

Beyond affluent foragers: rethinking hunter-gatherer complexity

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Whither affluently complex hunter-forager-gatherers?

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Introduction

Edited volumes play major roles in the affluent forager/complex hunter-gather literature as they do for hunter-gatherer studies generally. Establishing the intellectual lineages among edited volumes is also important. The volumes growing out of the International Conferences on Hunter-Gatherers (CHAGS) descend directly from *Man the Hunter*. For affluent forager studies, the founding edited volumes (eg, Koyama & Thomas 1979; Price & Brown 1985) are especially important. Editors of more recent volumes carefully place themselves and their books into intellectual genealogies extending back to either or both of these seminal works (eg, Habu et al 2003; Prentiss & Kuijt 2004), which can then claim descent from *Man the Hunter*. Sahlins' 1972 *Stone Age Economics* is also intellectually central because it is the best known articulation of 'Original Affluent Societies'. The volume at hand is a case in point. Its title claims to move us beyond and to rethink both founding books. It links itself overtly to Koyama and Thomas not only in its title, but by opening with a forward by Koyama and Junzo Uchiyama, one of the editors of the current volume.

In their foreword, Koyama and Uchiyama revisit the notion of 'affluence' developed in Koyama and Thomas

and set out the goals for the current volume:

Over twenty years ago, "Affluent Foragers" was coined as a short hand term for the specific socioeconomic conditions of coastal fisher-hunters in productive environments. A central implication of this label was that during the Holocene certain prehistoric groups were recognized as having achieved highly organized social structures and high degrees of sedentism in the course of adapting to temperate coastal environments (Koyama & Uchiyama 2006:1).

According to Koyama and Uchiyama, the term 'affluence' was used by Koyama and Thomas in its common sense of material well-being in deliberate opposition to Sahlins' usage. In the original volume, Sasaki (1979) equated 'affluence' with large populations and technological complexity.

Koyama and Uchiyama go on to suggest the concept of affluent foragers was recast by Price and Brown's *Complex Hunter-Gatherer* volume with its emphases on economics, hunter-gatherer mobility patterns and the Binfordian (1980) linkage among economy, mobility patterns and social organization. They assert:

[T]hese discussions have failed to adequately explain the mechanisms that produce the great degree of cultural variability among foragers, and tend to simplify the multi-factored and dynamic

processes involved in social and economic change among hunter-gatherers (Koyama & Uchiyama 2006: 2).

In their view, 'the sociality of hunter-gatherer lifeways' is among the crucial processes causing hunter-gatherer diversity. The current volume's purpose is to bring these social dynamics to the foreground. They see two fundamental issues facing hunter-gatherer studies: accounting for cultural variability among hunter-gatherers and explaining social and economic change, which, in their view, has been made difficult by oversimplification of the causes of change. Accounting for variability and change, of course, are the central issues in explaining any form of evolution.

To my mind 'Affluent Forager' and 'Complex Hunter-Gatherer' reflect somewhat different but overlapping realisations about ancient hunter-gatherers. As originally conceived 'Affluent Forager' was a way to grapple with the diversity among ancient hunter-gatherers becoming visible by the mid-1970s. The concept was originally applied to 'aquatic hunter-gatherers' (Ames 2002) who did not fit Lee and DeVore's succinct description of generalised hunter-gatherers: 'We make two assumptions about hunters and gatherers: (1) they live in small groups, and (2) they move around a lot (Lee & DeVore 1968:11)'. 'Complex Hunter-Gatherers' grew from the discovery that some cultural traits widely viewed as major thresholds in cultural evolution were not contingent upon agriculture. Chief among these traits were sedentism, food storage and especially permanent social inequality. Complex hunter-gatherers became an alternative set of evolutionary experiments in human social complexity with which to develop and test theories of cultural evolution.

Appropriately for its intellectual lineage, the current book is primarily about variability despite talking a great deal about complexity. In fact, I think it's more about variability than the editors realised or perhaps wanted. It has something of an oval-peg-stuffed-into-round-hole quality; the sum of the volume's papers is different from what the editors think. The book's strength is the array of case studies from disparate places, most especially East Asia and the southern hemisphere, regions for which the Anglophone literature is quite weak. It provides a number of researchers an opportunity to get their views out and attract others to their work and ideas. Its weaknesses are the array of case studies from disparate places, which, despite the editors' valiant effort, do not really cohere; some papers are extremely

narrow and empirical while others are innovative and broad in view, although always firmly rooted in their empirical case.

I do agree with the editors' assessment that we do not adequately understand hunter-gatherer diversity or have the theoretical tools to explain it (eg, Ames 2004); I first review the book, and then discuss some of the issues in the study of complex – affluent – hunter-gatherer-foragers that the book raises and against which it should be judged using the editors' concluding essay as a basis.

Affluent Foragers

The book contains 13 chapters in four sections: Introduction (Koyama and Uchiyama's Foreword); Case Studies (five chapters); Comparative and Theoretical Studies (six chapters); and the conclusions (Kim and Grier's final essay). The six case studies are quite varied in their scope and their direct relevance to the themes of the book.

Heather Bluiith's *Gunditjmarra Environmental Management: The Development of a Fisher-Gatherer-Hunter Society in Temperate Australia* continues long standing arguments (eg, Lourandos 1985) for complex hunter gatherers in south Australia. The premise of Bluiith's model is that stratification and hierarchy are consequences of resource control and material surplus, which in turn 'resulted from the technologically assisted exploitation of environmental opportunities offered by the local landscape', in this case wetlands and a productive anadromous fish – the short fin eel (*Anguilla australis*). The case study rests on well controlled field survey and GIS techniques to map the presence of elaborate and extensive stone features interpreted as houses, fish weirs and traps, and storage facilities thought to represent management for eel harvesting which supported a local specialisation in eel production that produced surpluses used for exchange. The complex's labour costs tied people to them (echoing arguments made elsewhere, on the Northwest Coast for example). Individuals and families had differential access to the surplus because of differential control of either the weirs or the patches in which they were deployed. Leadership positions arose from the need to coordinate construction and management of the weir systems. The essay relies heavily on analogies with the Northwest Coast and Interior Plateau of western North America and echoes many arguments made on the coast and plateau.

This good paper illustrates some of this volume's strengths. It rests on a solid empirical basis using modern field techniques; it is about an area that does not appear often in the international complex hunter-gatherer literature and Bluith controls her sources well. It also exhibits some weakness: it is overly reliant on the Northwest Coast/Plateau examples (despite the editors' warnings about over generalising from limited analogies); it assumes leadership and management are required to generate large scale (both in time and space) constructions and she has no direct evidence for leaders or inequality.

The next three papers, Plug's *The exploitation of Freshwater Fish during the Later Stone Age of Lesotho: Preliminary Results*, Ikeya on *Mobility and Territoriality among Hunting-Framing-Trading Societies: The Case of Bear Hunting in Mountain Environments in Northeastern Japan* and Nakajima's *The Significance of Freshwater Fisheries during the Jomon and Yayoi Periods in Western Japan: The Analysis of Pharyngeal Tooth Remains of Cyprinid Fisheries* also illustrate the diversity of places represented in the volume. They also exemplify weaknesses in volumes based on conference symposia. All three are narrow in focus. Plug discusses fish remains in Late Holocene lake deposits in interior southern Africa. Her well done paper is primarily of interest to regional specialists and zooarchaeologists specialising in fish. Nakajima's paper is similar. It is a history of fish and fishing in Lake Biwa, the major lake in central Honshu Like Plug's, it is an interesting paper with a very narrow zooarchaeological focus with no clear thematic linkage with complex hunter gatherers, unless fishing is a marker of complexity (which Bluith might argue).

Ikeya is interested in the circumstances under which hunting territories form and change size. He develops a formula to determine a value labelled 'K'. Shrinking K values predict increasing territoriality and competition – higher K values point to hunting territories held as commons. His formula is $K = \frac{\text{[amount of resources} \times \text{predictability]}}{\text{[number of hunters} \times \text{demand for hunted resources]}}$. He tested this by qualitatively examining bear hunting practices in northeastern Honshu. However, he doesn't actually do the calculations or show they might even be done.

The last paper in this section is June-Jeong Lee's *From Fisher-Hunter to Farmer: Changing Socioeconomy during the Chulmun Period on Southeastern Korea*. Lee examines shell midden

contents and settlement patterns in two localities on the southeast coastline of Korea to measure subsistence change through the Chulmun period (6000 – 1300 BC). In both areas, subsistence rapidly changes in the late Chulmun with residential shifts away from the coast and increased reliance on terrestrial resources. However, local scale patterns leading up to this shift differed in the two regions. To account for these changes, he examines population growth, environmental/climatic changes and increasing social complexity and concludes that the changes he documents at the end of the Chulmun are the result of a shift to increased use of cultivated plants driven by resource stress and amplified by social changes including increased complexity, which in turn was driven by subsistence changes and storage. This is a good paper. It has well-controlled data from an interesting and poorly known (for English speakers anyway) region on the coasts of the Sea of Japan, a region that is probably one of world's the best for investigating complex hunter-gatherers but is rarely examined (eg, Yuan et al 2002). This area includes the coasts of the Sea of Japan, the Yellow Sea, and Bohai Bay particularly during the Late Pleistocene–Early Holocene.

Jochim's *The Implications of Inter-Group food exchange for Hunter-Gatherer Affluence and Complexity* opens the section on Comparative and Theoretical Studies. In a very interesting but slight paper, Jochim argues for the importance of food exchange among hunter-gatherers. Food exchange is important in and of itself, but also as a contributor to the intensification of production and the development of complexity. Jochim describes three purposes of food exchange:

- 1 as a means of increasing efficiency by obtaining some items less expensively than would local procurement
- 2 as a means of increasing subsistence security either through direct exchange of food, or as 'social storage' – establishing partnerships which allow access to resources
- 3 a means of asserting individual prestige.

He argues food exchange can, but does not necessarily, promote increased production. He reviews its archaeological visibility, although the correlates he lists could reflect other processes as well. His two examples, exchanges of fish and meat in California and fish oil and salmon the Northwest Coast could be expanded with additional examples. Two are the exchange of

salmon and roots on the Columbia Plateau (eg, Anastasio 1972), and dried and fresh foods along the Lower Columbia River (eg, Hajda 1984; Sobel 2004) in the northwestern USA. This is an important and overlooked topic, probably because of the methodological problems it faces. It deserves further development and I hope either Jochim or one of his PhD students is working on it.

The next two papers are substantive, and cover regions rarely visible in the Anglophone literature. Ballbè and Escalera, in their paper *Last Foragers in Coastal Environments: A Comparison of the Cantabrian Mesolithic, Yamana of Tierra del Fuego and Archaic Foragers of the Central American Coast* undertake longitudinal comparisons of three case studies looking at, among other things, the causes for increasing reliance on littoral or marine resources. A key conclusion is that increasing reliance on aquatic resources (viz Ames 2002) visible in all three sequences is the consequence of intensification rather than the intrinsic productivity of aquatic environments. They also draw contrasts between what I would call the evolutionary trajectories of aquatic and neighbouring terrestrial hunter-gatherer gatherers. As do other papers, they conclude the economic changes they see do not seem to correlate with environmental changes. Rather, in an implied Marxian argument, they see the socioeconomic changes as consequences of contradictions internal to these societies.

The strength of this paper is the comparisons among several long sequences, although they need to control more explicitly for both the length of their sequences and the spatial scale of their data sets. A weakness of the paper, one it shares with much of the volume, is a false dichotomy between 'ecological' and 'social' explanations, which I will return to in the discussion.

The paper by Loponte, Acosta and Musali, *Complexity among Hunter-Gatherers from the Pampean Region, South America*, is an excellent review both of the literature and theoretical development of complex hunter-gatherers, and of the notion of complexity itself – including stressing that complexity does not inevitably mean inequality or hierarchical organisation. They review various definitions of complexity and prefer Rowley-Conwy's four-fold typology: foragers lacking storage; collectors that do not defend territories; collectors that defend territories and sedentary groups that defend territories and store foods, seen as a continuum from the third type (Rowley-

Conwy 2001:42). Their version of this emphasises the first and fourth types. Their case study derives from wetlands in Uruguay, another exceedingly interesting case study. They conclude that the crucial factors in the evolution of complexity include population growth, circumscription, the properties of primary rather than secondary resources (leading to focal or specialised production), localised catchments, a high incidence of plants (compare Binford 2001) in the subsistence economy, mass harvesting and processing with the attendant labour demands. Departing from the volume's theme, these authors do not especially advocate social explanations.

The final three substantive papers are by the editors: Grier, Kim and Uchiyama respectively. This is where one expects to see the editors' goals most clearly. Grier, in *Affluence on the Prehistoric Northwest Coast of North America*, is concerned to link 'affluence' to political economy: affluent foragers are hunter-gatherers with a political economy. This of course implies there are hunter-gatherers without one; these might be 'generalized', immediate-return foragers. In any case, he also links political economy with agency theory, stressing, in his words, that social actors, with their motives, intentions, etc, are the driving force behind the formation of political economies. However, he does not develop this; rather, he focuses on distinguishing economics from ecology, since economies are the result of human intentions and actions. Again, I see this as a false distinction.

His case study is on the development of complexity – inequality – in the Gulf of Georgia region of the southern Northwest Coast and is based on his on-going project at the Dionisio Point site (eg, Grier 2001). His methodology is scalar. He is interested in how regional scale surplus economies form 'in conjunction with social and economic inequalities', identifying four regional/organisational levels or scales: the region, the sub-region, the village, and the household. In this paper he looks at households and the region and argues the households he investigated at Dionisio Point, marked as they were by inequality, participated in regional scale interactions.

Echoing Jochim's paper, he looks at, among other things, the production of food stuffs for exchanges, an important yet methodologically difficult topic. He tries to get at it using stable carbon and nitrogen ratios from Dionisio Point dog bones. These suggest the dogs ate a lot of herring, which he argues were stored. From this

he builds a further proposition: the herring were processed not only for household consumption but for trade at a regional scale. I applaud the attempt to grapple with a very difficult methodological problem, but the argument feels like an effort, albeit a creative one, to somehow get the dog evidence, intriguing in its own right, into a paper on political economy and regional interaction.

He ends making several recommendations. We need to:

- 1 understand affluence as a dynamic, multi-dimensional property of a society
- 2 work at multiple scales of analyses
- 3 conceptualise actors as catalysts of change
- 4 see economics as more than simple material processes and focus more on the economics of surplus – to move beyond seeing intensification and affluence as adaptive or stemming from ‘ecological imperatives’.

Junzo Uchiyama’s *The Environmental Troublemaker’s Burden?: Jomon Perspectives on Foraging Land Use Changes* is concerned with shifting Jomon settlement patterns and essentialised accounts of the Jomon. Studies of Jomon have tended to homogenise it by time period and broad geographic zones (Western Japan vs Eastern Japan) (Habu 2004). The impression many western workers have of the Jomon is shaped by the middle Jomon of the Kanto district (broadly the Tokyo region) of central Japan and the Middle and Late Jomon in the Tohoku region of northern Honshu. Settlement patterns across the archipelago also tend to be fixed in a sequence of increasing sedentism and expanding settlement size. Uchiyama critiques this, particularly to the extent that it derives from notions of affluence based on environmental richness. He also rejects a common assumption that Jomon people exploited spatially restricted zones around their settlements. He proposes instead a model of an expanded collector system involving not only logistical task groups within a radius but much longer, more distant movements (eg, Lovis et al 2005). He also seeks to show there were shifts from clumped to dispersed settlement patterns. To support these arguments, he examines the settlement history of the Torihama site, near the Sea of Japan coast of north central Honshu.

Using several lines of evidence he argues the site was both a seasonal residential site and a specialised boar hunting locality in a system of logistic movements spanning mountains and the coast during the early

Jomon. Subsequently, site use declined and changed as settlement patterns shifted to what he describes as a dispersed, more forager like pattern. He suggests this shift was a consequence of anthropogenic environmental changes rather than to climatically induced changes. Interestingly, he does not cite Habu’s work on shifting Jomon settlement patterns in the Kanto (eg, Habu 2002) or her work on shifting intensities of occupation at the famous Sannai Maruyama site – both projects that would lend support to several of his major contentions.

The Jomon, in many ways, epitomises the problems addressed in this book; it is very diverse in time and space. To cope with that diversity, archaeologists have used a variety of classificatory schemes. As the pace of work continues and data accumulate – in this instance in vast quantities – variability at all scales inevitably increases as a simple function of sample size, and is increasingly more difficult to accommodate with the old schemes. However, the scale of analyses is also crucial. One can always find local sequences that violate broad generalisations, particularly when based primarily on one site. The issue here then is not coping with complexity but coping with diversity and sampling within and across scales.

The last paper, Janguk Kim’s *Resource Patch Sharing Among Foragers: Lack of Territoriality or Strategic Choice*: is among the volume’s best. Kim is interested in hunter-gatherer territoriality and their strategic choices. He first critiques theories that assume hunter and gatherers do not exploit overlapping territories, positing that groups may share resource patches, particularly via task groups. He tests the hypothesis looking at the use of small, near shore islands on the west coast of Korea. His effort to operationalise logistical mobility includes a useful discussion of the issues inherent in trying to separate residential sites from task specific sites, although he does not discuss sampling or monitoring position issues.

In any case, his test is ingenuous. He uses small shell middens dating to the Late Chulmun (3500–1500 BC), developing two alternative hypotheses:

- 1 if Late Chulmun (LC) groups did not share resource patches – ie, had non-overlapping territories, then the ceramic assemblages at task specific sites should not differ from those at residential sites. They will be less diverse if assemblage sizes from task-specific sites are smaller;
- 2 if resource patches are shared, then ceramic

assemblages at task specific sites should be stylistically more diverse than those at individual residential sites.

All of this assumes of course a relationship between coresidential group and ceramic styles. Employing 32 ceramic types he found:

- 1 residential sites vary among themselves in the styles and proportions of styles present in each
- 2 residential assemblages are less diverse than task sites (measured by relative frequency of designs)
- 3 the spatial distribution of ceramic types among task sites is a function of distance between task specific site and residential site leading him to conclude that patch sharing was a strategy employed during the LC in this region.

He then discusses this result using an Human Behavioral Ecology framework to argue that patch-sharing is an alternative strategy to either sharing harvested food or territory defense. I won't review his arguments except to stress his point that patch sharing does not indicate a lack of territoriality but that it is a strategy with relative costs and advantages to be weighed against other strategies. This is a very good paper.

Discussion

Kim and Grier conclude the book in a final essay revisiting Koyama and Uchiyama's issues and pulling together what they see as themes in the individual papers. Their goal as editors was to deemphasise what they term a 'program' that sees the goal of research to be broad generalisations based on an assumption that affluent forager socioeconomic organisation is explainable by a limited set of factors, primarily environmental ones. Their intention in this book is to present studies that 'acknowledge, emphasize and apply the complexity of factors the have been increasingly recognized as critical to understanding the development of affluent foragers in their total complexity' (Kim and Grier 2006:192). They stress several times the need to focus research locally, although Grier particularly argues for a geographically multi-scalar approach.

Beyond that statement, it is not clear what is the goal of research. While I doubt there is much disagreement that social organisation is an important causal dimension (or several dimensions) of hunter-gatherer diversity, the editors and authors advance almost nothing that might be termed social theory. They nod towards agency theory but no contributor, including Grier, develops a case study built around agency theory.

Interestingly, the work perhaps closest to that approach is Kim's on territoriality and patch choice, but it is nearer Human Behavioral Ecology than to what I think of as Agency Theory.

Despite disclaimers, the book is firmly grounded in the environment, mobility patterns, diet choices and the other dimensions of variability that archaeologists commonly use to grapple with these materials. Notwithstanding Koyama and Uchiyama's comments, the book illustrates the durability of Binford's forager – collector continuum. Absent is any discussion of aggrandisers, those ubiquitous, selfish, hardworking *dei ex machina* of many current theories of social evolution. A good thing perhaps, but conspicuous in a book claiming an interest in the role of individuals in cultural evolution.

They warn we need to avoid terminological and typological debates that muddy our waters by essentialising variability and overgeneralising certain key examples such as the Northwest Coast. Again, there is probably no argument with the recommendation, but the book offers no way forward. It makes no recommendations about how to conceptualise diversity and variability beyond what particular authors do in their papers. One of the major achievements of archaeological research on hunter-gatherers over the past 30 years has been the unbundling of the set of traits commonly used to define complex hunter-gatherers. Each of these traits (eg, sedentism, intensity of production, storage) could be treated as a dimension along which hunter-gatherers vary and variation mapped. This in fact is sort of what Binford does on a colossal scale in *Frames of Reference* (Binford 2001) using the ethnographic record. Some of the papers in the current volume offer useful temporal mapping and comparative work on these traits, but it is not conceptualised dimensionally; everyone struggles with or against the types: affluent vs complex. No one tackles the larger question of what's a hunter-gatherer and how do we know one when we see one. All the papers can sidestep that problem because their examples are drawn either from periods before farming or places without farmers until the arrival of colonialism. But many do deal with times or regions where the economies do not readily fit either our stereotype of farmers or of hunter-gatherers: those economies Bruce Smith includes in his Low-Level producers (Smith 2001). Curiously, especially for papers from a zooarchaeology conference, no one meets this issue even obliquely. Maybe it only worries

palaeoethnobotanists.

They are far more concerned, I think too much so, with the meanings of affluence and complexity. Generally complexity is defined here similarly to Price (1981), Hayden (1995, 2001), Kelly (1995), Ames and Maschner (1999) and others. For example in his original paper Price (1981) defined complexity as 'that which is made up of many, elaborately inter-related parts'. He saw the following traits as defining complex hunter-gatherers:

- 1 technological changes that included increased diversity and specialisation, particularly in subsistence related gear
- 2 elaboration of non-technomic artefacts
- 3 larger and more complex (internally and externally differentiated) settlements, with greater variety in size and localities of use, some degree of sedentism;
- 4 intensification of production – food production becoming both more diverse and more specialised – a wider diet breadth, but with more specialised technology
- 5 specialised procurement of some resources
- 6 larger populations and higher population densities
- 7 emergent social inequality, as well as other organisational changes which he did not specify.

There have been other versions of this list, but this suffices.

Arnold (1996) argues these trait lists do not adequately distinguish between causes of complexity (permanent inequality) and traits of complexity. She points out that hunter-gatherers, particularly in the past, were extremely variable in their residential, subsistence, technological economic and social organisations, and that these lists conflate this variability. She then suggests that explanations need to distinguish clearly between necessary preconditions for complexity and the consequences of complexity. She defines conditions as 'the environmental and historical circumstances, or context, in which complex organization emerged' (Arnold 1996, 95), and consequences as 'the intended or incidental results of increasingly complex organization' (Arnold 1996, 95). The latter includes such things as intensified production, new technologies, and specialisations that others (eg, papers in Price & Brown 1985) have seen as causes. She defines the causes of complexity narrowly, which rises from her definition of complexity. She defines complexity among hunter-gatherers as: 'mean[ing] institutionalized control by some individual(s) over nonkin labor. Societal

complexity also means hereditary inequality and leadership' (Arnold 1996, 93). More recently, Arnold (2001) has suggested applying the term 'Affluent Foragers' to hunter-gatherers lacking complexity as she defines it, but with the other traits scholars have used to distinguish complex hunter-gatherers from generalised hunter-gatherers.

The editors' solution to this terminological morass is decoupling inequality from complexity and redefining affluence as having a political economy. This suggestion is interesting for the issues it raises as well as for the confusion it might create. I agree with the first suggestion and disagree with the second and recommend rather that the whole thing be reconceived. All thinking about the evolution of inequality is based on the assumption that egalitarianism is humanity's default sociopolitical organisation in small groups. All theories are based on identifying mechanisms that move people from an egalitarian state to ranking/stratification. Some recent work has problematised egalitarianism (see Ames 2007 for a discussion); but the usual line of argument has either circumstances or people (or both) causing the development of elites/leaders from an egalitarian base with the concomitant development of a political economy to both fund and make inequality permanent. This is the essence of Arnold's definition, which could be both tightened and universalised simply by defining complexity as having a permanent political economy which then implies permanent inequality.

But some forms of permanent inequality may not require a political economy to reproduce them. Simple dominance hierarchies which rest on either innate or learned behaviors for example might not require economic support for their reproduction (although they might have economic implications). Feinman and Neitzel (1984) show that in many 'transegalitarian' societies, differentials in prestige and status have little or no material referents and therefore have little or no material costs or visibility. The issue then becomes: is it possible to have inequality in the absence of what is normally thought of as complexity or affluence? If so, what are its causes? If not, why not? In any case, I would argue that we cannot assume egalitarianism is the default for small human groups but need to demonstrate its existence.

The editors' other suggestion that affluence should mean the presence of a political economy seems to needlessly muddy the terminological waters. Why not

just say a group has a political economy. The suggestion is coupled with the very bad idea that there are affluent environments – affluent foragers in affluent environments. Environments are more or less productive from the standpoint of humans and their means of production. Calling productive environments ‘affluent’ creates the terminological confusion they want to avoid. However, I think their stress on economics is well taken, although I would not separate economics and ecology as much as the editors do, seeing them as two sides of the same coin.

Their separation of economics and ecology reifies both. Their thinking appears influenced by Halperin’s distinction between *locational movements* and *appropriational movements* to both distinguish and link ecological and economic anthropology, but they do not more fully develop her synthesis of ecology and economy (Halperin 1994:82–84). Rather, the editors and authors focus on the use of the environment by archaeologists as a *deus ex machina* to explain the evolution of complexity. While this is true, environmental explanations are here used as straw men. Presumably they have in mind the recent debate among scholars working in the Santa Barbara channel region of California over the nature of the environmental problems or crises that occurred there after AD1000 and its role in the development of Chumash elites. The environmental data in play there is extremely fine grained and well developed. At one level, environmental problems are *dei ex machina* in these models; they are the necessary mechanisms that force people to surrender egalitarianism to aggrandisers; at another level the critique as presented in the book is a caricature, which is unfortunate in a book in which environmental and subsistence data are often well used.

They amplify Grier’s argument in his own chapter that political economy is fundamental to affluence and complexity, particularly the production and control of surpluses. By stressing the production of surpluses they join a long and distinguished line of scholars and social theoreticians. They do not, however, do much more than say political economy and surpluses are important. It comes up here and there in the volume (eg, Bluith, Greir) but the notion is not developed, nor is the vast literature tapped. This is a very important opportunity lost.

It is an important opportunity because much current work on affluent foragers/complex hunter-gatherers focuses on explaining the origins and evolution of

inequality and the other traits that define them. The environment and production play roles as enablers (affluent environments) or as causes of stress or change. But detailed work on how hunter-gatherer economies actually pay for affluence (or complexity) is rare. A focus on political economy would help redress that. Grier attempts this in his paper but his evidence is not strong. Bluith’s paper is also about political economy but too focused on the Northwest Coast analogy.

Finally (in my order, not theirs) they discuss the evolution of complexity, briefly reviewing what they must see as the two principle strands of modern evolutionary archaeology: cultural selectionism (eg, O’Brien & Lyman 2000) and cultural transmission theory (eg, Boyd & Richerson 1985) as the two main branches of ‘neoevolutionary’ theory. Oddly they leave out Human Behavioral Ecology, which has actually been applied to hunter-gatherer complexity in at least one detailed case study (Fitzhugh 2003) and is employed, at least implicitly, in Kim’s own chapter. They assert that these approaches are inadequate because neither deals adequately with the generation of variability, which they see, correctly of course, as essential to evolutionary processes. For them, culture variability is the product of human actors, ‘working to create and manipulate circumstances’ according to their own ends. By this means, they slip in agency theory, without actually talking about it. More to the point, however, it also indicates a misunderstanding of much of the ‘neoevolutionary’ literature. Evolution requires variability and a mechanism or mechanisms for sorting that variability and passing the sorted results on to the next generation, which in turn yields new or reshuffled variability. In cultural evolution, the potential sources of variability are enormous, including invention, diffusion, mistakes in learning, transmission errors and so on. However, acting according to ends (consequences) is a sorting process (eg, Durham 1991). Most of the ‘neoevolutionary’ literature does not in fact deal with sorting.

In many ways this is a good and useful book. It contains a number of productive papers about very interesting ideas with interesting data. Its flaws include those commonly suffered by edited volumes based on conference symposia. The editors have worked hard to impose themes and coherence on the book, but with only partial success. The editors’ efforts to revive the concept of affluent foragers and move us beyond

affluent forager and complex hunter-gatherer are also only partially successful as they themselves became bogged down in the distinctions they wished to clarify.

They are, however, correct in that the central issues remain variability and evolution, and how we measure and account for the former and explain the latter.

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