EDITOR'S FORUM

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

Robert L. Welsch
The Field Museum
Chicago

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

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

Pacific Studies, Vol. 19, No. 3--September 1996

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

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

Toward Collaboration and Regional Studies in Melanesia

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Regional and Collaborative Research in Melanesia

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

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

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

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

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

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

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

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

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

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

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

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

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

The New Guinea Micro-Evolution Project: A Collaborative Project

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

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

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

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

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

The Micro-Evolution Project's Research Questions and Conclusions

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

The Micro-Evolution Project's Design

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Historical Background and Initial Reconnaissance

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

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

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

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

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

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

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

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

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

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

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

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

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

Social Networks, Ethnographic and Prehistoric

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

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

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

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

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

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

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

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

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

Conceptualizing Social Fields

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

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

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

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

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

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

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

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

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

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

Inherited Friendships

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

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

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

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

Interactive Networks and Diversity

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

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

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

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

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

The Persistence of Diversity

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

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

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

Conclusion

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

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

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

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

NOTES

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

REFERENCES CITED

Barlow, Kathleen
1985 The Role of Women in Intertribal Trade among the Murik of Papua New Guinea. *Research in Economic Anthropology* 7:95-122.

Bateson, Gregory
1936 *Naven: A Survey of the Problems Suggested by a Composite Picture of the Culture of a New Guinea Tribe, Drawn from Three Points of View*. Cambridge: Cambridge University Press.

Berndt, Ronald M.
1954-1955 Kamano, Jate, Usurufa, and Fore Kinship of the Eastern Highlands of New Guinea. *Oceania* 25:23-53, 156-187.
1962 *Excess and Restraint: Social Control among a New Guinea Mountain People.* Chicago: University of Chicago Press.
1965 The Kamano, Usurufa, Jate, and Fore of the Eastern Highland. In *Gods, Ghosts, and Men in Melanesia: Some Religions of Australian New Guinea and the New Hebrides,* edited by P. Lawrence and M. J. Meggitt, 78-104. Melbourne: Oxford University Press.
1973 Political Structure in Eastern Central Highlands of New Guinea. In *Politics in New Guinea: Traditional and in the Context of Change, Some Anthropological Perspectives,* edited by R. M. Berndt and P. Lawrence, 381-423. Seattle: University of Washington Press.

Biersack, Aletta, editor
1991 *Clio in Oceania: Toward a Historical Anthropology*. Washington: Smithsonian Institution Press.

Burridge, K. O. L.
1960 *Mambu: A Melanesian Millennium.* London: Methuen.
1969 *New Heaven, New Earth: A Study of Millenarian Activities.* New York: Schocken Books.

Chowning, Ann
1958 Lakalai Society. Doctoral thesis, University of Pennsylvania.
1965 Lakalai Kinship. *Anthropological Forum* 1:476-501.

Chowning, Ann, and Ward Goodenough
1966 A Flint Industry from Southwest New Britain, Territory of New Guinea. *Asian Perspectives* 9:150-153.
1973 Lakalai Political Organization. In *Politics in New Guinea: Traditional and in the Context of Change, Some Anthropological Perspectives,* edited by R. M. Berndt and P. Lawrence, 113-174. Seattle: University of Washington Press.

Clark, William C.
1966 From Extensive to Intensive Shifting Cultivation: A Succession from New Guinea. *Ethnology* 5:347-359.
1971 *Place and People: An Ecology of a New Guinean Community*. Berkeley: University of California Press.

Damon, F. H., and R. Wagner, editors
1989 *Death Rituals and Life in the Societies of the Kula Ring*. DeKalb: Nor-them Illinois University Press.

Du Toit, Brian M.
1975 *Akuna: A New Guinea Village Community*. Rotterdam: A. A. Balkema.
1994 A New Guinea Research Experience. Paper presented at the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

Evans-Pritchard, E. E.
1937 *Witchcraft, Oracles, and Magic among the Azande*. Oxford: At the Clarendon Press.
1940 *The Nuer*. Oxford: At the Clarendon Press.

Finsch, Otto
1888 *Samoafahrten, Reisen in Kaiser-Wilhelmsland und Englisch-Neuguinea, in den Jahren. 1884-1885 an Bord des deutschen Dampfers "Samoa."* Leipzig: F. Hirt & Sohn.

Firth, Raymond
1936 *We, the Tikopia: A Sociological Study of Kinship in Primitive Polynesia*. London: George Allen and Unwin.
1939 *Primitive Polynesian Economy*. London: G. Routledge and Son.
1940 *The Work of the Gods in Tikopia*. London: P. Lund Humphries for London School of Economics.

Foley, W. A.
1986 *The Papuan Languages of New Guinea*. Cambridge: Cambridge University Press.

Fortes, Meyer
1945 *The Dynamics of Clanship among the Tallensi*. London: International African Institute and Oxford University Press.

Fortune, Reo F.
1932 *Sorcerers of Dobu: The Social Anthropology of the Dobu Islanders of the Western Pacific*. New York: E. P. Dutton.
1935 *Manus Religion: An Ethnological Study of the Manus Natives of the Admiralty Islands*. Philadelphia: American Philosophical Society.

Friederici, Georg
1912 Wissenschaftliche Ergebnisse einer amtlichen Forschungs-reis nach dem Bismarck-Archipel im Jahre 1908. II. Beiträge zur Völker- und Sprachenkunde von

Deutsch-Neuguinea. *Mitteilungen aus den Deutschen Schutzgebieten, Ergänzungsheft,* number 5 (324 pp.).

1913 Wissenschaftliche Ergebnisse einer amtlichen Forschungs-reis nach dem Bismarck-Archipel im Jahre 1908. III. Beiträge zur Völker- und Sprachenkunde von Deutsch-Neuguinea. *Mitteilungen aus den Deutschen Schutzgebieten, Ergänzungsheft,* number 7 (182 pp.).

Friedlaender, Jonathan, with William Howells and John G. Rhoads, editors

1987 *The Solomon Islands Project: A Long-Term Study of Health, Human Biology, and Culture Change.* Oxford: At the Clarendon Press.

Gewertz, Deborah B.

1983 *Sepik River Societies: A Historical Ethnography of the Chambri and Their Neighbors.* New Haven: Yale University Press.

Goldman, Irving

1970 *Ancient Polynesian Society.* Chicago: University of Chicago Press.

Goodenough, Ward H.

1952 *Ethnological Reconnaissance in New Guinea.* University Museum Bulletin [Pennsylvania] 17 (1): 5-37.

1956 Some Observations on the Nakanai People. *Annual Report and Proceedings, 1954, Papua and New Guinea Scientific Society (Port Moresby):* 39-45.

1957 Oceania and the Problem of Controls in the Study of Cultural and Human Evolution. *Journal of the Polynesian Society* 66 (2): 146-155.

1961 Migrations Implied by Relationships of New Britain Dialects to Central Pacific Languages. *Journal of the Polynesian Society* 69 (2): 112-126.

Gregory, Derek, and John Urry, editors

1985 *Social Relations and Spatial Structures.* New York: St. Martin's Press.

Haddon, Alfred Cort

1906 A Plea for the Investigation of Biological and Anthropological Distributions in Melanesia. *Geographical Journal* 28:155-163.

1920 Introduction to Unexplored New Guinea, by Wilfred N. Beaver, 7-9. London: Seeley, Service & Co.

1924 Introduction to *In Primitive New Guinea,* by J. H. Holmes, 3-14. New York: G. P. Putnam's Sons.

1927 Introduction to *The Kiwai Papuans of British New Guinea,* by Gunnar Landtman, ix-xx. London: Macmillan.

1936 Introduction to *Papuans of the Trans-Fly,* by F. E. Williams, xxiii-xxxiv. Oxford: At the Clarendon Press.

———, editor

1901- *Reports of the Cambridge Anthropological Expedition to the Torres Straits.*
1935 Cambridge: Cambridge University Press. 6 volumes (volume 1: General Ethnography; 2: Physiology and Psychology; 3: Linguistics; 4: Arts and Crafts; 5: Sociology, Magic, and Religion of the Western Islanders; 6: Sociology, Magic, and Religion of the Eastern Islanders).

Harding, Thomas G.
1967 *Voyagers of the Vitiaz Strait: A Study of a New Guinea Trade System.* American Ethnological Society Monograph 44. Seattle: University of Washington Press.

Hays, Terence E.
1990 Mountain-Ok Bibliography. In *Children of Afek: Tradition and Change among the Mountain-Ok of Central New Guinea,* edited by B. Craig and D. Hyndman, 169-197. Oceania Monograph 40. Sydney.
1994 Discussant's comments for the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

Hocart, A. M.
1922 The Cult of the Dead in Eddystone of the Solomons. *Journal of the Royal Anthropological Institute* 52:71-112, 259-305.
1929 *The Lau Islands, Fiji.* Bernice P. Bishop Museum Bulletin 62. Honolulu: Bernice P. Bishop Museum.

Hogbin, H. Ian
1934/1935a Native Culture of Wogeo: Report of Fieldwork in *New* Guinea. *Oceania* 5:308-337.
1934/1935b. Trading Expedition in Northern New Guinea. *Oceania* 5:375-407.

Howells, William
1987a Preface to *The Solomon Islands Project: A Long-Term Study of Health, Human Biology, and Culture Change,* edited by J. Friedlaender et al., v-vii. Oxford: At the Clarendon Press.
1987b Introduction to *The Solomon Islands Project: A Long-Term Study of Health, Human Biology, and Culture Change,* edited by J. Friedlaender et al., 3-13. Oxford: At the Clarendon Press.

Kirch, Patrick V., and Roger C. Green
1987 History, Phylogeny, and Evolution in Polynesia. *Current Anthropology* 28:431-456.

Kirch, Patrick V., and D. E. Yen
1982 *Tikopia: The Prehistory and Ecology of a Polynesian Outlier.* Bernice P. Bishop Museum Bulletin 238. Honolulu: Bishop Museum Press.

Knauft, Bruce M.
1993 *South Coast New Guinea Cultures: History, Comparison, Dialectic.* Cambridge Studies in Social and Cultural Anthropology 89. Cambridge: Cambridge University Press.

Landtman, Gunnar
1927 *The Kiwai Papuans of British New Guinea.* London: Macmillan.

Lawrence, Peter
1964 *Road Belong Cargo: A Study of the Cargo Movement in the Southern Madang District, New Guinea.* Manchester: Manchester University Press.
1973 Introduction to *Politics in New Guinea: Traditional and in the Context of Change, Some Anthropological Perspectives,* edited by R. M. Berndt and P. Lawrence, 1-34. Seattle: University of Washington Press.

Lawrence, P., and M. J. Meggitt
1965 Introduction to *Gods, Ghosts, and Men in Melanesia: Some Religions of Australian New Guinea and the New Hebrides,* edited by P. Lawrence and M. J. Meggitt, 1-26. Melbourne: Oxford University Press.

Leach, J. W., and E. Leach, editors
1983 *The Kula: New Perspectives on Massim Exchange.* Cambridge: Cambridge University Press.

Leininger, Madeleine M.
1966 Convergence and Divergence of Human Behaviour: An Ethnopsychological Study of Two Gadsup Villages in the Eastern Highlands of New Guinea. Doctoral thesis, University of Washington.

Lewis, Albert Buell
1932 *Ethnology of Melanesia.* Department of Anthropology, Guide, part 5. Chicago: Field Museum of Natural History.

Lipset, David M.
1985 Seafaring Sepiks: Ecology, Warfare, and Prestige in Murik Trade. *Research in Economic Anthropology* 7:67-94.

Littlewood, Robert A.
1972 *Physical Anthropology of the Eastern Highlands of New Guinea.* Anthropological Studies in the Eastern Highlands of New Guinea 2. Seattle: University of Washington Press.
1994 The Micro-Evolution Project and a "Science" of Anthropology. Paper presented at the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

Lomnitz-Adler, Claudio
1991 Concepts for the Study of Regional Culture. *American Ethnologist* 18:195-214.

Lorentz, H. A.
1905 *Eenige maanden onder de Papoea's.* Leiden: E. J. Brill.

Lutkehaus, Nancy, Christian Kaufmann, William E. Mitchell, Douglas Newton, Lita Osmundsen, and Meinhard Schuster, editors
1990 *Sepik Heritage: Tradition and Change in Papua New Guinea.* Durham: Carolina Academic Press.

Malinowski, Bronislaw

1922 *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelago of Melanesian New Guinea.* London: George Routledge and Kegan Paul.

1926 *Crime and Custom in Savage* Society. London: Kegan Paul, Trench, Trubner and Co.

1927 *Sex and Repression in Savage* Society. London: George Routledge and Kegan Paul.

1929 *The Sexual Life of Savages in North-Western Melanesia.* New York: Eugenics Publishing.

1935 *Coral Gardens and Their Magic: A Study of the Methods of Tilling the Soil and of Agricultural Rites in the Trobriand Islands.* 2 volumes. London: Allen and Unwin.

McKaughan, Howard P.

1994 An Overview from Linguistic Micro-Evolution Studies of the Eastern Family of the East New Guinea Highlands. Paper presented at the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

———, editor

1973 *The Languages of the Eastern Family of the East New Guinea Highland Stock.* Anthropological Studies in the Eastern Highlands of New Guinea 1. Seattle: University of Washington Press.

Mead, Margaret

1935 *Sex and Temperament in Three Primitive Societies,* New York: William Morrow.

1938 The Mountain Arapesh: (1) An Importing Culture. *American Museum of Natural History, Anthropological Papers* 36 (3): 139-349.

Moore, Carmella C., and A. Kimball Romney

1994 Material Culture, Geographic Propinquity, and Linguistic Affiliation on the North Coast of New Guinea: A Reanalysis of Welsch, Terrell, and Nadolski (1992). *American Anthropologist* 96:370-392.

1995 Commentary on Welsch and Terrell's (1994) Reply to Moore and Romney (1994). *Journal of Quantitative Anthropology* 5:75-84.

Murdock, George, et al.

1950 *Outline of Cultural Materials.* 4th revised edition. Behavioral Science Outlines 1. New Haven: Human Relations Area Files.

Neuhauss, Richard

1911 *Deutsch-Neu-Guinea.* 3 volumes. Berlin: Dietrich Reimer.

Newman, Philip L.

1994 Elements in the Construction of the Awa Ethnographic Record. Paper presented at the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

Parkinson, Richard
1900 Die Berlinhafen-Section: Ein Beitrag zur Ethnographie der Neu-Guinea-Kuste. *Internationales Archiv für Ethnographie* 13:18-54.
1907 *Dreissig Jahre in der Südsee: Land und Leute, Sitten und Gebräuche im Bismarck-Archipel und auf den deutschen Salomo-Inseln.* Stuttgart: Strecker & Schröder.

Pataki-Schweizer, Kerry Josef
1980 *A New Guinea Landscape: Community, Space, and Time in the Eastern Highlands.* Anthropological Studies in the Eastern Highlands of New Guinea 4. Seattle: University of Washington Press.
1990 W. H. R. Rivers: Ethnology and Medicine in Papua, 1898. In *A History of Medicine in Papua New Guinea: Vignettes of an Earlier Period,* edited by Sir Burton G. Burton-Bradley, 269-279. Kingsgrove, N.S.W.: Australasian Medical Publishing.

Pöch. R.
1906 Bericht über eine Reise nach Neu-Guinea. *Deutsches Kolonialblatt* 17:647-655.
1907a Reisen an der Nordkuste von Kaiser-Wilhelmsland. *Globus* 92 (9): 139-143; 92 (10): 149-155; 92 (11): 169-173.
1907b Reisen in Neu-Guinea in den Jahren 1904-1907. *Zeitschrift für Ethnologie* 39: 382-400.

Powdermaker, Hortense
1933 *Life in Lesu: The Study of Melanesian Society in New Ireland.* London: Williams & Norgate.

Rappaport, Roy A.
1968 *Pigs for the Ancestors: Ritual in the Ecology of a New Guinea People.* New Haven: Yale University Press.
1984 *Pigs for the Ancestors: Ritual in the Ecology of a New Guinea People.* New enlarged edition. New Haven: Yale University Press.

Reed, Stephen Winsor
1943 *The Making of Modern New Guinea: With Special Reference to Culture Contact in the Mandated Territory.* American Philosophical Society Memoirs 18. Philadelphia.

Rivers, W. H. R.
1914 *The History of Melanesian Society: Percy Sladen Trust Expedition to Melanesia.* 2 volumes. Cambridge: The University Press. Reprint, Oosterhout N.B., the Netherlands: Anthropological Publications, 1968.

Robbins, Sterling
1982 *Auyana: Those Who Held onto Home.* Anthropological Studies in the Eastern Highlands of New Guinea 6. Seattle: University of Washington Press.

Roberts, John M., Jr., Carmella C. Moore, and A. Kimball Romney
1995 Predicting Similarity in Material Culture among New Guinea Villages from Propinquity and Language: A Log-Linear Approach. *Current Anthropology* 36: 769-788.

Sahlins, M. D., and E. R. Service, editors
1960 *Evolution and Culture.* Ann Arbor: University of Michigan Press.

Sapper, Karl
1910-1911 Beiträge zur Kenntnis Neupommerns und des Kaiser-Wilhelmslandes. *Petermanns Geographische Mitteilungen* 56 (1): 189-193, 255-256; 57 (1): 133.

Sarasin, Fritz
1917 *Neu-Caledonien und die Loyalty-Inseln: Reise-Erinnerungen eines Naturforschers.* Basel: Verlag von George.

Schlatinhaufen. Otto
1910 Reisen in Kaiser-Wilhelmsland (Neuguinea). *Dresden. (K) Zoologisches und anthropologisch-ethnographisches Museum, Abhandlungen und Berichte* 13 (1): 1-19.
1959 *Muliama: Zwei Jahre unter Südsee-Insulanern.* Zurich: Orell Fussli.

Schwartz, Theodore
1962 The Paliau Movement in the Admiralty Islands, 1946-1954. *Anthropological Papers of the American Museum of Natural History* 49, part 2: 211-421, plates 14-28.
1963 Systems of Areal Integration: Some Considerations Based on the Admiralty Islands of Northern Melanesia. *Anthropological Forum* 1 (1): 56-97.

Seligman, C. G.
1904 Note Concerning the Progress of the Cook-Daniels Expedition to New Guinea and the Solomon Islands. *Man* 4:180-181.
1905 Further Note on the Progress of the Cook-Daniels Expedition to New Guinea. *Man* 5:52-53.
1910 *Melanesians of British New Guinea.* Cambridge: Cambridge University Press.

Sexton, Lorraine D.
1982 Wok Meri: A Woman's Savings and Exchange System in Highland Papua New Guinea. *Oceania* 52:167-198.
1986 *Mothers of Money, Daughters of Coffee: The Wok Meri Movement.* Studies in Cultural Anthropology 10. Ann Arbor: UMI Research Press.

Simmons, R. T., J. J. Graydon, N. M. Semple, and D. R. Swindler
1956 A Blood Group Genetical Survey in West Nakanai, New Britain. *American Journal of Physical Anthropology* 14 (2): 275-286.

Slobodin, Richard
1978 *W. H. R. Rivers.* New York: Columbia University Press.

Speiser, Felix
1923 *Ethnographische Materialien aus den Neuen Hebriden und den Banks-Inseln*. Berlin: C. W. Kreidel's Verlag.
1991 *Ethnology of Vanuatu: An Early Twentieth Century Study*. Translated by D. Q. Stephenson. Bathurst, N.S.W.: Crawford House Press.

Stocking, George W., Jr.
1983 The Ethnographer's Magic: Fieldwork in British Anthropology from Tylor to Malinowski. In *Observers Observed: Essays on Ethnographic Fieldwork*, edited by G. W. Stocking Jr., 70-120. History of Anthropology 1. Madison: University of Wisconsin Press.

Strathern, Andrew J.
1968 Sickness and Frustration: Variations in Two New Guinea Highlands Societies. *Mankind* 6 (11): 545-551.
1969 Finance and Production: Two Strategies in New Guinea Highlands Exchange Systems. *Oceania* 40:42-67.
1971 *The Rope of Moka: Big-Men and Ceremonial Exchange in Mount Hagen, New Guinea.* Cambridge Studies in Social Anthropology 4. Cambridge: Cambridge University Press.
1972 *One Father, One Blood: Descent and Group Structure among the Melpa People.* London: Tavistock.
1994 Lines of Power. In *Migration and Transformations: Regional Perspectives on New Guinea*, edited by A. J. Strathern and G. Stürzenhofecker, 231-255. ASAO Monograph 15. Pittsburgh: University of Pittsburgh Press.

Swindler, Daris R.
1955 The Absence of the Sickle Cell Gene in Several Melanesian Societies and Its Anthropologic Significance. *Human Biology* 27 (4): 284-293.
1959 Racial Study of the West Nakanai of New Britain. Doctoral thesis, University of Pennsylvania.
1962 *A Racial Study of the West Nakanai.* Museum Monographs: New Britain Studies. Philadelphia: The University Museum, University of Pennsylvania.

Terrell, John
1993 Regional Studies in Anthropology: A Melanesian Prospectus. *Current Anthropology* 34:177-179.
1995 CA Comment on J. M. Roberts, Jr., et al. *Current Anthropology* 36:780.

Terrell, J., T. Hunt, and C. Gosden
1996 The Dimensions of Social Life in the Pacific: Human Diversity and the Myth of the Primitive Isolate. *Current Anthropology* 38:155-175.

Terrell, J., and R. L. Welsch
1990a Return to New Guinea. *In the Field: The Bulletin of the Field Museum of Natural History* 61 (5): 1, 10-11.
1990b Trade Networks, Areal Integration, and Diversity along the North Coast of New Guinea. *Asian Perspectives* 29:156-165.

Thurnwald, Richard C.
1910 Im Bismarckarchipel und auf den Salomoinseln 1906-1909. *Zeitschrift für Ethnologie* 42:98-147.
1917 Vorläufiger Bericht über Forschungen im Inneren von Deutsch-Neu-Guinea in den Jahren 1913-1915. *Zeitschrift für Ethnologie* 49:147-179.
1918 Drei Jahre im Inneren von Neuguinea 1913-1915. *Jahrbuch des Städtischen Museums für Völkerkunde zu Leipzig* 1915-1917, 7: 73-76.

Tiesler, Frank
1969- Die intertribalen Beziehungen an der Nordkuste Neuguineas im Gebiet der
1970 Kleinen Schouten-Inseln. *Abhandlungen und Berichte des Staatlichen Museums für Völkerkunde Dresden* 30:1-122; 31:111-195, plates C1-C14.

Tuzin, Donald F.
1994 Discussant's comments for the symposium "Sartor Resartus: The New Guinea Micro-Evolution Project, 1959-83 and After," American Anthropological Association, Atlanta, Georgia.

Valentine, C. A., III
1958 An Introduction to the History of Changing Ways of Life on the Island of New Britain. Doctoral thesis, University of Pennsylvania.
1961 *Masks and Men in a Melanesian Society: The Valuku or Tubuan of the Lakalai of New Britain.* Social Science Studies, University of Kansas Publications. Lawrence.
1963 Men of Anger and Men of Shame: Lakalai Ethnopsychology and Its Implications for Sociopsychological Theory. *Ethnology* 2:441-477.

Van der Sande, G. A. J.
1907 Ethnography and Anthropology of New Guinea. *Nova Guinea* 3:1-390.

Vayda, A. P.
1966 Diversity and Uniformity in New Guinea. *Acta Ethnographica Academiae Scientiarum Hungaricae* 15 (3-4): 293-300.
1971 Phases of the Process of War and Peace among the Marings of New Guinea. *Oceania* 42:1-24.
1989 Explaining Why Marings Fought. *Journal of Anthropological Research* 45:159-177.

Vayda, Andrew P., and Edwin A. Cook
1964 Structural Variability in the Bismarck Mountain Cultures of New Guinea: A Preliminary Report. *New York Academy of Sciences, Transactions* 26:798-803.

Vayda, Andrew P., Anthony Leeds, and D. B. Smith
1961 The Place of Pigs in Melanesian Subsistence. In *Symposium: Patterns of Land Utilization and Other Papers,* edited by Viola E. Garfield, 69-77. Seattle: American Ethnological Society.

Watson, James B.
1963 A Micro-Evolution Study in New Guinea. *Journal of the Polynesian Society* 72:188-192.

1967 Local Variation and Its Assessment in New Guinea. In *Behavioral Science Research in New Guinea,* edited by Mary Salisbury, 53-71. Publication 1493. Washington, D.C.: National Research Council.

1970 Society as Organized Flow: The Tairora *Case. Southwestern Journal of Anthropology* 26 (2): 107-124.

1972 Foreword to *Physical Anthropology of the Eastern Highlands of New Guinea,* by R. A. Littlewood, vii-xiii. Anthropological Studies in the Eastern Highlands of New Guinea 2. Seattle: University of Washington Press.

1974 The Ethnicity of Numerous, Small, Fluctuating Personnels in a Field of Low Ethnic Differentiation. Unpublished working paper in author's files.

1977 Pigs, Fodder, and the Jones Effect in Postipomoean New Guinea. *Ethnology* 16:57-70.

1980 Foreword to *A New Guinea Landscape: Community, Space, and Time in the Eastern Highlands,* by K. J. Pataki-Schweizer, vii-xi. Anthropological Studies in the Eastern Highlands of New Guinea 4. Seattle: University of Washington Press.

1982 Foreword to *Auyana: Those Who Held onto Home,* by S. Robbins, vii-xi. Anthropological Studies in the Eastern Highlands of New Guinea 6. Seattle:. University of Washington Press.

1983 *Tairora Culture: Contingency and Pragmatism.* Anthropological Studies in the Eastern Highlands of New Guinea 5. Seattle: University of Washington Press.

Watson, Virginia Drew, and J. David Cole

1977 *Prehistory of the Eastern Highlands of New Guinea.* Anthropological Studies in the Eastern Highlands of New Guinea 3. Seattle: University of Washington Press.

Welsch, Robert L.

1982 The Experience of Illness among the Ningerum of Papua New Guinea. Doctoral thesis, University of Washington.

1983 Traditional Medicine and Western Medical Options among the Ningerum of Papua New Guinea. In *The Anthropology of Medicine: From Culture to Method,* edited by L. Romanucci-Ross, D. Moerman, and L. Tancredi, 32-53. New York: Praeger.

1987 CA Comment on P. Kirsch and R. Green. *Current Anthropology* 28:448-449.

1988 The A. B. Lewis Collection from Melanesia--75 Years Later. *Field Museum of Natural History Bulletin* 59 (8): 10-15.

1994 Pig Feasts and Expanding Networks of Cultural Influence in the Upper Fly-Digul Plain. In *Migrations and Transformations: Regional Perspectives in New Guinea,* edited by A. J. Strathern and G. Stürtzenhofecker, 85-119. Pittsburgh: University of Pittsburgh Press.

1995 CA Comment on J. M. Roberts, Jr., et al. *Current Anthropology* 36:780-782.

1996 Language, Culture, and Data on the North Coast of New Guinea. *Journal of Quantitative Anthropology* 6:209-234.

1998 *An American Anthropologist in Melanesia: A. B. Lewis and the Joseph N. Field Expedition, 1909-1913.* Honolulu: University of Hawai'i Press, in press.

Welsch, Robert L., and John Terrell

1991 Continuity and Change in Economic Relations along the Aitape Coast of Papua New Guinea, 1909-1990. *Pacific Studies* 14 (4): 113-128.

1994 Reply to Moore and Romney. *American Anthropologist* 96:392-396.

Welsch, R. L., J. Terrell and J. A. Nadolski
1992 Language and Culture on *the North* Coast of *New* Guinea. *American Anthropologist* 94 (3): 568-600.

Whiting, John W. M.
1941 *Becoming a Kwoma: Teaching and Learning in a New Guinea Tribe.* New Haven: Yale University Press.

Wichmann, Arthur
1917 Bericht über eine im Jahr 1903 ausgefuhrte Reise nach Neu Guinea. *Nova Guinea* 4:1-493.

Williams, F. E.
1930 *Orokaiva Society.* Introduction by Sir Hubert Murray. Territory of Papua, Anthropology Report 10. London: Oxford University Press.
1936 *Papuans of the Tram-Fly.* Territory of Papua, Anthropology Report 15. Oxford: At the Clarendon Press.
1940 *Drama of Orokolo: The Social and Ceremonial Life of the Elema.* Territory of Papua, Anthropology Report 18. Oxford: At the Clarendon Press.

Woichom, John W.
1979 Canoes to Boats: Ali Exchange Tradition in Transition on the Siau Coast of the Northwest Papua New Guinea since 1896. Paper presented to the 49th ANZAAS Congress. On file at New Guinea Collection, University of Papua New Guinea, Waigani.

Wurm, S. A., and S. Hattori, editors
1981 *Language Atlas, Pacific Area.* Canberra: Australian National University.