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My fellow Micronesianists Mac Marshall, Mary McCutcheon, and DeVerne Reed Smith do me the distinct honor of taking seriously the arguments of *The Sacred Remains: Myth, History, and Polity in Belau (SR)*, and I welcome this opportunity to thank them and other reviewers of the book<sup>1</sup> for their careful attention and insightful criticisms. Taken together, the reviews have stimulated my own reexamination of the book's central themes and specific conclusions about the possibilities of a semiotic approach to Micronesian ethnography, and have caused me to become aware of ambiguities in its presentation and of more serious problems in its overall conceptualization. My goal in this response is fourfold: first, to state briefly what I believe to be *SR*'s main theoretical contributions and ethnographic conclusions; second, to respond to a few general areas of criticism found in more than one review; third, to clarify some misunderstandings that the book has unfortunately promoted; and fourth, to indicate avenues for future Belauan research in light of the reviews. Whereas this genre of author's response often functions to cut off further debate, I hope that mine will encourage it.

### Signs and History in Belau

The principal utility of *SR's* application of an analytic vocabulary taken from Peircean semiotics is the isolation of four "iconic" diagrams (paths, cornerposts, sides, and gradation) that recur in many contexts of Belauan culture and the specification of a category of "indexical" signs (called *olangch*) that function as cultural "shifters" by articulating these organizational diagrams with social institutions and historical experience. In contrast to structuralist approaches, the four Belauan diagrams are analyzed as differentially valued, and thus the possibility of transformation without change of meaning is denied.<sup>2</sup> And the focus on *olangch* as (Peircean) indices is intended to replace the (Saussurean) notion that semiotically constituted cultural categories operate independently of nonsemiotically constituted historical events. Thus, *olangch* are both signs of history and signs in history,<sup>3</sup> and the intentional manipulation of these semiotic markers depends on regularities in their contextual deployment.

In his comments Marshall (citing Victor Turner) makes an excellent observation in saying that, despite factional tensions and contradictory principles of social organization, members of a society share a body of "symbols" that can serve as signs of their collective identity (what Turner aptly calls "multifaceted mnemonics" [1977:58]). This can be fruitfully applied to Belau by viewing these indexical markers, whether stones, valuables, or names, as shared signs that then get organized or manipulated differentially.<sup>4</sup> The theoretical importance of Marshall's point is that, for societies like Belau at least, cultural identity consists of regularities of indexical structure rather than of Schneidereal "galaxies" of abstract symbolic constructs—a point repeatedly made by Michael Silverstein (1976).

Second, *SR* advances a generalization that the systematicity of these diagrams and indices is inversely related to indigenous awareness (p. 120; cf. Silverstein 1981). The "emic" categories, with distinct lexical labels and prototypical exemplifications, are put into play in narrative sequences and spatial patternings, but there is no consciousness of the complexities or implications of diagrammatic combinations, since the knowledge of actors relative to the system is necessarily limited by both temporal constraint and social position. For example, a storyteller from 1800, a period of intense confederation factionalism, would perceive the Milad polity as an archaism, while a contemporary storyteller would consider the opposed "sides of heaven" to be an equally "traditional" pattern: similarly, a person cannot occupy an Achimedean posi-

tion above the hierarchical social order. As a result, the analysis of Belauan culture as a system offered in *SR*, while obviously building on local models, texts, and interpretations, is not one that any Belauan would or could have suggested. Certainly I never had any hint from my informants concerning the semiotic conclusions proposed in the book—I only started to look for systematicity after a series of challenging questions from Marshall Sahlins, George Stocking, and Michael Silverstein at my thesis defense. Of course, having sent to Belau copies of my thesis in 1981 and the book in 1987, I would suspect that some Belauan readers have by now developed reactions at a systematic level.

Third, a goal in writing the book was to suggest that Sahlins's mode of analysis in *Historical Metaphors and Mythical Realities* stands in need of revision, especially with reference to the semiotic relationship between myth and history. Although I clearly indicate (and here reaffirm) my deep indebtedness to Sahlins, my teacher, thesis advisor, and now colleague, the criticisms of his model in *SR* are indirect and muted, as might be expected from a young scholar trained under Pacific studies' common "big man" turned "chief"-if not "sacred king." For Sahlins (1981), myth is to history as type is to token; that is: cultural categories are to contextual experience as general semiotic regularity is to realized signs. While clearly a devastating critique of all Malinowskian "character" theories of myth, since myths involve inherited categories rather than being merely present fictions created as projections onto the past, this analytic model is limited by its contention that the pan-Polynesian theory of myth and history is grounded in type-token relations, technically, a semiotic relation of trivial iconicity.<sup>5</sup> Thus, the transformation of mythic categories (e.g., the oppositions agriculture/warfare, fertility/sacrifice coded in the Lone/Ku myths) is inevitable when these categories become instantiated in social contexts manifesting conflicting human interests (e.g., Captain Cook's fateful visit of 1779; Sahlins 1988:43). Additionally, my Belauan research was designed to show that a culture's historicizing activity is not restricted to narrative discourse. This is why I attempt in *SR* to link up lithic and spatial signs to the more strictly linguistic material that Sahlins relied on.<sup>6</sup> In the end, for Sahlins myth and history are semiotically disjoined, a function of his concentration on symbolic values rather than on indexical values, that is, those signs with meaningful regularities of a temporal and spatial character.<sup>7</sup>

The Belauan materials examined in *SR* offer an alternative possibility: that type-token iconism is *only one* of several semiotic models, others being indexical shifters, quadripartite coordination, and hierarchical ranking. Clearly, Belauan "paths" featured in the Latmikaik-Chuab

stories, as well as in the "dynamic" perspective on political order (p. 136), are roughly equivalent to the Polynesian model of "replication" proposed by Sahlins. But the origin myth cycle goes on to describe the beginnings of not only a logic of indexicality (found paradigmatically in the journeys of Rak and Chuab, where *olangch* [place names and chiefly titles] are deposited in a chain of locations) but also a logic of "textuality" in the sign complexes underlying the sibling-based polity created by Milad. Furthermore, in Hawaii diachrony is restricted to the realization (and reformulation) of mythic templates stimulated by exogenous forces; in Belau diachrony can be seen in the narrative transformation of culturally valued semiotic models, with the power to typify exogenous forces of change in terms of local theories of history (p. 154). Thus in Belau the myths reveal the category of historical transformation, while in Hawaii the mythic categories are the object of transformation of historical forces.

Of course, what would be highly illuminating, though totally inaccessible, are data on the diachronic trajectory of mythic narratives' use of these alternative semiotic models. The sequence of myths reported in chapter 4 represent these models as themselves coming into being in a particular order (paths [plus indexicality], then cornerposts, then sides), but the history of that representation would enable us to specify the pragmatics of historical representation in a way exactly parallel to ethnographic data on the contextual manipulation of sacred stones (thus rendered signs in history).<sup>8</sup> Being denied this retrospective information, Belauan scholars can look forward with eagerness to the continuing work of Nero and Smith, who for Koror and Melekeok respectively are charting the path-or the sides-of contemporary historiography in Belau.

At the ethnographic level-and here I beg the patience of non-Belauan specialists-SR's most important contribution is the identification of forms of political organization other than the well-documented binary factionalism (the "sides of heaven") so elegantly analyzed by McKnight (1960) and others. In reviewing evidence for a conceptually powerful quadripartition (the polity of Milad) and for a mythologically projected polity based on linear paths (the polity of Chuab), I open up Belau to cross-cultural comparison with island Southeast Asia, where "cornerposts" and "paths" are widespread. Similarly, my analysis renews the possibility of interdisciplinary cooperation between ethnographers and archaeologists,<sup>9</sup> since the status of the polity of Chuab (i.e., a set of villages located roughly on the east coast of the archipelago) is at present without empirical documentation. These stories of migration

and relocation could be a mythic representation of either initial settlement at either the northern (Ngetmel) or southern (Ngeaur) extremities, of sequential in-migration from various island Southeast Asian sources, or of dramatic demographic dislocation due to overpopulation or natural disaster. Perhaps it will be possible, as the recent work of Van Tilburg (n.d.) points out, to correlate transformations in political organization with a diachronic typology of monolithic stones. The polity of Milad could also receive empirical confirmation if the pattern of quadripartition was found to characterize stoneworks throughout the archipelago. Without such corroboration, however, *SR* only argues that the distinct organization of the Chuab and Milad polities reflects semiotic and ideological concerns rather than actual political dynamics (p. 196).

Second, I link the “dynamic” historiographic perspective of low-ranking districts, villages, houses, and titles to the earlier Latmikaik-Chuab myths of linearity and the “static” perspective of high-ranking entities to the Milad cycle with its quadripartite order-ironic in that the myths referring to a period later in time are used to legitimize the higher-ranking perspective, but not ironic when contrasting implications are appreciated: that linearity carries the possibility of new paths and fresh combinations, while quadripartition projects an image of stable maturity, generated at one moment and immune from alteration.<sup>10</sup> The strongest evidence for the regularity of this linkage is that the cornerpost model of the pan-Belauan Milad polity is said to have been created at the moment the goddess gave birth to four stone-children in the forest of Ngerebesek, while the cornerpost polity of Ngeremlengui district is said to have been created in parallel fashion by the Ruchel gods at a stone pavement in nearby Ngerutechei village. These two acts of “dynamic synchrony” are thus indexically anchored and become thereby mutually reinforcing. The apparent strengthening of Chuab stories in contemporary Belau, far from challenging the thesis of *SR*, would in fact be equally elegant proof of the validity of the postulated correlation between political rank and historiographic perspective. That is, the Ngeremlengui-based model of Belauan polity should be strongly repudiated by modernizing political forces in Belau—a situation any ethnographer would find disconcerting, since the validity of *SR*’s conclusion requires the rejection of a central portion of its data by local people!

Third, I propose that the peculiarity of Imeiong’s title organization (the only capital village where female titles go to wives rather than sisters of the four cornerpost titleholders) can be accounted for by seeing

the relatively “endogamous” self-reliance of quadripartite politics as one way to isolate Imeiong’s presupposed high rank from the exigencies of exchange, warfare, and alliance. While awarding sacred female titles to in-marrying women may at first seem a strange way to limit the demands of affines, upon reflection the elegance of the strategy is obvious: giving in-married high-ranking spouses Imeiong-based titles derived from local house names deprives them of their own matrilineally generated titles; similarly, chiefly sisters sent to marry outside Imeiong are not accompanied by Imeiong labels. This analysis is, at a more abstract level, supported by the generalization that, in Belau as elsewhere in the Austronesian world, entities of the highest rank (villages, titles, valuables, stories, stones) are maintained as constitutive of the cultural order by being kept out of the ebb and flow of historical experience (p. 267).

### **Local Models and Analytic Constructs**

I turn now to three specific areas of criticism made by more than one reviewer: the biased character of narrative data, the putative awareness of semiotic complexities by Ngeremlengui informants, and the problem of missing ethnographic information. <sup>11</sup> *SR* has been criticized for relying too heavily on information provided by a single informant, the late Ngiraklang Malsol. To be sure, many of the texts translated in the book were recorded during sessions with Ngiraklang, who was my “father,” teacher, and friend. We worked together approximately three afternoons per week for two years, and my collection of his narratives runs to over a thousand typed pages, a corpus I hope to edit and publish in the future. Ngiraklang, as all who knew him will agree, was a genius, blessed with a remarkable memory that seemed to sharpen at the same time that his short-term memory and physical strength began to fade and a fierce dedication to finding out the “true” account. He was also proud of his position as Ngiraklang, traditionally the highest-ranking title of the capital village of the highest-ranking district in Belau. It was his passionate belief that knowledge of the stories and stones of Belauan tradition would be lost forever if elders like himself did not exhaust themselves in making both public and permanent what had once been “closely guarded” (p. 16).

I do need to explain, however, that during the course of my fieldwork I spent almost every free evening in the company of Chief Ngirturong Otaor and also studied with Ngirutelchii Dudiu, another member of the “cornerpost” council of titleholders in Ngeremlengui. But *SR* is not a

book based solely on “elite” sources. In order to develop sensitivity for contrasting perspectives I learned about the history of Ngeremlengui from untitled individuals from low-ranking Ngeremetengel village (the basis for the analysis in chapter 6). And in order to contrast male and female perspectives I worked closely with several female titleholders and young women without titles, several of whom have continued to be a great help over the years. Curiously, the biggest gap in my knowledge is from my own cohort, that is, young male adults, in whose sporting and fishing life-style I did not participate. More crucially, during my stay in Ngeremlengui the district conducted a “school” in Belauan traditions for all villagers; titled elders instructed young and old, male and female, titled and untitled in myths and chants, explained sacred stones, and recounted historical events.<sup>12</sup> All the elders in Ngeremlengui were most eager for everyone in the district to learn about their past, and I only hope that the stories I translate in this and future publications will assist in that effort.

My understanding of Belauan culture was, in addition, shaped by careful study of the Belauan texts collected by the researchers associated with the Palau Community Action Agency, especially the chants and stories recorded by Santos Ngodrii Kloteraol. That this vast archive of oral traditions has been destroyed by fire is an immense tragedy for the Belauan people, and my partial transcriptions of this material constitute only a small replacement. To summarize, *SR* is about Belau from the perspective of Ngeremlengui (p. xix), and its conclusions about the relationship between multiple perspectives biased by rank are derived from the widest possible distribution of resources within the district and in Belau.<sup>13</sup>

Second, several critics have claimed that my semiotic analyses are far too complicated to represent the conscious or “emic” models of my informants. I must apologize if any readers got the impression that this was my claim or if they were led to believe that my informants knew about overlapping diagrammatic complexes, sequential narrative transformations, or transcendent hierarchical categories. These constructs and the conclusions about their systematic relationship—both diachronic and synchronic—belong entirely to the analytic language of cross-cultural semiotic anthropology and comparative Oceanic ethnography. A point of possible confusion arises over any claim that people in Belau recognize a semiotic model, such as an iconic form (e.g., “path”) or an indexical pattern (*olangch*). This claim does not mean that people know that a “path,” for instance, is a semiotic construct, only that their intentionality and social action evidences shared rules of interpretability. We

can call these "ethnosemiotic" when they are given local lexical labels. The analytic vocabulary for these regularities developed through cross-cultural research is the business of anthropologists' metalanguage. Marshall's statement that the four icons taken "together constitute an underlying cultural structure" is correct but I contend that, as a system, this structure is not an "emic" one, that is, not subject to awareness or pragmatic manipulation. As noted above, the only claim about "psychological reality" (Barley 1988) pertains to the individual diagrams and distinct sets of *olangch*. The phrase quoted by Marshall from p. 54 about "the perspective of local actors and their cultural categories" was not meant to imply that Belauan culture as an "underlying structure" (Marshall's phrase) is the object of indigenous awareness. On the other hand, I completely side with Marshall's point about scientific regularities and, with him, insist that Smith's "two levels of discourse" is far from a "limitation of our models," but rather constitutes the necessary foundation for anthropology as a scientific discourse.

The only thing distinctive in my account is that it takes as data to be explained Belauan concepts and signifying forms (lithic signs, narratives, patterns of action) and relies on a limited set of ethnosemiotic constructs (e.g., *olangch*).<sup>14</sup> This attention to indigenous sources does not imply that Belauan storytellers are historians in the sense of being factual reporters of events or that the salience of myth places Belauan culture in some mythopoetic world where people believe they are fish. The point, rather, is that stories and myths provide important clues for outside observers to discover the meaningful categories operating in the culture that motivate actors to fight real wars, conclude real marriages, and confront real colonial powers. As should have been clear, I strongly oppose both the structuralist assumption about logical transformation as well as the Geertzian aim to adopt native explanatory models as our own.<sup>15</sup> And, at the level of ethnographic evidence, I sincerely hope that no other readers entertained the notion suggested by Smith that I am somehow the source of my own data.

Third, critics have pointed out that *SR* presents only a portion of Belauan ethnography, since it treats the culture at the political level (titles, villages, districts, confederations, polity, what Belauans call "public affairs") and neglects data such as kinship roles, exchange rituals, and marriage strategies (what Belauans call "household affairs"). While perfectly valid, this criticism is even more serious given the fact that in an early publication (Parmentier 1985) I proposed a model to explain the articulation of three levels of social reality (person, house, and district) by comparing the constitution and movement of three



classes of stones (valuables, gravestones, and monoliths). While I have not entirely rejected this admittedly premature synthesis, I could not manage to treat all these forms of data in a single book—may I claim Meyer Fortes and Raymond Firth as my models here? I have, however, published separate articles on the system of house affiliation (Parmentier 1984) and on exchange rituals (Parmentier 1988). In fact, *SR* suffers from a double delimitation of its scope: the political over kinship and “traditional” stories over contemporary narratives; the companion volume in preparation on kinship, exchange, and village-level social change will, I trust, remedy this situation.

### NOTES

1. Barley 1988; Goodenough 1989; Hanlon 1989b; Körner 1989; Lieber 1989; Lingelfelter 1988; Mason 1988-1989; Nero 1989.

2. Perhaps the closest ethnographic parallel to the range of Belauan diagrams is the case of the Inca, for which Tom Zuidema (1983, 1989) has identified dual opposition (upper/lower), quadripartition, lines, and hierarchical gradation. Even Inca sacred stones (*huaca*) resemble Belauan *olangch* in that they were carried away by conquerors.

3. Despite the use of quotation marks in Goodenough's review (1989:544), I never call *olangch* “symbols of history” or “symbols in history,” since this would confuse the point about their essential indexicality.

4. A slight problem with this application is that for Turner it is the *significata* rather than the variable signifying forms that are shared “values and norms” (1977:79). Furthermore, ritual symbols are not called *olangch* in Belau, whereas the Ndembu term for ritual symbol means “landmark, or blaze” (Turner 1973: 1367).

5. Sign occurrences that are instances of general regularities are produced and recognized in terms of formal resemblance between the template or type and the replicated instance. This kind of iconism should not be conflated with the more important resemblance that motivates the relationship between sign vehicle and object.

6. Much more work needs to be done on the role of stones in Belauan political action, especially to investigate the correlation between types of stone and patterns of manipulation; stones also need to be studied in relation to exchange valuables.

7. In the introduction to *Islands of History* Sahlins significantly modifies this argument in claiming that “prescriptive” and “performative” structures can be found within the same society and in noting that the realization of cultural categories in history is a matter of pragmatic construal (1985:xiii).

8. In *SR* I do not consider the problem that the Ngeremlengui stories as told today are peculiar in Belau in that, for the most part, storytellers do not participate in the rhetoric of binary political factionalism, for example, between Koror and Melekeok or between the “upper” zone and “lower” zone of the archipelago.

9. A first effort at this cooperation is Lucking and Parmentier 1990.

10. Since the skewing of mythical and historical narratives by rank is the subject of three chapters and the primary point of the entire book, I am perplexed that Smith found the topic "not explored consistently."

11. An unintentional laxity in proofreading and miscommunication with the press resulted in more typographical errors than is acceptable for scholarly publications. Readers can easily correct most of these, but I take this opportunity to correct errors in Belauan: p. vii *read* Iechadrachuolu; p. 32 n. 8 *read* Bairulchau; p. 971.10 *read* Ngerard district; p. 1141.41 *read* Uchelsung; p. 1421.26 *read* Obakeramechuu; p. 155 *read* (Ngerard); p. 203 1. 16 *read* kerdi; p. 2031. 18 *read* dosurech; p. 2111. 31 *read* Bungaruau; p. 248 1. 19, 1.21 *read* Ngchemesed; p. 2551. 8 *read* ulak; p. 273 1. 26 *read* Chemeruaol; p. 3021. 11 *read* Ngarameketii; p. 3021. 15 *read* Obilmeai.

12. Since my tapes of these sessions are the single greatest source of information I have about the transmission of cultural tradition in Ngeremlengui, I question Smith's contention that the information was collected without "traditional process of checks and balances."

13. Both Nero (1989) and Smith neglect this crucial limitation, though their specific corrections on details of Koror's and Melekeok's history are welcome. In particular, Nero's point that on p. 77 I confuse the removal of an individual Ibedul titleholder with the reordering of the system of titles is especially important because it illuminates the differences among Imeiong, Melekeok, and Koror with respect to chiefly titles: Imeiong recognizes no historical sequence of in-migrations of its four titles, Melekeok talks clearly about unification by a late-arriving line, and Koror insists that its chief came to power at a particular point and never lost it.

14. See Hanlon 1989a for an excellent review of trends in Micronesian history.

15. My substantial debt to the philosopher Charles Taylor should have been directly indicated; see his essay "Understanding and Ethnocentricity" (1985).

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