape did indeed occur among Samoans and might even have discovered the case of Selesa (which I have described) and thus corrected her inaccurate account of *moetotolo.* While Mead spells the name Sotoa correctly in the acknowledgements of *Coming of Age in Samoa,* it is spelled incorrectly as Soatoa on page 167 of *Social Organization of Manu'a* (1969, orig. 1930).

25. I apologize to the readers of *Pacific Studies* for the great length of this rejoinder and can only plead that this has been necessitated by the many points raised by Dr. McDowell. Even so, I have dealt only with the issues to which Dr. McDowell has given special emphasis and have considered not a few of her points to be altogether too trivial to warrant serious discussion. Although this rejoinder has been written under great pressure against a deadline. I have striven for accuracy at all times. I would also add that in writing it I have been conscious of the fact that since 1981 I have held the positions of Foundation Professor of Anthropology and Consultant on Samoan Studies at the University of Samoa.

26. McDowell cites a letter from the *New York Times* of 6 February 1983, in which Murphy, Alland, and Skinner state concerning Mead's conclusion about adolescence in Samoa: "whatever may be the Samoan facts, subsequent research in other parts of the world has sustained her essential theoretical stance." This is untrue. No other cultural anthropologist. to the best of my knowledge, has reached the same extreme conclusion about adolescent behavior that was reached by Mead in 1928.

REFERENCES

Allen, G. E.
1972 Introduction to the Reprint Edition of *The Mechanism of Mendelian Heredity* (orig. 1915) by T. H. Morgan, A. H. Sturtevant, H. J. Muller, and C. B. Bridges. New York.

1979 (orig. 1975)
Life Science in the Twentieth Century. Cambridge, England.

American Samoa: Hearings before the Commission Appointed by the President of the United States. 1931. U.S. Government Printing Office. Washington, D.C.

Amir, M.
1971 *Patterns in Forcible Rape.* Chicago and London.

Barash, D. P.
1982 *Sociobiology and Behavior.* London.

Barnouw, V.
1979 *Culture and Personality,* 3rd ed. Homewood, Ill.

Benedict, R.
1945 (orig. 1934)
Patterns of Culture. London.

Boas, F.
1916 "Eugenics." *The Scientific Monthly* 3:471-78.

1925 "What is a Race?" *The Nation* 120:89-91.
1928 *Anthropology and Modern Life.* New York.
1963 (orig. 1911)
The Mind of Primitive Man. New York.
1939 "Genetics and Environmental Factors in Anthropology." *The Teaching Biologist* 9:17-20, and 45.
1940 (orig. 1935)
"The Tempo of Growth of Fraternities." In *Race, Language and Culture.* New York.
1940 (orig. 1932)
"The Aims of Anthropological Research." In *Race, Language and Culture.* New York.

Boas, F. ed.
1938 *General Anthropology.* New York.

Bronowski, J.
1965 (orig. 1956)
Science and Human Values. New York.

Calkins. F.
1975 *My Samoan Chief.* New York.

Campbell, D. T.
1961 The Mutual Methodological Relevance of Anthropology and Psychology. In F. L. K. Hsu, ed. *Psychological Anthropology.* Homewood, Ill.

Cook, O. F.
1907 "Aspects of Kinetic Evolution." *Proceedings of the Washington Academy of Sciences* 8:197-403.

Corning, P.
1983 *The Synergism Hypothesis: A Theory of Progressive Evolution.* New York.

Cox, P. A.
1983 "Margaret Mead and Samoa." *American Scientist* 71:407.

Darwin, C.
1903 *More Letters of Charles Darwin.* F. Darwin and A. C. Seward, eds. 2 vols. London.

Department of Justice, New Zealand
1968 *Crime in New Zealand.* Wellington, N.Z.

Dobzhansky, T.
1957 (orig. 1937)
Genetics and the Origial of Species. New York.

Duff, R. Interview in the Christchurch *Press,* 23 November 1937.

Dunn, L. C.
1927 "The Theory of the Gene--a Review." *Journal of Heredity* 18:22-24.

Festinger, L., H. W. Riecken, and S. Schachter
1964 (orig. 1956)
When Prophecy Fails. New York.

Fisher, R. A.
1930 *The Genetical Theory of Natural Selection.* Oxford.

Freeman, D.
1972 "*Social Organization of Manu'a* (1930 and 1969) by Margaret Mead: Some Errata." *Journal of the Polynesian Society*. 81:70-78.
1983 *Margaret Mead and Samoa: The Making and Unmaking of an Anthropological Myth.* Cambridge, Mass.
forthcoming "The Burthen of a Mystery." *Oceania.*

Gerber, E. R.
1975 "The Cultural Patterning of Emotions in Samoa." Ph.D diss. University of California, San Diego.

Gladwin, T.
1961 "Oceania." In *Psychological Anthropology*. F. L. K. Hsu, ed. Homewood, Ill.

Goodman, R. A.
1983 *Mead's Coming of Age in Samoa: A Dissenting View*. Oakland, California.

Gould, S. J.
1983 "Genes on the Brain." *The New York Review of Books* 30 (11):5-9

Gray, J. A. C.
1966 *Amerika Samoa.* Annapolis.

Herskovits, M. J.
1943 "Franz Boas as a Physical Anthropologist."American Anthropologist, N.S. Memoir 61:39-51.

Holmes, L. D.
1957 "A Restudy of Manu'an Culture: A Problem of Methodology." Ph.D. diss. Northwestern University.

Huxley, J.
1949 *Heredity East and West.* New York.

Jennings, H. S.
1927 "Review of *The Theory of the Gene* by T. H. Morgan." *The Nation* 124:184.
1930 *The Biological Basis of Human Nature.* New York.

Judd, A. F.
1926 Expanded notes, ethnology, etc. American Samoa, February 15-April 2, Islands of Tutuila, Ofu and Ta'ũ. MSS. Bernice P. Bishop Museum Library, Honolulu.

Katchadourian, H.
1977 *The Biology of Adolescence.* San Francisco.

Kluckhohn, G., and O. Prufer
1959 "Influences during the Formative Period." In *The Anthropology of Franz Boas.* W. Goldschmidt, cd. *American Anthropologist* N.S. Memoir 89:4-28.

Konner, M.
1928 *The Tangled Wing: Biological Restraints on the Human Spirit.* New York.

Kroeber, A. L.
1916 "Inheritance by Magic." *American Anthropologist* 18:19-40.
1943 "Franz Boas: The Man." *American Anthropologist* N.S. Memoir 61:5-26.
1956 "The Place of Boas in Anthropology." *American Anthropologist* 58:151-59.

Lawick-Goodall, J. van
1971 *In the Shadow of Man.* London.

Leach, E.
1983 "The Shangri-la that Never Was." *New Society* (24 March 1983): 477-78.

Lowe, R. B.
1967 *Problems in Paradise.* New York.

Macdonald, J. M.
1975 *Rape: Offenders and Their Victims.* Springfield, Ill.

McDowell, N.
1980 "The Oceanic Ethnography of Margaret Mead." *American Anthropologist* 82:278-302.

Marsack, C.
1964 *Samoan Medley.* London.

Mayr, E.
1982 *The Growth of Biological Knowledge.* Cambridge, Mass.

Mead, M.
1928 "The Role of the Individual in Samoan Culture." *Journal of the Royal Anthropological Institute* 58:481-95.
1937 "The Samoans." In M. Mead, ed. *Cooperation and Competition among Primitive Peoples.* New York.
1959 "Cultural Contexts of Puberty and Adolescence." *Bulletin of the Philadelphia Association for Psychoanalysis.* 9:59-79.
1961 (orig. 1928)
Coming of Age in Samoa. New York.
1962 (orig. 1950)
Male and Female. Harmondsworth.
1962 "Retrospects and Prospects." In *Anthropology and Human Behavior,* T. Gladwin and W. C. Sturtevant, eds. Washington, D.C.
1969 (orig. 1930)
Social Organization of Manu'a. Honolulu.
1972 *Blackberry Winter.* New York.
1977 *Letters from the Field 1925-1975.* New York.

Mead, M. ed.
1937 *Cooperation and Competition among Primitive Peoples.* New York and London.
1962 *An Anthropologist at Work.* New York.

Morgan, T. H.
1919 *The Physical Bask of Heredity*. Philadelphia and London.
1929 "Lamarckism." *Encyclopaedia Britannica,* 14th. ed. 13:607-10.
1964 (orig. 1926)
The Theory of the Gene. New York.

Morgan, T. H., A. H. Sturtevant, H. J. Muller, and C. B. Bridges
1972 (orig. 1915)
The Mechanism of Mendelian Heredity. New York and London.

Noble, A. M., and W. Evans
1921 *Codification of the Regulations and Orders for the Government of American Samoa.* San Francisco.

Passingham, R.
1982 *The Human Primate.* Oxford and San Francisco.

Radin, P.
1939 "The Mind of Primitive Man." *The New Republic* 98:300-303.

Raum, O. F.
1967 (orig. 1940)
Chaga Childhood. London.

Russell, B.
1958 (orig. 1928)
Marriage and Morals. London.

Schneider, D. M.
1980 (orig. 1968)
American Kinship: A Cultural Account, 2nd ed. Chicago.

Schoeffel, P.
1979 "Daughters of Sina: A Study of Gender, Status and Power." Ph.D diss. Australian National University.

Shore, B.
1982a "Sexuality and Gender in Samoa: Conceptions and Missed Conceptions." In S. Ortner and H. Whitehead, eds. *Sexual Meanings.* Cambridge, England.
1982b *Sala'ilua: A Samoan Mystery*. New York.
1983 *In Barnard Bulletin,* 92 (9), April 13.

Shull, G. H.
1915 "Genetic Definitions in the New Standard Dictionary." *American Naturalist* 49:52-59.

Stocking, G. W., Jr.
1968 *Race, Culture and Evolution*. New York.

Vidich, A. J.
1966 "Introduction." In *The Method and Theory of Ethnology* by Paul Radin. New York and London.

Wendt, A.
1983 "Review of *Margaret Mead and Samoa* by D. Freeman." *Pacific Islands monthly* 54:10-12, 14, and 69.

Williams, J. J.
1936 "Boas and American Ethnologists." *Thought* 11:194-209.

Winston, E.
1934 "The Alleged Lack of Mental Diseases among Primitive Groups." *American Anthropologist* 36:234-38.

John Garrett, *To Live Among the Stars: Christian Origins in Oceania.* World Council of Churches in association with the Institute of Pacific Studies, University of the South Pacific, Geneva and Suza, 1982. Pp.412, maps, illustrations, bibliography, index.

John Garrett has undertaken the Herculean task of telling the story of the process of Christianization in the Pacific, from its earliest beginnings with Catholic Spain's exploration of the Marianas in 1521 to the twentieth-century development of a regional identity, all in a single volume of thirteen chapters. Very wisely he has limited himself to accounts of those churches within the Pacific Conference of Churches; to do otherwise would have been quite unmanageable. Perhaps he may be encouraged to consider another volume covering the rest of the Christian missions and other religious sects in the Pacific as a challenging project for the next few years.

A chronology of events that happened in the various mission fields in the Pacific during the nineteenth and twentieth centuries could not have been too difficult to compile. But a critical analysis of the reasons underlying both the successes and failures of the various attempts to convert the Pacific islanders was an extremely complex and difficult task, because of the vastness of the region geographically and the diversity of its peoples. Throughout this book, Garrett has incisively demonstrated how local cultures helped to shape the development of Christian churches throughout the Pacific, giving them their distinctive local flavor. Another contributing factor, namely the diverse backgrounds of the various missions and missionaries, is also critically discussed. This required a high degree of scholarship, patience, industry, and sympathetic understanding, and the author does not disappoint us. *To Live Among the Stars* is not merely scholarly, it is readable and written with obvious sympathy and understanding. But no attempt is made to whitewash or glamorize those who brought the Gospel from overseas, and due acknowledgment is given to the role played by Pacific islands missionaries; both their failures and successes are well documented.

Inevitably, of course, a work of this scope has to be selective and cannot treat every topic in its full complexity. However, since mass conversion and mission training institutions have been well covered in other

areas of the Pacific, some discussion of other subjects should have been included: for example, the mass conversion of the Maoris of New Zealand to Christianity after extremely stiff resistance for more than a decade and a half; and the success of the famous Te Aute College in producing outstanding Maori scholars, politicians, and community leaders. Considering also the importance of the Roman Catholic Mission in Papua New Guinea, it is regrettable that Garrett concentrates only on the Papuan mission and neglects the Catholic work in the New Guinea islands and mainland. Although the role of politics in local conflicts involving missionaries has been well treated in many areas of the Pacific, it is not given sufficient attention in other regions. In Tonga, for example, Garrett perpetuates the myth started by Peter Dillon (and later made widely known by Basil Thomson's *Diversions of a Prime Minister*) that the wars in Tonga in 1837, 1840, and 1852, were provoked by missionaries—particularly by the chairman, the Rev. John Thomas. According to Dillon's account, Tāufa'āhau, the hero of these wars, was Thomas's pawn (76), and Tāufa'āhau and the Christian soldiers "had engaged in indiscriminate vengeance under orders" (77) presumably from Thomas. This somewhat superficial discussion of the situation neglects to mention that the so-called missionary wars were a continuation of the political struggle for power that had begun in Tonga long before the arrival of the Wesleyan missionaries. The acceptance of one of Dillon's charges at face value is also regrettable since Dillon had a personal grudge against the missionaries, was extremely biased and untruthful, and was determined to destroy John Thomas and the mission. His later efforts to encourage the establishment of a Roman Catholic mission in Tonga were a continuation of his malicious efforts in this respect.

The above criticisms are of a very minor nature and do not in any way detract from the overall merits of the book. In addition, I am presenting here the following factual and typographical errors for consideration in the publication of a second edition: Ma'afu had already established himself in the Lau Group in 1848 (114) and not after the Battle of Kaba in 1855 (80). *Lotu Tonga* was introduced to Samoa in 1828 and not in 1832 (122). Tonga has only one constitution—promulgated in 1875 (274)—not two. The so-called first Constitution of 1862 (273) is only a code of laws. The first President, not Chairman (275) of the reunited church, the Free Wesleyan Church of Tonga, was a Tongan, Setaleki Manu, and not Page. The United Church in Papua New Guinea was established in 1968 and not 1962 (303).

Each mission has produced its own history from time to time. After World War II, anthropologists and professional historians, particularly

missionary historians such as Professor C. W. Forman, Professor A. R. Tippett, and Dr. A. H. Wood, began to produce more objective studies of the Christian missions in the Pacific. This trend has been enhanced considerably by the works of scholars from the Department of Pacific and Southeast Asian History, Australian National University, Canberra, concentrating either on a particular island region or on a single missionary society. Most of these scholars have been supervised by the distinguished Pacific mission scholar Dr. Neil Gunson, and most of these academic studies, including Gunson's own scholarly works, have been published.

What was needed was a broader overview bringing some of this research together in a single study. John Garrett's book fulfills this need and will undoubtedly become the major textbook of the history of Christianity in the Pacific for many years to come. The inclusion of local maps, a most comprehensive bibliography, and an extensive index enhances the value of the volume as a textbook. The publishers must also be congratulated for producing the book for a price that is within the reach of students in the Pacific. *To Live Among the Stars* will be an invaluable addition to the library of anyone with interest in the Pacific or the history of Christian missions.

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T. J. Macnaught, *The Fijian Colonial Experience: A Study of the Neo-Traditional Order under British Colonial Rule Prior to World War Two.* Canberra: Australian National University Press, 1982. Pp. 203. $14.95.

The British governance of Fiji began in 1874 with a degree of idealism in the ethic of trusteeship which doctrinaire critics of colonialism are reluctant to believe. From its title, one might expect this book to be an analysis of that experiment in colonial trusteeship, which has been such a rarity in the history of domination. The theme has great possibilities, especially considering the repeated pronouncements about guided social change from a tribal organization, economy, and value system to that of a modern state: an exercise in simultaneous protection and development; a massive experiment in education. The experiment fell rather short of the ideal image, and the history of that divergence during the ninety-six years of colonial rule should be one of enormous interest.

Macnaught's treatment of his subject, however, lacks the continuity of this theme. His book is not a history of colonial Fiji, nor is it a study in colonial theory and practice. Neither can it be "a study of the neo-traditional order" without being these other things as well. Consequently, a reader wishing to gain a comprehensive knowledge of Fijian affairs during the colonial period will not get it from this book. That is to be regretted all the more because he will not get it anywhere else either.

Instead, what Macnaught presents is a comparatively static picture of Fijian society at some ill-defined time in the first half of the twentieth century. His portrait of colonial Fiji is constructed by the presentation of a succession of themes, presented in such a manner that each chapter is virtually a self-contained essay. Each essay is interesting in its own right, but has only a tenuous connection with the others. Thus the reader will look in vain for a framework of periodization, and in consequence, the discussion is apt to become confusing. For example, in treating the Fijian discontent with British rule, Macnaught implies that the agitations of Apolosi Nawai kept the villagers in ferment for a quarter of a century rather than merely for a few months in 1915. Similarly, the technique of discussion by themes leads to distracting repetition: the question of the reform of land tenure in the first decade of the century is discussed in chapters one and two, and is broached again in Chapter Eight, "Compromise for a Multi-Racial Society."

In not providing a continuous narrative, the author has had to assume that the reader is well acquainted with Fijian twentieth-century history, on which so little has been published. Familiarity with names, circumstances, and developments--in short with the whole context of this study--is taken for granted. At the same time, those readers with a specialist knowledge of twentieth-century Fiji will probably find the book disappointing for the lack of aggressive, persistent analysis of the themes it broaches. The method of analysis is on the whole anecdotal and exemplary, and while this may be understood as possibly reflecting the nature of the sources, it is nonetheless true that the reader does not acquire a comprehensive, detailed knowledge of the subject. The book thus falls between two categories, satisfying the needs of neither the specialist nor the nonspecialist.

Readers with particular interests will find parts of the book useful; however, on matters of emphasis and interpretation (with which they might disagree) they will not find the exposition sufficiently comprehensive to allow an evaluation of Macnaught's interpretations. For example, what is one to conclude on discovering that the author's Fijians

are rational, sensible people--or at least that their actions and motives are comprehensible--but that his Europeans in Fiji behaved in ways that were wrongheaded and inexplicable?

The book encompasses the following subjects: the attempts by Governors O'brien and Im Thurn to reform village life and the land tenure system; Im Thurn's attempts to undermine the hereditary nobility in the interests of democracy and administrative efficiency; the continued political awareness and machinations of aristocratic officials; village life (which sounds like a structural-functionalist ideal model); the career of Apolosi Nawai as a nationalist revolutionary ahead of his time; the Fijian discontent with their colonial and undeveloped status (which fits badly with the portrayal of village life); the failure to develop a truly multiracial society (oddly called "Compromise for a Multi-Racial Society"); and experiments in transforming Fijians into a people who would apply the development ethic more pertinaciously. Unfortunately, it is my opinion that none of these issues is dealt with in sufficient depth, nor with sufficient attention to the broader context of Fijian affairs. Macnaught's best chapter is his epilogue (which actually falls outside the scope of his study) surveying the major developnnents and themes in Fijian history after 1940. The usefulness of all chapters is further impaired by the absence of an index.

It is nearly forty years since Derrick's history of Fiji in the nineteenth century was published. That book remains unsuperseded. Derrick did not achieve his goal of writing a sequel for the colonial period, and unfortunately the subject still awaits its historian.

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Mac Marshall, ed., *Through a Glass Darkly: Beer and Modernization in Papua New Guinea.* Boroko, Papua New Guinea: Institute of Applied Social and Economic Research, 1982. Pp. xxiii, 482, tables, appendices, bibliography, glossary. PNG K15.00.

In recent decades Melanesian anthropology has been challenged to demonstrate how research provides practical payoff for those we study. This volume, with contributions by nearly forty different writers, is the heftiest response to date and goes some distance toward showing what anthropologists have to say about practical problems facing contemporary Papua New Guineans.

The question of alcohol use is bound to be controversial, and it is probably for this reason as much as any other that so little has been written about the topic in Papua New Guinea. Discussions of alcohol seem inevitably to conjure lurid visions of drunkenness and dissolute behavior. In Western history alcohol abuse epitomizes our sense of social evil, and often becomes part of arguments concerning moral decay, class oppression, and so on. As a social issue, drinking is unavoidably political. This is all the more true in the Third World where it is enmeshed in histories of colonial domination.

Alcohol has always played a part in the colonial situation. Sale of alcohol to aboriginal peoples in much of North America and Australia is synonymous with exploitation and symbolizes the subversion of indigenous society at the hands of outsiders. At the same time, the use of alcohol often served to segregate the colonizers from the colonized. V. G. Kiernan, a historian of colonialism, has argued that the nineteenth-century empires could not have been won except at the cost of pervasive alcoholism among administrators and colonial officials. This was symptomatic of elite status as defined by luxury consumption and social isolation in exotic locales. It is thus not surprising that legislation restricting sale of alcoholic beverages to local people in colonial territories had an inherently ambiguous nature: envisaged by many (especially missionaries) as a measure protecting local people from some of civilization's ills, it also smacked of paternalism while establishing what were effectively sumptuary laws on a par with those forbidding the use of European clothing. In such circumstances alcohol is at once emblematic of danger, power, and prestige, a character it retains in the contemporary Pacific scene.

In Papua New Guinea prohibition of alcohol sales to local people was a morally offensive token of colonialism, especially to a growing indigenous urban elite. Lifting this ban in the 1960s was seen as a signpost toward parity with expatriates and is a major public symbol of political independence. It is thus no exaggeration to say that the use of alcohol in Papua New Guinea carries with it overtones not only of modernity but of emancipation as well, and for this reason it remains important in the ideology of nationhood. But if this is so, there is nonetheless public concern with problems popularly thought to be associated with the use of alcohol. It is here that the collective results of ethnography have something to contribute, and the essays in this collection provide what must surely be the most comprehensive coverage of alcohol use for any comparable region in the world.

As is to be expected from a collection this large and covering such a diverse area, the resultant overall picture is complex. Despite this, how-

ever, some consistent patterns emerge. Many of these are detailed in the conclusion by Marshall, Piau-Lynch, and Sumanop. One of these is the near-universal perception of alcohol as a commodity whose consumption carries with it associations of luxury, sophistication, and success. As such, it is particularly appropriate as a vehicle for expressions of self-esteem and achievement of modernity. At the same time it is also neatly fitted into more traditional attitudes associated with prestige, production, and festivity; here beer serves as a ceremonial gift in largely public settings. Drinking in these contexts seems to encourage both conviviality and contention, but the general pattern suggests that the presence of the community as a whole serves to limit unruly behavior. Though the picture remains complicated, particularly with regard to private drinking and domestic violence, it would seem that many of the behavioral problems we associate with drinking are more likely to become acute in circumstances of alienation and atomization.

Traditional economies of Papua New Guinea place a premium on distribution rather than accumulation, and increased involvement in the cash economy suits beer especially well to a role in local prestige transactions. A number of papers demonstrate the ways in which ceremonial exchanges of alcoholic beverages mediate between the cash economy and the prestige economy. This is particularly true of the central highlands where the prosperity of the coffee boom often gets channelled into extensive collective presentations, liquidating large cash accumulations while transforming them into social debts and credits. Here it seems clear that the Western ideal of capital accumulation takes a back seat to the purchase of luxury commodities such as beer and imported foods. One implication of this is that purchases of beer for distribution may come to motivate cash-crop production while discouraging the emergence of permanent class differences, raising the issue of the relation between local values and the cash economy. Although such questions are not resolved in the collection as a whole, they receive good coverage throughout the set. Here the book makes a major contribution to our knowledge of the impact of money on rural economies in Papua New Guinea.

One of the book's strengths is that it goes out on a limb and offers concrete policy recommendations concerning the availability of alcohol in rural areas. A number of these are likely to be contentious, but they have the virtue of being clearly phrased and based upon a sober analysis of a broad range of empirical material. The book's weaknesses are relatively minor and are mainly sins of omission. A stronger analysis of the role of missions would seem called for since alcohol has always been near the top of the list of missionary concerns for local welfare. Given the fact that al-

cohol often found its way into the lives of local people through expatriates, a closer look at European drinking patterns is also appropriate (here Poole's paper stands out), The fact that beer is a near-proverbial feature of town life whets one's appetite for a promised volume on alcohol in urban settings. Finally, a book this size deserves an index to help the reader through so many different pieces. All of these complaints merely serve to underscore the fact that this collection addresses a need that we have only barely began to satisfy, and we should all hope to see more work of this kind in the future.

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Donald Kilolani Mitchell, *Resource Units in Hawaiian Culture.* Honolulu: Kamehameha Schools, 1982. Pp. 300, illustrations. $18.95.

Resource Units in Hawaiian Culture is a revised and expanded version of a book that first appeared in 1969. Since its first release, it has undergone four reprintings and has become a classic in the teaching of Hawaiian studies. Two new chapters have been added to the original work, a chapter on Hawaiian warfare and weapons and a chapter on the land and the people. Additionally, it is greatly enhanced by a new design format and original artwork. According to the book's foreward, "Emphasis was placed on including illustrations of Hawaiian artifacts not found in other Hawaiian culture texts." Indeed, many of the illustrations are themselves great pieces of art and would do credit to any art gallery. The book is impressive for the quantity as well as quality of its artwork, a detail distinguishing it from most of the Hawaiian culture texts presently on the market.

Its greatest virtues, however, are its index (usually inadequate in other similar works) and the extensive bibliographies that follow every chapter. This bibliographic format is particularly helpful as it includes many of the newer texts and resources in Hawaiian studies that have become available during the last twenty years. This is of great importance to Hawaiian scholars, as most of the texts currently being written contain relatively no new information or insight, and usually present repackaged data that quite often were old, out of date, and erroneous when they first appeared in print. For example, the chapter on religious beliefs and practices also contains references to scholarly research conducted within the last few

years, some of it refuting contentions made by earlier scholars now shown to have been incomplete, wrong, or both. Additionally, the chapter makes reference to, but does not necessarily agree with, some present-day writers whose scholarship is suspect and whose views are more popular than they are profound.

One thing to keep in mind when reading this book is its intended audience: Hawaiian studies students as well as teachers. This is both helpful and disconcerting. For someone who knows very little about Hawaiian culture, the book is a treasure trove of information and insight. For the serious student of Hawaiian culture there is an almost condescending attitude throughout the book that assumes its reader's total ignorance of things Hawaiian. But this is only a personal perception. Whether justified or not it is hardly a serious flaw and does not diminish the overall excellence of the book.

One of the book's best units is the newly added, "The Land and the People." This book is worth acquiring on the merits of this unit alone. It is a simplified, detailed, carefully crafted, and up-to-date (1980) explanation and review of one of the least understood and most hotly debated topics in modern Hawaii: Hawaiian charitable trusts. Very few people in Hawaii can go to one source to find information about the history and scope of these trusts as they operate in Hawaii today. Additionally, legal terms such as "adverse possession" are explained in nonlegal and nontechnical language.

Kilolani Mitchell and the Kamehameha Schools are to be commended for writing about things Hawaiian without showing the biases that quite often characterize such works. The book is written with skill, humor, sparkle, and aplomb, making it a pleasure and a joy to read--unusual qualities among books on Hawaiian subjects.

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Louise Morauta, John Pernetta, and William Heaney, eds., *Traditional Conservation in Papua New Guinea: Implications for Today*. Boroko, Papua New Guinea: Institute of Applied Social and Economic Research, Monograph 16, .1982. Pp. 392. K 4.50.

The fourth national goal of the constitution of Papua New Guinea asserts the need to conserve the country's natural resources, and to use them

wisely for the present benefit and for that of future generations. This volume of papers is based on a conference held in Port Moresby in October 1980, which brought together a vast range of specialists interested in conservation of all kinds. It includes over forty papers, some by technical experts, some by planners, and some by Papua New Guineans themselves, whose expertise lies in their unique view of conservation and knowledge of their home areas. The information presented is too diverse for individual comment, yet many common themes emerge from the papers.

The most crucial theme in my view is how to balance development and conservation. A number of papers relate to this issue. Papua New Guinea as a nation is both in a unique position and at a critical juncture of its history. It can benefit from the developmental follies of other countries by acting to safeguard its resources from overexploitation at too rapid a pace and cost. But as in other developing countries, both the government and local individuals need revenue. How to weigh these inherently contradictory desires is only part of the problem dealt with in many of the conference papers; the issue runs deeper still, to individual rights and freedom versus national goals and needs.

A second major theme concerns traditional knowledge of the environment and how traditional practices affected resource conservation. There are certain differences of opinion here. Some papers argue that cultural practices and values had a conservational element to them, that taboos and access to resources were partially "about" conservation after all, and left to their own devices "traditional societies" were sensitive to resource limitations and practiced restraint in the use of valued resources. Conservation in many papers is seen as a latent function of cultural fact. Other papers suggest that, while local knowledge of resources was great and detailed, traditional practices had serious nonreversible environmental impact. Some faunal species were extinct in pre-colonial times through human overexploitation; erosion, burning, and other practices threatened livelihood in pre-European days. While these two positions conflict, surely the real issue is how to bring together both internal and external expertise and knowledge to confront such problems and solve them in the future.

A third major theme that is manifest throughout partially relates to the first: the relationship between "external" pressures and "internal" ones. How are the development projects of multinational corporations, courted by the national government, to be reconciled with local sentiment, values, and resource use? Can logging operations and the sanctity of land be made compatible? This seems to be the most complex issue ad-

dressed at the conference, and although there are no solutions offered, Papua New Guineans themselves are the most vociferous and clearminded of the contributors.

This volume is a sourcebook of great importance. If it is short on specific recommendations for solving problems, it is nonetheless an ambitious attempt to bring people, viewpoints, and issues together. The clear message of the conference is that essential to any conservation solutions is the active participation of Papua New Guineans in their own destiny. External experts with all their scientific knowledge will never be sufficient, for the solutions must be sought and phrased in the values held by Papua New Guineans. Without that most crucial component in any solution, attempts to solve environmental and conservational problems will certainly fail.

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David Weisbrot, Abdul Paliwala, and Akilagpa Sawyerr, eds., *Law and Social Change in Papua New Guinea.* Sydney: Butterworths, 1982. Pp. 319, bibliography, index.

Among the major issues confronting newly independent nations is the question of an appropriate and well-functioning legal system. For Papua New Guinea, the question was considerably more complex, especially in view of the decolonial context in which such an undertaking was initiated. To that effect, the contributors to this much needed work have accomplished what they intended to do in capturing "the flavour of the exciting legal developments in Papua New Guinea of the past decade."

Broadly speaking, a formalized legal system serves two major purposes. First, to resolve public and private disputes according to established social principles. Second, to mete out justice for offences against society. In effecting both purposes, the legal system assumes the role of an agent of social control, thus acquiring profound political significance in the process. The debate over the role of customary law in the national *corpus juris* reflected the underlying ambivalence of the political decision makers. In the opinion of nationalists John Kaputin and Father John Momis, law was "no longer to be a colonial fraud, but a genuine expression of the felt needs and aspirations of our Melanesian people." Bernard Narokobi advanced similar ideas as a member of the Constitutional Plan-

ning Committee (CPC), arguing that "if Independence was to mean anything, we must free ourselves from the imposed web of laws, built up over the years, based upon special conditions in England and Australia." Legal conservatives, on the other hand, resisted the recognition of custom as the underlying principle of law, advocating instead that the common law and equity would better promote economic development, particularly in the "state's promotion and the self-advancement of the big peasantry." Though the CPC urged the recognition of customary law as the principle source of legal authority and the relegation of Anglo-Australian law to secondary roles, the administration of the constitutional scheme resulted in a reverse situation.

The integration of customary law with the criminal justice system proved to be a "major problem area." The role of customary law made minor inroads in the constitutional and judicial system, being considered in limited situations where the "reasonable man" test was applicable in provocation issues as well as in sentencing procedures. The Law Reform Commission, established to "develop a new Melanesian jurisprudence," proposed that the courts be required to "ascertain and apply customary law whenever possible." While it was forthrightly assumed that the village court magistrates would be well acquainted with local custom, the public legal service had "little training in or feeling for customary law." The complexities of not only interpreting but also incorporating custom into the national legal system may be mitigated through the "expansion of the role and jurisdiction of officially recognized customary dispute settlement agencies."

The policy that a nation follows in the administration of land is of central significance, both as an indication and determination of its social and economic development strategy. The inherited national land administration system reflected the aims and attitudes of the preceding colonial administration. The report of the 1973 Commission of Inquiry into Land Matters "stressed the fundamental importance of land as the basis of social, political and economic relations." The report also "strongly challenged the established system of land administration, and asserted priorities which threatened the privileges of dominent commercial entities in the country which that system had protected." Corollary legislation, particularly the Land Disputes Settlement Act were attempts to "balance traditional flexibility with the increasing need in some areas for greater certainty in land rights."

One of the more provocative legal issues involved family law. Perhaps in no other matter did the conflict between custom and common law become more apparent. Under the European system, marriage was a con-

tract between individuals, while native custom considered marriage to be essentially an alliance between kin groups. Under European law, the interests of the children are the primary concern of the courts, while custom weighs the concerns of the kin group in child custody disputes. The reconciliation of these two divergent systems of jurisprudence will require more reflective consideratian and more decisive action.

This well-edited volume describes and discusses the "significant legal events of the period both as to the initiation and development of reform proposals and their ultimate fate." Among the many underlying conclusions that may be drawn from the essays is the nature of issues raised in a new nation attempting to cope with change while still committed to the traditions of the past. The inherent ambivalence in the constitutional debates and policy making reveals the problems of modernity itself and the will to fashion a new order in the allocation of power and authority set in a familiar context of customary institutions.

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