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Archaeology as Cultural History

The Argument

Archaeology is cultural history or it is nothing.

I hold this truth to be self-evident, but like most such truths, the problems begin when we try to say exactly what it means. I start with a simple proposition, then spend most of this chapter elaborating on it. Archaeology is the study of what survives of the material culture of people who lived in the past. Insofar as archaeology is about people who lived in the past, it is historical; and insofar as it is about material culture, it is cultural. Therefore archaeology is cultural history.

This sounds like common sense, but surprisingly few archaeologists seem to agree. Fifty years ago, it was a truism in Europe (though less so in America) that archaeology was the handmaiden to history. That perspective now seems distinctly limited. Some people would see archaeology as a natural science; others, as a social science, probably anthropology. Others still hold that archaeology is simply itself: archaeology is archaeology is archaeology. Or maybe it is like literary criticism, or even a form of political activism. Some postprocessual archaeologists call themselves historians of the long term (e.g., Hodder 1987a); others look for ways to bring texts and artifacts together (e.g., Kepecs and Kolb 1997; Ravn and Britton 1997). But overall, the one group that archaeologists hardly ever hold up as a model is the tribe of historians, the only other scholars to devote themselves systematically to the human past.

Yet saying that archaeology is cultural history is rather a quiet revolution, because cultural history is already to a considerable extent anthropology, sociology, literary criticism, and a whole string of other things. We live in an age of what Clifford Geertz (1983: 19–35) calls blurred genres, and one of its most striking developments is a

"historical turn" all across the social sciences (e.g., Cohen and Roth 1995; McDonald 1996). Anthony Giddens (1979: 230) bluntly claims that "There simply are no logical or even methodological distinctions between the social sciences and history – appropriately conceived." William Sewell (1996: 272) spells out the implications of this: history is not the past tense of sociology, increasing the number of comparisons available. Rather, thinking historically requires an "eventful sociology," in which "temporalities . . . are path dependent, causally heterogeneous, and contingent, and reconfiguration of structures by social action is at the core of explanatory models."

If this is so, what does it mean to say that archaeology is cultural history? I explain what I mean by "cultural history" in the next section, but for now I emphasize just two features of what historians of all stripes do. First, they study how people construct and contest culture and meaning *through time*; and second, they explore these issues empirically, through concrete analyses of real people in the past. Despite the obvious relevance of such work, archaeologists seem to understand what historians do less well than many other groups of social scientists.

To clarify what I mean by thinking "through time," I draw on Fernand Braudel's well-known proposal "to dissect history into various planes, or, to put it another way, to divide historical time into geographical time, social time, and individual time" (1972 [1949]: 21). He criticized his predecessors for concentrating on *l'histoire événementielle*, the doings of kings and diplomats, measured in individual time. He suggested that the fundamental temporal level was geographical, the barely perceptible rhythms of *la longue durée*, measured in centuries, which could even be *l'histoire immobile*. Political events were no more than "surface disturbances, crests of foam that the tides of history carry on their strong backs" (Braudel 1980 [1958]: 21). Between these levels lay the time of *conjonctures*, social time, measured in periods of anywhere from five to fifty years. This was the scale on which economic cycles and institutions worked.

This layering of time is central to the historical turn in the social sciences. For Giddens, tracing Braudel's terminology back to Henri Bergson's,

there are three intersecting planes of temporality involved in every moment of social reproduction. There is the temporality of immediate experience, the continuous flow of day-to-day life: what Schutz, following Bergson, calls the *durée* of activity. Second, there is the temporality of *Dasein*, the life-cycle of the organism. Third, there is what Braudel calls the *longue durée*. (Giddens 1981: 19–20)

Archaeologists now regularly draw on the ideas of the Annaliste school of historians which Braudel led for twenty years (e.g., Bintliff 1991; Knapp 1992), following his emphasis on the *longue durée*. But they have taken little notice of the Annalists' subsequent reaction against the "tyranny of the long-term" (*Annales* 1988; 1989). Jean-Yves Grenier (1995) and André Burguière (1995), for example, would deconstruct the very notion of a *longue durée*. I propose less drastic steps in this book; but the core of the approaches pioneered by social scientists like Bourdieu (1977), Sahlins (1985), and Giddens (1979) lies in linking microanalysis of social interactions via institutional analysis to long-term processes, working at all three temporal levels. Yet archaeologists have virtually ignored human time.

Empirical constraints have a lot to do with this. Much of the most ambitious postprocessual archaeology focuses on neolithic Europe, where the data rarely allow us to think in human time. John Barrett's important study of southern England between 2900 and 1200 BC illustrates the issues. Although noting that "we are obviously dealing with a high degree of chronological uncertainty" (1994: 47), he suggests that the real problem lies in the way we think of "individuals . . . as given, pre-existing the material consequences of their actions." This reduces discussion to "the methodological question of how we might recognize an individual's life." In the neolithic we cannot do so, leading some to conclude that "the individual is not an analytically useful unit of study." But Barrett proposes "that we should abandon this whole approach and begin instead to investigate the more interesting questions concerning the ways in which lives were constituted as knowledgeable and motivated." This requires that "we move away from asking 'what kinds of people made these conditions?', to an understanding of what the possibilities were of being human within those material and historical conditions" (Barrett 1994: 4–5).

I single out Barrett's book because he faces the issues so directly. But in doing so he redefines the terms more than he engages with human time. The same problems arise in other postprocessual work dealing with such vast sweeps of time. Their strengths are great but they remain *unhistorical*, because they cannot relate structure and conjuncture to human time. They leave us with what Lynn Meskell (1996: 7) calls "uninhabited histories and non-peopled pasts." Most prehistorians evade individual time by taking refuge in evolutionism or burying it in abstract theorizing.

In this book I want to offer a more properly historical archaeology, taking all three temporal levels equally seriously. Following historians' normal practice, I do this through empirical analysis of a particular time and place, Iron Age Greece (roughly the half-millennium

1100–600 BC). I want to make two points about this period at the outset. First, in certain places and certain contexts, its archaeological chronology is exceptionally precise. It has its problems, and I have set out my views on these elsewhere (I. Morris 1987: 10–18, 158–67; 1993a; 1996), but from about 750 BC on, we can date some kinds of deposits within a margin of plus or minus 25 years. In practical terms, this is about as tight a margin as archaeologists are ever likely to get. It is not what Braudel had in mind when he spoke of individual time, and modern historians might think a quarter-century chronology absurdly crude. But as Anthony Snodgrass (1987: 36–66) shows, classical archaeologists' attempts to write narrative political history have been unsuccessful. This chronological framework leaves us on the cusp between social and individual time, but it is nonetheless a timescale which makes sense in human terms (cf. Manning 1998: 321). Before 750 there is more debate, although dendrochronology now supports the traditional dates (Kuniholm 1996).

Andr n suggests that archaeologists think of texts as “contemporary analogies.” Texts are not a different order of evidence, telling us what artifacts mean. Nor should we expect the material record to prove written accounts right or wrong. All archaeological interpretation is analogical. The best analogies will be those that Ian Hodder (1982: 11–27) calls “relational,” where there are so many similarities between the overall contexts being compared, or between the archaeological data and a general ethnological model, that we can plausibly extrapolate from an ethnographic or historical comparison to an archaeological case. Andr n argues that

Closeness in time, space, and form (such as technological similarity) is always held up as a criterion of a good analogy. The field of historical archaeology can thus be seen as a special case in archaeology as a whole, since the analogies are particularly close because artifact and text are “contemporary analogies.” (Andr n 1998: 156)

Whether interpreting a mesolithic site through an Eskimo ethnography or a classical site through contemporary literature, we are making analogical arguments. But when we read primary texts alongside the artifacts, our analogies are far stronger than when we rely on information from other times and places.

Geertz (1973: 28) suggests that the goal of cultural anthropology is “to draw large conclusions from small, but very densely textured facts; to support broad assertions about the role of culture in the construction of collective life by engaging them exactly with complex specifics.” The problem is that for prehistorians, the facts rarely seem

Table 1.1 Standard periodization for central Greece

Late Bronze Age/Mycenaean	c. 1600–1200 BC
Late Helladic IIIC	c. 1200–1075 BC
Early Iron Age/Dark Age	c. 1075–700 BC
Archaic	c. 700–480 BC
Classical	480–323 BC
Hellenistic	323–31 BC

The Early Iron Age is usually subdivided by pottery styles. The dates of these vary locally, but the Athenian sequence is normally dated as follows:

Submycenaean	c. 1075–1025 BC
Protogeometric	c. 1025–900 BC
Early Geometric	c. 900–850 BC
Middle Geometric	c. 850–760 BC
Late Geometric	c. 760–700 BC

densely textured enough, and the appraisals (the word Geertz (1973: 16) prefers to verification) not rigorous enough.

The more varied and detailed our evidence, the thicker and better our descriptions will be. That much is obvious. Written evidence makes our accounts more specific and our analogies much closer. We can play categories of evidence against each other more confidently and subtly than if we had to rely entirely on analogy with other societies. But that does not mean that archaeology can *only* be cultural history when we have texts. As Barrett (1994: 71) points out, “we must allow for non-linguistic and pre-linguistic knowledges, as practical ways of knowing the world, but which informants would have considerable difficulty in ever expressing verbally.” The crucial issue is not the presence or absence of writing but the density, quality, and variety of data points. When we study fifth- or fourth-century BC Athens, for example, there is a rich literary record. This is a fully historical period, even though certain genres – private letters, diaries, household accounts – are largely lacking. When we move back into the archaic period (table 1.1), the range of sources narrows, to poetry, then in the eighth century to epic poetry, until by the ninth century there is nothing at all. But even when we think about the tenth century, as I do in chapter 6, we gain much by looking back in the light of archaic poetry. It does not make much sense to say that there is a threshold defined by the quantity, the range, or even the presence of

written sources, which a period must meet to be dignified as “historical.” Rather, as we move back in time from the fourth century, or out in space from Athens, it gradually gets harder to write accounts which meet the challenges I discuss in this chapter. As the quantity of finds from the Greek Dark Age has increased, our accounts have become more historical, although we have not found written sources older than Homer (and probably never will).

In a few cases, the texture of prehistoric facts is dense enough for thinking on something like a human timescale. The rebuilding of neolithic long houses on some central European sites can be broken down into phases of just twenty years (Bradley 1998: 43–8), and at Çatalhöyük the replastering of houses and the burials beneath their floors offer even finer chronologies. Matthews *et al.* (1997: 304) argue that “These discrete lenses perhaps allow us to come close to attaining the required degrees of precision for tracing remains of individual actions or sets of activities which are central to current methodological and theoretical objectives in archaeology.” These are exceptional cases, but dendrochronology holds out the possibility of extending historical approaches, combining all temporal levels, back to the third millennium (Baillie 1995; Manning 1998: 322–5).

Cultural history is not, then, solely the concern of text-aided archaeologists. But taking history seriously will change the relationships between text-aided and prehistoric archaeology. Kathleen Deagan (1988: 10) notes that as Americanist historical archaeology became more theory-conscious in the 1980s, it moved from being the “handmaiden to history” to being a “handmaiden to prehistoric archaeology,” a testing ground for models developed in prehistory, where the real action was. Incorporating human time means that text-aided archaeology, which has the thickest and richest evidence, should move to the forefront of theoretical and methodological debates. Some historical archaeologists are already claiming this place (e.g., Johnson 1996; Orser 1996a,b).

In the next section, I define more fully what I mean by cultural history. I stress (1) a shift from materialist causation toward studying representations; (2) interest in the construction and contestation of meaning; and (3) a focus on voluntarism. I then turn to cultural analysis in archaeology, asking why archaeologists show so little interest in cultural history. I explain this historically, focusing on the effects of institutional confinements. In the fourth section I discuss the methods of this book, and set out its arguments.

I hope to reach several audiences. The first is that which I think the editorial board of the *Social Archaeology* series imagines: a broad

archaeological community including workers in the new and old worlds, from the palaeolithic through the twentieth century. I want to say something of interest to archaeologists of all times and places. But since my argument is that archaeology is cultural history, I set about my task like a historian. As I explain in the final section of this chapter, Parts III and IV of this book are a detailed study of changing notions of time, place, and space in Iron Age Greece. I therefore want classical archaeologists and ancient historians to read this book too: the methods of cultural history and arguments within nonclassical archaeologies have still had only limited impact in these fields. Finally, saying that archaeology is cultural history should be as significant for cultural historians as for archaeologists. Some of the best work in modern cultural history is being done on material culture, but few of the scholars engaged in it seem to care about how archaeologists approach the world of things. As so often, promising lines of inquiry coincide with institutional and disciplinary boundaries, and I hope to do something to bridge these.

Cultural History

If anything unites cultural history, it is polemic. The “new cultural history” of the last fifteen years comes in several national schools (the French, drawing on Foucault and Parisian re-readings of Marx; the British, influenced heavily by E. P. Thompson, and often focusing on political language; the American, sometimes leaning on Geertz and Sahlins as well as Foucault, other times more influenced by deconstruction; and Italian microhistory, borrowing from postmodern ethnography), divided by misunderstandings and controversies (e.g., Cerutti 1997; Joyce 1998; Stedman Jones 1998). But two main themes recur. The first is a rejection of material causation. Introducing a volume of essays called simply *The New Cultural History*, Lynn Hunt (1989: 7) suggests that “Economic and social relations are not prior to or determining cultural ones; they are themselves fields of cultural practice and cultural determination – which cannot be explained deductively by reference to an extracultural dimension of meaning.” The second theme follows from the first. For older social histories, with their “almost tyrannical pre-eminence of the social dimension, which predefines the cultural cleavages that are to be described” (Chartier 1988: 30), we should substitute what Chartier calls “a cultural history of society.” This focuses on how people *represent* their worlds, the social categories they create, and the conflicts these generate:

The definition of cultural history . . . must be conceived as the analysis of the process of representation – that is, of the production of classifications and exclusions that constitute the social and conceptual configurations proper to one time or one place . . . [but t]his history must also be understood as the study of the processes by which meaning is constructed. Breaking with the old idea that endowed texts and works with an intrinsic, absolute and unique meaning which it was the critic's task to identify, history is turning to practices that give meaning to the world in plural and even contradictory ways. (Chartier 1988: 13–14)

But beyond a shared commitment to extending cultural analysis to all dimensions of life, and shifting from digging out social facts to enjoying the play of representations, there is little agreement. I divide the debates into three groups.

1. *Cultural History as Community*

The first line of thought is that cultural history is about widely shared dispositions, which unite the inhabitants of a village, city, or nation at a given time. This concept is familiar in most human sciences. In the heyday of functionalism, scholars tended to imagine the world as made up of discrete, spatially bounded cultures. Any ethnographic yarn could begin “among the so-and-so,” and listeners would immediately understand the frame of reference. For most of us, expressions like “Old Regime France” or “Jacksonian America” conjure up particular (if vague) images of “what it was like” back then. And with good reason; cultural historians and ethnographers regularly find that even in the most conflict-ridden societies, certain mentalities cross-cut most or all boundaries, allowing them to define the group in question as a “society.”

Kroeber and Kluckhohn (1952) traced this idea of culture back to the eighteenth-century Göttingen school of historians, whose approach was marginalized in the nineteenth century by the Rankean model of document-driven political history. Robert Darnton, who has worked closely with Geertz, is the best-known exponent of what he calls “the anthropological approach.” Darnton (1984: 4–5) argues that the key to cultural history is the fact that “What was proverbial wisdom for our ancestors is completely opaque to us . . . When we cannot get a proverb, or a joke, or a ritual, or a poem, we know we are on to something. By picking at the document where it is most opaque, we may be able to unravel an alien system of meaning. The thread might even lead into a strange and wonderful world view.” Such a world view, the ability to get a joke, belongs to the whole community:

All of us, French and “Anglo-Saxons,” pedants as well as peasants, operate within cultural constraints, just as we all share conventions of speech. So historians should be able to see how cultures shape ways of thinking, even for the greatest thinkers. A poet or philosopher may push a language to its limits, but at some point he will hit against the outer frame of meaning. Beyond it, madness lies – the fate of Hölderlin or Nietzsche. But within it, great men can test and shape the boundaries of meaning. Thus there should be room for Diderot and Rousseau in a book about *mentalités* in eighteenth-century France. By including them along with the peasant tellers of tales and the plebian killers of cats, I have abandoned the usual distinction between elite and popular culture, and have tried to show how intellectuals and common people coped with the same sort of problems. (Darnton 1984: 6–7)

2. *Cultural History as Conflict*

But there were jokes at the court of Louis XVI which peasants in Languedoc did not get, and vice versa, and some of Darnton's best essays highlight just such failures of comprehension. Cultural history is about divisions and conflicts as much as agreements and shared understandings. Returning to the example of Jacksonian America, our feel for “what it was like” fragments on closer inspection. We see distinctions between the worlds of European colonists, native Americans, and African slaves. Even among whites, we find remarkable differences between subcultures of men and women, rich and poor, urban and rural, not to mention immigrants from different nations and members of different churches. Even, say, among rich, male, white Anglo-Saxon Protestants, we find profound disagreements over that most fundamental question, the nature of the good society. Some clung to an ideal of a republic of yeoman farmers; others vigorously promoted urbanization, industry, and modern financial institutions. Culture breaks down; one man's shared disposition is another's ideology, a false consciousness. Where Geertzian/Darntonian cultural historians find meaning and complexity, Marxists and feminists may see mystification and oppression.

1820s north America was a complex society going through rapid changes, but even in the most isolated groups ethnographers find similar distinctions. However much people have in common, they still have to reproduce their attitudes through time. Try as we might to remain faithful to what we take to be the wisdom of the ancestors, we always encounter new situations, and must adapt it to them. What is more, we regularly find that other people have different ideas about how to do this (and different ideas about what the traditions are). We

can hardly doubt that the culture of the !Kung San is more homogeneous than was that of Jacksonian America, but the contrast between the two affects the answers we come up with more than the questions we have to ask. Criticizing Darnton, Chartier (1988: 102) suggests that it is to deal with this that "a definition of history primarily sensitive to inequalities in the appropriation of common materials or practice has come into being... it is indisputable that the most pressing question inherent in cultural history today... is that of the different ways in which groups or individuals make use of, interpret and appropriate the intellectual motifs or cultural forms they share with others."

3. *The Cultural History of Society*

Some cultural historians suggest that whatever kinds of documents we deal with, the fact of their textuality always traps analysis at the level of competing language games. "It is clear from the outset," Chartier argues, "that no text, even the most apparently documentary, even the most 'objective' (for example, a statistical table drawn up by a government agency), maintains a transparent relationship with the reality that it apprehends" (1988: 43). There can never be a neutral reflection of unmediated social realities. We cannot move from how our sources present the world to how the world really is; every presentation is a re-presentation. All we can do is play off one (mis)re-presentation against another.

For a century, most historians have distinguished between how people in the past saw the world and how that world really was, explaining the former (culture) in terms of the latter (economy and society). In the *locus classicus*, we read that "Just as our opinion of an individual is not based on what he thinks of himself, so can we not judge of a period of transformation by its own consciousness; on the contrary, this consciousness must be explained rather from the contradictions of material life" (Marx 1977a [1859]: 390). Cultural historians now question the materialist consensus.

Gareth Stedman Jones, for example, concludes about the British industrial revolution that "We cannot therefore decode political language to reach a primal and material expression of interest since it is the discursive structure of political language which conceives and defines interest in the first place" (1983: 22). For Stedman Jones, confronting textuality not only drives a wedge between representations of social structures and the reality of such structures; it also breaks down the distinctions between such structures and the ways people construct them. Class exists not in Marx's objective sense of relations to the

means of production, but only insofar as people interpret the productive relations into which they are born or enter as class relationships. Moving from representations to realities is not just tricky; it is a mistake even to try, since economy and society are themselves constituted in discourse.

Some cultural historians now attack all forms of structural analysis. Where Giddens and Bourdieu move from social structures to ongoing processes of structuration, Bernard Lepetit rejects the long term altogether:

To Saussurian linguistics, we oppose situational semantics; against determination by *habitus*, we insist on the plurality of the worlds of action; the substantive rationality of economic actors is challenged in the name of the conventions of procedural rationality; structural anthropology is contested by the study of the modalities and effects of historicizing cultures. (Lepetit 1995a: 14)

For Simona Cerutti (1995: 131), history becomes "the explication of the strategies of manipulation of social subjects in the face of a plurality of normative fields, whose principal characteristic is that they are mutually contradictory." The historian's job is to share the experiences of past actors, grasping their meanings, even in the absence of direct evidence. Historians move, in Raphael Samuel's (1992) telling phrase, from being "fact grubbers" to being "mind readers."

This undermines historians' traditional realist, empiricist, and documentarist assumptions. Some critics even question the distinction between primary and secondary sources, the bedrock of historical science, suggesting that claims to ground historical narratives factually in primary sources are no different from the intertextual methods of other literary genres:

"History" refers in actual practice only to other "histories," in the eyes of these critics. Thus they fail to see much, if anything, in the distinction drawn by normal historians between fact and fiction, for factual reconstruction is really nothing but construction according to the working "fictions" of normal historical practice, which, in turn, are the premises of both historical realism and realistic mimesis... Normal, that is, traditional history is shown to be but a conventional, hence arbitrary, mode of coding communication as factuality by presenting the representation as if it were entirely referential and realistic. (Berkhofer 1988: 445-6)

A postmodern cultural history celebrates the inventiveness of agents cut free from structural constraints, and gives more leeway for the

inventiveness of historians, to the point that boundaries between history and literary criticism rapidly lose their meaning.

It often builds on Hayden White's classic *Metahistory*. White (1973: 13, 432–3) argues that "History remains in the state of conceptual anarchy in which the natural sciences existed during the sixteenth century, when there were as many different conceptions of 'the scientific enterprise' as there were metaphysical positions." He identifies four strategies of explanation through narrative emplotment, each with specific modes of argument, ideological implication, and literary trope. White concluded that "*on historical grounds alone* I have no basis for preferring one conception of the 'science' of history over the other. Such a judgment would merely reflect a logically prior preference, either for the linguistic mode in which Tocqueville and Marx prefigured the historical field or for the ideological implications of their specific figurations of the historical process."

Not surprisingly, there have been heated arguments over these claims. There have been explicitly theoretical contributions (Jenkins 1997), but most debates nestle into substantive disputes, such as those over the history of science and the Holocaust. In the early 1980s, advocates of a "strong programme" (British spelling) of constructivism argued that there was nothing special about scientific knowledge (Barnes and Bloor 1982). Poring over scientists' less-studied writings, they showed how nonscientific factors – Newton's obsession with alchemy (Dobbs 1991), or Darwin's with the inferiority of the poor (Oldroyd 1984) – drove research agendas. But by the early 1990s, there was growing concern about how much the strong programme could *not* explain, particularly how discoveries withstand empirical testing outside their immediate cultural context. We do not care about the same things as seventeenth-century Englishmen, but gravity still works. A new school advocated a "hard program" (American spelling), insisting that radical constructivism had failed (Schmaus *et al.* 1992). They called for modified objectivism. Hard programmers recognized that constructivism had overthrown the heroic model of science, and that no one can go back to models of value-free scientific facts and laboratories driven purely by internalist, rational forces. But, they added, historians must now explain how the cultural and the natural coexist, and how, for all the power of the discursive formations within which we make sense of the world, scientific creations can outlast their creators' worldviews. After all, the Nazi scientists' rockets still worked after 1945 (Beyerchen 1992).

The Holocaust raises even tougher questions: if there truly are no historical grounds for preferring one form of emplotment to another, can we only evaluate accounts of this horror according to how they

conform to our aesthetic and ideological preferences? White (1992) concedes that here the events themselves rule out comic and romantic plots, and proposed that historians need a new language of representation, something like the middle voice of ancient Greek, allowing the facts to speak through them. Some historians conclude that the Holocaust is unrepresentable; others, that it was a singular event calling for a special language; others still, that it exposes the limits of postmodern historiography, and the need to ground analysis in traditional conceptions of evidence, temporal layers, and structures.¹

Cultural historians have moved with the times. Modifying his earlier claims, Chartier now insists on

the exteriority and the specificity of practices that are not in themselves of a discursive nature in relation to the discourses that, in many ways, are articulated on the basis of these practices. Recognizing that access to such non-discursive practices is possible only by deciphering the texts that describe them, prescribe them, prohibit them, and so on, does not in itself imply equating the logic that commands them or the "rationality" that informs them with the practices governing the production of discourses. Discursive practice is thus a specific practice . . . that does not reduce all other "rules of practice" to its own strategies, regularities, and reasons. (Chartier 1994: 174–5)

There could be no greater error, he goes on, than to "constitut[e] ideology as the determining instance of social operations, whereas all regimes of practices are endowed with a regularity, logic, and reason of their own, irreducible to the discourses that justify them" (1994: 177). Stedman Jones defends the "linguistic turn" in British cultural history against Chartier, but concedes that

It would be foolish to deny that there are processes in the past which are not encompassed – or at least not sufficiently or adequately encompassed – by the languages and discourses of the past. Urbanisation and population change would be good examples . . . In all these instances, it is possible to refer to social practices which were not governed by the rationality of individual agents, or whose macro-social effects bore only a paradoxical relationship with individual intentions. (Stedman Jones 1996: 27–8)

Marx famously observed (1977b [1852]: 300) that "Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past." The great contribution of cultural historians like Stedman Jones

lies in showing how often people *have* made history as they pleased, with discursive forces – Marx’s superstructure – dominating supposedly nondiscursive infrastructures to the point that Marx’s categories need redefinition. But like the strong programmers, they have to recognize that this is only part of the story, and that the 1980s emphasis on self-fashioning and language can be as misleading as the 1970s obsession with class and production.

Stedman Jones points out that Chartier cannot specify where to draw lines between the cultural and the social, given the textuality of all evidence. But we might respond with some of historiography’s most traditional methods, setting up a hierarchy of sources. Returning to Chartier’s example of a government statistical table (p. 12 above), such a document may well have contributed to and been constructed within new concepts of state surveillance, requiring it to be read as part of an argument about what the community should be like. But that does not necessarily invalidate readings which move past form to content. There will always be problems in this, but these are problems of well-known types, and historians have tools to tackle them. In some cases the discourse of state control operates in such a way as to rule out attempts to go beyond form, but that must be demonstrated empirically, not assumed.

When we read texts by authors engaged in different language games, and find that they nonetheless represent an external social/economic reality in similar ways, we are on to something. At the very least we are uncovering shared dispositions, culture in the Geertzian sense, cutting across lines which in other contexts act as boundaries; and if the contexts are different enough and numerous enough, we may conclude that for all the complexities of the exercise, we have reached a nondiscursive reality apprehended by past actors. Thus when Emmanuel Le Roy Ladurie (1974: 23–9) found that all the sources relating to land distribution in Languedoc from 1400 through 1800 pointed to a cyclical pattern of concentration and dispersal, despite the differences in the motives and cultural worlds of their producers, he was surely right to conclude that there was an increase in the number of middle-sized farms at the expense of very small and very big holdings in the fifteenth century, and that many of these middling properties disappeared in the sixteenth and seventeenth centuries, some breaking into several smaller farms, others being absorbed into larger estates.

What this meant (an interpretive question) and why it happened (a causal question) are different matters. But the important points are that there *are* ways to move outside a world of competing representations,

and that this *is* necessary if we want a cultural history of society rather than a bloodless, aestheticized literary exercise. It is one thing to reconstitute Old Regime folk tales, as Darnton does with great skill (1984: 9–72); it is another (fuller and more complex) thing to read them against the background of a population growing as fast as mice breeding in a barn, as one sixteenth-century observer put it (Le Roy Ladurie 1974: 53), and rapidly outrunning resources. Refusing to set cultural constructions of reality into their material contexts fulfills Geertz’s fear (1973: 30) that culturalism might lose contact with the hard surfaces of life. For most people in seventeenth-century Languedoc these surfaces were hard indeed. To overlook that is to write just the kind of idealist history that Marx tried to end.

Conclusion

Archaeologists study material culture in the past. They therefore need to take cultural historians’ debates seriously. The linguistic turn within cultural history has undermined older social histories of culture, and in this book I try to write something more like the cultural history of society that Chartier advocates, recognizing both individual agency and the limits of self-fashioning. Cultural history is, as Chartier indicates in the subtitle to his 1988 collection of essays, a matter of moving between practices and representations.

E. P. Thompson, who pioneered many of the cultural historians’ concerns, explained that

Historical and cultural materialism cannot explain “morality” away as class interests in fancy dress, since the notion that all “interests” can be subsumed in scientifically-determinable material objectives is nothing more than utilitarianism’s bad breath. Interests are what interest people, including what interests them nearest to the heart. A materialist examination of values must situate itself, not by idealist propositions, but in the face of culture’s material abode: the people’s way of life, and, above all, their productive and familial relationships. (Thompson 1978: 176)

We must recognize different kinds of historical time. No structural, long-term history gets to the heart of experience unless we interweave it with short-term, event-oriented, cultural histories. Events are a primary category of analysis. But the other side of the coin is that they only really make sense when set into grander flows of conjunctural and structural time.

Cultural Archaeology

Archaeologists largely ignore what historians are saying. In part this is because historians often decline to draw the implications of their work to the attention of anyone not specifically interested in the Investiture Conflict, the Corn Laws, etc. Yet anthropologists and literary critics find plenty to read on the history bookshelves. Archaeologists seem uniquely insulated from historical thought, despite being the only other academic group defined by an obsessive focus on the human past. Why so?

This is really a historical question. It seems to me that a kind of institutional inertia – the dull compulsion of situated, routinized patterns of communication – has made it difficult for archaeologists and historians to talk to each other. A hundred years ago, administrators generally grouped prehistoric archaeology with ethnology as part of the study of primitives. Prehistoric artifacts found their way to Museums of Ethnology, and when universities wanted to hire prehistorians (which, in most countries, was not often), they attached them to Anthropology departments or, in Europe, carved out separate Archaeology departments. Those who worked in the Mediterranean, on the other hand, were placed in higher-status Classics departments or Divinity schools, and had famous museums of art chasing their discoveries. Yet neither group had strong professional contacts with historians.²

Alain Schnapp (1996) has skillfully described the slow emergence of cultural approaches to artifacts up to the 1850s, and I will look back only as far as the early twentieth century, when Europeanists began a series of great syntheses of the evidence. They looked chiefly to ethnology for frameworks, often consciously reacting against earlier antiquarian extrapolations from Mediterranean history. Gustav Kossinna, rejected by the German academic establishment (particularly its classicists), insisted in his wildly successful 1912 booklet *Die deutsche Vorgeschichte* that archaeologists could answer ethnologists' questions about the origins of the German people better than philologists. Contemporary ethnologists held that each *Volk* had its own *Kultur* (see Stocking 1996). Kossinna suggested that by following a "siedlungsarchäologische Methode," archaeologists could treat what they excavated as ethnic markers. When they consistently found certain types of artifacts together, they could equate the *Kultur* with the *Volk*, documenting the prehistory of the German race.

Already in the early 1920s British archaeologists were using the word "culture" in much the same ways as the Germans (S. Jones 1997: 16–17).

Gordon Childe's celebrated proposition – "We find certain types of remains – pots, implements, ornaments, burial sites, house forms – constantly recurring together. Such a complex of regularly associated traits we shall term a 'cultural group' or just a 'culture.' We assume that such a complex is the material expression of what today would be called a 'people' (1929: v–vi) – formalized what many archaeologists were already thinking. What made Childe stand out was his combination of this model with extraordinary empirical knowledge and Marxism. He went beyond (though without abandoning) migration and diffusion to emphasize internal developments and revolutions.

By 1950, Childe's methods had entered mainstream British archaeology. But his historical/political agendas fell on stony ground. This is a recurring theme: archaeology's institutional confinements separated it from historical practice, and when mavericks like Childe tried to cross the barriers, other archaeologists took over those ideas which worked well within existing frameworks, and ignored those which did not. Childe's culture history was useful; his Marxist history was not.

Americanists followed similar paths. North American data were poorer than those in central Europe, and synthesizers relied more on seriation than on stratigraphy, but by the 1920s they had enough evidence to conclude that the natives did have some kind of history before Europeans arrived. Archaeologists began using "culture" in a sense like Childe's, and fixed relative chronologies for their cultures, describing what they did as "culture history." In a programmatic essay on "The Strategy of Culture History," Irving Rouse listed archaeology's "historic objectives": the diffusion and persistence of cultures, independent invention of culture traits, migration and other mechanisms of spread, participation in culture, acculturation, ecological adaptation, biological models of phylogeny, parallel development, evolution, and assorted "other processes" (Rouse 1953: 98–100).

Given the empirical situation they faced, it made sense for 1930s archaeologists to concentrate on chronology. But as Rouse's essay shows, what they called "culture history" had little to do with history as practiced by historians. This was an age of ferment in American historiography. As archaeologists created culture history, James Harvey Robinson and Charles Beard established the "New History," raising profound epistemological questions and insisting on economic and social causation. By the 1950s, when culture history was virtually unchallenged among archaeologists, historians like Kenneth Stampp and Stanley Elkins were tearing down the old consensus models of American slavery and moving historical scholarship into the center of the political uproar over racism (see Novick 1988: 86–108, 250–64, 348–60).

Even more than Europeanists, American culture historians reacted against the low status of nineteenth-century archaeology, trying to show that natives had history, and that archaeology – as the only discipline with direct access to it – was not just an appendage to ethnology. But as often happens, the debate took place within frameworks established in earlier periods. Archaeologists did not move beyond the ideas about culture they inherited from late nineteenth-century ethnology, even in years when historians were energized by economic questions that archaeologists might under other circumstances have felt able to address. The institutional and intellectual gap was already wide. Culture history grew up without much interchange with other fields. Its foundations were, as the authors of the only systematic study of it say, “an ad hoc consensus concerning some empirical generalizations that, in the absence of theory, were incapable of serving as explanations” (Lyman *et al.* 1997: 11).

The exception that proves the rule is Walter Taylor’s extraordinary *A Study of Archaeology* (1948). Taylor identified a contradiction in Americanist archaeology: archaeologists taught in Anthropology departments, yet said their goal was to recreate culture history. He attacked the field’s leaders for confusion and intellectual sloppiness, concluding that they were writing

mere chronicle, the ordering of cultural materials in temporal sequence together with an attempt to demonstrate their derivations and cross-cultural relationships... They have categorized events and items, tagged them, but not investigated them in their contexts or in their dynamic aspects. As a result of these conditions, Americanist archeology is not in a healthy state. Its metabolism has gone awry. It is wasting and not assimilating its foodstuffs. (Taylor 1948: 94)

Taylor urged that archaeologists think of material culture as the residues of functioning societies, produced by real people, rather than reducing cultures to abstract entities interesting only for their “influences” upon one another. He understood trends in 1930s historiography and cultural anthropology, and insisted that the only difference between the two was that the former aimed at understanding specific contexts while the latter moved on to generalize. This meant that “ethnography is really a branch of historiography and not of anthropology” (Taylor 1948: 41), since it was about cultural details in their context. To do archaeology was to do history:

One may “do” historiography by interpreting the written record (archives, letters, cuneiform tablets, Maya hieroglyphs, inscriptions on seals and coins and monuments), by interpreting the oral record

(informants of non-literate or illiterate peoples, oral tradition of literate peoples), or by interpreting “non-verbal documents” (stone axes, Greek temples, Toltec pyramids, Hohokam pottery, Magdalenian cave art). The important fact is that the moment cultural data are synthesized into a context representing past actuality, the result is historiography. (Taylor 1948: 43)

Archaeologists generally ignored Taylor through the 1950s, but Lewis Binford (1972: 2, 6, 8–9) identified *A Study in Archaeology* as a stimulus in his own revolt against culture history. Like Taylor, Binford thought that culture historians treated archaeological cultures simplistically, as units in which artifacts, regardless of function, were interchangeable traits which could be compared with artifacts from other cultures to document blending or influence. Instead, Binford suggested, “artifacts having their primary functional context in different operational sub-systems of the total cultural system will exhibit differences and similarities differentially, in terms of the structure of the cultural system of which they were a part.” He recommended dividing artifacts into three classes, according to whether they functioned primarily (a) to deal directly with the physical environment (what he called “technomic”), (b) to signal status and group membership (his “socio-technic”), or (c) to communicate ideology (“idio-technic”). Developments within each class would be different. Instead of comparing artifacts between cultures, archaeologists should look at the structure of subsystems within cultures, above all those which “function to adapt the human organism, conceived generically, to its total environment both physical and social,” because “Culture is viewed as the extra-somatic means of adaptation for the human organism” (Binford 1972 [1962]: 22).

Despite his admiration for Taylor, Binford took no more from his discussion of historical method than had 1950s culture historians. Taylor’s lone voice could not overcome three generations of institutional divisions and anthropological hostility to historicism, any more than Childe’s had overcome British empiricism. Binford took for granted culture historians’ definitions of anthropology, archaeology, and history. He merged the methods of contemporary Americanist archaeologists and Boasian particularists in anthropology into a single category of “historical” thought, and distinguished his own approach as “processual.” These categories were canonized in debates over the new archaeology, and Taylor’s historical perspective disappeared.

In an influential confrontation, Jeremy Sabloff and Gordon Willey (1967) defended a “historical” explanation for the collapse of the

Classic Maya, by which they meant that an invasion had been the prime mover, rather than long-term social and economic trends. Binford (1972 [1968]: 114–21) rightly responded that even if there had been an invasion around AD 900, this would only have led to collapse if it fed into ongoing processes within Maya society. This was the kind of argument social historians had been making for forty years, but to culture historians and new archaeologists alike, the issue was one of “process” versus “history” *tout court*. “History” lost. A decade later Binford described himself as feeling “uncomfortable” even thinking about addressing a conference on historical archaeology, because “I felt that persons doing ‘historical’ archaeology were different from myself with different interests . . . Why? That word ‘historical’ again!” (1977: 13). He got over his discomfort, however, suggesting that historical archaeologists did have a part to play: they could test his hypotheses against textual data, making them almost as useful as ethnoarchaeologists’.

In the 1970s, academic archaeology developed similarly in western Europe and the United States, with a culture-historical mainstream and a processual critique, albeit stronger in America than in Britain, and stronger there than on the continent. Cultural and social anthropologists were by now in full revolt against the functionalist verities which underpinned the new archaeology; and at the end of the decade, Ian Hodder and his students at Cambridge began to engage with Giddens’ and Bourdieu’s work.³ They concluded that material culture, like all culture, was actively manipulated by thinking people in pursuit of their own ends. People used it to construct meanings, sending messages to themselves and others about how they wished to be seen, and also building and challenging group identities. Identities and hierarchies were not objective facts, even though people might perceive them as such, but existed only because actors accepted them and tried to reproduce them in practice. Above all, meanings were disputed. Hodder argued for a contextual or postprocessual archaeology which explored what objects and contexts of activity meant to prehistoric people, and the need

for interpreting specific, not general meanings. Unlike most other approaches in archaeology, the contextual approach, close to thick description, seeks to ask questions such as ‘Why was this particular shape or decoration of pot used rather than any other?’, ‘Why were the tombs this shape?’, ‘What specifically did the tombs mean?’ It is only by asking such questions that we can understand the way in which material culture was socially active and was involved in long-term change. (Hodder 1992: 22–3)

This would be a better archaeology than the functionalist and reductivist new archaeology. But, Hodder also insisted, postprocessual archaeology was not just aesthetically preferable to other forms. It was the *only* form which made sense, because “in archaeology *all* inference is via material culture. If material culture, all of it, has a symbolic dimension such that the relationship between people and things is affected, then *all* of archaeology, economic and social, is implicated” (Hodder 1986: 3).

But, he recognized, actually doing postprocessual archaeology was no simple thing:

In the construction of the cultural world, all dimensions (the height and colour of pottery for example) already have meaning associations. An individual in the past is situated within this historical frame, and interprets the cultural order from within its perspective. The archaeologist seeks also to get ‘inside’ the historical context, but the jump is often a considerable one. (Hodder 1987b: 7)

Postprocessualists developed various ways to make the leap. The main one was to contrast different contexts of activity. Like cultural historians, they recognized that while evidence might always come to us already implicated in representational strategies, these strategies were not all the same. If a particular site had impressive ritual buildings but uniform housing and simple, poor, burials, we might conclude that in this culture self-promotion was problematic. There may have been rich people, who might have won prestige through funding communal rituals, but directly claiming high status by building palaces for themselves, or tombs to rival the houses of the gods, was not appropriate. Postprocessualists quickly established ideology as a major concern.

Their methods were initially dominated by structuralism: because there were no informants to give clues about meaning, prehistorians had to treat what they dug up as signifiers standing in essentially arbitrary relationships to signified ideas. This was productive, but tended toward formalism. Christopher Tilley (1990: 65–6) observes that “Different aspects of the archaeological record may be regarded, from this perspective, as embodying a series of transformational homologies or inversions of the same structured relations of difference . . . But the meaning[s] of the structures recovered have been interpreted through grafting various Marxist, hermeneutic and post-structuralist perspectives . . . to structuralist forms of analysis.” Meaning, the object of the enterprise, had to be imposed in blatantly intrusive ways, initially through ethnoarchaeological analogies, but

increasingly by drawing on poststructuralist (and particularly literary) theory.

Like cultural historians, postprocessual archaeologists moved from being fact grubbers to being mind readers. Some describe themselves as “historians of the long-term” (Hodder 1987a). In his original critiques of functionalism, Hodder (1986: 77–102; 1992 [1982]: 92–121) suggested that Robin Collingwood’s (1946) view of history as empathetic re-imagination might be a more useful model for archaeologists than Binford’s natural-science approach. But Hodder seems to have been little more involved with what historians actually do than Binford; as examples of historical practice he cited Weber and Sahlins. Among his students, Shanks and Tilley (1989: 7) urged that “to forge an acceptable practi[c]e of archaeology . . . we need to take history *seriously*,” but their discussion of “archaeology – history” remained at the severely abstract level of Nietzsche, Adorno, and Ricoeur (1987a: 16–22). Similarly, their critique of evolutionary thinking owes much to Giddens and Sahlins, but little to historiography (1987b: 137–85). Most of the comments on archaeology as history which came out of a postprocessual conference at Cambridge in 1991 were even more rarified (Hodder *et al.* 1995: 141–78).

Despite postprocessualists’ rhetoric, the 1980s historical turn was less serious in archaeology than in other social sciences, largely, I think, because it went on within frameworks inherited from 1960s arguments about culture history vs. culture process, and ultimately from late nineteenth-century divisions of academic labor. To the extent that sociologists and anthropologists talk about history, postprocessualists take over their language. They have been excited by what they have read in Bourdieu, Giddens, and Sahlins, but they have taken history at second-hand from them, feeling little need to engage directly with historians.

Words and Things

Taking cultural history seriously means thinking on all three temporal levels described by Braudel and Giddens. And this requires a shift away from grand theory, toward more prosaic concerns – creating the densest possible texture of data and the tightest chronology. Text-aided archaeologists are particularly well placed to begin these shifts.

There are many ways to do this. I work mainly through what Andr n calls a method of correlation, looking for similarities between the structure of the material record and the members of a past society’s verbal accounts of themselves. Andr n suggests that

correlation often assumes that there is an association between the references of artifacts and texts. Correlations are thus based in large measure on perceptions of what is probable, which is ultimately defined by different research traditions. What can be seen in one perspective as an innovative correlation may seem uninteresting or impossible from a different point of view. (Andr n 1998: 166)

Andr n is right to say that such arguments are probabilistic and can only be true on the terms of a specific interpretive community. But that is the nature of all historical arguments (e.g., Appleby *et al.* 1994; McCullagh 1998). I follow up Andr n’s emphasis on competing research traditions in Part II. But I also put special emphasis on the interpretive communities of the ancient Greeks themselves. Greek writers represented material culture as something to use creatively, in the same way as words, to construct images of themselves and the world around them. They knew that ways of dressing or building houses were different from styles of speech, but they implicated material culture in the same rhetorical games as words. Material culture was ambiguous, and they felt that it required linguistic interpretation. We make most sense of Greek material culture by using the closest analogies, the Greeks’ own discussions of it.

It is easy to pile up examples. In our earliest sources, Homer put down much of Odysseus’ success to his ability to apply *noesis*, “intelligence,” more effectively than anyone else to the material *semata*, or “signs,” he came across in his adventures. Throughout the *Odyssey* he identified meanings which eluded others, and took advantage of this to further his own ends. The hero had to be adept at reading nonverbal cues, from architecture to smiles (Nagy 1990: 202–22; Lateiner 1995). And in later centuries, when we have more literary evidence, the complexity of reading material culture and its embeddedness in the same contests over meaning as the written sources are clear. I discuss archaic debates over the meanings of Near Eastern artifacts in chapter 5; and by the fifth century tragedians were, if anything, even more interested in the ambiguities of material symbols. Aeschylus took it for granted that the audience of his *Agamemnon*, staged in 458, would pick up the nuances of its famous carpet scene. By unrolling a purple carpet between her returning husband’s chariot and the entrance to the palace, Clytemnestra trapped Agamemnon between either belittling his own authority by refusing to step on such a symbol of kingliness or hubristically soiling the wealth embodied in the filmy material. The play turned on his vacillation in the face of the material trappings of power (Crane 1993). Where our evidence is densest, in the fourth century, any good orator

knew that a passing reference to hairstyle, choice of cloak, or tableware spoke volumes about his rivals' wicked intentions (Ober 1989).

Classical historians sometimes respond to the ancient obsession with material culture with mechanical direct interpretations (I. Morris 1992: 17–21). The method is simple: we read the texts, which tell us that object A signifies idea B, etc. Symbolism is a code. We find out what A means, and our job is easy. We look at a carved Roman sarcophagus and can say that the snake means death, the olive means life, the egg is a sign of rebirth, and so on.

These one-to-one associations are not necessarily wrong, but our sources make it clear that things were more complicated. Even when we have texts directly relating to the objects we have dug up, we can rarely assign “the meaning” to an artifact, or assume that it had any such meaning independent of its context of use. A gold cup in a grave may mean something very different from one given to a god, or displayed in a dining room. The best example is the so-called “Orphic” graves of the late fourth and third century. The people who cremated a man at Derveni in Macedonia around 350 used grave goods much like those in other rich burials, but burned with him a papyrus roll describing an afterlife radically different from the mainstream Hades (Themelis and Touratsoglou 1997; Most 1997). Gold cups had different religious meanings for different buriers (I. Morris 1992: 17–18, 104).

Some associations carry over from one context to another, and in that sense we can speak of an irreducible core of meanings given to gold cups by a particular group at a particular moment; but many important meanings were entirely context-dependent. To pour libations to the gods from gold cups as the Athenian fleet sailed for Sicily in 415 was a fine and patriotic thing (Thucydides 6.32), but to say that a man took pride in owning gold cups was to imply that he lacked the qualities of the true citizen (Demosthenes 22.75). To say that your enemy went round positively bragging about his cups was even worse – it evoked images of antisocial hubris (Demosthenes 21.133, 158). When Andocides (4.29) wanted to convince a jury that Alcibiades was beyond the pale of civilized society, he took advantage of these associations by alleging that Alcibiades tried to create the impression that gold vessels belonging to an Athenian embassy were his own, not only pretending that cups made him a better man, but even lying about owning them.

To bury a gold cup with a dead relative may have been even more hubristic. In the roughly 3,000 fifth- and fourth-century graves known from Athens, there is not a single example of this (I. Morris 1992:

108–27), although we know from exports to Thrace that Athenian craftsmen made superb precious tableware. The literary sources do not give us “the meaning” of gold cups, which we can then apply to our finds. But they do give a sense of the semantic range of artifacts, the possibilities available to the people who used them, and the limits of plausible interpretation.

Verbal and nonverbal languages are not the same. Geertz, one of the main advocates of what he calls the “life is a text” model in anthropology, notes (1983: 33) that its “proponents incline toward the examination of imaginative forms: jokes, proverbs, popular arts,” but have been less successful – indeed, have hardly tried their hands – at examining institutions, worship, or war. The gaps between material culture and texts are even more pronounced. Discussing archaeologists' borrowings from linguistic structuralism, Ernest Gellner observed that “the whole point is this: . . . the entities used in [linguistic] symbolism and communication operate under a rather special economy, without scarcity. Or, better, the other way around: symbolic systems choose as their units, their vehicles of communication, elements whose cost approaches zero” (Gellner 1985: 150). That is patently not true of the material world, whose symbols are very much governed by rules of scarcity. An Athenian could not just decide to flaunt gold cups, as Andocides' story about Alcibiades, true or not, illustrates. First he had to get hold of some.

We need different intellectual tools to analyze pottery than poetry, but we should analyze both correlatively, within the same cultural framework. Pots and poems were used by the same people, who – in case we should be foolish enough to doubt it – make it clear in their writings that they employed both in efforts to construct and contest categories. The resolutions differed from one context to another, but there is no reason other than the defense of academic boundaries for us to lump together all material culture, regardless of context of use, as one discourse and to separate all verbal culture as another, so that we can look for “the mismatch between texts and archaeology [which] can articulate important contradictions between operative social contexts” (Small 1997: 218). I argue in Parts III and IV that we must examine verbal and nonverbal languages together, comprehensively, in contextual detail, to identify cleavages which often have little to do with the medium through which people expressed them.

Borrowing another Geertzian phrase, all these categories of evidence are the remains of models *for* reality as well as models *of* reality (Geertz 1973: 93). Neither texts nor artifacts are radically thinned descriptions against which thick descriptions based on the other can be tested. But looking for the intersections of arguments

based on such different forms of evidence ties our interpretations to themes which would have made sense to the ancients, and thickens our descriptions.

Andrén closes his book (1998: 180) by asking “Is it possible for one and the same person to master artifact and text, and the scientific traditions surrounding them, in such a way that new, interesting, and meaningful contexts really can be constructed?” We expect prehistorians not only to know the full range of archaeological data, but also to understand excavation and survey methods, and shifting theoretical debates. Historical archaeologists need to do all of these things, but also need to know the primary textual evidence, the difficulties of locating and reading the texts, and the arguments raging among text-oriented historians. To do anything less would be the equivalent of a prehistorian deciding to work only with survey data and ignore excavated material, or perhaps to take no notice of the secondary literature.

David Austin, discussing the state of medieval archaeology, complains:

Try discussing 7th-century Poland without mentioning the Slavs, or 13th-century rural England without reference to the manor, and our credentials as commentators on the past would be seriously questioned by historians. We allow ourselves, therefore, to be compelled to integrate, and this carries with it the obligation to understand primary written sources, how to extract social or economic meaning from them, and how to apply them to the locations and communities we are researching . . . After a career of reading the documents, the history and the historical geography of the medieval landscape of Britain, for example, I would earn no discredit from my colleagues if I were to say that I had little time, inclination or energy to take on the whole framework of social theory and anthropology, the main disciplines to which our prehistorian colleagues have turned . . . The fact is that we have been so trapped by the agenda set by historians and so weighed down with the paraphernalia of medieval history that we scarcely feel able to interpret and analyse in the modes of contemporary archaeology. When we do try, we are accused by historians of, at best, irrelevance or lack of scholarship, and at worst of uttering jargonistic claptrap. (Austin 1990: 12–13)

It is asking a lot for one person to control so many fields, but as Americanists realize, the whole promise of historical archaeology lies in combining approaches, potentially transforming both text-based historiography and archaeology. Our educational institutions may not encourage people to feel equally comfortable with Chaucer,

abandoned fourteenth-century villages, and Bourdieu; but that is no reason not to try.

The Structure of This Book

I divide the rest of the book into three pairs of chapters and a conclusion. I have already mentioned the importance of the institutions within which we work and the audiences we imagine in shaping research. In Part II I consider these factors in the history of archaeologies of Greece. I ask why archaeologists ask the questions they do, why they collect particular kinds of evidence to answer them, and specifically why archaeologists of Greece rarely think of what they do as cultural history. Tensions run high between those who would align Greek archaeology with anthropological archaeologies of other parts of the world and those who think it is doing just fine, but in chapter 2 I argue that this opposition is unproductive. We can only understand the issues historically.

A hundred years ago, eminently sensible people made reasonable decisions about how to organize scholarship. They found solutions to aesthetic, institutional, and political dilemmas, and worked out ways to align Greek archaeology with science while maintaining connections to high-status philology and contributing to burning questions of the day about the origins of Europeanness. They won general agreement that Greek archaeology was fundamental to the humanities. But it is as if their very success prevented later generations from moving on. In the 1990s we face different questions, for which our long-standing academic confinements are no longer so appropriate, and which threaten the prestige and funding enjoyed by classical archaeologists. Historical analysis of our own subject is not narcissism; only in this way can we understand why we have asked some questions and not others. And only when we know this can we respond to our situation, which makes new questions more interesting than some of the old ones. Throughout the book I return to this issue of how we decide what are important questions to ask.

In chapter 3 I explain why I think Iron Age Greece is an important time and place; why I think certain things about it are more important than others; and why I think certain combinations of evidence are more promising than others. In the 1890s academics finally accepted Schliemann's discovery of Bronze Age civilization as a fact, and at the same time, Flinders Petrie cross-dated Mycenae with the Egyptian New Kingdom. This created an unforeseen problem: there was a four-century gap between the end of the Mycenaean world and the

first Greek poetry. Until the 1930s this “Dark Age” was a low-status field, receiving little serious attention, but after 1945 it became a major focus of research, particularly in Britain. Some historians redefined the Dark Age as a Heroic Age, arguing that Homer reflected Iron Age rather than Bronze Age society. Archaeologists continued to see the Iron Age as a Dark Age, but now said that a uniquely Greek way of life was created in this Dark Age (and particularly the eighth century). I examine how these radically opposed views co-existed and how they fused in the 1980s. Post-war Dark Age scholars transformed this field from an intellectual backwater into the site of an epochal transformation, when “the Greeks” stepped onto the stage of history. These were the claims that drew me to Iron Age Greece in the early 1980s; they continue to make it worth thinking about and arguing over.

I use intellectual history to denaturalize the questions we ask. By taking our forebears seriously, setting them in context and thinking of them as real people, rather than putting them on pedestals or reviling their stupidity, we make their questions and methods make sense. We see what they tried to do; we see why we maybe should not try to do the same things any more; and, hopefully, we develop some humility about our own claims. Looking at things on a timescale several generations long makes it easier to grasp how serious and fair-minded appraisal of the evidence then available constrained ideas, but also how external forces – power, money, politics – shaped what parts of that evidence people took seriously, and what they thought it meant. But above all, a historiographical approach shows in ways that nothing else can that it is up to *us*, here and now, to set the agendas for archaeology. There is nothing in the data themselves to tell us what is important. If anything, scholarship works the other way round, with our decisions about what is important determining what we count as data. We decide, and the more we know about the forces which influence our decisions, the more scientific we can be.

Classical scholarship has many strengths, but one abiding weakness is the survival of the positivist idea that researchers fashion bricks for some communal building project. If we all make bricks properly, the theory runs, someone will eventually put them together into a giant edifice of knowledge, like the great classical encyclopedias. Classicists often talk as if engaging with other scholars’ arguments, taking the time to check references, re-read sources, break down the case into its component parts, and offer alternative interpretations is a profound personal attack, an act of vandalism against the sacred structure, rather than the highest compliment one specialist can pay another. I cannot share this attitude. None of us enjoys seeing work over which we have labored long hours picked apart and called superficial, speculative, or

misguided. But doing history is a conversation. The conversation may not reach definitive answers, but history as an academic discipline is done in the act of arguing. I offer Parts III and IV in this spirit. Each consists of two quarrelsome chapters, examples of history as what Lucien Febvre, the founding father of the *Annales*, called *combats et débats*: that is, arguing about ideas, evidence, and methods, and in doing so trying to shape what friends, colleagues, and students think is worth talking about.

In Part III I set out an area for debates which I think makes the archaeological/cultural history of the Iron Age worth doing. I build on the intellectual history in Part II: ancient Greece is important partly because our forebears made it so. For the past two-hundred years, westerners defined classical Athens as the fountainhead of European-ness, the beginning of a unique cultural tradition setting us apart from (and above) the rest of the world. Late twentieth-century academics look on this grand narrative with suspicion, and in some circles anything to do with ancient Greece is tainted by association with Eurocentric charter myths. Some feel we should forget Greece. I believe precisely the opposite. The appropriation of Greece within this ideology demands that we continue to debate categories which intellectuals of the last two centuries have made into some of the most potent symbols of western identity. A new theory about the Parthenon frieze or Socrates’ skin color is front-page news, and it is the height of folly for academics to abandon this ideologically charged subject to the platitudes and deceptions of politicians and advertising agents. We need to take ancient Greece seriously and to bring to bear on it the same intellectual tools we apply to other parts of world history.

And when we do so, I suggest, we find a second reason to study this small, far-off land. The Greek evidence resists appropriation, stubbornly failing to fit our frameworks. This was an odd place. In particular, I argue, Greek ideas about equality, often seen as the origins of liberalism and democracy, utterly confound the categories of thought which archaeologists bring to complex societies. Troublesome societies like Greece show up the limitations of our frameworks better than abstract analysis.

The interest of these questions dawned on me in 1993, when some people seemed intent on making the 2,500th anniversary of Cleisthenes’ political reforms into a line of power from Athens to modern representative democracy. In the flurry of activity that year, I found myself thinking more and more about what ideas of equality made the institutions of democracy possible. In chapter 4, I look at literary sources from fifth- and fourth-century Athens. Engaging with the words Athenian men used to describe their political system makes

democracy an issue in cultural history. My central question is what made democracy *thinkable*: that is, how and where people drew imaginary lines within the community, saying that some were full members and some were not. What the Athenians called *demokratia* was the rule of men. Athenian men systematically downplayed wealth and class while raising ethnic, gender, and cosmological distinctions to unusual heights. In Athens, poor, local-born men inherited the earth. Athens was no paradise, but it remains important because it was *different* – it fits into no ready-made modern schemes.

In chapter 5 I ask how such a society came to be. I examine poetic constructions of the self across the seventh and sixth centuries, arguing that we see the creation and then the collapse into each other of anti-theoretical “elitist” and “middling” ideologies of the good community. These competing visions created opposed identities. The former blurred gender distinctions, and linked aristocrats with gods, heroes of the past, and rulers of the east, to create a single distinction between nobles and commoners. The latter did the opposite. I explore the tensions and antagonisms between the two worldviews, suggesting that ideas of time and space were central to these.

Part III is not what most of us would call archaeology. Material culture plays only a small part, and I will doubtless be accused of returning the field to its “handmaiden to history” role. But the methods I follow are central to my vision of archaeology as cultural history. Text-aided archaeologists and archaeologically minded historians too often concentrate on one kind of evidence and rely on secondary syntheses for the other. I believe that the best opportunities lie with people who are equally at home with written and mute evidence, knowing the potential and the limits of both. In chapter 5 I argue that archaic poetry provides a framework for reading the material record, but I also emphasize its limitations. Panhellenizing processes generated our texts, creating poetry relevant to all central Greek cities but specific to none, dealing with themes of enduring interest, but which cannot be tied to specific events. The poems leave us with a sense of archaic central Greece as a unit, but blur diachronic and regional distinctions within it; and most serious of all, the texts only appear in the late eighth century, right in the middle of the changes we most need to examine.

In Part IV I try to connect texts and artifacts. I draw on Darnton’s methods (pp. 10–11 above), tracing ideas of the ancient race of heroes and the wonders of the east back to around 1000 BC. I concentrate on the site of Lefkandi, but put it into a larger context, combining micro- and macro-archaeologies, empathetic and statistical approaches, and material and mythological evidence. I argue that a “Dark Age” began

in central Greece in the late eleventh century with the formation of a new social and cultural order, which made a usable past out of the ruins of the lost Mycenaean age. I see continuities between ideas about Greekness, the heroes, and the east which took shape in the tenth century and those of archaic and classical times, but also see profound discontinuities in the eighth century, in which the Dark Age cultural traditions took on new meanings. Against recent revisionists, I insist on the importance of the eighth-century transformation, which permanently changed concepts of wealth, gender, ethnicity, and cosmology, and created the patterns which I identify in the archaic literary sources. It was a rupture of Foucauldian proportions. The new identity of the male citizen was created, and along with it the preconditions for extreme gender inequality, large-scale chattel slavery, male democracy, and an astonishing cultural explosion. A mixed bag indeed.