

# THE SIGNIFICANCE OF LINGUISTIC INTERACTION SPHERES IN RECONSTRUCTING MICRONESIAN PREHISTORY[1]

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A sea change has taken place in the field of Pacific prehistory. As a result of recent voyaging experiments, many scholars now recognize that early and frequent interisland contacts are possibilities that must be considered in reconstructing the prehistory of this region. This shift in perspective is of significance to linguists since it potentially affects how we conduct our research as well as how others perceive its value. A case in point is Geoffrey Irwin's claim that the orthodox settlement model for Micronesia, based upon linguistics, is defective. He contends that the patterns of language distribution in this region can largely be explained as resulting from prehistoric linguistic interaction spheres. I argue that Irwin's reasoning is flawed, but that, nevertheless, there are problems associated with depicting the relationships of some Micronesian languages in the form of a conventional family tree. I then compare the relative chronology implicit in the linguistic model with actual dates from the archaeological record and discuss discrepancies between the two. I conclude by calling for improved communication and cooperation between these two disciplines.

**1. NEW VIEWS OF PACIFIC COLONIZATION.** During approximately the past fifteen years, a significant change has taken place in the way most Pacific prehistorians think about the events involved in the colonization of the islands of Remote Oceania.[2] During the period of research circa 1950–1980, many archaeologists utilized a "closed system" model of settlement, in which island cultures were viewed as developing in near or total isolation. More recently however, primarily as a result of the successes of the Hōkūle'a voyages, that approach has been abandoned in favor of a more dynamic perspective. Today, most archaeologists recognize that early and frequent interisland contacts are possibilities that must be considered in reconstructing the prehistory of this region.

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Many of the archaeologists who first worked in Oceania endorsed the views of Andrew Sharp (1957, 1963),[3] who maintained that the islands of Oceania were settled largely as a result of accidental, one-way voyages. The evident shortcomings of Sharp's claims were soon exposed, however, by scholars of a more experimental bent. Researchers such as Thomas Gladwin, David Lewis, Ben Finney, and Geoffrey Irwin undertook both actual and simulated long-distance voyages employing technology believed to have been available to the early explorers of the Pacific. The work of these men provides a different view of the settlement of the Pacific islands. From this new perspective, the ocean is seen not as a barrier, but as a highway.[4] The Austronesian peoples of Oceania are viewed not as wayfarers with modest maritime skills, but as explorers possessing sophisticated long-distance-voyaging capabilities. The islands of this region are thus no longer thought to have been discovered as a result of fortunate accidents, but as an outcome of systematic exploration. And, of most significance to linguists, the islands of Oceania are no longer conceptualized as terminal destinations—instead, they are, and must be, seen as potential crossroads.[5]

These changes in attitude have already had a profound impact on contemporary views of Pacific prehistory. In Polynesian studies, they have triggered what Sutton (1987:135) characterizes as a "paradigm shift," resulting in part from "a change in the collective mind about how things worked in the past; specifically, about the degree of interrelationship which existed between islands and archipelagos during Polynesian prehistory." Ben Finney (1994:259) notes: "Archaeologists are no longer looking at the different island societies of West Polynesia as isolates. Instead they see them as parts of a communication sphere marked by frequent inter-island voyaging and the flow of goods, ideas, and people, a regional perspective that is now being fruitfully extended to central East Polynesia." Micronesia, too, as I will discuss in this paper, is being viewed by some scholars from this same perspective.

**2. CONSEQUENCES FOR LINGUISTICS.** This shift in thinking about Pacific prehistory is obviously of considerable significance to linguists working on the prehistory of Oceanic languages. It affects both how we do our work as well as how others perceive its value.

First, we must recognize that a widespread assumption about Remote Oceania has been that it provides a nearly ideal environment in which to apply the comparative method.[6] Pawley (1970:302) writes concerning Polynesia that "Proto-Polynesian and many of its daughter languages apparently developed without being significantly influenced by contact with other speech communities. The family-tree model, with its assumptions of sudden cleavages and subsequent independent development, can probably be applied to Polynesia with fewer qualifications than in almost any other linguistic region. The dialect-chain situation so familiar on large land masses with a long history of occupation is not characteristic of

Polynesia." Essentially the same assumptions have been made about at least some parts of Micronesia. Topping (1977:6) comments, "Until the colonial period, there was practically no awareness of each other's [other Micronesians'] existence. And in areas where there was precontact interaction, such as that between Truk [Chuuk] and Ponape [Pohnpei], the traditional relationship was marked by hostility . . ."[7] But many archaeologists would now argue that we can no longer take such isolation for granted.[8] If considerable prehistoric contact did take place among most island communities, then we must heighten our sensitivity to the possibility that innovations shared by languages might be the result of borrowing. We must also consider the effect such contacts could have had on language speciation.[9] Obviously, the rate and even the manner in which speech communities diverge is, at least in part, a function of the extent to which they interact.[10]

Second, we should respond to the fact that this paradigm shift is being used by some archaeologists to support their contention that linguistics is a field with no essential connections to their own. As linguists, we justifiably take pride in the fact that the most widely accepted claims about the colonization of Oceania are based largely upon the findings of our discipline. But, not surprisingly, some archaeologists dispute these claims, arguing basically that they are simplistic and misleading. John Terrell, for example, has called for a new way of thinking about Pacific prehistory—one that is free from what he considers to be the current tyranny of linguistics.[11] In my opinion, interdisciplinary research involving linguistics and archaeology is, in fact, an exemplary feature of some of the best work that has been done in this region.[12] However, if such cooperative efforts are to flourish, I believe it is essential that linguists take this paradigm shift seriously and demonstrate to skeptics that our methods are compatible with it.

**3. MICRONESIAN PREHISTORY.** A case in point—the one I wish to consider in this paper—is the influence that this paradigm shift has had on claims about the prehistory of Micronesia. In truth, relatively little is known about this topic. Irwin (1992:117) rightly notes: "Of [the] three conventional regions of Oceania [Melanesia, Polynesia, Micronesia], Micronesia's early prehistory is largely unknown, and its role in Pacific colonization most problematical." As Davidson (1988:83) observes, "The currently orthodox model of Micronesian settlement [is] derived largely from modern studies in linguistics," a model that she believes "needs careful evaluation."

Irwin, in his book *The Prehistoric Exploration and Colonisation of the Pacific,* essentially dismisses both this model and linguistics in general as a tool for understanding Micronesian prehistory, saying, "it appears that the colonisation of Micronesia is still open to a range of possibilities. The orthodox settlement model is essentially a linguistic one, but the patterns of language reflect late prehistoric contact spheres more than anything else" (Irwin 1992: 130). Given the influence of this work—Graves (1995:161) characterizes it as

a book "destined to change forever the way we think about the prehistory of the Pacific"—it is essential that Irwin's views be carefully evaluated to determine if, indeed, his assertions are valid. As a first step in appraising Irwin's claim about Micronesian prehistory, it is necessary to clarify further two issues raised in the preceding quotation. The first is, what does Irwin understand "the orthodox settlement model" of Micronesia to be? The second is, why does he believe "the patterns of language reflect late prehistoric contact spheres"? I consider these two questions in the following sections.

**3.1 THE ORTHODOX SETTLEMENT MODEL.** Irwin (1992:118) observes, "The current theory of Micronesian colonisation, which rests mainly on linguistics, proposes a twofold entry from both east and west (Bellwood 1978:282). The languages of the Marianas and Belau are of Western Austronesian type and, while not closely related to one another, are most similar to languages of Southeast Asia. Yapese and Nauruan appear to be linguistic isolates, while the rest are grouped as Nuclear Micronesian—a subgroup of Eastern Austronesian (Oceanic)—and are related most closely to the southeast Solomons and northern Vanuatu. The implication is that the western high islands were settled from Indonesia or the Philippines, while eastern Micronesia was settled from eastern Melanesia . . ." He further adds "Marck (1975:70–71) suggested that wind patterns would point to the Nauru/Kiribati/Marshalls area as the site of initial arrival and would allow a return crosswind voyage. Settlement in the east was followed by expansion west throughout the Caroline Islands" (Irwin 1992:119–120).

Most Micronesianists, I think, would agree with Irwin's characterization of "the orthodox settlement model" except for the following four points.[13] (1) Irwin, no doubt inadvertently, overlooks the fact that two of the languages in geographical Micronesia are Polynesian outliers—Nukuoro and Kapingamarangi. (2) Nauruan, rather than being an isolate, appears to be related to the Nuclear Micronesian languages. Jackson (1986:211–214) treats Nauruan as a sister of the Nuclear Micronesian languages in a group he calls Greater Micronesian. (3) The closest relatives of the Nuclear Micronesian languages, apart from Nauruan, remain uncertain.[14] (4) The idea that the homeland of the PMc peoples was in the east was first suggested by Bender (1971:457), not Marck, and on the basis of linguistic, not meteorological evidence. Apart from these minor flaws, however, Irwin's representation of the settlement model proposed by linguists is reasonably accurate.

**3.2 PREHISTORIC CONTACT SPHERES.** Let us now consider the second issue: Why does Irwin believe that "the patterns of language reflect late prehistoric contact spheres"? Irwin bases this claim upon observations made by Marck, who postulated for Micronesia what he calls the "overnight voyaging hypothesis": "If two islands are separated by a voyaging distance involving a single night at sea or less in traditional craft, their dialects will be mutually in-

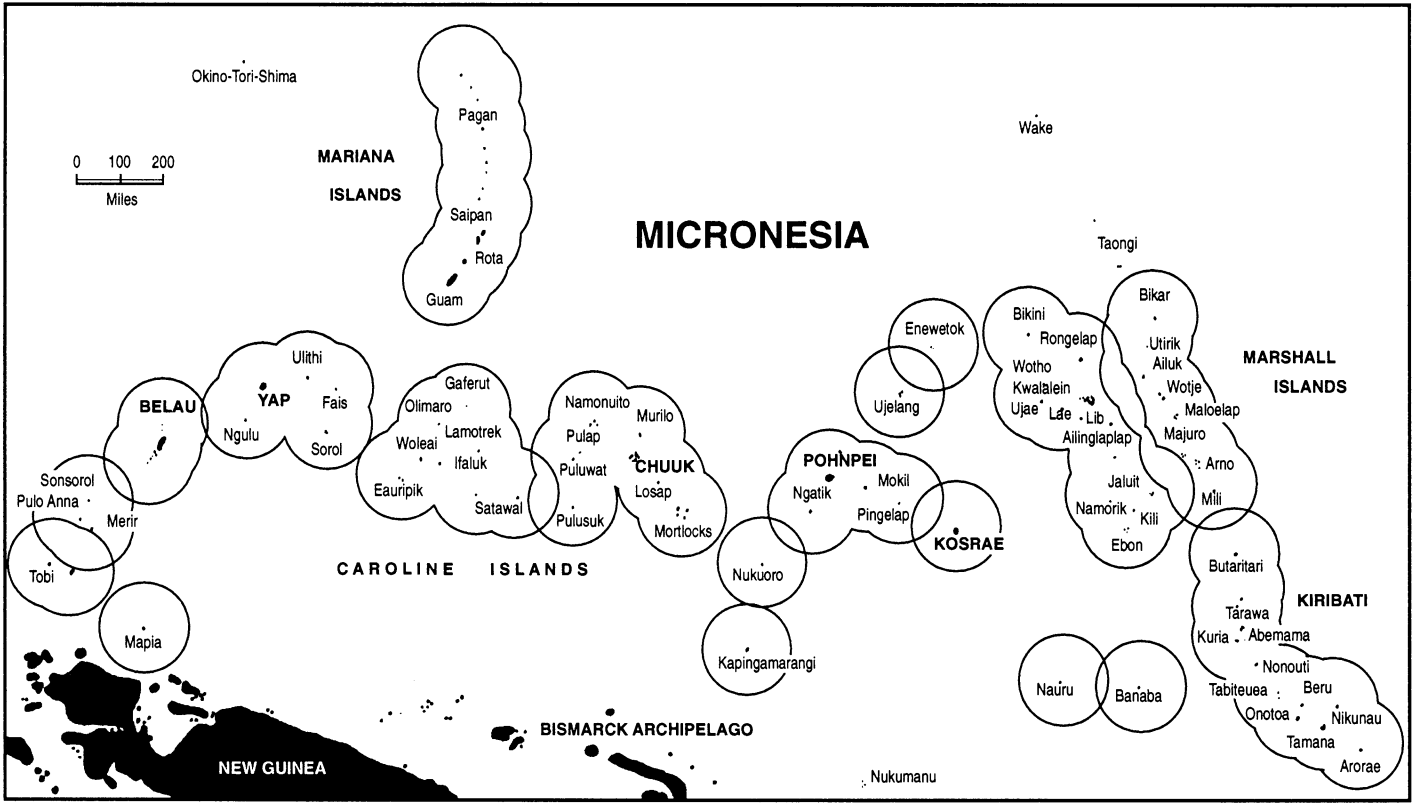
telligible. The maximum distance is about 100 miles in Micronesia" (Marck 1986:256). Marck observes that if a canoe departs at dusk one day, landfall up to 100 miles away can reliably be made during daylight of the following day. Such a voyage requires an average speed of approximately four knots.[15] Even junior navigators in Micronesia, Marck notes, are capable of guiding a voyage of this distance.

The consequence of this hypothesis is indicated in Figure 1 (reproduced from Marck 1986:254). On this map, Marck has drawn circles with 100 mile radii from each of the major islands in Micronesia. These circles suffice to define language boundaries within Micronesia, with the following qualification, "The rule must be modified to dismiss such cases as the comparison of Yapese to the Ngulu Trukic dialect, since they derive from different prehistoric arrivals of people and languages into the area. That is, the rule relates to limits toward which dialects of a language are expected to diverge, not the extent to which languages not immediately related are expected to converge. Other than that, the rule appears to need no other modifications and there are no exceptions" (Marck 1986:256). The significance of Marck's work is, of course, that language boundaries in Micronesia can be predicted on the basis of the overnight voyaging hypothesis. More simply, they can be predicted on the basis of geography. This finding is obviously of considerable interest and importance.

**4. EVALUATING IRWIN'S CLAIMS.** The point to be considered now is this: Is Irwin correct in claiming that "the patterns of language reflect late prehistoric contact spheres"?[16] That is, does Marck's overnight voyaging hypothesis render linguistics largely useless as a tool for understanding the prehistory of this region? The answer to these questions, I believe, is clearly no. I will argue that Irwin has misinterpreted the meaning of Marck's findings and has drawn from them a number of faulty conclusions.

First, it is not clear what Irwin means by "late prehistoric contact spheres." "Late" makes sense only if we interpret it to mean "continuing until lately." "Late" as opposed to "early" would seem to imply that these languages were once more different from each other than they are now, and that they converged only lately. There is, so far as I am aware, absolutely no evidence for this improbable hypothesis. In fact, Marck specifically excludes this interpretation of his findings where different languages are involved.[17] Second, interaction spheres obviously do not account for the division between the Nuclear Micronesian languages and the others. The settlements of the high islands of Western Micronesia and of the Polynesian outliers are clearly the result of historical events quite distinct from those that led to the colonization of the rest of this region. Third, even if Irwin's claim is restricted to just the Nuclear Micronesian languages, it can be correct only if the genetic relationships of these languages can be depicted in the form of a flat tree.

FIGURE 1. MAP OF MICRONESIA (FROM MARCK 1986)



**4.1 THE FLAT TREE MODEL.** A flat tree model for the Nuclear languages of Micronesia is given in Figure 2. It constitutes a claim that the Nuclear Micronesian languages form a subgroup, but it specifies nothing about possible internal subgroupings. In the 1970s, a tree like this was considered to be possible for the languages of Micronesia.[18] No very persuasive evidence for further subgrouping had yet been found.[19] Bender and Wang (1985:83) provide an account of how such a tree might develop: "it may be that the Proto-Micronesians—even assuming that there was only one group of original settlers who all came from the same 'homeland'—spread so rapidly across nuclear Micronesia that there was virtually no period of innovations, either within nuclear Micronesian or between major branches of the putative subgroup." Given this scenario, only geography (that is, interisland distances) is relevant for the purpose of understanding linguistic divergence in Nuclear Micronesia. Irwin's claim that "the patterns of language reflect . . . prehistoric contact spheres more than anything else" is consistent with such a tree, as indeed linguists would have been the first to demonstrate. However, if this tree is wrong, then Irwin's hypothesis must be examined afresh.

**4.2 JACKSON'S TREE MODEL.** What Irwin was apparently unaware of is that a different tree for the Nuclear Micronesian languages was proposed by Jackson (1983, 1986). Jackson's tree is given in Figure 3; dotted lines are employed where the historical relationships are not clear.

Obviously, there is no way that Irwin's hypothesis can account for the complexity of this tree. Therefore, while geography no doubt played an extremely important role in shaping Micronesian prehistory, it alone cannot explain why some Micronesian languages are more closely related to each other than they are to others. Geography does not explain, for example, why Ponapean is more closely related to Trukic, and even to Marshallese and Gilbertese, than it is to the geographically closer Kusaiean.

If the flat tree model is wrong, and Jackson has presented some rather persuasive evidence that it is, then Irwin's conclusion that the patterns of Nuclear Micronesian languages can be explained solely in terms of geography is also wrong. The implication that he draws from Marck's work—that linguistics is of little use in understanding the prehistory of this region—is also mistaken.

**FIGURE 2. A FLAT TREE MODEL OF THE NUCLEAR LANGUAGES OF MICRONESIA**

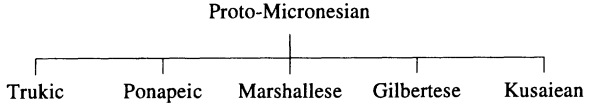
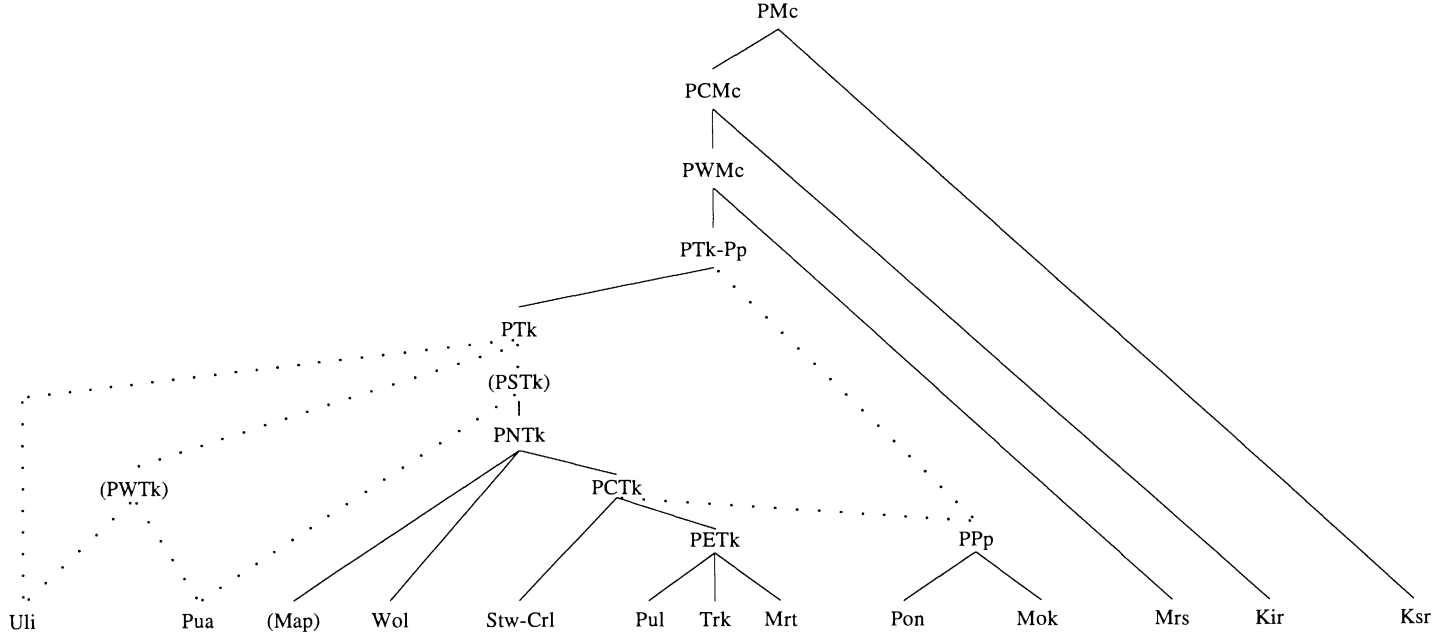


**FIGURE 3. HISTORICAL RELATIONSHIPS WITHIN THE MICRONESIAN SUBGROUP OF OCEANIC**




From Jackson 1983:433. Abbreviations: PMc, Proto-Micronesian; PCMc, Proto–Central Micronesian; PWMc, Proto–Western Micronesian; PTk-Pp, Proto-Trukic-Ponapeic; PTk, Proto-Trukic; PSTk, Proto-Sonsorol-Trukic; PWTk, Proto–Western Trukic; PNTk, Proto–Nuclear Trukic; PCTKk, Proto–Central Trukic; PETk, Proto–Eastern Trukic; PPp, Proto-Ponapeic; Uli, Ulithian; Pua, Pulo Anna; Map, Mapian; Wol, Woleaian; Stw-Car, Satawalese-Carolinian; Pul, Puluwatese; Trk, Trukese; Mrt, Mortlockese; Pon, Ponapean; Mok, Mokilese; Mrs, Marshallese; Kir, Gilbertese; Ksr, Kusaiean.

It might therefore seem reasonable to conclude that Irwin's discussion of the linguistic evidence for the colonization of Micronesian should be dismissed as a minor flaw of an otherwise praiseworthy work. I will not end this paper on that note, however, because I believe that the linguistic model for Micronesia does have problems, as I shall discuss next.

**5. A REAPPRAISAL OF THE LINGUISTIC MODEL.** Some of the doubts that archaeologists have about our linguistic model for Nuclear Micronesian languages are, I believe, justified. A major problem with this model, and with many others like it, is its use of branching trees to depict linguistic relationships. It is, in fact, precisely these simplistic two-dimensional trees that archaeologists are often most suspicious of, and rightly so.

**5.1 THE TREE MODEL OF LINGUISTIC RELATIONSHIPS.** Linguists have long been aware of the problems associated with the traditional family-tree diagram. The wave model proposed by Schmidt in 1872 represents an early reaction to its shortcomings. A half-century later, Bloomfield (1933:311) wrote, "The earlier students of Indo-European did not realize that the family-tree diagram was merely a statement of their method; they accepted the uniform parent languages and their sudden and clear-cut splittings as historical realities." Bloomfield (1933:318) went on to note that since these presuppositions of uniform speech-communities and sudden sharp cleavages are "never fully realized, the comparative method cannot claim to picture the historical process."

Southworth (1964) proposes ways to improve tree representations so that they might better serve to depict historical developments as understood by linguists, and Biggs (1972:143–144) expresses concerns similar to Bloomfield's: "It should be emphasized that linguistic subgrouping is concerned with the internal relationships of languages in a language family. Inferences as to migrations, first settlements, homelands, cultural affiliations and so on should be drawn from such data with caution, and a full awareness of the limited application of linguistic conclusions to such a problem." Still more recently, Grace (1986:1) observes that "the real lesson in all of this recent work [on Oceanic subgrouping] is precisely that the family tree model is not adequate to represent the genetic relationships among the Oceanic languages. And I suggest furthermore that by continuing to use the family tree terminology we inhibit ourselves from realising the full benefits of this lesson." (See also Grace 1985, 1987.)

**5.2 JACKSON'S TREE.** Scholars from disciplines other than linguistics, including some archaeologists, are inclined to interpret linguistic trees as claims about past migrations—an interpretation that is not always discouraged by linguists. Yet, we know better. As Pawley (1984:133) notes, "The comparative method yields knowledge of prehistoric languages but does not place those

languages in time or space." The branchings that occur in Jackson's tree have chronological significance only in relative terms, and this tree does not tell us where Proto-Micronesian was spoken. The most we can determine from it is that it was spoken on Kosrae or someplace else—actually, anywhere else.

Another problem with Jackson's tree is that, if we interpret it as a literal model of migration patterns, then we must conclude that Micronesia was settled by a series of discrete moves through the islands, and that at each point where we identify a subgroup, there was a pause of sufficient duration to allow a unique set of innovations to develop by which we can identify the subgroup. This seems to me to be unlikely as an account of the settlement of the entirety of this region.

The problems of family trees in general and of Jackson's tree in particular do not, however, invalidate the comparative method. As Bloomfield (1933:321) observes, this method "with its assumptions of uniform parent languages and sudden, definitive cleavage, has the *virtue* [emphasis added] of showing up a residue of forms that cannot be explained on this assumption." That is, the data that the comparative method unearths often do not lend themselves to a neat interpretation in the form of a tree. The dotted lines that appear in Jackson's tree are prompted by such data. They represent instances where the tree model does not appear adequate to account for the facts.[20]

**5.3 THE TRUKIC CONTINUUM.** Jackson states that he employs dotted lines in his tree "if the historical relationships are not reasonably clear" (1983: 483). A number of such lines occur under Proto-Trukic. An insightful discussion of the data that prompted these uncertainties is provided by Jackson in his 1983 dissertation. It is not my intention here to reiterate or evaluate Jackson's analysis. Rather, what I would like to deal with is the more fundamental issue of whether a conventional tree diagram is appropriate for representing the linguistic relationships of the Trukic linguistic communities.

I first wish to observe that the language names listed under Proto-Trukic represent points in a dialect continuum that spreads across the Western Carolines (Quackenbush 1968). The majority of these dialects are spoken on islands lying in an east-west chain extending from Ulithi to Lukunor, a distance of approximately 1000 miles. Three Trukic speech communities—Carolinian, Pulo Annian, and Mapian (now extinct)—lie outside this region and might be considered Trukic outliers.

In Figure 4, I have excluded the Trukic outliers and arranged the remaining varieties of Trukic in a sequence that largely conforms to the east-west geographical location of these speech communities. Only Trukese is slightly out of order here. Mortlockese is spoken on a group of atolls that actually lie southeast of the Chuuk Lagoon, where Trukese is spoken. But, I have placed Trukese at the end of this continuum, because only Trukese is spoken on high islands, and, as a result, its cultural history is somewhat different from those of

**FIGURE 4. THE TRUKIC CONTINUUM (EXCLUDING OUTLIERS)**

Ulithian Woleaian Satawalese Puluwatese Mortlockese Trukese

other varieties of Trukic, all of which are spoken on atolls.[21] Cognate percentages between adjacent speech communities, as they are ordered here, are given in Table 1.[22] Note that all percentages fall approximately within a range that, in lexicostatistical theory, would characterize these Trukic varieties as being dialects of a single language.[23] The same high percentages, however, do not persist throughout the entire continuum. An examination of Table 2 shows that these percentages decrease systematically as one moves from left to right or, in geographical terms, from west to east. The lowest cognate percentage among all varieties of Trukic is between Trukese and the Trukic outlier, Pulo Annian, at .665.

The factors that contributed to the maintenance of this continuum have been described in detail elsewhere.[24] Three, however, are worth brief mention here.

(1) At the time of first Western contact, the people of the Trukic atolls possessed perhaps the most advanced maritime technology in the Pacific (along with possibly the Marshallese). They were excellent navigators sailing extremely sophisticated voyaging canoes.[25]

(2) The atoll peoples of this region maintained extensive interisland socioeconomic ties for the purposes of barter, marriage, adoption, and the exchange of gifts.

(3) The atolls from Ulithi to Puluwat were politically bound together as part of the Yap Empire, in which they maintained religious, economic,

**TABLE 1. COGNATE PERCENTAGES BETWEEN PAIRS OF TRUKIC SPEECH COMMUNITIES**

| | |
|---|---|
| Ulithian-Woleaian | .803 |
| Woleaian-Satawalese | .884 |
| Satawalese-Puluwatese | .881 |
| Puluwatese-Mortlockese | .883 |
| Mortlockese-Trukese | .856 |

Adapted from Jackson 1983:275

**TABLE 2. TRUKIC COGNATE PERCENTAGES**

| | WOL | SAT | PUL | MRT | TRK |
|---|---|---|---|---|---|
| Ulithian | .803 | .770 | .724 | .721 | .678 |
| Woleaian | | .884 | .824 | .778 | .750 |
| Satawalese | | | .881 | .820 | .794 |
| Puluwatese | | | | .833 | .814 |
| Mortlockese | | | | | .856 |

Adapted from Jackson 1983:275

> and social ties with the high island of Yap to the west. Yap also served as a refuge and a source of food in the wake of typhoons and other ecological crunches. The Chuuk Lagoon and the Mortlocks were not part of this empire, a fact partially reflected in the linguistic patterns of this area.

Given the extensive interaction among these speech communities, and given that language boundaries in this region are indeterminate, it seems highly inappropriate to depict the relationships of the communalects of the Trukic continuum in the form of a tree. Trees reflect linguistic speciation, with convergence being unexpected, if not impossible. Dialects, on the other hand, can and do undergo convergence. The wave model might better serve here.[26] In fact, Quackenbush (1968) employed such a model to illustrate a number of features of this continuum.[27]

What the Trukic region seems to represent, then, is a vivid example of what Irwin incorrectly assumed was true of all of Micronesia. The Trukic continuum, excluding the outliers, reflects linguistic interaction spheres that continued until Western contact and still persist in a somewhat modified form today.

**5.4 THE TRUKIC/PONAPEIC SUBGROUP.** The next highest node on Jackson's tree is the one labeled Proto-Trukic-Ponapeic. This node represents a claim that Trukic and Ponapeic together form a subgroup, as first postulated by Dyen (1965:33–34). However, Jackson's tree evidences his uncertainty about where Proto-Ponapeic fits. The dotted lines in this tree indicate that it might be either (1) coordinate with Trukic or (2) derived from within Trukic. The source of his dilemma is briefly as follows.

Trukic and Ponapeic are well-defined subgroups. Each subgroup is marked by a substantial number of unique innovations. On the face of it, it is a relatively simple matter to identify a language as being either Trukic or Ponapeic.[28] Nevertheless, several major innovations that Jackson persuasively argues are not due to drift are uniquely shared just by Ponapeic and the Central Trukic languages. That is, major isoglosses representing innovations cut across otherwise well-established subgrouping boundaries.

I will illustrate this problem by considering a single example of such an isogloss, one involving the reflexes of PMc *t. Spirantization and the loss of *t are phenomena that have slowly spread through both the Ponapeic and Trukic lexicons. Jackson (1983:427) notes: "PP [Ponapeic] languages attest diffusion of spirantization and loss of *t that is almost identical, down to the lexical items that are involved, with what is attested in CRL [Saipan Carolinian, southern dialect], a CTK [Central Trukic] language whose ancestors split off from PCTK [Proto-Central Trukic] prior to the development of the ETK [Eastern Trukic] group. It would appear to be very difficult to account for such similarities by proposing two independent developments. Far more likely is the hypothesis

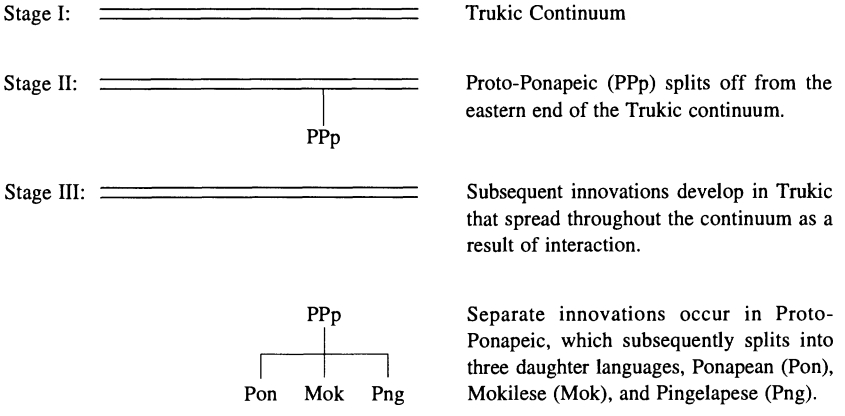
that the CTK languages and PP shared a period of common development." The problem, then, is how does one account for the fact that seemingly good evidence exists that Ponapeic is both a subgroup coordinate with Trukic and a subgroup coordinate with Central Trukic? Jackson (1983:421–431) attempts to resolve this problem by suggesting that Trukic constitutes a paraphyletic grouping, a notion he takes from Grace.[29] A paraphyletic grouping is one that might arise as depicted in Figure 5.[30] Obviously, there is no way such a grouping can be represented in a conventional tree diagram.[31]

**5.5 THE UTILITY OF FAMILY TREES.** A traditional family tree is not adequate to account for the linguistic relationships either within Trukic or of Trukic with Ponapeic, at least so far as we currently understand them. Whether or not still higher levels of Jackson's tree will prove to be resistant to representation in the form of a tree diagram remains to be determined. Jackson indicates no major problems at these levels.

There are, of course, circumstances under which family trees can be employed without distorting the historical facts. When speech communities divide from each other at a single point in time, and when they remain relatively or totally isolated, or when, if contact occurs, its effects can be discerned, then the use of trees is appropriate. It may be that most if not all higher levels of linguistic relationships within Micronesia are of this type. Although contact has certainly occurred among at least some of the more distantly related languages, I suspect its linguistic effects, if any, can be detected by careful analysis.[32]

The tree model, in my opinion, is thus not hopelessly flawed. Robert Blust, Andrew Pawley, and Malcolm Ross, for example, have written insightfully on the problems associated with representing linguistic relationships in the form of conventional, branching trees, but all three also have employed trees when

**FIGURE 5. POSSIBLE ORIGIN OF A PARAPHYLETIC GROUPING**



appropriate to do so.[33] How in our research do we determine when trees are appropriate? At least part of the answer is that we must meticulously track the distribution of all innovations without regard to preconceived notions of language and subgrouping boundaries. Given the increased interest in contact phenomena that has arisen among Pacific prehistorians, it is essential that we not filter out isoglosses that overlap otherwise well-established subgrouping boundaries. Such isoglosses, where they can be attributed to contact or some more complex set of historical events (and not to drift), provide valuable information for understanding the prehistory of a region.[34]

**6. ARCHAEOLOGICAL EVIDENCE FOR MICRONESIAN SETTLEMENT.** A final issue to be considered in this paper is the extent to which the linguistic model of Micronesian settlement and the archaeological record are actually in conflict. Table 3 provides a summary of the earliest radiocarbon dates available for some areas of Micronesia.[35] I have arranged these dates, according to my interpretation of them, into two columns labeled conservative (representing minimal time depths few archaeologists would dispute) and possible (dates that are more controversial). All dates are approximate. If only the conservative dates are considered, then the Nuclear Micronesian region would appear to have been settled nearly instantaneously. Rainbird (1994:299) comments: "It appears that all the high island of the Carolines were settled at roughly the same time by people possessing the necessary skills to manufacture pottery." And Irwin (1992:131) claims: "There is no incontrovertible archaeological evidence of settlement prior to A.D. 0 in either [Micronesia or Eastern Polynesia]." He also observes (1992: 127): "the existing evidence confirms none of the specific chronology for population movements within Micronesia as predicted by linguistics, apart from the general suggestion that the settlement of eastern Micronesia as a whole should

**TABLE 3. SUMMARY OF THE EARLIEST RADIOCARBON DATES AVAILABLE FOR SOME AREAS OF MICRONESIA**

| | CONSERVATIVE | POSSIBLE |
|---|---|---|
| WESTERN HIGH ISLANDS | | |
| Marianas | 1000 B.C. | 1500 B.C. |
| Yap | 0 A.D. | |
| Belau | 0 A.D. | |
| NUCLEAR MICRONESIA | | |
| Fais | 100 A.D. | |
| Chuuk Lagoon | 0 A.D. | 150 B.C. |
| Pohnpei | 0 A.D. | 250 B.C. |
| Kosrae | 0 A.D. | 500 B.C. |
| Marshalls | 0 A.D. | 1500 B.C. (Bikini) |
| Gilberts | No dates | |
| Nauru | No dates | |

post-date Lapita [due to the presence of pottery in this region]." Obviously, if the conservative dates hold up, the flat tree is right, Irwin is right, and Jackson and other linguists who accept subgrouping hypotheses higher than the level of Trukic and Ponapeic are wrong.

The possible dates given in the right-hand column of this chart, however, appear to better fit the linguistic model for Micronesia. Given the considerable diversity of the Micronesian languages, a date of A.D. 0 for Proto-Micronesian is almost certainly too recent. These possible dates, however, are problematic, especially the earliest one from the Marshalls. One ostensible problem with the date of 1500 B.C., as well as with other dates from the Marshalls in the 500+ B.C. range, is that they are all from Bikini atoll, the site of atomic bomb testing. Streck (1990:254) claims, however, that if the artifacts from which these dates were obtained were contaminated, later rather than earlier dates should be expected. A second difficulty is that, if the Marshall islands are the homeland of the Micronesian peoples, consistent with Bender's hypothesis that the homeland should be in the east, then we need to find an explanation for the fact that indigenously produced pottery is found on the high islands of Kosrae, Pohnpei, and Chuuk. Pottery cannot be manufactured on atolls.[36] A third concern about this date is that, as a consequence of changing sea levels, the Marshalls might not have been available for settlement at that time.

The issue of how Holocene sea levels might have impacted the colonization of Micronesia, and more generally of Remote Oceania, is one that deserves, and no doubt will receive, increased attention in the future. Nunn (1994b:14) offers the following significant observation about Bikini: "Most authorities believe that vertical reef growth lagged behind early Holocene sea-level rise for at least part of the time in most places . . ., a condition which was demonstrated for . . . Enewetak and Bikini Atolls in the northern Marshall Islands."

Nunn suggests that these atolls became habitable only after reef growth caught up to current sea levels in the first or, possibly, in the second millennium B.C. In light of this scenario, the 1500 B.C. date from Bikini must be viewed with even more suspicion.[37] The possibility that many Pacific atolls and coastal plains only recently emerged also provides a feasible explanation for the paucity of $C_{14}$ dates prior to the B.C./A.D. border elsewhere in Micronesia, as well as in Eastern Polynesia, especially if atoll corridors were involved in the colonization of these regions.[38] However, Irwin (1992:125) notes: "in a recent review of Pacific data, Clark (1989) emphasizes the effect of local variations on wider patterns, and it may be premature to build Pacific-wide models of rising or sinking land and sea into archaeological theories of settlement."

It must be kept in mind that it is only since the late 1970s that much archaeological work has been done in Micronesia; consequently, our understanding of the prehistory of this region is quite uneven.[39] Recognition of the fact that sea levels might have played a significant role in the colonization of these islands is even more recent. But, if the upper levels of Jackson's tree are

correct, or largely so, then more dates like those listed in the possible column should be expected, from high islands or from atolls available for habitation circa 3000 B.P.[40]

**7. CONCLUSION.** I have attempted in this paper to make two major points. The first is that the linguistic model of Micronesian settlement is not necessarily flawed in the way Irwin believes it to be. Considering how influential Irwin's book is (and, in my opinion, deserves to be), I feel obligated to make this point publicly. The second is that Irwin and other archaeologists are nevertheless sometimes justified in being suspicious of claims about settlement models based exclusively on linguistic trees. Trees can be used to draw inferences about dispersal patterns, but only if appropriate caution is exercised.[41]

Archaeologists, though, also need to be cautious about employing contact as an explanation for linguistic groupings. The prehistory of Micronesia, as a case in point, appears to be far too complex to permit such a simplistic hypothesis. Further, even if considerably more interaction than we previously assumed took place among the Micronesians, it is linguists rather than archaeologists who may have the better tools to detect it. Cultural traits and the know-how associated with the manufacture of items of material culture can diffuse without leaving archaeological traces. The comparative method, however, often enables us to identify the linguistic borrowings that are commonly a consequence of such contact.[42]

Bayard, in a review of Bellwood's *Prehistory of the Indo-Malaysian Archipelago* rightly praises Bellwood, an archaeologist, for his "invaluable use of the Austronesian linguistic evidence." He further comments (1987:116): "There may yet be a few prehistorians who remain unconvinced of the utility of such data to our discipline; if so, I can only urge them to become more acquainted with linguistic methodology. Its limitations . . . are far outweighed by its value, and to ignore its implications in our present state of knowledge is simply foolish."

## NOTES

1. I wish to thank Terry Hunt for first prompting me to think about some of the issues discussed here. I also want to express my appreciation to William H. Alkire, Byron Bender, Robert Blust, Joel Bradshaw, George Grace, Robert Gibson, Paul Lassettre, Jeff Marck, Kimi Miyagi, Albert Schütz, and an anonymous reviewer, all of whom read and commented on one or more earlier drafts of this paper. I alone am responsible for its shortcomings.

2. Pawley and Green (1973:4–5) divide Oceania into two regions. Near Oceania includes all of Oceania west of the Reef–Santa Cruz Islands. It encompasses the western half of Melanesia, an area containing many relatively large, intervisible islands. Remote Oceania comprises the remainder of Oceania, including the eastern half of Melanesia and the far-flung islands of Polynesia and Micronesia.

3. Many, in effect, endorsed Sharp by their silence.
4. In a Guam commencement address delivered in 1985, the noted botanist Francis Raymond Fosberg observed: "The concept of insularity ranges, in different people's minds, from that of a narrow restricted outlook, or the deprecating attitude of the person who speaks of himself as 'stuck on this rock,' on the one side, to the idea expressed by the schoolboy from Ponape who said his island was a wonderful place because it was surrounded by water, which made it easy to visit other islands" (McCutcheon 1994:351–352).
5. See, for example, Terrell 1988.
6. The concept of "islands as laboratories" is, of course, an old one. Nunn (1994a:12) quotes Alfred Russell Wallace (1895:241), who wrote: "Islands possess many advantages for the study of the laws and phenomena of distribution. As compared with continents they have a restricted area and definite boundaries."
7. In this paper, I employ new spellings for Micronesian place names—Belau (Palau), Chuuk (Truk), Pohnpei (Ponape), Kosrae (Kusaie), Kiribati (Gilberts)—but conventional English spellings for all language names. This practice is based on one employed in Hawai'i, where the place name is spelled with a single opening quotation mark (representing a glottal stop), but Hawaiian, the English name of the language formed with an English suffix, contains no quotation mark.
8. At least occasional contact among speech communities is probable, except perhaps in the case of extremely isolated islands such as Rapanui.
9. Terrell (1986, 1988) provides a provocative discussion of these issues.
10. The Marshall Islands in Micronesia provide an interesting illustration of the effects of contact. The vast Marshall Islands archipelago contains 29 atolls and 5 raised limestone islands in two chains that span a distance of approximately 750 miles northwest to southeast. These islands have apparently been inhabited for at least 2,000 years and perhaps as long as 3,500 years. But, this is also an area of extensive interaction and contact. Today, only a single language is spoken in this region with very minor dialect differences. No other language in Oceania is spoken over such an extensive area. See Rehg and Bender 1990 for related comments.
11. See Terrell 1986, 1988. My characterization of Terrell's position is a paraphrase of Davidson (1988:92), who aptly comments on "Terrell's recent . . . rejection of the linguistic tyranny (Terrell 1988)." I am not aware of an instance where Terrell himself uses the word "tyranny" in association with linguistics, though it is reasonable to infer that he would not object to such usage.
12. An obvious example would be the collaborative efforts of Andrew Pawley and Roger Green.
13. For a discussion of the current linguistic model, see Jackson 1983 and 1986.
14. See Jackson 1986 for an evaluation of claims about the external relationships of Micronesian languages.
15. Finney (1994:97) notes that this was the average speed of the sailing canoe Hōkūle'a on its voyage from Hawai'i to New Zealand and back.
16. This claim is based upon an inference Irwin draws from Marck's work, not upon anything Marck directly states in his paper.
17. As Jeff Marck (pers. comm.) has also noted, the late prehistoric period was probably characterized by increased rather than diminished isolation.
18. Such a tree can be found in Rehg and Bender 1990, where it is employed to demonstrate the possible consequences borrowing might have upon subgrouping hypotheses.
19. Dyen (1965:33–4) earlier noted that Trukic and Ponapeic appeared to be closely related, but his claims were based on lexicostatistics, an approach to subgrouping most Micronesianists consider unreliable.

20. Jackson, much to his credit, does not sweep such data under the rug. He is to be commended for his scholarly integrity.
21. Carolinian is also spoken on the high islands of the Northern Marianas, but as a result of migrations within historical times.
22. All cognate percentages are from Jackson 1983:275.
23. Wurm and McElhanon (1975), for example, propose that, to qualify as dialects of a single language, speech varieties should share 81% or more of their core vocabulary.
24. See, for example, Alkire 1965, Lessa 1966, and Jackson 1983.
25. Some Trukic peoples have maintained and still practice these maritime skills. Mau Piailug, the first navigator of the Hawaiian voyaging canoe Hōkūle'a, for example, is from Satawal.
26. Bob Blust (pers. comm.) observes that "the early Indo-European dialectologists concluded that the wave model usually worked best with dialect continua, and the family tree with more distantly (and geographically separated) languages."
27. For example, see Quackenbush (1968:76), where he provides what he labels "a composite map of the chief phonological isoglosses" of the Trukic continuum.
28. See Jackson 1983 for further discussion.
29. Grace discussed paraphyletic groupings in the keynote address delivered to the symposium on Reconstruction and Classification in the Austronesian Language Family at the 15th Pacific Science Congress in 1983, later published as Grace 1985. He takes the term from biology, specifically from Mayr (1981:514), who states, "A paraphyletic taxon is a holophyletic group from which certain strikingly divergent members have been removed."
30. Figure 5 is my interpretation of Jackson's claims. In it, I have employed the notation system used in Ross 1988.
31. In order for this explanation to work, however, substantial differentiation within Trukic would apparently have had to occur before Proto-Ponapeic split off. I am not yet entirely convinced that Ponapeic arose within Trukic.
32. See Rehg and Bender 1990 for a discussion of the linguistic consequences of contact between Marshallese and Mokilese.
33. See Pawley and Green 1984, Ross 1988, and Blust 1993a, b.
34. See Rehg 1991 for a discussion of overlapping isoglosses that are attributed to drift in Micronesian languages.
35. These dates are from Irwin 1992 and Rainbird 1994. Since I am not an archaeologist, I urge readers to exercise caution in accepting my interpretation of these dates.
36. Jun Takayama (1981:1) speculates that "at least the central, volcanic, high islands of Truk, Ponape, and Kosrae may have been initially settled from the West (possibly Yap) by migrants carrying with them calcareous, sand-tempered pottery, prior to or around the beginning of the Christian era, and . . . this area was subsequently settled from the East (eastern Melanesia or western Polynesia) by people without pottery." I am currently unaware, however, of any evidence, either linguistic or archaeological, that would support such a settlement sequence. Irwin (1992: 128) suggests: "the first colonists and the earliest pottery could have arrived separately and by different routes." Of related interest are Alkire 1984 and Mauricio 1987.
37. See Tracey and Ladd (1974), Dye (1987), Streck (1990), and Nunn (1994a, b) for varying opinions on when Bikini might have been available for settlement. High islands in Micronesia, though, were obviously not awash at this time, nor apparently were the atolls of Kiribati (Nunn 1994b:14).
38. See Marck 1995.
39. Hunter-Anderson and Graves (1990:7) note: "the atolls [of Micronesia] continue to be under-represented in archaeological surveys and excavations." Shun and

Athens (1990:236) note that the sequence established in the Marshalls is tentative and further observe: "it may be found that a pattern of continuous use of very limited available land on atolls has hindered the preservation of early deposits and sediment layers."

40. B.P. (before present) technically means before 1950.

41. Linguistic trees, where appropriately employed, can however be used to make inferences about population movements. See, for example, Dyen 1956.

42. See Rehg and Bender 1990.

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