BOOK REVIEW FORUM

Response: GLENN PETERSEN
PROFESSOR AND CHAIR, DEPARTMENT OF SOCIOLOGY AND ANTHROPOLOGY, BARUCH COLLEGE, CITY UNIVERSITY OF NEW YORK

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

Pacific Studies, Vol. 37, No. 2—August 2014

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.

REFERENCES

Alkire, William

1980 Technical knowledge and the evolution of political systems in the central and Western Caroline Islands of Micronesia." *Canadian Journal of Anthropology* 1 (2): 229–37.

Ballard, C., P. Brown, R. Bourke, and T. Harwood

2005 *The sweet potato in Oceania: A reappraisal*. Sydney: Oceania Monographs 19.

Fosberg F. R.

1960 Introgression in *Artocarpus* in Micronesia. *Brittonia* 12:101–13

Hanlon, David

1989 Micronesia: Writing and rewriting the history of a non-entity." *Pacific Studies* 12 (1): 1–21.

Hunter-Anderson, Rosalind, and Yigal Zan

1996 Demystifying the Sawei, a traditional interisland exchange system." *Isla* 4:1–45.

King, Thomas, and Patricia Parker
1984 Pisekin Noon Tonaachaw. Micronesian Archaeological Survey Report No. 18. Southern Illinois Univ. at Carbondale Center for Archaeological Investigation. Occasional Paper No. 3.

Lawson, Stephanie
1996 *Tradition versus democracy in the South Pacific*. Cambridge: Cambridge Univ. Press.

Lessa, William
1961 *Tales from Ulithi Atoll: A comparative study in Oceanic folklore*. Folklore Studies, No. 13. Berkeley: Univ. of California Press.

Petersen, Glenn
1990 *Lost in the weeds: Theme and variation in Pohnpei political mythology*. Center for Pacific Islands Studies Working Papers No. 35. Honolulu: Univ. of Hawai'i.
1992 Off-the-shelf tradition: Variation versus invention. In *Pacific History*, ed. D. Rubenstein, 201–11. Mangilao: Univ. of Guam Press.
1995 The complexity of power, the subtlety of Kava. In *The power of Kava*, ed. N. Pollock, special issue, *Canberra Anthropologist* 18 (1/2): 34–60.
1997 A Micronesian chamber of chiefs? In *Chiefs today*, ed. G. White and L. Lindstrom, 183–96. Stanford: Stanford Univ. Press.
1999 Sociopolitical rank and clanship in the Caroline Islands. *Journal of the Polynesian Society* 108 (4): 367–410.
2006 Micronesia's breadfruit revolution and the evolution of a culture area. *Archaeology in Oceania* 41:82–92.
2007 Hambruch's colonial narrative: Pohnpei, German culture theory, and Hamburg expedition ethnography of 1908–10. *Journal of Pacific History* 42:317–30.
2014 Led astray by too much Kava. Paper presented in the session Fieldwork on the cusp: Anthropologists in the western Pacific, 1960–1985 at the ASAO meetings, Kona.

Ragone, D., and B. Raynor
2009 Breadfruit and its traditional cultivation and use on Pohnpei. In *Ethnobotany of Pohnpei*, ed. M. Balick, 63–88. Honolulu: Univ. of Hawai'i Press.

Schneider, David
1984 *A critique of the study of kinship*. Ann Arbor: Univ. of Michigan Press.

Watson, James
1965 The significance of recent ecological change in the central highlands of New Guinea. *Journal of the Polynesian Society* 74 (4): 438–50.

Yen, Douglas
1974 *The sweet potato and Oceania: An essay in ethnobotany*. Honolulu: B.P. Bishop Museum Bulletin 236.

Zerega, N., D. Ragone, and T. Motley.
2004 Complex origins of breadfruit: Implications for human migrations in Oceania. *American Journal of Botany* 91 (5): 760–66.
2005 Systematics and species limits of breadfruit (Artocarpus, Moraceae). *Systematic Botany* 30 (3): 603–15.
2006 Breadfruit origins, diversity, and human-facilitated distribution. In *Darwin's harvest: New approaches to the origins, evolution, and conservation of crops*, ed. T. J. Motley, N. J. C. Zerega, and H. B. Cross, 213–38. New York: Columbia Univ. Press.