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BOOK REVIEW FORUM [Journal page **97**]

on

Harriet Whitehead's

Food Rules: Hunting, Sharing, and Tabooing Game in Papua New Guinea
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Contributing Reviewers include:
Naomi M. McPherson, Daniel M.T. Fessler, Sandra Bamford, and Patricia Townsend

Response to reviews by ***Harriet Whitehead***

We are pleased to present JRS Review Forum No. 2
Pamela J. Stewart and Andrew Strathern, Co-Editors *Journal of Ritual Studies*

Food Rules: Hunting, Sharing, and Tabooing Game in Papua New Guinea
(Harriet Whitehead,University of Michigan Press, 2000)

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[Journal pages **98-104**]

This is a most fascinating, complex and in-depth analysis of the topics of food, food taboos and hunting. Whitehead attempts two things in this work: a major critique of a concept of culture, that "old and still regnant paradigm in cultural anthropology" which "lies in ruins" yet remains "the average cultural anthropology professor's" mainstay in the classroom (p.2). Her second objective is an ethnography of Seltaman attitudes and feelings, rules and social relations that surround hunting, game meat, and sharing. Although Whitehead's theoretical perspective informs the ethnographic analysis at every turn, she deliberately separates theory and data by inviting readers uninterested in the first thirty pages of "deep theory" to "take this opportunity to walk the dog" (xiii), skip the theoretical machinations and proceed directly to Chapter One. The book thus has two parts: theory presented in the "Theoretical Introduction: Culture and Causality," the "Conclusions," and in Chapter One: "The Research Background;" and, secondly, an ethnography of Seltaman food complexes. I shall follow that lead and address myself first to the "deep theory" and secondly to the ethnography.

Whitehead demonstrates effectively that, in addition to the allocation of resources, food taboos are part of a complex of gustatory attitudes, feelings and abstentions which are implicated in the creation and maintenance of social boundaries, masculine authority and gender inequalities. However, Whitehead argues that at least two issues stand in the way of anthropological (or social scientific) understanding of such "food complexes." First is the much "overblown" emphasis in anthropology of the "symbolic meaning" of food taboos. Second, is the predominant, twentieth century concept of culture as systemic that has "stymied the understanding of food complexes and indeed many other human patterns as well." The paradigm shift we are to engage in is to perceive of cultural forms, such as food taboos, gender relations and the like, as "the shifting dynamical outcome of diverse, and often contradictory, causal forces arising in diverse and not necessarily related areas of social, psychological, and ecological reality"(xii). The theoretical premises for the new paradigm are adapted from current trends in the life sciences. From the cognitive and computational sciences Whitehead adapts the concept of dynamical systems theory as a means to apprehend "causal scenarios in complex evolving systems;" from experimental psychology, she adapts the concept of "modularity of mind" (p.1).

A dynamical system "is a set of variables that mutually affect each other's changes over time" (Geertz 1994 in Whitehead p.18). Modularity of mind has to do with the concept of "specialized cognitive domains" or "area[s] of cognition or perception possessed of distinct parameters, distinct information detectors, and distinct algorithms for processing detected information" (p. 6). Sperber (1994:40) adds further insight to the modularity of mind concept by explaining that a

> cognitive model is a genetically specified computational device in the mind/brain (henceforth the mind) that works pretty much on its own on inputs pertaining to some specific cognitive domain and provided by other parts of the nervous system (e.g., sensory receptors or other modules).

Given a multiplicity of domains that comprise the "exceedingly complex architecture called mind," Whitehead argues that culture is merely "one type of extrasomatic component of [the heterogeneity of] mind" (p. 8). Therefore, as only one component of a dynamical system, culture cannot be a causal force; rather, culture emerges from the complexities of interactants whose "laws" or internal principles cannot be read from surface representations. The analysis of culture is grounded in a methodology to

> observe a given amalgam of mind [culture, or aspects of culture such as gender or food complexes] and its external scaffolding as it unfolds in individual development; we must observe it in motion over

> extended time scales among communities of mature users; and we must consider it in comparison with similar amalgams elsewhere if we hope to get a handle on its hidden laws—its protocol (p.9).

Since, as Sperber makes clear above, a domain is a 'genetically specified computational device in the mind/brain' or 'mind,' and Whitehead maintains that "diverse and plural mental fields" or domains are "called mind" (p.8), we are clearly involved in a higher order theoretical exercise, nothing less than a "deep theory" of mind and of culture as an extrasomatic, heterogeneous, expression of mind/brain. Thus, ethnographers must overcome "the culturalist's self-righteous shrinking from areas of inquiry that smack of biology [mind/brain] and other forms of material causality" and "avoid uniting around sterile struggles for supremacy in the causal field"(p.23). Rather than culture being homogenous and "causal," we are dealing, metaphorically, with a "palimpsest of dynamical outcomes" or a "tapestry" such that analysis entails tracking a thread and all its possible variables over an extended period of time, "rather than through the snapshot samples we now call 'fieldwork'" (p.23-24). Finally, anthropologists should find "explanatory satisfaction" by taking "delight in the causal intricacies of mundanely plausible human factors rather than the elicitation of mysterious cultural laws" (p.24).

Clearly the mind/brain is seen as a computer, and both Sperber and Whitehead use the language of computer jargon: computational device, inputs, protocols, architecture, and "mature users" of culture. Perhaps we are concerned with computers/brains as expert fifth generation systems that have a certain architecture (or hardware) which includes "a matrix of *innate* human social reflexes…evident even in their diverse cross-cultural manifestations…. [that] encompass sharing, reciprocity, responding to and asserting authority, and operating both a calculus of exchange and a calculus of equality" (p.28, emphasis added). Even Whitehead's critique of "culturalists" (said like a bad smell) is laden with computerese. Culturalists, she contends, assume 'mind' to be some sort of *tabula rasa* which possesses "general information processing routines" and takes on board (the mother board?) cultural material (data?) that can be "easily programmed with the cultural software of the local community" (p.2). Merely perceiving of the computer as mind influences how we think about mind as computer such that our language is peppered with computer jargon that further constrains us to think of mind in terms of a computational model. Simply because the human mind has conceived and created the computer, does not mean that the human mind *is* a computer, or thinks like a computer or, more importantly, that computers 'think' like humans think. Even a cursory review of the history of anthropological theory cannot but impress us with the fact that our models structure the manner in which we think about and apprehend our reality. But models of reality are not reality, to think that they are is to mistake the map for the territory: the universe is not a machine; society is not an organism; the mind is not a computer. Models assist us in thinking about our universe and devising alternative ways of imagining our universe; models are also conservative and constrain our ability to conceive of our reality differently. The problematic here is not modeling but the model and Whitehead's re/viewing of the mind/culture dichotomy is not ameliorated by such modernist techno-reductionism.

In order to make her theoretical points in high relief, Whitehead sets up the 'old' culture concept paradigm. She is particularly critical of the concept of culture as a causally deterministic, exogenous force (a Durkheimian social fact, in essence) that inscribes itself on a "passive and receptive" mind in system-like ways (p.4) so that people will "do virtually anything their culture tells them to" (p.2). This is not a new approach, of course, but is a familiar critique of, for example, the Sapir-Whorf hypothesis. Equally suspect are anthropologists who believe there is "a generic realm, culture, which can be isolated from other hypothetical realms—'the material,' 'the psychological,' 'the social,' 'the practical,' 'the political,' and assigned a kind of causal superiority" (p.5). While she can be forgiven the polemics involved in setting up her argument, Whitehead is, I offer, much too heavy-handed with the brush used to tar and feather anthropologists whom she accuses of applying a kind of naïve, reductionist Cartesianism that makes up the "recent paradigm [of the]…average cultural anthropology professor" (p.2). This generalized anthropology professor dismisses any relationship between biology and culture and argues that kinship, male dominance, food taboos, and the maternal bonding response "have nothing to do with biology" (p.3). Not only are anthropologists depicted as cultural determinists, but they also espouse a

naïve and ethnocentric view of the kind that posits "material acquisitiveness [as] no more than a Western cultural virus spreading to previously unspoiled tribal peoples…" (p.3). I think it would be difficult to find a contemporary "anthropology professor" who fits this somewhat offensive characterization and the case is annoyingly overstated.

Nevertheless, we are given, as exemplar of this culturalist anthropologist, the early work of Clifford Geertz, most notably his collection of "dazzling" and "adroit" essays (p.4) in *Interpretation of Cultures* (1973). Geertz's culture concept is characterized as a determinative, organized (read homogenous) system with a kind of A → B, "billiard ball causality eschewed by the more sophisticated" (p.5) among us. Geertz's concept of brain/mind is subsumed in his "memorable metaphor or the 'unviable cabbage'" upon which culture inscribes itself when, in fact, the quotation Whitehead cites from Geertz reads that, "the *Homo sapiens* brain, having risen within the framework of human culture, would not be viable outside of it" (p.4) and thus specifies an interaction of biology and culture, not a rejection of biology. Pace Whitehead, this sounds like an aspect of a "dynamical system" of interactants, rather than a culture-is-causal theme. The "Geertzian mind" is not "passive and receptive" and does not present a "simple cultural automaton portrait of humanity" (p.4) as Whitehead would have it, but is, in fact, minimally an outcome of a complex relationship between 'culture' and 'nature.' In a recent work, Geertz refers to his two early articles—which Whitehead cites as epitomizing the classic depiction of culture as causative—and characterizes his own, 'Geertzian' approach to culture as "constitutive as opposed to an add-on view of the role of culture in human evolution" (2000:202 n19).

Geertz's essays were written forty years ago at a time when mainframes still filled airplane hangars and personal computers were not something that most six-year-olds in North America had available to them. While I am not an apologist for Geertz, Whitehead clearly has created out of his early work a kind of straw man, useful for tearing down in order to build up a new theoretical premise. Yet, Whitehead's primary objective, to "tease apart the multiple causal strands that …weave together into the complex of [Seltaman] food restrictions" (p.267) is not unlike Geertz's Weberian "webs of significance" (1973:5) and ethnographic analysis as a sorting out of "a multiplicity of complex conceptual structures, many of them superimposed upon or knotted into one another, which are at once strange, irregular, and inexplicit and which [the anthropologist]…must contrive somehow first to grasp then to render" (p.10). Geertz is quite clear that ethnography is "not an experimental science in search of law" (p.5). In his more recent work, "Culture, Mind, Brain/Brain, Mind, Culture," Geertz is much more generous in his discussion of the state of play (2000:207) in anthropological theory and method than Whitehead is here. He is also particularly clear that

> the way toward an improved understanding of the sociological, the psychological, and the sociocultural is not through arranging them into some sort of chain-of-being hierarchy stretching from the physical and biological to the social and semiotic, each level emergent from and dependent upon (and, with luck, reducible to) the one beneath it. Nor is it through treating them as discontinuous, sovereign realities, enclosed, stand-alone domains externally connected ("interfaced," as the jargon has it) to one another by vague and adventitious forces, factors, quantities, and causes. Constitutive of one another, reciprocally constructive, they must be treated as such—as complements, not levels; aspects, not entities, landscapes, not realms (2000:206-207).

I don't see Whitehead's "average cultural anthropology professor" (p.2), or the unsophisticated anthropologist who has not eschewed "plain old billiard ball causality" (p.5), or anyone engaged in "self-righteous shrinking" from "areas of enquiry that smack of biology" (p.23) in Geertz's 1973 or 2000 publications

Whitehead's discussion, which has so much potential, leaves much to be desired as an introductory treatise to her theory and method. The whole section could have been left out, presented as a publication on its own, presented straight up as the perspective informing the analysis (without the disparaging comments and straw men) or discussed within the concluding section after the reader has worked through the ethnographic analysis. The author's thesis is presented clearly in the Conclusions as "multiple causal strands…[that] weave together into the complex of food restrictions that an

anthropologist would label 'the Seltaman food taboo system'" (p.267). The whole of the second part of the book is focussed on the ethnography of Seltaman food complexes, and is much more rewarding (than the preceding theoretical excursions) in the richness of the data presented and the demonstration of "clusters of dynamically interacting factors" (p.24) and their various outcomes in Seltaman society. The analysis is sustained and complicated and, by its very theoretical premises, not easy to parse.

Anyone who has worked in self-sufficient, subsistence-based Melanesian societies is aware of the importance of food beyond its sustenance value and the analysis of Seltaman foodways adds many more facets and dimensions to an anthropology of food. The analysis of food taboos, the "rules governing what category of person must abstain from what category of food" (p.40), is particularly enriching. Food rules are demonstrably the outcomes of three clusters of dynamically inter-relating, mutually causative forces. The first cluster includes the availability of game meat, the psychology of habituation and disgust formation processes, and the social comfort or shareability of the food. Another cluster includes hunting technology and pragmatics, such as motivations for hunting certain species and not others, factors "to some degree hard to assess," such as those "constituent to food taboos [that] are causally impacted…by the food taboos themselves" (p.25). A third cluster includes availability of game meat plus "social reflexes and expectations regarding meat sharing" plus considerations of social space which affect eating and consumption generally in terms of privacy. Clearly this is not a food "system" in the "classic anthropological use of that term, a smoothly integrated set of mutually adjusted parts that change in coordination with one another and are informed by an overriding unitary logic" (p.57). What we have here is a food "complex," that is, an "evolving, shifting weave of tendencies that depending on the historical specifics of the larger contexts in which they are embedded, may unequally express themselves … and even at points come into contradiction" (p.256). Whitehead does an incredible job of presenting these intricately complicated complexes despite the linearity imposed by the narrative medium.

Food sharing is a critical component of food taboos (p.101) and Whitehead's re-appraisal of the "resource management" explanation of food taboos is spot on. It does not seem to serve much purpose to talk about tabooing scarce meat resources so as to discourage the hunter from pursuing a creature with a "low pay-off" in terms of both calories and social sharing. Rather than a causal arrow in one direction, food taboo → resource management, Whitehead takes a much more matter-of-factly satisfying, yet circular explanatory stance: why hunt what is taboo and therefore nonedible; thus, hunters go after creatures that they can eat and leave alone those they cannot. Of course, this is another chicken and egg issue (see p.110), but obsession with such ontogenetic concerns have been rightly critiqued as being unknowable, unverifiable and, one might add, uninteresting. What is interesting here, given that the vast majority of organic material is edible, is the complex model of interactants swirling around food taboos, hunting, male cults, social sharing, social groups, age and sex to name but a few ingredients, that classify whole categories of organic, potentially edible substances, as nonedible.

The complexity of various interactants in the realm of food taboos is especially salient and well described in the discussion of 'disgust' as it pertains to the consumption of dogs and eagles, and variables such as edibility, availability and—like it or not—symbolic value. Adult food disgusts are part of the food taboo phenomenon. Foods habitually avoided because tabooed become dishabituated when the taboo is lifted as part of the ordeal of male life cycle rituals that facilitate entry into a new life phase and its concomitant food rule prerogatives—the relieving of a food taboo. Dishabituation is accompanied by disgust reactions, such as gagging and vomiting. However, ritual "*…participants prevail over their personal revulsions and eat the newly permitted substance anyway"* (original emphasis, 87). This leads to a stimulating discussion in Chapter 5 of "zones of edibility" and how the close-to-self nonedible category such as dogs and other humans, is confounded when ritual initiands to warriorhood gag down bits of "disgusting" dog meat rather than ingesting what might seem the more symbolically appropriate flesh of the harpy eagle. Whitehead presents Seltaman descriptors of the eagle as a fierce, successful carnivore which inspires war chants and dances, its talons often included in a ritual elder's sacred bundle; yet, its flesh is restricted to very senior men, who are hardly warrior material, at the apex of the initiation hierarchy. Why is this the case? Because the harpy eagle is not readily available, whereas the

disgusting flesh of domestic dogs is. In other words, the harpy eagle, rare, difficult to capture and of a size that makes it insufficiently shareable, is a marginal food category which, when it is captured, is reserved for another marginalized category, the few very old men. Harpy eagles are not eaten by young men becoming warriors as this would make them symbolically so fierce and volatile that they would turn into "homicidal maniac[s]" (p.105). On the other hand it is old men, those beyond their warriorhood days, who eat eagle flesh and young men who eat the dis-gustatory flesh of the domestic dog as a means to ritually ingest warrior-like fierceness. In her zeal to deny any quarter to anything vaguely 'culturalist,' Whitehead argues that "all the symbolic potential embodied in the harpy eagle is recognized [by the Seltaman], but at the same time is wasted…" (p.105).

If the symbolism of the harpy eagle is recognized by the Seltaman (rather than a construct of the analyst), perhaps it is not 'wasted' at all, since it is precisely these elders who might need a recharging of their masculine fierceness, or whose masculinity is such as to sustain the fierceness of the ingested eagle flesh without injury, and whose authority over the younger generation is enhanced by eating the symbolically potent harpy eagle. Whitehead argues that it is not the symbolism of the eagle that is important here, it is the scarcity of the creature that makes it suitable for elders, but not young warriors, to eat. This is the scarcity argument. Eagles are relegated to and mark the boundaries of small social categories, the elders. Indeed all "off" meats—'off' in the sense of rotting or putrid and 'off' in the sense of not being on the menu because they are scarce, inconvenient or dangerous to hunt, thus not usefully employed for collective feasts—are taboo (forbidden) to all but this small group, the elders. We are not told why the young warriors must choke down dog flesh except that, despite being habituated as disgusting, this flesh is more readily available than eagle flesh. Perhaps it is also the case that young warriors need to be reminded that, in their role as warriors who kill those who are close-to-self, they too are symbolic eaters/consumers of human flesh (another food taboo). Both dog and human flesh are taboo because they are in the too close-to-self zone of edibility. Yet, it makes a kind of *symbolic* sense that young aspiring warriors must choke down the human-excrement-eating, house-dwelling creature, the dog—an ambiguous extension of the social self—before consuming the life of another human being, who is by definition, an unambiguous extension of the self as human. Both the domestic dog and the enemy human are "otherized", made not "us" but "them" despite being members of the same community and inhabiting the close-to-self zone. Cannibalism is a radical othering making it acceptable to kill and, in some instances without qualm, to eat the other-self (p.114). Perhaps the symbolism, rather than being wasted, could be construed as another interactant in this particular cluster such that, in readiness to hunt and kill an enemy, young warriors choke down repulsive close-to-self dog flesh, while elders ingest the flesh of the harpy eagle so as to revivify their virility and their 'stomach' for the ways of warriors.

Despite another stab at the straw man (p.177, 180), the discussion of gender relations, gender inequalities and the concept of ancestral authority is really quite elegant in capturing the complicated nature of gender concepts, gender relations and the hoary question of female subordination. According to the food taboo trajectory, women, children and old men, foods are a life-long category for women: women always and only eat foods that are designated as women's foods. Males, on the other hand, abnegate the category of foods identified as woman-child-old man in the process of achieving manhood and sexual beauty. Thus, when embarking upon the cultic male initiation rites, boys abnegate certain meats that, heretofore as children, they had consumed. By tabooing certain foods and, when captured, handing them over to women and children, men are perceived as generous, and generosity is conceived of as an essential masculine attribute, a characteristic that becomes synonymous with manhood. Women never have an opportunity to develop the socially valued attribute of prestige because women are excluded from the cultic activities through which 'manhood' and the characteristics associated with manhood, viz. generosity and its concomitant, moral authority, are attained. Women and girls, "by dint of being anatomically female" (p.161) are excluded from the initiatory structures of the ancestral cult which earn socially valued attributes for men. Women thus become even further distanced from access to generosity, prestige and authority, because they are "structurally stingy," since hunting practice and game

eating taboos relegate small creatures to women and children, game insufficient in size to share with the larger group. Females are "essentially" ungenerous thus unsuited to the mantle of authority (p.176).

While that might be the official party line, what do the women think about these "male exclusionary practices" (p.203)? Apparently, not much. While women may resist men's authority in terms of marriages, wife abuse, marital infidelities and the like, they are "amazingly accepting of male authority in most of the areas where it is exercised. Male ascendancy is accepted with a sort of pridefulness" (p.216). Seltaman women "ceded" social power to men because men are strong and women are not. 'Ceded' is a poor choice of words inasmuch as "to cede" implies that something possessed is given over to another, either freely or under coercion. Seltaman women simply have no power and authority to cede to men since the cultural meaning attributed to their sex (their gender) excludes them from access to the culturally valued means for achieving prestige and authority. Only men can cede prestige, power and authority to women by permitting the anatomically female access to the power of ancestors through the men's ancestor cults.

Women's objections to the male cults are "fairly narrow" complaints of inconvenience and reduced access to trails and thus gardens. Their exclusion is not an issue: no woman would want to have to do what the men do. Besides, women perceive of the men's cult and men's secrets as effectively channelling ancestral powers for the benefit of the total community (p.217). Seltaman women appear to accept male authority rather than grant it to them, since structurally there is no alternative to that authority, except perhaps death—socially or actually. Men are not likely to cede power and authority to women. Much of men's explanation for their secrecy hinges on reasons to avoid "status" death: the women would doubt, they would laugh, they would want a share, they would take control…whatever the explanation, in the final analysis the men would experience a 'status death.' And the men are probably quite correct. Tuzin (1997) has already shown what transpires when the traditional ideas and rules are 'delegitimized' and set aside. With the death of the Tamberan everything changed and Ilahita men did indeed lose their status.

What Seltaman women do or do not know is inadequately dealt with here. Whitehead concedes that she became alienated from the women because they constantly deferred to the men, hence made poor informants, or because they were indifferent to her (p.225). By banning children, disruptive of the ethnographic enterprise, women also stayed away because "[w]oman-space is ipso facto child-space;" thus, Whitehead became "more and more thoroughly identified with the men, with male socializing and with male secrets" (p.226). Consequently, "when an opportune moment arose and [she] tried to ask a close women [sic] friend to speculate on the nature of men's activities in the culthouse, [the woman] would suddenly grow coy and shake a finger at [her] naughtiness….[for she] was a 'spy' for the men" (217-218). This is a most telling observation. Of course, the women are not going to divulge what (if anything) they happen to know about men's affairs to someone who is gendered man/masculine regardless of that individual's female anatomy. Since male secrecy is upheld by force consequent on male unity, it is neither safe nor comfortable for a Seltaman woman to know these things, let alone tell others she knows. Perhaps Seltaman women see themselves playing an important role in their society and in support of the men's dealings with the ancestors as Kwaio women do (cf. Keesing 1985, 1987). Seltaman women do seem to see the men's cult as positive for them and the community since it is occupied with ancestral powers that ensure the weather, the crops, pigs and children; indeed, women participate in those cultic occasions by raising pigs and produce and children for the support of various initiatory and other men's cult sacred events. Whitehead goes so far as to blame the women for their relative powerlessness when she states that by "neglecting to intrude…[the women] let form up around them a larger male system over which they have lost control and which turns and controls them" (p.225).

This book disappoints in two important theoretical issues. First, I am dismayed at the manner in which the author labels contemporary cultural anthropology and anthropologists as "theoretically incoherent" (p.22) and in possession of "unwieldy models that degenerate into speculation all around their edges" in order to present a model that would appeal to those who have "a more rigorous turn of mind" (p.23). Second, the presentation of this "more restricted model" drawing on dynamical systems

theory and modularity of mind domains is not clearly laid out. Whitehead admits she did not conduct research "with these new trends in mind and [her research] is not well suited to illustrating them in a rigorous way" (p.1). And this raises a methodological question about how one would go about conducting research according to these trends and concepts? Finally, although the exigencies of field work result in all sorts of interesting dynamical outcomes, I am dismayed at the exclusion of women's voices and concerns and an androcentric analysis of Seltaman culture and society. Here, however much Whitehead decries the ethnographer's marriage to causal culturalism, she finds it difficult to avoid in her own analysis of gendered meanings and cultural consequences. The ethnographic insights into the Seltaman culture of food, hunting and food taboos are incredibly rich in detail and presented in a way that captures the complexities of Seltaman culture. Challenging theoretically and ethnographically, this work makes a major contribution to the discipline and I look forward to the development of the ideas contained within it.

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Food Rules: Hunting, Sharing, and Tabooing Game in Papua New Guinea
(Harriet Whitehead,University of Michigan Press, 2000)

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[Journal pages **105-111**]

Whitehead's work is a refreshing combination of fine-grained ethnography and ambitious efforts at analysis, theory-building, and agenda-setting. To the extent that I am equipped to evaluate them, her ethnographic observations both ring true and demonstrate remarkable breadth in the range of ideas and behaviors attended to. My comments will focus primarily on Whitehead's theoretical approaches.

Whitehead situates her analysis of Seltaman food taboos in the larger anthropological landscape by beginning with an attack on the perspective, long a foundation for much of cultural anthropology, that cultural phenomena should be analyzed in and of themselves. Arguing for "the essential vacuity of the truism that something in human life is culturally constituted" (21), Whitehead proposes two radical shifts in scale as a means of understanding beliefs and practices. In the first shift, she suggests that, because ideas only exist as a consequence of the minds that create, hold, and transmit them, the structure of those minds becomes a fundamental determinant of both the content and the organization of cultural information. In contrast to such psychological determinacy, cultural anthropologists have naively treated the mind as a homogeneous vessel to be filled by culture. Drawing on a variety of contemporary findings, Whitehead joins a growing school in arguing that the mind is composed of an assortment of domain-specific mechanisms. The impression that cultural beliefs constitute an integrated, lawlike structure is therefore likely to be erroneous, since any given domain of shared information will be shaped by the discrete psychological mechanisms that produce and process that information. In the second shift of scale, Whitehead draws on dynamical systems theory in again attacking the notion that beliefs and practices are subject to analysis in their own terms. Whitehead argues that the beliefs and practices evident at any one point in time are the product of a variety of complex events the particularistic details of which, especially early in the causal chain, can have an enormous impact on resulting patterns.

I enthusiastically endorse Whitehead's goal of combating cultural anthropologists' myopic one-level-of-analysis approach, and have elsewhere argued for the systematic shifting of analytic levels as a core investigative heuristic for anthropologists (Fessler 1996). Furthermore, I strongly concur with Whitehead's position that a modular evolutionary approach to the mind should play a key role in the analysis of shared beliefs and practices. With regard to her second theoretical argument, I also concur that historicity is of great importance in understanding the particulars of any given time and place and, moreover, that complex causal chains, possibly involving emergent phenomena, may lie behind many observable beliefs and practices. That said, however, I am skeptical as to the value of a dynamical systems approach to the analysis of culture. My skepticism stems not from doubts regarding the sensitive dependence of beliefs and practices both on the initial conditions that gave rise to them and on the multiplicity of past and present events that buffet them. Rather, it seems to me that, once one has agreed that the world is complex and full of contingencies, the dynamical systems approach has little more to offer. It provides no guidelines for dissecting the phenomenon of interest into component parts, nor does it offer any hint as to the temporal, social, geographical or ideational boundaries within which events and ideas should be considered potentially relevant to our understanding of that phenomenon. The dynamical systems approach thus does not constitute a coherent theoretical framework within which analyses can be undertaken -- at least as presented in Whitehead's work, it is merely a call for broad-mindedness when considering questions of causality. In short, the dynamical systems approach is a style rather than a

paradigm, theory, or premise. Styles come in and out of fashion, but they do not lead to great leaps forward in knowledge.

Whitehead advocates a psychological approach to culture in which parochial shared information is explicable with reference to a panhuman psychological architecture that is both modular and the product of natural selection. Whitehead relies on this approach in exploring the relationships between disgust and meat, between disgust and taboos, and between disgust and habituation. Less explicitly, both Whitehead's explication of the relationship between commensality and social solidarity and her discussions of hierarchy hint at similar approaches. While I believe that Whitehead has likely hit on a number of important hypotheses, my principal complaint is that, perhaps as a consequence of the richness of the ethnographic presentation, these arguments do not fully live up to the agenda that she sets both for herself and for anthropology in general. If we are to flesh out the skeletal theoretical structure that is woven into Whitehead's ethnography, we must explore more closely, and expand on, existing approaches to the relationship between culture and mind.

The notion that an understanding of shared idea systems necessitates an analysis at the psychological level is not new, dating back at least to the heyday of psychological anthropology in the 1950's. However, many of the earlier attempts to achieve this linkage were handicapped by their reliance on portraits of the human mind that were, at best, inaccurate. Nevertheless, it is possible to hold aside the limitations of the theories that pioneering psychological anthropologists borrowed in order to consider the larger enterprise in which they were engaged.

The general approach of classical psychological anthropologists, exemplified by the 'culture and personality' school (cf. Benedict 1946), was to examine how beliefs and practices a) shaped experience, and b) served an expressive role. Although it is now clear that there are problems with the assumption that social groups are sufficiently homogeneous as to allow investigators to describe a 'cultural personality type,' nevertheless, these two concerns have remained central to psychological anthropology in one form or another. A fundamental issue raised by this early work, one that has continued into the present, is the question of universality versus parochiality in psychological functioning. On the one hand, if the mental apparatuses that generate experience and lead to behavior are the same in all people, then understanding events, feelings, and behaviors in a given culture is simply a matter of explicating the requisite cultural context, i.e., 'if you accept the premises of the worldview of culture X, then you can see how you would think and react in the same manner as people X.' On the other hand, if the impact of beliefs and practices is profound enough, experience and expression will be incommensurate across cultures, i.e., 'while an ethnographer can portray a culture, it is impossible to ever really put oneself in the Other's shoes.'

The polar positions described above remain starkly evident in contemporary research on human emotions. The universalist position, championed by Paul Eckman and his associates (Ekman and Friesen 1971), argues for the existence of so-called 'basic' emotions that are elicited by prototypical event sequences or stimuli (i.e., death of a family member, a rotting corpse, etc.) and expressed by discrete facial expressions (sad face, disgust face, etc.). The relativist position was stated in its polar form by Catherine Lutz (1988), who argued that the constituent elements of emotional experience could be combined in such diverse ways that the emotional world of one culture has little in common with that of another culture.

What is the relationship between psychological universalism, psychological relativism, and the claim that the mind is composed of an assortment of distinct and somewhat autonomous mechanisms, each dedicated to processing information for a demarcated type of task? Importantly, modularists are universalists, for they claim that the psychological architectures they describe are panhuman. Given that, as Whitehead shows, considerable evidence in favor of modularity exists (and is growing by the day), what are we to make of cross-cultural variation, the very gist of most cultural anthropology?

I can think of a number of ways of making sense of cultural variation from a universalist modularist perspective. First, it appears that we are not born wholly naive. For example, infants seem to have built-in expectations about the physical properties of the world (Spelke et al. 1992), and young

children seem to have some understanding of even such complex phenomena as death relatively independent of experience (Barrett and Behne 2001). It is thus plausible that some modules may contain a considerable amount of information about features of the possible environments into which their possessor will be born; various aspects of this information may be foregrounded or backgrounded in response to environmental input. Hence, combined with sensitivity to particular cues in the social and physical environments, such built-in information may be a source of patterned ideas and behaviors across members of a group -- because all humans contain the same built-in information, individuals who are exposed to similar circumstances will each autonomously generate similar ideas and responses, leading to commonalities within groups (Tooby and Cosmides 1992).

One difficulty with relying upon the above concept, termed 'evoked culture,' to explain cultural variation is that human cultures contain information that is clearly the product of cumulative experience over generations, information that is too complex, and too parochial, to be plausibly explained as the product of similar responses to a shared environment. For example, the kayaks of the traditional maritime Inuit were engineering marvels. Rather than being the evoked face of some hidden built-in body of knowledge, such sophisticated designs are best explained as the consequence of our ability to learn from our elders, to improve on their knowledge, and to pass the resulting sum on to our juniors (R. Boyd, personal communication). Moreover, this is true not just of technology, but of many aspects of culture. Nevertheless, contrary to traditional anthropological approaches to the mind, acknowledging the complexity and cumulative nature of human cultures does not necessitate a tabula rasa view of learning. Rather, our reliance on culture is explicable from a modularist perspective once it is recognized that there are discrete psychological mechanisms dedicated to acquiring socially transmitted information (cf. Fiske 2000). Perhaps the clearest example of this is the mechanism that manifests as the emotional/behavioral orientation commonly termed admiration. We admire successful individuals (skilled kayak builders, for instance). When we experience admiration we seek to imitate the actions and ideas of a successful individual, thereby increasing the likelihood that we too will succeed in the local environment; we are also motivated to be near, observe, and be in good graces with, the admired individual, all of which facilitates adaptive information-gathering (Henrich and Gil-White 2001).[1]

In rejecting functionalist explanations of taboos, Whitehead (77) remarks on "the possibility that customs spread through their capacity to attract and cohere groups of followers rather than through their capacity to biologically reproduce them." There is considerable explanatory utility in the view that the creation, perpetuation, and spread of cultural information can be understood in terms of an "epidemiology of ideas." In this perspective, an updated and highly cognitive version of the Levi-Straussian notion that some ideas are "good to think," concepts are viewed as propagating entities, the success or failure of which is contingent on their relative fit or lack thereof with specific attributes of the minds that hold them (cf. Sperber 1996; Atran 1990; Boyer 2000). Although Whitehead does not fully explore the theoretical linkages implicit in the epidemiology of ideas approach, it can nevertheless unite the two perspectives discussed earlier. Some ideas may be the relatively direct products of content-rich universal mental mechanisms. Because such mechanisms are panhuman, these ideas are likely to arise repeatedly. In turn, ideas spread in part because of the workings of relatively content-free mental mechanisms. Simply on the basis of the frequency of their genesis, ideas that arise repeatedly are likely to often be spread by such mechanisms. However, this process can be greatly augmented if actors share the same content-rich mental mechanisms, as the ideas and behaviors of others are more likely to resonate with actors' endogenous intuitions.

Because, as Whitehead reviews, meat has strong evocative power as a disgust stimulus, it is more likely that, as a consequence of idiosyncratic events, individuals will come to avoid particular meats than that they will come to avoid particular vegetables. Indeed, humans are far more likely to develop conditioned aversions to meats than to other foods (reviewed in Fessler and Navarrete n.d.). Like everyone else, high-status individuals possess the mental mechanism responsible for the special salience of meat, hence they too are more likely to develop meat-avoidance practices than they are to develop equivalent behaviors with regard to plant foods. Once this occurs, mechanisms such as that responsible

for admiration lead to widespread imitation of the idiosyncratic behaviors of high-prestige persons. Importantly, however, while mechanisms such as admiration are themselves relatively content-free, they do not operate in a vacuum. Suicide cults, for example, are noteworthy precisely because they are rare -- most people question the legitimacy of a leader who advocates mass suicide, presumably because their motivations for self-preservation outweigh their motivation to imitate a high-prestige individual. Hence, while admiration biases us toward adhering to ideas held by others, additional mental mechanisms, including content-rich ones, also influence the attractiveness of those ideas. It is highly plausible that the ideas that are most likely to spread and persist are those that are congruent with such endogenous information. Accordingly, the universal possession of a propensity to preferentially direct disgust toward animal products not only ensures that high-prestige individuals are likely to evince meat avoidances, it also helps to ensure that others are likely to imitate such avoidances.

If the above discussion seems very abstract, consider the following scenarios: You notice that Bob, a respected member of your community, avoids eating friggle meat, displaying apparent revulsion at the prospect. You also notice that Tom, another respected member of your community, avoids eating wimple greens, and displays a similar revulsion. Our predisposition to respond to animal products with disgust makes us more likely to imitate Bob than Tom -- Bob's avoidance resonates with our intuitions in a way that Tom's does not.[2]

The above discussion moves Whitehead's approach closer to her stated goal of explaining culturally-constituted dietary practices in terms of the operations of discrete psychological mechanisms. However, a number of questions remain. First, an account must be provided as to the special salience of meat as an elicitor of disgust. As Whitehead notes (p. 285), the special status of meat is consistent with the unique hazards of pathogen and parasite ingestion associated with meat-eating (cf. Haidt et al. 1997; Curtis and Biran 2001). While such dangers may be indexed by olfactory cues in cases of overt spoilage, the presence of both parasites and lower levels of bacteria are not readily detectable prior to ingestion. In contrast, phytotoxins are produced by plants as a means of defense against consumption, and hence evolution has favored highly detectable advertisements of plant toxicity. Accordingly, while vegetal hazards can be effectively coped with through a simple aversion to bitterness, the risks associated with animal protein call for a complex and highly focused avoidance learning system (Fessler 2002; Fessler and Navarrete n.d.). The double-edged sword of meat's high nutritional value and its role in disease transmission has thus apparently resulted in both an evolved attraction to meat and an evolved propensity to learn to avoid those specific meats that, under local ecological conditions, constitute a source of danger.

The above argument is consistent with Whitehead's claim of a "subterranean linkage, with phylogenetic components, running from animals-of-the-diet to adult disgust to formal food taboos," (p.90). However, the account remains incomplete since it fails to address the last link in this chain, the processes whereby widespread avoidances become institutionalized as food taboos. Why should a dietary avoidance acquire moral, cosmological, and ethnognomic significance, and how does this transformation occur? Although there are probably multiple pathways through which widely shared avoidances become taboos, one process, which I term normative moralization, seems especially important.

People exhibit a robust tendency to assign positive moral value to prevailing patterns of behavior. For example, right-handedness is far more common than left-handedness and, in many societies, the right hand (and, more generally, the right side) is associated with positive concepts and esteemed social behavior, while the left hand has the opposite connotations (Corballis 1980). It seems that people survey the social world around them, note common actions, and assign positive moral valence to them. Since multiple actors engaging in the same process will arrive at similar conclusions regarding the moral rectitude of a prevailing pattern, it is only a small step to the sorts of discussions and elaborations that transform such patterns into formally articulated rules, laying the foundation for the creation of negative sanctions. However, while plausible, this explanation still remains incomplete. In keeping with Whitehead's agenda of grounding both culture in psychology and psychology in evolution,

we must ask from whence springs the human propensity to assign positive moral weight to prevailing patterns of behavior. The answer, I believe, lies in the importance of cooperation for our species.

To an evolutionarily unprecedented degree, humans rely on socially transmitted information in coping with their physical and social environments. Some of this information takes the form of inherently useful recipes or instructions. Cultural schemas such as 'how to build a kayak,' 'how to plant corn,' or 'how to escape a charging water buffalo' bestow direct benefits on their possessors. However, employing this type of information on an individual basis is only one of the ways in which culture allows humans to adapt to, and exploit, their surroundings. In contrast to such individualistic enterprises, many of the most efficient and productive activities in which humans engage are cooperative -- knowing how to kill a rabbit is clearly useful, but knowing how to conduct a large game drive is vastly more so. Humans are unique in their ability to engage in highly variable complex cooperative activities. Such activities are wholly premised on cultural information; by sharing understandings of the goals of a given enterprise, the methods to be used, and the roles to be played by various actors, individuals can coordinate their behavior in a nearly infinite number of endeavors conducted in enormously disparate domains.

Once culturally-mediated cooperation is a possibility it behooves individuals to do two things. On the one hand, they must evaluate all potential collaborators and discriminate amongst them. Individuals will maximally reap the benefits of cooperation if they preferentially ally themselves with actors whose actions demonstrate that they share the observer's cultural templates for behavior and, furthermore, are motivated to adhere to such standards, since these individuals are likely to behave in a predictable fashion that complements others' actions during cooperative endeavors. On the other hand, because every individual is herself subject to such scrutiny, individuals who consistently and conspicuously advertise their conformity to shared standards for behavior are more likely to be selected as cooperative partners, and hence are more likely to maximally reap the benefits of cooperation. Together, these two features will have constituted a source of selective pressure shaping the human mind during our species' evolution: Natural selection will have favored individuals who were strongly motivated to a) survey local forms of behavior, b) recognize prevailing patterns, c) assign positive value to such patterns, d) affiliate with actors who behaved in a manner consistent with such patterns and avoid actors who did not, and e) conform to such patterns themselves. Hence, there are grounds for supposing that the human propensity for normative moralization constitutes a discrete psychological adaptation that evolved in response to the rewards offered by complex cooperation.

I have attempted to expand Whitehead's argument along lines consistent with her stated goal of explaining food taboos as culturally-constituted standards that are produced by, and that articulate with, minds composed of a collection of diverse mechanisms, each of which constitutes an adaptation. It is important to recognize that, consistent with the modular approach advocated by Whitehead, adaptations are discrete features, each of which was produced by natural selection in response to a distinct set of recurrent challenges that confronted ancestral hominids. Accordingly, adaptations can be relatively independent of one another. This insight explains a fundamental feature of proscriptions evident throughout Whitehead's account, namely that, from the perspective of maximizing the efficiency with which available resources are exploited, food taboos are often either irrelevant or dysfunctional. Meat is a mixed blessing, hence it often pays to possess a mechanism that singles out meat as a target of disgust and conditioned aversion learning. Attention to the behavior of prestigious individuals is often an effective means of acquiring locally useful information. And the propensity to moralize and conform to prevailing patterns of behavior often increases the benefits that can be reaped from cooperation. However, while each of these adaptations may, on average, operate efficiently, it is quite likely that, in combination, when instantiated in multiple minds simultaneously, they can produce maladaptive misfirings.

An efficient consumer must balance the costs of toxin avoidance against the costs of food acquisition -- there is no point avoiding a wide range of ingestible hazards if doing so limits the actor's diet to the point that food acquisition becomes prohibitively expensive in terms of time and calories. Accordingly, we can expect psychological mechanisms governing ingestion to operate such that dietary

disgust sensitivity will covary with current dietary adequacy. It is not difficult to find examples of, on the one hand, the reduction or even suspension of disgust sensitivity among poor or starving individuals and, on the other hand, the exaggeration of disgust sensitivity among highly prosperous persons. Because high-prestige individuals will typically have the greatest access to food resources of any member of their community, it is thus expectable that such persons will often express disgust toward a wide range of potential foods (cf. Aunger 2000). When combined with the fact that each individual possesses an idiosyncratic history that may include erroneously acquired conditioned food aversions, this means that actors who imitate the food avoidances of high prestige individuals do so at the risk of maladaptively constricting their own diets. Apparently, either this cost has been consistently low enough to prevent natural selection from refining the appropriate mechanisms, or the nature of the mechanisms is such that refinements are not possible. Either way, when combined with normative moralization these features virtually ensure that many, and perhaps most, food taboos stemming from prestige-biased transmission will be maladaptive for many of the members of a society.

The above account explains why cultures may accumulate a diverse array of bizarre taboos on animal products. However, because small-scale societies such as that of the Seltaman are typically meritocratic to some degree, burdensome taboos may disappear if, despite the initial social costs, innovators are able to prosper sufficiently by exploiting an abundant but tabooed resource. This is consistent with Whitehead's account of the manner in which availability affects taboo targeting, as there is little pressure to modify taboos on scarce animals, but considerable pressure to do so if ecological circumstances change. This is yet another example of the manner in which Whitehead's work contains vital material. I applaud the thoroughness of her ethnographic portrait, endorse much of her theoretical agenda, and urge her to systematically act on her own call for an evolutionary psychological analysis of cultural phenomenon.

Endnotes

1. Prestige-biased belief acquisition explains the adoption of both avoidances and additions to the diet, a pattern noted by Whitehead (p. 88).
2. It is likely that, in addition to domain-general culture-acquisition mechanisms such as prestige-biased transmission and conformist transmission (Boyd and Richerson 1985), domain-specific mechanisms direct attention to the diets of conspecifics -- see Fessler and Navarrete n.d.

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Food Rules: Hunting, Sharing, and Tabooing Game in Papua New Guinea
(Harriet Whitehead,University of Michigan Press, 2000)

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[Journal pages **112-117**]

Beyond Structure: Reflections on Harriet Whitehead's *Food Rules*

The last few decades have witnessed a series of profound changes in the discipline of anthropology. One of the most significant of these has been the demise of the 'culture concept.' Once comfortably ensconced as the disciplinary marker of the anthropological enterprise, the concept lies in ruins, or "near ruins" (Dirks 1998) at the turn of the twenty-first century. The spread of global capitalism coupled with our growing recognition of the contested nature of social forms, has precipitated a "crisis of confidence" concerning the appropriate focus of ethnographic inquiry. Occurring in tandem with the aforementioned development has been our growing dissatisfaction with several dualities that have traditionally guided social science thought. Immediately implicated here are the conventional distinctions between the "mind" and "body", "culture" and "biology", the "mental" and the "material", "cultural logic" and "practical reason". Over the past few years, social scientists have been striving to develop models that transcend the long standing dichotomy between the "world as constructed" and the "world as given" (see, for example, Ingold 2000; Bloch 1992; Csordas 1994; Yanagisako & Delaney 1995; M. Strathern 1992; A. Strathern 1996).

Harriet Whitehead's book *Food Rules: Hunting, Sharing and Tabooing Game in Papua New Guinea* is intended as a contribution in this direction. The book takes on one of anthropology's most celebrated topics – the interpretation of food taboos. Positioning herself in opposition to classic theorists in the field (i.e., Claude Levi-Strauss 1966, 1949; Mary Douglas 1966; Edmund Leach 1964; and Stanley J. Tambiah 1969), Whitehead argues that orthodox interpretations of eating behavior have erred in their tendency to focus almost exclusively upon ideology – a problem which she claims has marred anthropological interpretations of social life more generally:

> ... A great many cultural anthropologists persist in thinking that there is a generic realm, culture, which can be isolated from other hypothetical realms ... and assigned a sort of causal superiority. If not determinative in a direct push-button kind of way, culture is said nonetheless to be "determinative in the final instance." Frequently ... this superior causal realm is presented as being an organized system ... My argument here is that there is simply no such realm, nor such a system, as the posited culture and that the forces that go into the constitution of human life-ways and human knowledge cannot be pigeonholed into realms at all (Whitehead 2000:5).

Whitehead's aim in this book is to demonstrate the utility of developing a more multi-causal framework. Drawing upon "dynamical systems theory" and the "modularity of mind" approach in psychology, she argues that dietary restrictions are best seen as the unstable coalition of diverse forces including social, psychological, and ecological factors.

Whitehead's book is based upon research that was carried out with the Seltaman – a highland New Guinea people who inhabit the heavily forested interior of Western Province. Linguistically and culturally the Seltaman belong to the Mountain Ok tradition – a region well-known to scholars of Melanesia through published accounts that describe their complicated systems of male initiation (Barth 1975, 1987; Jorgensen 1981; Poole 1976, 1981; Robbins 1995). Yet, if the Mountain Ok people have

achieved a certain measure of ethnographic fame owing to their colorful rituals and elaborate cosmologies, Whitehead plans to take a different tack in this work. Instead of focusing on those meanings that underlie Seltaman food prohibitions, she hopes to grapple with a different set of questions. Stated simply, her main thesis is that Seltaman food taboos perform a "strategic allocation" of game by matching the size of an animal to an appropriately sized congregation of feasters (Whitehead 2000:ix). Seltaman sharing units range in size from the small to the large and include the single family, the extended family, the hunting camp, the village, co-residents of a garden site, and the people of the men's house. Creatures that are small in size (or infrequently hunted) are likely to be subject to numerous interdictions. Chances are good that these items will be routed to the smaller social groups, such as a domestic unit, consisting of a mother and her children. Larger animals (or those that are more commonly found) will be subject to fewer interdictions insomuch as they can be more readily distributed among a broader segment of the community.

If Whitehead's argument has a ring of practicality about it, this is not the entire picture. Throughout the book, the author is at pains to demonstrate that Seltaman eating behavior plays an important role in legitimating a system of sexual inequality. When a boy first enters the men's house (and for many years thereafter) the flesh of several species becomes taboo to him. Although most of these taboos will be repealed later in life, during the time that he follows them, he demonstrates to other members of the community his growing strength and responsibility. The initiate's redeployment of this game to women and children helps to underscore Seltaman ideas that men are the generous and beneficent provisioners of hunted game to women. Men come to be known for their bountifulness and largesse – traits that elevate them in the eyes of the community. Thus, "men are not only the main providers of the meats, but they exercise a specific discipline in regard to consumption. The social effect of this instituted male open-handedness ... is a heightened respect for male authority" (p. 175).

Having invoked prestige and the shareability of game as the twin principles that undergird Seltaman eating behaviors, Whitehead makes an unexpected move during the second half of her book – she disassembles the cornerstones of her interpretive apparatus. In the later chapters of the text, we learn more about the men's cult, including the practice of sponsoring secret ancestral feasts. As the author describes these events, Seltaman men, unbeknownst to women, frequently undertake secret hunts in the bush in which the accumulated game is brought back to the men's house and is shared by all ritually eligible males. The purpose behind these feasts is to secure the blessing of the ancestors (a benediction realized in the form of good crops, fine pigs, and the successful completion of future hunts) by offering ancestors a portion of meat in the form of a sacrifice. The sense of "brotherhood" that is produced through the consumption of a shared meal is enhanced by men's guarding of a ritual secret: much of the game that is consumed on these occasions *should* by virtue of the allocational principle be shared with women and children of the community:

> The men are eating *nuk* (i.e., regular game animals). Not special kinds of *nuk*, just *nuk*. They are eating the very *nuk* that on other occasions women and children eat. Were this revelation to occur, it would not be long, I suspect, before what looks at first puzzling begins to look at last offensive (p. 223).

In the final analysis, then, a contradiction exists at the heart of Seltaman eating behavior. Items that can and should be channeled to women by virtue of their size or availability are, instead, consumed by men in secret feasts. Seltaman men contravene their own canons of reciprocity in the interest of building a larger male community – a unity that will make them a more effective force in inter-community relations, including the practice of warfare.

One of the most refreshing aspects of Whitehead's work is her recognition that social life can be sloppy and messy around the edges. In keeping with the post-modern turn, the author points out that in the past, anthropology has suffered from both over systematization and an untoward reliance upon treating culture as an integrated system of ideas. Orthodox analyses of taboo, for instance, approached

the subject from one of two directions: either they have focused on discerning an "underlying structure" to the system (cf. Leach 1964; Tambiah 1969; Douglas 1966; Levi-Strauss 1966), or have concentrated on unpacking the meaningfulness of dietary interdictions (Meigs 1984): "Why is it that category x of person cannot eat category y of food? What is it that x and y have in common?" Whitehead points out that although the Seltaman were highly focused on food, they were unwilling / incapable of offering up exegetical accounts that grounded their eating behaviors in a discussion of symbols and meanings. To ask someone: "Why is it that rat is taboo to young boys" will likely elicit little more than a non-committal shrug of the shoulders and the bland statement, "It is our custom." Many readers will feel a sense of sympathy with Whitehead's plea that the time has come to develop new theoretical frameworks that avoid assigning culture an autonomous and explanatory efficacy.

Notwithstanding this important contribution, Whitehead's analysis suffers from an important flaw that ultimately undercuts the interpretative power of the framework that she seeks to develop. Implicit throughout her text (although never discussed in these terms) is the very traditional distinction between "culture" (as a system of ideas) and "society" (defined as an aggregate of social group identities). Whitehead's critique of theory is directed exclusively towards the former. As she describes her aims:

> Aren't systems supposed to be governed by uniform and internally consistent principles? ... [The] understanding that I wish to communicate in the current work is that the over-systematizing in anthropology of cultural phenomena has in fact obscured the nature of many of these phenomena. The food taboos of the Seltaman and their neighbors are best viewed not as tight systems but as an evolving, shifting weave of tendencies that depending on the historical specifics of the larger contexts in which they are embedded, may unequally express themselves, first in one way then the other (Whitehead 2000: 256).

While Whitehead is quite willing to challenge the idea that "culture" exists as an overarching set of ideas, she does not subject the notion of "society" to the same measure of scrutiny. Throughout her work, Seltaman "society" is taken as a given. Indeed, the entire "allocational principle" upon which her analysis is based, rests upon the idea that "society" exists as a set of ready-made social distinctions. Different types of game get funneled to different social units including "villages", "families", "males", "females", regional associations, etc. What is lacking here is any sense that taboo can also have a *constituent* component –*more than mapping on to already existing groups dietary interdictions may help to elicit the distinctions upon which social life is based*.

Whitehead's neglect of this point is somewhat surprising, particularly given the ethnographic region within which she has conducted her research. From the late 1950s onward, when anthropologists first began to carry out fieldwork in the newly contacted highlands of Papua New Guinea, the search has been on for a theoretical paradigm that could adequately account for the forms of sociality they confronted. Unlike their colleagues working in Africa and elsewhere, ethnographers of Papua New Guinea have always been hard-pressed to identify anything resembling stable social groups. Described in terms of its "looseness" (Pouwer 1960; Watson 1965); "plasticity" (Kaberry 1967), and interminable "fluidity", Melanesian sociality appears to fly in the face of everything that Westerners hold dear to their conception of an ordered "society. The difficulties which have been faced by ethnographers were described early on by J. A. Barnes (1962) in a now classic article where he documents the inadequacy of descent based models for this part of the world. New Guineans, in short, appear to be far less interested in building a "society" than they are in creating those distinctions which make particular kinds of social relations possible (Wagner 1975; M. Strathern 1988).

Within this context, food – both its sharing and interdiction – has been shown to play a significant role in the ongoing elicitation of Melanesian sociality. In one of the earliest accounts of highland social life, Wagner (1967) documented how Daribi clans and lineages were created not on the basis of descent based principles, but rather through the exchange and sharing of salient food items. In a like manner, A. Strathern (1973) demonstrated early on that a Kawelka man can change his group

affiliation by continually consuming food that has been grown on another clan's land. Similar results have been reported elsewhere in the region (see, for example, LiPuma 1988; Clay 1982; Counts & Counts 1983; Schieffelin 1976) (1). However, it is important to note that food practices do more than elicit "kinship" relations. In my own work with the Kamea (1998), I found that gendered agency is brought about in large measure through eating behavior. A male body is created through years of meticulous adherence to food taboos; a female one by eating many of these same interdicted items in copious quantities. This more creative / constituent component of food rules is often hinted at in Whitehead's book, but never fully developed (2). In Whitehead's book, food rules are superimposed upon social groups but play no role in their constitution.

Having implicitly drawn a distinction between "society" and "culture", Whitehead is at pains to put them back together again. Throughout her text, it is often difficult to discern where Whitehead ends and the Seltaman begin. There is a tendency to conflate the interpretative framework of the author with those ideas and beliefs that are held by the Seltaman themselves. Take, for example, Whitehead's examination of the men's cult. As noted previously, the author contends that a fundamental contradiction exists at the heart of Seltaman society: items of food that can and should be shared with women are consumed, instead exclusively on the sly by men. Male solidarity is said to rest on men's collective agreement to keep this "secret" in common. Yet, Whitehead's tendency to downplay the significance of indigenous exegesis makes it difficult for the reader to assess the validity of this argument. So far as I can tell, the "allocational principle" is a product of Whitehead's interpretative framework. No Seltaman man or woman ever said to her "We attempt to match the size of animals to the size of social groups." (Indeed, the very fact that men hold ancestral feasts to begin with seems to call this principle into question). But, if the "allocational principle" is, in fact, a product of Whitehead's analysis, then it is difficult to know how men could experience a sense of shame at having violated it. In effect, they would be transgressing a principle that they don't know they have (3).

As a contribution to the development of new approaches to taboo, *Food Rules* is ambiguously situated somewhere in the gap between modernist and post-modernist modes of interpretation. On the one hand, the author advocates moving beyond the Durkheimian assumption of culture as a coherent or overarching set of beliefs to a view that emphasizes its contradictory, conflicting, and partial nature. On the other hand, she accepts a view of the world in which social distinctions (gender, kinship, ethnicity, and the like) are seen to be grounded in "the nature of things." Given this predilection, it is not surprising that Whitehead's book has an oddly out-dated feel about it, despite its attempt to weigh in on contemporary debates. The author's approach to gender is a case in point. Instead of emphasizing the multiple and conflicting nature of gender ideologies (J. Butler 1990; M. Strathern 1988), Whitehead asserts that sexual asymmetry has a biological basis, a position that was roundly critiqued over twenty years ago (4). Throughout her work she takes fellow-Melanesianist, Raymond Kelly (1994) to task on the grounds that he understands Etoro gender hierarchies as being based on indigenous cosmologies. Whitehead claims that her own position has the advantage of integrating "cultural" factors with "biological" ones. However, nowhere is it recognized that "biology" is itself, a system of ideas (Lewontin 1992; Haraway 1989) - that it has been constituted through a set of metaphorical borrowings that were based upon nineteenth century understandings of "human kinship" (M. Strathern 1992). The "mental" and the "material" are reinvented in Whitehead's text, as polar opposites rather than newly integrated bedfellows.

In summary, although I am highly sympathetic to Whitehead's aims, I do not think she takes her analysis as far as she might, particularly if the intent is to build a truly synthetic and multi-stranded approach to social life. A framework that weaves together social, psychological, material and ecological variables is certainly a first step in this direction. So too is Whitehead's insistence that such an approach is never stable but must be viewed as the ever-changing product of contradictory and jostling forces. Yet, I would argue that it is not enough to examine how all of the aforementioned factors fit together to sculpt the overall tenor of Seltaman eating behavior. It is also necessary to examine how Seltaman food rules operate to create the distinctions upon which social life is based.

Several post-structuralist interpretations of food taboos have pointed in this direction. Lambek's (1992) work among Malagasy-speakers of Mayotte and Robbins' (1995) work with the Urapmin (a neighboring Mountain Ok group) have both demonstrated the utility of adopting a more dialectical approach. In each case, we become acquainted with the extent to which bodies, food, history, ecology, eating, and social identity are mutually constituitive aspects of a "social whole". In the end, and despite its very much-coveted aims, *Food Rules* reproduces many of those distinctions that it sets out to critique.

Endnotes

(1) Carsten (1995) and Weismantel (1995) have written about similar practices outside of Melanesia.
(2) Her discussion of Jermone, a Seltaman youth who failed to mature as a consequence of taboo violations is a case in point.
(3) I do not mean to intimate that only consciously articulated principles should form the basis of ethnographic analysis. It has been common practice in anthropology for years to elucidate "principles" (i.e., "cultural logics") that are not explicitly verbalized by the people with whom we work. This is part of the work of interpretation. Still, the allocational principle strikes me as being of a different kind in that it is not consistently followed by the people to whom it is attributed. Without further recourse to indigenous statements, the end result is unconvincing.
(4) Ironically, it was Whitehead's own co-edited volume (Ortner & Whitehead 1981) which played a major role in deconstructing the biological basis of gender.

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Food Rules: Hunting, Sharing, and Tabooing Game in Papua New Guinea
(Harriet Whitehead,University of Michigan Press, 2000)

Reviewed by ***Patricia K. Townsend***
(University of Buffalo, NY)
[Journal pages **118-119**]

The two-word title "Food Rules" contains a double irony. In a New Guinea culture area where the generic word for food is 'taro,' *imen,* the book restricts itself to meat. And in the debate in cognitive science between rule-based symbolic-computational theories and habit-based associative-connectionist theories that frames this book like a pair of bookends, the author comes down on the side of habits rather than rules. The title "Meat Habits" would have been more accurate, if not as catchy.

The meat habits that Whitehead describes are familiar from my own fieldwork among people in the Upper Sepik who do not belong to the Mt. Ok language family but who share with them a congeries of food taboos that play out as variations on the themes that Whitehead describes for the Seltaman. For example, the "gendering" of a marsupial to provide a shared meal for women only or men only by the Seltaman is akin to the Saniyo treatment of red oil pandanus. The process of acquiring food aversions that Whitehead describes fits the Saniyo disgust at earthworms or the experience of the minority of people who cannot eat sago cooked with boiling water without gagging. The manipulation and interplay of gender, age, and initiation status in the meat habits of old men are familiar. Apparently missing from Seltaman culture are the food taboos imposed on Saniyo widows and severe sanctions for their violation. In short, I delighted in the opportunity for comparison provided by the rich description in this work, though often I wanted more: more about vegetable foods, more about cooking, more about the etiquette of eating.

Reading *Food Rules* took me back to 1968, when I struggled to make sense of Saniyo food taboos for a section of my dissertation, seeking to find a single key that would unlock the system. My informants resisted providing me with *ad hoc* explanations of the allocation of various species of marsupials, reptiles, and birds to be consumed by men only or women only. My analysis then, like Whitehead's now, stressed the function of these taboos in encouraging the wider sharing of meat. Lacking the field guides to birds and mammals that would later be published, I was unable to identify many of the species. When I shared my frustration at the lack of elegance of my analysis with my advisor, Roy Rappaport, he conspired with me in my functionalism, simply suggesting that I add the sentence, "It is only because there are so many varieties of small game animals, including birds and reptiles, which are more or less equally valuable as food resources, that it is possible to elaborate these domains to such an extent for social purposes." At the examination the co-chair Marshall Sahlins muttered, "There's a lot of easy functionalism in this dissertation," drawing Skip to my defense. The content of their subsequent argument is lost to my memory, though not lost to memory is my relief at not having to do the defending myself. I revisited the Saniyo food taboos in an even more stubbornly neo-functionalist paper, "Food Taboos, Solidarity, and Antagonism," for the Southwestern Anthropology Association meetings in 1974, again deciding that the material was too messy to be publishable.

Having twice failed to satisfy myself that my material was worthy of publication as a journal article, I can hardly fail to admire Whitehead for successfully pulling off a book based on similar material. What has changed in these thirty years? The publication of important work by Flannery, Bulmer, Hyndman, Dwyer, and others on New Guinea animals was of course helpful to other fieldworkers who stand on their shoulders. The accumulated production of ethnographies also allows testing hypotheses in comparable New Guinea societies, as Whitehead does with Kelly's Etoro and Morren's Miyanmin material. Most importantly, as she indicates, the change in theoretical climate

accommodates a messiness, a tangled web of causalities, that we once considered unacceptable.

Even so, who but another Melanesian ethnographer would think that we needed a book on food taboos among the Seltaman, a society of some 200 people living in two small villages? There are several strategies that we anthropologists use to establish the relevance of our exotic subject matter. Whitehead uses two of them prominently. First, she brackets her material between a heady theoretical introduction and conclusion that draw on recent work in cognitive science. This is work that has not yet significantly influenced anthropology (nor does it seem integral to her analysis). Second, she uses what we could call the Mead strategy, familiarizing the exotic by exoticizing the familiar, informally comparing Seltaman food rules with our own, particularly the Southern regional meat habits of her childhood. A third strategy for asserting relevance, which she does not use, to the detriment of the book, is to examine the ways that Seltaman are enmeshed in processes of globalization. While Whitehead does not conceal the Seltaman proximity to a huge multinational mine in full production and an active Christian evangelistic movement of *Rebaibel* she seems reluctant to divulge any details about Seltaman responses and resistance to these changes. At least a brief treatment of these topics would have increased the interest of the book, and the space to provide it could have been made by disciplined editing of the rambling text.

With regard to questions of space, the publisher's decision to place the three appendices on its web site rather than printing them is also bound to stimulate reflection on the future of ethnographic publication. The material on the web site about hunting returns and animal identification is essential to understanding and evaluating the argument. The data there would be of value to a fieldworker carrying the book to the bush to collect comparable material. So why not publish the hard data in hard copy and put the theoretical argument, likely to be soon superseded, on the Internet, where it could easily be debated? These are perhaps dangerous questions to ask.

Whitehead was poorly served by reviewers and copy editors. There are relatively few outright errors ('Principle' is consistently misused for 'principal,' the adjective) but many stylistic infelicities and awkward neologisms ('stingeing,' p. 139, and 'officialization,' p. 268). A careful reviewer would have demanded at least a map and possibly also village layouts and more and earlier information about settlement history. This is the background that is critical to evaluating her argument that taboos are related to the size of food-sharing units. A reviewer thinking about using the book with anthropology students would have insisted that she tell us more about Seltaman culture in her introduction than she tells us about her boredom with it (p.37). We need to be told more about the field methods and language competence of the fieldworker and her informants of differing ages and gender.

The most glaring defect of *Food Rules* is its idiosyncratic pattern of citing the relevant scholarly literature. Where the author does not choose to pursue other lines of thinking about food taboos, she at least owes her readers a fairer start on doing so. She dismisses optimal foraging theory with reference to a single biology text, not referencing the important anthropological literature on food taboos and optimal foraging theory in Amazonia. She dismisses the nutritional consequences for infant development of pregnancy food taboos with a single back-handed reference to a 1977 study in Guatemala ("if nutritional research on this point is to be believed," p. 77). If one reference were all that could be afforded, a current review article on child nutrition would be more appropriate. In her New Guinea comparisons, she inexplicably cites her summary notes on 30 pages of Jorgensen's unpublished Telefolmin dissertation (p. 233) while ignoring an accessible publication on the terrestrial cuscus (Jorgensen 1991) that is pivotal to her chapter on the loss of the taboo on this mammal.

All in all, it is a book of interest to both cognitive science and anthropology that could have been made much better by being reviewed rigorously *before* publication rather than only now.

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Response to Reviews of ***Food Rules***

Review Forum, ***Harriet Whitehead*** (Duke University)

[Journal pages **120-126**]

> In certain meterological contexts, clouds form into thunderheads that have a particular shape, internal complexity, and behavior. There is a clear order and directionality to the way thunderheads emerge over time. Likewise, in the establishment of ecological communities in the…growth of a secondary forest, the types and abundance of various plants and animals follow a well-defined sequence leading to the climax ecosystem. Again, there is order, direction, and structure. …But there is no design written anywhere in a cloud, or a program in the genes of any particular species that determines the final community structure. There is no set of instructions that causes a cloud or a group of plants and animals to change form in a particular way. There are only a number of complex physical and biological systems interacting over time, such that the precise nature of their interactions leads inevitably to a thunderhead or to a forest [Thelen & Smith 1994:xix]

I am grateful to the *Journal of Ritual Studies* for giving me a chance to meet and respond to these diverse and highly engaging critics of my recent book, *Food Rules: Hunting, Sharing and Tabooing Game in Papua New Guinea.* These commentators seem to have volunteered because of their own encounters with the issue of food taboos – often Papua New Guinea food taboos. It is thus all the more gratifying to hear some words of praise from all of them, however grudging in some cases.

But they seem to have found a lot to complain about as well. If there is any one theme that unites them it is that they would all prefer I had done something other than what I did. I should have studied globalization, followed the Wagner--M.Strathern--Weiner style of cultural analysis, spoken fluent post-modernese, tried harder at symbolic analysis, or chased every piece of Seltaman human nature down to its evolutionary roots.But of course the reason I wasn't inclined to wander down -- or wander further down -- any of the suggested byways was that I was busy promoting trends that interest *me* – the modularity of mind concept and dynamical systems theory – in the way that works best for an ethnographic presentation. These are the quite contemporary issues that I am weighing in on, and on which I seem to be more up to date than certain of my reveiwers.

The subject of food taboos by itself, even food taboos of Papua New Guinea, produced very little convergence of reasoning. Of the four reviewers, only Dan Fessler has an overlapping theoretical interest (modularity of mind); only Pat Townsend can claim to have threaded her way down the "social allocation" path that I have highlighted. Fessler and Townsend both show enthusiasm for those parts of my argument that they can relate to. None of the reviewers, however, demonstrates much familiarity with dynamical systems theory. The lack of theoretical overlap means that they often just aren't getting it, and this shows up in a number of ways. My critique of cultural determinism is confused with the post-modernist critique of the concept of culture; the modularity of mind approach is declared to be some form of connectionism, and dynamical systems theory is reduced to a mere "style." Bamford imagines gaps and contradictions in my argument. Fessler takes off on flights of evolutionary fancy. McPherson complains about computer language, while Townsend picks on copy edit mistakes and missed references! The result is a composite of commentary that is unfocused, extraneous and often widely off the mark. The challenge here is to weave these non-related tangents into a productive discussion.

It might help to restate my intentions in writing the book. Ethnographically, my goal was to adequately illuminate a pattern of food practices among the Seltaman people of Papua New Guinea, a pattern that conventionally would be referred to as their "food taboo" system and that would appear on its surface rather trivial; it turned out to be the tip of a hidden iceberg. Theoretically, my goal was to tap into the two trends I designated above – "modularity of mind," or "domain-specificity" theory, and dynamical systems theory - both to guide the task of understanding the food pattern and to raise issues concerning the effects wrought by these trends upon our usual concepts of culture. Judging from my commentators' remarks, it is the "effects upon our usual concept of culture" discussion that is the most

problematic. Since no one correctly stated what my argument is, however, it is not clear to me whether anyone was actually arguing against it.

A quick review. Both domain-specificity theory and dynamical systems theory challenge that "A-realm-of-our-own" vision of culture that is so common among cultural anthropologists. This vision emphasizes the autonomy of culture from the "brute" forces in human life, and dictates that this distinctive area have its own experts to deal with it – cultural anthropologists. The vision emphasizes as well culture's crucial role in shaping the otherwise formless and chaotic individual (or psyche or self) -- emotionally, intellectually, and bodily -- and in "constituting" human abilities and practices of every sort from color recognition to gender relations. When multiple factors enter into the formation of a human pattern, some of them clearly "brute," the cultural ones are always, by implication at least, the critical ones.

Domain-specificity, aka "modularity of mind" theory, problematizes this vision by arguing that the human mind, that which acquires, expresses and manipulates culture, is not a general induction mechanism passively receiving external ("cultural") input, but is composed of multiple, diverse and intricate subject-specific innate subsystems without which no learning and little familiar human behavior would take place. (Empirical research supports the notion of innate principles governing language acquisition and manipulation; recognition of living kinds; understanding of spatial and physical relationships in the material world; social relationality; sexual relationality; numeracy; facial expression recognition; food tastes and disgusts – to name a few. A quick guide to the concept is Hirschfeld and Gelman 1994b).

When mind is understood as heterogenous in this way, culture can no longer be seen as all of a piece with a general expertise covering it all. Animal taxonomies, for instance, take shape in a different psychological arena than does the sorting and classification of human artifacts; social relationality is distinct from sexual relationality in critical non-apparent ways; grasp of another's mind and mental state is quite distinct from grasp of a mathematical formula. Regardless whether the animal taxonomy, the artifact classification, the mathematics or the relationships are all socially shared and in certain of their aspects socially transmissible (to give one definition of culture), regardless whether they can also be viewed as systems of concepts embodied in symbols (to give another), they each have a hidden dimension – those domain-specific principles – that distinguishes them from each other and causes them to behave non-comparably in human life.

Under this view, one is no longer secure in freely interpretively interrelating the different pieces of a given culture, using, for instance, a sequence from myth to illuminate a quirk in animal classification [cf. Whitehead 1995 for related discussion], or using the morpheme meanings in the language as intertexts for the local ritual sumbols [cf. Gell 1975]. One also cannot be sure a given element of culture, say the appearance of racial categorizing or same-sex playmate preference among young children, is a simple case of social transmission or the result of the invisible interplay of domain-specific principles and social context [Hirschfeld 1994; Maccoby 1998, pp.15-31]. One is no longer secure, either, in arguing that offensive aspects of human nature, such as those tendencies that give rise to ethnic animosity and gender hierarchy, are simply the artefact of western culture and should blow away with a little cultural re-programming. This last corollary of innatism, that attributes we have come to deplore have an innate underpinning, is often interpreted by its foes as the principal reason anyone argues for it! Innatists, it is felt, wish to relax into their deplorability since it is, after all, just "human nature." One response to this is that one cannot change the world for the better without first understanding the world and what is required to change it. A second response is that undesirable innatisms, stubborn though they may be, are no more stubborn than the innatisms that we invoke to oppose them – our sense of fairness, for instance, or our compassion, or our desire for inclusiveness. The "human nature" that the mind design trend is uncovering is as rich, complex and self-contradicting as the humans we know: it has no one social agenda.

As I note in *Food Rules,* in presenting both the culturalist vision and the innatist response, I am covering, sketchily, much the same ground covered by evolutionary psychologists Tooby and Cosmides

in their "The Psychological Foundations of Culture," [Tooby & Cosmides 1992]. The reader might wish to consult their ground-breaking essay for a fuller account. Bamford might wish to consult it regarding the shaky intellectual position of innatist-skeptics such as Harraway, Gould and Lewontin.

Dynamical systems theory is a second approach that challenges the Realm of Culture vision. It does so in two related ways. First, it presents us with a coherent account of "emergence" as opposed to the self-contradictory account that Durkheim and many following him such as Talcott Parsons and more recently yet Marshall Sahlins have used to support the notion of an autonomous cultural/ social realm. Yes, a patterned phenomenon can crystallize ("emerge" or "self-organize") from a pre-existing state that contained no surface indication of its possibility and it may sustain an organization quite distinct from the organization of its causal sources. But this gives the pattern no causal autonomy from its origins, as Durkheim wished to assert. And it gives us no license to see human culture in the abstract or human sociality in the abstract as emergent *totalities*, i.e. realms. Certain types of patterned phenomena within human culture or sociality – language, for instance, or kinship organization – may submit to the notion of emergence (though we await a full test of this possibility), and many many particular human patterns (a particular kinship organization, a particular cultural genre, a particular pattern of oscillating political structure, a particular complex of food restrictions) submit to that notion. But these crystallizations do not, in dynamical systems theory, direct us to study the emergent phenomenon simply as a thing *sui generis*, as Sahlins, reviving Durkheim, would insist. On the contrary, it directs us to analyze the conditions out of which the phenomenon crystallized, seeking to correctly spell out the causal scenario that brought the pattern into existence.

The second way in which dynamical systems theory leads us away from specializations in an isolated culture sphere is in its indifference to generic and hypothesized classes of phenomena – the "cultural," the "meteorological," the "chemical," the "social." Dynamical systems theory addresses patterned phenomena (*any* phenomena, but we are concerned here with human phenomena), that are the outcome of a mix of interacting causes. A single system of interacting causes may have more than one outcome, traceable to different ratios between the interacting ingredients. Every ingredient in the mix shares causality in interactive co-dependency with the other ingredients. One can, in some systems, pinpoint the interactant that, scaling up or down, triggered the emergence of an outcome, e.g. temperature when water suddenly becomes ice. But these "key" interactants must be arrived at for each pattern studied and cannot be predicted from the generic class to which a component belongs. The hanging of generic tags on the ingredients – e.g., one could be tagged cultural, one biogenetic, one meteorological, etc. – is totally irrelevant to the analysis and irrelevant to the identification of the "key" ingredient. One must remain focused on how the causal drama is playing out, not on the generic identities of the actors in the drama. While any human patterned phenomenon will include in its causal base interactants that we are accustomed to view as "culture," it will usually also contain things that we, trapped in a false dualism, have been viewing as "biogenetic" or "environmental." These are matters about which cultural anthropologists often have little expertise. The realm-of-our-own vision has long been used as the justification for granting these matters no significance, but such a move is deeply invalid.

Whatever one's expertise, in dynamical systems analysis one must follow where the system seems to be leading. In *Food Rules*, for instance, the patterned outcome in question was the Seltaman system of food taboos. While I could make some headway with this pattern by concentrating on Seltaman conscious thinking and deliberate social process, there were points at which I was led off into the distribution of New Guinea fauna, or the quality of local hunting dogs, or into hidden "domain-specific" principles of human psychology, such as disgust formation. Far from producing a portrait unrelated to the one that Seltaman are consciously operating, these excursions into the more "meaning-distant" factors in the system enriched my understanding of what Seltaman had to say and exposed the relevance of other parts of their discourse previously ignored. One cannot ask for better in an ethnographic project.

It should be pointed out, however, that because of its indifference to generic categories of all sorts, dynamical systems theory cannot be termed a quintessentially "cultural" mode of analysis, or as a new way to target and explain "culture." It targets and explains patterns, period. In doing so, it greatly

enriches one's grasp of whatever chosen patterned outcome is at issue, whatever we may have been calling that pattern - cultural, biological, economic, ecological, etc.

There are several cautions that need to be uttered in regard to the two trends, especially dynamical systems theory. DST does not easily apply to just anything. It has nothing to say about things that have a single sufficient cause. There are millions of instances of these in human life certainly: a cultigen fails to flourish in the local soil and is abandoned, warfare predations drive a community to change its location, an infusion of currency inflates prices, etc. Hudson Stuck writes about Nome, Alaska, "Nothing in the world could have caused the building of a city where Nome is built except the thing that caused it: the finding of gold on the beach itself and in the creeks immediately behind it. It has no harbor or roadstead, no shelter or protection of any kind; it is in as bleak and exposed a position as a man would find if he should set out to hunt the earth over for ineligible sites." [Stuck 1914/1988: 121] A wonderful observation, but no opening for the dynamicist. DST also need not be invoked for cases of simple compounding causality either, such as when the finding of gold *and* the presence of a good harbor, a good roadstead and shelter combine to cause the city site to be chosen. Finally and by no means trivially, when there are too many causal factors at play, or when too many causal pathways are not accessible to our scrutiny, dynamical systems theory will not be of much help. Lack of historical record in non-literate societies is the accessibility issue most vexing in anthropological research.

Use of either of these trends will pull the pure cultural anthropologist out of his/her area expertise and into other disciplines. Hyphenated anthropologists who have a second speciality in "bio," "psych" or "cog sci" may be at an advantage when it comes to exploring domain-specific interpretations. The area of needed additional expertise in a dynamical account, on the other hand, is purely a matter of what phenomena are being studied. I am not advocating that we all rush out and get degrees in some new field; I certainly didn't. But a willingness to collaborate with partners of other disciplines and expand our own educational horizons does not seem too much to ask. In my own most recent research, I collaborated with experimental psychologist Carol O. Eckerman to study the emergence of peer imitation among a sample of Seltaman toddlers, comparing them to U.S and other samples taken from Eckerman's earlier research [Eckerman and Whitehead 1999].

In my conclusions, I touch upon the fact that it is still an unresolved question whether the two trends, DST and modularity of mind are compatible. J. Fodor, who gave prominence to the term "modularity of mind," apparently still clings to the notion of causally sealed off emergent realms [Thelen and Smith 1994:39], while Esther Thelen and Linda Smith, among the strongest advocates for DST in developmental psychology, are fiercely opposed to the idea of "hardwired" (i.e. context-free) psychological attributes and have spoken out as well against the information-processing language so favored by evolutionary psychologists [Thelen & Smith 1994: xiii-xxiii.] I feel that there are concepts within the terms of their own argument that can be employed to bring the two viewpoints together, but that discussion must await a future elaboration.

Let me turn now to my reviewers' reactions to the extended version of these points that appeared in *Food Rules.* I will begin with Naomi McPherson's review since she is the reviewer who has most sincerely engaged the work, and engaged it from the perspective of a cultural anthropologist. Sincerity notwithstanding, her commentary is full of misreadings and misrepresentations, fostered perhaps by an overly emotional reaction to my assault on sacred icons. She construes me as saying for instance, "As *only one* component of a dynamical system, culture cannot be a causal force…[my emphasis]." The same misreading appeared in an earlier review of my book by cultural anthropologist Joel Robbins. He wrote: "…culture neither determines how people live, nor is it integrated…Instead, it is merely one, often it seems fairly weak, causal strand among others, largely biological and environmental, that come together to produce what she calls 'human life-ways.'" [Robbins 2001]. I can't help but imagine that these two have spent too many hours in departmental meetings arguing over over the future of the four- fields approach. Our human being must have his ecological dimension, his biological dimension, his cultural dimension etc. More to the point, I suspect that seeing culture dethroned from its customary position of causal *superiority,* which it enjoys in the realm-of-our-own vision, felt to both these reviewers as if I had

deprived it of all causality.

But as I explained in my theoretical chapter, in a dynamical system (as oppposed to in an anthropology department), there is no reason for "only one" factor to be cultural; *all* could be, and the broader our definition of culture, the more will be. (This, of course, makes no difference to the DST analysis, and will make no useful difference to our overall perspective if our definition of culture is flawed from the start, as the modularity approach argues it is.) Furthermore, all interactants in a dynamical system *share* causality. Thus should it prove to be the case, in some instances, that "only one" designated interactant is something Robbins or McPherson could agree to call "culture," it would nonetheless be a causal force, perhaps even a "key" force for that particular pattern, but certainly no less significant than any other ingredient. Shared causality is still causality whether one likes to share or not.

McPherson also feels that my lightly satirized "average anthropology professor" who depicts people as culturally programmed automatons has no existence in the real world. But in fact, a handy example of this type of cultural determinism can be found in her own review. "Seltaman women," she writes, " simply have no power and authority to cede to men *since the cultural meaning attributed to their sex* excludes them from access to *the culturally valued* means for achieving prestige and authority" (my emphasis). There we have it in a nutshell: culture dictates, the Seltaman automatons respond accordingly. Any sense that authority relations might be the subject of continuous requestioning and renegotiation is lost in this flattened rendition of my points.

Clifford Geertz is not a cultural determinist in this simplistic a fashion, nor do I ever accuse him, as McPherson claims, of billiard-ball causality. Indeed, Geertz is at pains to remind us that he finds causal analyses of any sort not the appropriate task of the cultural – or *interpretive* – anthropologist. He does, however, insist on running cultural differences between people into their core [Geertz, 1973, pp. 36-37], suggesting little room for that level of shared pan-human culture that is the inevitable corollary of a shared human nature. And he did suggest we liken the mind to a general purpose computer that requires cultural programming to effect any purpose [Geertz 1973, p. 44]. Thus he is one of the exploiters of the computer analogy that McPherson so despises in my other cited theorists. Both of these moves -- disengaging from causal reasoning and treating exogenous information as totally constitutive of the individual -- put Geertz squarely in the camp of the autonomous realm vision of culture.

McPherson does get one thing right about Geertz: he is a more generous reviewer than I. More generous, much smarter, far more erudite, and an all-around classy guy. If I challenge the paradigm that he has shared with generations of social scientists and that he has helped to establish as dominant, it is not out of any indifference to his enormous contribution, but simply because knowledge evolves and valid questions are being raised. I think he will understand.

Bamford's review, much more polished and aloof than McPherson's, also reveals the deepest innocence about the issues involved. I must confess a similar innocence of her issues. She apparently does respect *someone's* critique of our concept of culture, and thus hesitates to totally condemn mine, but there are many indications that her favored critiques and mine do not greatly overlap. Still very much a part of her approach is an enmeshment in the old stand-by generics "culture" and "society," with the former being predictably cast in the role of "constituting" the latter. She argues that I have deconstructed the "culture" side but not the "society" side (the insinuation being that I have retro political views about gender, which she glosses as "society"), and have done little to show how culture "constitutes" society. In other words, she missed the whole point of my theoretical chapter and conclusion. Durkheim's autonomous realm, the *conscience collective*, it might be pointed out, is more often termed "society" than "culture."

Besides overlooking the irrelevance of these generic labels to my analytic approach, Bamford has not bestowed on us her definitions of these two constructs (either of which draw long and tedious definitional arguments from anthropologists), but chooses rather to freely sprinkle labels as suits her desire to criticize. If one wishes to do this, one could easily find just as many Seltaman "cultural" phenomena that space and research materials did not allow me to fully explore, as "social" ones. One could also easily find examples of stuff we tend to call "culture" constituting stuff we tend to call

"society" right at the heart of my analysis. Chapters 8, 9, and 10 are devoted exhaustively to the role of the male cult and the cult ordained food taboos in establishing an image of Seltaman manhood and legitimizing men's precedence over women and senior men's precedence over their juniors. How Bamford could have missed the import of three whole chapters is a mystery to me, but apparently she did.

As for my "disassembling the cornerstone of my interpretive apparatus" midway through the book, we are once again in the Bamford imaginary. She is referring to my discussion of the contradiction at the heart of the Seltaman male cult. She seems to initially confuse a contradiction in the analyzed phenomenon with a contradiction in my argument, but a moment later she suddenly recalls that its currently fashionable to view culture as contradictory and "sloppy around the edges." So, without retracting her grand "disassembling" allegation, she abruptly shifts ground and states that she doesn't find the allocation part of my interpretation convincing. Without this component there would be no contradiction, so I suppose this is the cornerstone she is referring to. I hope that Pat Townsend's gut assent to the allocation argument will suggest to the reader that some New Guineasts at least find this line of reasoning quite resonant with their own New Guinea experience.

Switching to a quite different front, Dan Fessler performs an important service to readers who may still be relatively unschooled in evolutionary psychology, the discipline that is one of the major engines behind the elaboration of the modularity of mind approach: He models an Ev Psych demonstration for us. He takes my points on disgust and food dishabituation as far as they go, and fills in what he, as an evolutionary psychologist, perceives to be the gaps. The rather lame filler that he uses – admiration, valuation of the norm, etc. – is less important (and I'm sure he knows this) than his illustrating what kinds of questions an evolutionary psychologist would ask and expect to have answered in studying any behavioral regularity thought to indicate an evolved psychological response. His demonstration gives me a chance to discuss why I am not a fully evolved evolutionary psychologist.

The practical and surface reason why I am not a fully evolved evolutionary psychologist is that I did not give myself time to get deeply into a study of this new field but rather, seeing its general import and persuaded by its logic, I wrote this book in a rush, wanting to make use of the mesh between certain well-thought-out innatist researches – notably those of Rozin on disgust and Fiske on social relationality - and my own ethnographic findings. But the deeper reason was that I am not particularly attracted to the kind of research that evolutionary psychologist do. Their findings, yes! Their research agenda – that's another story. For instance, within the context of an ethnography, *Food Rules,* I don't find it deeply interesting why the disgust reflex evolved, only that research seems to have established its robustness and outlined its general character. No doubt it originally came into being for some ancestral reason. What is more interesting to someone raised in the cultural anthropological tradition is the way in which once present, the reflex is put into new forms of social or intellectual play that go beyond its plausible origins, such as the use of disgust provocation in group initiations. (The new uses for old domain competencies also intrigues Dan Sperber who suggests they may have a lot to tell us about culture formation [Sperber 1994]) Being blocked from exploring these new dimensions of meaning by colleagues who rev up their anti-innatist bromides drives me crazy. On the other hand, I do not feature mixing too strong a dose of evolutionary psychology into the ethnographic enterprise. It enriches the latter, to be sure, and its findings can be incorporated into revealing "sidebars" as long as this is not overdone. But when every third paragraph of an ethnography starts with such saturating generalities as "people exhibit a robust tendency to . . ." or when functionalist reasoning – the favored reasoning of evolutionary psychology – begins to dominate our discussion, the task of opening a foreign world to the reader runs aground, and the search for a Universal Human Nature that takes over the text becomes oddly dehumanizing.

The reader can see that I have issues with both sides of this debate and am trying, awkwardly at points, to sidestep the bad and retain the good from each. Generally, of course, when a war is brewing, the person who tries to bridge between the two camps winds up getting shot first, and this may be what happens to me. In fact if these four reviews are any indication, it already has! The danger notwithstanding, for now at least, I am not advocating conversion to an evolution-oriented kind of research for cultural anthropologists, but rather that we in the ethnographic enterprise begin to take

seriously and make use of the flood of new findings that knocking down the barriers to modularity of mind understanding has unleashed.

In winding down, Pat Townsend's review gives me a chance to explain some of the problems having to do with the form of the book. Everyone has their miserable publishing stories, I'm sure, so I will try not to whine too much about my own. Rather than the tyrannical author hectoring her press into poor decisions, as Townsend envisions, my editor shared with me a delusion that with appropriate simplification and trimming – tables and appendices posted on the internet, reduction of "insider" debates and esoteric terminology, theory confined to a detachable chapter and conclusions – the book could be made accessible to a wider audience than just New Guinea anthropologists. I was to do the trimming, the press promised to offer the book at an affordable price and bring it out in paperback within a year. Both promises were broken. (The press will undoubtedly argue that the trimming wasn't sufficient.) I also wound up having to redo, in haste, a poor copy-edit job. Missed references are, of course, my own fault. I've gnashed my teeth already over the Dan Jorgensen one, and will take this occasion to acknowledge him [Jorgensen 1991]. In short, I am frustrated myself by some of the same things that irritate Townsend. But books nowadays routinely include copy edit mistakes, reference omissions and errors (one usually notices these when looking for one's own name in the references) and numerous assorted small scholarly headaches. These are embarrassments to be sure. Glaring defects? Hardly.

Finally, I am perplexed that anyone would imply, as Townsend does, that a group numbering only 200 can make no contribution to our understanding of human life. Groups this small or smaller routinely form the basis of well received qualitative works in sociology, psychology, child development, comparative religon and urban anthropology to name just a few areas. If one studies a phenomenon found among 200 people and gets it right, isn't this superior to studying a phenomenon found among millions and getting it wrong?

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