ON THE LOCATION OF THE PROTO-OCEANIC HOMELAND

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The linguist Malcolm Ross has advanced a series of inferences pointing to somewhere in the Bismarck Archipelago as the probable "homeland" in the Pacific of the Oceanic or Eastern Austronesian languages. This conclusion is not the only one that can be reached based on current linguistic evidence and inference. Reviewing why he has singled out the Bismarck Archipelago as the homeland is advisable, for Ross's deductions have been seen as substantial support for associating proto-Oceanic with what some archaeologists have characterized as the "Lapita cultural complex." We find, however, that social-network models based on alternative assumptions about linguistic variation and the impact of strong and weak ties between language communities lead to more-plausible inferences about linguistic diversity in Melanesia, the convergent effects of geography, and the patterning of language history.

SINCE THE EARLY DAYS of European exploration and colonization in the Pacific, "the intriguing and complicated question" (Handy 1930:3) of Polynesian origins has been an enduring theme in the literature on the islands. As the archaeologist Jack Golson once candidly phrased the issue, "the so-called Polynesian problem" has long been "how to get the linguistically and cultur-

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ally homogeneous Polynesians into the central Pacific without racial contamination from the more diversified and presumed longer established Melanesians to the west" (1972:19).

Even though we are now in the twenty-first century, what is the right answer to this old riddle continues to be fiercely debatable. Nonetheless, today there is broad agreement in the scholarly world that the immediate ancestors of the Polynesians came from somewhere in Melanesia west of the great Polynesian triangle of islands in the central and eastern Pacific (Green 1999). While this conclusion is compatible with linguistic evidence, the archaeological facts and figures look especially compelling. The most distinctive "signature" of the first signs of human settlement in the Tongan and Samoan archipelagoes of western Polynesia is an often highly ornate style of ancient ceramics that is found exclusively in the Pacific and is called *Lapita* (Kirch 1997; Terrell and Welsch 1997). And as Jean Kennedy once remarked, Lapita has "a Melanesian distribution with a Polynesian extension" (1982:24).

Geography, however, is not history. The association between this pottery and the first (presumably "Polynesian") settlers of western Polynesia seems secure, but elsewhere in Oceania—including the Fiji Islands just west of Tonga—this pottery and other ancient signs of human occupation commonly associated with it are found in places where today the islanders are conventionally labeled as dark-skinned "Melanesians," not as lighter-skinned "Polynesians" (Terrell, Kelly, and Rainbird 2001). Does this imply that however different they may look to us, when all is said and done, Polynesians *are* Melanesians?

This may be the right answer to the riddle of the Polynesians, but precisely what such an answer historically implies is hard to say. Seen in global perspective, as Golson said, Polynesians certainly do look like a strikingly homogeneous "people," "ethnic group," or "population." So what remains at issue is precisely *how* Lapita links the Polynesians and the Melanesians. There is a real possibility that we may never know for sure, but it seems revealing, nevertheless, that today molecular genetics research is also showing that islanders in Polynesia and Melanesia share clear ties of biological kinship (Capelli et al. 2001; Kayser et al. 2000, 2001; Kirch 2000; Lum and Cann 2000; Spriggs 1997). Hence we may be closer to a solution to the old Polynesian conundrum than we think, however astonishing such a simple and straightforward solution to (or deconstruction of) the "Polynesian problem" may seem to some. After all, none other than the distinguished physical anthropologist W. W. Howells once wrote, "as physical beings, the Polynesians simply could not have emerged from any eastern Melanesian population; they are just too different genetically" (1973:234).

In any case, genetics is not all that there is to history any more than geography is. One historical question about Lapita pottery that remains unresolved is how quickly and how directly the craft of making this ware was carried from the Bismarck Archipelago in Melanesia—where the oldest Lapita pottery dating to around 3,300–3,200 years ago has been excavated (Specht and Gosden 1997)—to other Pacific archipelagoes as far east in Oceania as Samoa. According to some authorities today, Lapita may have taken at least 450 years to move that far east from the Bismarcks via a clinal progression of exploration and colonization; others think its eastward journey was so rapid that no chronological gradient at all can be distinguished within the radiocarbon record (Anderson and Clark 1999; Burley, Nelson, and Shutler 1999; Sand 1999, 2000). Given this broad range of chronological possibilities, considerably different historical scenarios about Lapita's eastward progression to Polynesia can be reconstructed. Common sense suggests that the longer this pottery took to reach Samoa, the more complicated the story of Lapita—and possibly of Polynesian origins—must have been, especially when we reconstruct its travels eastward not just in radiocarbon years but in human lifetimes.

A second and obviously related historical issue is how singular or "unified" Lapita was as a cultural expression (Spriggs 1997:13, 21). Put simply, is Lapita a story about "one people" who had "one culture," or was this pottery a shared cultural thread running through the local histories of island communities having different traditions and other distinguishing ethnic elements? On this issue, there appears to be growing consensus that a "one people, one culture" reading of Lapita would be too simple. Sand has argued, for instance, that the "Lapita phenomenon" was not as homogeneous as some may think (2000:31; see also Green 1994:35). He has cautioned that we must pay close attention to how Lapita varies geographically if we want to understand "the mechanisms of spread, settlement, and local adaptations of these Austronesian potters."

Sand's reference to Lapita as "Austronesian" introduces a third historical issue. Lapita archaeological sites are found on islands where people today who have plainly dissimilar cultural practices and who are quite diverse genetically (Oppenheimer and Richards 2001) nonetheless all speak languages classified as belonging to the Austronesian family—specifically, to what linguists talk about as the "Oceanic" branch of this great family of historically related tongues. Thus it would seem obvious enough that there must have been some kind of historical correlation, or connection, between this pottery and this particular group of languages. But precisely what was the connection? Or connections, for let us not forget Sand's words of advice or the present cultural and biological diversity of these islanders (Terrell,

Kelly, and Rainbird 2001). Was this pottery at first made by people who all spoke the same early Austronesian—specifically, "proto-Oceanic"—language? If so, were there other communities in Oceania at that time where people also spoke Austronesian? Or were the first Lapita communities linguistically unique in this part of the world, where we know archaeologically that people (presumably speaking Papuan, or "non-Austronesian," languages) have been in residence for something like 40,000 to 60,000 years?

Some archaeologists propose that if both Austronesian and the craft of pottery-making—but not the distinctive Lapita style of ceramic decoration, which many now agree was first created in the Bismarcks—are "intrusive" to Melanesia, then by far the easiest historical explanation would be that both cultural traits were introduced to Oceania from somewhere else (most probably from somewhere in southeast Asia) at the *same* time by the *same* small group of adventurous Austronesian voyagers. In the words of Matthew Spriggs, there must have been a movement of pioneers that "resulted in an Austronesian and Lapita settlement in the Bismarcks by 3500 BP and the break-up of Proto-Oceanic (POC) as Lapita settlements spread south and east through the Solomons and out into the Pacific after about 3200 BP" (1997:97). Indeed, according to Peter Bellwood, equating the introduction of Austronesian with the arrival of Lapita pottery in the Bismarck Archipelago is "so firmly accepted by linguists and archaeologists alike that it no longer needs lengthy justification" (1997:123).

But again, what about Christophe Sand's words of caution, and the enduring historical puzzle that traits of biology, language, and culture—with the exception of Polynesia's seemingly obvious homogeneity—are today so poorly correlated in Oceania? If we accept the conjectured primal unity of Lapita in the Bismarck Archipelago as a cultural, linguistic, and (presumably) biological phenomenon, why did people elsewhere in the Pacific come to make Lapita pottery? And are we to just ignore Lapita's heterogeneity as an archaeological phenomenon from place to place in Oceania?

Not according to Spriggs: "[T]here may have been a moment in the Bismarcks when there was a single people using Lapita pottery, genetically, linguistically and culturally distinct from their neighbours. But this unity and distinctiveness would have been short-lived. Lapita-using populations which spread to Polynesia and those in Island Melanesia subsequently had divergent genetic and linguistic histories" (1997:100). Nevertheless, the chief point for Spriggs and Bellwood is seemingly the idea that the "creolization" of Austronesian and non-Austronesian (Papuan) cultures in western Melanesia only happened *after* Lapita and Austronesian had made their first shared bridgehead somewhere in the Bismarck Archipelago (Bellwood 1997:236).

We think this thesis is too simple.[1] True, there is an undeniable elegance to simple historical scenarios. It can also be claimed that reconstructions of the past should not be made more complicated than necessity demands. And according to the archaeologist Patrick Kirch, "the correlation of the early Lapita phase with Proto Oceanic, and of the subsequent Lapita dispersal with the spread and later break-up of [the] Proto Oceanic speech community, is an extremely robust hypothesis—indeed, the only explanation which makes consistent sense of *both* the linguistic and archaeological evidence amassed to date" (1997:89; emphasis in original). Perhaps so, but we think there are at least three good reasons to be suspicious of what we see as overly simple historical scenarios about how the Pacific Islanders got their first Austronesian language and their first Lapita pots—and by implication, how the Polynesians came to be.

Three Reasons

There is broad agreement today that no one can understand the human diversity of the Pacific in matters of language, culture, and human biology without first understanding the history of the Pacific itself—not just its human history, but also the history of its plants, animals, and even the earth's continental plates. There seems to be far less consensus on what this consensus implies. Some scholars appear to see history largely as a story about crucial turning points in the past, others, mostly as an accounting of the pedigrees of ancient "tribes" or "populations" (Terrell 2001a:216). In our view, however, history is best seen as a contingent story of cause and effect: a concatenation of events, actions, and reactions leading at best to only broadly predictable results or outcomes. And the longer the chain of historical events one is describing, the less certain the outcome.

From this perspective, understanding the history of the Pacific and its people takes more than correlations linking Lapita with the Oceanic Austronesian languages (or linking Polynesians with Melanesians). Yet our first reason for being suspicious of simple scenarios about Lapita, Austronesian, and Polynesian origins is simple enough, and on its own, is perhaps far from compelling. Even some who say that Lapita began as a singular and unitary historical phenomenon somewhere in the Bismarck Archipelago or Southeast Asia also accept Roger Green's suggestion that Lapita culture was not a "package" of entirely exotic elements imported into Oceania from somewhere else (Kirch 1997:46–47, 93; 2000:93). Instead, argues Green (1991, 1994:35–36), only some of the properties of the cultural phenomenon that he calls "the Lapita cultural complex" were foreign imports; other elements instead had local roots in Melanesia and some were innovations created by

Lapita-making people only after their pioneering forebears had arrived in the Bismarck Archipelago.

We think Green's hypothesis about Lapita's trihybrid roots calls into question, perhaps unintentionally, how important it is to see Lapita at first as a historical "unity." As the archaeologist Les Groube wrote years ago (1971:313), it is now widely agreed that the Polynesians, strictly speaking, did not "come from" anywhere. On the contrary, they *became* Polynesians *after* their Lapita ancestors colonized Fiji and western Polynesia (Green 1995). Using the same logic, perhaps it is true that Austronesian was initially brought to the Bismarck Archipelago by only a small group of exotic pioneers; perhaps, too, everyone in this "founder population" shared much in common genetically, linguistically, and culturally. But if we adopt Green's hypothesis, then it was only after this small pioneering band or founder population of human beings reached the Bismarcks that they and their descendants set about creating the "Lapita cultural complex." And following Green, they did so not in isolation from others living then in Oceania but through interaction with their longer resident non-Austronesian neighbors. Under this scenario, the issue of whether "Lapita people" had a "moment" of primeval genetic, linguistic, and cultural "unity" would seem to be little more than a historical red herring. Why worry about whether there was a time of pristine Lapita—and by implication, proto-Oceanic—unity?

This question takes us to our second and third reasons for being skeptical about simple models of Lapita, Polynesians, and proto-Oceanic history. We think there is good reason to question the premise that the first Lapita potters in the Bismarcks were the first and only Austronesian speakers in Oceania (Green 1999:3; Kirch 1997:88–89; Pawley and Ross 1993:445–446; 1995:63; Spriggs 1997:97–98). Furthermore, we think there is little reason to believe that Lapita potters back then were the only people who—had linguists been on the job in the Pacific that long ago—would have been classified as speakers of proto-Oceanic Austronesian languages.

Both of these reasons for being skeptical about the historical value of simple correlations linking Polynesians, Lapita pottery, and proto-Oceanic are chiefly linguistic reasons, as we will now explain.

Oceanic Linguistics

During pioneering research on over 250 Oceanic languages leading to his celebrated study of the Polynesian languages and their position within the Austronesian (Malayo-Polynesian) language family, George Grace found that he could divide the Oceanic languages into nineteen subgroups (1955, 1959, 1961). Relationships among these groupings seemed to be structured by

little more than geography. With few exceptions, each subgroup is noticeably localized, and how similar each is to the others evidently corresponds with how near they are to one another geographically. "This is precisely the sort of situation which we should expect if the linguistic diversity had been produced through differentiation on the spot" (Grace 1964:366–367).

However, Grace was not offering a definitive classification of the Oceanic languages (1968:72). He concluded instead that more research—and a great deal more information—was needed before such a classification would be feasible. The nineteen subgroupings he had identified should only be regarded, he said, as "an approximation of the actual relationships" within the Oceanic branch of the Austronesian family.

Nonetheless, some scholars did interpret Grace's classification as historically meaningful—as showing us that Oceanic speakers must have settled most of western Melanesia quickly, and that while these pioneers were expanding over the Bismarck Archipelago, the Solomon Islands, and into Vanuatu and New Caledonia, they evidently spoke a single language that could be equated with proto-Oceanic (Pawley 1981:280). This inference was not entirely at odds with what Grace himself initially concluded (1961:364, 367). By 1964, however, Grace reported being impressed—as was the linguist Isidore Dyen (1965)—by the apparent language diversity encountered on the island of New Britain and its environs. Grace hesitantly suggested that the homeland of proto-Oceanic had probably been located somewhere in this general region, including northeastern New Guinea (1964:367).

In 1973, the linguist Andrew Pawley and his archaeological colleague Roger Green concurred with Grace's suggestion, which was concordant with their inference that proto-Oceanic had split off from the rest of the Austronesian family following a movement of people from eastern Indonesia into the immediate New Guinea region (Pawley and Green 1973:51). In light of later assessments, however, it should be noted that in 1973 Pawley and Green explicitly added that it seemed unlikely to them that Lapita pottery had been directly associated with this initial movement of Oceanic languages through Melanesia. The dating of Lapita to the second millennium B.C. looked far too late, they said, for the observable diversity of the Oceanic languages in Melanesia today to have developed only since then. Furthermore, it was likely that "the Lapita peoples who arrived in the Southeast Solomons and New Hebrides [Vanuatu] apparently found these areas already occupied" (Pawley and Green 1973:49).

A decade later, however, Pawley and Green had stepped back from these assumptions. They observed that the many subgroups by then recognized within Oceanic were geographically distributed in a chainlike (or "rake"-like) manner from New Guinea in the west to New Caledonia in the east. They

also noted that no single place within this range could be taken as a visible center of language diversity. Consequently, "it is hard to see how Proto-Oceanic could have diversified more or less simultaneously into a large number of branches scattered from New Guinea to Vanuatu unless it was already spoken over a wide area" encompassing most, if not all, of these many localized subgroups. Therefore, "at least in the final stages of its development, Proto-Oceanic was spoken by a widely dispersed population, centred in the chain of intervisible islands running from New Guinea, New Britain and New Ireland in the northwest to Malaita and San Cristobal in the southeast" (Pawley and Green 1984:135, 137).

By this time, too, Pawley and Green had reversed their previous position on Lapita pottery. In their estimation, the association between this pottery and the dispersal of Oceanic languages through Melanesia and as far out in the Pacific as Fiji and western Polynesia seemed much stronger than they had previously thought likely, although they still observed that this dispersal need not have been carried out solely by people with Lapita pottery (Pawley and Green 1984:142).

Ross's Subgrouping Hypothesis

Scholarly thinking about possible associations between proto-Oceanic, Lapita pottery, and the Bismarck Archipelago changed swiftly with the 1988 publication of Malcolm Ross's doctoral dissertation on proto-Oceanic and the Austronesian languages of western Melanesia. Ross's proposed subgrouping of the Oceanic languages retained much of the general rakelike (nonhierarchical) structure of previous analyses (see Spriggs 1997: fig. 1.3). However, he argued, most of the Oceanic languages in western Melanesia could be coalesced into three major linguistic (and geographic) clusters: the Meso-Melanesian cluster, the North New Guinea cluster, and the Papuan Tip cluster (Figure 1).

To achieve this degree of aggregation within the Oceanic subgroup, however, Ross had to relax the methodical prerequisite of family-tree models in comparative linguistics (and in biological systematics; see Clarke 1978; Hennig 1979) that membership in a proposed subset of languages must be based on evidence for a number of uniquely shared linguistic innovations (Ross 1988:7–9).[2] This stipulation is largely what had kept other linguists from proposing higher-order subgroupings within Oceanic (beyond the grouping in remote Oceania for which Grace had laid the foundations in the 1950s and 1960s).

Probably because of Ross's evident willingness to relax this formal prerequisite, there is now wider acceptance that traditional family-tree models

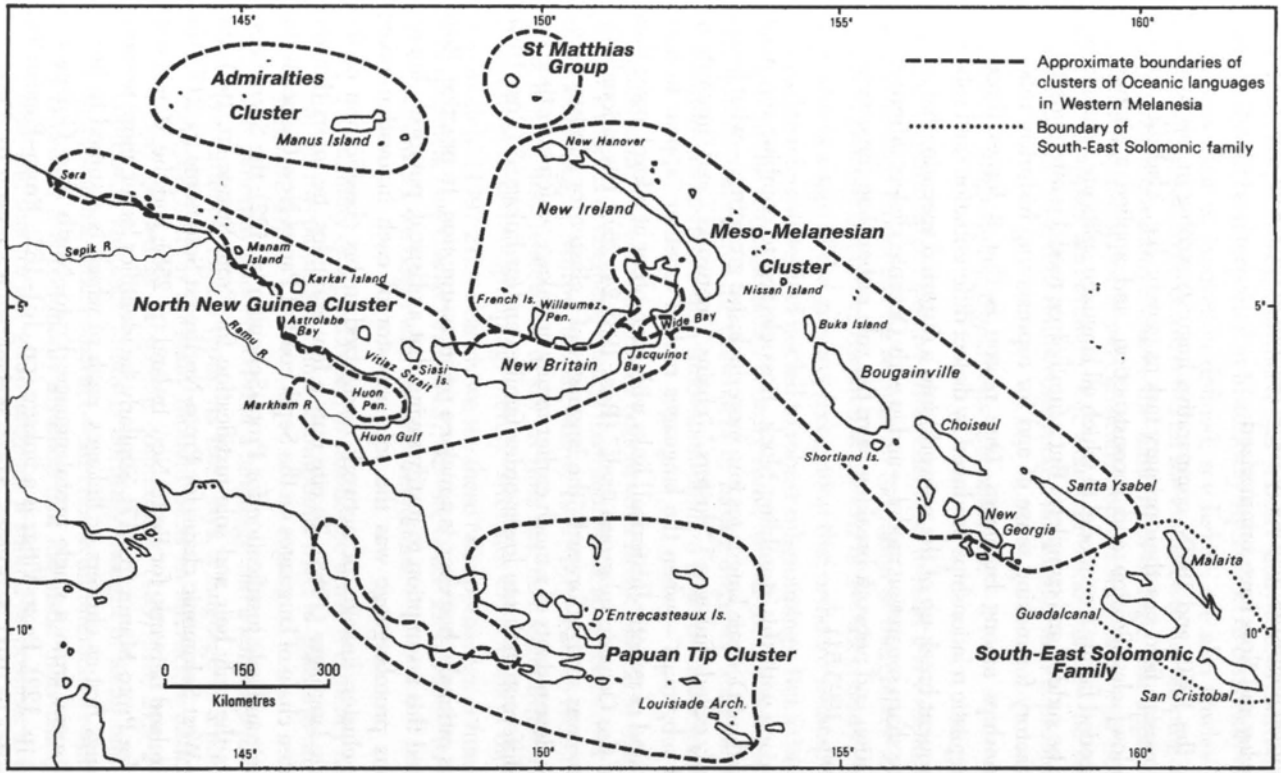

FIGURE 1. **The largest groups of Austronesian languages in western Melanesia proposed by Malcolm Ross.**
(Reprinted, with the author's permission, from Ross 1988:25, Map 2)

in historical linguistics may not fit social realities in the Pacific (Green 1999). As Pawley and Ross have summarized:

> In the 1950s and 1960s, comparative linguists writing in the Austronesian field saw their primary task as genetic [i.e., cladistic; see below] classification and reconstruction, and applied a simple-minded family tree model in which all language splits are assumed to be sudden and complete. But a family-tree model is often unsatisfactory for making sense of and for representing historical relationships among languages. One reason is that it forces those linguistic relationships produced by dialect differentiation (and subsequent break-up of the network) into a distorted scenario, that of the sharp separation model—and in early Oceanic, dialect differentiation and network-breaking were the rule rather than the exception. (1995:51)

To cope with this stumbling block, Ross elected to combine the many hundreds of Oceanic languages into more-inclusive groupings—what he has variously called "linkages," "clusters," "chains," "networks," and "innovation-linked subgroups"—when the languages ranked together "appear to have some kind of genetic [historical] links with each other at a level lower than that of the Oceanic subgroup itself" (Ross 1988:24–25).[3] His rationale for doing so was straightforward: "the languages of a cluster [or a linkage, etc.] are the descendants of a much earlier family or linkage which over time has itself differentiated into lower-order families and/or linkages" (Ross 1989: 137).

This rationale, however, is a major a priori assumption. In practice, Ross has used this assumption to justify saying that a classical, putatively homogeneous protolanguage was the sole ancestor of each innovation-linked chain, cluster, linkage, or network recognized in his classification of the Oceanic languages (1988). To cite only a few examples, he infers that the Schouten chain of languages on the Sepik coast of Papua New Guinea is the only heir and sole justification for Proto-Schouten (p. 122); the Suauic network is the only heir and sole justification for Proto-Suauic (p. 192); the North-West Solomonic chain, for Proto-Northwest Solomonic (p. 217); the New Ireland network, for Proto-New Ireland (p. 258); and the Manus network, for Proto-Manus (p. 317). Similarly, he identifies larger, more tenuous groupings first as clusters or linkages, each of which is assumed to justify and descend from a single protolanguage: Proto–North New Guinea is a cluster (p. 122), Proto-Vitiaz is a linkage (pp. 161–162); Proto–Papuan Tip is a cluster (p. 191), and Proto–Meso-Melanesian is a cluster (p. 258).

We are not, however, convinced that subsets analytically resolved in this fashion should be interpreted as historically equivalent to classically defined subgroups (that is, innovation-defined; see below; see also Pawley 1999). Permitting subsets to be constructed on the evidence of overlapping but not fully shared innovations changes the basic rules of logic that apply.[4] Clusters, linkages, and so forth and classically defined subgroups may not be as different as apples and oranges, but what can—and cannot—be said historically about these differently constructed taxonomic units is problematic.

1. Ross's Subgrouping Hypothesis: Classification

Viewed in historical perspective, Ross's decision to relax a key stipulation of the family-tree model can be seen as an indication of the greater sophistication of Oceanic linguistics research at the end of the last century and perhaps also as a sign of the growing recognition that orthodox assumptions in comparative linguistics had not led to robust subgroupings. But as historical linguists in the late nineteenth century came to realize, this view of scholarly history underplays the drawbacks of basing linguistic subgrouping on innovation-linked rather than innovation-defined traits or characteristics.

Assessment. We wholeheartedly concur with Ross (1997:215) that social-network models of language change have significant advantages over family-tree models (e.g., J. Milroy 1992; L. Milroy 1987; Terrell 1981, 1986). Yet we think it is an understatement to say, as Ross has, that lectal differentiation (innovation-linked subgrouping) merely "stretches the bounds of the family tree models" (1997:212). Frankly we see these two approaches as fundamentally dissimilar (see Clarke 1978:35–37, 42–83; Grace 1985, 1986). To explain why we think so, we begin with these observations.

1.1 The stability of logical types, or taxonomic units, is critically dependent on the parameters and assumptions used to frame them (Atran 1990:47–80).

1.2 It is generally understood that a *language* and a *language family* are two different logical types. As Ross says, a language as a phenomenological unity is normally thought of as an *interactive entity* commonly glossed as "a speech community" (1997:210, 212–214). In contrast, a language family is normally thought of as an ontological unity—a *historical construct*—and not as an interactive entity.

1.3 However, it is commonly assumed that linguistic traits change over time and that for any given language it should be possible to identify a series of linguistic traits defining the transformative historical stages

of development through which that language has passed, that is, how it has actually changed over time.

1.4 Yet despite these trait changes, another common assumption is that the language marked by these changes—that language as a historical phenomenon—has maintained its ontological integrity (Porter 1981: 122–123). Its linguistic traits may change, but not the ontological unity that they define (seen perhaps as a Kantian *Ding an sich*). As Ross has said, there is no "break in linguistic continuity," and consequently the "continuity of the language itself is not in question" (1997:241). For example, the entities called *English* at times T_1, T_2, . . . T_n are said to be ontologically "the same as" the interactive entity defined as *English* at time T_0.

1.5 Yet however substantive we judge the logical types "a language" and "a language family" to be, unlike "languages," "language families" are generally not seen as interactive phenomenological entities. Instead they are historical constructs that *both* define and are defined by the subgroupings they contain (a vexing conundrum in itself; see Ross 1997:250–251).

1.6 Furthermore, if the conventional assumptions and procedures of comparative historical linguistics are rigorously followed, it is generally assumed that the traits used to define subgroupings within a language family will prescribe a branching array (a "cladogram") that also prescribes a nested, or hierarchical, series of subsets, or subgroups (Figure 2).[5]

The critical point here is that when orthodox linguistic assumptions and comparative procedures are followed, membership in one of these subsets (subgroups) is determined by possession of one or more uniquely shared innovations made on the original suite of language traits exhibited by their common ancestor or "protolanguage" (at the base of "A" in Figure 2). Linguists recognize that the estimation error of the fit between any given family-tree model and the data it is meant to summarize cannot be blindly assumed to be negligible. History, after all, is not simple. Therefore, the innovations used to construct family-tree models must have "diagnostic substance" (Ross 1997:220). Ideally, assignment of a language to a subgroup is based on a suite of reliable traits, not just one or two, although achieving this methodological goal in practice is not always easy.[6]

Here it can be seen how innovation-defined and innovation-linked subsets have critically different properties. In the first instance, membership in a subgroup is uniquely defined on the basis of one (or more) exclusively shared diagnostic innovations; in the second, membership in a subgroup is

determined on the basis of some number of traits, none of which is rigorously defining.[7] As Ross has remarked, this distinction has important implications for the reconstruction of protolanguages, a task vital to the definition of subgroups, and for inferring geographic centers of diversity (1988:8–9; Pawley and Green 1984:133; Pawley and Ross 1995:432).

Conclusion 1—About Classification. Orthodox methods of comparative linguistics were designed in the nineteenth century to produce nested subsets of languages that (by definition) may be interpreted as chronologically ordered subgroupings. By relaxing the central stipulation that subgroups be defined on the basis of derivative innovations exclusively shared by all members of a subgroup, Ross has coalesced many of the previously recognized language subgroups within Oceanic into a smaller number of innovation-linked subsets. But these subsets need not be (by definition) consistently nested subgroups; hence their standing as historical higher-order subgroups within the Oceanic branch is problematic.

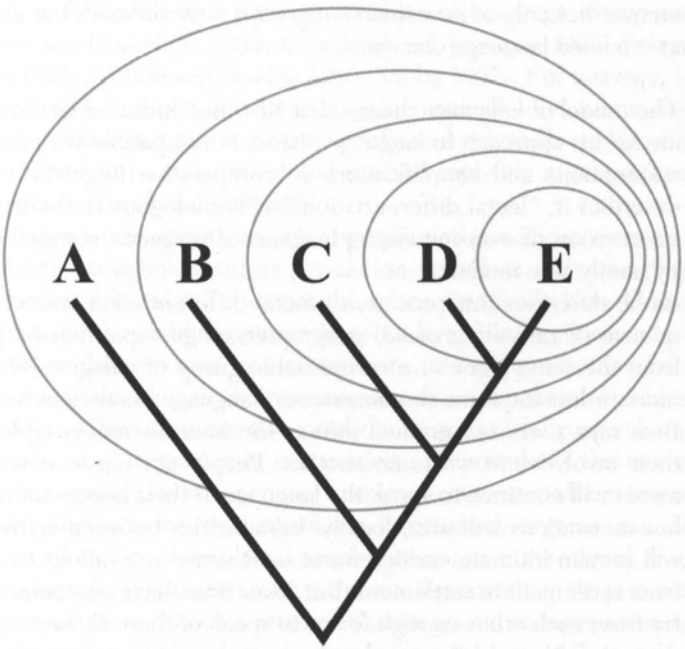

FIGURE 2. **A nested, hierarchical series of subsets.**

2. *Ross's Subgrouping Hypothesis: Change*

In the biological sciences, the formula *divergence = mutation + isolation + time* is perhaps the most basic way of thinking about the evolution of species diversity. Too often the same idea is used in historical linguistics to talk about dialectal differentiation and language divergence, except that the word "change" or "innovation" is substituted for "mutation." While it may be true that languages are changing all the time (they do so even in the course of our own lifetime), it does not follow that languages diverge as time goes by. In other words, *language change* does not automatically lead to *dialectal differentiation* or *language divergence* (although some writers say that any place where people have been living for a long time must be a place where there are many languages; e.g., Diamond 1997, 2000). Change leads to differentiation and divergence only under specific conditions—which in the formula just given have all been reduced to the single variable called *isolation* (Swadesh 1971:26).

Assessment. We think that some of what we see as the historical ambiguity of the higher-order subgroupings within Oceanic that Ross has proposed is an outcome not only of how these subgroups were defined, but also how Ross has modeled language change.

2.1 The model of language change that Ross has endorsed as the rationale for his approach to language history is compatible with orthodox assumptions and simplifications in comparative linguistics. As he describes it, "lectal differentiation"[8] is homologous to the temporal progression of ever-increasing language divergence mapped ideally by family-tree models.

2.2 As he describes this process, dialectal differentiation is one consequence of (usually gradual) geographic range expansion by people from the same place or a recognizable group of villages who speak more or less the same (homogeneous) language. As they move apart, Ross says, there is a gradual shift in the intensity and complexity of their involvement with one another. People moving to new settlements will continue to speak the language of their home community, but innovations will arise locally. Initially, ties between settlements will remain intimate enough for at least some innovations to spread from settlement to settlement. But "over time, lects may come to differ from each other enough for us to speak of them as 'separate languages', [although] the overlapping pattern of innovations remains" (Ross 1997:223–224). When divergence advances to this degree, the

resulting linguistic pattern is what Ross calls an "innovation-linked subgroup."

2.3 Hence, as he depicts them, innovation-linked models are homologous to traditional family-tree models of progressive language divergence. They might even be called "family-tree models in the making." In both models, it is assumed that linguistic diversification is a function of two variables, isolation and time. As Ross describes lectal divergence (1997:212–213), the critical difference between classic language "splitting" (what Ross calls "language fissure") giving rise to innovation-defined subgroups (which can be mapped ideally as family trees) and lectal differentiation giving rise to innovation-linked subgroups (which cannot be so mapped) is how quick and absolute the rupture or "break" is between the speakers of what had been a homogeneous speech tradition.

Conclusion 2—About Change. Our hesitations about Ross's model of dialectal differentiation arise not because we think it is wrong, but because it does not go far enough. Modern dialectology and sociolinguistics have now advanced research on dialectal differentiation well beyond the premise that time and isolation are the primary variables of language divergence (for examples, see Chambers 1995; Coupland and Jaworski 1997; Foley 1997; Holmes 1992; Scollon and Scollon 1995; Sebba 1997). For instance, Chambers notes that "mobility has been sociolinguistically underestimated as a reformative force, just as it has been socially and politically underestimated. . . . It deserves to be recognized and studied explicitly as a social variable with linguistic correlates" (1995:66).

Although we cannot pursue such a complicated issue here, even a variable as seemingly straightforward as population growth is more puzzling than Ross's model of lectal differentiation implies. People in New Guinea, for example, not only establish new settlements, but abandon them: a historical ebb and flow that can lead to such intricate, changing patterns of diversity that Bradshaw has likened the phenomenon to a "population kaleidoscope" (1997, 2001). Characterizing the social dimensions of language change and divergence—and capturing in our models and postulates the active roles that human agents play—is not easy and some might say, all but impossible. It is clear, for instance, that significant linguistic divergence can develop across social fields where local communities are not isolated from one another (Terrell 2001a). As a case in point, the sociolinguist James Milroy notes evidence from his own research that linguistic changes are spreading across physical and psychological barriers from Protestant East Belfast into portions of Catholic West Belfast (1992:185–195). Milroy also cites evidence

from the work of William Labov and others in Philadelphia that linguistic norms are diverging between black and white speakers living there, despite frequent contact between these two groups. In the Philadelphia case, the operative factor is community conflict, not physical contact or isolation.

3. Ross's Subgrouping Hypothesis: Interaction

As Ross has outlined his model of language change, what happens when separate languages are created following the formation of neat cleavages within a previously homogeneous speech community (language "fissure") and what happens when dialects evolve as one consequence of population growth and territorial expansion (dialectal "differentiation") are seen as homologous changes that can be classified alike as *speech-community events* (1997: 210–212). While historians argue about how to define "events" (e.g., Porter 1981; White 1987), we think it is unusual to equate short-term, decisive "events" and long-term event sequences or "processes." We think dialectal differentiation might be better categorized as a speech-community process or class of speech-community events, not as a single homogeneous speech-community event.

Ross cites the sociolinguist James Milroy to support calling language change a speech-community event, but we believe he does not take Milroy's model far enough. In his 1992 book on linguistic variation and change, Milroy devotes an entire chapter to the historical sociolinguistics of English, examining the *time* depth of variability. He cites cases in which nonstandard variants have persisted in parts of a community for a very long time, sometimes even rising to prominence as a new standard for a time before fading into obscurity. Milroy's model not only attenuates the time variable, it also replaces the notion of change as a relatively abrupt mutation with the notion of change as a gradual fluctuation in the relative prominence of competing variants at different nodes within social networks. This approach is much more consonant with models of genetic change that focus on relative frequencies within the gene pools of populations rather than on random individual mutations.

Case Study. We think not equating "events" with continuing "processes" is an important issue. Consider this example. Possibly as many as sixty languages belonging to perhaps twenty-four different language families are spoken along the seven hundred kilometers of coastline between Jayapura in modern Papua (formerly Irian Jaya) and Madang in Papua New Guinea. Since 1987 the New Guinea Research Program at the Field Museum in Chicago has been running a continuing program of ethnographic and archaeo-

logical research on this coast. Our work in this program is confirming that communities there and on the nearby offshore islands are focal points, or nodes, in a vast encompassing network or reticulated "social field" that has enabled people on the coast to share a common cultural complex even though they do not share a common language (Terrell and Welsch 1997; Welsch and Terrell 1991, 1998).

Ross judges all of the Austronesian languages spoken on this coast to be members of what he calls the *North New Guinea* cluster of Oceanic languages. Since he has found that these languages share no linguistic innovations to the exclusion of all other Oceanic languages, he has proposed that they must have "all originated from the same ancestral linkage, which was apparently located in or near the centre of dispersal around the Vitiaz Strait" (Ross 1989:146–147).

The Oceanic-speaking communities on the Sepik coast where the Field Museum is working have been assigned by Ross to the *Schouten* chain (1991), a closely related set of languages distributed between Manam island and Medebur village southeast of the Sepik-Ramu delta and Serra village west of the town of Aitape. Ross argues that all of these languages are descended from a single earlier language, *Proto-Schouten.*

Ross has shown convincingly that the twelve languages in the Schouten chain differ among themselves in a number of ways, both phonological and morphosyntactic (1991: tables 1–2). He considers the Schouten language spoken on Manam island to be the most "conservative" of them all, that is, this language is most like what he has reconstructed as Proto-Schouten. Interestingly, the number of linguistic innovations exhibited by the rest of the Schouten languages increases as you travel west farther and farther from Manam (Ross 1991:438). Ross has argued that this clinal pattern indicates that Proto-Schouten must have spread westward initially as a chain of dialects from (what he believes was) the homeland of proto-Oceanic in the New Britain/Vitiaz Strait area.

Assessment. Ross has found this cline perplexing, and he has offered a theory to explain it that we think illustrates both the strengths and weaknesses of innovation-linked subgrouping.

3.1 There is circularity in his observation that the Schouten language spoken on Manam is "the most conservative" in the chain. This designation means that Manam most closely resembles the other modern Oceanic languages in the North New Guinea cluster used to define the Schouten languages in the first place as a minimal group of languages sharing innovations in common.

3.2 There are well-established ways of explaining why innovations in a group of languages may form geographic patterns. Perhaps the best known explanation is the premise underlying the wave model: "namely that if speakers of related communalects are in contact with each other, it is to be expected that an innovation occur[r]ing in one communalect will diffuse to its neighbours" (Ross 1991:442). Since it is well-known that trade and travel are prominent features of life on the Sepik coast of New Guinea (Welsch and Terrell 1998)—and probably have been for millennia—there is good reason to think that the Schouten languages ought to fit wave models and other ways of modeling speech-community interaction (Bailey 1973; Bloomfield 1933; Hock 1986:444–456; Holmes 1992:218–224; Romaine 1982:252–273).

3.3 Ross, however, rejects the relevance of wave models to the Schouten chain on the basis of an elementary computer simulation showing how (in an idealized case) innovations arising within a chain of twelve language communities (communalects) will lead to a pattern of divergence unlike the clinal pattern actually observed for the twelve Schouten languages. Specifically, his simulation shows that "the communalects at the two ends of the [simulated] chain are least likely to undergo innovations (because each has only one neighbour from which it can receive a diffused innovation) and that the communalects in the middle of the chain have the greatest probability of undergoing innovations" (Ross 1991:443). In short, he says, communities at both ends of the chain ought to be—other things being equal—the most conservative (unlike the Schouten chain), while those in the middle ought to be the most innovative.

3.4 These model-generated expectations, however, are realistic only if we accept that the twelve language communities being simulated comprise a closed system, and distance has no effect on how widely an innovation arising at random in any one of the twelve model communities will be adopted by others in this closed system. Since there is ample reason to think people on the coast have long been trading and communicating with one another at least as far as the Bismarck Archipelago, there is little reason to model the Schouten languages as a closed system or to think that isolation-by-distance (which typically leads to clinal patterns of variation) has had no impact on these speech communities.

3.5 Ross has argued, nonetheless, that the clinal pattern of innovations observed among the Schouten languages not only shows that ancient Oceanic speakers moved east to west along the coast from the direction of the Vitiaz Strait, but also that each of the modern Oceanic-speaking communities on the coast was established in prehistoric

> times more or less where it is located today, one community after another in a step-by-step fashion (1991:446)—and additionally, each subsequently established settlement then became the ancestral homeland of all those established yet farther west (p. 445). Today, however, as we have already noted, all of these communities are each other's neighbors and friends. We know that people and entire communities there move around from place to place. In truth, it is anyone's guess where the first Oceanic settlements on the coast were located. There is little reason, therefore, to think that the present clinal patterning of innovations exhibited by the Schouten languages still maps the ancient steps and stopping places of the first Oceanic speakers on this coast.

Conclusion 3—About Interaction. We concur with Ross that the Schouten languages are a small-scale example of the conundrum of language variation in Melanesia.[9] What is it about these speech communities in northern New Guinea that has given them the linguistic appearance of not speaking with their neighbors despite our knowledge that they are closely tied to one another by culturally structured and possibly quite ancient relationships of friendship, marriage, commerce, and shared social responsibility? Whatever the answer to this conundrum (see Terrell 2001a), we conclude that innovation-linked subgrouping contributes reticulate taxonomic units that may exhibit the properties of both a language and a language family.[10]

Consequently, both the taxonomic and historical status of such entities or analytical units should be seen as unstable. "[T]here is no way of knowing whether an innovation shared by all member languages of the linkage was present in the proto language or has arisen since differentiation and subsequently spread through the linkage. . . . [T]here is no criterion by which to decide which stage of development the term 'proto language' should be applied to . . . [and] if 'proto language' refers to a set of already differentiated communalects, then a unitary proto language cannot sensibly be reconstructed" (Ross 1988:8). As a result, how innovation-linked taxonomic units are to be interpreted historically is acutely dependent on ancillary (nonlinguistic) ad hoc arguments (e.g., his interpretation of Oceanic prehistory on New Ireland; see Ross 1997:246).

4. Ross's Subgrouping Hypothesis: Centers of Diversity

Ross and others who locate the ancient proto-Oceanic homeland in the area of New Britain employ a long-standing rule of thumb that biologists also sometimes use, that "the area of a phylum which shows the greatest [cladistic] diversity is likely to be its homeland" (Ross 1997:255). Within the Oce-

anic subgroup, the primary split appears to fall between the languages of the Admiralties and an innovation-defined group that includes everything else (Blust 1998; Pawley and Ross 1995). This split could argue for an initial dispersal somewhere in the neighborhood of the Admiralties. Similarly, within Austronesian as a whole, the primary split appears to fall between the languages of Formosa, on the one hand, and the innovation-defined Malayo-Polynesian group that includes all Austronesian languages outside Formosa (Blust 1984–1985, 1999; Ross 1997; Tryon 1995). This split has often been used to argue for an initial Austronesian dispersal out into the Pacific from somewhere in the neighborhood of Taiwan.

Ross (1988, 1989, 1991) and Pawley and Ross invoke similar arguments to locate the proto-Oceanic homeland "in the Bismarck Archipelago, where several fairly well-established high-order subgroups meet" (Pawley and Ross 1995:58).[11] These subgroups include the comparatively isolated, innovation-defined Admiralties group and the much larger, more tenuous, innovation-linked Western Oceanic group, which is comprised of the three innovation-linked North New Guinea, Meso-Melanesian, and Papuan Tip "clusters" (see Figure 1, and Ross 1988:382–392), all of which have developed in areas with long histories of human interaction. Such an inference, however, is questionable on both general theoretical and location-specific grounds.

General Theoretical Assessment. Biogeographers studying the distributions of plants and animals have repeatedly tried to advance simple rules for determining the center of origin for any given taxon. Some have said the center of origin should be located where the greatest number of species in the taxon reside. Others have favored looking for where the most derived forms occur. Still others have insisted that the center of origin must be where the most primitive forms are found. Unfortunately, none of these criteria may be correct in any particular case, for the distribution of organisms depends in part on how they speciate, disperse, and interact with their biotic and abiotic environments (Brown and Lomolino 1998:346).

Consequently, scientists studying species diversity normally insist that any rule of thumb is only that and little more. No single criterion should be trusted to tell us the place of origin of a group of historically related species. As Barry Cox and Peter Moore explain in their now classic textbook on biogeography:

> At one time, some biogeographers believed that the area in which a group was represented by the greatest number of species was likely also to be the area from which the group dispersed. This hypothesis, however, assumes that new species will appear at a constant rate, whatever the environmental conditions, and that the presence of a

> large number of species in a particular area therefore indicates that the group has existed for a long time in that area. In fact, of course, the rate of speciation depends mainly upon ecological opportunity. (1980:111)

While here Cox and Moore are talking about numbers of species, and linguists do not normally talk about "ecological opportunity," linguists generally agree that languages do not all change at the same constant, uniform rate, and even if that were so, change and diversification are not two different words for the same phenomenon. However fast, slow, or irregularly any given language changes, few scholars would seriously maintain that change alone leads to language differentiation. Something else is needed—classically, something that puts people out of touch with one another (Swadesh 1971:8–42). Perhaps, therefore, the word "opportunity" does not apply, but the phrase "the ecology of language" may not be a bad way of talking about the variables that contribute to dialectal differentiation and language diversification (Mühlhäusler 1996)—although most linguists may prefer to use a term that sounds more familiar to them, the word "sociolinguistics" (Coupland and Jaworski 1997; Holmes 1992).

Location-Specific Assessment. In a provocative pair of recent papers, Blust has begun to explore a factor that is as important as it is neglected by those who seek to locate original homelands, namely, the possibility of extinction at the source (1998, 1999). At the Eighth International Conference on Austronesian Linguistics in December 1997, Blust presented a keynote address (published in 1999) in which he examined, inter alia, "the relationship between the linguistic and archaeological evidence with regard to the Austronesian homeland" (Ross 2000:385). Blust notes that, while the surviving linguistic evidence points to Taiwan as the Proto Austronesian homeland, it is very likely that "people of closely similar language and culture were found at the same time in coastal regions of southern China" but that the latter "have been progressively extinguished by the inexorable southward expansion of the Chinese" (quoted in Ross 2000:386).

Similarly, Blust notes that the primary split among Oceanic languages is between those of the Admiralties and the rest (1998). He calls the combination of the two groups "Broad Oceanic" (BOC), reserving "Oceanic" (OC) for the innovation-defined group outside the Admiralties.[12] Since the immediate ancestor of Broad Oceanic, Proto–South Halmahera-West New Guinea, is thought to have been located where its name suggests, the assumption seems logical that Broad Oceanic at one time ranged from the Sarmi coast in Papua (Irian Jaya) all along the north coast of New Guinea to New Britain and into the Admiralties. "But the recalcitrant linguistic fact that

remains is that all OC languages from the Sarmi Coast to Polynesia appear to form a subgroup as against the Broad Oceanic languages of the Admiralties. This observation suggests that the linguistic history of AN [Austronesian] speakers in Western Melanesia must have included episodes of extinction as well as episodes of expansion" (Blust 1998:186).

Blust therefore suggests a revised interpretation of the available evidence that spreads the "homeland" over a much broader area.

> Speakers of PBOC [proto–Broad Oceanic] settled the north coast of New Guinea and the Bismarck Archipelago as far as the Admiralties. . . . They had pottery . . . but lacked the distinctive Lapita design elements. The characteristic traits of Lapita were acquired instead by speakers of POC [proto–Oceanic], a community that had undergone certain linguistic and cultural innovations while in contact with Papuan-speaking populations along the north coast of New Guinea and/or in the larger islands of the Bismarck archipelago. The rest is a story of expansion leading to extinction: as POC speakers expanded through Western Melanesia, they replaced the remaining BOC populations everywhere except in the isolated Admiralties group, carrying Lapita with them. (Blust 1998:187)

We present Blust's hypothesis here not so much to endorse it as an alternative to Ross's argument but as an illustration of how ambiguous the evidence for a particular proto-Oceanic homeland becomes once one begins to factor in other possibilities such as extinction at the source.

Conclusion 4—About Centers of Diversity. When there are fields of expertise such as anthropology, dialectology, and sociolinguistics to be drawn on for help, it is difficult to accept statements about ancient human diversity and prehistory that seek to calculate probable homelands on the basis of anachronistic evidence from the modern era combined with simplifying assumptions about single migrations, long-standing sedentary settlements, divergence in isolation, and language preservation. Such determinations fail to take into account extinctions, mobility, enduring social contact, and a host of ecological factors. For instance, just during the past decade, communities along the north coast of New Guinea and in New Britain have experienced frequent earthquakes, prolonged drought, a devastating volcanic eruption, and a recent tsunami that utterly destroyed several coastal villages. Ross's relaxation of subgrouping criteria in this area was not designed so much to take these factors into account as to allow weaker evidence to support traditional branching-migration models. A succession of his works (Ross 1988,

1991, 1997) have maintained assumptions about continuous sedentary settlement and language preservation within those communities that we find strongly questionable.

Alternative Hypothesis

Our purpose here is not to present a "better hypothesis" about where the supposed proto-Oceanic homeland was *really* located. Instead, we suggest that the broad groupings that Ross has identified can be better accounted for using reticulate social-network models rather than branching-migration models of historical change. In our view, innovations shared between two or more socially and geographically linked communities are more likely to count as evidence of diffusion through contact than as evidence that they each participated in a shared migration history in the distant past. As Ross has noted (1997:215), Lesley Milroy (1987) has outlined a social-network model of language change that we think is much more directly applicable to Melanesian communities than isolation-based models. This model has been further developed and applied to historical research by James Milroy (1992). Based primarily on their sociolinguistics research on interaction within and across conflicting but interdependent communities in Belfast, Lesley Milroy suggests (following Granovetter 1973) that strong ties within communities inhibit change, while "weak ties *between* groups regularly provide bridges through which information and influence are diffused" (1987:197–200; emphasis in the original).

Although the Milroys developed their model to account for language change in large urban settings, the same model can yield interesting results when applied to the small, fluid, and diverse linguistic communities so common in Melanesia, as Ross agrees (1997:217). A social-network model would predict that larger linguistic communities in which internal ties are stronger than external ties would be more conservative, while smaller linguistic communities with fragile internal ties that must depend on a diverse network of external ties would be less resistant to change (see Hill 2001). This offers a different explanation for why a fairly large, relatively self-contained community like Manam might be more linguistically conservative than its smaller, more externally dependent neighbors westward in the Schouten chain. The same explanation has been offered for the conservatism of Icelandic relative to Danish or English (Milroy 1993:227). J. Milroy sees weak external ties as "the normal channel for the diffusion of innovations" (1992:189). He focuses less on the role of prestigious innovators at the center of a particular network and more on the role of early adopters of external models who occupy peripheral areas between overlapping social networks.

Another major advantage of social-network models is that they assume that linguistic contact rather than isolation is the norm, and "that linguistic change is one of the things that is *negotiated* by speakers in the course of speech-exchanges" (Milroy 1993:217; emphasis in the original). In place of random mutation or abstract structural pressures, such models explain change in terms of "strong/weak ties, the identity function of linguistic variation, and models of linguistic accommodation and politeness" (Milroy 1992:192). These factors seem much more applicable to real communities throughout Oceania than do factors such as isolation and random mutation.

Conclusions

We argue here that the innovation-linked groupings Ross has proposed within Oceanic are not only polythetic sets (Clarke 1978:36–37), but are also interactive subsets. Since relationships within and between taxonomic units thus defined are not necessarily ancestor-descendant relationships (Grace 1986; Rieppel 1980), they should be treated as cladistic only with great caution. We do not contest that there is observable diversity among the Oceanic languages spoken today on New Britain and elsewhere in the Bismarck Archipelago (as there also is elsewhere in Melanesia). We are skeptical, however, that any historical inferences based on what Ross (1989:140, 1997:255) describes as the long-standing rule of thumb among linguists that the "center of greatest diversity" within a language grouping is also its probable homeland are compelling when the proposed taxonomic units cannot be more rigorously defined and contrasted—and there is little reason to argue that the diversity observed solely reflects the passage of time.

In other words, changing the rules for determining linguistic subgroups as Ross has done compromises whether the units thus delimited can be read as if they expressed family-tree relationships. We think it is more appropriate, therefore, to label innovation-linked taxonomic units in comparative linguistics as interaction-defined subsets[13] and treat such analytical units as areal divisions somewhat along the lines of George Grace's noncladistic "Waveland" model (1962).

We have argued against models of language change in Melanesia based on the formula *time + isolation + random mutation = divergence.* Instead, we favor social-network models of language change with opposite assumptions: *time + contact + targeted change = convergence.* If the convergence effects of geography increase with time, then geographically contiguous subgroups (especially innovation-linked subgroups) in long-settled areas cannot be explained in terms of isolation and random mutation.

We think Ross's western Melanesian subgroups align too neatly with geog-

raphy. They are all contiguous, with none of the countergeographical internal subgrouping that may occur in the younger, innovation-defined, Polynesian or Nuclear Micronesian families (see Rehg 1995). Innovations of a different kind have had far more time to spread across the long-standing, localized, and geographically contiguous social networks of Melanesia, so that any geographical surprises have long since been obliterated (just as they have in Fiji; see Geraghty 1983; Hunt 1987). Bradshaw (1997) and Lynch (1981) have suggested ways in which convergence can still account for much of the greater linguistic diversity in Melanesia. Similar innovation-linked groupings may also confound historical interpretation of the family trees constructed among Polynesian or Nuclear Micronesian languages.

It has been known for several decades that the number of reported languages spoken on any given island in the southwest Pacific is positively correlated with the size of the island in question (Terrell 1974, 1986). Evidently, language in this part of the world has often reached an equilibrium where divergence is balanced by convergence. As noted earlier, divergence leading to mutually unintelligible speech traditions (different "languages") can clearly develop even when communities are not isolated from one another.[14] More research must be done to tease apart the social and situational circumstances leading to such patterned linguistic equilibrium.

Janet Holmes notes that to understand "why we don't all speak the same way, and why we don't all speak in the same way all of the time," scholars first need answers to four basic questions (1992:12): (1) Who are the participants? (i.e., who is speaking and who are they speaking to?); (2) what is the setting or social context of interaction? (where are they speaking?); (3) what is the topic? (what is being talked about?); and (4) what is the function? (why are they speaking?). The sociolinguistic concept of "domains of language use" popularized by Joshua Fishman can also be helpful (1972:43–54). Briefly described, "a domain involves typical interactions between typical participants in typical settings" (Holmes 1992:24).

Terrell recently suggested that scholars interested in what has been routinely labeled "language contact" (Thomason and Kaufman 1988) in New Guinea probably need to study at least these key dimensions of local language learning and use: (1) language acquisition during early childhood, (2) language use later in life, and (3) communicative competence, that is, how well people need to communicate with other people when they are away from home or when people are visiting from elsewhere (Terrell 2001a).

If linguistic diversity in western Melanesia reflects language change in the context of interaction through social networks, then what conclusions can we draw about Pacific prehistory? It should be obvious that we do not think that an Oceanic "homeland" can be securely or discretely located somewhere

on New Britain or even in the Bismarcks. For the reasons we have described here, we doubt that it will ever be possible to use linguistic evidence to locate such a homeland. However, we do think several hypotheses may be worth exploring:

1. We suspect that languages ancestral to those now identified as Austronesian were spoken in Oceania well before the appearance of Lapita pottery in the Bismarcks (as Blust 1998 also argues), although the evidence for saying so is mostly circumstantial. Experts do not agree on the magnitude of the eustatic downdraw of sea level during the last Ice Age, but estimates of 120 to 130 meters are common (Dickinson 1995:2). Whatever the exact figure, much of the northern shoreline of New Guinea, which is the second largest island in the world and is 2,400 kilometers (1,500 miles) long, was probably only sparsely inhabited during the last Ice Age. At that time, there was little suitable land for settlement on that side of the island—and, as a consequence, the northern shoreline may have been then more a barrier than a landbridge between Southeast Asia and the Pacific (Terrell 2002). Around 6,000 years ago, however, the world's sea levels had risen to near their current highstand. Experts are only beginning to document the impact that this new equilibrium may have had on coastal ecosystems and human settlement in the Pacific. Along the northern shores of New Guinea—a region viewed by some as strategic for understanding prehistoric Southeast Asian–Melanesian connections (Kirch 1997:55; Spriggs 1997:98)—newly stabilized coastal lagoons likely reached levels of natural resource productivity great enough to support significant local human population growth fueled mostly but perhaps not entirely by the harvest of wild foods (notably fish, shellfish, nuts, and edible starch from the pith of the sago palm) (Terrell 2002). If so, perhaps languages ancestral to those now called Oceanic may have come into wider use as regional population growth founded on a wide spectrum of subsistence resources, some wild and others carefully managed, began to transform the give-and-take between people in different places in this long-inhabited part of the world (Terrell and Welsch 1997).
2. Even if this tentative reconstruction of Holocene times in the southwestern Pacific should prove to be incorrect, we think it likely that proto-Oceanic was widely spoken—at least in its final stages of development, as Pawley and Green once wrote (1984)—along the chain of intervisible islands running from New Guinea, New Britain, and New Ireland in the northwest to Malaita and San Cristobal in the southeast.

3. If so, then the seemingly rapid spread of Lapita pottery (and perhaps other associated social and material traits) from the Bismarck Archipelago to Vanuatu and New Caledonia (and ultimately to Fiji and western parts of Polynesia around 3,000 years ago) may have been facilitated—at least as far as the southeast Solomons—by already long-established social networks among Austronesian- and non–Austronesian-speaking communities in island Melanesia.

Broader Implications

The observations we have made here also have broader implications. Bellwood (1996) and others have suggested that the patterning of languages and cultures observable on continental and millennial scales in the linguistic and archaeological records is so large-scale that such patterning cannot be explained—and may even be overlooked—by those who like to build their historical explanations step-by-step out of ordinary, everyday processes such as borrowing, trade, competition, recruitment, adoption, marriage, moving around, and inventing new ways to meet life's challenges. Most examples of large-scale patterning that Bellwood has offered come from linguistics—for example, Austronesian, Indo-European, Sino-Tibetan, and Uto-Aztecan. Recognition of these higher-level language categories, however, is ex post facto, as is the ordering of the subfamilies, languages, dialects, and the like seen as their subcomponents (see Simpson 1953:324, 376). Therefore, Bellwood's observation that these language families are geographically widespread and are deeply subdivided, or differentiated, is not in itself evidence that the evolution of such large-scale patterns of linguistic diversity calls for similarly large-scale explanations. As we have argued here, relationships among the Oceanic languages of western Melanesia are more reticulate than phylogenetic (see Terrell 2001b). It is doubtful, therefore, that Bellwood's "phylogenetic approach" can be used in this region with much success.

As Whaley recently observed, whatever the merits of a phylogenetic (that is, cladistic) approach, it must be stressed that the claims made by those advocating such an approach in anthropology and archaeology usually apply only at a remarkably general level.

> A multiplicity of details, many of which do not align with the larger picture, arises at a finer level of investigation. These details also require some account if we are to have any confidence in our claims about the compositions of macrofamilies of languages, population expansions, and the transmission of material culture in prehistory. Of course, one may treat such details as theoretical noise

> that can safely be ignored at macrolevel views of prehistory, but another possibility is that these details are in fact the more telling feature of the language family, regional population, or culture complex being investigated. (Whaley 2001:106)

Furthermore, as Pawley and Ross have pointed out, linguistic subgrouping on its own even under ideal circumstances only leads to relative chronology: "[T]o give absolute dates to prehistoric linguistic events, we need to be able to relate them to archaeological events" (1995:43). Unfortunately, how to achieve this added step is problematic. While in the case of Lapita and Austronesian in the farther reaches of Oceania—or at least in Fiji–West Polynesia—archaeology and language would appear to be in step with one another, the correlation between the two in the most recently settled parts of the Pacific can only be used to estimate Austronesian's *minimum* age as a language family. The link between language and culture in Polynesia tells us nothing about the deeper chronology of Austronesian in the older settled parts of the southwestern Pacific.

NOTES

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1. For instance, we would anticipate that if the first Austronesian speakers to reach the Bismarck Archipelago comprised a fairly homogeneous population—biologically, culturally, and linguistically—the most likely reason would have been that they were a small "founder" group, that is, a kin-biased "population sample" drawn from a heterogeneous source region (probably located somewhere in the island world of western New Guinea and the Wallacean region of southeast Asia; see Terrell 2002). In other words, we think there is little reason to assume that their "creolization" began only after their arrival in the Bismarcks.

2. As Ross acknowledges: "A proto language is usually reconstructed only where its descendant languages all share a number of innovations: it is more likely that a collection of shared innovations reflects inheritance from a unitary proto language then [sic] that these innovations have spread through a network after differentiation" (1988:8–9).

3. As Ross remarks (1991:433), these taxonomic units are equivalent to what Isidore Dyen (1965) has called *minimal groups* in the sense that languages within such a group are seen as more closely related to each other than to any language outside the group thus recognized.

4. It should be noted that Ross acknowledges that the innovation-linked classificatory units he has proposed are not necessarily first-order Oceanic subgroups as traditionally recognized and defined (1989:137).

5. As Pawley and Ross recount, "subgrouping under the Comparative Method can be applied recursively to identify subgroups within subgroups, that is, to construct what is conventionally called the 'family tree' of a set of genetically [i.e., historically] related languages" (1995:42).

6. The same holds true in biological cladistics where similar assumptions and procedures are used, although biologists are generally more stringent in how they recognize ancestor-descendant relationships (see Hennig 1979; Mayr 1981; Platnick and Cameron 1977; Rieppel 1980; Sokal 1974).

7. In such cases, as Pawley and Ross have recounted, the "innovations form an overlapping pattern, such that, for example, languages A, B and C reflect one bunch of innovations, languages C, D, and E another bunch, languages D, E, F, and G yet another, and languages G and H still a different bunch of innovations" (1995:50).

8. Ross glosses what he calls *lectal differentiation* (i.e., dialectal differentiation) as "the progressive break-up of a lectal linkage to form a group of separate languages." Since there is no objective way to discriminate between "a language" and "a dialect," he uses the noun "lect" and the adjective "lectal" in reference to both logical types (Ross 1997:212; see also Sebba 1997).

9. Our comments on Ross's model of language diversification mirror in many respects those that Bradshaw has already made about languages in the Huon Gulf region (1997, 2001).

10. Another standard way of describing innovation-linked subsets is to say that they are polythetic sets that similarly require many properties (traits) be used to classify entities (Sokal 1974).

11. If Lapita pottery and early Oceanic correlate in some way, it could be argued that Ross's "North New Guinea cluster" is far too young to be taken as evidence supporting this observation, since the earliest securely attested pottery-making traditions in northern New Guinea and the Vitiaz Strait—which are now seen as being immediately derived from the Lapita ceramic tradition—date back only to around 1,500–2,000 years ago (Lilley n.d.; Terrell and Welsch 1997). On other archaeological grounds (McEldowney and Ballard 1991; Spriggs 1997:111–113; Wahome 1997), the Admiralties cluster might also be removed, leaving only the current geographic distributions of the Meso-Melanesian cluster and the "St. Mattias group" as conjunctive evidence supporting a "Bismarcks homeland" for proto-Oceanic. At very least, therefore, Pawley and Ross's 1995 statement quoted in the text should be amended to read: ". . . the proto-Oceanic (POC) homeland in the Bismarck Archipelago, where *two* [not *several*] fairly well-established high-order subgroups meet."

12. There are obvious parallels here with Blust's revival of the older term "Malayo-Polynesian" for all Austronesian languages external to Formosa. However, at least according to Blust, the stay-at-home Admiralties languages appear to constitute an innovation-defined subgroup, while the Formosan languages do not (1998, 1999). Both Malayo-Polynesian and Oceanic appear to constitute innovation-defined subgroups.

13. If we were in favor of neologisms, we might be tempted to call Ross's Oceanic divisions *metalanguages* (see above, sections 1.2 and 1.5).

14. In view of the close conceptual parallels between modern biological cladistics and the classical techniques and assumptions of the comparative method of historical linguistics, it is worth noting in this context that the role of sympatric speciation—speciation occurring within a single geographic area where individuals have the opportunity to interbreed—is being increasingly acknowledged today in evolutionary biology due to new models substantiating its plausibility and new evidence showing that the conditions specified by these models are found in nature (Via 2001).

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