
HOW DO ARCHAEOLOGISTS COMPARE EARLY STATES?

Michael E. Smith

Trigger, Bruce G. *Understanding Early Civilizations: A Comparative Study*.
New York: Cambridge University Press, 2003. xiii + 757 pp.

Smith, Adam T. *The Political Landscape: Constellations of Authority in Early
Complex Polities*. Berkeley: University of California Press, 2003. xv + 331 pp.

Bruce Trigger's and Adam Smith's comparative studies of state-level societies provide new theoretical approaches and are important components in a resurgence of explicit comparative analysis of early states by archaeologists. Trigger presents a massive systematic comparison of seven ancient states on an unusually large number of themes, whereas Smith carries out more intensive comparisons of a smaller sample on more limited themes. These well-written works make significant contributions to a number of areas, including empirical analysis, theory, and comparative methods.

Keywords: States, complex societies, ancient civilizations

These outstanding books signal a return to true comparative analysis by archaeologists working on ancient state societies. Comparative studies were common during the height of the New Archaeology, when “the rise of the state” was a dominant research issue. Then, starting in the 1980s, postprocessual approaches discouraged comparative approaches, and those archaeologists who did compare early

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states limited themselves to article-length works. Collections of essays loosely structured around common themes were passed off as comparative studies, and research on early states suffered. The two books under discussion join other recent single-author works (Blanton 1994; Scarborough 2003; Yoffee 2005) that are helping to re-establish a true comparative approach to the archaeology of early state societies. In this article I discuss the two books individually and then use them as points of departure to explore issues in the comparative analysis of early states.

BRUCE G. TRIGGER'S *UNDERSTANDING EARLY CIVILIZATIONS*

Archaeologists, particularly those working on ancient civilizations, are often perceived as particularists, spending all their time on potsherds or inscriptions while ignoring larger social issues. Bruce Trigger (2003), on the other hand, boldly takes on the big issues of society and history. Here is the opening sentence of the book: "The most important issue confronting the social sciences is the extent to which human behavior is shaped by factors that operate cross-culturally as opposed to factors that are unique to particular cultures" (p. 3). In *Understanding Early Civilizations*, a massive tome of 757 pages, Trigger presents archaeological and historical data on seven of the best-documented early state societies in order to probe the issue on an empirical level. Although he finds numerous cross-cultural similarities in his survey, his conclusions differ greatly from those of most scholars who have considered these data.

Bruce Trigger has always been something of a maverick among archaeological theorists, staking out his own approach to the past that emphasizes historical context and materialist factors with an appreciation for the social construction of knowledge, both in ancient societies and among modern scholars. He never jumped on the New Archaeology bandwagon, preferring a more historical approach to the past (Trigger 1976) that contrasted with the rejection of history by Lewis Binford and other New Archaeologists. Trigger was an early critic of the simplistic claims of many New Archaeologists and one of the first to point out the importance of the social context of archaeological knowledge today (Trigger 1978, 1984). Although Trigger's work anticipated many strands of the postprocessual approach that took root in the 1980s (Hodder 1986; Shanks and Tilley 1987), he was quick to criticize that work for its relativism and lack of methodological and empirical rigor (Trigger 1991). His book, *A History of Archaeological Thought* (Trigger 1989), is widely

acknowledged as the definitive work on the subject, and his more recent work, *Sociocultural Evolution* (Trigger 1998), can be seen as an extension of some of the earlier work's themes.

Trigger has a long-standing interest in the comparative analysis of early states. In 1972 he published the first archaeological application of a new functional approach to urbanism (functional in the sense of investigating economic and spatial functions of cities, not in the sense of functionalism; Trigger 1972). He later updated his discussion in an important comparative paper (Trigger 1985) published in a hard-to-find collection. In 1992 he delivered a series of lectures at the American University in Cairo in which he placed ancient Egyptian civilization within a comparative context; this was soon published as a slim volume (Trigger 1993). The present book is an updated and greatly extended version of that work.

Understanding Early Civilizations contains 27 chapters. Most are devoted to individual social themes organized into three major sections: sociopolitical organization, economy, and cognitive and symbolic aspects. Four chapters constitute an introductory section, and there are two concluding chapters. The introductory chapters set out Trigger's intellectual and methodological approach. Chapter 1 ("Rationalism and Relativism") situates archaeology within the wider context of long-standing theoretical debates in the social sciences. Trigger concludes that "the challenge is to stop simply supporting one or the other of these alternative positions in a partisan manner and examine in greater detail the nature of cultural similarities and differences as the basis for constructing a more realistic theory of the factors shaping human behaviour and cultural change" (p. 11). Chapter 2 ("Comparative Studies") begins with a historical review of cross-cultural approaches in anthropology, from Herbert Spencer through the holocultural comparisons associated with the Human Relations Area Files. Trigger then covers archaeological comparative research on early states, criticizing recent work for its unilinear perspective on change (all societies followed the same path of development) and its homogenizing approach (all early states were the same); the targets of his critique include some of the leading neo-evolutionist scholars working today.

Chapter 2 also contains Trigger's rationale for selecting the seven cultures that constitute his sample for comparative analysis (see Table 1). He selected cultures from a variety of world regions that meet three criteria: (1) abundant archaeological data; (2) some level of written documentation, whether an indigenous script or written descriptions by outsiders; and (3) indigenous development independent of outside control or heavy influence. Trigger departs

Table 1. Samples for comparative analysis

Culture	Period	Political form
Trigger		
Egypt	Old and Middle Kingdoms	territorial state
North China	Late Shang to Early Western Zhou	territorial state
Inka	Inka	territorial state
Mesopotamia	E. Dynastic III to Old Babylonian	city-states
Aztec	Late Postclassic	city-states
Maya	Classic	city-states
Yoruba and Benin	Late Precolonial	city-states
Smith		
Maya	Classic	
Urartu	Iron Age	
Mesopotamia	Ur III to Old Babylonian	

from several decades of archaeological orthodoxy in which “pristine” or “primary” states are singled out for emphasis in comparative analyses (Spencer and Redmond 2004; Wright 1977). He accuses scholars such as Joyce Marcus and Henry Wright of making the unsupported claim that “all first-generation early civilizations differed in some striking fashion from all later ones” (p. 29). He feels that anthropological archaeologists “have erred in trying to explain changes without first seeking to understand how what is changing functioned” (p. 13). In other words, we need to determine how ancient states worked before we can figure out how and why the first states developed. The way to do this, Trigger argues, is through the method of controlled synchronic comparison. Although his synchronic perspective has already been criticized (Adams 2004), Trigger is quite explicit about his methods and spends much of the first four chapters justifying his approach (Table 1).

In Chapter 3 (“Defining ‘Early Civilization’”) we are told, “[e]arly civilization’ can thus be summarily defined as the earliest and simplest form of class-based society” (p. 46). Most archaeologists prefer to call these societies states, given the cultural baggage of value judgments and progressivism associated with popular uses of the term *civilization*. Trigger seems to want to reserve the term *state* for the form of political organization found in his early civilizations. This is a thorny issue; there are problems with both terms, leading Adam T. Smith to reject them both in favor of *early complex polities* (see below). Trigger reviews briefly the concept of social class and the Asiatic mode of production (“a byproduct of inadequately documented nineteenth-century speculation,” p. 52).

Trigger completes his lengthy and entertaining setting of the scene in Chapter 4 (“Evidence and Interpretation”). Variation in the quantity and quality of data bedevils all comparative analyses, and Trigger discusses the archaeological and historical evidence frankly and clearly. He is not an expert in the archaeology of any of his seven cases, and it goes without saying that a work like this will have numerous details that experts can critique. Nevertheless, I am pleasantly surprised, even amazed, at how well he commands the data on the Aztecs, my own specialty. He seems to know a good deal of the primary literature, and he uses the secondary literature well, producing few mistakes of fact or interpretation.¹ Chapter 4 also considers the nature of scholarly biases in the analysis of ancient states, from the “dogmatic misconceptions” (p. 59) of ideologically motivated mid-twentieth-century scholars such as Wittfogel and Polanyi to the regional scholarly biases that affect all seven of his case studies. A discussion of the emic and etic approaches rounds out the chapter. These four chapters should be required reading for scholars and students interested in the comparative analysis of ancient societies.

Most of the twenty three substantive chapters are arranged thematically, and within each theme relevant material on each of the seven case studies is reviewed, typically followed by a synthesis. For example, Chapter 5 (“Kingship”) has sections on “Qualifications for Kingship,” “The Sacred Character of Kingship,” and “Validating Power.” The strong attention to empirical data and the constant shuffling through the pack of seven cultures, several times in each chapter, makes the reading slow going at times. Indeed, there is enough information presented that one could reorganize the book to produce short monographs on each of the seven case studies plus a concise work on comparative analysis.

Every one of the twenty-three substantive chapters in *Understanding Early Civilizations* contains interesting and insightful points. There is not space to discuss all of the chapters, so I will list them (to give an idea of how Trigger organizes his comparisons) and single out several for comment. The section “Sociopolitical Organization” contains eight substantive chapters: Chapter 5, “Kingship;”

¹Just to establish my credentials as a cranky specialist, here is my list of Trigger’s errors on the Aztecs: (1) he claims incorrectly that nobles did not pay taxes (p. 151); (2) his characterization of the *calpolli* as an “endogamous, collective landowning group” (p. 178) is outdated; (3) he relies too heavily on June Nash’s (1978) speculative and often incorrect account of gender relations; and (4) his discussion of private property in land (pp. 322, 332) is outdated and misleading. Given the detail of his overall discussion of the Aztecs and other case studies, this is a remarkably short list. Most of these problems arise from his failure to use one of the fundamental sources on Aztec social organization, Lockhart (1992).

Chapter 6, “States: City and Territorial;” Chapter 7, “Urbanism;” Chapter 8, “Class Systems and Social Mobility;” Chapter 9, “Family Organization and Gender Roles;” Chapter 10, “Administration;” Chapter 11, “Law;” and Chapter 12, “Military Organization.”

One of the key features of the whole book is Trigger’s classification of his case studies into two political types: city-states and territorial states (see Table 1). City-states were small polities consisting of an urban capital and surrounding farmland and smaller settlements; they tended to occur in groups of interacting polities that shared a language and culture. The rulers of territorial states controlled much larger areas through administrative hierarchies and a larger number of urban centers. This is an important distinction for Trigger, and it structures much of the discussion in the remainder of the book. Indeed, one of Trigger’s major findings is that city-states and territorial states differed systematically in numerous important ways. His results provide support for one position in a current archaeological debate over the city-state as a form of political organization. In the views of some scholars, the city-state was a limited form in ancient times, with very few cases outside of the well-known Classical Greek *poleis* (Marcus 1998; Marcus and Feinman 1998). Others see the city-state as the basic or normal form for all or most ancient civilizations (Nichols and Charlton 1997). The middle position, as argued most explicitly by Hansen (2000b, 2002), defines the city-state carefully and identifies a number of ancient civilizations as the settings for city-states and city-state cultures, in contrast to other ancient state forms, such as territorial states, empires, and various forms of weak states (M. E. Smith 2004). Trigger’s data support Hansen’s view of city-states.

The eight chapters listed above are followed by a synthetic comparative chapter, “Sociopolitical Constants and Variables.” Trigger concludes, “[t]here was considerable cross-cultural uniformity in the sociopolitical institutions of early civilizations” (p. 272). In addition to two types of polity (city-states and territorial states), Trigger identifies two basic forms of territorial administration (delegational and bureaucratic). He also finds similar processes of increasing male dominance in the early states in comparison with their predecessors. To Trigger, “[m]any uniformities may reflect functional requirements” (p. 272). It is worth quoting his concluding statement from this chapter in full:

In drawing attention to cross-cultural regularities in many major features of sociocultural organization in early civilizations and to the limited range of difference in other features, I am not seeking to impose

undue uniformity on the data. Kingship had a different specific meaning in every early civilization, as did slavery and the concept of an upper class. Yet these differences cannot be allowed to obscure the great similarities in sociopolitical organization that early civilizations came to share as a result of convergent development. To ignore these similarities out of loyalty to hoary dogmas of cultural relativism or historical particularism would be as misleading as to ignore cultural differences in the name of unilinear cultural evolutionism. [pp. 274–275]

The second major substantive section in *Understanding Early Civilizations* is “Economy.” It is divided into the following chapters: Chapter 14, “Food Production;” Chapter 15, “Land Ownership;” Chapter 16, “Trade and Craft Specialization;” and Chapter 17, “Appropriation of Wealth.”

Given the generally low level of sophistication that most archaeologists apply to the economic analysis of ancient states (M. E. Smith 2004), Trigger’s rigorous discussion of economic topics is welcome. Again, this section of the book closes with a synthetic chapter, “Economic Constants and Variables.” In opposition to mid-twentieth-century discussion of the rise of states by Wittfogel, Steward, and their followers, Trigger reports that “Unexpected variation has been found amongst these societies, however, in environmental settings, population density, intensity of agriculture, and the geographical mobility of people” (p. 395). His data support the notion that population pressure causes agricultural intensification, but not the extension of this argument that posits population pressure (or intensification) as causes of political centralization or urbanization. Trigger’s discussion of this issue, a long-standing area of research and debate in archaeology, would be better if he had cited the recent literature on intensification theory (e.g., Morrison 1994; Netting 1993). He notes that “Interpretations of the economies of early civilizations were long distorted by Karl Polanyi’s claim that in these societies trade and production were completely ‘embedded’ in political organization” (p. 402; see also pp. 59–60), a point with which I fully agree (M. E. Smith 2004). Trigger closes this chapter with insightful observations on the nature and social significance of conspicuous consumption in ancient states.

The third and final major section of Trigger’s book is “Cognitive and Symbolic Aspects,” and it has the following chapters: Chapter 19, “Conceptions of the Supernatural;” Chapter 20, “Cosmology and Cosmogony;” Chapter 21, “Cult;” Chapter 22, “Priests, Festivals, and the Politics of the Supernatural;” Chapter 23, “The

Individual in the Universe;” Chapter 24, “Elite Art and Architecture;” Chapter 25, “Literacy and Specialized Knowledge;” and Chapter 26, “Values and Personal Aspirations.”

Most comparative discussions of early states by anthropological archaeologists give little attention to these topics—except perhaps ritual and public architecture—focusing instead on social, political, and economic phenomena. Treatment of these issues for early states tends to be particularistic, not comparative. Trigger’s analysis in this section thus forges new ground. In these chapters he takes many well-known authors and viewpoints to task. He is strongly critical of the heavy use of Mircea Eliade’s work by archaeologists who study ancient cosmology: “his general ideas seem to have been applied too dogmatically” (p. 470), and “his ideas may have become self-fulfilling,” (p. 445). He calls the notion of power struggles between priests and secular authorities in ancient states a “fantasy” (p. 495): “The idea of priests and kings as rivals and of competition between church and state in early civilizations is not supported by the evidence. Concepts that seemed appropriate for the understanding of the history of medieval Europe have been misapplied to these societies” (p. 507). His discussion of writing systems, distinguishing semasiographic systems from true scripts, is outstanding, leading him to criticize Jack Goody’s notions: “[w]riting in the early civilizations did not produce the intellectual transformation that Goody (1986) associates with it” (p. 597).

Like the previous sections, this one closes with a comparative chapter, “Cultural Constants and Variables.” Trigger is struck, even surprised, by the degree of similarity among his seven cases in the realm of religion and symbolism. Although the particulars differ greatly, he concludes:

There appears to have been only one major view of the operation of the supernatural. Far from being an epiphenomenal reflection of culture as an adaptive system, this view appears to have been the convergent outcome of profound reflections on how class societies served the practical needs of their members. In early civilizations, religion was not only the main locus of symbolic production but also the principal medium for vital discussions and debates on political issues.
[p. 650]

One component of the religious worldview of these cultures was the notion that the gods required the active support of people, through offerings and sacrifice. Trigger contrasts this notion with the religious

views of small-scale societies on the one hand, and with the later transcendental religions on the other (p. 484). He suggests that “[c]ross-cultural uniformities in the religious beliefs of early civilizations resulted from analogous reflections on the taxation systems that were generically common to all these societies. Sacrifice projected the taxation system into the cosmic realm” (p. 643).

Chapter 28 (“Culture and Reason”) contains the major conclusions and implications of *Understanding Early Civilizations*. Trigger returns to the theoretical dichotomy with which he began (rationalism versus romanticism), noting the cyclical alternation of the popularity of the two viewpoints over time. He argues for the fundamental importance of two very different forms of empirical pattern: the unique and integrated configuration that characterizes each human society, and the cross-cultural regularities that characterize sets of societies (e.g., ancient states). The unique patterns of individual cultural traditions are celebrated by idealists and postmodernists, and dismissed by modernists such as the New Archaeologists: “[a] powerful tendency, derived from functionalism, nevertheless persists to dismiss cultural traditions as non-explanations of human behavior” (p. 660; Trigger cites Lewis Binford here). Trigger argues for the ecological importance of cultural traditions, citing Boyd and Richerson’s (1985) dual inheritance model of sociocultural evolution, and suggests the following:

This argument calls into question the notion that it is practical reason that accounts for cross-cultural uniformities and cultural reason that explains cross-cultural particularities. Ecological patterns are adaptive precisely because they are variable from one natural setting to another. Other idiosyncratic cultural patterns persist not because stylistic traits are selectively neutral but because they are selected for internal consistency in situations that relate to identity, cooperation, and psychological well-being rather than to serve a cross-cultural goal. [pp. 660–661]

Trigger then summarizes his findings on cross-cultural regularities, noting numerous specific and general parallels, from craft production systems to divination. He concludes, “[t]here is no sector of early civilizations that did not display a significant combination of cultural idiosyncrasies and cross-cultural regularities. Such findings do not correspond with the expectations of either neoevolutionists or cultural determinists” (p. 674). Bruce Trigger has not only provided a massive empirical and theoretical survey of early states, but he has

also succeeded in placing his analysis, and the seven case studies, firmly within the major theoretical debates of our time.

ADAM T. SMITH'S *THE POLITICAL LANDSCAPE*

The concept of “*landscape*” has become ubiquitous in archaeology today. Some archaeologists analyze historical landscapes, symbolic landscapes, ritual landscapes, and ethnic landscapes (Anschuetz et al. 2001:172–181); others focus on clearly defined landscapes, organically evolved landscapes, and associative cultural landscapes (Cleere 1995). For some, there are constructed landscapes, conceptualized landscapes, and ideational landscapes (Knapp and Ashmore 1999: 10–13). The “L” word can also come first: landscape as memory, landscape as identity, landscape as social order, and landscape as transformation (Knapp and Ashmore 1999:13–19). All kinds of archaeologists, from ecologists to phenomenologists to archaeoastronomers, are rushing to use the term in their work, or at least in the titles of their works (here are just a few recent archaeology books with “landscape” in the title: Alcock 2002; Ashmore and Knapp 1999; Koontz et al. 2001; Ruggles et al. 2001; Scarborough 2003; Silverman and Isbell 2002; Wilkinson 2003). It seems that for many archaeologists, all the world’s a landscape, and for some, the term is poised to take over as the dominant concept in archaeology (Anschuetz et al. 2001).

Some of us, however, remain less than enthusiastic about the grand claims of the landscape movement in archaeology. In many cases, landscape is a code word signaling an archaeologist’s adherence to the “social archaeology” approach (Preucel and Meskell 2004). Other archaeologists use the phrase “landscape” as a buzz word—it sounds good in a title, but doesn’t signal any change in one’s theoretical orientation, methods, or approach (I must admit to my own guilt here; see M. E. Smith 1995). Finally, landscape continues to be used as a technical term in geomorphology and geoarchaeology.

Is Adam T. Smith’s book, *The Political Landscape* (2003), just another rider on the landscape bandwagon? Fortunately, the answer is an emphatic “no.” This is a major work of comparative archaeology, extremely well grounded in theory and data. It makes numerous important contributions to our understanding of early states, and also places the study of those polities within the framework of contemporary political theory and philosophy. His argument is rich, sophisticated, and nuanced, and it would take a longer essay than the present one to comment on all aspects of this rewarding work. Much of the thought is original and creative, and it is worth laying out Smith’s conceptual approach in some detail.

This is not a book about landscape per se but rather a book about political dynamics and organization. In Smith's view, polities—particularly ancient states—cannot be understood without coming to terms with ways their rulers use, shape, and create space and spatial relationships, and “landscape” provides a concept for approaching those relationships. Although Smith draws on the extensive social science spatiality literature (de Certeau 1984; Lefebvre 1991), his work does not fit comfortably with much of the work in that genre. Compared with that approach, his book is at the same time broader, in terms of anthropological and political theory, and narrower, in terms of its focus on empirical archaeological and historical data. Smith is not trying to answer the grand questions of society and space, but instead to use space (and landscape) to answer some of the grand questions of the political dynamics in early states. This strong focus on the political is what makes *The Political Landscape* such an important work. Although there are some quite high-level discussions of theory and philosophy, the entire enterprise is strongly problem oriented, never losing sight of the focus on early states.

Adam Smith begins in Chapter 1 (“Sublimated Spaces”) with a three-part classification of the ways archaeologists (and others) have dealt with space in theory and practice: absolute space, subjective space, and relational space. Most of the chapter is concerned with demonstrating the inadequacies of the first two concepts. Much traditional archaeological research holds to an absolute view of space:

Although social evolutionism is, above all, a theory of time, of the shape, pace, and direction of history, its foundational conceit—that world history may be understood under the rubric of a unified law of social change—is predicated on the reduction of space to a social constant. That is, variations in space must be insulated from affecting social transformations so that explanatory power may be vested exclusively in the temporal axis. If space were to hold the potential to shape the course of future transformations, then temporal variation would be difficult to define in universal terms. Space, within an evolutionary approach to social life, must be described as absolute. [p. 34]

Smith isolates two types of absolute space as common in archaeological studies of early states, mechanical absolutism and organic absolutism: “The mechanical absolutist position holds that space

has no effect on historical process, and as a consequence, spatial variables are largely irrelevant to historical explanation” (p. 36). The goal of spatial analysis in this approach is to illuminate the fundamental geometries of society. He traces this view from geographical positivism, through the work of the nineteenth-century evolutionists, to more recent archaeological approaches such as central place theory, the use of settlement hierarchies to identify states (e.g., Spencer and Redmond 2004), and attempts to identify specific building types as markers of the evolution of states (e.g., Flannery 1998). Smith associates this approach with consensual or integrative theories of state formation and suggests that “in their tendency to find regularity in settlement distribution, locational approaches have the effect of an *ex post facto* legitimation of political authority, dismissing the vagaries of power, domination, and hegemony under the banner of a naïve contractarianism” (p. 43).

An alternative “organic absolutist” view of space originated in nineteenth-century organic analogies for society: “The role of analyses of space within an organic absolutist ontology is to define determinative processes organizing the spatial relationships between components. Foremost among these organic determinative process in the twentieth century has been adaptation” (p. 36).

This has been a popular position in archaeology, arising out of ecological approaches to culture and society. Among its adherents Smith singles out Julian Steward and his archaeological disciples such as Robert McC. Adams and William T. Sanders, and current neoevolutionists such as Allen Johnson and Timothy Earle (2000). He concludes with V. Gordon Childe’s critique of this approach: “Yet even after several decades of organicist accounts of early complex polities, Childe’s observation that ‘men seem to be impelled to far more strenuous and sustained action by the idea of [a] two-headed eagle, immortality, or freedom than by the most succulent bananas!’ (Childe 1964:8) remains a powerful, unanswered, commonsense rejoinder” (p. 53). Smith ends this section with a philosophical critique of absolute space (pp. 53–54), using high-level arguments that I frankly found difficult to follow.

Smith then moves on to critique two versions of subjective space. “Romantic subjectivism” in past scholarly accounts of early states is signaled by appeals to a culture’s “collective spirit or genius” (p. 58); this viewpoint has particularly affected study of the ancient Near East and the Classic Maya. Problems with this approach include (1) spatial essentialism, the assumption that there is a direct relationship between a people and place that determines norms and values; (2) an over-aestheticization of material forms, leading to

judgments of superior and inferior architecture; and (3) “its deeply embedded tendency to naturalize contemporary politics under the rubric of empathic understanding” (p. 60), leading to a celebration and naturalization of the victorious and the dominant. For me, this section was one of the places in the book where I had an “Aha!” reaction. Smith is able to articulate clearly the problems with a number of approaches that I have found inadequate, yet difficult to critique effectively. I have always considered such romantic subjectivist accounts problematic on a gut level, and now Smith has clarified just what is wrong with them. Such “Aha!” moments made reading this book a satisfying and entertaining experience.

The other variant of subjective space, phenomenological neosubjectivism, hits closer to current archaeological practice. Smith criticizes accounts of ancient city planning that articulate “a set of built forms with a culturally specific account of mind,” as well as the phenomenological approach to ancient monuments articulated by Christopher Tilley and other British postprocessualists (Tilley 1994). Such studies assume that affective responses to landscape (natural and cultural) are universal, and that “landscapes exist preloaded with meanings” (p. 65). His major criticisms of this work are that they provide no account of just how spaces become imbued with meaning, and that they portray space as a purely reflective category, ignoring the materiality of the landscape.

Smith then moves on to his own approach to ancient landscapes. He employs three “practical dimensions of landscape” that come into play later in his substantive chapters. *Spatial experience* describes the material practices of the built environment, encompassing both the construction of buildings and the experience of moving through a landscape. *Spatial perception* describes the sensual interaction between actors and physical spaces, and *spatial imagination* is the world of representations, from maps to art to spatial theory and philosophy. Attention to these dimensions produces a “relational landscape,” which, to Smith, is the most productive approach to ancient states. He briefly reviews past work by archaeologists that employs this perspective, notably Colin Renfrew’s peer-polity interaction model (Renfrew and Cherry 1986), and work on Maya cosmology and city planning by Linda Schele and David Freidel (1990) and Wendy Ashmore (1989, 1991). In his view, archaeologists using an absolute space perspective view the large monuments of early states as “symbols of authority,” whereas those scholars following the relational space perspective treat such monuments as “constitutive of authority” (p. 76), a distinction that gets to the heart of his approach.

Chapter 2, “Archaeologies of Political Authority,” focuses on concepts of the state and political authority. Smith is even harder on those evolutionists who emphasize primary states than is Trigger:

As social evolutionist fantasy, the concept of primary states presents the possibility of a handful of historical cases where externalities are sufficiently well controlled such that conditions of study mimic the laboratory, hence the hermetic connotations of the adjective ‘pristine.’ To assume such hermetic conditions falsely demarcates early complex polities as islands, isolated from the less developed world around them. [p. 83]

Smith reviews traditional definitions of the archaic state, and in a section titled “Against the State: An Archaeological Critique” he calls the state “an entirely nebulous object of study without a clear referent” (p. 95), arguing that it is a reification of relations of political authority in complex polities.

This leads him to a discussion of “early complex polities,” his preferred term for what most of us refer to as early states and what Trigger calls civilizations. Acknowledging debates over what constitutes “the political” (is this an aspect of behavior, or a realm of society?) Smith presents his own view:

We can describe the political in terms of practical relationships that are strongly shaped by public forms of civic action. I suggest that four relationships in particular must lie at the heart of how we conceptualize politics:

- interpolity ties, or geopolitical relationships;
- relations between regimes and subjects that forge the polity;
- ties among power elites and their links to grassroots social groups (such as kin groups, occupational associations) that constitute political regimes; and
- relationships among governmental institutions [p. 104].

There are four substantive chapters, one for each of the four topics listed above. Each chapter centers on a single detailed case study that is discussed in terms of experience, perception, and imagination. Chapter 3, “Geopolitics,” examines the nature of relationships among polities in early states; the major example is

the city-state culture of the Classic Maya. In reviewing past work on the topic, Smith singles out Joyce Marcus's "dynamic model" (Marcus 1998) for criticism, claiming that Marcus adheres to an absolutist conception of space: "Like the models of political transformation offered by *The Rise and Fall of the Great Powers* (Kennedy 1987) and the Sumerian King List, the central premise of Marcus's analysis is that the geopolitical organization of the lowlands was organized by a regular temporal cycle of emergence and collapse, rise and fall" (p. 122).

Under the heading "Experience," Smith critiques applications of central place theory to Maya polities, pointing out that this is an economic model inappropriate for addressing political dynamics. He then promotes Simon Martin and Nikolai Grube's (2000) inscription-based model of Maya polities—a model quickly becoming the current orthodoxy in Maya studies—for its focus on the experiential dimension of political dynamics: "Through warfare, marital alliances, and diplomatic exchange, the experiential dimension of the geopolitical landscape was produced and reformed as coalitions were assembled and recalcitrant allies were subdued" (pp. 134–135). In other words, the sociopolitical dynamics documented in the hieroglyphic inscriptions produced the Classic Maya political landscape.

For the "Perception" of the Maya political landscape, Smith identifies three processes: memorialization of rulers in public art; emulation of city layouts and buildings by less-powerful rulers; and authorization, "an aesthetic expression of legitimate empowerment whereby a polity expresses its status as an important feature of the geopolitical landscape" (p. 137). Such authorization processes are manifest to the archaeologist in inscriptions, buildings, and city layouts. Overall, the perceptual dimension describes relations of domination and subjection as they are portrayed in the landscape. "This is not simply an embedding of history within the built environment but also an attempt to use that environment as an instrument in realizing political goals" (p. 139).

The third dimension of geopolitics, "Imagination," is applied to the political claims made by Maya rulers in their inscriptions and art. In this realm, his views come rather close to those of Joyce Marcus (although Smith does not acknowledge this), who has long championed the view of Maya inscriptions as propaganda texts (Marcus 1992). Smith also invokes the Sumerian King List here, another example of political claims that did not at all correspond to the actual geopolitical landscape.

Chapter 4, "Polities," focuses on relations between regimes and their subjects. After a discussion of polities and nation building,

ranging from the ancient Egyptian Narmer palette to Anthony Smith's theories of modern nationalism, the chapter launches into its major case, Urartu. This Near Eastern polity is not on anybody's list of the best-known ancient states, but it has several advantages for the analysis of ancient political dynamics. First, Smith's own field-work focuses on this polity, giving him access to considerable detailed information. Second, the polities of Urartu crystallized in relation (and opposition) to the expanding Assyrian empire, and there are inscriptions from both the Assyrians and the Urartians with political content. An introduction sets the scene and outlines conflicting views of the degree of centralization of the polities of Urartu.

In this chapter, the theme "Imagination" comes first. Smith analyzes a series of stone "landscape inscriptions" detailing the heroic actions of the kings of Urartu in founding fortresses, cities, and fields. He isolates three themes of this corpus: evacuation, the claim that the landscape was empty prior to the arrival of the king; reduction, the attribution of specific buildings to individual kings; and integration, the claim that the king's works improved or integrated the entire kingdom. "Perception" is here concerned with breaking down people's ties to previous polities by "obliterating the built environments of prior political communities" (p. 166) and resettling communities. "Experience" describes the polity's expanded construction of fortresses within its territory: "The political regime, no longer an aloof presence with its center of power high in the mountains, was a much more direct presence defining the polity through regularized surveillance of areas pivotal to the political economy" (p. 175). The synthesis of this chapter argues that the Urartu polity was created not by establishing territorial borders, but "through the experience, perception, and imagination of landscapes that (1) regularize demands of regimes on subjects and (2) legitimate these demands in reference to both sense of place and descriptions of the proper world order" (pp. 181–182).

The title of Chapter 5 is "Regimes," relations between elites and grassroots social groups, and the major topic is urbanism. Smith criticizes major archaeological approaches to urbanism for their absolute spatial orientation, which he traces to V. Gordon Childe. In passing he makes the useful point that in spite of common archaeological opinion, Childe's concept of the "urban revolution" was not about cities but about states. Archaeologists have tended to reify "the City," just as they have reified the state, and the dominant approaches to ancient urbanism have downplayed political dynamics. Smith argues for a change of direction: "[t]he proper object of study, I suggest, is not the City but the political regimes that produce urban landscapes as built environments and imagined places" (p. 189).

The major case study in this chapter is the original “heartland of cities” (Adams 1981), southern Mesopotamia. After reviewing the archaeological data, Smith criticizes past views of Mesopotamian urbanism. He devotes considerable attention to Robert McC. Adams, whose regional-survey-based analyses of urbanization are characterized as upholding an organic absolutism of space. The current major work on early cities in this area, Marc van de Mieroop’s (1999) *The Ancient Mesopotamian City*, is similarly attacked for its portrayal of “a timeless urban space to which authorities fit themselves rather than a dynamically changing urban landscape that is produced, in large measure, by political practices” (p. 201).

The “Imagination” component of urbanism and regime in Mesopotamia is approached through the Epic of Gilgamesh and other early inscriptions. Cities were portrayed as coherent ordered places where kings exercised legitimate authority as allowed by the gods. The discussion of “Perception” focuses on the key features of the urban built environment in the lives of urbanites: city walls, palaces, state temples and neighborhood shrines. Under “Experience” Smith turns to the ways in which cities, particularly Ur, were segmented into zones and sectors by walls and canals. Access analyses of residential neighborhoods at Ur support the notion of a fragmented cityscape. The dissonance between the coherence of imagined cities and the fragmentation of experienced cities is attributed to the deliberate actions of urban rulers.

This discussion leads Smith to the topic of ancient urban planning, and he comes up with the most sophisticated critique to date of the age-old dichotomy between planned and unplanned cities. Urban historians typically refer to “unplanned cities,” that is, cities whose layout does not correspond to a Western-style orthogonal scheme, as having an “organic” layout. Smith counters that, “the ‘organic’ description of irregular cities often mistakes cultural variation in aesthetics for decentralization of urban planning” (pp. 225–226); “the opposition is thus not between the planned and the organic but between various competing plans and their vision of the proper role of political authorities in landscape production” (p. 226). This is a refreshing and insightful discussion that can help reinvigorate the analysis of ancient city planning.

The final substantive chapter, “Institutions,” focuses on the relationships among governmental institutions. Smith’s central task here is to explore further the image of urban and political coherence produced by the regimes discussed in the previous chapter. How can scholars overcome this ancient ideological production to arrive at a better understanding of variation among the different institutions of government? The chapter begins with a review of architecture and city layout

in relation to governmental institutions in Berlin, Washington, DC, and Sumerian Lagash, and then returns to Urartu for the major case study. After a brief section on “Perception,” Smith enters into a lengthy discussion of “Experience” based upon Urartian fortresses. There are good architectural plans of a good number of these complex structures, and Smith employs access analysis to explore the significance of different layouts. His methods focus on access graphs, avoiding the use of access statistics promoted by Hillier and Hanson (1984) and employed by many archaeologists (e.g., Blanton 1994).

Although hampered by weak data on room functions, Smith forges ahead to isolate three types of functional space in the palaces on the basis of his access graphs and other data: bureaucratic/royal, religious/temple, and distributive/economic. Their joint occurrence in each fortress suggests that “[u]nlike Assyrian or Persian royal programs, the entire complex of Urartian institutions seems to have been part of a single, highly integrated governmental package that followed conquest and occupation” (p. 249). Although this sounds reasonable, Smith’s argument would be stronger if comparable data were available from the other empires. Under “Imagination,” the focus turns to inscriptions in stone and other materials with representations of fortresses. Compared to the Urartian inscriptions analyzed in Chapter 4, these fortress images differ in narrative structure, aesthetic style, and ideological content. Smith concludes that the “landscape inscriptions” of Chapter 4 were messages from the king, whereas the fortress images presented here were messages from religious authorities signaling their authority and their adherence to the king: “the differing ideological programs for securing legitimacy are the products of distinct institutions within the governing regime seeking legitimacy for Urartian political authority in terms most favorable to its factional status” (p. 267).

A concluding chapter starts with a short analysis of a boast by Timur, an early king of Samarkand. “If you doubt our might—look at our buildings.” Smith reviews three ways this statement can be interpreted, recapitulating approaches archaeologists have used to the relationship between political authority and the built environment. This brief example signals one of the major strengths of *The Political Landscape*: Adam Smith’s ability to get to the heart of various theoretical positions and distill their content and implications in clear language. He moves on to advocate more comparative analyses of early states and then explores some of the implications of his research for understanding political dynamics in modern nation-states. Finally, Smith reviews and justifies some of the choices he made in developing his theoretical approach to early states; these

include “the modest analytical position that I have accorded the natural environment” (p. 278), the central emphasis given to political authority, and an avoidance of issues of resistance. He justifies the latter choice as a result of his emphasis on ideological production rather than ideological reception. The book closes by recapitulating the relevance of early states for larger questions of history and society, and a plea for the value of history.

THE COMPARATIVE ANALYSIS OF EARLY STATES

Understanding Early Civilizations and *The Political Landscape* are both comparative analyses of early states, and each author argues explicitly and repeatedly for the need for additional comparative works, yet their approaches to comparison are very different. One can portray methods of cross-cultural comparison along a continuum from systematic to intensive. Systematic comparisons involve many case studies, carefully selected through sampling, analyzed with rigorous attention to consistently defined variables; the field of holocultural cross-cultural research² associated with the Human Relations Area Files provides the best examples of the systematic approach to comparison (Burton and White 1987; Ember and Ember 2001). Intensive comparisons are more subjective analyses of a small number of case studies in which the contextual setting of each is given priority. Adam Smith explicitly positions his book toward the intensive end of the continuum: “The book is intended to help resuscitate a genre of anthropological writing that explores material in a comparative spirit without yielding to the reductionist tendencies that tend to cripple many such works. Thus, it was critical that each case be allowed to develop in its own right without the compression that results from traditional comparison” (p. 28).

²To most anthropologists, the phrase “cross-cultural” refers to comparisons among distinct cultures (Ember and Ember 2001; Fox and Gingrich 2002). A new definition of the phrase has emerged in recent years, however, that refers not to comparisons of cultures, but to interactions among people belonging to different cultures. This usage is now found in the fields of international business (Gannon and Newman 2002) and multicultural studies (Reynolds and Valentine 2004), as well as among postmodern scholars in a number of disciplines (Guth 2003), including anthropology (Mutua and Swadener 2004). In the field of international management, for example, the concept *cross-cultural* refers to three topics: research done in the United States that is applied to other countries, research done in other countries, and research on multicultural social groups (Gannon and Newman 2002:217). This is a prime example of the problems caused by disciplinary isolation in the human sciences (Wallerstein 2003).

Trigger's work, on the other hand, falls somewhere in the middle of the comparative continuum. His discussion has far fewer cases and far more context than holocultural comparisons, but it is far more systematic and schematic (or, as some might say, reductionist) than a study like Smith's.

How do these books compare to other comparative studies of early states? Table 2 is a list of books (monographs and edited collections) since 1966 that compare three or more early states. One way to examine these comparisons is derived from the continuum mentioned above. Trigger's *Understanding Early Civilizations* is the latest in a series of books that make complete comparisons (that is, comparisons of all or at least many aspects of the societies in the sample) of a small number of cases; these are category 1 in Table 2. Smith's *The Political Landscape*, on the other hand, focuses on a more reduced social realm (political dynamics), but also compares a small number of cases. More common are books that use a moderate or moderate-to-large sample of early states (ca. ten or more) to either make a wide variety of social comparisons (category 3) or else to focus on a more limited social domain (category 4). Holocultural statistical comparisons (category 5) will be discussed below, and books included under "Haphazard comparisons" (category 6) are not really comparative works at all; their nonanthropologist authors either juxtapose different cases without comparison, or else they toss in some exotic examples now and then without theoretical justification.

Fully two-thirds of the comparative works in Table 2 (categories 1–5) are edited collections. This observation alone suggests the sorry condition of the comparative analysis of early states during the 1980s and 1990s. As recently pointed out by Richard Blanton (2002), the mere act of gathering together case studies within the covers of a single volume does not constitute comparative analysis. What is required is a strong and clear position paper, the explicit reference to the concepts or models in that paper by the contributors, and a synthesis that actually compares the case studies in each chapter. Unfortunately, not many of the edited collections in archaeology hold to this standard. Editors rarely take a strong enough stance to enforce a common structure on independently minded chapter authors. However, those few collections that do achieve a high level of integration, such as Hansen's city-state project (Hansen 2000a, 2002), make a greater contribution to knowledge than the typical archaeological edited volume. Still, the books by Trigger and Smith stand head and shoulders above the mass of poorly integrated collections that for too long have passed for comparative analysis in archaeology.

Table 2. Approaches to the comparative analysis of early states

Category and title	Citation
1. Small sample, complete comparisons	
<i>Understanding Early Civilizations</i>	Trigger 2003
<i>Early Civilizations of the Old World</i>	Maisels 1999
<i>Early Civilizations: Ancient Egypt in Context</i>	Trigger 1993
<i>The Rise of Civilization</i>	Redman 1978
<i>Origins of the State and Civilization</i>	Service 1975
<i>The Evolution of Urban Society</i>	Adams 1966
2. Small sample, limited-domain comparisons	
<i>The Political Landscape</i>	A. T. Smith 2003
<i>War and Society in the Ancient and Medieval Worlds</i>	Raaflaub and Rosenstein, eds. 1999
<i>Archaeological Views from the Countryside</i>	Schwartz and Falconer, eds. 1994
<i>Ideology and Pre-Columbian Civilizations</i>	Demarest and Conrad, eds. 1992
<i>The Collapse of Ancient States and Civilizations</i>	Yoffee and Cowgill, eds. 1988
<i>Early Civilization and Trade</i>	Sabloff and Lamberg-Karlovsky, eds. 1975
3. Moderate sample, varied comparisons	
<i>Complex Polities in the Ancient Tropical World</i>	Bacus and Lucero, eds. 1999
<i>Archaic States</i>	Feinman and Marcus, eds. 1998
<i>Ideology and the Formation of the State</i>	Claessen and Oosten, eds. 1996
<i>Power Relations and State Formation</i>	Patterson and Gailey, eds. 1987
<i>Early State Dynamics</i>	Claessen and van de Velde, eds. 1987
<i>Early State Economics</i>	Claessen and van de Velde, eds. 1991
<i>The Study of the State</i>	Claessen and Skalnik, eds. 1981
<i>The Early State</i>	Claessen and Skalnik, eds. 1978
4. Moderate to large sample, limited-domain comparisons	
<i>The Flow of Power: Ancient Water Systems and Landscapes</i>	Scarborough 2003
<i>The Archaeology and Politics of Food and Feasting in Early States and Empires</i>	Bray, ed. 2003
<i>Empires</i>	Alcock et al., eds. 2001
<i>A Comparative Study of Thirty City-State Cultures</i>	Hansen, ed. 2000a
<i>Order, Legitimacy and Wealth in Early States</i>	Richards and van Buren, eds. 2000
<i>The Archaeology of City-States</i>	Nichols and Charlton, eds. 1997
<i>Heterarchy and the Analysis of Complex Societies</i>	Ehrenreich et al., eds. 1995
<i>The Collapse of Complex Societies</i>	Tainter 1988
5. Holocultural statistical comparisons	
<i>Encyclopedia of Prehistory</i>	Peregrine and Ember, eds. 2001
<i>Houses and Households</i>	Blanton 1994
6. Haphazard comparisons	
<i>Ancient Cities</i>	Gates 2003
<i>Visions of Power: Ambition and Architecture from Ancient Rome to Modern Paris</i>	Tinniswood 1998
<i>The Idea of a Town</i>	Rykwert 1976

Note: This table includes books and edited collections since 1966.

A recent development in the comparative analysis of early states is the expansion of the holocultural approach into the archaeological world. Archaeologists have long made use of holocultural cross-cultural data for purposes of analogy and material culture correlates (Ember and Ember 1995; Peregrine 1996), but now the Human Relations Area Files are in the process of compiling archaeological data for cross-cultural research. This project, under the direction of Peter Peregrine and Melvin Ember, has assembled standardized archaeological descriptions of several hundred archaeological “traditions” around the world. Written reports by regional experts have been gathered into the nine-volume *Encyclopedia of Prehistory* (Peregrine and Ember 2001), and these data are in the process of being coded for use in cross-cultural research (Peregrine 2003). Peregrine (2004) reviews comparative research in archaeology and criticizes that work (including Trigger’s *Understanding Early Civilizations*) for not being truly “controlled” (for region, time period, and other variables) and for using samples that fail to include the entire range of cultural variation. This leads him to a methodological justification for the application of the holocultural cross-cultural approach in archaeology.

Many archaeologists, including Adam Smith (2003:28), criticize the holocultural approach for its reductionism. Indeed, it is tempting to apply DeBoer and Lathrap’s (1979:103) celebrated observation to the realm of comparative analysis. They discuss “the familiar quandary of choosing between a significant pursuit based on faulty method or one which is methodologically sound but trivial in purpose.” The books reviewed here fit the former pattern, whereas holocultural research, whose application to archaeology has so far produced only rather trivial results (Peregrine 1996, 2001, 2004), typifies the latter pattern.

But I think there is a larger problem with the *Encyclopedia of Prehistory* project that Adam Smith’s approach helps identify. In their desire to construct “a sample representing the entire range of variation” in ancient societies (Peregrine 2004:204), the organizers may have ruled out the very features that make ancient states interesting and different. I was asked in 1997 to write one of the entries for the *Encyclopedia of Prehistory* volume on Mesoamerica (Peregrine and Ember 2001). Authors were given a set of guidelines designed to produce the standardization required for a project like this. The basic concept is the “tradition,” defined as “a group of populations sharing similar subsistence practices, technology, and forms of sociopolitical organization, which are spatially contiguous over a relatively large area and which endure temporally for a relatively long period” (Peregrine and Ember 2001:xi). The concept of tradition has a long

history in archaeology (Lyman et al. 1997:160–166; Willey and Phillips 1958). Prominent as a theoretical concept between the 1940s and 1960s, it has recently been revived and given a more contemporary theoretical spin (Pauketat 2001). In the instructions to the nine-volume *Encyclopedia of Prehistory* project, traditions are described as necessarily composed of subtraditions, regionally based smaller traditions. I found that I could not fit my knowledge of the assigned tradition into the categories required by the project without completely violating my own understanding of the dynamics of ancient states in the area, so I declined the invitation to contribute a chapter.

This disjunction between my own understanding of ancient states and the framework of the *Encyclopedia of Prehistory* bothered me at the time, but I could not articulate its cause beyond a vague feeling that states work differently than simpler societies, and that the tradition framework seemed designed for simpler societies. *The Political Landscape*, in another of the “Aha!” moments mentioned above, clarified the basic reason for my dissatisfaction with the tradition format. In a nutshell, the *Encyclopedia of Prehistory* framework uses a strongly absolutist conception of space, thereby ruling out the spatial dynamics of state societies. Yet the manipulation and transformation of space lie at the heart of how ancient states operated. The political landscapes of states are radically different from those of nonstate societies. This makes the notion of stable traditions and subtraditions inappropriate for the political landscapes of most ancient states. Perhaps this is why Gordon Willey, whose massive textbook on North American prehistory (Willey 1966) was organized around the concept of tradition, was able to define numerous traditions for the nonstate societies of North America north of Mexico, but only a single tradition for Mesoamerica.

I do not intend this critique as a wholesale indictment of the *Encyclopedia of Prehistory* project. There is clearly a need for more comparative research of the systematic sort in archaeology, and if this work will contribute to such a goal, it will be a worthwhile endeavor. But in light of Smith’s insights in *The Political Landscape*, I doubt that much progress will be made in the understanding of early states from the *Encyclopedia of Prehistory* data. States are different from nonstates, and the very act of creating “a sample representing the entire range of variation” among ancient societies (Peregrine 2004:204) may prevent the adequate analysis of ancient states.

Bruce G. Trigger and Adam T. Smith have each forged distinctive and productive approaches to social and archaeological theory. These books are worth reading if only to hear their fresh insights on a major anthropological topic. Both scholars are strongly critical of the New

Archaeology and of the neoevolutionist thinking that remains a vigorous strain of contemporary scholarship on early states (e.g., Feinman and Marcus 1998; Spencer and Redmond 2004). Neoevolutionists such as Kent Flannery, Joyce Marcus, and Timothy Earle are also comparativists, but Trigger and Smith take them to task for reductionism, unilinear models, and, in Smith's case, inadequate conceptualizations of power and space.

Although they reject neoevolutionism, Trigger and Smith do not fit comfortably within the various camps of postmodern archaeology. Their comparative perspective is antithetical to the relativism of the postprocessual approach (Hodder 1986; Shanks and Tilley 1987), something Trigger points out explicitly. Although Smith's concerns with ideological production and landscapes indicate theoretical links to postmodernism, his strong empirical orientation distances his (and Trigger's) work from the theory-driven speculation of the "social archaeology" approach (Preucel and Meskell 2004), an evolved version of postprocessualism associated with the *Journal of Social Archaeology*. Trigger and Smith have each forged distinctive theoretical orientations that are historical and materialist while allowing a significant role for ideas, ideology, and human action. They both cite Giddens and Bourdieu, but neither finds practice theory adequate for modeling archaeological data. As an archaeological political economist comfortable with neither neoevolutionism nor postmodernism, I find the theoretical approaches (and empirical analyses) of Trigger and Smith refreshing and useful.

The two books reviewed here provide some of the clearest and most insightful commentary on ancient states available today. I highly recommend both works, not just to archaeologists but to sociocultural anthropologists, historians, political scientists, and others interested in a comparative perspective on ancient states. These are among the best books I have read in the past few years, and they will have important impacts on archaeology for some time to come.

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