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Book Review Forum

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BOOK REVIEW FORUM

Glenn Petersen. *Traditional Micronesian Societies: Adaptation, Integration, and Political Organization.* Honolulu: University of Hawai'i Press, 2009. Pp. 288. ISBN: 978-0-8248-3248-3. US$42.00 cloth.

Review: Laurence Marshall Carucci
Professor of Anthropology, Montana State University

In his comparative work on the inhabitants of the vast swath of the Western Central Pacific region that has come to be known as Micronesia, Glenn Petersen adopts a functionalist, ecological adaptation perspective to analyze social organizational components of the varied societies that are found in this sector of the globe. Even though Micronesia is a European category that local people do not use to ground their own senses of identity, Petersen argues that Micronesia makes sense as a viable culture-area construct since it reflects a set of common cultural adaptational strategies that are shared by traditional settlers of this part of the Pacific, though not evidently by recent newcomers, who have not been required to adhere to the same principles of adaptation. The salience of "traditional Micronesian" adaptations is borne out, Petersen claims, by their deeply embedded and long-lasting nature.

Petersen's work paints Micronesian social practices with a broad brush in order to posit a general set of social features shared by all Micronesian societies. While the author relies on a wide array of archaeological, ethnological, and historical sources to inform his work, little time is dedicated to ascertaining how these sources are themselves differentially constituted in ways that reflect certain theoretical biases and epistemic contours. Instead, *Traditional Micronesian Societies* adopts a classical comparative anthropological approach that requires an equivalence of analytic categories. Not

unlike Julian Steward, Peterson turns his readers' attention to elements of everyday life that fall within the domains of kinship, land tenure, and political organization—the core cultural categories considered most critical to establish a common grounding for the way traditional Micronesian peoples once lived their daily lives. Such an analytic dissection of everyday life marginalizes other recurrent features of activity among West Central Pacific peoples, including those that bridge into the Euro-American domains of cosmology, religion, and ritual practice. These conceptual domains and activities are all condensed in a single chapter (Chapter 8) of *Traditional Micronesian Societies*, pointing to their diminished importance in Petersen's view.

In the author's analysis, the core set of regionally shared sociopolitical features provides ample evidence of a past era of unified social practices. The first such shared feature is an organization into matriclans with constituent matrilineages. Petersen contends that this type of social organizational structure provides the flexibility needed for people to maintain expandable networks of kin that have proved adaptive for people who have had to live with the intermittent stress of an environment made unpredictable by typhoons and drought. A secondary organizational principle that can be found across the region is the "interweaving of lineage and land" (185). Here, too, Petersen sees the Micronesian version of a classical pattern to be one that provides added flexibility in order to adapt to highly variable local conditions. He posits a first settlement scenario under which ranked "lineages" have a priori claims to certain land parcels, but, at the same time, Petersen recognizes that a very different principle—actively working an area of ground—provides a contravening way for Micronesian people to demonstrate connections to land (105 et seq.). Rank, also aligned with land-holding practices, forms the tertiary political component of Micronesian social organization that Petersen believes to be adaptive, and chiefs are the most marked institutional manifestation of the principle of rank by "matrilineal primogeniture" (176). In the ideal, chiefs provide leadership, and Petersen imagines them as functionally beneficial persons able to organize and coordinate social activities and keep exchange networks flowing (185).

Peterson hopes this work will be a general text for anthropology students, a work young Micronesians may consult for a "respectful account of their ancestors' lives," and an account that resonates for fellow Pacific scholars (3). Indeed, *Traditional Micronesian Societies* is the first work of its type in well over a generation, and it is unquestionably the most thoughtful work of this genre to have been written. Certainly, the book is a must read for Pacific scholars inasmuch as it offers a comprehensive analysis of a number of the most important works on the inhabitants of the region.

At the same time, in my view, the analysis is hampered by Petersen's theoretical dependence on a functionalist paradigm that is unable to account for the multifaceted array of symbolic domains and social practices that must be analyzed in a dynamic, historically sensitized fashion for each of the societies and social arenas in the region. Petersen's functionalist theory works from the top down, forcing problematic classifications of divergent social practices onto activities that vary temporally and that are contested depending upon the positionality of social actors on the ground. The condensation of these varied practices is necessary to Petersen's thesis, since adaptational forces, while presenting some very broad constraints on cultural forms, are simply inadequate to account for the diverse and dynamic, historically and intersubjectively nuanced nature of cultural actions in this part of the world. Indeed, Petersen's own line of argument seems to admit the inadequacies of an adaptation-grounded theory. At several junctures, most notably with his chapter on "exceptions"—Kiribati, Nauru, Yap, and the Mariana Islands—Petersen suggests that such things as Polynesian influences from Samoa (in the case of Kiribati) change the shape of cultural patterns. While undoubtedly true, the ready reformulation of the social forms developed in other socioenvironmental settings contradicts the thesis that the social forms emerged to fulfill local adaptive functions. Petersen's oversimplification of such complexities also includes his acceptance of a view of "traditional" that predates the entire "invention of tradition" literature and, therefore, fails to recognize the critical ways in which tradition is an imagined feature of an ever-emergent, constantly changing, cultural epistemic imaginary.

Equally, of course, readers must accept the idea that each of the core categorical features that Petersen posits for the societies of the West Central Pacific are immutable and long-standing cultural beliefs or practices of the peoples who inhabit this region rather than contestable interpretations framed by European and Asian observers with their own historical and theoretical biases. As part of his comparative agenda, Petersen reasserts the Euro-American institutional domains—social, political, economic—without amply questioning their legitimacy. In so doing, Petersen sidesteps several long-standing critiques of such a priori institutional analyses. For example, in his discussion of the Fortes/Worseley controversy, Sahlins (1976: 6–18) points out the problem that Fortes creates for himself by artificially subdividing Tallensi social action into the Euro-centric domains of kinship, politics, economics, etc. (also see Schneider 1984, Chapter 15). In adopting such an analytic approach, Fortes refashioned unitary Tallensi practices into activities that fulfilled different social functions. The apparent functional differences then provided Worseley with the opportunity to critique

the ethnographic construction of Tallensi social action precisely because, in Fortes's rendition, kinship-grounded activities fulfilled economic functions and, therefore, came to be expressed in economic terms. Sahlins, of course, argues that the entire controversy was ephemeral inasmuch as Tallensi themselves do not segment their activities into distinctive institutional domains.

In *Traditional Micronesian Societies*, Petersen engages in an analogous "exercise in classification" (Sahlins 1976, 14). Not unaware of local complexity, Petersen begins cautiously, encouraging his readers to adopt a Socratic idea of "politics," the core institutional domain of interest in this book. Socrates's more inclusive, but pragmatically restricted, view of politics refers to "what people want their communities to do for them and how they set about achieving these goals." In the very next paragraph, Petersen returns to a much more circumscribed interpretation, wherein government means only the "formal structures of political life" (125). By chapter's end we have political and economic systems, sociopolitical rank, and lineage descent groups operating along principles of genealogical seniority and nested hierarchy, land tenure systems controlled by those descent groups, and a selective array of local activities that have been fitted into these Euro-American inspired analytic categories (Chapter 6). While Petersen recognizes the variations and flexibilities in local practices, nonetheless he reimagines the unified social acts of the residents of many social arenas in Micronesia and provides them with a certifying mark as political, economic, or religious activities. Those activities then take on a foreign contour unsuited to the performative contexts and analytic universes in which they were imagined and made meaningful. Having fractured the actions into their proper functional domains, Petersen then seeks to demonstrate an equivalence of kinship principle or political form from one edge of the region to the other. For Marshall Islanders (whom, I presume, may share this feature with other residents of the West Central Pacific), it is hard to imagine any activity that does not have what Europeans or Americans might consider a political dimension. For Petersen, however, the politicized dimension of seating arrangements in a cookhouse are part of the domestic sphere and, therefore, do not figure as political activity. Equally, virtually every Marshallese activity involves elements of exchange, but this is inadequate to have Petersen place all such activities within the economic domain. To presume that a select set of daily activities marks them as primordially "political" or "economic" robs those activities of their character as what Mauss called "total social facts" and extracts them from the historical trajectory in which local actions may, at a specific historical juncture, come to be conceptualized as "religious," "political," or "economic" in ways that overlap

with, but are not identical to, the religious, political, or economic domains imagined by European observers. A good example of this emergent view of local "religious activity" can be seen in John Barker's work with the Maisin or Joel Robbins's work with Urapmin (Barker 2008; Robbins 2004). Similarly, the group I know best, Enewetak/Ujelang Marshallese, develop uniquely religious components of their collective identities as one component of an emergent historical project of reimagining themselves as part of a cosmopolitan world community. Analogous arguments can be made about islanders coming to classify their own activities as specifically political or economic in ways that clearly demonstrate the inappropriateness of presuming that residents of the region known as Micronesia, during the long-unchanging *durée* of traditional times (as imagined by Petersen), considered their actions as inherently political, economic, religious, kin-grounded, etc.

Similar problems exist with each of the core cultural categories that Petersen sees as the principles that unify Micronesia. Most obviously, Petersen asks his readers to think of all Micronesian societies as sharing a common grounding in matriliny. Applying the lineage concept to Pacific societies in general is highly problematic, as extensive literature from J. A. Barnes to Schneider, among others, has pointed out. But Petersen avoids this controversy. The oversight is most notable in the author's selective use of the work of David Schneider, "one of American anthropology's preeminent theorists and an ethnographer of Yap" (244). Peterson references Schneider's early work numerous times but avoids serious consideration of Schneider's *A Critique of the Study of Kinship*, wherein Schneider uses Yap to demonstrate the nonexistence of kinship as a viable domain of comparative study and implores scholars to take seriously local Yapese ideas about *tabinau* and *genung*, Yapese terms Schneider had formerly translated as "patriline" and "matriline." After years of closer reflection, Schneider found these analytic terms to be entirely inappropriate. Significantly, while Schneider recognized that the comparative method was unable to account for the rich semiotic contours of local categories and social practices, Peterson requires the a priori acceptance of an etic grid of common anthropological terms to undergird his use of the comparative method to demonstrate the unity of social practices across the region. Micronesian traditional societies *must have* a universal matrilineal clan organization to ground the social organizational unity of Micronesia. For Petersen, *genung* must mean matriline in the same way that Marshallese *jowi* must mean matriclan and *bwij* must, therefore, be a Marshallese clan segment, or matriline. Unfortunately, the distinctive characteristics of *genung* (shared belly) get lost in this formula, as conical clan type social forms are recloaked by Petersen into acceptable anthropological lineage garb. The prevalence of

adoption in Yap and the deeply embedded way that *magar* (work) requires a radical rethinking of the basic postulates of what lineage actually means to anthropologists are lost in Petersen's need to discover universally shared Micronesian matriliny. Whereas Schneider suggests that neither *tabinau* nor *genung* represent anything remotely similar to biological identity transmitted across generations, Petersen reinvents them as lineage forms. Ironically, Peterson does not even consider Schneider's first formulation of Yap as a society with double descent—with patrilineal-like *tabinau* the dominant/public social form and *genung* the "hidden," unmarked, form that has few social "functions" outside of defining appropriate partners in a marriage. Like Kiribati, such a patriline-dominant formulation could only create additional headaches for Petersen's thesis of universal Micronesian matriliny. Indeed, for Kiribati, which Grimble considered to be patrilineal, Petersen sees the "patrilineal emphases now obvious in Kiribati social organization" (214), as a substrate of the underlying (necessarily earlier) flexible and nested characteristics of Kiribati social forms. In Peterson's view, these characteristics point to "an underlying pattern ... rooted in Micronesian matriliny" (214). Petersen's thesis requires readers to agree with projected waves of invented culture histories to explain the contorted appearance of anthropological classificatory categories that simply do not exist on the ground. Rather than working from the ground up, from *tabinau* and *genung*, for example, to meaning and practices that shift through time, Petersen works from anthropological categories like matriliny down to the highly varied social forms that are the momentary products of historically contoured local imaginaries and highly varied in social practice. Based on my own research among Marshall Islanders and my reading of the literature from other locales, I agree with Petersen that there are filaments of a matri-biased imaginary that can be found throughout this region. But to transform the multifaceted threads of that cultural consciousness into lineage identity grounded in the transmission of biogenetic material from one generation to the next is a radically different proposition about shared social practices. If Schneider is correct, the latter view has no support among Yapese, either in the 1940s or the 1970s, and it has no support among Marshall Islanders with whom I have worked. To contend otherwise is truly the invention of an anthropological tradition to provide categorical support for an imagined Micronesia.

The ways in which residents of Micronesia interweave their constructions of person and land in my estimate seem to be widespread throughout the region, even though the symbolic imaginary used to lend specific contour to this idea varies from place to place. Therefore, both in terms of flexibility and widespread distribution, Petersen may well be on target. But

these features also extend into parts of the Pacific beyond the Micronesian region. Moreover, the flexibility of these patterns is far more notable than is Petersen's idea that ties to land will be interwoven with matrilineally grounded identities. As early as 1949, Spoehr raised serious doubts about the lineal character of the supposed matrilineages on Majuro Atoll (Marshall Islands), and certainly on Ujelang and Enewetak, outliers to the Ralik Chain of Marshall Islands, all claims to land are bilateral in character, aligning with the contours of *bwij*, or bilateral extended families. *Bwij* means "umbilicus/belly button," with a direct link to feeding/nurturance and some association with females who are, in some idealized sense, expected to be nurturing. But, other than the fact that Ujelang/Enewetak *bwij* are typically (though not universally) defined as female-headed, there is nothing in their character that makes them lineage-like. They take the shape of ramages or conical clans, and membership may be claimed through many avenues, not only through an umbilical link at birth. In particular, acts of feeding (not only feeding through an umbilicus) are frequently used to create a pathway to *bwij* membership. Moreover, there is substantial evidence that the contemporary linkage of identity and land is historically constituted and becomes a predominant way of grounding identity as daily activities shift away from sailing and fishing toward copra production in the late 19th century. Thus, the "mud and blood" hypothesis may itself be daubed together as the contours of daily life shifted for Marshall Islanders and others during that particular era. All this is to say that Petersen is observant in his recognition that land and identity are closely intertwined, but to project this congeries of symbolic alignments onto the ossified imaginings of a long-standing past is beyond what can be demonstrated by a close assessment of a variegated array of Micronesian pasts.

When Petersen turns his attention to issues of rank and chieftainship, he is hampered by some of the same constraints of method that have been noted for the other domains he has selected for analysis. My own experience in the Marshall Islands suggests to me that Petersen is probably correct in thinking that considerations of rank are deserving of particular scrutiny when investigating social relations in this section of the world, though I do not believe there is a sharp line that meaningfully separates the types of formulations found in Micronesia from those in parts of Polynesia or Melanesia. However, rather than viewing rank as a conceptual model grounded in certain counterbalancing principles that are continually tested and contested on the ground, Petersen focuses on the social relational components of chiefly action and ultimately sees all Micronesian people manifesting some institutional variant of the principle of rank by "matrilineal primogeniture" (176). Unfortunately, this is simply not true,

inasmuch as Ujelang/Enewetak chiefs are, in accord with the dominant model, governed by a principle that is rationalized (most consistently by those who are members of the chiefly lines) as patrilineal. Equally, while primogeniture is intermittently evident in the local historic accounts of these lines, other principles are also considered in the selection of any particular chief. Given his own reliance on an etic typology to typify Micronesian chiefs, Petersen's critique of Sahlins's overly general formulation of the contrast between chiefs and big men as "abstracted sociological types" (246) seems particularly ironic. While informed by a substantial knowledge of Pohnpeian chiefly practice, ultimately Petersen relies on a comparative typology of Micronesian chiefs that is separated only by the level of generality when compared with the comparative schema proposed several decades ago by Sahlins. Unquestionably, Sahlins's big man/chief typology is an ideal-type formulation that posits characteristics so general they are of limited use in accounting for the vast array of particular formulations and practices dealing with rank that are found in the Pacific. But, Petersen's proposal that matrilineal primogeniture serves as an appropriate designata for the diversity of rank formulations found throughout Micronesia suffers from precisely the same dilemmas of overgeneralization and reification as Sahlins's formulation. Of course, being an astute scholar of Pohnpeian social practices, Petersen correctly notes that the principle of matrilineal primogeniture is often modified in practice by performative elements that require the best chiefs to display the aspirations of Sahlins's big men (247). Certainly this is true in the Marshall Islands, where extant chiefs are far from impersonal in their activities. In practice, Marshall Islands chiefs are always judged by their ability to counterbalance elevated rank with practices of generosity and "caring for" those who indulge them with a sense of superiority. Nevertheless, this pervasive feature that always places a constraint on the power of extant authority figures simply cannot account for the fact that certain forms of social hierarchy in Micronesia, much like Ujelang/Enewetak chiefs, are the antithesis of matrilineal primogeniture.

Obviously, while Petersen's field of interest is selective—largely dealing with concerns of anthropologists and others with an interest in political economy—his coverage of the literature is stellar. The problem I have with Petersen's analysis has to do with his theoretical and methodological focus. Nevertheless, these analytic choices have very real implications for any understanding of the worldviews and social practices of the peoples who inhabit the West Central Pacific. Nowhere is my disagreement with Petersen's method more obvious than in his discussion of chieftainship. In an extended footnote, Petersen recounts an exchange with David Schneider in which Schneider argued that the use of the English word chief was

problematic, since he doubted that it was truly meaningful. That is, "there was, even within Micronesia, no indigenous category or concept that was consistent enough to be translated by a single English-language term" (244). In contrast, Petersen says, "there is little doubt in my mind that Micronesian societies do share a common sense of chieftainship," with differences within any society as great as differences among various Micronesian societies (244).

This footnote is particularly important since it deals with what Schneider considers a significant problem—the issue of translation. As he suggests for Yap, it was his translation of Yapese statements and actions that led him to suggest that *tabinau* meant "patriline" and *genung* meant "matriline." It was Schneider's own semiotic practices that caused him to refashion Yapese practices into the reified categories of anthropology in his "first description" of Yap. In Schneider's second description, translation remains a messy matter, but he moves directly from *tabinau* and *genung* to rough approximations of what local Yapese mean by these terms, eliminating the anthropological categories. Of course, Schneider's perspective led to a reification of shared cultural meanings and placed comparative projects on the back burner. Petersen works very hard to resurrect the comparative project and also remain attuned to local discourses and social actions. This commitment is critical, since contemporary anthropologists clearly recognize that cultural practices are historically emergent and, ultimately, embedded in larger interactive contexts that are regional or even global in scale. Nevertheless, by reintroducing the anthropological categories and forcing local meanings and practices to submit the primacy of their own contextually refined meanings to the formal meanings of the analytic categories, Petersen requires his readers to adopt an unacceptable solution to the comparative project. Apparently, Petersen feels compelled to follow Guyer's logic (Petersen 2009, p. 85n1)—"one has to use descriptive terms"—to relegitimize the discursive categories of analytic kinship. If Schneider imagined culturally distinct and potentially irreconcilable "senses" of dealing with concerns of social hierarchy throughout the region, Petersen's theory requires him to project a priori anthropological categories, including a shared category "chieftainship," onto all Micronesians.

In contrast, as a scholar with long experience in the Marshall Islands, I have absolutely *no* idea whether Micronesians share a common idea about chieftainship, and I am not sure this is actually knowable by any single anthropological researcher. Indeed, even Marshall Islanders do not share a cohesive sense of *irooj*, much less common senses of chiefs throughout the region. What is eminently clear is that there is a huge gap between Marshallese people's statements about the abstract idea of *irooj* (as well as

comments about how ideal chiefs once acted), the practices of any particular person who claims to be a chief, and contentions about the adequacy of that person's actions as a chief. By failing to systematically separate for his readers this distinction between the (locally contested) conceptual idea of *irooj*, the actions of those who claim to be *irooj*, and discussions about any person's adequacy as an *irooj*, the waters become more than slightly cloudy. What about the further claim that "Micronesian societies ... share a common sense of chieftainship"? I presume that Petersen means that the members of those societies share a common sense of chieftainship. Equally, readers must think carefully about the grounding of Petersen's claim, i.e., that he has witnessed people from across the region discussing this common sense of chieftainship. These regional claims require a much larger leap in reimagining what all residents of a large sector of the Pacific may believe, even if we take what they say they believe as representative of their conceptual notions about their actual beliefs (and ironically, in other intellectual contexts, Petersen and I share a belief that what people from Pohnpei or the Marshall Islands say in a certain social setting is often specifically designed to obfuscate what they may believe or, at least, the types of things they say about the same topic in other social contexts). In discussing a facet of this issue with an Ujelang-Kosraean man and another Ujelang resident several years ago (in the Ujelang dialect of Marshallese), the Ujelang man said: "Deacons and elders are the *irooj* of Kosrae these days," and the Ujelang-Kosraean man responded, "Well, sir, there were once Kosraean *irooj*, the thing is, well, almost like *irooj* in the atolls of the Marshalls, but today, well those elders in the church, they walk about acting like *irooj* even though they have no *maron* (right/ability/legitimate claim)."

The complexities of Petersen's highly overdetermined claim that all people in Micronesia share a common sense of chieftainship are apparent here. In my interpretation, the Ujelang man suggests that church elders are today's "*irooj*" in Kosrae. In other words, Kosraean chief-like beings are close enough to *irooj* to fall within the Marshallese category, and deacons and elders fit that category. But, the Kosraean/Marshallese consultant doubly contested this view. For him it was not clear if Marshallese chiefs, *irooj* (already lumping considerable differences between Ujelang chiefs and other Marshallese *irooj*, something Ujelang people frequently avoid), and the once existing Kosraean chiefs were the same. His interjection suggested they are sort of the same, but not entirely. And then, for this consultant, the idea that Kosraean church elders are chiefs exceeded the acceptable limits of what it means to be a chief within his own view of the world. Unfortunately, I did not ask whether the church elders were not chiefs because of the way they acted, on account of their lack of a pathway to a

chiefly line, or because they were not *inea* (that is, they lacked supernatural power/*mana*).

To complicate this scenario, current-day Enewetak/Ujelang people (along with many other Marshall Islanders) contest the very idea that today there are *irooj* in practice at all. Of course, in making the statement, those who hold this view demonstrate that each of them does have a conceptual category of *irooj*, even if there are no longer any living *irooj*. It is just that today's *irooj*, like the Kosraean church elder/*irooj*-like beings, do not exhibit the characteristics that would allow them to fit that person's culturally imagined category. They refer to today's would-be *irooj* as *riap in irooj* ("false chiefs," *riap* meaning literally "lies"). And today's "nonchiefs" take several forms. In the Marshall Islands, this may mean that today's *irooj* are in reality only *bwidak in irooj* (the descendants of male members of matriclans whose female members once could birth "real chiefs"). It may also mean that today's *irooj* are "bourgeoisified chiefs" (Carucci 1997a) who, all too often, act in selfish ways rather than in proper ways (by distributing their wealth in accord with the practices of imagined chiefs of old). Or on Enewetak, it may mean for local people that the patrilineal-linked pathway through which today's *irooj* claim their right as "chiefs" was invented just over 100 years ago, in line with the desires of German administrators, and, prior to that time, identity as an *irooj* could only be claimed through females (as in the remainder of the Marshall Islands). Of course, this latter claim is highly contested by those who assert that they *are* the contemporary *irooj* because their fathers (by birth or adoption) were *irooj*. Until recently, they could track their chiefly roots back to Aninij, Boninij, and other figures who constitute the primordial array of Enewetak chiefly persona, just as the would-be *irooj* with matrilineal-linked claims could do the same (though, of course, those legitimizing pathways were *not* identical). All of this, of course, only points to the reasons it is critical to keep discourses regarding the concept of chiefs separate from chiefs on the ground and from the statements about the adequacy of the practices of anyone who claims to be, or even acts like, a chief. What is imminently clear is that Schneider, working from local categories up to the analytic categories that anthropologists valorize, had good reason to doubt any universalizing claims that are required by a comparative functionalist model. Even if all residents of the Micronesian region do formulate comparable discourses about the idea of chiefs as a way to conceptualize rank, it is quite clear that matrilineal primogeniture is an inadequate way to typify what is shared in the nature of these beliefs.

Indeed, when it comes to his discussion of matriclans and theories of chiefly relationship, it is my belief that Petersen does himself a disservice by his distrust of local accounts of the past (i.e., 63n34). The reason for

Petersen's differential ranking of certain cultural features over others escapes me, inasmuch as all are statements about various parts of a cultural imaginary that people selectively deploy to make sense out of their world. Nevertheless, there are real effects to Petersen's overreliance on stories about contemporary social organizational practices and his distrust of accounts of the past. This is particularly important in relation to chiefs since, as I have just noted, the *talk about chiefs* is every bit as important as the daily practices of chiefs and, indeed, the two exist in a complementary, dialectical relationship to one another. Given the conclusion to his chapter on Politics and Leadership (Chapter 7), I believe that Petersen and I are in agreement on this point, though the way in which our arguments are framed are quite different. Here, Petersen talks of the "contradictory dynamics of these (social structural) principles and practices" and the ways "people make conscious use of centralization and decentralization, hierarchy and equality, shared knowledge and concealment, people and place, and tribute and redistribution" (186) pointing out that dynamic outcomes of the application of these counterbalancing principles in practice would make it extremely difficult to reconstruct what an original regional social structure might have looked like. In the case of chiefly hierarchy, however, we do not have to know what the shape of an original social structure may have been to know what local people believe those shapes to have been. Whether actually true or not, these primordial ideas have direct effects on practices in the current day. Thus, in terms of primogeniture, one of Petersen's universal features for ascendancy to chieftainship, Marshall Islanders have various versions of the story of Jebro and Loktanur, primordial deity/chiefs of the Marshall Islands, which already provide a template for some critical expectations of *irooj* in the current day. Indeed, while the story (which I have analyzed previously [Carucci 1997b]) does reference the conceptual possibility of chiefly claims through primogeniture, it also (in several variant versions) provides a type-case in which the characteristics of "loving and caring for others" are even more important characteristics of Marshallese chiefs. In the story, it is through the actual enactment of these practices that Jebro, the youngest sibling born to Loktanur, the primordial chieftainess, comes to become the chief of the Marshall Islands or (in other renditions) to share the position of *irooj* in a seasonally shifting manner with his oldest sibling, Tumur. Again, as with most of Petersen's analysis, I agree wholeheartedly with his emphasis on the multivalent character of local practices, with their flexibility, and with their dynamic balance and seemingly contradictory character. It is his requirement to universalize categories and principles and apply them across the board to all of the members of all of the societies in Micronesia for all of "traditional times" with which I have a problem. In this case, the story of the primordial

Marshallese chiefly family, Loktanur and Jebro does contain an element that suggests "inheritance by primogeniture," but, at the same moment, it suggests a more important principle that must characterize chiefs—a principle that, in fact, gives church elders, or the most generous and caring of them, an equally legitimate claim to be considered an *irooj* as anyone who may fall along a pathway of matrilineal- or patrilineal-ascendance to the chieftainship. On Enewetak, for those who wish to legitimize the patrilineal-ascendance model, there is a huge gap where one older male sibling is entirely overlooked as the proper person to hold the position of *irooj*. The rationalizations for why this person did *not* become chief are multiple, but all focus on social relational inadequacies of this person and of his close relatives. Not until the House of *Irooj* was created with the founding of the Republic of the Marshall Islands did a member of this family finally occupy the Enewetak/Ujelang seat therein. While this family member was not the first to be seated, a local decision to elect appropriate members to occupy this formal position, and the decision to rotate among various people with claims to be an *irooj*, reversed the long-standing decertification of members of this pathway—a route that certainly held the most legitimate patrilineal-pathway claims of primogeniture among the patrilineal claimants on the atoll. People adopted a "let's see" attitude since, given a new set of social conditions, if the person occupying the Council of Irooj seat proved to be as stingy as the rationalizations about this family of miscreants contended, if that person proved as unable to watch over the members of the atoll as they were unable to watch over their own extended family . . . whatever the "ifs," the selection would only be temporary, and a new choice could be made during the next election cycle. Conceptual frames, while far more consistent than daily practices, are themselves dynamic. They are fashioned in relation to a far greater number of inputs than just adaptive constraints of an island or atoll environment.

For these reasons, I believe that Petersen's model is inadequate to the task he has set for himself. He tells us about political organization as government, when emergent and shifting ideas about "governmentality" (Ong 1996) would provide a much more powerful model. Petersen works from the top down, attempting to find evidence of universally shared chieftainship and of shared matriliny. In the case of chiefs, Petersen tells us that what he has learned about people's common understandings of chiefs today somehow indicates that these shared ideas are not an emergent historical phenomenon but have been around since precolonial times. In terms of matriliny, Petersen overlooks the very real possibility that the entire idea of lineality may not have conceptual validity for Pacific Islanders, even if patrilineal and matrilineal linkages of various sorts do appear in the conceptually varied array of symbolic devices that West Central Pacific Islanders

have designed to interweave their identities with one another and with the sea or the land where their daily lives are lived. Rather than relying on an imagined traditional base line of cultural forms, Ong encourages us to see the way that local people engage with a shifting global dynamic to produce emergent forms of power at both the conceptual level and at the level of practice. In the case of chiefs, I would argue, everything we know as a result of reading manuscripts that presume to inscribe past practice must be analyzed as part of the shifting global dynamic that Ong suggests. Given these complexities, using the Marshallese practices I know best as an example, I fear we know nothing substantial about what *irooj* were like at the moment of European contact.

For this reason alone, I am unsure we can say anything meaningful about "traditional" *irooj*. Nevertheless, subsequent European and American records do provide evidence that Marshall Islanders thought about governmentality in terms of *irooj*, *latoktok* (intermediaries), and *kajur* (strength/"commoners"). However, the effects of colonialism on chieftainship as a classificatory form rapidly and radically transformed the intersubjective understandings of *irooj*/commoner relationships as well as the set of practices that could be observed on the ground. As Petersen and I agree, Europeans presumed Marshallese (and other people in the region) adhered to a feudal model, and they inscribed local chiefs as instantiations of that model. The model suggested that Marshallese chiefs were all-powerful, holding rights to all of the land, and the people were their subjects. Yet, local practices once involved rituals of *kairoojoj*, installation ceremonies, through which chiefs were literally "made" by their supposed subjects. Nevertheless, in their new interrelationships with Marshallese *irooj*, new types of *irooj*/chiefs began to be fashioned. Thus, contemporary local accounts often contend that ancient *irooj* were all-powerful, even though nineteenth-century documents indicate that chiefs of that day were not as powerful in practice as in story form. In 1879 or 1880, "the principle chief and lord of Ralik," considered by Europeans to have life and death decision-making power over his subjects, had difficulty gathering more than a handful of his subjects to work on the pier in Jaluij (Finsch 1893, 22). Indeed, looking closely at the accounts of this now-famous Kabua line of Ralik (the "sunset" chain of Marshall Islands), their practical empowerment clearly depended on access to European technologies, including iron, weapons, and ships. Sixty-five years prior, a visit by Kotzebue had a similar effect on Ratak *irooj*. In both cases, the chiefs expanded their power from one or a handful of atolls to a much larger territory. Equally, the appropriation and use of chiefs as intermediaries in the newly established German copra trade had transformative effects on daily practices. With imported labor considered too costly, local labor had to be used to produce copra,

and the disciplinary authority of chiefs to gain access to local labor was both presumed and reinforced at every juncture.

These comments provide only the briefest outline of the way in which Marshallese *irooj* were gradually refashioned into chiefs by certain formulaic alignments with European expectations that were grounded in feudalism. In lieu of those alignments, we have *irooj*, but no chiefs. Therefore, as Ong might suggest, the contours of governmentality were crafted in direct relation to the categories and practices of Europeans, Japanese, and Americans. Rather than hypothesizing as "traditional," universal Micronesian chiefs who obtained their power through matrilineal primogeniture, and then dealing with the degenerate varieties that result from interactions with outsiders (as does Petersen), Ong's perspective of a varied set of emergent and shifting modes of governmentality, which align with an equally dynamic array of negotiations of power in the specific historical contexts in which those relationships in fact arose, provides a more powerful framework for analysis.

Of course, while imagining "Micronesia" as a valid analytic category, Petersen points out that Micronesia was not a meaningful category to local people, but rather, he argues that it serves as an appropriate geocultural category to classify a set of societies that, in his estimate, were all (matri-) lineage-based, all shared perspectives on rank, and all shared practices that interwove notions of identity and land. As Petersen undoubtedly recognizes from various local perspectives of identity construction, and from Ong's related ideas about emergent modes of governmentality, in certain circumstances (particularly among migrants from this region and undoubtedly in other contexts within the Federated States), "Micronesian" is an emergent identity category that is used selectively in an analogous fashion to the ways the identity categories of "Marshallese," "Chuukese," etc. were used in the recent past. Even though Petersen rationalizes his analytic use of "Micronesia" in relation to Hanlon's view of "Micronesia" as a product of European imagination, Petersen's analysis also certifies the legitimacy of an alternate, ahistoric use of the term. By isolating the era of "traditional Micronesian life" from a dynamic historical perspective, Petersen fails to engage with a variety of indigenous culturally and historically emergent uses of "Micronesian" as a meaningful identity category, a category with multilayered meanings that are of increasing concern to local people.

Peterson's depiction of the flexible nature of social organization in the various societies of Micronesia is appropriate. However, analyzing those flexible contours does not require the adoption of an overly simplified set of universalizing anthropological categories to make sense of the complex cultural historical processes that have shaped cultural practices in these locales. *Traditional Micronesian Societies* is well-grounded in the historical

accounts, and at times Peterson brilliantly critiques those sources, recognizing, for example, that early Marshall Islands' ethnographers refashioned Marshallese chiefs in the feudal mode and, in neighboring Kiribati, that Grimble worked assiduously to explain away matrilineal-biased features to purify his patrilineal depictions. But, Peterson's incisive critiques are selective. In his own theorizing, categories like matrilineal clanship continue to drive the analysis in spite of the multiplicity of variegated local forms and practices that make it up. Ultimately, Peterson's theory forces him to resurrect a kin-based society reliant, in part, on "acts of procreation" that "can be conceptualized in essentially the same terms as Western notions about genetics" (110). David Schneider argued strongly that neither the semantic fields of *tabinau*, nor those of *genung*, could be logically aligned with such a biogenetic formula. Certainly, Marshall Islanders, who I know best, do not share any such genetically grounded formulations. If Peterson's kinship categories are generalized Euro-American forms projected onto the symbolic constructions and daily practices of local people, then "Micronesia" remains unified only through European and American symbolic machinations. For this reason, I question whether Peterson has provided the necessary support to justify the classification of Micronesia as a distinct culture area.

Does this mean that a comparative project on the grand scale Petersen has proposed is simply invalid? The answer is certainly "No!" In spite of the widely varied set of discourses and social practices found in the West Central Pacific, I do not think we have to move back to a Schneiderian solipsism to appropriately analyze and compare the various forms. Indeed, Schneider's cultural categories are themselves too rigid from edge to edge, not allowing for the types of positioned social actors with varied conceptual imaginaries that Bourdieu has encouraged us to incorporate into any meaningful social analysis (Bourdieu 1990, 1991). Even though Schneider notes that Yapese changed their conceptual frames between the time of his own research and that of Labby and his compatriots (Schneider 1984, 28), his theory still lacks the sort of expansive and power-infused dynamic of a comparative theory like Elizabeth Povinelli's (2002, 2006). Rather than arguing from a hypothesized construction of Belyuen belief and social practices, Povinelli argues that those beliefs and practices are both dynamic and internally varied. She allows her readers to hear the internally situated voices of members of the Belyuen community and, simultaneously, makes it extremely clear that Belyuen beliefs do not exist in a cultural vacuum. Rather, community members, while acting in terms that make sense internally, are simultaneously governed by a set of ethical sensibilities and claims to legitimacy that circulate in liberal settler colonies as geophysically separate as Australia and the United States or Canada. The imagined scenarios

that are manifest in stereotypes about how Aboriginal peoples must act, to be heard, as "genealogical" subjects have real effects on the everyday shapes of social action. As Belyuen beliefs and practices change, local people may wish to act in ways that demonstrate their existence as autonomous "autological" subjects. However, both within the Belyuen community and certainly in their interactions in the courts and other settings where Euro-Australians are present, Belyuen people are disciplined and constrained by the requirement to act as genealogically motivated subjects should act. While Povinelli encounters certain dilemmas, including (as she admits) the ability to specify the clearly varied contours of a unitary and epistemically central liberal settler ethos, she provides a dynamic comparative theory with the necessary parameters to explain the types of substantial historical, cultural, and interpersonal variations that Peterson encounters in the complex region he has selected for investigation. Povinelli ties the forms of an historically generated consciousness to the daily practices of social actors on the ground in order to demonstrate how differential access to, and the differential deployment of multisited constructions of, emotional energy and power come to have very real effects on people's lives (Povinelli 2006).

In spite of the complexities created by Peterson's analytic framework, *Traditional Micronesian Societies* is the finest comparative work to date that deals with this part of the Pacific. Certainly, his work represents a much-needed update of William Alkire's *An Introduction to the Peoples and Cultures of Micronesia* (1977). Undoubtedly, *Traditional Micronesian Societies* will become one of the new standard works for students enrolled in introductory courses on the Pacific. In this regard, I see the work in a mixed vein. Clearly, Petersen's engagement with the detailed variants of Micronesian social practices is extraordinarily valuable. At the same time, the theoretical premise of the work will not place students in the most empowered position to demonstrate the critical nature of anthropological inquiry in today's world. Equally, Petersen's attempt to write a "respectful account" of Micronesian pasts for the residents of this region may not be as well received as he hopes. In my estimate, without close attention to local voices and to locally sensitized histories, young islanders cannot understand "why Micronesians do things the way they do them" (3). Indeed, the grand comparative history proposed by Petersen elides the entire significance of a multiplicity of locally contested histories that are of great interest to specific island and atoll dwellers throughout this region. Nevertheless, *Traditional Micronesian Societies* is a critical comparative work and a must read for Pacific anthropologists and historians, as well as for scholars in other disciplines who can appreciate Petersen's substantial knowledge of this section of the Pacific and who have the time to give this work a close and critical reading.

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BOOK REVIEW FORUM

Glenn Petersen. *Traditional Micronesian Societies: Adaptation, Integration, and Political Organization.* Honolulu: University of Hawai'i Press, 2009. Pp. 288. ISBN: 978-0-8248-3248-3. US$42.00 cloth.

Review: Karen L. Nero
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Traditional Micronesian Societies, the first major scholarly overview of the societies of the Northwest Pacific for decades, is a significant contribution to young Micronesians, scholars, and the general public. Petersen offers readers new perspectives of the region, simultaneously drawing upon knowledge his mentors shared with him during his thirty-year experience working in Pohnpei and the region, as well as data and perspectives from all fields of ethnology, anthropology, genetics, botany, history, political science, and philosophy. Following Pohnpeian (and Micronesian) values of respect and deference to both epistemologies, he presented the information in a clear set of discussions, so that the anticipated readers may reach their own conclusions concerning contested interpretations. Carefully chosen lithographs and photographs drawn from early explorers de Freycinet, d'Urville, Duperrey, and Lütke; the later German Südsee Expedition; and the unidentified portrait of a Pohnpeian man on the book's jacket permit the reader to picture some of the peoples and their activities described in the text.

No one argues the Eurocentric origin of the unifying label "Micronesia" (Tcherkézoff 2003) or the often-devastating impacts and influences of the succession of Spanish, German, Japanese, and American colonizers mainly during the twentieth century. Petersen disagrees with assertions by scholars

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Hanlon and Rainbird that "Micronesia" exists only as a colonial construct, arguing instead that there are strong cultural continuities among the peoples of the region who have maintained ongoing social relations through active networks of marriage and exchanges. I have already published a comprehensive review of *Traditional Micronesian Societies* (Nero 2010), in which I agreed with Petersen (and other scholars) that the peoples of the Northwest Pacific (the Marianas, Carolines, Marshalls and Kiribati archipelagos, and Nauru) comprise a Micronesian cultural area as a subset of the Pacific societies. In this book review forum, therefore, I briefly summarize Petersen's contributions and discuss some recent publications, not as a critique but as a continuation of the critical discussions Petersen has challenged us to consider. I focus on Palau, where I have mainly worked as a researcher.

Peterson's cultural ecological approach equally values the region's rich marine resources as well as appreciating its ecological constraints. I laud this extremely well-written and accessible syncretic study describing how the many different settler communities of the region not only survived but at times thrived in their often demanding environment. What did the agriculturalists and fishers find when they arrived at their new homes, and what cultigens, domesticated animals, tools, and ethnobiological, sociocultural, and navigational knowledge had they brought with them? How did they then adapt to their new environments over the last two millennia, during periods of major environmental changes? We do not yet have all the answers to these questions, but Petersen has provided a thoughtful overview of current research on the region, which provides an important foundation for ongoing research both of the early settlement history of the region and contemporary studies.

Petersen identified the "traditional Micronesian" period as the nineteenth century prior to most European contact (4). Despite potential inconsistencies, he used the present tense to discuss this period, in recognition of the underlying "*common framework of organizing social life*" (4, italics added) throughout the region. He argued that "Micronesia Perseveres" (230), and continues to rely upon its traditional matriclans, principles of social organization, and traditional values as they continue to adapt to their changing environments. One such value is the importance of unquestioned sharing of resources. I will not repeat my earlier chapter-by-chapter review, but I will provide a brief overview through this topic. Petersen (2) observed that in traditional Micronesian societies virtually everything a Micronesian possesses is shared with family and neighbors, and every family and community is connected by a web of strands to many other islands and communities. In this way, everyone is ensured of being cared for and protected

when in need. While in contemporary life this may be an ideal not always practiced, with new forms of currencies more easily hidden, daily sharing is routinely offered and at times demanded. Following these values ensures that all are cared for. Petersen identified the *"central point of this book"* (2, italics added) as the ways that the region's *"interlocking lineages and clans"* provided the primary mechanism to ensure this sharing. Throughout the book Petersen continuously shifted focus between the lineages and the clans (Chapter 4). These descent groups inhabited the named houses, so important throughout the Austronesian region, that are grounded in their lands and the labor that reproduces these houses (Chapter 5). Chapter 6 focuses on the descent-based chieftaincies and their governance, related to but separable from issues of politics and leadership (Chapter 7). All of these components, analytically separated but integrally connected, are held together by common aesthetics, beliefs, values, and prescribed behaviors (Chapter 8). Each of these is part of the whole, shifting in importance depending upon the circumstances. It is therefore difficult to identify one overriding or governing principle of Micronesian social organization or to state it in the common regional language of English, much less the nuanced differences found in each of the region's languages.

Eastern Micronesia is quite rightfully Petersen's focus (39–44). The peoples of the eastern and central Carolines, Marshalls, Kiribati, and Nauru speak nuclear Micronesian languages that belong to the Austronesian Oceanic Languages dominant in much of the Pacific. They were settled approximately 2,000 years ago (Dickinson 2003) when the mainly coral islands and atolls, and few volcanic islands, became inhabitable. Scholars agree that initial settlers would have come from the various Melanesian islands, including the Santa Cruz and Reefs Islands that are comprised of volcanic and atoll islands far beyond sight of the Solomon Islands. These islands too maintained an interisland network of exchanges and relied on tree crops. Eastern Micronesia could be considered a culture area in its own right according to many of the normal criteria used to identify subregions within the larger set of regions. Micronesia fits two of Burton et al.'s (1996, 88) criteria for higher-level regions: historical and physical continuity, and homogeneity and pattern, the latter defined in terms of social structure. Closely related languages are another key criterion. However, such a subregion would not include all the island groups included in Micronesia on geographical grounds.

The islands considered Western Micronesia—the Palau and Marianas archipelagos—were settled initially from two different areas of islands in Southeast Asia, as early as 4,500–3,200 calibrated years before present (cal BP) by voyagers speaking Western Austronesian languages. Interpretations

vary whether early pollen and charcoal in core samples indicate human-induced or natural environmental changes. These interpretations are difficult to resolve, since the earliest archaeological sites are buried under current shorelines (Clark et al. 2006; Carson 2011; Dickinson and Athens 2007). Over the next millennia multiple waves of settlers arrived in the Palauan archipelago from Southeast Asia, including the Philippines, New Guinea, and elsewhere in Oceania, not to mention traders from Malay and other areas. Undoubtedly the new settlers brought new cultigens, including the important *Artocarpus altilis* breadfruit, called *meduu* in Palau.

Geographically, Yap is also in Western Micronesia, but current research suggest that it was settled perhaps 2,000 years later than the Admiralty Islands based on linguistic analyses by Ross. Clearly Yap played an important role as an intermediary between Palau and the other Carolinian islands, and more than 24 generations ago the small islands and atolls of Hatahobei and Sonsorol of Palau were settled by voyagers from Ulithi. There is a long history of interactions, exchange, and cultural/linguistic sharing between Palau and Yap. In contrast, the early prehistory of Guam and the Mariana Islands is less understood, partly due to the environmental changes discussed earlier, but see the recent study of Carson (2011).

We do not have the bases from which to argue that Western Micronesian islands comprised a cultural area prior to the settlement of Eastern Micronesia; it is only after the settlement of Eastern Micronesia that one could argue a cultural area developed through social and trade interactions. While Palau in particular is peripheral, one of Peterson's tasks in this book was to demonstrate ongoing communications and exchanges (52). He discussed research documenting the presence of Yapese and Palauan pottery in sites of the Central Carolines, especially Lamotrek, dating from AD 1200 to 1400 by Alkire and Fujimura and Intoh, and links between the Chamorros of the Mariana Islands with the Western Carolines by Barratt. Riesenberg and Lessa also documented that very early drift voyagers from the Central Carolines to the Philippines, who then sailed home again via Palau and Marianas, were aware of Palau and drew maps of the region's islands for the priests.

Petersen's endnotes hold a wealth of supporting materials that he has not included in the texts. For instance, he was aware of the recent genetic studies of Micronesia and the Pacific (236), but judiciously did not discuss them in the text due to inconsistencies in these early works. However, the genetic studies do treat Micronesia as a region and focus both on matrilineages (via mitochondrial DNA, or mtDNA) and a later biparental perspective (Lum et al. 2002; Cann and Lum 2004). The literature including Micronesia is growing, including mtDNA research on the Marianas (Vilar et al. 2013).

Petersen provided a full review of the research that demonstrates that the islands of the northwest Pacific were not isolates (like many Polynesian islands after their settlement) but were societies linked together by voyaging networks such as the *sawei* that have long provided ways to share and trade economic resources, access marital partners across islands, and provide security from environmental challenges through interisland support systems. In a recent publication, Fitzpatrick (2008) reviewed "Maritime interregional interaction in Micronesia," drawing upon established and recent research on the region's exchange system that he presented using Stein's (2002) "paradigm of interregional actions" (137). Fitzpatrick (2008) provided a "Topologic structure of the Yapese Empire," following Hage and Harary (Fig. 7, 141) and modeled the acquisition and distribution of goods and services that expanded the normal schema of the sawei exchange (Fig. 6, 140) by incorporating other nodes of interactions in the Marianas, Philippines, Palau, and Indonesia, to which I would add New Guinea and the Marshalls. Although these models are undated, and Hage and Harary's book received mixed reviews, Fitzpatrick's figures and review of supporting literature are worth considering.

Petersen recognized that connections between Eastern Micronesia could have been achieved either indirectly through the Yap–Outer Islands sawei exchanges (33–35) or perhaps through direct trips from the Central Carolines to Palau (31). (Palau no longer practiced long-distance voyaging by the time Eastern Micronesia was settled.) Links to the Mariana Islands had been broken after their harsh colonization and depopulation under the early Spanish wars of the 1600s and remained more tenuous because of those early colonial histories.

I believe one of Petersen's important contributions to Micronesia's regional studies is his identification and research upon what he has called "the Breadfruit Revolution" (53–64) that has spurred at least this writer to further research. Petersen had noted that prehistorians identified the period AD 1000–1500 as a time of sociocultural transformation. He did not connect this transformative period with early climate change events in the Pacific, perhaps due to an ongoing controversy among archaeologists about the degree of variation in both timing and characteristics of such events across the Pacific. Recent and continuing archaeological, botanical, paleoecological, and oral historical research on Palau has considerably clarified our understanding of Palauan society prior to and during this period. Based on a series of early studies, Clark et al. (2006) summarized current research that "while Babeldaob may have been colonized by 4300 cal BP on palaeoenvironmental evidence . . . at present [h]uman arrival in southern Palau is dated at no earlier than 3100–2900 cal BP" (215). This is a significant

length of inhabitation and adaptation to the islands and their resources. Masse et al. (2006) published an evaluation of early climate change in Palau that has now been modified by Clark and Reepmeyer (2012) for the occupation and abandonment of Rock Island villages. The Clark and Reepmeyer report should settle earlier debates on regional variation, since the Palauan data do not fit within Nunn's (2007) expected sequence of events during a pan-Pacific catastrophe. Rather than Nunn's expected shift from large island villages to offshore islands around AD 1300, Clark and Reepmeyer's (2012) recent research and careful recalibration of radiocarbon dates demonstrate that "permanent settlements in the Rock Islands were established as early as AD 800–1100" (33), and the majority of the village sites were abandoned between AD 1350 and 1500 (34), much earlier than expected. The Rock Islands' Stonework Villages, with a population estimated between 4,000 and 6,000 people, were inhabited during the same time period as the Palauan ceramics found in Central Carolinian sites, demonstrating the existence of direct or indirect exchange relationships.

This is important for our understanding of the Breadfruit Revolution. It is unlikely that researchers will ever identify the site(s) where the important *Artocarpus mariannensis* × *Artocarpus altilis* was hybridized. The Palauan salt-tolerant seeded breadfruit (*A. mariannensis* Trécul), known as *chebiei*, is native to Palau and is found predominantly in the Rock Islands and southern islands of Peleliu, Angaur, and the Southwest Islands; an alternate name is *meduuliou* (southern breadfruit). Palauans are excellent agriculturalists and arboriculturalists, and the salt-intolerant *A. altilis* with their large breadfruits would probably have arrived with early waves of settlers. The hybrid between the two would have provided an improved food resource important to those living on small atolls and limestone islands and coastal fringes. Based upon his observations in 1946, Fosberg (1960) reported both that identifications of wild and cultivated *A. mariannensis* and *A. altilis* were often confounded, and due to introgression at that time there were perhaps four different forms, part of "hybrid swarms" that could be related to both. Given the large populations living in the Rock Islands in during the AD 800–1500 period who relied upon the chebiei, among other tree, root, and marine food resources, it is possible that there may have been both natural and carefully monitored genetic cultigens developed either in Palau and/or on Eastern Micronesian sites. The possibility of a Western Micronesian hybridization should not be excluded without further research.

I believe that Petersen's identification and analyses of "the Breadfruit Revolution" sheds light on an extremely transformative period in the region that strengthened the foundations of traditional Micronesia societies.

Petersen provided very complete analyses of Eastern Micronesian perspectives on the period (53–64) as well as seeking to understand views from the periphery. He also reconsidered how this new understanding of breadfruit might help solve a long puzzle in Micronesian ethnology recorded by Goodenough concerning a "cult of *Achaw* or *'Kachaw'*," involving Chuuk and Pohnpei (that are recurrently linked linguistically), and the islands to the east perhaps as far as Kiribati. I agree with his conclusion that in the early periods "interactions between west and east were probably as important as were the webs of linkages among the Nuclear-speaking peoples." (65)

It is always more difficult to understand the participation of societies on the periphery. Petersen hypothesizes that (a) only Eastern Micronesians were responsible for developing the *A. mariannensis* × *A. altilis* hybrid, (b) the Eastern Micronesians were responsible for disseminating the hybrid breadfruit cultigens throughout the region, and (c) in the process the Eastern Micronesians with their matrilineages and dispersed clans strongly influenced the existing kinship practices and social organization of Palau. The second hypothesis, the Eastern Micronesian dissemination of the hybrid breadfruit cultigens, is strongly supported by the existence of their extensive trading networks across the region. The first and third hypotheses are possible and pose an excellent challenge to researchers to prove or disprove, if that is possible so long after the events.

The popular Palauan Breadfruit Story is associated with the overturn of the offshore island of Ngibtal and clearly retains cultural memory on the importance of breadfruit at that period of transformation in a series of transitions in the title used for the Goddess. Dirrabkau was her final incarnation just preceding the transition to Milad, mother of the four stones representing the currently high ranked villages. Palauans maintain a number of strong migration histories of the people who escaped when Ngeruangel was overturned by storms, traveled on to Kayangel and then to many places throughout Babeldaob and Koror; some of the migrants founded the clans of the two paramount chieftaincies. However, Palauans normally discuss the people from Ngeruangel as being the descendants of Portuguese sailors. And, understanding Palauan social organization and Palauan "clans"—the *kebliil* whose membership is based on factors other than just "blood" (Smith 1983, 59) and *klebliil* sometimes glossed as super-sib—is a task for experts. Smith (1983: 37–71) provides a careful analysis of the nuances of Palauan group membership in relationship to changing land rights. Tracing the establishment of the "clans" over the long period of Palauan habitation and identifying a particular period when the matrilineages first began recognizing clans are interesting challenges. Did that happen at one time, or over a long period?

While interesting, these questions may not address the issue of whether a Micronesian cultural area or subregion is a reasonable model of the histories and lifestyles of the peoples of the region prior to the Europeans' arrival. I do not believe that either being able to link Palauan matriclans to Eastern Micronesia or identifying the site(s) where the salt-tolerant *A. mariannensis* × *A. altilis* hybrid was first developed is necessary to the main task of confirming the presence of a Micronesian cultural area. Eastern Micronesia is clearly the core of the matrilineal cultural area, including some strongly connected overlapping dialect groups, interisland marriages that supported the dispersion of the associated clans, and long-established extensive trade networks. I agree that Palau's documented early social relationships and either direct or indirect involvement in trade networks linking the islands of Yap and Eastern Micronesia suffice to confirm membership of this peripheral island group.

In conclusion, I briefly return to the controversy over whether Micronesia was ever anything but a colonial construct. I believe that the recent research on Palau has if anything strengthened Petersen's argument that around 2,000 years ago, a Micronesian culture area began to develop across the region despite a long hiatus between the Western and Eastern settlements. I do concur that at the core Micronesia is matrilineal and that this is one of and perhaps the most important characteristic that separates Micronesia from the other subregions of the Pacific. However, I believe the stronger foundation of Peterson's argument that Micronesia is a cultural area is that these societies have long been bound together through social relationships that link islands and people across this very large area of the northwest Pacific.

Relationships in Micronesia are often couched in kinship terms. Petersen focuses on matrilineages and dispersed clans, but all Micronesians and many visitors know that their "*multiple, crosscutting, and sometimes apparently contradictory principles allow for a great deal of flexibility*" (211) both within societies and across the region. Petersen discusses the considerable variation in regional social organization during the traditional period—variation that continued, of course, to adapt and change during colonial periods and new, postindependence governments. Perhaps the culture is not best described by a close focus on matrilineages despite the region's strong matrilineal social organization. Following Burton et al. (1996) in their Regions paper, the broader description "matri-centric societies" might be more useful and less contentious when making comparisons across the entire Micronesian region. One could reserve the focus on matrilineages to a single society and its direct linkages through dispersed clans where applicable. But this is an approach that would work best with living societies,

where they can be more directly traced. And I expect that not all of the important interisland linkages now, or at any time period, would follow kin lines, no matter how broadly defined, so once again we are refocused upon the social relationships of long-term linkages between islands. The unconditional support one might receive in case of need might be through matrilineages or dispersed clans, but these are not the only types of relationships that link societies in closest communication. One must look beyond clan relationships, and Palau and Yap provide an excellent example. Their deep interisland relationships, including the quarrying of Yapese stone money pieces on Palau, were mediated primarily through their respective high chiefs, who retain strong relationships to this day.

Once again, I thank Petersen for providing us with a very challenging and thoughtful overview of the traditional period of Micronesian societies. I leave it to the readers to discover and enjoy the richness and depth of the book and come to their own conclusions of the degree to which Petersen's analyses of the traditional period are persuasive and might be relevant to contemporary issues in the region. I expect and hope that this exciting book will continue to spur heated discussions and further research.

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BOOK REVIEW FORUM

Glenn Petersen. *Traditional Micronesian Societies: Adaptation, Integration, and Political Organization.* Honolulu: University of Hawai'i Press, 2009. Pp. 288. ISBN: 978-0-8248-3248-3. US$42.00 cloth.

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TRADITIONAL MICRONESIAN SOCIETIES is a welcome addition to the literature on a vast area of the northern Pacific that is less well covered than the southern islands. Petersen presents Micronesia as a social entity characterized by matriclans, with hierarchies of chiefs as leaders. He argues that a "breadfruit revolution" was part of the settlement of this area.

The term Micronesia is widely used to refer to islands in the northern Pacific, to complement the terms Polynesia and Melanesia as a tripartite division of Oceania. Petersen presents an argument for Micronesia as a single entity, "a category because in the dynamism of the historic survival of its peoples, they drew upon a set of shared strategies eminently adapted to the environments in which they lived" (16). It is not a cultural area, he argues, nor a construct that anthropologists, historians, geographers, and others have imposed (but for summary of arguments, see Hanlon 2009). Rather, Petersen sees Micronesia as a strong and single social entity of societies in the past. The shared strategies that unified the region included networks of dispersed matrilineal clans, "that perform in a great variety of adaptive ways" (226) around a clearly defined cultural, historical, and linguistic base. Petersen builds a generalized picture of social organization, which he views as applicable to all societies in this area of the northern Pacific.

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This perspective has resonance with the ongoing discussions about the New Melanesian ethnography (Godelier and Strathern 1991). Critics of writings about Melanesia as an undifferentiated entity stress the diverse practices and indigenous theories of being what Scott terms "poly-ontologies" (2007, 31). Thus, Petersen's suggested assessment of early Micronesian societies as a cohesive social entity based on matriclans may be considered a "new Micronesian ethnography" wherein kinship was the predominant link between societies, but such an approach overlooks the view that anthropology built its academic exclusivity around kinship, whereas Micronesians may not view themselves as so closely related.

Key social characteristics of Micronesian societies are in need of the close scrutiny that Petersen suggests (see Alkire 1972 for earlier summation). Although acknowledging that there is diversity in social relationships between the societies within Micronesia, particularly between eastern and western island communities, he focuses on the similarities between societies in the past, while noting many exceptions. Anthropological and historical texts are fewer in number and less well known than Melanesian texts; thus, Pacific scholars and students alike are less familiar with the communities in this vast area of ocean. Very few Micronesians have written ethnographies of their own societies. The author draws on the work of linguists and prehistorians to examine the pre-European past of these small islands, "traditional societies" as the title of the book suggests, but makes few references to ethno-historical views, myths, and legends that Micronesians themselves refer to when thinking about their past. Goodenough's Kachaw cult (1986) is the one exception. Use of the term traditional societies presents a further problem.

Many readers and students have trouble locating the boundaries of Micronesia. Petersen addresses this dilemma by arguing for a set of related languages, in two waves, but he has trouble including societies on the western and eastern margins of the area. The Marianas, and Guam as well as the Gilberts/Kiribati, and Nauru are only briefly mentioned in the text, with summary information appended in the final chapter (he does not mention Banabans of Ocean Island); this suggests that the author is uncertain about the boundaries of the entity labeled Micronesia.

I have long voiced my concerns about which societies belong within the label Micronesia. When teaching Pacific Studies in the "South Pacific," namely in New Zealand, to Samoans, Tongans, and Pakeha (European-descended) students, I have found they have little awareness of the Pacific north of the Equator, nor how those communities have impacted on their own societies in the past. Micronesia is a mysterious term. Bringing illustrations from my own fieldwork in the central Pacific, from Marshall Islands,

Nauru, Kiribati to Wallis and Futuna, Fiji, Cook Islands and Society Islands, I may have confused them more than clarified what and where Micronesia societies lie, their similarities and differences, and their shared histories and social ties. There is no one text to which to refer these students for clarity.

A further dilemma from a south Pacific perspective is that many authors, media commentators, musicians, etc., treat Micronesia as coterminous with the US Trust Territory (USTT). I have to point out to my students that these US Trust Territories are only part of the cultural area known as Micronesia. The six former United Nations Trust Territory states—Palau, Marianas, Truk, Ponape, Kusaie, and Marshall Islands (as they were known)—were entrusted to the United States by the United Nations in 1946. Nauru, Kiribati, and Ocean Island Banabans, are societies generally included under the heading Micronesia, culturally, historically, and linguistically but did not belong within the Trust Territory/ "American" framework. They and the Marianas and Guam are generally considered Micronesian but fall outside the generalized social features discussed in this book. Petersen reveals his own bias when he uses the term "Micronesia" several times as a short-hand for Federated States of Micronesia from his long-term associations with Pohnpei. The boundaries of Micronesia are debatable.

Whether residents of the area consider any relevance of the term Micronesia to their lives is not addressed. At the time of the creation of the Congress of Micronesia in the 1960s, many Trukese, and also Marshallese students at school in Hawai'i, debated the creation of this overarching political body, while residents on Namu, a Marshallese atoll where I was living, saw no relevance to an entity labeled Micronesia when a visiting aspiring politician sought their votes for him as a Senator in the Congress of Micronesia. Subsequently, I have found that residents of various Marshallese atolls prefer to refer to themselves first by their atoll affiliation, such as ri-Namu, or ri-Majol. The distinction between atolls in the Ralik (sunset) chain and the Ratak (sunrise) chain are apparent in local thinking as well as in local dialects and local histories. The twenty-six atoll communities only partially recognize their label as belonging to the Marshall Islands. And Micronesia is barely an identity concept.

In a monograph focusing on "traditional Micronesian societies" the reader might expect close attention to the detailed records from the earliest writings about Micronesian societies in the mid- to late1800s when Europeans began writing about them. The German *Sudsee* expedition accounts of individual Micronesian societies in the period 1910–14, such as Kramer and Nevermann's Ralik Ratak (1938) or Hambruch (1914–15) for

Nauru, provide us with a vast amount of social detail from which to derive comparative reconstructions of political prehistory of the area. I refer to these as "transitional accounts" because they indicate the bridge between precolonial and neocolonial impacts. Instead the author draws mainly from post 1950s anthropological writings. The term "traditional" in the title of this book is problematic.

Petersen suggests two waves of settlement across Micronesia. Drawing on historical linguists' and prehistorians' analyses, he proposes that the earliest wave is unclear because of the paucity of archaeological work. But those travelers probably came from the west, whereas he proposes that the second wave entered Micronesia from the south through the Santa Cruz region and the Lapita region. This latter wave consisted of a number of "loosely connected" and "highly mobilized peoples who voyaged within extended interaction spheres" and for whom patterns of interaction were by no means identical (43, 44). Thus, he links Pohnpei with Chuuk to the western side and Kosrae on the eastern side, as settled at the end of the first millennium BC. He proposes that these eastern Micronesian populations spread westward to bring breadfruit, matri-clans, and chiefly systems to western Micronesian societies, thereby changing former patterns of social organization. This second wave provides the base for his argument for the cultural coherence of Micronesian societies as a result of the breadfruit revolution.

Breadfruit Revolution

The author's construct of a breadfruit revolution is based on his view of the development of new hybrid forms of breadfruit (*Artocarpus altilis*) derived from *Artocarpus camansi* and *Artocarpus mariannensis* "that thrive on Micronesian atolls" (56). That hybrid form may have provided a major source of carbohydrates on Pohnpei, alongside *Dioscorea* yams, etc., because it was widespread across Micronesia and as far as eastern Polynesia, particularly the high islands and atolls of Marquesas and Tuamotus and Tahiti.

Botanical evidence has established that both seeded and seedless breadfruit were first domesticated in the western Pacific, perhaps New Guinea, and spread by humans beginning 3000 years ago (Ragone 2006, 2). The ongoing work of the Breadfruit Institute on Kauai (Hawai'i) on DNA and early profiles has revealed that both seeded and seedless hybrid varieties were best adapted to atolls and most common in Micronesia, as well as in the eastern islands of Polynesia (Ragone 2006, 4). Hundreds of named varieties have been recorded. In 1968, I recorded seven varieties of seedless and three varieties of seeded breadfruit growing and producing many

fruit prolifically in season on Namu, a drier northern atoll of the Marshall Islands. Of these the Bitaakdaak variety was considered "king of the breadfruit." We ate breadfruit daily during the season, March to July, with a few fruit ripening in October (Pollock 1992, 46). During the rest of the year, we had no alternative food other than a daily pot of rice with coconut cream (Pollock 1970a).

Breadfruit is a common food resource that is well documented from archaeological, botanical, historical, and social perspectives (e.g. Bellwood 2005; Ragone 2006; Pollock 1970a). Captain Cook experienced a marked change in food gifts in Tahiti in 1769, when the abundance during the breadfruit season ended in May; thereafter, he found it difficult to obtain local food supplies (Pollock 2012). As travelers crossing the Pacific by canoe carried their favored varieties to their new islands, the range of varieties has become extensive (Pollock 2013). Favored attributes include length of fruiting season, seeded alongside seedless varieties, and suitability to island environments, whether high islands or atolls. This process of dispersal has been ongoing over 2000 years—there is no evidence of a sudden breadfruit revolution in Micronesia.

Breadfruit trees, both seeded and seedless, can only be reproduced by human agency, that is, vegetative reproduction (Ragone 2006). Shoots from the roots must be planted by hand and nurtured for ten years before the tree produces fruit. The occurrence of a hybrid form is unclear as to "where or when this process took place" (Petersen, 56), but many varieties have been carried across the breadth of the Pacific to provide fruit, leaves to wrap foods cooked in the earth oven, a shade tree, and wood for canoes. Vegetative propagation enabled very close selection of varieties, leading to the diversity within and across Pacific societies that Ragone has reported in recent times (2006).

Breadfruit was only one of some ten starch foods on which Pacific communities have relied for their main food supply (Pollock 1992). It contributed to local needs alongside yams, taro, and other starch foods on which Micronesian atoll populations have continued to rely. The practice of fermenting breadfruit in pits not only provided a supply of food beyond the season but also provided an acidic, more flavorful taste than did the ripe fruit (Pollock 1984). As populations increased, these pits were used more frequently; thus, fermented fruit lasted for shorter times. Whether an increased supply of breadfruit on Pohnpei contributed to the building of Nan Madol on Pohnpei (AD 1300–1600) is not yet evidenced in the documentation.

The ramifications of breadfruit hybridization to support an argument for linking communities across Micronesia need further consideration in light of the plant's botanical evidence, reproduction systems, and usage as food.

Matriclans and Matrilineages

Matriclans and matrilineages linked to maternal founders have been reported for many societies across Oceania, but they are not the only, nor necessarily a cohesive, form of social organization across the region of Micronesia that Petersen suggests. I will illustrate with examples from the Marshall Islands and from Nauru.

Petersen's promotion of matriclans and matrilineages as the dominant feature of traditional Micronesian society that spreads westward after the second wave of settlement may more closely represent the anthropological theory of the mid-twentieth century for Euro-American anthropologists than represent a key social formation of Micronesian communities. Petersen's reading of post-1950s ethnography suggests that kinship relations through maternal links were the key feature of traditional Micronesian societies. Existence of a common distant ancestor may appear in myths and legends of individual Micronesian societies, but those do not feature in this discussion of traditional social features.

Even within the Marshall Islands, which comprise twenty-six atolls in two chains, there are marked differences in the accounts of such traditions. For the Ralik chain, Liwatoinmour was a recognized founding ancestor represented by a rock on Namu, whereas for the Ratak chain her sister was the founding ancestor on Aur (Pollock 1975). And more such accounts of ancestry are still to emerge (Tobin 2002). There are no accounts that link Marshallese matriclans or matrilineages to neighboring islands, either Kosrae, Pohnpei, Kiribati, or Nauru.

Marshallese clans are less localized than matrilineages. Several clan (*jowi*) names occur across several atoll societies in both chains of the Marshalls but mainly for older Marshallese. On Namu atoll, I recorded seven clan names across the population of 600 in 1967. Names such as Mekauliej, Jemeliwut, etc., also occurred on Wotje and Utrik (NJP fieldnotes 2003, 2004). However, today they are just names, sometimes jokingly associated with characteristics drawn from myth, that is, Jemeliwut as Trickster, a well-known mythical character that Luomala (1949) recorded across the central Pacific. That clan name may be shared with Nauru, whereas Eamwit and Eamwidumwit are still prominent clans/tribes today; but the link has not been recognized.

The role of clans has undoubtedly changed over time as populations have grown (and reduced). Memories of personal clan affiliations are fading, because younger generations find little use for them (NJP fieldnotes, Wotje 2003). A woman should marry/cohabit with a man from another clan, but that social restriction is fading and remains in the memory of those few

members of atoll communities dedicated to "*manit in Majol*," Marshallese custom.

Marshallese matrilineages have stronger social relevance than do clans today as also in the past. German ethnographers writing about the Marshall Islands in the 1890s recorded details of matrilineages (e.g., Kramer and Nevermann 1938). They recorded the links between lineages and landholdings, as well as rules of exogamy and of residence.

The concept of matrilineality, however, has been used widely to encompass several key structural features of societies, such as links to the ancestors, exogamy, inheritance rights (mainly land), residence rules, and labor commitments—all showing variable ties to a mother. Furthermore, matrilineality is as much about relationships between siblings as it is about links to a mother (Pollock 2003). When we consider the obligations and practices of matrilineality across the atolls of the Marshall Islands, significant variations in social relationships emerge, along with probable variations over time. Earlier forms of matrilineages when island populations were very small may have represented smaller groups within matriclans. The two forms of organization have become more distinct as populations have grown.

Marshall Islands siblings share matrilineal ties (*bwij*) with their common mother. Lineages have no distinctive names, other than that of the main house/residential land in which they share rights of access, and the many work sites to which they have rights (Pollock, Morton and Lalouel 1972). Members of a matrilineage, female and male, share rights to named pieces of land (*wato*), often running across narrow atolls, or subdivided in the case of Wotje, or Laura, both wide islets. These are inherited through both matrilineal and patrilineal ties, as well as received as gifts from a person often glossed as "chief," called an *Iroij* (Pollock 1974; for Laura, Majuro atoll, see DeBrum and Rutz 1967; Tobin 2002). Siblings have access rights to reside in several households, usually within one atoll, allowing them to move frequently. Sisters maintain close links with their mother's household, with at least one brother resident from time to time; brothers must care for, provide breadfruit, coconuts, and fish, etc., for their mother, as well as for their sisters and wives, wherever they reside. The senior sister or brother, appointed by the lineage as *alab*, or land manager, represents the lineages' interests to the Iroij of those lands.

The lands that a matrilineage can access are under an Iroij who has responsibilities for residents on "his"/"her" lands. Each matrilineage's land access rights are managed by an alab who represents all those *rijerbal* (workers) of her/his matrilineage to the Iroij. The rijerbal live on their specific pieces of land, as entailed through the alab, make presentations of

food, particularly first breadfruit, to the Iroij, if he is present. In return the Iroij provides them (today) with material items, such as rice, fish-hooks, roofing material, or a walkie-talkie (in the case of Iroij Lejolan Kabua). He also provides them with spiritual benefits that include healing powers (Carucci 1997). The rijerbal and alab are bound to the Iroij by land, not kinship ties, although those may have been significant in the past.

The link between matrilineages and their lands is a vital part of Marshallese social organization. The lands not only provide a residence, as a space of belonging, for living alongside sisters and their mother and father, but those lands also provide the means of identity. Those who hold lands on Namu, for example, refer to themselves as ri-Namu (people of Namu). Because mobility has increased even more markedly than in the past, and extends to urban households, that identification with lands on a particular atoll has become the basis of political affiliation and social belonging even for those not always resident on their "home" atoll.

Marriage is exogamous to a matrilineage. A young woman must select her partner from another matrilineage, even for her first cohabitations (*koba bajjik*). On Namu, this rule presented dilemmas for young women with only six other lineages from which to find a suitable young man of the right age. In addition in the past, she should marry/reproduce with a man from the same atoll (atoll endogamy); my records for six generation depth on Namu revealed the prevalence of this practice in eighty-three percent of past unions (Pollock, Morton, Lalouel 1972). It was reinforced by cross-cousin marriage. Today the choice is widened through meetings in high school and other urban contexts. That past restriction on reproduction, I argued, served to keep access rights to land closely controlled by matrilineages. Also, it enabled a wider choice of sites for the couple to use as residences and work sites (i.e., for making copra). Nuclear family units moved frequently when pressures required. Matrilocal residence was not an operational concept. Sisters stayed close to their mother, whereas at least one brother should also take his share of responsibilities of providing for her.

Links to ancestors, and other beliefs, were known to a few knowledgeable residents but did not particularly pertain to any lineage. Lineages were heavily interwoven through the practice of cross-cousin marriage and alternative schemes of managing social relationships.

Matrilineages in the Marshall Islands were and still are only one aspect of social relationships. The major commitment is and has long been to a particular atoll; thus, identity, understood largely in terms of "place," in Marshallese society is recorded by the atoll where the person has their

main rights of access to land. A person is referred to as ri-Namu even if she is living on Ebeye. A Rongelap woman married to a Namu man, residing on Namu, still maintained her identity as ri-Rongelap (*ri-* refers to "rib of"). House sites are part of a social division of the island into halves known as *jittoen/jittoken*; the two halves "compete" in songs and compositions, particularly at Christmas time (Pollock 1970b). Fishing groups are important, because men share an outboard or *boomboom* today, as a continuation of groups of men who built large *tibnil* sailing canoes in times past. Work groups draw on residents rather than specific lineage groups. Church groups and women's groups have become strong affiliations that go beyond kinship groupings.

Thus, belonging to place is as important, if not more so, than belonging to a lineage. The place, whether the whole atoll or an islet within it or a residence site, provides the basis for social maintenance and well-being. That identity as, for example, ri-Namu or ri-Wotje, has gained significance as people move freely to the urban centers of Majuro or Ebeye, where island groups have specific locations among other urban populations.

Nauru

For the single island of Nauru south and west of the Marshall Islands, a raised reef with a population of less than 1000 at the time of German writings in the 1880s (e.g., Hambruch 1914–15), clans were known as tribes, as they still are today. Legends tell of close ties to atolls of Kiribati and nearby Banabans on Ocean Island. The 12 "Tribes of Naoero" are a significant part of Nauruan local culture history, even printed on souvenirs today (NJP, Frigate Bird Practices and Beliefs, in Eastern Micronesia, unpubl. data).

Every Nauruan's birth is recorded in the Nauru Bulletin according to the tribe of the mother. Between the twelve tribes numbers are uneven, with Eamwit and Eamwidumwit as the most numerous, according to the 1999 Census (Nauruan Eamwit may have linguistic links to Marshallese Jemeliwut). New female arrivals on Nauru were given distinct tribal names, for example, Ranibok (trash from the sea in Hambruch's translation) or Iruwa (stranger), which their children inherited. New male arrivals had no tribal affiliation (NJP, Social Impact of Mining on Nauru, unpubl. data)

Belonging to a tribe provided one form of relatedness, with households, districts, ritual groups, fishing groups, and warrior groups forming alternative organizations in context. Nauru had no matrilineages, probably because the population was too small.

Leadership

Leadership in Pacific societies has challenged many anthropologists and historians to question Sahlins' (1963) suggested dichotomy of Big Man in Melanesia, as distinct from Chiefs in Polynesia. Such generic categories have been extended by Godelier's addition of Great Man (1986). None of the writers has attempted to include Micronesian societies. Petersen proposes that Micronesian leadership involves "some degree of duality or multiplicity of chieftainship" (156), based on kin ties through matriclans as local control growing out of "continual interplay between hierarchy and equality" (158). Kin relations, he argues, are foremost; local residency and feasting provide a web of ties between and among communities across Micronesia (155). Petersen draws on his own extensive work on politics in Pohnpei to suggest these pan-Micronesian features of leadership.

Such generic features are not as applicable to eastern Micronesian societies, such as the Marshall Islands, Nauru, and Kiribati, as they are to Pohnpei, Kosrae, and Chuuk. Examples of the former linkages include Goodenough's Kachaw cult and perhaps the Frigate Bird cult (Pollock 2010). Kiribati falls outside the model on many counts, notably the absence of matriclans. Nauru's matrilineal tribes provide a base of leadership through chiefs drawn from the descendants of the senior sister in a sibling set. In the Marshall Islands, chiefly leadership by an Iroij varied between communities in Ralik and Ratak chains, but in neither case did Iroij have kin ties with the general populace (rijerbal).

The concept of apical stratification, as applied to a limited form of hierarchy, is more relevant to access to land in the Marshalls, not kinship. A chief (Iroij) controlled named pieces of land (*wato*) on several atolls in one chain, rights gained by warfare, marriage, and other arrangements (Tobin 2002). (It must be noted that the term Iroij is used for many concepts beyond human relationships, such as king of breadfruit or "king of the Birds for the Ak," frigate bird.) S/He allotted workers' rights to several matrilineages for specific pieces of land in return for goods and services and also allotted traditional ties such as ancestry, spiritual links, sharing feasts, and residence. Iroij were seen to have special powers derived from their high ranking ancestry. Four chiefs held lands on Namu, but only one actively fulfilled his duties to the people in the 1960s; that Iroij, Lejolan Kabua, carried a business card that claimed (on one side) that he was "king of the Marshalls."

Succession to chiefly titles was more complex in Ratak chain than in Ralik chain (for Laura, Majuro atoll, see DeBrum and Rutz 1967). A key principle applied in both chains, namely that an *Iroij laplap* (paramount

chief) should be "two-shouldered" that is both her/his mother and father should be of chiefly status. Where one parent was of lesser status, the chief was known as *Iroij erik* (lesser chief). The complexities of successions on Majuro atoll had led to eight persons holding Iroij erik titles and only one Iroij laplap for all the lands on one islet (Laura) (DeBrum and Rutz 1967). Iroij erik were not known in Ralik chain (Iroij L. Kabua, pers. comm., 1966). Therefore, land associations rather than kin relations predominated in Marshallese chiefly systems.

Leadership in other social arenas was also important. Control of knowledge, especially navigation and ancestral ties, as well as providers for feasts, healers, and midwives were all recognized for their particular contributions to atoll community life. Individual warrior leaders achieved significance in times past, according to ethno-historical accounts, whereas those elders with knowledge of myths, past leaders and events, useful plants, wato boundaries, etc., all held significant place in local communities. They were not specific to any matrilineage but were called on for their knowledge when applicable. They and the Iroij were recognized for their role in society, not kinship (Tobin 2002).

When we consider leadership on another eastern Micronesia island, Nauru, the transitional literature refers to the chiefly system that German ethnographers encountered. It was more hierarchical than apical.

Leadership on Nauru was stratified into three tiers, with descendants of older sisters, the *temonibe* class, providing the group from which a chief was chosen, whereas descendants of younger sisters, the *amenengame* class, provided the support group. All Nauruans were either temonibe or amenengame. A third strata, the *itsio*, included all non-Nauruans (arrivals from other Pacific islands) and those who lost land after war. They had to find a chief who could provide them with land, often through marriage with a Nauruan wife. Thus, access to lands was a key feature of Nauruan stratification system. It was not tied to residency in a particular district because Nauruans moved frequently between several households in different named districts where they had access to land. For ease of administration, German authorities reduced the number of chiefs in 1890 to one per district, as if Nauruans belonged to a particular district. This violated Nauruan principles (NJP, Social Impact of Mining on Nauru, unpubl. data).

Other Nauruan leadership roles included war leaders and those with specialist knowledge of magic, dance, healing, and plants. Male initiates in the frigate bird cult prepared for taking leadership roles by gaining access to magic; the *amenename* was the leader of the cult, as holder of magic, with several designated assistants (Kayser 1935/2005). The women's supportive mat-weaving groups came under their own leaders. A chief

communicated with these leaders about when and how to begin the annual cultic event needing magic and about the practices of catching and nurturing the birds. The birds were considered an integral part of each clan group, because they represented the ancestors and, thus, linked the past with the present (Pollock 2010). Such cults were an important social feature as practiced by Banabans of Ocean Island, in the Marshall Islands and Kiribati, and on Chuuk, and perhaps in Palau (see also Intoh and Eda 2009).

Kiribati leadership varied, with Tabiteuea and southern atolls differing from the northern people. Key ancestors were honored. Mat patterns were the property of special Kiribati women's groups (but not widely documented). Dance composers and leaders were highly significant in social life, particularly in Tabiteuea (see Autio 2011).

Thus, in eastern Micronesia, leadership was not directly linked to matriclans. Kinship was not the generic feature, although it played some part in some principles and on some occasions. Leadership through chiefly systems was only one dimension of social dynamics. Status links, decision making, and spiritual bonds were recognized in various sectors of society appropriate to a particular event or necessity, whether access to land, or propitiating the outcome of an encounter or arranging gifts for feasts. Women's and men's groups each had their own leaders, sometimes inherited, sometimes appointed. Differentiation from neighboring societies occurred sometime before the recording of transitional ethnographies. Legends are the main source of such early information (e.g., for Tabiteuea, Kiribati, see Autio 2011).

Summary

Traditional Micronesian Societies introduces *Pacific Studies* readers and students to the least considered sector of Oceania, Micronesia; Melanesian and Polynesian societies have a much wider literature. Anthropological perspectives on Micronesian societies through ethnographies have been largely ahistorical; thus, we must rely on transition accounts mainly by European, particularly German, ethnographers for a perspective on the nineteenth century and perhaps before and local legends. Petersen's construction of Micronesia out of post-1950s material provides an impetus for further reconstructions of what may be considered as traditional ways of life.

Just which of the atoll and high island societies should be included in such a volume remains inconclusive. Petersen relies heavily on American Micronesia, but these six former Trust Territories are not coextensive with the culture area of Micronesia. Guam, Kiribati, Nauru, and Ocean Island

remain tangential. The tendency to slip from extending the concept of Micronesia, as in Federated States of Micronesia, to the much wider cultural/geographic entity covering one third of Oceania must be carefully handled.

Reconstructing settlement patterns for Micronesia from all too few accounts by prehistorians is still in early days. Compiling a picture of the area from published archaeology and historical linguistics, as Petersen cites in Chapter 3, is still rudimentary. We need an enlarged picture that incorporates local legends and other key features that may indicate social connections between the islands. Navigation and orientation to the sea are not considered here yet are an important aspect of inter-connections between islands (e.g., Gladwin 1970; D'Arcy 2008; Diaz 2010; Genz 2011). I have suggested a gastronomic perspective to show how food systems, including breadfruit, have been dispersed across this area by voyagers out of Asia. Canoes full of travelers and their foods shared their produce and their genes as they settled on new islands (Pollock 2011; 2013).

The proposed reconstruction of social relations, especially matriclans, is based on very thin evidence for traditional—that is, pre-1900 times. Sibling sets that maintain structural and practical ties to a mother have been noted widely across Oceania. Moreover, both female and male siblings feature in myths of settlement and ritual in early times. The Kachaw empire for societies surrounding Chuuk, the Frigate Bird cult that was prevalent across Eastern Micronesia as a male initiation ritual (Pollock 2009), and Sawei exchange groups all provide some indicators of how formalized social ties between islands were established and maintained, involving both women and men. These need to be incorporated with the early ethnographic material and legends into a text that addresses Micronesia in the nineteenth century and before.

Matrilineages as an anthropological construct have been paralleled by constructs such as hierarchies with chiefs at the apex. A paramount chief may have derived respect from significant connections to ancestors, but whether those ancestors came from other atolls and high islands is not apparent (see, e.g., Sahlins 1985 on Stranger-Kings). Respect was also accorded to healers (including midwives), ritual leaders, dance composers (particularly in Kiribati), and resource controllers. Godelier's suggestion from Melanesia of adding Powerful men to Sahlins' proposed Big Man/Chief dichotomy also needs to be addressed for Micronesia.

Petersen's book challenges scholars to address the small but extensive links across a vast area of the northern Pacific Ocean. Rethinking the constructs of Polynesia and Melanesia, whether as cultural divisions of Oceania or as geographically convenient terms, is already happening; Micronesian

material needs to be included. Mobility and spatial concepts need to be considered alongside kinship and exchange as integrating features. The concept of New Ethnography raised by Godelier, M. Strathern (1981) and others for Melanesia offers a pattern for points of discussion raised by Petersen, Hanlon, Hezel (1983), and others for Micronesia.

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BOOK REVIEW FORUM

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LET ME BEGIN by thanking these three colleagues for the time, thought, and effort they've put into their commentary, and the editors at *Pacific Studies* for giving me this opportunity to respond. Because a number of the criticisms raised here stem from what I think are misunderstandings of what I was attempting to do in *Traditional Micronesian Societies*, let me begin by explaining what I set out to accomplish.

The prehistory of eastern Oceania and especially of Polynesia has been well explored and continues to be a focus of much inquiry. The number of classic works is striking, and includes Sahlins' *Social Stratification in Polynesia*, Goldman's *Ancient Polynesian Society*, Kirch's *On the Road of the Winds*, Irwin's *Prehistory in the Pacific Islands*, and Kirch and Green's *Hawaiki, Ancestral Polynesia*. Although some of these touch upon Micronesia, the region's story is largely neglected. Only Alkire's *An Introduction to the Peoples and Cultures of Micronesia* (1977) and Rainbird's *The Archaeology of Micronesia* (2004) have treated this region at any length, but both volumes focus on discrete treatments of the various archipelagoes and do little to integrate materials from across them. Micronesia deserves historical and comparative treatment comparable to that of the rest of Oceania.

My sense of myself is as an ethnographer and I have long tried to avoid writing about things I don't know firsthand. I hadn't seen myself as a scholar of the sort likely to undertake an ethnological task like this. When I read R. Hunter-Anderson and Y. Zan's piece on the origins and development of

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systems of social rank in the Central Caroline Islands (1996), however, I was struck by the underlying assumption they shared with Alkire (1980)—whose conceptualization of the topic they were critiquing—that these systems of rank developed in situ and de novo. It seemed that the social systems these scholars were describing and analyzing were almost identical to systems of rank in the Eastern Carolines, and this prompted me to write "Sociopolitical Rank and Clanship in the Caroline Islands" (Peterson 1999) as a corrective.

I had hoped to demonstrate that the basic framework of dispersed matrilineal clans and the structures of rank that organize key aspects of them were shared by many Caroline Islands societies. When my article appeared, Leonard Mason, Ward Goodenough, and Douglas Oliver urged me to expand my treatment to wider aspects of social organization in these islands. It was this impetus that overcame my long-held reluctance to write about things I did not know from direct ethnographic experience.

At about the time that I started writing this book, I served on Nyree Zerega's doctoral committee and consulted at some length with her and Diane Ragone, among others, on the history and molecular biology of breadfruit in the Eastern Carolines. They taught me about the hybridization of the two forms of breadfruit that took place there and about some of the botanical and agronomic consequences of this process—the explosive increase in the number of cultivars and the development of a tolerance for saltwater, which led, in turn, to the diffusion of hybrid variants throughout the islands of Micronesia. As I was drafting my chapter on the original settlement and prehistory of Micronesia, and was rereading T. King and P. Parker (1984) on archaeological sequences in Chuuk Lagoon, which indicated a significant rise in population densities and the appearance of archaeological features and artifacts connected with breadfruit processing and cultural influences from the east, that is, Pohnpei and Kosrae, I had what I think of as an epiphany: my notion of a breadfruit revolution, and a way of conceptualizing the spread of social features—the dispersed conical clans—in which I was especially interested.

When I completed that chapter, I was quite skeptical about my own findings—the pieces simply seemed to fit together too well. I thought I might be overlooking something; thus, I presented my analysis at the 2004 Global Perspectives on the Archaeology of Islands conference in Auckland. Roger Green was in the audience, and I began by challenging his assertion that eastern and western Micronesia were essentially separate cultural spheres. When I finished describing my conclusions, Roger was the first to raise his hand. He said I had changed his mind. No one present contradicted the paper's main thrust, and an editor from *Archaeology in Oceania*

asked to publish the paper (Petersen 2006). Although I have had to tweak the analysis occasionally since then, and have read in reviews that I haven't definitively proved the matter yet (a point I agree with), the central themes of my thesis have thus far stood the test of time. I believe I have demonstrated that Micronesia is indeed a coherent region for intrinsic reasons, not merely a colonial construct as David Hanlon maintains (1989). Hanlon tells me I haven't changed his mind; therefore, I'll have to be satisfied with Green and, I hope, at least a few other doubters.

Many of the arguments that Lawrence Carucci and Nancy Pollock bring forward here are about a somewhat different issue: how well my general model of Micronesian social organization explains detailed, specific points in the Marshall Islands. I am comfortable acknowledging that a conceptual framework meant to explore connections among islands stretching across more than three thousand miles of the Central Pacific doesn't explain each local situation. I repeatedly stress this in the book, but I note that factors shaping the origins of a particular trait or practice are not necessarily going to explain much about later ways in which it is put to use. Therefore, I am comfortable with their criticisms of some of ways in which things are different in the Marshalls.

I need to address several recurring themes in their commentaries before I take up the more specific issues they raise. First, I feel compelled to stress that my book is a work of ethnology and is focused on a range of general themes. I am fully aware that exceptions to these generalities occur everywhere in the region; indeed, this is why I devoted an entire chapter to exceptions. Second, I readily acknowledge that there is a tilt toward the Eastern Carolines in my approach. As I explained at the outset of the book (6), it was only by knowing one subarea well that I felt competent to conceptualize the larger region. Finally, because there is such a paucity of archaeological data from the islands, I have necessarily focused on ethnological data.

Let me begin with Carucci's detailed critique. His generous praise of my work notwithstanding, Carucci says that by adopting a "classical comparative anthropological approach" focusing on kinship, land tenure, and political organization, I marginalize cosmology, religion, and ritual practice (83). I acknowledge that I have done so for several reasons. First, I trace my own intellectual history through Julian Steward, as Carucci suggests, and I continue to observe the world of social phenomena through lenses that bring aspects of political economy and problems of making a living most sharply into focus. Second, it is my sense that religion, cosmology, and other matters dealing with interior states are more prone to misunderstanding by observers, thus rendering the relevant materials somewhat less reliable.

Third, I feel that in my own ethnographic work I've done a relatively inferior job of appreciating and analyzing these latter sorts of categories. When I read, for instance, Catherine Lutz's work on the emotional lives of Ifaluk's people, I gasp in recognition of what she describes; I'm aware of striking similarities on Pohnpei, but I lack the insight and skills to systematically study and analyze them. Fourth, I believe that data available for comparative purposes are significantly more reliable in the veins I have chosen to explore. I suppose that some will level against me the classic charge of using the data available in much the same way that a drunk employs a light post—more for support than illumination. So be it.

According to Carucci my analysis is hampered by a functionalist paradigm that fails to "account for the multi-faceted array of symbolic domains and social practices that must be analyzed in dynamic, historically-sensitized fashion for each of the societies and social arenas in the region" (85). Again, I acknowledge short-comings in my exploration of symbols, which I believe call for far more direct knowledge of mental constructs than I have ever been able to achieve. However, as far as historically sensitive analysis of social practices in each of the island groups or societies goes, I stress that one of my primary concerns was to educe from the materials I did work with an overall framework that would accommodate historical processes of local adaptation, idiosyncratic development, and change. At the center of my study is an examination of a specific set of dynamic, historically sensitized processes, the diffusion of highly productive breadfruit hybrids, political and economic changes wrought by a significant increase in subsistence production, and a host of attendant social phenomena. It is one thing to construct a model that allows for significant variation across such a broad swath of the Pacific and two millennia, though, and another to account for all the variants.

Carucci suggests that the "ready reformulation of social forms developed in other socio-environmental settings contradicts the thesis that the social forms emerged to fulfill local adaptive functions" and that I, thus, overlook the "invention of tradition" literature (85). Actually, an article I published questioning important aspects of the invention of tradition as it applies to Pohnpei (Petersen 1992) played a crucial role in shaping my approach to using local traditions in the book. As a consequence of examining an entire corpus of mythohistorical accounts from Pohnpei (Petersen 1990), I had concluded that for virtually any given account there would be a range of counter-narratives. In my 1992 "Off-the-Shelf Tradition" piece, I went on to argue that what often appears as invented tradition is simply a variant version that had not been previously recorded. Both Carucci and Pollock express dismay that I failed to rely much on local traditional accounts, but

it was precisely because of my awareness that local accounts tend to be contradicted by other local accounts, and that outside Pohnpei I had little access to a full range of these accounts, that I opted not to rely very heavily upon them. It was, in fact, my own dissatisfaction with some of the ways archaeologists were mishandling these accounts in their explanations of the origins of Pohnpei's Nan Madol complex that led me to examine this problem, and I entirely appreciate the concerns Carucci and Pollock express about my failure to make use of Marshalls mythohistorical materials, but I feel fairly certain that if I had tried to employ them, I would have been on the receiving end of even more criticism for misusing them. Mine may or may not have been an appropriate course to follow, but it was a deliberate methodological decision on my part and not, as Carucci says, a "fail[ure] to recognize the critical ways in which tradition is an imagined feature of an ever-emergent, constantly changing, cultural epistemic imaginary" (85).

In a similar vein, Carucci says "Petersen reasserts the Euro-American institutional domains—social, political, economic—without amply questioning their legitimacy" (85). It may well be that our notions of what constitutes ample questioning substantially differ, but I certainly addressed the question head-on at the beginning of my chapter on chiefs and government, where I reflect on the ways in which I have segmented key aspects of Micronesian social and cultural life.

The grounds upon which I distinguish between these grow out of my own experience; they reflect the ways in which I have come to understand Micronesian societies, not any preexisting disciplinary or philosophical models. I want to make it clear, however, that my approach is informed by classical western political thought. As I explained in the preceding chapter, ideas long debated by some of the western tradition's most influential thinkers have helped me think about how best to explain Micronesian sociopolitical life to non-Micronesians, while remaining as faithful as I can to Micronesian conceptions. I have tried hard to avoid forcing Micronesian social life into western models; I use them to elucidate rather than to categorize (p. 125).

That Carucci disagrees with my choices is clear and understandable, but I made them carefully, for reasons I took care to spell out. I wrote this work for several different audiences, including Western scholars and young Micronesians, and I did so believing that at least some Micronesians would be interested in seeing how their societies' institutions provided solutions to the sorts of problems of government that European thinkers have debated for centuries. I had in mind quite specifically the sorts of claims political scientists are apt to make about traditional Pacific Islands governments' lack of responsible and participatory forms of leadership (e.g., Lawson 1996).

To this end, I specifically invoked Aristotle (not Socrates) and a notion that political life "refers to what people want their communities to do for them and how they set about achieving these goals. This includes the formal structures or constitutional grounds of governance, the ways in which people actually participate in political life, and some of the ways in which individuals, groups, and institutions interact" (125). I differentiated between formal structures on the one hand, which I framed as "government," to make it clear that traditional Micronesian societies were fully engaged in governing themselves (and continue to apply their political precepts to the expectations they hold for their national governments in the twenty-first century) and other political facets of social life, on the other, precisely to indicate that politics can and do merge with virtually every aspect of life, as do other spheres or categories—religion, aesthetics, etc. When Carucci writes that "For Petersen, however, the politicized dimension of seating arrangements in a cookhouse are part of the domestic sphere and, therefore, do not figure as political activity" (86), I am flummoxed. Having written at length about just these sorts of seating arrangements on Pohnpei (Petersen 1995), my perspectives on ways domestic activities discharge political duties certainly did shape my entire approach to this analysis.

Let me turn now to several more specific criticisms Carucci levels. He writes that "Petersen requires the *a priori* acceptance of an etic grid of common anthropological terms to undergird his use of the comparative method to demonstrate the unity of social practices across the region. Micronesian traditional societies *must have* a universal matrilineal clan organization to ground the social organizational unity of Micronesia" (4, his emphasis). There is some truth to this, I suppose, in the sense that once I began to see the commonalities extending across the Caroline Islands, I did strain a bit to find them elsewhere. But as I explained at the outset, this project derived its impetus from my original realization that many Micronesianists did not recognize that key aspects of social organization on one island or among a group of islands might not have originated there but instead diffused from elsewhere. I confess that it never occurred to me, though, that my understanding of descent-organized groups in the Marshalls as having a significant matrilineal component might simply be a projection of Carolines sociocultural organization onto societies where they are, in fact, absent. Although the dearth of archaeological materials makes it difficult to speak with much certainty about connections between the Marshalls and the islands to their west, linguistic, ethnological, and ethnohistoric data do make it clear that ample connections existed.

It is at this point that I must address the crucial divergence between what I have tried to do in my book and what Carucci seems to think

I should have done. At the heart of his objections, as I understand them, is a perception that I have imposed a preconceived model of social organization on Marshalls societies and thereby done them a great disservice. Rather than teasing out Marshallese understandings of their own societies, and thus explaining the idiosyncrasies of belief, social practice, and symbols on their many atolls, I have made a number of assumptions about how these societies are organized, many of them in his eyes quite erroneous. But what he seems to be looking for is ethnography, whereas I was, as I say, undertaking an ethnological project.

On Pohnpei there are social groups known as *sou* (and in nineteenth-century orthographies Pohnpei's *sou* was often written as *jou*); in the Marshalls *jou* or *jowi*; on Kosrae as *sou*; and in Chuuk and the Central Carolines as variants of *sowu*. That is, there are groups that are organized in similar ways, engaged in similar activities, and called by virtually identical terms. In my analysis I was not trying to assess Marshallese notions of how these groups developed or how they are constituted. I was attempting to demonstrate that these different versions of descent groups did not develop entirely independently but rather descended from some common ancestral form. Likewise, on Pohnpei, there is a related but different (that is, smaller and less inclusive) sort of group known as *keinek*; in Chuuk and the Central Carolines it is *eyinang*, *ainang*, *kainang*, *hailang*, or some closely related variant; and in Yap it is *genung/ganong*. I confess that I believe that the purpose of all the hard work of ethnography carried out on so many islands is to both provide us with exquisitely detailed accounts of how these kinds of groups are conceived on their respective islands and to allow us to compare the ways in which they have developed, adapted, and even apotheosized. Having done local ethnography for a very long time, I thought it worth trying my hand at ethnology, but I do not mistake one for the other. However these groups are organized in the modern Marshalls, and however they function, they have at least some of their origins in patterns shared with the rest of Micronesia.

The same, then, holds for the question of matrilineal organization. I believe I demonstrated ample evidence of matriliny in the Marianas, Kiribati, and Nauru, but I fully recognized the dynamics that led to significant variations in these places. This is why I devoted an entire chapter to exceptions to my general model—I truly aimed to avoid squeezing the data too tightly into any simple model, whether preconceived or painstakingly teased out of the data.

I went to great lengths to explain that matrilineal precedents and practices apply only to a limited range of social practices in Micronesian societies. Even in the areas where these forms and practices tend to be of greatest

importance, that is, in the realms of land tenure and succession to leadership roles, there are invariably crucial paternal and bilateral inflections. However, these matri-forms seem to appear everywhere in Micronesia, in one guise or another, and there is simply no evidence that they arose entirely independently or diffused in from outside Micronesia (except during the original settlement of the area). I appreciate that my account does not do full justice to the character of Marshalls kin groups, leadership dynamics, and land tenure practices. However, that was hardly my intent; I wanted to demonstrate historical linkages and to formulate an explanation for why these forms spread as widely and as successfully as they did. I am prepared to consider counter arguments challenging my own, but will not admit culpability for not having achieved something I did not set out to do, that is, plumb the depths of sociocultural life in every Micronesian society.

Carucci does agree that "there are filaments of a matri-biased imaginary that can be found throughout this region" but goes on to characterize my position on the extensive role played by dispersed matrilineal clans as "the invention of an anthropological tradition to provide categorical support for an imagined Micronesia" (88). David Schneider gave me a good deal of help when I was first in the field and unprepared by my materialist training to grasp the descent dynamics I encountered. I later found his *Critique of the Study of Kinship* (1984) useful, and have taught it in graduate seminars, but it is not intended for the purposes of comparative study. In the end, I suppose, Carucci and I disagree on the relative importance of what he calls a matri-biased imaginary. As I have made clear, I see the nature of these matri-groups diverging among individual communities and my primary interest has instead been in trying to understand why some version of them appears virtually everywhere in the region.

In this same vein, he concurs with my observations about the intertwining of land and lineage but nonetheless describes me as projecting a "congeries of symbolic alignments onto the ossified imaginings of a long-standing past" (89). Because my aim was to promote appreciation for the dynamics of change, adaptation, and local innovation, and because I took care to describe the earliest forms as having been multiple and fluid, I am again puzzled.

Nowhere, perhaps, do the differences in our approaches become more marked than around our conflicting understandings of chieftainship. For me, as an engaged political actor in a number of realms, I think of leadership examples set by Pohnpeian chiefs as being among the greatest influences my ethnographic work has had on me as a person. As I have recently written, I now understand in retrospect that my grasp of Pohnpeian attitudes toward the resolution of Micronesia's political status issues in the

1970s were deeply influenced by discussions people were simultaneously having about traditional disputes over chiefly succession (Petersen 2014). In Awak, where I work most intensively, members of the same local chiefdom (*kousapw*), and indeed members of that chiefdom's ruling lineage (*keinek*), disagree rather sharply about the rules of succession—about who should properly become the next chief when the reigning chief dies. These disagreements reflect both specific political calculations and the fact that chieftainship bears multiple meanings, facets, and responsibilities.

I have always interpreted these disagreements as establishing the vitality of the institution of chieftainship, rather than its impotence. By analogy, I note, Americans disagree rather demonstratively about the actions of their presidents, and about the legitimacy of these actions, but nearly all of them agree both that there should be presidents and about who is currently the president. There are in Micronesia all sorts of disparities and differences regarding the intricacies of chieftainship. When Carucci quotes me as writing that "Micronesian societies . . . share a common sense of chieftainship" (91, his ellipsis), he questions the grounds of my claim that I "have witnessed people across the region discussing this common sense of chieftainship" (92). I was perhaps lax in spelling out exactly what I was referring to in the note he cites. In that note, I referred to a dialogue I'd had with David Schneider about Micronesian chieftainship, but I may not have made my point clearly: "Subsequent to that conversation, and Schneider's death, I have had ample opportunity to work together with Micronesians on chieftainship as a constitutional issue" (Petersen 1997). I assumed that by citing my paper on debates over constitutional roles for chiefs I was making clear the source of these observations but apparently not.

Let me explain more fully. At both the 1975 Micronesian Constitutional Convention in Saipan, which included delegates from all the old Trust Territory districts, including the Marshalls, and the 1990 FSM Constitutional Convention, a great deal of time and attention were given to the question of creating a "chamber of chiefs" in the national government, whether this government included Micronesia broadly construed (as in 1975) or only the Eastern and Central Carolines (as in 1990). These discussions included nuanced examinations of chieftainship and its meanings (along with references to its absence in modern Kosrae), and as with, for example, disputes about chiefly succession, there were disagreements within local delegations about the nature of chieftainship in their respective societies. Among all the debates about who is a chief and what powers chiefs rightfully exercise, however, no one in my hearing (and I was present at virtually every formal discussion in 1990, if not in 1975) ever questioned the existence of chiefs

or their relevance to the problem of creating and regulating Micronesian governments. It is in the nature of all government and political life, I think, that there is debate. Micronesians' differences over the nature of chieftainship in no way imply that they do not share some sense that their societies all have (or until recently had) chiefs. And the fact that, as Carucci says, "current-day Enewetak/Ujelang people (along with many other Marshalls Islanders) contest the very idea today there are *irooj* in practice at all" (93) does not change the existence of an underlying domain of leadership, however it continues to play out historically.

Carucci also finds problematic what he describes at my "requirement to universalize categories and principles and apply them across the board to all of the members of all of the societies in Micronesia for all of 'traditional times'" (94). Given my emphasis on change, diffusion, development, and local adaptation, I simply don't comprehend this claim; I certainly have no sense that I do this.

Carucci argues that, in comparison with Hanlon's (1989) view that Micronesia is a product of European imagination, my "analysis also certifies the legitimacy of an alternate, ahistoric use of the term." By "isolating the era of 'traditional Micronesian life' from a dynamic historical perspective, Petersen fails to engage with a variety of indigenous culturally and historically emergent uses of 'Micronesian' as a meaningful identity category" (97). I will own to this latter claim, and will endeavor in the future to engage with this criticism. My final chapter, "Traditional Micronesian Societies and Modern Micronesian History," is perhaps inadequate to the task Carucci charges me with failing to address, but I would rather be convicted for falling short of my goal than for not having made the attempt at all.

Carucci further questions my analysis of Micronesia as a valid culture area: "If Petersen's kinship categories and generalized Euro-American forms projected onto the symbolic constructions and daily practices of local people, then 'Micronesia' remains unified only through European and American symbolic machinations. For this reason, I question if Petersen has provided the necessary support to justify the classification of Micronesia as a distinct culture area" (98). As I discussed at some length, I take it as axiomatic that all culture areas are constructs. "First, all culture areas or regions are intellectual, rather than naturally occurring, categories, and second, issues of homogeneity and heterogeneity are not of primary importance if we keep in mind the dynamics of adaptation and historical development, and focus on the ways in which these dynamics result in changes through time and space" (15).

> The relevant questions are whether these categories are purely mental constructs or whether they in some measure reflect reality and how useful they are as we try to make sense of the world around us. We must remember that we are talking about real people, real places, and real behaviors. The ways we group them together and the distinctions we make among them, however, are no more than perspectives we impose upon them. We construct these categories, and make distinctions among them, for specific reasons. In the end, we must keep in mind just what the purposes of these categories are, and judge their validity with these purposes in mind (15).

I certainly did not go to Micronesia looking to study clans and lineages. Pohnpeians pretty much thrust them on me. I became aware in time that much of what I was learning from Pohnpeians about Pohnpei was not peculiar to Pohnpei but was, in fact, widely shared. There clearly are, as I have said, historical linkages among the islands. How my examination of this history squares with Carucci's sense that my analysis fails to adequately explain the nature of Marshalls social thought and process is, in fact, a quite different matter. Although he ultimately praises the quality of my comparative work, he does so in the context of faulting it for missing "the entire significance of a multiplicity of locally contested histories that are of great interest to specific island and atoll dwellers throughout this region" (13). Having devoted so much of my career to studying locally contested histories, I appreciate his point but can only reiterate that that was not the purpose of my book.

I turn now to Nancy Pollock's comments.

Let me note at the outset that, as I understand Pollock's more general opening comments, she seems to misunderstand some significant aspects of what I was attempting to do. I would like to think, for example, that I have not defined Micronesia as a "cohesive social entity" (113). As I have already noted, I de-emphasized issues of homogeneity and heterogeneity (15). She further observes that "Micronesians may not view themselves as so closely related" as I do (113) and that "Whether residents of the area consider any relevance of the term Micronesia to their lives is not addressed" (114). However, I wrote that "Micronesia's peoples did not have a shared sense of themselves as a single people, any more than the Polynesians did, before European navigators and cartographers conferred their respective cognomens upon them" (22). Anyone familiar with the work of the Congress of Micronesia in the 1960s and 1970s can recall multiple points at which its members worked together as Micronesians, as well as the degree to

which they chafed against one another. In this, they showed themselves to be like any other peoples with fluid identities, fully capable of being inclusive and exclusive simultaneously as well as sequentially.

Pollock thinks I may be "uncertain about the boundaries of the entity labeled Micronesia" (113). I explained at the outset that "Micronesia extends across the Western Pacific Ocean from the southwest islands of Belau and the northernmost islands of the Marianas archipelago eastward to the northern outliers of the Marshall Islands' Ratak chain and the southern islands of Kiribati" (7), and on pages 15–36, I discuss in varying degrees of detail all the islands included in my account (including Banaba).

Pollock observes that I do not pay close attention to written accounts from the mid- to late 1800s nor to the work of German ethnographers from the early 1900s (114, 115). This is true. I did discuss at some length some of the difficulties I encountered in making use of early sources (5), and I do cite the *Journal of Pacific History* article (2007) in which I analyze at length key problems in the German ethnography of Micronesia. The issue here is related to one that I shall take up below when I return to the question of incorporating local legends: it is difficult, if not impossible, to devote space in an overview of an entire region to scrupulous analysis of variant versions. I was not prepared to immerse myself in either the missionary records for all Micronesia (and having studied those for Pohnpei, I am quite familiar with just how extensive and contradictory they can be) or the evolution of German culture theory as filtered through the writings of the German ethnographers.

The truly crucial aspect of the differences between us can be found in the matter of what I call Micronesia's "breadfruit revolution." I borrowed the basic concept from James Watson (1965), who wrote of what he called the "ipomean revolution," referring to the population expansion into the New Guinea Highlands as a result of the introduction of sweet potatoes (*Ipomea batatas*). The implications and some of the details of this seminal work have been the source of considerable debate, but in general, the concept has proved resilient and important (Yen 1974; Ballard et al. 2005). Subsistence in the high islands of the Eastern Carolines is overwhelmingly organized around breadfruit, the wide variety of other staple crops notwithstanding, and I suggest that the hybridization of two different breadfruit species that botanists tell us took place in this area had an impact on life there that can reasonably be compared to that of the sweet potato in the New Guinea Highlands. I could be wrong—I made it clear that the concept I developed was no more than what I believe to be true—but I scrupulously weighed the evidence.

Pollock, whose knowledge of subsistence crops in the Pacific Islands I hold in the highest regard, points to breadfruit's long history of diffusion in the Pacific, emphasizing that "This process of dispersal has been ongoing for over 2000 years—there is no evidence of a sudden revolution in Micronesia" (116). However, as I argued at length in the book, it is precisely this longer-term process of dispersal that resulted in a shorter-term process of hybridization/introgression between two different breadfruit species in the Eastern Carolines, and this is, in turn, spurred what I term the breadfruit revolution. There is indeed a great deal of evidence that something along these lines took place. I can understand if the evidence does not persuade Professor Pollock, but not the claim that it doesn't exist.

The data on which I draw appear in a series of papers. For brevity's sake I will rely primarily on only one of these, Zerega, Ragone, and Motley's "Breadfruit Origins, Diversity, and Human-Facilitated Distribution" (2006; also see their 2004 and 2005 papers). They note that in studying the "great variability of breadfruit cultivars" in the early years after the United States seized the Micronesian islands from Japan, Raymond Fosberg suggested this diversity was a product of introgression (i.e., hybridization) between *Artocarpus altilis* and *Artocarpus mariennensis* (Fosberg 1960). This demonstrates a crucial distinction in the history of breadfruit dispersal, because "Melanesian and Polynesian breadfruit cultivars are derived from *A. camansi*," whereas "Micronesian cultivars appear to be of hybrid origin" (2006, 226). Their own molecular research leads them to conclude that "diploid *A. camansi*-derived breadfruit was introduced into the range of *A. mariennensis*, allowing the two species to hybridize. Subsequently, varying degrees of introgression and human selection have led to the diversity of cultivars unique to Micronesia. This hypothesis is supported by another source of evidence that diploid *A. altilis* and *A. mariennensis* can hybridize" (2006, 233). Moreover, "breadfruit cultivars without *A. mariennensis* traits do not grow well in harsh atoll conditions" (2006, 234). This hybridization, specific to the Eastern Carolines, resulted in both the unique diversity that characterizes local crop inventories, thus allowing for breadfruit harvesting virtually year round and the spread of highly productive breadfruit varieties to the adjacent atolls.

In their ethnobotanical report on Pohnpei's breadfruit, Ragone and Raynor explain that Pohnpeians classify breadfruit into two basic types. One is typical of eastern Melanesian–Polynesian seedless breadfruit, they explain, whereas the other encompasses hybrid cultivars found only in Micronesia. "The greatest number of hybrid cultivars occurs in Pohnpei, and the productivity of the traditional agroforestry system and the almost year-round availability of breadfruit result from this incredible diversity of

cultivars." A list of 131 breadfruit varieties has been compiled, and in recent years botanists have verified the presence of close to fifty breadfruit cultivars on the island. Studying the seasonality of just five of these cultivars over the course of a year, researchers found that fruit "was available year-round" (2009: 65–67, 73). The botanical aspects of the revolution would seem to be well established. Ragone does note, however, that my conception of a "'Breadfruit Revolution' is aptly named for Micronesia but could also apply to eastern Polynesia as that area (Marquesas and Society Islands) was a center of breadfruit diversity and use with myriad seedless triploid varieties" (D. Ragone, pers. comm. 2014).

On pages 56–58 of my book, I discuss at considerable length and detail some of the major subsistence and economic consequences of a crop inventory that provides for nearly continual production of breadfruit in Chuuk, Pohnpei, and Kosrae. On pages 58–64, I describe the diffusion of the hybrid breadfruit varieties throughout Micronesia and the social and cultural developments that accompanied this expansion. As I say, this is all hypothesis, but it is built on careful marshaling of a great deal of evidence. I understand that Pollock is not convinced, but this does not mean that "there is no evidence."

In a different vein, Pollock writes that matri-organizations "are not the only, nor necessarily a cohesive, form of social organization across the region of Micronesia that Petersen suggests" (117). Again, I am in complete agreement with her observation that matriclans are not the only form of social organization in the region and that they aren't necessarily cohesive. I disagree, though, with the notion that I have made any claims to this effect.

I chose to foreground descent because my primary goal was to explore what Micronesian societies have held in common, as a means of examining historical connections within the region. The chapter following my treatment of descent and descent groups focuses on household, family, land, and labor. I carefully delineated the myriad ways in which land and social groups are conceptualized and linked. Additionally, in two more chapters, I did the same with political titles, land, and social groups. Pollock takes me to task for overlooking or ignoring these complexities in Marshalls, much as Carucci does. I acknowledge that I have not probed deeply into local details there. In a work of ethnology in which I compare a hundred or so different island societies, there simply was not space for detailed, nuanced coverage of local cases. I note in particular Pollock's observation that "Leadership in other social arenas was also important" (122), but in fact, I discussed the many sorts of roles and qualities entailed in Micronesian leadership at great length on pages 130–157.

In the context of what I do and do not address, there is one more key point I would like to amplify. Pollock repeatedly calls for an approach that incorporates local legends (123, 124). This is, of course, a good idea, at least on its face, but also it poses significant obstacles. I am fully aware of the importance of carefully considering local mythohistorical accounts, particularly because they so readily lend themselves to misinterpretation. Responding to the misappropriation of Pohnpeian mythohistory by prehistorians, I published an entire volume devoted to examining a corpus of variant versions of Pohnpei's central origin and political charter legends, *Lost in the Weeds: Theme and Variation in Pohnpei Political Mythology* (1990). In the course of that work, I reached the general conclusion that for every variant of a socially or politically significant myth there is an equal and opposite version. The overall importance of these accounts lies in the entire body of materials, but any individual version has probably been shaped to the advantage of one specific group or another within the larger society. In a work that attempts to include virtually every Micronesian society, as mine does, there was simply no way I could make use of local legends without being forced to pick and choose from among materials over which I had little or no command. There are, unfortunately, few other studies that examine an entire corpus of a society's stories (e.g., Lessa 1961), and in the absence of reliable guides to these materials, I felt obliged to steer clear of what I perceive as something of a minefield.

Karen Nero draws primarily on her experience in westernmost Micronesia, and her concerns differ significantly from those of Carucci and Pollock. She aptly notes the absence of evidence indicating that the "Western Micronesian islands comprised a culture area prior to the settlement of Eastern Micronesia" (104). This is an important point, and one that I did not really address. My sense of the archaeology is that we grow increasingly closer to locating the sources of Palauan settlement in what is now Indonesia and of the Marianas in the Philippines. There is no reason to think that there were no interactions among Palau, the Marianas, and Yap before the Nuclear Micronesian–speaking peoples moved west, but neither is there direct evidence of this. This point is not crucial to my thesis, but it is nevertheless important. Inasmuch as I entertain hope that my arguments will in time provoke further archaeological research into the area's prehistory, I am eager to learn more.

Also, we can look forward to further work on climate change and the habitability of the islands in the era of earliest settlement. Nero points in particular to occupation of Palau's Rock Islands (106), but these issues also concern the atolls and many of the earliest sites in Guam and the rest of

the Marianas. In a related vein is the very pertinent question of whether breadfruit hybridization took place in western Micronesia as well (106). I welcome new research that challenges my focus on the east by locating sites of transformation in the west.

Nero concludes that "recent research in Palau has if anything strengthened Petersen's argument that around two thousand years ago, a Micronesian culture area began to develop across the region despite a long hiatus between the Western and Eastern settlements." Although she concurs that at its core Micronesia is matrilineal, she adds that "Perhaps the culture is not best described by a close focus on the matrilineages despite the region's strong matrilineal social organization," suggesting instead the locution "matri-centric societies" (108). I am more than willing to consider this possibility, but it is, in fact, with the extensive, persistent, and flexible webs of connections I am most concerned and not their matrilineal aspects per se.

As I said at the outset, I had many reasons for writing this book, but demonstrating the essential validity of "Micronesia" as a culture area was among the most important. These reviewers all agree that I have to some degree achieved this, and I hope that I have satisfactorily responded to the doubts they raise. Micronesia is considerably more than a colonial construct.

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