

Comments: Rethinking Complex Early Societies in Asia



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THE EXPLANATION OF EMERGENT social complexity is a perennially challenging issue in archaeology. In these collected papers, the authors advance our understanding of this problem by analyzing early Asian cultures ranging from simple villages to full-blown empires. Their use of a common conceptual vocabulary, drawn from American anthropological archaeology,¹ affords grounds for thoughtful comparisons both among these cases and with others elsewhere in the world. Although the studies all concern societies that are in some sense complex, the cultures vary markedly in scale. At the simple end are the Longshan incipiently ranked societies of North China, discussed by Anne Underhill. More complex are the regional chiefdoms of the sixteenth-century Bais Region of the Philippine coast, examined by Laura Junker, and the complex chiefdoms to incipient states of the Xiajiadian and Erlitou cultures of early China studied by Gideon Shelach. At the most complex end are the imperial states, examined in Carla Sinopoli's discussion of mobile capitals in the Mughal empire, and Kathleen Morrison and Mark Lycett's evaluation of power and symbolic expression in India's Vijayanagara polity. Francis Allard's paper on the Chinese Lingnan Culture, in contrast, offers a view of the consequences of interaction between a peripheral area and a series of Chinese empires.

In this commentary, I would like to consider four key issues that tie together all of the papers. The first theme concerns how the authors use comparative analytical perspectives to approach prehistoric developments in regions that have individual intellectual traditions. The second issue involves the authors' shared interest in the regional nature of power in complex society, which was the problem that united the symposium from which these papers derive. The last two questions concern specific facets of regional power relations: the archaeological assessment of the significance of symbols and ideas and the role of economics in the formation of social complexity. Rather than simply recapitulate the contributions made by the authors, which are stimulating and enlightening, I prefer to emphasize particular points and to extend their arguments in an effort to think about potential worthwhile lines for future research.

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EXPLANATORY MODELS AND SOURCES OF INFORMATION

In analyzing their particular research areas, most of the authors here draw from interpretive frameworks developed from Service's (1962, 1975) and Fried's (1967) evolutionary constructs. Despite the limitations of stage models (e.g., Adams 1984: 120; Feinman and Neitzel 1984) and evolutionary archaeology (e.g., Friedman and Rowlands 1977; Hodder 1990), the combination of data analysis and general analogies works effectively. Legitimate comparison presupposes substantive research that uses a shared language of ideas, even if the theoretical arguments differ. To their credit, the authors of papers in this issue use approaches that invite further comparisons with other areas of the world.

It is worth underscoring, however, that most of the authors are constrained in their presentations by a necessary reliance on other researchers' data. As several contributors observe, the potential explanations for prehistoric change in South and East Asia are heavily affected by the history of research in each region. Political and intellectual circumstances, the search for national histories, and the methods used to obtain data have defined the direction of field investigations and molded explanations of culture history. Coupled with the challenges presented by language barriers, these factors have limited the issues that can be addressed through Americanist archaeology. Among the consequences is a tendency for study to be focused on regions that were home to complex societies in the transition to the historical era. The effective outcome is a lack of comprehensive survey for other areas of precocious development.² Underhill's paper on early developments in the Longshan Culture area focuses our attention on this issue, but most contributors point out the lacunae in the baseline understanding of culture history in the regions in question.

A second difficulty stems from the frequent assumption, in regions with long histories, that the relationship of *form* to *meaning* is essentially stable within a cultural tradition. The search for cultural roots often presupposes that, as long as material form is apparently constant, so will be its associated meaning. The direct historical method is certainly useful, despite the known or presumed inadequacies of the historical record, but it should not be the sole, or perhaps even the dominant, source of concepts about how societies may have worked in the past. In practice, this approach curbs consideration of explanations for which there is little historical testimony and creates a tendency to slant the interpretation of early finds.

In her paper, Underhill draws attention to the hazards involved in archaeologists' search for Chinese urbanism in early villages. Shelach similarly expresses well-founded doubts about the efficacy of efforts to find the Xia civilization's roots in societies already a millennium old by the time the histories were written (see also Chang 1986). Discussion of "cities" and "palaces," as these authors observe, may provide a misleading sense of the complexity of Longshan and Xiajia-dian Culture societies, which are intriguing without having to draw their significance from subsequent civilizations.

Nonetheless, several authors, especially Sinopoli, and Morrison and Lycett, favor the notion that historical context and ideas current at specific times were crucial to the more general developments that they evaluate. Their access to written materials certainly provides a great advantage in this regard, and an analytical

approach that combines historical particulars with analogical models has been productive elsewhere (e.g., Brumfiel and Fox 1994; Cowgill 1993; Flannery and Marcus 1983). Morrison and Lycett pointedly remind us, however, of the potential for manipulation of information by those producing the historical record. The authors here appropriately attempt to compensate for such problems by drawing on both historical conceptions and cross-cultural models in their interpretations.

Considering the diversity of societies that the authors discuss, it might be useful to explore more fully the domains of research favored in the study of complex society. Generally speaking, studies of emerging chiefdoms and states focus on such issues as the appearance of hierarchical political institutions, economic specialization, and class-based society. That is, the heart of research is that which is in itself complex. The papers in this collection share this orientation, because all place a heavy analytical emphasis on sociopolitical elites. The studies of chiefdom-level society—by Underhill, Shelach, and Junker—concentrate on the ways that elites can be distinguished and the ways that they exercised power, especially through manipulation of prestige goods. In a parallel fashion, Allard, Sinopoli, and Morrison and Lycett highlight the ways in which imperial and subordinate or peripheral elites legitimized their power. The authors' emphases are certainly reasonable, given that the advent of elites is arguably a universal in chiefdom and state formation.

As important as they may have been, however, elites constituted only a limited sector of prehistoric societies. To gain a fuller understanding of the context within which the elites operated, it would be useful to know more about the rest of the populace. Among other possibilities, the activities of the general populace plausibly could have included continuities in daily life, changes originating from a need to meet elite demands, and changes deriving from new opportunities that arise in an altered sociopolitical climate. For example, it would be interesting to know more about changes in general access to resources associated with the rise of elite power, or about potential changes in the household economy resulting from labor exactions rendered to the elite (D'Altroy and Hastorf n.d.; Smith 1987). It also would be of interest to have a better grasp of the degree to which emergent elite-focused ideologies integrated societies as a whole or set apart the upper echelons (see below).

Much of my concern with the balance between change and continuity stems from the likelihood that social complexity probably emerged in fits and starts and in restricted sectors of social life. Earle (1977) made this point for complex chiefdoms in Hawai'i, by observing that the elite class did not participate directly in the subsistence economies of subject communities. Similarly, one of the most constructive results of detailed study of prehistoric states is the increased recognition that state formation does not involve only the emergence of specialization and hierarchy. It also entails efforts to maintain stability, security, or tradition in the face of changing circumstances. The emergence of greater complexity thus entails increasing rifts between the specialized and elite sectors of society and the more generalized low-level entities, such as households and kin groups. An example of such a rift may be found in early Mesopotamian states, where the central bureaucrats apparently had little or nothing to do with managing local irrigation systems (Adams 1981).

How these divergent elements of society were linked in novel ways—that may appear to recapitulate older forms—has long been a key area for archaeological inquiry in prehistoric states. It might be beneficial to partially redirect future lines of inquiry in the societies examined here, however, by thinking more closely about why social hierarchy or political administration developed while other areas remained relatively simple, such as labor organization for subsistence activities. I suspect that, by concentrating on elite contexts and prestigious materials, students of early Asian cultures may be underplaying key continuities that provided much of the character of societies that, in retrospect, apparently underwent tumultuous changes.

If I may draw from studies done in the Andes, my own area of research, the apparent paradox of life under Inka rule stands as a case in point. Numerous researchers have cited the ubiquitous effects of state intrusion into the daily life of the populace, through *corvée* exactions, military duty, sumptuary rules, and forced resettlement (e.g., Morris and Thompson 1985; Rowe 1946). As John Murra (1980) noted, however, the Inka strategy was apparently to dress a new relationship of extraction in old clothes of mutual obligation (see also Rowe 1982). At the same time, the state economy was built on the productive capacities of the subject population. It was therefore in the state's interest that the basic productive units of society remain relatively self-sufficient and stable. The state thus intruded selectively into the life of the general populace, and the organization of household activities remained relatively constant (see Earle et al. 1987).

Although they concentrate on the elite domains of society, the authors here do show that eliteness did not necessarily imply leadership in the same domains of society cross-culturally. Several papers addressed the problem of sorting out the linked elements of complexity by assessing a wide range of activities or organizational features. The most wide-ranging paper is Shelach's discussion of Xiajiadian and Erlitou Cultures, in which he considers the nature of subsistence, settlement patterns, ritual and mortuary activities, and craft production. His approach to the problem is promising, in that he suggests how diverse sectors of life may have changed in different ways. Morrison and Lycett, in their paper on political power in Vijayanagara, similarly recognize the importance of considering multiple sources of power—military, political, economic, and ideological. They draw attention to the key points that the exercise of political power varies contextually and that multiple sources of status and power exist within complex polities. Both methodologically and empirically, their description of the varied ways in which the nature of political power and the production of material record are coupled is valuable.

THE LINKS BETWEEN LOCAL AND REGIONAL POWER

Virtually every author here emphasizes that, although the formation of more complex societies can be understood only regionally, such change entailed both internal restructuring and outside interactions. Underhill, Shelach, and Sinopoli especially focused on settlement pattern analysis as a means of getting at organization of regional power. Underhill usefully stresses that we not accept morphological similarities among settlements as a sole basis for functional similarity. Instead,

she ties together architectural form, labor investment, and craft production as means for getting at the emergence of social differentiation in Longshan Culture. Shelach, in partial contrast, examines settlement patterns as a baseline organization, within which a range of diverse activities was pursued. Junker's paper on Philippine chiefdoms nicely illustrates the importance of both internal relationships and links to external societies in the negotiation of politics of chiefdom societies. In particular, her suggestion that there were multiple patron-client relationships, with coastal chiefs acting both as patrons to their own populace and clients to Chinese traders, strikes me as being well supported by the archaeological evidence that she uses. The link between local polities and major Chinese civilization is also at the heart of the changes that Allard discusses for Lingnan Culture.

Sinopoli's approach to settlement organization takes a different tack, because she is concerned more narrowly with the nature of imperial capitals, both permanent and mobile. Her discussion of the transient nature of the imperial camps raises several interesting concerns. From a purely archaeological perspective, it is troubling to think about how readily the enormous amount of labor expended in the construction of Mughal facilities would be recognized in the material record. More broadly, it would be intriguing to know how sites were chosen for the location of a new, but impermanent, capital. What was the balance among military, political, ideological, and logistical concerns in setting up a mobile camp? Sinopoli's discussion also draws attention to the tensions of politics and economics in empires with mobile paramounts. The kinds of ideological lurches that played a role in underwriting the choices of capitals must have reconfigured local political relations with disconcerting frequency. The logistical demands of moving and sustaining such immense entourages must similarly have been disruptive in the extreme to local societies and required rapid development of a support infrastructure. Under these circumstances, it would be fascinating to gain an understanding of shifts in local settlement organization and seats of power that resulted from the changes at the top.

THE ROLE OF SYMBOLS AND IDEAS IN REGIONAL POWER RELATIONS

The third theme that I would like to consider concerns one of the main interests of archaeologists today: how the power that draws vitality from elite ideology is expressed in a material sense (e.g., Brumfiel 1992; Castillo et al. n.d.; Cowgill 1993; Demarest and Conrad 1992). That is, how are material devices used to express or construct political relations? The two papers that deal most specifically with this issue are Sinopoli's discussion of Mughal royal capitals, and Morrison and Lycett's examination of the expression of power in successive Vijayanagara polities.

In her paper on the Mughal mobile capitals, Sinopoli considers how the flexible content of elite ideology became a driving force in settlement patterns. She suggests that three principal factors underlay the practice of shifting capitals: (1) responses to local political and military conditions that affected imperial stability, (2) economic factors that allowed for conspicuous displays of material wealth, and (3) ideological factors, which she takes to be the most important. Her discussion

provides a top-down view of how royal camps and capitals were established, based on the manipulation of Islam and Hindu religion as a pivotal means that paramount leaders used to forge power.

Sinopoli's paper raises issues concerning the material effects of political and ideological machinations at the imperial center. The practice of developing a physical infrastructure of imperial installations, which she describes for the Mughal polity, was widespread in ancient empires. Frequently, and probably with exaggeration, the emperor is credited with conceiving and directing the construction of everything from canals to capitals. At the risk of simplifying the arguments greatly, two perspectives have been put forward concerning the importance of paramount leadership in empires in which military affairs played a central role. One view, expressed by van Creveld (1985) and Keegan (1987), for example, is that charismatic leadership is an essential driving force in imperial formation and management. Alternatively, Luttwak (1976) argued that, at least in the Roman empire, the glorified but erratic actions of the emperors were at odds with the more gradual, systemic expansion, consolidation, and retrenchment of the empire. Much military action directed by the emperors was for political consumption, not military gain.

Sinopoli suggests one means of tying together the two views, by arguing that the forces driving the establishment of new imperial capitals were contingent on a mix of politics and military/logistical requirements, but that the undertaking required potent ideological leadership. One doesn't have to question the legitimacy of Akbar's efforts to reform the role of paramount, however, to wonder how much of the reformation truly transformed leadership and how much was elaborate propaganda. Sinopoli observes that the construction of imperial capitals cost no more than about 5 percent of the annual income of the imperial household, which left immense assets to apply to other ends. It would be intriguing, therefore, to look at the proportions of resources committed to other kinds of enterprises underwritten by the imperial household. In the larger context, it would be useful to know what, if anything, the rest of the population of some 130 million people in the Mughal polity were doing that had an effect on the shifting location of the political centers. Specifically, how did local ideologies play into the imperial conceptions and shifting use of ideology for establishing legitimacy?

In a related vein, Morrison and Lycett set up their discussion on the dynamics of politics in the Vijayanagara polity with the insight that the material record contains within it both claims to and reflections of power. They suggest that we can resolve the distinction in part by evaluating the flows of goods, material, and services against written inscriptions. A key point in the discussion is the apparent dominance by local elites in "gifting" inscriptions, which they interpret to imply that those elites dominated the activity. Although I am not sufficiently familiar with the region's archaeological record to cite alternative evidence, two other explanations for this pattern come to mind. One is that royal activities were represented materially in ways other than "gifting" inscriptions. Second, such local elite activities were part of an effort to assert status, and were not necessarily a reflection of power that they already had. Morrison and Lycett further note that the material record contains dissimulations concerning access to power, communicated through physical expression of relationships. Given the fluidity of Vijaya-

nagara politics, it would be intriguing to understand more about the relationships among the distribution of inscriptions, military activity, marital alliances, and patron-client relations. For example, taking into account that both the utility and threat of a client are proportional to his strength (Luttwak 1976), how was public imagery manipulated by the central authorities and subject elites as the balance of power changed?

Together these issues point to a key question: to what degree and in what ways was the emergence of complexity masked by efforts to hide the unequal status of groups within society (cf. Fried 1967:182)? Were individuals or groups attempting to assert legitimacy or a higher status through manipulation of symbols of rank or power? Morrison and Lycett's paper suggests that this process may well have been going on in Vijayanagara. Sinopoli suggests that blending of multiple sources of legitimacy or reversion to single sources may be seen as a political strategy or as a culturally inspired motivation, within a particular historical situation. As she points out, a plausible case can be made for both uses of symbols of power, using the available data.

These arguments focus our attention on an ongoing dilemma in archaeology: how can we recognize circumstances in which the representation of power was at odds with its exercise? Morrison and Lycett's suggestion that we compare the monumental and written portrayals of power with other material features (e.g., forts, storehouses) provides one fruitful line of inquiry. Part of the answer may also lie in the treatment of gravelots, which form a key data source for Allard and Shelach. Despite their obvious linkages to the extant society, mortuary remains are not a direct reflection of the social order, as Shelach notes (cf. Menzel 1976). Instead, they are a constructed, selective representation by the living social group—at least partially an idealization of social position (O'Shea 1984; Peebles and Kus 1977; Saxe 1970). There may be fairly strong relationships between status and some features of burial treatment, such as energy investment (e.g., Earle 1987). However, the mortuary record is the material context, *par excellence*, to fashion social lies through construction or manipulation of symbols (McGuire 1992; William Macdonald, 1984, pers. comm.). Because social position in increasingly complex society is contested and negotiated (Brumfiel 1992), it would be useful to compare patterning in disposition of status-related goods and investment of labor in two kinds of reconstructed organizations: the habitat of the living and the mortuary record. The disparities between these two contexts may help us to get at how people thought about what was important in their lives and what they actually did on a regular basis. Such an approach may be especially valuable in the Chinese societies discussed here, in which substantial mortuary collections are available.

Together, these considerations indicate that there are notable gains to be made from following Morrison and Lycett's suggestion that we think more explicitly about how to compare symbolic representation (e.g., inscriptions) with changing social organization and behavioral relations. This point, of course, is a matter of great concern to a wide range of archaeologists today, working elsewhere in the world (cf. Cowgill 1993; Hodder 1990). For some authors here, the human agency and contingent meanings behind material constructions are not an issue, partly because the nature of the data currently available unfortunately limits the degree to which such concerns can be addressed at present in Asian archaeology.

However, a few authors of papers in this collection have begun to take on this issue, which shows promise for future work.

ECONOMIC COMPLEXITY

The final recurrent theme among the contributions is the role of specialized economics in increasing social complexity. In part to address this issue, a number of authors draw on Earle's distinction between attached and independent specialists (see Brumfiel and Earle 1987). The former are underwritten by elites largely for sociopolitical ends, whereas the latter arise as household or community producers pursuing a diversified economic strategy. Several authors suggest that specialized production in their research areas is most visible in prestige goods, from which they infer attached specialization. That is, the emergence of at least some specialized production appears to be more closely tied to political activities than to household risk management or targeted use of localized resources by the nearby populace (see Costin 1991).

In her paper on Philippine chiefdoms, for example, Junker suggests that chiefs were heavily responsible for economic reorganization in their domains, in part as a means of consolidating their control over access to such prestige goods. Similarly, chiefs undertook military action in large part to control maritime trade in exotic sumptuary goods. I wonder, however, if the elites were responsible for, say, utilitarian ceramic production, or were they focused primarily on targeted resources for use in the political economy? That is, is there room for economic reorganization started at the household or kin group level, in response either to opportunity or to elite demands?

An emphasis on the importance of attached specialization is worthwhile, but we could also benefit from looking more closely at the varied relationships between *producers* and *consumers* that contributed to defining access to resources and structuring labor relations in the political economy (Rice 1978). For example, even in production fostered by elites, specialization may be reflected in radically divergent trends in craft production. Among them are (1) elaboration of products and labor intensiveness, which may be a consequence of elite consumption (Costin 1986; Hagstrum 1986), and (2) simplification of the product, which results from mass production (Feinman et al. 1984; Johnson 1973). These two extremes illustrate that the organization of labor and access to products are dependent upon complex relations between producers and consumers and that attached specialization encompasses widely divergent organizational trends.

The use of prestige goods, as contrasted to their production, is similarly a consistent issue for authors. As they observe, one of the best ways to recognize the presence of elites is by identifying concentrations of exotic materials, labor-intensive goods, and symbols of apparent status. Allard emphasizes this point, by treating the changing distribution of bronzes, which were visual expressions of power, as a window into the changing landscape of economic relations between a periphery and successive empires whose elites were interested in obtaining materials unavailable in their home territories. His essential point is that the spatial location and use of culturally foreign markers of status in the Lingnan Culture are partially a consequence of interaction between the empire and well-situated peripheral elites.

Such a concern with prestige goods is warranted, but a number of researchers have pointed out that the limited resource in economic power in increasingly complex society may be as much labor as productive resources or material goods (e.g., Fried 1967; Price 1984). Junker's treatment of Philippine chiefly feasting draws attention to this issue, by discussing the relationship among long-distance exchange and political competition (local and regional), within which public ceremonial celebrations were an important element. I wonder, however, to what degree the luxury goods trade helped form power differences or elaborated differences established on the basis of other factors, such as the ability to mobilize labor for warfare or for subsistence production. It may be useful to think of the proliferation of these items as a tool used to attract supporters in the context of negotiated political relationships (Wright 1984) and not principally as a means of asserting dominance. The ability to amass such goods is an advertisement of potency, with which people may choose to ally themselves.

This point brings the discussion to a difficult, but critical, issue in studying the economics of complex prehistoric society: how to assess mobilization of labor for activities that did not have an obvious (or maybe proportionate) material consequence. With apologies, I return to Andean material for exemplary material. In 1549, the Chupaychu of Peru's northern highlands reported to the Spaniards that only 1 percent of the personnel mobilized for state labor duty was committed to making pottery and only 10 percent to building or maintaining the physical infrastructure (Helmer 1955–1956). That is, only 11 percent of labor duty was assigned to activities that produced almost all of the remains that archaeologists rely on. Among the 35 other named duties were agricultural, mining, weaving, and military service. Tellingly, about 40 percent of the labor was committed to activities that would not have yielded readily visible material remains (e.g., guard duty, household service, and portage).

It seems likely that a similar creation of new labor statuses was key to the development of social complexity in many of the societies discussed here. Although getting at the problem of labor mobilization is elusive, several kinds of material evidence can be adduced. Among them are mass-produced serving vessels (Johnson 1973); massive storage facilities, potentially associated with administrative architecture (e.g., LeVine 1992; Morris 1967, 1972); barrackslike architecture at state settlements (e.g., Morris and Thompson 1985); specialized production centers making products in distinctive state styles (e.g., Espinoza Soriano 1975; Lorandi 1984); and areas of large-scale production associated with elite residential sectors in urban contexts (e.g., Topic 1990). None of these kinds of approaches is new, but a fuller understanding of the complex balance between labor organization and production and use of material goods is an area of study that promises to yield considerable insight into the Asian societies described here.

CONCLUDING COMMENTS

In sum, these collected papers are useful in advancing our understanding of the nature of complex early Asian society. They provide both substantive discussions of particular cases and welcome balances to the region-bound explanations that have predominated in much of the literature. As indicated in the papers, however, the particularities of each situation need to be accounted for in some way

as well. The interpretive challenge is to balance the particulars of the case studies with broader anthropological concepts. Reconciling the two is a daunting task, and it is likely that any given characterization will be supplanted in the near future. As we expand the scope of our comparative analyses, however, it is in the balance that we will gain a clearer understanding. I therefore encourage both sides of what the contributors to this collection have presented and look forward to continued work in the same vein.

NOTES

1. As an outsider who works in the Americas, I have found it edifying to consider how researchers investigating regions with different histories of inquiry approach problems of regional prehistory. I would like to thank Kathleen Morrison for the opportunity to comment on the papers in this collection. In the comments that follow, I appeal to the forbearance of specialists in the field of Asian prehistory for any ignorance resulting from my lack of detailed familiarity with regional culture histories.
2. In addition, sampling problems are present in virtually all of the studies described here. It is not clear to what degree the material studied represents the full range of materials about which the authors wish to draw conclusions. This is a problem inherent in working with secondary sources, and, much to their credit, several authors are explicit about the limitations of their samples. As they point out, this situation calls for more of the tedious basics of fieldwork—survey, chronology, and site description in every region discussed.

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