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Cambridge World Archaeology’s list continues to grow and a recent pair of studies adds to the increasingly large body of English-language archaeological literature on Eurasia. These new volumes—P.L. Kohl, *The Making of Bronze Age Eurasia* (hereafter *Eurasia*), and L. Koryakova and A.V. Epimakhov, *The Urals and Western Siberia in the Bronze and Iron Ages* (hereafter *The Urals*)—are in many ways complementary, though perhaps more by accident than design. As is only to be expected, the perspectives and theoretical concerns of an anthropologically-trained American archaeologist (Kohl) who has long been one of the leading disseminators of Central Asian and Caucasian archaeology in the West, are very different from those of two Russian archaeologists (Koryakova and Epimakhov). While there is some overlap in subject matter, there are plenty of areas in which the books differ and complement each other, geographically and chronologically as well as in theoretical scope.

When I was an undergraduate, Ruth Tringham’s *Hunters, Fishers and Farmers of Eastern Europe, 6000-3000 B.C.* (London, 1971) was my introduction to European archaeology and a geographically-challenged student could have been forgiven for thinking that the Bosporus was about as wide as the Atlantic, for the gulf between European and Near Eastern archaeology, intellectually at least, was massive. Central Asia was not yet even a concept for many of us (and not just undergraduates), though that was to quickly change with the publication of Masson and Sarianidi’s *Central Asia: Turkmenia before the Achaemenids* (New York, 1972). It is, therefore, intriguing to note that Kohl’s overview of the Chalcolithic prelude to his main subject opens with a discussion of Cucuteni-Tipol’ye sites in Bulgaria and Romania, a subject that was squarely within the purview of Tringham’s book. This begs the question, just where are we when we speak of Eurasia, and how do we draw boundaries to delimit the object of study? What stays in, and what remains out, in presenting a survey of this subject? This is an extremely difficult question, and it is not addressed directly in Kohl’s introduction. The frontispiece (p. xxiii) shows a map of the ‘Eurasian Steppe Zone and the Greater Ancient Near East’, which indeed extends (p. 126) from Hungary in the west to Manchuria in the east (Bulgaria and the Aegean have somehow been joined to Turkey in fig. 1.1, perhaps a Freudian archaeologist with Eurasian interests would have something to say about this?), but the scope of the book is much broader. The penultimate chapter (ch. 5), examines, among other things, ‘secondary states east of Sumer’, including the recent discoveries in southeastern Iran (Jiroft/Hallil Rud region) and Xinjiang. But why stop there? There is not much rhyme nor reason in what has been included (obviously some sites and finds could not be omitted) and what has not. One suspects that writing a book devoted to Eurasia in the literal sense, Europe + Asia, was never on the agenda. One can see why Kohl has been vague about the geographical boundaries of his study, which in reality would never have been fixed, but a reader is nevertheless often left wondering why some topics or cultures were included and others were not.

By contrast, *The Urals* is concerned with a much more narrowly circumscribed region. Fig. 0.1, a ‘physical map of Eurasia, with area under study’, extends from China to the North Atlantic, taking in all of northern Europe and Asia. The southern margin is a line that seems to run quite arbitrarily through Iran, Afghanistan, Pakistan, and northern India across to southeast Asia (obscured by the scale on the map). *The Urals* is concerned only with ‘the central part of northern Eurasia’, clearly outlined by a solid black line on Fig. 0.1, which includes ‘the Cis-Urals or easterly part of eastern Europe, the Trans-Urals or the westerly part of Siberia, coinciding with the basin of the river Irtysh, mainly its western bank’ (p. 2). The only inconsistency
seems to be a chronological one. The date of the latest material included varies from region to region and while some coins from the 1st and 2nd centuries AD are included from Glyadenovo (p. 274; discussed further below), this is generally later than most of the Iron Age data discussed. Moreover, although the authors suggest (p. 19) that the Late Iron Age ended at 600 AD, their table 0.5, ‘Chronology of the Iron Age’, generally cuts off sometime after 300 AD, and there is certainly no consistent treatment of Parthian (ca. 250 BC - 250 AD), let alone Sasanian (ca. 250-650 AD) material in Eurasia, or of the material culture of indigenous groups, like the Alans and the Huns, who were contemporaries of the Sasanians. It would probably have been preferable if the authors of The Urals had picked an historical event, like the collapse of the Achaemenid empire, the death of Alexander the Great, or the foundation of the Sasanian empire, or even the Islamic conquest if they were truly intent on dealing with the Late Iron Age, and applied this chronological datum consistently across the region to bring the book’s coverage to a conclusion.

It would be extremely easy to go on about problems like these—not faults or errors, but archaeological, palaeoclimatic, chronological, and sociological problems that appear on virtually every page. There are so many problematic aspects of terminology (definitions of cultural groupings of varying size and their constitution), chronology, and socio-economic reconstruction in these volumes—each of which tries to present a brief but still coherent overview of an absolute mountain of primary literature and data—that one could harp on about all of these shortcomings. But certain it is that Kohl, Koryakova, and Epimakhov are acutely aware of these issues, as the repeated caveats, qualifications, and cautionary exhibited on virtually every page testify.

Instead, let me be up front in saying that, having read both volumes, I am absolutely disinclined to harp on about shortcomings for I am overwhelmed by the sheer richness of the archaeological materials presented. I would advise all readers to simply enjoy this archaeological richness, revel in the names of cultures and their distinctive material that will be foreign to many, and just enjoy what is offered here. By the time I was halfway through each of these books, I had come to the conclusion that there was simply no point in worrying about whether culture X was nomadic, or culture Y was a fusion of cultures A and B. These are not the sorts of issues that the authors, no matter how competent, can solve. They have diligently surveyed the literature, primary and secondary, presented the prevailing and dissenting views on these and many other issues, but often concluded that we simply don’t know the answers yet. Rather, instead of taking all of this to heart and quibbling with the interpretations, I found it far more productive to simply try to soak up, at least in a first reading, what was for me mostly new data about which I had previously read very little.

While it may sound deeply unacademic and downright uncritical, it is perfectly possible to read these volumes without paying too much attention to the interpretations of either the authors or their predecessors. To even list the many fascinating topics discussed in Eurasia and The Urals would require more space than I intend to devote to this review, but by way of a sample, albeit a non-random one, let me single out a few.

In The Urals I was struck by the clear discussion of the Sintashta culture (p. 60ff); the presentation of the manifold variants of the Andronovo cultures (p. 123ff); the summary of the ‘Bronze Age Trajectory’ (p. 178ff); the explanation of the many Eurasian worlds of the 1st millennium BC (p. 201 and fig. 5.6); the theoretical discussion of the origins of Eurasian nomadism (p. 209ff); the ethnology of the Scythians and Cimmerians (p. 222ff; cf. Damayev 1979, 1992:159-162; Zadok 1977:121-124); the numismatic finds (Kushan and Han Chinese) from the Glyadenovo bone-producing site (p. 274); the evidence of Choresmian writing using Achaemenid-style Aramaic (‘Reichsaramäisch’) from Isakovka (p. 304ff); and the heavy iron weaponry and armor of the Sargat culture (fig. 8.18), which is particularly interesting in light of what we know about the evolution of the armor and weaponry of Parthian and Sasanian heavy cavalry (so-called clibanarii, cf. Mielczarek 1993; Olbrycht 1998).

In Eurasia I was particularly interested in the discussion of the huge Cucuteni-Tripolye sites in Romania and Bulgaria (p. 23ff); the Caucasian (Kura-Araxes culture) connections with Halaf and Uruk Northern Mesopotamia (p. 68ff); the dispersion of Early Transcaucasian...
settlements and their dating (p. 96ff); the sources of the tin used in Georgia and Armenia (p. 108); the Karashamb silver goblet (pp. 116-117 and fig. 3.28); the extraordinary anchor-shaped axes from Karashamb, Bedeni, and Kyurdluk (fig. 3.29); the up-to-date treatment of horse domestication (p. 137ff); the detailed discussion of Bronze Age herding vs. Eurasian mounted pastoral nomadism (p. 158ff); the presentation of data on the impressive Kargaly metallurgical complex and the extraordinary bone tools found there (p. 170ff); the presentation of the Gonur Depe 'royal' burials in Turkmenistan (p. 196ff); and the presentation of the Bactria Margiana Archaeological Complex or BMAC materials (p. 201ff).

As noted above, the sheer breadth of both The Urals and Eurasia necessarily enforced a large measure of selectivity on the authors with respect to the choice of subject matter. While The Urals attempts to be fairly systematic and aims for more in-depth coverage (albeit within a smaller geographical area), Eurasia is characterized by a more punctuated approach. This is presumably because, in Kohl's opinion, “The evolutionary ‘action’ takes place in different areas at different times across the steppes and along the northern frontier of the Ancient Near East. In the Neolithic, cultural developments are most spectacular in Anatolia; in the Chalcolithic, it is the Balkans, stretching across Romania, Moldova, and western Ukraine, then the Caucasus, southern Central Asia and eastern Iran, across the Urals and so forth” (p. 258). These shifts, he suggests, are reminiscent of “the modern historical era with its consecutive shifts in world power from Portugal and Spain to the Netherlands, France, England, and, currently, the United States”.

In conclusion, I have intentionally said nothing about the very interesting observations on archaeology and language or the Indo-European problem in Eurasia, just as I have not dwelt on the often suspect sociological categories and reconstructions in The Urals. As noted above, there is an enormous amount one could criticize at a conceptual level in the past century of archaeology across Eurasia, but it seems more productive, now that Western scholars have access to such an immense range of material, to simply look at it, think about it, come to one's own conclusions, and not dwell on the shortcomings of this or that interpretation. My only quibble with these otherwise fascinating and highly stimulating books concerns a few technical matters. Too many of the photographs and drawings are of sub-standard quality for works published by Cambridge University Press. In many cases the original scans were probably not done at a high enough resolution. More annoying, however, is the large number of spelling and syntactical errors in The Urals which should have been caught by a competent copy-editor. I have no intention of recording here for posterity every one I came across, but it is clear that misspellings like ‘Athlantic’ for Atlantic (table 0.1); ‘Parphian’ instead of Parthian, ‘Assiria’ for Assyria, or ‘Neo-Babilonian’ for Neo-Babylonian (table 0.5), should have been caught. Russian syntax and usage in English is perfectly acceptable from a non-native speaker, but again, it is the responsibility of the copy-editor to sort these problems out before a book appears in print. Examples abound, such as, ‘The continuance of aridity and moisture, if to judge by comparative data from eastern Europe and northern...
Kazakhstan, was different on either side of the Ural mountains’ (p. 10); or ‘Here the climatic and landscape variability was not as contrasting as in the south, but it did take place’ (p. 11). It is perfectly obvious what the authors meant, but that is not the point. This is not acceptable English for a volume published by CUP. Some names have been mangled as well. One suspects that the editors had no idea what the authors of The Urals meant (p. 274) when they referred to the Kushan ruler ‘Kundzhula Kadfiz’—commonly known as Kujula Kadphises—or Tsar (!) Khuvishka—another Kushan ruler better known as Huvishka.

In conclusion, these two books are very welcome additions to the burgeoning body of English-language literature available on the prehistoric and early historic cultures of Eurasia. They provide helpful syntheses for non-experts in the field, opening up these vast areas to archaeologists more familiar with the adjacent areas. Undoubtedly they will fulfill admirably what must be one of their main goals—to stimulate more research by a wider array of scholars on the archaeology of the vast super-continent of Eurasia.

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Historical Archaeology

Whither Historical Archaeology in Africa?

By Paul J. Lane


Introduction

Historical archaeology first emerged as a distinct sub-field of the broader discipline in North America during the mid-20th century. Initial focus was principally on the study of archaeological remains of buildings, artefacts, and settlements associated with European colonization for which documentary sources also survived. Attempts to marry written sources with the material traces of human activity and critically assess these against one another continue to be primary concerns. However, the sub-field has matured significantly in recent decades, especially as different geographical areas have been subject to academic study from the perspective of ‘historical archaeology’. Nevertheless, there remains considerable variation in definitions, methodologies, and theoretical underpinnings. These range from the highly cynical suggestion that historical archaeology can be ‘one of the most expensive ways of finding out what is already known’ (see Deetz 1991:1), through definitions which equate it with the archaeological study of time periods and events for which written sources are available, and of societies that have developed a literate tradition (e.g., Deetz 1977; Beaudry 1988), to the suggestion that its focus is the archaeology of European expansion and exploration from the 15th century onwards (Deagan 1991; Deetz 1991; Hall 1993) and/or the emergence of the modern world (Schuyler 1970; Orser 1996). These last two perspectives have come to dominate the field, and both Schulyer’s definition of historic sites archaeology as study of “the material manifestations of the expansion of European culture into the non-European world starting in the 15th century and ending with industrialization or the present depending on local conditions” (1970:84), and Deetz’s definition of historical archaeology as “the archaeology of the spread of European culture throughout the world since the fifteenth century and its impact on indigenous peoples” (1977:5), are widely cited.

That none of these definitions is entirely comprehensive, especially when viewed from a non-European or non-Western perspective, is evident from recent overviews and syntheses of what constitutes historical archaeology in different parts of the world (e.g., Funari et al. 1999; Reid and Lane 2004; Hall and Sillman 2006). Negotiating one’s way through such diverse definitions and deciding on which if any is most appropriate is thus not only fraught with pitfalls and contradictions, but also often a matter of personal inclination and/or the dominant research traditions of different regions.

This is especially well illustrated on the African continent. A recent Encyclopedia of Historical Archaeology (Orser 2002), for instance, includes a range of entries on aspects of African historical archaeology. These encompass regional overviews for West, East, North, and South Africa, a continent-wide discussion of ‘maritime archaeology’, and various individual entries about specific settlements such as Aksum, Oudepost, Elmina, Qsar es-Seghir, Great Zimbabwe, and Cape Town. Leaving aside all the practical difficulties, such as constraints on space, availability of contributors, failures by authors to deliver and so on, which inevitably have a significant effect on the final content of any volume, this selection is in no small part due to the editor’s objectives, which match his own wide-ranging and inclusive definition of historical archaeology in global perspective expressed elsewhere:
we may define historical archaeology as a multi- and interdisciplinary field that shares a special relationship with the formal disciplines of anthropology and history and seeks to understand the global nature of modern life (Orser 1996:27, emphasis added).

Orser goes on to stress that in his view, historical archaeology “should not be interested in all literate cultures, but only those that inhabited a time…broadly termed ‘modern times’” (ibid.).

Yet, both the individual entries to his edited encyclopaedia and the absence of other, equally viable entries, also speak volumes not just about the diversity of approaches to historical archaeology on the African continent, but also about significant gaps in current research. To judge from the different regional reviews, for instance, it would appear that in South Africa (Malan 2002) the dominant notion as to what constitutes historical archaeology is that it deals with the archaeology of European colonial encounters (see also Hall 1993), and generally with greatest reference to the European spaces and contexts of that encounter rather than from that of indigenous places and settings (cf. Parkington and Cronin 1979; Hall 1998). Based on the entry for West Africa (de Corse 2002), much the same could be said for this region as well, especially with reference to the Atlantic slave trade (e.g., de Corse 2001a), although there are signs that rather more attention is now given to indigenous settings (e.g., de Corse 2001b; Kelly 1997, 2004) than was the case a decade or so ago.

These research traditions contrast quite markedly with the situation in East Africa (see Schmidt 1990; Lane 2002), where the archaeological investigation of European colonialism, with a few notable site-specific exceptions (e.g., Kirkman 1974; Posansky 1959, Garlake 1967; Matson and Sutton 1965; Sutton in press), has been rare (see also Horton 1997). Instead, much of what could be considered historical archaeology deals with the archaeology of the Swahili coast (Kirkman 1957; Kusimba 1999), where historical sources range from early Classical sources such as the Periplus of the Erythrean Sea (Casson 1989) through 10th-12th century Arabic material (e.g., Gibb 1962) and various Chinese maps and texts (Wheatley 1964), to later European and Swahili documents (Strandes 1899; Freeman-Grenville 1962; Chittick 1976), and the as yet largely untapped potential of Omani and Indian sources as well. Alongside this well-established research tradition, which currently goes under the name of coastal or Swahili rather than ‘historical’ archaeology, there also exists a more radical perspective, first proposed by Schmidt (1978, 1990), which uses local oral traditions as the main external, non-material evidential source.

The final regional entry in the Encyclopedia of Historical Archaeology covers North Africa (Straughn 2002). Here, owing to the very different historical trajectory of the region when compared with much of sub-Saharan Africa (although there are obvious parallels with early work in East Africa), much of the initial focus was on urban settlements such as Carthage, Leptis Magna, and comparable North African cities of the Classical world (for an overview of Roman North Africa, see Mattingly and Hitchner 1995), and more latterly on Islamic and/or Arab sources and towns such as Qsar es-Seghir (Redman 1983, 1986) and Al-Basra (Benco 2004). There has been far less research on, for instance, the archaeology of the Ottoman Empire (although see, e.g., Adams 1992; Alexander 1995, 2000) or post-15th century European incursions, and even fewer studies aimed at integrating any of the local ‘indigenous’ oral traditions (although see Edwards 2004).

The critical point to note here is not that archaeologists working in one part of Africa have either a ‘better’ or ‘more flawed’ understanding of what constitutes historical archaeology. Rather, what needs to be recognised and acknowledged is just how much influence research traditions can have, and how important it is to reflect on these and address them by changing research questions and agendas (Robertshaw 2000). Assuming, that is, that one accepts that the concept of historical archaeology has some validity on the African continent, which it would appear not all scholars believe to be the case.

In a recent review of African Historical Archaeologies (Reid and Lane 2004), for instance, Professor Graham Connah questioned whether the notion of historical archaeology was a particularly helpful or appropriate concept when applied to African contexts (Connah 2004a:477; see also Connah 2006, 2007; Robertshaw 2004). At first glance, such musings...
might seem rather surprising, coming as they do from one of the doyens of African archaeology, whose own work on the continent has often focused on archaeological sites and landscapes for which supplementary written and oral sources are available, such as at Benin in Nigeria (Connah 1975) and Kibiro in Uganda (Connah 1996). Connah is also the author of a book devoted to the archaeology of colonial settlement in Australia (1988), in which he makes good use of documentary and photographic sources to aid and expand on interpretations of the surviving material record of this phase in Australian history. Most recently, he has co-authored a paper on the historical archaeology of a 9-pounder British naval gun preserved in Kampala, Uganda (Connah and Pearson 2002). Yet, it is precisely Connah's experience of seeking to integrate material, written, pictorial, and oral sources so as to provide a richer and more textured understanding of the past in two quite contrasting continents, that should give us pause to reflect on the usefulness of the concept of historical archaeology and how it has been, and is being, applied by archaeologists working in different parts of Africa.

**The Case Against Historical Archaeology in Africa**

Connah's dissatisfaction with the use of the term historical archaeology with reference to African contexts and materials stems from two core concerns. Firstly, that several generations of archaeologists have worked hard to rid the study of Africa's past of the kind of bias that a division between 'history' and 'prehistoric' has often invoked. Namely, that 'prehistoric' instead of being regarded simply as a time period prior to the invention of a system of writing and the production of written texts (which in global perspective is of variable duration and date of both beginning and ending), is taken to imply a lack of or absence of historical processes and events. This latter interpretation of the term can convey an impression of cultural stasis and backwardness, and typically also ideas of racial inferiority or unimportance, as most famously encapsulated in Trevor-Roper's assertion in the early 1960s that, aside from the history of Europeans, the rest of Africa's history amounted to no more than "the unrewarding gyrations of barbarous tribes in picturesque but irrelevant corners of the globe" (1963:871). Archaeologists of Connah's generation were among some of the first to explicitly confront such views, and their accomplishments and the work of a later cohort of scholars demonstrate unequivocally just how mistaken Trevor-Roper was, as is well illustrated in David Phillipson's overview of African archaeology now in its third edition (2005), and also Connah's recent collection of brief yet highly accessible essays on different aspects of three million years of human and hominid activity on the African continent (2004b), written specifically with the non-specialist in mind. No wonder then that he now believes that archaeologists investigating the African past should seek "to create a seamless account of human endeavour" (2006:2) and "cease to think of 'prehistoric archaeology' and 'historical archaeology' as separate entities" (2004a:477), as opposed, for example, to his earlier belief that the African past (or at least, that of Benin) could be reasonably split into prehistoric and protohistoric periods (1975:2).

Connah's second argument against the use of the concept 'historical archaeology' is that there are a number of competing definitions and as a result the term is ambiguous. Far better then, and in keeping with his desire for a seamless narrative of human endeavour, to talk simply of just plain 'archaeology' when dealing with the African continent. This too seems to represent a change in his thinking, as it is certainly at odds with the views expressed in his book on the settler archaeology of Australia (tellingly sub-titled, *The Archaeology of Australia's History*, which given its subject matter would seem to imply that the continent also has a prehistoric past!), in which his principal aim was, explicitly, to provide a brief introduction to the historical archaeology of Australia (1988:2). This, he felt, was a valid endeavour on three counts (ibid., 2-5): 1) European settlement of Australia has left a mass of material evidence which archaeologists, as the primary specialists skilled at recovering information from material remains and interpreting it in socially meaningful ways, are best placed to study; 2) that written documents and other archival (i.e., in the narrower sense of the term, historical) sources are often partial and uninformative about certain processes and practices; and 3) that studies of the archaeology of the recent past for which textual and related...
sources are available, provide a good testing ground for models of the relationships between material culture and human behaviour that can be used to interpret the material remains of periods for which neither written nor oral sources survive—i.e., eras that are often glossed over in different parts of the world as being part of ‘prehistory’.

All scholars are entitled to change their ideas about particular aspects of their discipline, and many often do. Thus, the issue here is not that elements of Connah’s oeuvre as an archaeologist seem to have been either explicitly (as in the case of his 1988 book), or more implicitly (as in the case of some of the stated motivations for his research at Benin (Connah 1975:2-3) and Kibiro (Connah 1996:1-2) concerned with historical archaeology, and thus seemingly contradict his recent remarks about the usefulness of the concept. Instead, the issues are: 1) whether the concept does have some validity in reference to African contexts and materials; and 2) whether other archaeologists working on the African continent share his views or not.

On this latter point, judging from the titles under review, it would seem that perhaps the majority of practitioners are happy to dispense with, or see no real need for, the prefix ‘historical’ when writing about aspects of the archaeology of the continent even when making use of oral and written sources in their interpretations of the material record. This said, since these are very different books concerned with very different subject matters, geographical areas and temporal ranges, no proper comparison can be drawn between them in terms of content. Instead, for a better indication as to whether the concept ‘historical archaeology’ has some validity for those who research Africa’s past we need to examine how different authors approach the integration of written and oral sources with the material remains of past activity.

**Methods, Sources, and Source Criticism**

A key feature of historical archaeology, regardless of which definition is preferred, is that it seeks to integrate and interrogate strictly archaeological types of sources (such as artefactual, ecofactual, structural, and architectural remains and their contextual, spatial, and temporal associations and characteristics) with other non-archaeological sources that can be broadly defined as ‘historical’. These include various types of written texts and documents (such as formal histories, unpublished archival records, personal letters, newspaper accounts); pictorial and cartographic materials (such as drawings, maps, and photographs); different forms of orally-transmitted information (such as myths, oral traditions, praise songs, king lists, interview responses, and personal memories); and, in a few cases, also information gleaned from historical linguistic analyses (e.g., Schoenbrun 1998).

In recent years, there have also been attempts to link these kinds of sources with various proxy indicators of environmental conditions (such as pollen and phytolith records, carbon- and oxygen-isotopic signatures, and geoarchaeological information) in an effort to reconstruct the long-term historical ecology of particular regions and habitats (e.g., Mrozowski 2006; for an African example, see Schmidt 1997a).

The degree of attention given to these different non-archaeological sources by individual researchers depends as much on their specific research questions and personal definitions of what constitutes ‘historical archaeology’ as it does on the actual availability of different types of historical information. Orser, in discussing these issues has suggested that three broad approaches can be identified (Orser 1996: 23-28). Thus, where historical archaeology is treated as the study of a time period during which at least some individuals are literate (and thus distinct from earlier periods of ‘prehistory’, i.e., pre-writing), emphasis is obviously placed on the presence of written sources. Since writing was invented and adopted at different times in different parts of the world, the date when such time periods began and their duration are highly variable when viewed in global perspective. Alternatively, where historical archaeology is regarded principally as a method that entails the consideration of material archaeological remains alongside other types of historical evidence, then more emphasis is placed on the nature and availability of ‘other historical’ sources, such as texts, oral testimony, pictorial evidence, and/or linguistic and even ethnographic data, rather than simply on written texts. Again, the temporal range of historical archaeology from this perspective is also highly variable between different continents. Likewise if, as Orser prefers, histor-
ical archaeology is regarded as the archaeological study of 'modern times', then the issues of literacy and the existence of written texts are not so important. Instead, emphasis is placed on documenting how men and women, whether literate or not, responded and contributed to a set of broader historical processes that according to Orser, include global colonisation, Eurocentrism, capitalism, and modernity.

In his book on *The Archaeology of Southern Africa*, Peter Mitchell opts for precisely this definition (p. 380), devoting an entire chapter (chapter 13, pp. 380-412) to this topic. In this he is also continuing a trend that has come to characterise the practice of 'historical archaeology' in South African (as opposed to southern African) archaeology that has already been alluded to. Acknowledging that a number of studies in the region have done little more than document archaeologically what is already known from documentary sources, Mitchell considers that the main contributions of the sub-field have been attained through “the close engagement of material culture and textual evidence” with reference to four, closely related themes as played out against the context of European colonialism. These are, the nature of colonial frontiers; the archaeological evidence of underclasses (who are rarely represented in documentary sources); the material expressions of cognitive structures; and the “archaeology of text” (pp. 381-382). The rest of the chapter is devoted to providing a narrative of European colonial expansion across southern Africa, salted with different examples of archaeological evidence associated with this, and more focussed archaeological studies of the processes involved. Most of the latter have been carried out in South Africa, and include well-known studies such as that by Carmel Schrire and colleagues at Oudepost 1 (e.g., Schrire 1988, 1990; Schrire and Deacon 1989; Cruz-Uribe and Schrire 1991; Schrire et al. 1993), and the work of Martin Hall and other members of University of Cape Town Historical Archaeology Research Group, on the historical archaeology of the greater Cape Town area and the construction of the colonial landscape (e.g., Brink 1990; Hall 1992, 1993, 2000; Hall et al. 1990; Hall and Markell 1993; Malan 1990).

As the work by the historical anthropologists John and Jean Comaroff (1991, 1997) on the impact of Protestant missions and missionaries on southern Africa’s Tswana populations has highlighted, an important component of the process of colonisation is the reconstruction of ordinary everyday lives, the use of space, modes of behaviour, styles of dress, and ways of thinking. Many of these leave some form of material trace making them particularly amenable to archaeological investigation. Analysis of the dissonance between textual, oral, and material sources can reveal something of the ambiguous nature of contact situations and the fluidity of identity constructs that can accrue, and as Mitchell’s review makes clear, archaeologists working in southern Africa are becoming increasingly interested in this aspect from the perspective of historical archaeology. In particular, although such concerns have often been addressed with reference to places primarily associated with the European side of the colonial encounter, such as Cape Town and Oudepost, it is evident that more attention is now being given to localities more directly linked to the indigenous populations and, even more critically, to indigenous accounts and representations of the colonial encounter. Recent examples include studies of 19th century Tswana towns such as Ntsweng and Phalatswe in Botswana, especially with reference to Tswana responses to Protestant evangelism (Reid et al. 1997; Lane 1999); the transformation of the Mutapa state in northern Zimbabwe following the expansion of Portuguese trading networks along the Zambezi basin (Pikirayi 1993); the consequences for Nama and Herero pastoralist communities following the creation of new trading opportunities provided by European whaling and sealing expeditions on the Namibian coast (Kinahan 2000), and of German colonialism, land division, and genocide (Lindholm 2006); and the evidence for resistance by local San hunter-gatherer populations to the expansion of European-style farms and the decimation of local wildlife in the Seacow Valley South Africa (Saitowitz and Sampson 1992; Sampson 1995), and the decimation of other San communities in the Northern Cape (Deacon 1996).

Not all archaeologists working on the continent would agree with this narrower definition of historical archaeology, however. As Peter Schmidt’s book, *Historical Archaeology in Africa: Representation, Social Memory and Oral Tradi-
tions, highlights, archaeological understanding of the later pre-colonial past of all parts of the continent is deeply informed by the wealth of oral traditions concerning patterns of human settlement, political structures, and belief systems that have been assembled by scholars. Chapter 12 of Mitchell’s book (pp. 344-379) is a case in point. Entitled “Later farming communities in southern Africa”, this traces the expansion and consolidation of farming communities across southern Africa from ca. 1000 CE onwards, their interactions with neighbouring pastoralist and hunter-gatherer societies, and the emergence of the major Bantu-language-speaking ethnic groups, such as the Sotho/Tswana, Shona, Zulu, and Xhosa who occupied the eastern and northern parts of the region at the time of the European colonial encounter. Mitchell expertly integrates oral, documentary, and archaeological sources, alongside environmental evidence to provide a highly readable narrative and valuable summation of the evidence and different debates. Yet nowhere in this chapter does he refer to this as an example of ‘historical archaeology’, and so Mitchell’s text could be read as implying that history only starts when the Europeans arrive. This is, of course, not his intention – as a reading of the rest of the book amply shows. But, it is a dilemma that needs addressing, not least because widespread use is made of ethnographic and ethnohistoric information concerning southern Africa’s ethnic populations by archaeologists working in the region so as to interpret the archaeological record of much earlier periods. In many cases, this ethnographic information is used with care and due attention to the pitfalls of simply analogies. However, the general failure on the part of scholars to historicize these ethnographies has often had the unfortunate effect of portraying the indigenous populations of the region as unchanging and static (for broader discussion of these issues, see e.g., Stahl 1993; Lane 1994/5, 1998, 2004a).

Internal and External Sources

One response to the kind of critique outlined above would be to abandon, as Connah suggests, the use of the term ‘historical archaeology’ altogether and simply accept that for different time periods archaeologists have the opportunity to use a variety of different sources, but the nature of these sources is not a constant and written sources, in particular, are only available for much of the continent as a direct consequence of European expansion and colonialism. Integrating such sources thus becomes more of a methodological rather than a theoretical issue, although deciding what constitutes, for instance, a ‘European’ as opposed to an ‘indigenous’ source is not always straightforward (see e.g., Schrire 2004:293). Moreover, as recently emphasised by Schoenbrun, archaeologists and historians also need to be aware that the “contents of a written source draw on other types of sources, and any source of one type or another can be put to use in other contexts” of historical production and representation (2006:1413).

Similar points have been made recently by Pikirayi, with particular reference to the distinction that needs to be made between ‘external’ and ‘internal’ sources (2006). By the former term Pikirayi means sources produced by outsiders, either as direct observers, or as often in African contexts, transcribers and copiers of verbal accounts provided by various visitors to foreign lands. Thus, examples might include the large number of published first-hand accounts of voyages of discovery and exploration in Africa by different individuals of European origin that begin to appear from ca. 1500 CE onwards, and especially after 1800; the numerous official documents produced by the different European colonial powers that are now lodged in archives in Africa, Europe, and elsewhere; the records kept by different trading companies, ship captains, and others engaged in commercial activity; personal diaries and letters written by European missionaries, explorers, and administrators; and the various pre-1500 descriptions and guides to parts of Africa written in a variety of languages, such as those referred to above. By ‘internal sources’, Pikirayi means the range of historical sources produced by different African societies. Most obviously these include the oral traditions and histories, myths, and personal memories that have been transcribed by professional historians and anthropologists. However, as he notes (2006), there also exist various texts written in the local vernacular for certain parts of the continent, as well as some indigenous writing systems, notably the Vai script developed in what is now Liberia and the Barnum hieroglyphs that occur in Cameroon.
Just as archaeological sources are subject to a wide range of processes that can affect their formal, spatial, bio-chemical, and contextual characteristics which may affect the integrity of the scientific information they carry, so too can oral, textual, and documentary sources be subject to a wide range of factors that may introduce different kinds of bias and selectivity. For the African continent, both the specific and general value of oral traditions, in particular, as reliable sources of historical information have been discussed at some length and various methods of source criticism devised (e.g., Vansina 1965, 1985; Henige 1974, 1982; Miller 1980; Tonkin 1992; Willis 1996; White et al. 2001). There is also widespread recognition of the need for detailed source criticism when handling documentary and archival sources. In terms of the latter, racial bias is obviously one factor, but many others also came into play. Equally, European perceptions of and attitudes toward Africans were by no means uniform, varying between individuals, different nationalities, as well as between different kinds of sources and over time.

For instance, European pictorial representations of Africans were highly diverse initially and were influenced as much by the conventional rules that governed all forms of pictorial representation as by particular prejudices or views regarding the African continent and its peoples. Consequently, there was no single stereotypical image of Africa or Africans in early Modern Europe, although there was often a tendency to either Hellenise Africans in terms of their appearance and poses, or draw heavily on the styles of earlier, medieval Christian painting (Friedman 1981), which often cast Africans as one of the Magi (Mark 1988). More importantly, in the representations of actual African peoples and settings, a primary concern initially seems to have been with the cultural as opposed to physical differences between black Africans and Europeans, as in the Flemish merchant Pieter de Marees’ early 17th century account of the Gold Coast (Iselin 1994). Inevitably, not all European representations were as favourable. The broadly contemporary written accounts of Khoikhoi herders living around the Cape, by members of the De Houtman expedition of 1595, by John Jourdain (1608), and also Herbert’s fanciful image of a Khoi family (1634), all emphasise an apparent ‘savagery’ and predilection towards cannibalism (Smith 1993). The obvious conclusion to be drawn is that each external source needs to be considered on its own terms and scrutinised for traces of bias and veracity.

Ancient Egypt in Africa, Africa in Ancient Egypt

One widely influential source of ideas about Black Africa and the inspiration of comparisons between Europe and the African ‘other’ has been Ancient Egypt. The history and scope of the influence of these notions on European thought over the centuries is amply illustrated in the collection of papers assembled by David O’Connor and Andrew Reid in their edited book Encounters with Ancient Egypt (2003). This is one in a series of eight books published together as a set under the overall editorship of Peter Ucko, and arising from a conference held at the Institute of Archaeology, University College London in late 2000. While the other books in the series cover many of the other ways in which Ancient Egypt has influenced Western thought and also consider Egyptian’s ideas of their past at different points in history, it is this volume which most explicitly examines Egypt’s relationships with the rest of the continent. As the editors state early in their introduction, the main purpose of this volume is to address the related questions, “was Ancient Egypt to some, or even much, of Africa the source of sophisticated culture as Greece was to much of Europe, or, did Egyptian civilization incorporate fundamental African concepts markedly different from those dominant in the ancient Near East and the Mediterranean lands?” (p. 1). Inevitably, in attempting to answer such questions a more fundamental one, to wit, ‘Is Egypt part of Africa?’, also comes to the fore, and the contributors and editors attempt to address both the narrower ones which provided the stimulus for the volume and the wider one from a variety of perspectives, that include a consideration of both the substantive evidence and different ideological constructions of Africa and Egypt.

Aside from the editor’s introductory chapter, the book comprises eleven single-authored contributions. Of these, seven chapters (chapters 2-8) explore the ideological, political, and economic aspects to scholarly debates on Ancient
Egypt and its role within Africa, while the remaining four (chapters 9-12) focus more on the actual evidence for Ancient Egypt’s engagement with its southerly neighbours. This said, all of the chapters are linked by a number of shared themes. Among these is the degree to which Classical scholars have ignored the evidence for the presence of Black Africans in the Classical world other than as slaves, and more importantly, the contributions African cultures (including Egyptian) made to the shaping of ‘Western Civilization’. The main argument in favour of greater recognition of African contributions and presence are outlined by Bernal in chapter 2, while in chapter 3 North offers a critique of some of the arguments forwarded by Bernal in his chapter and at greater length in Black Athena volumes I and II (Bernal 1987, 1991). While acknowledging the racism and prejudice of earlier generations certainly shaped the tenor of scholarly research on the history of Greek, and thus Western, civilization and underplayed Ancient Egypt’s part in this, North is critical of Bernal’s insistence that the origins of Greek civilization are Black African and argues instead for a more cautious recognition of African contributions without losing sight of the contributions of other non-African societies and historical processes. There are other dangers involved since, when one looks south, the glorification of Ancient Egypt as the font of civilization actually becomes once again a source of racism and prejudice. This is well illustrated in the following four chapters, which in their separate ways highlight how a belief in Ancient Egyptian superiority has often resulted in the downplaying and neglect of the contributions made by sub-Saharan African societies to their own technological, social, economic, and political development. This is perhaps most evident with reference to European colonial and missionary concepts of Hamitic superiority as discussed by Reid (chapter 5) and Bennett (chapter 8), but is also evident in the work of the writings of ardent Afrocentric authors such as Cheikh Anta Diop and Léopold Senghor, as elaborated by Folorunso (chapter 6) and MacDonald (chapter 7) (see also Holl 1990).

Nevertheless, as these authors all acknowledge, scholars are faced with something of a dilemma. Specifically, whereas it is important that local achievements and inventions are recognised for what they are and not simply explained away as the outcome of cultural diffusion or even population migration from technologically or racially superior regions, as was common in the 19th and early 20th centuries, it is equally important that the evidence for actual contacts between Ancient Egypt and its sub-Saharan neighbours is acknowledged and the consequences that such contacts may have had are explored. In his chapter Reid, for instance, traces the influence of theories about the southward migration of a superior race of cattle-keepers of Semitic origin referred to in the literature as Hamites (after Noah’s son Ham), on interpretations of the history and archaeology of the Great Lakes region. As he makes clear, attitudes toward possible connections between Ancient Egypt and the Great Lakes have undergone many changes, and have been shaped by the historical and cultural context in which these theories were produced. A recurrent element of these claims has been the use of superficial similarities between the two regions in terms of different material traits, ranging from the common use of the bow-harp and the wearing by men of cloth-wrappers, to similarities in the shape of canoe prows and the construction of clay walls with arched doorways, as evidence for contact and/or migration. All can be shown to be based on a combination of conjecture, racial prejudice, and poorly constructed formal analogies, and there is virtually no securely dated material evidence (other than the bones of domesticated cats!) to suggest contact between the two regions until the modern era. In fact, the Great Lakes region seems to have been one of the few parts of Africa where long-distance trade and external contact played minimal roles in the emergence of social complexity.

Just as for East Africa, Hamitic hypotheses have had considerable influence on interpretations of West Africa’s past, and here too there is only limited evidence to support the claims of Christian missionaries and Afrocentric authors alike that the rise of complex societies and urbanism in the region were as a consequence of links with Ancient Egypt. As MacDonald notes, current archaeological and related evidence suggest that ceramic technology, cereal domestication and iron-working, all commonly regarded as key traits of early civilization, were all separately invented by communities in sub-Saharan
Africa independently from Ancient Egypt, and what little material evidence there is to suggest contact points to links with Nubia rather than the lower Nile region. Nubia, as is well known, of course did have close links with the Egyptian kingdoms to the north, and the nature of these links and how they have been investigated form the focus of the last four papers in Encounters with Ancient Egypt. All of these papers focus on the Nile Valley, since this was the only corridor through which Ancient Egypt effectively had access to Sudanic Africa, and especially on the rise of the Nubian kingdoms. All also concentrate on the influence that ideas about Egypt, notably those that envisaged Ancient Egypt as the font of knowledge and civilisation, have had on the direction of scholarly research in the region. In so doing, all offer alternative readings of the material evidence that challenge the hegemony of Egyptology and the tyranny of the text. Wengrow in his paper (chapter 9), for instance, notes the considerable similarities between the early Neolithic traditions of the Egyptian Nile Valley and Central Sudan (known respectively as the Badarian and Khartoum Neolithic). On the basis of this comparison, he suggests that they shared a set of common cultural traditions, and that the archaeological record of the Badarian and Khartoum Neolithic represent the remains not of settled, agricultural villagers, but of cattle-keeping nomadic pastoralists. Drawing on the ethnography of 20th century pastoralists of the southern Sudan, such as the Dinka, Shilluk, and Nuer, who all hold notions of divine leadership, Wengrow makes a convincing case to the effect that Ancient Egyptian ideas of divine kingship had their origins in similar kinds of societies rather than in the kind of urban contexts found in Western Asia that are conventionally regarded as having provided the prototype for Egyptian civilization.

The remaining three papers by Edwards (chapter 10), Morkot (chapter 11), and Fuller (chapter 12), concentrate on a different period —namely the interaction between Ancient Egypt and Meroe and the Kushitic/Napatan Kingdom. Until recently, interpretations of the origins and the evolution of social structures and political forms of Meroitic Nubia have been driven by a northern perspective, as is made abundantly clear in Morkot's critical essay on the writings of scholars such as Breasted, Reisner and Gardiner. A similar point is also made by Burstein in his contribution (chapter 4) to Africa & Africans in Antiquity, where he notes that because “of the priority assigned to literary sources in this model, topics were selected for analysis not because of their intrinsic historical significance but because of the chance survival of written evidence” (p. 141), underlining one of the key reasons why a critical interrogation of textual sources in the light of available archaeological evidence is so crucial.

In their chapters, Fuller and Edwards similarly touch on aspects of this historiography, but also offer alternative readings of the material evidence. Edwards, for instance, points to the existence of a long-term culinary frontier between Egypt and Nubia as expressed through different preferences for wine and wheat-based breads in the former, and beer and sorghum-based porridge in the latter. This was also given material expression through differences in ceramic vessel forms and also the use of metals. Edwards also speculates on the possibility that copper rather than gold may have been more highly valued in Nubian society in common with other traditions further south, and in contrast to the conventions of Ancient Egypt. Fuller develops some of Edwards's arguments about the contrast between Pharonic and Sudanic models of state formation, noting that even though Nubian society and elites drew on various Egyptian architectural and iconographic forms and based their script on Egyptian hieroglyphics, the patterning of the archaeological evidence suggests that Kush was organised as a segmentary state rather than on the centralised lines exhibited in Pharonic Egypt, and so may have shared similar traditions of political authority and leadership found in societies further to the south.

Attempts to Verify Classical and Medieval Texts

In common with other parts of the world, much of what might be considered historical archaeology on the African continent has been concerned with the verification of various written historical sources, especially those concerning the pre-modern era. Outside of Egypt and Nubia, where texts written using the hieroglyphic script first emerged in an archaic form perhaps as early as 3400-3200 BCE (Dreyer
and, as discussed above, have shaped the direction and content of the majority of archaeological research in the region, the main areas where text-based archaeology has been attempted are Libya and adjacent parts of North Africa, the East African Swahili coast, and Sudanic West Africa. In the former two areas, the primary relevant texts are those written by Greek and Roman authors (although for more recent centuries, Arabic sources are also of value), while for West Africa, the earliest documentary sources derive from the writings of Arabic scholars such as Ibn Batuta, Ibn Khaldun and Al Bekri. In all three areas scholars have tended to give primacy, until recently, to the available written sources, with the result that certain misrepresentations and distortions within these texts have often acquired the status of historical fact (Smith 2003). A good example is the claim made by an earlier generation of historians, based on their reading of the original sources, that Islam was introduced to the Empire of Ghana as a direct result of its forcible imposition by Almoravid warriors from Morocco and southern Mauritania. Recent reappraisal of both the Arabic and local oral historical sources, however, has shown that the Ghana Empire was never conquered by the Almoravids (Conrad and Fisher 1982, 1983). Tellingly, the available archaeological evidence from Koumbi Saleh in southern Mauritania, which is believed to be the site of a merchant town attached to the royal capital of Ghana, is also inconclusive, and is certainly open to an alternative reading (Insoll 2004:168-169).

The collection edited by Edwin Yamauchi, *Africa and Africans in Antiquity* (2001), provides further examples of some of the challenges archaeologists face when using textual sources as well as offering a valuable introduction to current approaches to text-aided historical archaeology for a region that is more often than not hived off from African archaeology and considered more from the perspective of Classical archaeology and/or Egyptology. Of the ten essays, one is concerned with the evidence of historical linguistics, and in particular, commonalities in different Afroasiatic languages (Hodge, chapter 1); four deal with different aspects of Egypt’s relations with Nubia and the kingdoms of Kush, Meroe, and Ballân (Yurco, Russmann, Burstein, and Adams, chapters 2-5); three with other parts of north and north-eastern Africa, specifically Carthage and the Maghreb (Bullard, chapter 6), Cyrenaica and Marmarica (White, chapter 7), and Ethiopia (Bard and Fattovich, chapter 9). The remaining two essays (Snowden, chapter 8, and Swanson, chapter 10) deal with how different aspects of the perception and representation of Africa and Africans in antiquity and in the modern era have shaped understandings of the history and archaeology of the continent. The majority of the contributors are either Egyptologists, Classical archaeologists, or specialists in Oriental (or Middle Eastern) studies, the main exceptions being Hodge, a specialist in African linguistics, Adams, who is a leading specialist on the archaeology of northern Sudan and the Middle Nile, and Bard and Fattovich, whose area of specialisation is on the emergence of the Aksumite kingdom in what is now Ethiopia and Eritrea and its Pre-Aksumite origins. It is interesting, therefore, that a common theme to these papers is the extent to which, firstly, the Classical and Egyptian textual sources demonstrate not just the presence of black Africans in areas north of the Sahara during antiquity, but also the extent to which sub-Saharan communities and polities contributed to the growth and operation of complex, state-level societies along the Mediterranean littoral.

In common with the contributors to *Encounters with Ancient Egypt*, a recurrent theme of the papers in *Africa and Africans in Antiquity* is the extent to which the dominance of text-aided archaeology has encouraged scholars to focus on particular topics and issues to the neglect of others. This is most apparent in the papers by White on Cyrenaica and Marmarica, and by Bullard on Carthage and the Maghreb. For the former area, the available textual sources include various New Kingdom accounts concerning clashes with the nomadic chiefdoms that occupied the areas inland from the coastal littoral, who are also mentioned by Herodotus who referred to them generically as ‘Libyans’. Textual sources also provided valuable information about the circumstances behind Greek colonization of this area toward the end of the 7th century BCE, and archaeological research at several of these sites has advanced our knowledge of these colonial settlements considerably. Much less research has been conducted aimed at inves-
tigating the impact of Greek colonialism beyond the confines of the urban centres, or on the archaeology of the nomadic communities further into the interior. Much the same could also be said about the Maghreb, where the Phoenician colony of Carthage was established in the 9th century BCE. Textual sources point to the existence of tribute relations with the neighbouring Berber leaders until they were revoked by the Phoenicians in 450 BCE, and successive archaeological campaigns at Carthage have provided vivid insights into life in the city over the centuries. Far less is known from either written or material sources about Berber society and none of it features in Bullard's account (cf. Blench 2001). This is precisely the kind of context, and contra Connah, where the application of the concepts of historical archaeology, whether these are simply methodological ones concerning the interrogation and integration of textual and material evidence alongside one another, or the sub-field's more conceptual and theoretical concerns with the effects of colonialism and indigenous responses, and the impacts of world systems, would seem to have the potential to make a profound difference on the direction and content of archaeological research in a particular geographical region.

Recent ongoing research on the Garamantes, who lived in the area of the Fezzan in the Central Sahara of Libya, provides a case in point. The Garamantes were first mentioned briefly by Herodotus in Books II and IV of his Histories probably written around 430 BCE, in which they appear as a somewhat warlike group of people who used four-horse chariots to attack neighbouring cave-dwellers, although he also mentions that they had farms and long-horned cattle. They also figure on the first world map in history made by Ptolemy (90-168 CE), and are referred to by other Roman historians including Pliny and Tacitus (McCall 1999). However, questions as to the identity of the Garamantes, whether and how they managed to survive in the desert, and the nature of their relationship to the Roman empire and the earlier Cathiginian polity have only recently been addressed by archaeologists. From the various results of combined archaeological and palaeoenvironmental research, it is now evident that the Garamantes emerged around ca. 750 BCE from earlier pastoralists communities (e.g., Liverani 2000a, 2000b). Interestingly, the consolidation of settlement, horticultural intensification, and the growth of social complexity and political hierarchies coincided with a progressive decline in rainfall following the onset of increased aridity across the Sahara around ca. 1050 BCE (Brooks 2006:34-35). Archaeological research has further demonstrated that occupation of the desert margins was made possible by the construction a sophisticated underground system of water conduits and galleries, or foggaras, so as to draw ground water from the southern edge of Wadi al-Ajal towards the centre where their capital, Garama, was situated (Mattingly 2003). The development of trade links with Carthage and the later Roman colonies also helped sustain these communities, with salt possibly being one of the most important commodities. Gold obtained via trans-Saharan trade with the emergent West African kingdoms may have been equally important, although the material evidence for this remains limited.

Oral History as Metaphor

The growth of scholarly study of the oral history and traditions of African societies coincided with a rise in interest in the later Holocene archaeology of the continent, and especially that concerning the origins and spread of food production, metal-working, urbanism, and complex political systems (see Robertshaw 1990). As part of this intellectual trend, the use of such traditions became a common component of most archaeological surveys, principally as a means of providing an overview of the more recent history of the area under investigation. Moreover, a particular characteristic of a great many oral traditions concerning different African societies is the emphasis placed on migration as the primary driving force of social change, and the first generation of Africa's historians to be trained in the Western academic tradition were often keen to see their archaeological counterparts provide material verification that the various routes and stopping places described in the oral accounts were indeed associated with the settlement histories of specific ethnic groups. It is rare however, to find unequivocal confirmation of this kind in the archaeological record, as many archaeologists were quick to point out (e.g., Siiriäinen 1973; Andah and Okpoko 1979), and over the subse-
quent decades there has been a steady distancing between the two disciplines. This has even prompted a debate as to whether they now share common concerns (Vansina 1995; Robertshaw 2000).

One area which has featured prominently in some of these debates, and which has also fostered alternative approaches to the uses of both oral and archaeological sources is the Great Lakes region of East Africa, and particularly with reference to places associated in the oral traditions with an elite known as the Bacwezi. According to at least one set of traditions, the Bacwezi had been the historical rulers of a large region centred in the lush grasslands of western Uganda, which, by calculating from genealogical data, would appear to have existed sometime during the 14th and 15th centuries CE (Oliver 1953; Nyakatura 1973). A number of extensive complexes of ditched earthworks, including the sites of Bigo, Munsa, Kibengo, and Kasonko, are known from this area (Wayland 1934; Lanning 1953, 1955). During the 1950s-1960s, various archaeological campaigns were undertaken at some of these in an attempt to provide a clearer understanding of their date and function, and their link with the Bacwezi dynasty (Shinnie 1960; Lanning 1966; Posnansky 1966). The discovery at Bigo of an enclosure similar in form to that found at some of the later royal capitals in Uganda, and a suite of radiocarbon dates from the site suggesting occupation between the 13th-16th centuries CE, led to the conclusion that Bigo was indeed the capital of the pastoral Bacwezi kingdom, and that the other sites were part of the same political system (Posnansky 1969). Reappraisals of these investigations, however, have called into question many of the historical interpretations that were used to guide the archaeological excavations (Berger 1980), and highlighted some of the more general problems associated with trying to substantiate oral traditions archaeologically (Schmidt 1990). Moreover, the results of more recent field investigations by Robertshaw at Munsa and Kibengo indicate that despite some superficial similarities these not only differ from one another but also from Bigo, in terms of their site inventories and material culture traditions. Thus, rather than belonging to a single state, Robertshaw suggests that each of these sites represents the centre of an independent polity that was in competition with its neighbours over resources and control of the local populace (1999).

Over the years, Peter Schmidt has written extensively about these and associated sets of oral traditions and the insights they provide into changing structures of social and political power (e.g., 1978, 1983a), as well as on the related topics of the symbolism of iron and iron production (e.g., 1997b), the origins, technology, ideology, and environmental consequences of iron production (e.g., 1978, 1997a; Schmidt and Childs 1985; Schmidt and Mapunda 1997), the linkages between oral traditions and archaeology (e.g., 1983b, 1990), the use of historic and archaeological sites as mnemonics in the process of history production, the socio-politics of the past and archaeological praxis in Africa (1995) and the historiography of Great Lakes scholarship (e.g., 1990, 1995). His new book, *Historical Archaeology in Africa: Representation, social memory and oral traditions* (2006), brings together a selection of these and other related material with the stated goals of highlighting the significance of linking oral traditions and archaeology to provide a better understanding of “social memory and the role of deep-time mnemonics”; to illustrate “how historical archaeology as defined and practiced in North America has the capacity to escape the bounds of its ethnocentrism”; to map out the kind of “questions that count” (Deagan 1988) with reference to African historical archaeology; and to “show why history and ‘prehistory’ fit together in history making when applied to issues of historical interpretation” (pp. 3-7). Rather than simply reproduce a selection of previously published papers and book chapters verbatim, however, Schmidt offers a series of interlinked chapters, in some cases newly written and in others using extensive extracts from previously published work, but also with accompanying new introductions, and in some cases, new conclusions and even elements of an auto-critique.

The book consists of eleven chapters and is divided into two sections, the first of which addresses issues of representation, social memory, and oral traditions, while the second is concerned with historical archaeology and representation, and the book as a whole is written in Schmidt’s characteristic polemical style. Entitled, ‘Questions that Count: Africa and
Beyond’, chapter 1 sets out Schmidt’s objectives and his vision for historical archaeology, emphasising in particular how new theoretical questions in historical archaeology might be posed through the study of the use of tropes in historical representations, and the need to consider the political implications of the production of historical and archaeological knowledge. In chapter 2, Schmidt offers an historiographical perspective on the use of oral traditions in African archaeology, with particular reference to the varied attempts to understand the Bacwezi corpus (as summarised above) so as to set the scene for his revisionist approach. Chapter 3, co-authored with Jonathan Walz, is in essence a review of definitions and constructs of historical archaeology as applied on the Africa continent which address many of the same themes as this review—notably the problems of text-driven approaches, the weaknesses of verification approaches to the use of oral histories, and the privileging of ‘external’ (to use Pikirayi’s terminology) over ‘internal’ sources and representations (see Schmidt and Walz 2007, for a similar version of this chapter). It is here that Schmidt, with his co-author, sets out his vision of historical archaeology on the African continent and in particular how it might be possible “to make historical archaeologies of groups that fall outside the margins of European ethnocentrism, colonialism, capitalism and what Orser (1996) calls ‘modernity’” (p. 45). For Schmidt, oral sources represent the most authentic voice of indigenous communities, and it is only through understanding how oral traditions are created and express knowledge about historical events and processes that a truly indigenous African historical archaeology will result.

Consequently, in chapters 4-6, Schmidt returns to his early structural analyses of Bacwezi mythology and the associated archaeological and ethnoarchaeological evidence concerning iron technology and the symbolism of iron and iron production in the Great Lakes region and elsewhere in Africa, so as to illustrate how societies employ the material traces of past generations as material mnemonics in their constructions of history (chapter 4), the interpretive potential and dimensions of a tropic analysis of oral traditions (chapter 5), and how places and landscape acquire the symbolic meaning through productive practices such as iron smelting (chapter 6). Since he first began research on the corpus, Schmidt has argued consistently that the Bacwezi ‘myth’ must be seen as a symbolically loaded, metaphorical account of trends in the region’s history and changing power relations, rather than as a literal description of actual historical events and relationships. This point is reiterated several times in Historical Archaeology in Africa. Early on, for instance, Schmidt observes that, “strict congruence between the oral tradition claim and archaeological evidence may be missing the point of the oral tradition’s historical importance” (p. 27). Instead, he suggests that the real historical value of oral traditions emerges only once it is recognised that they contain tropic expressions, “linked to ritual and performative life” (ibid.). However, because “tropes are symbolic representations, their use easily masks disjunction and contradictions between differing or contested histories”, and consequently it is through the task of tropic analysis that the processes of this appropriation and the power relations that enabled it are revealed (p. 100).

Thus, for instance, Schmidt found during his research in the Kagera region of northwest Tanzania that oral traditions, as in Uganda, linked the more recent, immigrant Bahinda ruling dynasty with the Bacwezi, and that the power and authority of the Bahinda clans were generally associated with control over iron-working, rain-making, and fertility rites. However, genealogical reckoning placed the period of Bacwezi rule up to 20-25 generations ago and thus significantly earlier than had been estimated from the Ugandan traditions. Moreover, excavations at Katuruka, the former capital of Rugomora Mahe, one of the Bahinda rulers of the Kiamutwara kingdom during the 17th century CE, led to the discovery of iron smelting remains associated with the very beginnings of settled farming in the region and dating to around 500 BCE. Although similar remains of early farming and iron smelting communities were found at many of the other ritually important places within the historical topology of the Bahinda landscape, in other respects there was a lack of direct settlement continuity as evidenced by typological differences between the Early Iron Age ceramics and those associated with the 2nd millennium CE kingdoms. On the basis of this, Schmidt concluded that whereas the Bacwezi
may have been indigenous rulers of small-scale polities during the 1st millennium CE or possibly earlier, later leaders, unconnected with Bacwezi, subsequently manipulated traditions and appropriated the archaeological remains of the Bacwezi’s evident iron smelting abilities to legitimise their own assumption to power.

Whereas the first section of *Historical Archaeology in Africa* is concerned primarily with accessing and reconstructing indigenous understandings of the social and political world and of ‘the past’, the second section is more concerned with external representations (or, in Schmidt’s view, misrepresentations) of African societies and their pasts, and the socio-politics of archaeology on the continent. Once again, Schmidt draws heavily on his archaeological, ethnoarchaeological, and historical research in and on Buhaya, northwestern Tanzania, particularly with reference to Euro-American misconceptions regarding the origins of iron smelting in Africa and the technological skills of rural African communities (chapter 7), and on his broader experience of teaching archaeology and conducting research in eastern Africa (chapter 8). Chapters 9 and 10 (co-authored with Kharyssa Rhodes) reproduce elements of previously published historiographies of the investigation of Bigo and Cwezi oral traditions. Chapter 10 also critiques more recent archaeological assessments of Bigo and the associated site of Mubende Hill, as forwarded by Robertshaw (1999). Specifically, Schmidt and Rhodes argue that as the site is an important shrine associated with the Cwezi, its history and the symbolic capital this confers may well have been appropriated by a new elite in much the same manner as occurred with the Rugomora Mahe under the incoming Hinda royal clan. In support of this argument, they note the presence of Early Iron Age ceramics at Mubende Hill and suggest that these have the same significance as the extensive traces of Early Iron Age (EIA) smelting activity that were incorporated into the ritual practices by the Bahinda elite clans at Rugomora Mahe and other similar sites. Although Robertshaw notes the presence of EIA ceramics at Mubende Hill, in his view these were limited in number and such evidence as there was, did not have a direct bearing on the subsequent emergence of political complexity (Robertshaw and Taylor 2000). Schmidt and Rhodes take the opposite stance, arguing that there is far more evidence for EIA activity at the site, and it is precisely because of the presence of this evidence and memory of this history that Mubende Hill had sacred significance for the later emergent elites and was selected by them as a political centre. Given the radical nature of these claims, it is unfortunate that no illustrations of the relevant ceramics or anything more than cursory descriptions of the characteristics of the diagnostic material are provided, and reference is made simply to an as yet unpublished report on the results of two separate analyses conducted by Schmidt and later Rhodes on the museum collection.

The broader purpose of this critique, irrespective of the actual strength of the evidence put forward, is to suggest that archaeologists working on African materials and sites need to re-appraise the legacy of what is often termed “the colonial archive” and create space within their interpretive narratives for indigenous voices and perspectives through the interrogation of oral traditions and the archaeological record. A rather similar point is made in the final chapter, in which Schmidt critiques the continuing dominance of notions of the Arabian (and hence foreign) origin of, and inspiration for, the emergence of social complexity in Eritrea and Ethiopia and especially the growth of the Pre-Aksumite kingdom of Damaat (also rendered DMT). In the absence of oral traditions, in this case study Schmidt relies more heavily on the results of recent archaeological surveys and excavations conducted around Asmara in collaboration with Matthew Curtis, to make the point that colonial paradigms continue to dominate many archaeological reconstructions of Africa’s because of the power of the text (in this case previous archaeological interpretations) in diverting attention away from the alternative readings of history offered by the material evidence in the ground. What matters, he suggests, “is not the antiquity of the material remains”, but rather, “how the reach and embeddedness of historical misrepresentations are displaced and diminished by contradictory material evidence” (p. 260).

**Indigenous Archaeologies**

Schmidt’s insistence on the need to indigenise African archaeology is a sentiment with which most other archaeologists who work on the con-
tinent would agree. One clear sign indigenisation is proceeding is the number of African scholars engaged in the production of archaeological knowledge. The collection of essays edited by Chapurukha Kusimba and Sibel Kusimba—East African Archaeology: Foragers, Potters, Smiths and Traders (2003), provides preeminent testimony of this trend as it is one of only a handful of international publications where the majority of contributors are African (specifically, two-thirds of all contributors). Here, perhaps more than anywhere else, we might expect to find truly indigenous voices and takes on the continent’s archaeological past. None of the contributions are explicitly concerned with “historical archaeology”, and the term is not even used. Similarly, none of the “usual suspects” of historical archaeology such as colonial encounters and modernity, form the core focus of analysis in any of the papers, a point which Mitchell, in his overview of the contributions from a southern Africa perspective (chapter 11), draws attention to (p. 179). Nevertheless, several papers address the use of archaeological data in conjunction with written and/or oral sources, and also the historiography of these approaches and the effects they have had on the direction of research in different parts of the region. Specifically, three consider aspects of the history and archaeology of the Swahili coast (Chami, chapter 6; Kusimba and Killick, chapter 7; and Kessy, chapter 8) and three others (Kusimba and Kusimba, chapter 1; Mapunda, chapter 5; and Robertshaw, chapter 10) draw on oral traditions and/or historical ethnographies in their discussions of the historical significance of particular archaeological sites and assemblages, and it is these I focus on here.

A theme shared by all of the contributions that concern the archaeology of the Swahili coast is the extent to which the available Classical and Arab documentary have tended to mask the significance of certain historical processes and have directed research energies toward certain research questions and away from other topics and themes (a point which is also made by Robertshaw in his review [chapter 10] of changing archaeological approaches to the study of state formation in the region). Thus, in his paper on early iron-working communities on the East African coast, with particular reference to the site of Kivinja in the Rufiji Delta, Tanzania, Felix Chami is highly critical of an earlier generation of scholars, who, because they gave primacy to the historical sources, tended to regard the origins of Swahili urbanism and state-level political systems as foreign-inspired. Recent research along the East African coast and on the offshore islands by Chami and others, as well as research at the Swahili stone-town sites (see Kusimba 1999; LaViolette and Fleisher 2005:339-343 for a review), has clearly demonstrated the presence of active and thriving agricultural communities along the Western Indian Ocean littoral well before the establishment of maritime trading links with the Persian Gulf and Indian Ocean world from the 8th/9th century ce. Moreover, in Chami’s view, finds of various imported goods, such as glass, ceramics, and even gold items, from the Classical Mediterranean world associated in stratified contexts with early farming (EIA) ceramics at a variety of sites, not only “corroborate the Periplus of the Erythraean Sea and other Greco-Roman documents by Ptolemy” but also indicates that these communities were active trading partners well before the arrival of Arab merchants on the coast (p. 96).

Emanuel Kessy is also critical of the role that historical texts have had in shaping the direction of research on the East African coast. However, his concerns are rather different from those expressed by Chami. In particular, he illustrates the extent to which the presence of textual sources has encouraged a possible over-emphasis on the Swahili era (ca. 9th–15th century ce) sites by archaeologists to the neglect of the earlier and later periods. Drawing on survey and excavation data from the islands of Pemba and Zanzibar on site distributions and contents, he offers a convincing narrative to suggest that there were several changes in site location strategies on these islands over the course of the last two thousand years or so, which correlate in part with changes in the degree of reliance of marine resources over time, but were also influenced by shifting trading patterns, the arrival of the Portuguese, and Omani colonialism. Whereas Chami’s use of the historical sources is essentially one of verification, Kessy intertwines the historical and archaeological sources to provide a seamless narrative of the kind called for by Connah.

In their paper, Chapurukha Kusimba and
David Killick, like Kessy, are also critical of the role that text-driven archaeology has had on the pattern of research on the Swahili coast, although like Chami, they are also concerned to establish whether there is archaeological support for some of the statements in these texts. Their specific concerns are with understanding the technology of iron production along the Swahili coast, and its development over time. They note, for instance, that the *Periplus* states that iron tools were among some of the items imported to East Africa in the 2nd century CE, but by the 10th century, various Arabic sources indicate that iron was one of the major exports to India. Drawing on the results of archaeometallurgical of a range of iron objects from five Swahili sites on the Kenya coast, the authors are able to demonstrate for the first time the technological capabilities of the Swahili smiths, and even more critically, that iron-working and the trade in iron products played an important role in the emergence of Swahili city states, contrary to earlier preconceptions. Their analyses also detected the presence of nails made of crucible steel. At present, all of the evidence indicates that Swahili blacksmiths produced iron and steel products using the bloomery process and there is nothing to indicate that they were familiar with the techniques for making crucible steel. The latter techniques, however, were practiced on the Indian sub-continent from at least the 6th century CE and the products so produced were of high quality and known to be expensive.

As Kusimba and Killick note, the presence on Swahili-era sites of mundane items such as nails produced using from foreign crucible steel thus seems rather anomalous as one might expect that any imported high-quality and expensive items would, instead, have been luxury artefacts. In fact, the presence of these nails is even more curious given that the historical sources claim that the Indian merchants who visited the Swahili coast considered the iron and steel products made in East Africa were of a better quality than those produced in India. Kusimba and Killick partly explain this by suggesting that while the East African bloom was of better quality it may have also been cheaper than the crucible steel produced in India, and so consumers there would have preferred it at least for their utilitarian products (pp. 113-115). This still does not explain the presence of the nails made of crucible steel, however, and it is clear that this and related issues need to be the subject of further research on both sides of the Indian Ocean.

The technologies, symbolism, and development over time of iron smelting in eastern Africa is also the subject of Bertram Mapunda’s paper (chapter 5) with reference to the Ufipa area of Tanzania toward the southeastern end of Lake Tanganyika. This is a region well known for its iron, and the Fipa are known from 19th and early 20th century records to have been highly regarded as iron producers and actively continued their craft until as recently as the 1950s. Historical memory of the various practices have also allowed various scholars to organize reconstructions of the smelting process and have made it possible for them to document in some detail the symbolic associations and social significance of the technology. However, as Mapunda notes, these previous ethnographic and ethnoarchaeological studies have all tended to focus on only one specific type of iron-working technology as practised in Ufipa, which he terms *Malungu* technology that relies on the use of tall, natural draught furnaces. This is perhaps understandable, since the remains of these furnaces survive as prominent features in the landscape and the most obvious tangible evidence of iron smelting practices. Research on the archaeology and oral traditions of the area, however, has demonstrated that at least two other technologies were employed in Ufipa in the past, that these technological styles had different geographical distributions and may also have had different origins. Mapunda terms these the *Katukutu* and *Barongo*-type technologies. The former appears to have flourished during the 16th–18th centuries CE along the shores of Lake Tanganyika and the Fipa escarpment and involved the use of comparatively short, globular natural draught furnaces. In contrast, the *Barongo*-type (named after its similarity to the technology employed by Barongo smelters in the Mwanza region in northern Tanzania at the southern end of Lake Victoria) involved the use of taller, forced-draught furnaces. It was also restricted to the shores of Lake Tanganyika and employed mainly during the 19th century. The Malungu tradition also developed during the 19th century, continuing into the 20th century, but involved the use of much taller furnaces without the use of bellows and was restricted to
the Fipa escarpment away from Lake Tanganyika. There were other differences between these three technologies (pp. 71-78).

There is a long history of iron production around Lake Tanganyika, initiated during the Early Iron Age/Early Farming era around 400 CE, as documented several decades ago by Desmond Clark at Kalambo Falls and at a range of other sites located more recently by Mapunda. The Kalambo Tradition continued to around 1000 CE, when a new pottery type emerged, similar to or possibly part of the Triangular Incised Ware (TIW) ceramics found along the East African coast associated with the precursors of Swahili stone towns (see Chami, chapter 6). Thereafter, the evidence points to a phase of more rapid stylistic change possibly associated with an intensification of population movement, some of which may have been behind the introduction of different iron production technologies. For instance the “abrupt appearance of full-fledged iron technology and a new pottery type” in the 16th century in Nkansí District probably indicates “that a large population influx took place” and that these migrants were responsible for the Katukutu technological style (p. 81). Local oral traditions would link these to the present inhabitants of the region, namely the Lungu (to the south of Kala) and the Fipa (to the north of Kala). Archaeological evidence collected by Mapunda in the current Fipa region is consistent with the information provided in the oral histories and royal genealogies that Fipa arrived in the northern area (from the southern area now occupied by the Lungu) around the mid-17th century. It was in this area that the Malungu technology developed, and judging from the archaeometallurgical evidence, evolved out of the earlier Katukutu. However, the origins of the Katukutu technological style is rather more ambiguous, since some Fipa oral traditions state that the area they now occupy was empty prior to their arrival, while others state that the land was occupied by Mbonelakuti (Batwaa) pygmies who were hunter-gatherers but also proficient in iron smelting. At present, it is not possible to determine which of these accounts more accurately reflects the evidence on the ground. However, as Mapunda notes, if the latter is the case it could call into question many of the conventional models regarding the southward expansion of early farming and iron working communities and their relationships with neighbouring, autochthonous hunter-gatherer groups as part of a moving frontier (cf. Lane 2004b). On this point, it is worth noting that there is perhaps some support for the argument that Batwaa groups had knowledge of iron smelting from recent work in the Ituri Forest, DRC (see Mercader et al. 2000).

Conventional ideas about hunter-gatherers and the dangers of projecting ethnographic data and models of hunter-gatherer behaviour into the distant past are also raised in the papers by Sibel Kusimba and Chapurukha Kusimba (chapter 1) and Audax Mabulla (chapter 3). In their paper, Kusimba and Kusimba compare and contrast the archaeological evidence relating to hunter-gatherer mobility practices and subsistence strategies from two quite separate time periods in two different lowland ecotones in southern Kenya. Specifically, the lithic and faunal evidence from Later Stone Age sites around Lukenya Hill indicate that during the Pleistocene, hunter-gatherer were highly mobile, concentrated on the exploitation of large migratory grazers, and had ready access to exotic as well as local sources of lithic raw material either through exchange or as part of a seasonal round. In contrast, the evidence from ‘historic’ (ca. 100-300 BP) hunter-gatherer sites in Tsavo point to a concentration on small, non-migratory mammals, as well as birds and reptiles, and a highly expedient lithic technology that was dominated by the use of locally available vein quartz. While there is plentiful evidence that these groups were linked by trade with neighbouring pastoralist and farming communities (and also indirectly with the Indian Ocean trade networks), they appear to have been far less mobile than was the case at Lukenya during the Pleistocene.

For Mabulla (chapter 3), the “study of contemporary forager land use provides a fruitful approach to understanding prehistoric landscape use and archaeological spatial patterning”, as well as “a better understanding of prehistoric forager adaptive strategies” (p. 33). While there has been considerable ethnoarchaeological research on the Hadzabe, as Mabulla notes, the vast majority of this work has been directed toward understanding the spatial structure of base camps, the formation process that operate
in these, and the diagnostic characteristics of different butchery practices and patterns of food sharing. Much less ethnoarchaeological work has been conducted on documenting Hadzabe patterns of mobility and landscape use. In this regard, Mabulla’s paper provides a useful synopsis of these landscape practices and their possible archaeological signatures, and on the basis of this material he argues that despite increasing contact between Hadzabe and neighbouring groups from the later 19th century onwards, and evidence for tangible changes in their material culture repertoire and economic practices, modern Hadzabe strategies provide reliable analogues for much earlier ‘prehistoric’ Later Stone Age and even Middle Stone Age behaviour (pp. 50-53). This stands in marked contrast to the observation made by Kusimba and Kusimba in their chapter that individual cases of modern hunter-gatherers are often poor analogues for ‘prehistoric’ behaviour (p. 15), and failure to consider the historical trajectories of modern communities undermines the value of much ethnoarchaeological research. On this basis, there is very evidently a need for more not less ‘historical archaeology’ in the region—a view with which this reviewer would concur.

Conclusion
This review began with a discussion of a series of issues concerning the use of the term ‘historical archaeology’ with particular reference to Africa. Alongside matters of definition, one important question asked was whether the term had any validity at all for African archaeology, or whether it would be better to abandon its use altogether, as recently recommended by Connanah (2006, 2007). Having examined various studies of African contexts and materials which can loosely be described as examples of ‘historical archaeology’, although not always so defined, it is worth reflecting again on these issues. Several points can be made.

First, outside Southern Africa (and even here, mostly only in South Africa) and parts of Anglophone, Atlantic West Africa, the term ‘historical archaeology’ is only rarely used. Moreover, much of what is termed ‘historical archaeology’ in South/Southern Africa is concerned with the various dimensions of the encounter between indigenous communities and European settlers and colonists. One consequence of this is that ‘historical archaeology’ here is often subsumed under, or equated with, the archaeology of [European] colonialism. In contrast, the focus of ‘historical archaeology’ in West Africa has been primarily concerned with the growth, expansion, and consequences of the Atlantic Slave Trade. Hence, in contrast to South Africa, ‘historical archaeology’ here has often come to stand for the archaeology of the [Atlantic] slave trade. Exceptions to these propositions can of course be found, nevertheless, I suggest that the archaeology of European colonialism and the archaeology of the Atlantic slave trade, in each case broadly defined, are respectively the dominant paradigms of historical archaeology in these two areas.

A second set of observations arise from the foregoing remarks. First, the emphasis on European colonial encounters in South Africa and on the Atlantic slave trade in West Africa (and on neither of these in other parts of the continent), is, in large part, due to the different historical trajectories of the different cultural and geographical regions of Africa. Specifically, European colonial settlement did begin and did become entrenched significantly sooner in South Africa than elsewhere on the continent; West Africa was the focus of trans-Atlantic slaving activities. As the use of square brackets around the terms ‘European’ and ‘Atlantic’ above further implies, also embedded within these constructs is the assumption that ‘historical archaeology’ is somehow associated with, if only implicitly, European activities, sources, or, at the very least, a European presence. It should come as no surprise, therefore, that while such an assumption prevails, then the term will have only limited resonance for the regional archaeological traditions of those parts of the continent where a European presence is of relatively recent origin (say the last 150 years), and/or did not result in a restructuring of settlement, society, demography and productive activities on the scale experienced in southern Africa and those West African countries impacted by the Atlantic slave trade. A clear case in point is East Africa where, despite a very early use of the term ‘historical archaeology’ to describe the archaeology of the coastal zone from the later 1st millennium AD onwards (Kirkman 1957), the appellation never stuck and instead the terms ‘Swahili archaeology’ or ‘coastal archaeology’ came to be
favoured. It follows, then, that even where used on the continent, ‘historical archaeology’ is partial and implies rather different things even when modelled most closely on the dominant North American conceptions of the subfield.

Third, a diverse range of other written, oral, cartographic, pictorial, and linguistic sources also exist, only some of which concern Europeans, or periods after 1500 CE, or both. As illustrated above, archaeologists working on African materials have long made use of these categories of historical evidence in combination with material remains. An equally diverse range of approaches, methodologies, and theoretical underpinnings characterise such studies as those which are more explicitly defined as ‘historical archaeology’. The use of such supplementary information is often simply a matter of course as part of ‘normal’ archaeological research, so it is questionable whether the majority of instances can really be said to constitute ‘historical archaeology’. The well established tradition of using (or at least referring to) historical linguistic data associated with Bantu languages in studies of the establishment and spread of early farming and metal-working communities (conventionally referred to as Early Iron Age societies) across vast expanses of eastern, central, and southern Africa, is a case in point and terming this ‘historical archaeology’ simply because some non-material sources are used to support an archaeological argument would seem pointless. This said, as Schmidt’s research using oral traditions has demonstrated, and also Schoenbrun’s detailed analyses using historical linguistics, a ‘historical archaeology’ of early Bantu-speaking farmers is possible. By the same token, I have consistently argued that the ethnographic data used to construct analogies for the reconstruction of ideologies and worldviews among early Bantu-speaking farmers is possible. By the same token, I have consistently argued that the ethnographic data used to construct analogies for the reconstruction of ideologies and worldviews among early Bantu-speaking farmers is possible.

A fourth and final set of observations follow from this and which, once again, are illustrated by several of the case studies discussed above. Specifically, as part of the process of critically assessing particular historical discourses, archaeologists need to examine the history of those discourses and the disciplines that have generated them so as to produce, in a manner as discussed more generally for academic disciplines by Foucault (1972), the archaeology of archaeological knowledge. As part of this process, archaeologists must also examine how, in Stahl’s terms, the legacy of different unexamined epistemologies “actively create and maintain a series of silences about Africa’s past … that are perpetuated by contemporary academic practice” (2002:1). Trouillot’s argument about how such ‘silences’ are created and reproduced by selective ‘mentions’ at each of the main portals of historical production, i.e., “fact creation”, “fact assembly”, “fact retrieval”, and the “moment of retrospective significance” (1995:26), is critical here, especially in light of the debate over definitions of ‘historical archaeology’. Put another way, the questions of importance are not what ‘historical archaeology’ is or might be, or even how it differs from ‘prehistoric’ since this latter distinction is an ontological one rather than simply a matter of chronology or sources (see Lucas 2005: 121-132), but instead concern how history works in different contexts and under different historical conditions. This requires more than consideration of how historical knowledge is produced or represents, although these are important starting points. Neither is it simply a matter of also examining and the power relations that structure the production of historical knowledge and historical representations, although these also are equally important. Instead, as Schmidt and Walz have recently urged, a start must be made on “asking questions derived from African knowledge” while simultaneously accepting and valuing “the integrity of African historicities” (2007:66-67). Many of the texts reviewed here offer ways for moving toward these goals. I would hazard, however, that Africa’s archaeologists still have much further to travel, and that making this intellectual journey also requires more overt recognition that ‘sources’, ‘archives’, and ‘narratives’ cannot be treated merely as sites of knowledge retrieval but are also, simultaneously, sites
of knowledge production, and that, equally, the processes of retrieval and production stand in dialectical and recursive relationship with one another. It follows from this, I suggest, that since 'history' as both event and narrative is common to all times and places, then we must also acknowledge that 'our' archaeological sites emerged from an accumulation of acts of 'history making' each of which sought to define a particular vision not just of the present, but of also the past and the future.

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Archaeological Theory

The Quest for Modern Human Behavior: Breaking a Stalemate

By John R.F. Bower

A few years ago, I published an article in this periodical wherein I considered some of the literature revealing diametrically opposed views concerning the emergence of “modern” behavior as it relates to the study of modern human origins, aka MHO (Bower 2004). I believe that anyone who has kept an eye on MHO research over the past four years would agree that there has been little, if any, progress toward resolving the contradictions described in my earlier article. So, in the spirit of “rushing in where angels fear to tread,” I have chosen to review a body of literature whose core concepts and insights seemed capable of helping to break the stalemate. But, before leading the reader on, I hasten to note that the outcome is frankly more speculative than substantive, more tentative than conclusive. This is essentially an exploratory piece, ergo, caveat emptor!

By way of setting the stage for what follows, let me briefly reprise the issues addressed in my earlier article (Bower 2004). From one point of view, the emergence of “modern” human behavior can be traced to at least as far back in time as the origin of modern humans (Homo sapiens) around 200 ka, if not substantially earlier (cf. McBrearty and Brooks 2000). We can call this the “early emergence” model. From the opposing perspective, the “late emergence” model, “modern” behavior is not archaeologically manifested until about 40 ka, broadly coinciding with the replacement of Middle Stone Age/Middle Palaeolithic cultures by those grouped in the Later Stone Age/Upper Palaeolithic (Klein 2002). The “early emergence” model admits the possibility that the origin of “modern” behavior is linked to the origin of H. sapiens, while the “late emergence” model uncouples the origin of “modern” behavior from the origin of the modern human species. The two models also differ radically with respect to the packaging of evidence for “modernity,” such that in the “early emergence” model various indications of “modern” behavior are scattered over a time span from 200 ka to 40 ka, while the evidence for “modernity” in the “late emergence” model is tightly packaged, appearing more or less simultaneously around 40 ka.

In part, the differences between the two models stem from differing lines of evidence, as well as differing criteria for “modernity.” For example, the “late emergence” model draws heavily on evidence of human predatory competence, while the “early emergence” model inclines more toward indications of technological and symbolic capabilities. Such differences point to what I regard as a major problem in searching for the dawn of “modernity,” namely, the amorphous, basically undefined character of the goal (Bower, in press).

In the absence of a clear research objective, the quest for “modernity” has given rise to something resembling a dialogue of the deaf. Simply put, one cannot compare, let alone evaluate, scenarios for the emergence of “modern” behavior without first defining what constitutes “modernity.”

There is, to be sure, a variety of notions on the subject, but most are problematic and none can be said to have become established as a widely accepted definition of “modernity.” However, most reflect a shared sense, sometimes explicitly stated, that “modernity” is basically underwritten by advanced mental competence. And some, a smaller number, point toward culture in the anthropological sense as a defining expression of behavioral “modernity.” But, overall the quest for “modern” behavior seems to rest on a pervasive sense that its definition is intuitively obvious, thereby exempt from formal consideration.

In a mildly bizarre way, this “we’ll know it when we see it” frame of mind has been retrofitted on MHO research, to wit: “The archaeological record does not always reflect people’s behavioral capacities. However, this is the only source of real information about modernity, for we contend that ‘human populations are modern when they behave in modern ways, no matter what they look like’ ” (Wolpoff and Caspari 1997:445, italics added). What a tangled web is here confectioned, though surely not deceitfully intended!

Returning to the issue broached at the outset,
I suggest that the establishment of a workable definition of modernity will not only help resolve the stalemate in question, but also point the way toward related, though as yet unexplored, areas of inquiry that offer possibilities for greatly expanding and deepening the contribution of MHO research to our understanding of the human condition.

To my sense, a workable definition in the context at hand is one that meets two basic criteria: (1) it reflects a strong anthropological consensus regarding an essential aspect of the behavior of living humans and (2) it is archaeologically accessible, being expressed in the form of durable material remains. With these criteria in mind, it may be possible to construct a definition of behavioral “modernity” that is substantial enough to occupy a more or less determinate spatiotemporal position. And the discovery of its location in space and time should open the door to investigating the causes and consequences of “modernity’s” emergence, an evolutionary development that is of comparable importance with such landmarks in prehistory as the rise of bipedalism, lithic technology, and food production.

As previously noted, the prevailing criteria for behavioral “modernity” generally fall into two, non-mutually exclusive categories, one relating to mental competence and the other to the practise of culture. I have elsewhere commented extensively on the mental criteria (Bower 2004; in press) and will not repeat my detailed critique here. Suffice it to say that, in general, the proposed benchmarks for mental “modernity” are not workably defined in the sense proposed a paragraph ago. Apart from this, it seems to me that, for reasons to be developed anon, a criterion for “modernity” should include some reference to the social dimension of human existence (cf. Renfrew 1984:39-44). As will presently be discussed, this dimension certainly entails a crucial element of mental acuity. But the social dimension has its own ontological status whose properties give rise to aspects of “modernity” that leave a substantial imprint on the archaeological record. And I suggest that these aspects of “modernity” have precipitated a major inflection point in the human career.

Earlier in this paper, I have referred to culture “in the anthropological sense.” To specify somewhat, with all due respect to the plethora of definitions that have been proposed (Kroeber and Kluckhohn 1952), I am basically comfortable (mutatis mutandis) with Tylor’s (1871) articulation of the concept as “that complex whole which includes knowledge, belief, art, law, morals, customs, and any other capabilities and habits acquired by man as a member of society” (italics added). But it needs to be emphasized that this definition refers to living people and therefore admits the possibility that the behavior of early prehistoric humans may have included an attenuated version of culture, what could be called protoculture, distinguished in part by the absence of “society” as seen in the true culture of living populations, while still exhibiting some of the characteristics of true culture, such as the social transmission of knowledge. Thus, for example, one can imagine a repertoire of proto-cultural behavior that includes some form of language, which is a quintessentially symbolic practice, but lacks any form of social organization resembling what we would recognize as “society” (cf. Müller-Beck and Porr 2004).

From such an incremental perspective on cultural emergence, there comes into view a way of breaking the stalemate described at the outset of this article. Few would deny that the behavioral record of Later Stone Age/Upper Palaeolithic times is closely comparable with the kinds of behavior observed in living hunting-gathering societies (Klein 2002:8). In other words, the “late emergence” model is focused on a point in the human career that clearly reflects true culture, but not necessarily its earliest expression. Thus, the issue confronted by the “early emergence” model can be recast as a quest for the earliest conclusive evidence for true culture, a quest that hinges mainly on evidence of socially mediated behavior. The stalemate between the “early emergence” and “late emergence” models has been resolved as a result of refocusing the search for the origins of “modern” behavior on its social dimension, thereby admitting the possibility of a third model we could label “in between emergence.”

All well and good, but a major question remains unanswered, namely, what kinds of archaeological evidence can attest to socially mediated behavior? Indeed, how can we define socially mediated behavior in archaeologically workable terms? Such questions provided the motivation for my review of the works listed at the top of this article, the rationale for which needs to be exposed. (There is a long version of
this, which I will here condense into its points of articulation between socially mediated behavior and the archaeological record.) Drawing mainly on the literature of psychology and social psychology (e.g., Mead 1934; Ashmore and Jussim 1997), it seems clear that human society is fundamentally rooted in two kinds of identity, one stemming from self awareness and the other from group awareness. As summarized in Ashmore and Jussim (1997:12), “At the most inclusive level, self and identity figure in the production and reproduction of societies and cultures.” Thus, it can be suggested that archaeological confirmation of such awareness would constitute convincing evidence of socially mediated behavior and therefore the existence of true culture.

To my knowledge, there are few prehistoric artifact types that are routinely accepted as incontrovertible evidence of self and/or group awareness, and most are substantially younger than the time frame under consideration. However, various kinds of decoration, including beads and pendants, have been represented in archaeological discourse as if they were a material expression of identity; in effect, symbols for personal and/or group identity (Bower 2004; in press; Hodder 1982). Since this notion clearly involves semiotic concepts, I turned to Preucel’s (2006) publication in hopes of discovering an approach to testing the presumed symbolic relationship, an approach which circumvents Binford’s (1987:402) critique of the assumption that “all artifacts…are direct semiotic evidence,…[presenting] themselves as clues to the intellectual determinants of the ancients’ behavior.” In this case, we are concerned only with assessing the possibility that a specific kind of artifact may yield credible evidence of its role in mediating prehistoric human social behavior. While Preucel’s book has proven edifying, covering a wide variety of topics that range from linguistics through pragmatic anthropology to cognitive archaeology, interweaving relevant aspects of semiotics along the way, it has failed to yield the hoped-for method of demonstrating the manner and extent to which prehistoric body decoration is a material expression of identity. Perhaps Preucel’s most illuminating observation on the subject is to be found under the heading “Reading Material Culture” (pp. 135-137), in which he notes Hodder’s (1987) conclusion that “most semiotic analyses provide an inadequate base for understanding both meaning content and the relationships between signs and the world of material action.”

Clearly, we have reached a dead end in this direction. However, before embarking on a different tack, I would like to enlarge on what I had hoped to obtain from Preucel’s study of archaeological semiotics. If one considers the semiotic role of body decoration in the modern world—say, for example, in football uniforms—the link between symbol and identity is perfectly clear to those who know anything about the sport. Not only does the design of the uniform identify the player’s team membership, but the numerals on the jersey also identify within broad limits the player’s role on the team, since lower numbers are generally identified with ball handlers (backs and receivers) while higher numbers are commonly identified with those who protect or tackle the ball handlers. Thus it can be said that a player’s “body decoration” (i.e., uniform) is semiotic evidence of that individual’s identity in the world of football, but only for those who are familiar with the sport. For all others, the semiotic evidence is non-existent, barring information from a knowledgeable source. Since Preucel failed to serve as a “knowledgeable source” with respect to the kind of semiotic problem with which I am concerned, I am left with an inference regarding the relationship between prehistoric human body decoration and the identity of the wearer, but no conclusive evidence to that effect.

So I conclude this paper with a look at the volume edited by Timothy Insoll on “The Archaeology of Identities” in hopes of finding what was not available in Preucel’s book. As readers on archaeology go, this one contains a highly varied and interesting assemblage of papers, ranging across such titles as The Politics of Identity in Archaeology; The Constitution of Archaeological Evidence: Gender Politics and Science; Beyond Mother Earth and Father Sky: Sex and Gender in Ancient Southwestern Visual Arts and Ethnography; Archaeology’s Humanism and the Materiality of the Body; and Changing Identities in the Arabian Gulf: Archaeology, Religion and Ethnicity in Context. Included in this diverse collection of seventeen previously published articles, plus an extensive introduction, are numerous insightful comments on archaeology’s contribution (or potential contribution) to such identity related issues as ethnicity, interethnic tensions and animosities, territorial claims,
and multiple, overlapping identities, to name but a few. In short, Insoll’s reader is a highly useful exploration of the place of identity in archaeological research. Moreover, Insoll’s introductory chapter (Configuring Identities in Archaeology) confirmed not only the central role of sociology and anthropology in developing theories of identity, but also the urgency of such inquiry in the world today and the opportunity this opens for archaeology to produce work of contemporary relevance. It is also worth noting that this chapter includes an explicit recognition of the problem of self identification, to wit: “The archaeology of identities is essentially concerned with the complex process of attempting to recover an insight into the generation of self at a variety of levels: as an individual, within a community and in public and private contexts” (p. 14).

Upon reading this passage, I experienced a surge of optimism regarding the prospects of finding a path toward the discovery of a form, or forms, of evidence that could clinch a connection between prehistoric body decoration and self/social identity. However, the optimism soon faded as I read the opening lines of the chapter’s last paragraph: “Finally, for the archaeologist of the future, new challenges seemingly await. Material culture is emblematic of identities…, but also the urgency of such inquiry in the world today and the opportunity this opens for archaeology to produce work of contemporary relevance. It is also worth noting that this chapter includes an explicit recognition of the problem of self identification, to wit: “The archaeology of identities is essentially concerned with the complex process of attempting to recover an insight into the generation of self at a variety of levels: as an individual, within a community and in public and private contexts” (p. 14).

So, as promised in my introductory paragraph, I arrive at my inconclusive conclusion. But, however, discouraging as this may seem, let me hasten to proclaim my continuing commitment to the proposition that the rise of true culture at some as yet undetermined point in late palaeolithic time depended on human self awareness and the resulting emergence of human society. And I remain committed to the possibility of discovering a line of archaeological evidence that will demonstrate a link between material culture and self/social identity.

FOOTNOTES:

1. I put “modern” in quotation marks to indicate that the adjective is used in a special sense, referring to the broad behavioral competence of living humans, not their actual forms of behavior (such as bottle feeding infants or studying for diplomas).

2. I distinguish between working and workable definitions, the former being tentative and provisional, the latter being convincingly and purposefully linked to a particular conceptual framework.

REFERENCES CITED:


Northeast U.S.
Invited Review:
Archaic Traditions and Long-Term Patterning in the Far Northeast
By Susan Blair
University of New Brunswick

The recent publication of *The Archaic of the Far Northeast*, edited by David Sanger and M. A. P. Renouf, marks a significant transition in the regional archaeological discourse of northeastern North America. The volume is the result of a special invitational symposium that was held at the University of Maine at Orono in 2001. The symposium sought to explore a broad period between 9,500 and 3,000 years ago—a period encompassing as much as 60% of the entire archaeological record of the Northeast. An explicit goal of the symposium was to update the 1974 *Symposium on Moorehead and Maritime Archaic Problems in Northeastern North America*, hosted by the Smithsonian Institution, and published as proceedings in the 1975 volume of the journal *Arctic Anthropology*. In the intervening decades, the extent of Archaic research in this portion of the Northeast has increased dramatically, and the 2001 symposium and its ensuing publication represents a much-needed update, and a glance at new information.

Three major themes emerge in this volume that address central issues for the region, and in a broader sense, for prehistoric archaeological research processes in North America. First, all of the papers reflect, at some level or another, an attempt to work through the issue of the meaning of large-scale spatial and temporal patterning in the archaeological record—what do these kind of patterns reflect?—do they relate to an anthropological notion of “culture”?—if so, how? The second theme is focused on the nature of the research process itself, and its impact on our interpretations—how do we construct understandings at large spatial scales that transcend differences in theory, preservation, and research? In this sense, this volume is a case study in how archaeologists tend to approach regional analysis and work towards larger scale syntheses. A third, related, but less explicit theme, that becomes clear through a careful and informed reading of the volume, is the persistently uneven and localized state of our knowledge about the prehistory of this large and diverse area—how do we find a balance between the use of convention and imagination as we develop larger interpretations in a region characterized by a few bright spots surrounded by darkness?

The volume itself consists of 14 chapters, as well as a critique and overview chapter, representing the work of 24 researchers. Although many of the papers make reference to broad, horizon-like patterning and the implicit goal of seeking large-scale understandings, the editors have organized the papers by subregion, and not by chronology or thematic categories. In part this reflects the fact that most archaeologists continue to work locally and strive to create local sequences based on their work. Many of the papers attempt to address the Archaic as a whole in a particular place, an approach that renders a chronological ordering of papers impossible. This structure, however, plays up one of the major challenges to the incipient synthesis. The “Far Northeast”, as an archaeological region, is both fractured by modern boundaries (including numerous geopolitical, linguistic, and biophysical divides) and “uncentered” as a research universe. The latter problem appears to be particularly pernicious. Not only are the archaeological research traditions of Newfoundland and Labrador, southwestern Québec and Ontario, and northern New England and the Maritime Peninsula independent and distinctive, each of these areas have centered in them distinctive culture historical frameworks and cultural and technological traditions. It has been difficult to evaluate to what extent these different archaeological traditions represent ancient regional developments, and to what extent they reflect these different research histories. Overall, a general image emerges of a region described by its many edges, each defined by distinctive, monumental, long-standing cultural traditions (although there are clearly many debates as to how distinctive each tradition is, and which one should be given more prominence in the larger region).
To the north, there is the Maritime Archaic tradition (or "Maritime Archaic Indian" [MAI]). To the northwest, there is the Shield Archaic tradition. To the southwest, there is the Laurentian Archaic tradition. To the south, there is the Small Stemmed Point tradition, with its links to the Atlantic Slope Archaic macrotradition. Also in the south (or perhaps, the south-center) is the Gulf of Maine Archaic, in northern New England and adjacent parts of the Canadian Maritimes and Québec. This latter entity differs somewhat from other manifestations. It is the most recently proposed of all of these entities and, perhaps as a result, its advocates (especially Robinson and Sanger) explicitly propose it as a modification of the 'whole-culture tradition' approach, to allow for independent technological traditions and horizon-like regional configurations.

This region-based structure leads to interesting patterning within the volume itself. Some papers deal with archaeological sites and materials centered in a particular tradition, while others try to sort out the boundaries and gradations between traditions. These result in very different research questions and interests from paper to paper. The subregion north of the Saint Lawrence is represented by six papers. Renouf and Bell, Fitzhugh, and Jelsma address materials from sites and areas that were key to the initial formulation of the concept of the Maritime Archaic, and, thus, they are largely concerned with its inner workings—issues of such as settlement, social structure, and symbolic behavior. Pintal, Plourde, and McCaffrey are all working in the shadow of this phenomenon, and consequently, on one level or another, attempt to understand to what extent the sites in their areas resemble, or do not resemble, those of the Maritime Archaic.

Similarly, researchers centered in Maine, including Sanger, Clark and Will, Robinson, and Spiess and Mosher continue to flesh out local and regional patterning, largely within the framework of the Gulf of Maine Archaic tradition. However, northeastern Nova Scotia and Cape Breton, the eastern portion of New Brunswick, and Prince Edward Island, all remain virtual blanks in the Archaic, largely due to a lack of sustained research and excavation in these areas. How should these areas be approached? On the one hand, the traditions developed in other areas can act as a guide, a view adopted by Deal et al. On the other hand, it would be foolish to use particular elements of these traditions as direct interpretive keys in adjacent regions (the "index fossil" approach that the volume reviewer, J. V. Wright, quite correctly rails against)—after all, might not the reality be something completely new instead of a distant echo of the patterns in Maine or Newfoundland?

Chapdelaine and Clermont, Bourque, Cox, and Lewis, and to a certain extent, the volume discussant, Wright, are more explicitly working the edges of the problem. Each of these authors discuss bridging assemblages with links to the traditions and macrotraditions of adjacent regions. Chapdelaine and Clermont reveal a great deal about the nature of the Laurentian Archaic in the upper Saint Lawrence and the Great Lakes basin through the lens of Morrison Island and Allumette, and in this sense, their paper serves to define elements of Laurentian Archaic from within. While the shadow of this tradition clearly falls far to the east, many working in the more distant parts of the Far Northeast (i.e., far from the 'center' of the phenomenon) view this tradition with some uneasiness; in Sanger's (p. 237) work, it "...fits uncomfortably with the data available from the Maritime Peninsula...", as its identification depends on "...how much weight to assign to one specific class, the large, side and corner-notched bifaces" (see also Wright pp. 457, 460). In keeping with the notion that the Laurentian Archaic tradition has a center to the west (and therefore, away from marine and coastal areas), many have inferred that a Laurentian Archaic presence must, by default, be interior-related and most visible in the western portions of the larger region, a view directly challenged by Spiess and Mosher (p. 390). Similarly, Bourque, Cox, and Lewis present information on a series of sites in southern Maine, at the southern margins of the region, providing insight into linkages with southerly affiliated entities, such as Small Stemmed Point Archaic, and the Susquehanna tradition.

Although none of the papers in the volume reflect a site-based view of the Shield Archaic, its presence is likewise felt in the larger region, looming to the northwest. Unlike the other macrotraditions, however, it appears that
researchers in the far Northeast largely view Shield Archaic as a force to be resisted or counterbalanced by the presence of other more proximate and well-defined sequences, such as that of the Maritime Archaic tradition to the northeast, a perspective most clearly expressed by McCaffrey. From his response to this paper, it is apparent that Wright finds this baffling and disturbing: “It is clear that the concept of a Shield Archaic is an anathema for a number of Québec subarctic researchers and, despite the stress on a regional focus, there is little excuse for limiting comparisons to the east and ignoring the west” (p. 453). However, researchers working in areas between these large-scale spatial and temporal entities consistently struggle with identity and meaning. For those working on sites and sequences closer to the presumed center of a phenomenon, the relationship of an archaeological tradition to a particular cultural group is easier to ignore. In the ‘between’ areas, where particular attributes and tool types occur in complex and uncoordinated pattern, these questions loom large.

Many of the papers in the volume also reflect a fear of the potential downsides of imposing outside traditions in comparatively unknown regions. At the time of the 1974 symposium, the inability of researchers to match sites and assemblages in the Maritime Peninsula with any of the known Archaic phenomenon in the Northeast (many of the very traditions—Lau- rentian Archaic, Atlantic Slope Archaic, or Maritime Archaic—discussed in the volume) led many to assume that there was either a gap in habitation in this region during the Archaic, or that all sites from this time had been destroyed by coastal erosion (Sanger and Renouf p. x). It turned out that many Archaic assemblages from this area looked nothing like any of these traditions, lacking key diagnostic tool forms because they lacked strong bifacial reduction traditions (see papers by Sanger and Robinson). This was compounded by Archaic preferences for floodplains, and site survey strategies that failed to appreciate the potential depth of floodplain deposits. These problems have led to a profound unease in less well-known portions of the region with overly simplistic ascription of cultural affiliation using particular tool types. Fortuitously, it has also led the development of more comprehensive treatments of assemblages and technological systems, as is evident in the papers by Robinson, Sanger, Clark and Will, and Spiess and Mosher, and as is evident in the reaction to typologically based analyses, such as Wright’s reaction to Deal et al. As suggested by many of the authors, these monumental entities only retain their value in the larger region when they are approached with a degree of sophistication: “It is emphasized that these expansive traditions provide important insight into regionally shared practices, environments and social networks, but they also reflect the limitations of archaeological visibility. Their value lies, in part, as models against which to test local variability” (Robinson p. 346).

In a more profound way, the regional perspective that unites these papers also highlights theoretical and methodological tensions inherent in the process of unit construction itself. Many of the papers represent a valiant attempt to integrate multiple scales of analysis in a subject with tremendous temporal and spatial scope. Some, however, are more explicit in addressing the normative underpinnings of these understandings than others. Indeed, the notion of “tradition”, at the heart of large-scale regional patterns, is typically construed as a temporally and spatially consistent cultural phenomena that has clear edges, both at its initiation and its conclusion, and in space. As a result, regional researchers are trying to use models best for defining centers in a region best understood by defining its edges.

The treatment of “tradition” ranges from implicit to explicit, with theoretical underpin-nings that range from classic culture history to a kind of ecologically-informed evolutionism. Although many papers grapple with the link between culture, ethnicity, and tool traditions, some view the relationship as straightforward and clear. For example, Fitzhugh employs the notion of tradition as culture in his analysis of fluctuations in tool forms and lithic types: “The alien tool styles and southern lithics suggest an intrusion by southern peoples into a territory dominated before and after by a local Labrador tradition and ethnic group” (Fitzhugh p. 54). He sees this as coinciding with the appearance of “early Siberian-derived Paleoeski-mos who arrived in northern Labrador from the west”, possibly introducing bow and arrows...
to regional Archaic cultures, and negatively impacting on population size of the local Maritime Archaic Indians due to “the military advantage of their Asian-derived bow and arrow technology” (p. 63).

Similarly, there appears to be a widespread consensus for understanding an array of shifts in technology, subsistence, settlement, and ceremonialism in the Maine/Maritimes area that occurs around 3700 bp as a replacement of Late Archaic people with Susquehanna people (see Sanger, Deal et al., Bourque et al., Robinson, Spiess and Mosher). In this latter case, researchers working in the Maine/Maritimes area repeatedly point to temporally consistent change in such a large constellation of attributes, that there truly does appear to be compelling evidence for large-scale cultural change around this time. Whether this change represents profound material culture change, adaptational change, or population change that encompasses both is unclear. After all, however one constructs one’s notion of culture, there appears to be a growing consensus in the Far Northeast with the conclusion that the ‘culture’ of the people on either side of the Late Archaic/Susquehanna temporal divide are different. On the other hand, many are less certain of the implications for the genetics and ethnicity of those same people. As stated by Sanger (p. 242), “…in the absence of skeletal remains… it is hard to assess the possibility that interbreeding may have occurred, resulting in a totally new suite of artifacts, mortuary ceremonialism and settlement patterns.”

Despite the caution of some, others regularly confound ethnicity, genetics, and culture in the construction of traditional culture history frameworks. Perhaps the most surprising example of this is in the paper by Jelsma, on the biological anthropology and genetics of the Archaic population of Port au Choix, Newfoundland. On the one hand, Jelsma is able to demonstrate that the women of ancient Port au Choix represent a much more genetically diverse subpopulation than the men, suggesting that they are marrying into the community from some distance away. He then focuses his analysis of the cemetery on the achieved status of the men as indicated by burial location and diet based on stable isotope analysis. Not only are the presence of women and children in these graves and in the larger population disregarded for the remainder of the paper, the interesting implications for the relationship between material culture, social relationships, ethnicity, and genetics are ignored. By so doing, Jelsma misses an opportunity to develop a more subtle approach to understanding the relationship between diachronic archaeological unit construction, and the synchronic links between individuals, groups, and territories. In his critique of this paper, Wright correctly suggests that these very links may allow us to understand how particular materials (or by extension, ideas and technological practices) move within and between regions, a view hinted at by Burke.

Some of the authors in the volume address this problem of unit construction directly and explicitly. Robinson and Sanger react to the ‘whole-culture’ approach, which equates an archaeological tradition in concept to “an ethnographically described culture” (Sanger p. 224). Instead, they both use the term to explicitly refer to a part of culture, such as a particular technological tradition (Robinson p. 345), much in the manner originally intended by culture historians Gordon Willey and Philip Phillips. This perspective is particularly key to Robinson’s approach, as he successfully contrasts a series of lithic traditions with a more widespread set of mortuary traditions. The disarticulation of particular subsystems allows Robinson to explore widespread, macroregional patterns, or horizons, while developing an understanding of profound cultural continuity within regions and landscapes, and the complexity inherent in how people arranged themselves in them. Spiess and Mosher undermine the internal cohesion of these traditions even further by comparing faunal assemblages from sites attributed to a range of different complexes and traditions, finding a lack of consistency that contradicts “…the conventional wisdom of the last 30 years of Maine archaeology” (p. 401).

A further theoretical unease rests with the set of largely unstated assumptions about evolution and long-term change. In many other parts of the world, analysis of such a long period would lead to at least some explicit statements about the role of evolutionary change. Although few address this unease, it may be rooted in several challenges. On the one hand,
many of the papers represent site-based treatments and comparatively patchy datasets. On the other hand, the evidence that currently exists suggests very complex patterning that defies simple, unilinear modes. The former problem is one exacerbated by the small number of active researchers in the region. The volume of new research that is presented represents a great deal of hard work by very few people. In this sense, this problem will partially resolve itself over time. Indeed, one of the most heartening aspects of the book is that it reveals the accretional nature of archaeological research, especially when one compares it to the state of knowledge 25 years earlier. On the other hand, there are also significant differences between assemblages within and between subregions of the far Northeast that contribute to the problem. These differences may be unsolvable in the long term, and indeed, may grow more acute with time. Renouf and Bell have produced a refined and sophisticated treatment of site location and sea-level change, exploring one of the major regional confounders—sea-level change due to isostatic rebound. In some parts of the Northeast, coastlines are subsiding, while in others, they are rising. In parts of the Gulf of Maine, this problem is so severe that many Archaic sites are now inundated, and are often completely eroded or destroyed in the process. The process is ongoing and promises in future to increase in impact. Regional analyses that seek to explore demographic change, for example, are rendered impossible because the total site assemblage is forever unavailable for research in even the most basic way. Even extant sites can be very difficult to compare. Components from the large river valleys with well-developed floodplains are difficult to correlate with components from interior areas forested with conifers. In these latter settings, most sites are essentially surface sites rendering stratigraphic analysis and radiometric data problematic. Interior and mainland sites are often found in highly acidic soils, resulting in very poor or non-existent organic preservation. On the other hand, sites on the Island of Newfoundland, and coastal sites with neutralizing deposits of shellfish shells, may have fairly good preservation of hard organic tissue, but poor soft tissue preservation. In all cases, the resulting regional unevenness remains a significant problem for larger model-building exercises.

However, what we do know about the region often seems to defy comparatively straightforward evolutionary sequence building, especially when the Archaic is compared with the successive Maritime Woodland (or Ceramic) period. In many parts of the region, the earliest Archaic lithic reduction systems seem to emphasize flake and core technologies, which are later replaced by an increasing focus on biface reduction (see Clark and Will, Robinson). Archaic mortuary behavior throughout the region is very intricately patterned (particularly as discussed by Fitzhugh, Jelsma, Robinson), with relatively large, complex, and richly furnished mounds being replaced over time with more simple, and eventually, virtually invisible mortuary sites. The intensity of exchange and interaction referenced by many authors and described in detail in papers by Chapdelaine and Clermont and Burke fluctuates over time, reaching macroregional proportions in some periods, and becoming very localized in others. Unfortunately, in almost every case (with the exception of McCaffrey), there is little or no reference to the post-Archaic period. This may, at least in some cases, reflect the notion that post-Archaic people are somehow profoundly different from Archaic people, and are therefore not relevant to the main subject matter. It may also reflect general interest in older sites and materials over the more common (although, I would argue, far less well understood) post-Archaic material. I know, based on personal knowledge of many of the researchers, that in most cases, it does not reflect a view that temporal distance from modern First Nations people may render the subject less political. Nonetheless, it is an odd omission, given the focus on long-standing regional traditions, and the implications of this view for understanding long-term cultural change.

In a broad sense, the papers in this volume grapple with both large-scale patterns, and a legacy of early generalizations about the Northeast, many of which were formed near the margins of the region, or in adjacent regions. While these have provided researchers in the Far Northeast with an analytical framework, which has served as a sounding board against which new data have been compared, it is a frame-
work that clearly has had the potential to mislead.

Much of the more recent research has been focused on fleshing out the details of local patterns, and many of these patterns are proving to be resistant to integration into the old frameworks. “The need to give more attention to regional manifestations and less to the integrative construct is still part of the whole taxonomic problem” (Chapdelaine and Clermont p. 206). Perhaps the greatest challenge in the construction of a regional synthesis is in resisting the interpretive momentum of these frameworks, and their alluring ability to cover over the many dark, unexplored areas that fill the spaces between well-known sites, and well-surveyed jurisdictions, with facile explanations. Otherwise, we risk the closure of narrative possibilities and the loss of our ability to imagine unanticipated possibilities—such as the possible presence of long-standing lithic traditions that do not include the manufacturing of easily-typed projectile points to act as our archaeological signposts.

The most exciting implication of the research presented in this volume is that the greater part of the challenge lies ahead. Clearly there is still a great deal to be learned about the prehistory of the Far Northeast. The incipient syntheses presented in this volume can only function to stimulate further research, and will undoubtedly act as a sounding board, a body of research against which we can continue to test local variability.

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Commentary:

What’s New? Some Basic Issues in the Study of Cultural Innovation
By Michael J. O’Brien

It would be difficult to find another topic in anthropology, and by extension archaeology, that has played as important a role as innovation in framing arguments about why and how human behavior changes. Likewise, it would be difficult to find another topic that has so consumed researchers over the past century and a half as attempting to derive a framework that addresses both the production and spread of innovation.

Ethnologists working early in the twentieth century paid particular attention to what typically were termed “culture traits,” using them as a means of linking related cultures together. Archaeologists did the same. Rarely, however, was there consensus on what a culture trait entailed and at what scale it should be examined. Beginning in the 1980s there occurred an emerging interest in applying Darwinian principles to the study of culture, and one area in which considerable advance was made was the study of cultural inheritance. As interesting and valuable as these studies are, there remain areas that need in-depth research, especially with respect to the production of cultural innovation and the scale and tempo at which it is produced. No longer is it sufficient to think of selection “tinkering” with subtle variations, slowly effecting change over long periods of time. Rather, there are times when innovation appears as larger packages, the product of emergent human behaviors at a fairly large scale.

I considered these issues as Stephen Shennan and I were organizing a workshop on innovation hosted this past September by the Konrad Lorenz Institute in Altenberg, Austria. The workshop, which brought together archaeologists, anthropologists, psychologists, philosophers, geneticists, and evolutionary biologists, focused specifically on the role that innovation plays in effecting human behavior. One issue that underlay many of the discussions was the distinction between invention and innovation. Writing in the 1920s, Austrian economist Joseph Schumpeter made the distinction between invention—the creation and establishment of something new—and innovation—an invention that becomes economically successful and earns a profit. Although not all seminar participants saw the need for a distinction—some maintained that the term “variation” works well for both the origination and spread of a trait—there perhaps is some merit in keeping them distinct, if for no other reason than it separates two distinct processes: the production of variants and the spread of those variants.

Innovation was explicit in the nineteenth-century writings of ethnologists such as Tylor (1871) and Morgan (1877), just as it was in the work of later cultural evolutionists such as Steward (1955) and White (1959). For the latter, the evolutionary process was less orthogenetic than it was for the earlier evolutionists, with the source of innovation wrapped up in the kind of mechanisms a group needs to meet the challenges of its physical and social environment. Innovation has also played an essential role in American archaeology, as even a brief perusal of the literature of the last hundred-plus years will show (Lyman 2008). American culture historians of the twentieth century routinely looked to diffusion and trade as sources of innovations, and hence of culture change, adopting without comment the models of their ethnological colleagues. Sometimes innovations were viewed as having been borrowed—often from incredible distances and by almost superhuman means (e.g., Ford 1969)—and other times they were viewed as products of what Adolf Bastian in the mid-nineteenth century referred to as the “psychic unity of mankind” (Lowie 1937). There was a third alternative, and it was manifest most clearly in the work of Steward (1955)—what became known as multilinear evolution.

Steward asked why, for example, did many of the same culture traits that occurred within, say, a patrilineal hunting-and-gathering group in West Africa also occur within a hunting-and-gathering group in the Great Basin? Obviously these two groups were not historically related, so the similarities must be the result of something else. For Steward, the answer resided in what he termed the cultural cores of the groups—similar solutions not to similar environments but to similar environmental problems. Steward argued that if the ethnologist (or archaeologist) could determine which traits were at the core of a culture and which ones were secondary—not necessarily a simple task (Jordan and Mace 2006;
Moylan et al. 2006)—then the traits could be used to assess the degree of cultural relatedness between that culture and others.

Despite the widespread use of culture traits as measures of relatedness or of functional convergence, there has long been much less emphasis on trying to figure out exactly what a culture trait is. Researchers have long assumed that such traits are mental phenomena that one acquires through teaching and learning (see below), but through much of the twentieth century there were few explicit theoretical definitions of a culture trait (Osgood 1951). This was highly problematic and meant that the units varied greatly in scale, generality, and inclusiveness (Lyman and O’Brien 2003). Biologists might be quick to point out that there are also procedural problems in their discipline, where there is no standard set of characters used in the creation of taxa, but I would argue that the situation is more murky in anthropology. Biologists, for example, learn early in their training the difference between a character and a character state, but the distinction is made less frequently in anthropology, which has a history of emphasizing nominal units that are at best vaguely specified.

The one place where I think anthropologists have made insightful comments is with respect to what early in the twentieth century became known as trait complexes—minimally defined as “groups of culture elements that are empirically found in association with each other” (Golbeck 1980). Although trait complexes have traditionally been used as another means of comparing cultures, the concept has a role to play in modern cultural evolutionary analysis, if for no other reason than it reminds us that cultural phenomena evolve as complex wholes, not as tiny parts (Boyd et al. 1997; Guglielmino et al. 1995; Henrich and McElreath 2003; Pocklington 2006). The parts change over time, but it is the phenomena that evolve. And, the parts can change in tandem as opposed to singly. Selection can, and often does, act as a tinkerer—and “one who does not know exactly what he is going to produce but uses whatever he finds around him” (Jacob 1977:1163)—but it’s the “cascading” effects (Schiffer 2005) of that selection that are important. My point is this: Novelties are quite often more than simple character-state changes shaped by selection (Basalla 1988; Reid 2007). This is more or less what Trigger (1998:364) apparently had in mind when he presented his opinions of the scope of evolutionary archaeology:

I agree that evolutionary archaeology, with its emphasis on selection, has [much] to offer archaeology. . . . What seems essential, however, is to recognize that among human beings selection has ceased to be largely external—it also involves culturally mediated perceptions, agency, and choice. . . . I believe that evolutionary archaeology would be more widely accepted and better equipped to account for sociocultural phenomena if its sponsors abandoned what seems like a reductionist biological terminology in favor of one that explicitly takes account of the unique, emergent aspects of human behavior.

I agree with Trigger. It is those “emergent aspects”—aspects that have irreducible novel properties—that are important considerations in any discussion of cultural innovation.

Cultural Transmission—
The Spread of Innovation

From the beginning, regardless of how ethnologists and archaeologists viewed culture traits themselves, and irrespective of their arguing over whether a particular trait was transmitted vertically (cultural ancestor to cultural descendant) or horizontally (cultural group to unrelated cultural group), there was complete agreement that traits, like culture itself, were acquired (learned), not inherited. Kroeber (1923), for example, explicitly distinguished between the transmission of genes, which involves heredity, and the transmission of culture, which involves acquisition and learning. For Kroeber (1923:3), “heredity is displaced by tradition, nature by nurture.” In his view, tradition involves a “non-biological principle” because biological transmission is limited “only to blood descendants,” whereas cultural transmission can be between “individuals and groups not derived by descent from” the originators of the culture trait being transmitted (Kroeber 1923:7). Thus, cultural transmission does not necessarily produce change in the sense that the genotype of a descendant differs from that of its ancestor; rather, the results of cultural transmission
involve what Kroeber (1923:7) referred to as “accretion to the stock of existing culture.” This is true—cultures, however defined, take on traits, adding them to the repertoire of already acquired traits. Organisms do this too, but over vast periods of time (think of the change in eukaryotes from the earliest organisms to what we see now). Those periods are so vast that it is not worthwhile to quibble with Kroeber. For the sake of argument, let’s ignore the similarities and differences between cultures and organisms and focus instead on a problem with Kroeber’s statement “accretion to the stock of existing culture.” The problem is that Kroeber apparently overlooked the fact that cultures lose traits in addition to “accreting” them. To him, once the cultural stock was formed—similar to Steward’s (1955) “core”—it became simply a matter of hanging ornaments on it. But cultures aren’t stable; rather, they are constantly evolving amalgams of traits at every conceivable scale. Cultural transmission assures that this is the case. Traits are acquired, and traits are lost, all at a dizzying pace and through a variety of processes. To Kroeber, though, and others both before and after him, what really mattered was diffusion—the sharing of ornaments across the cultural landscape.

The fact that culture and biology take different pathways in transmitting information in no way precludes the application of Darwinian principles to the study of cultural features, although anthropologists and archaeologists fought mightily for over a century to keep biology and culture separate. There were numerous uses of biological evolutionary terms and schemes (e.g., Colton 1939; Gladwin and Gladwin 1934), but they were usually either metaphorical or at best epistemologically unsophisticated. Such uses were usually met with derision, including Brew’s (1946:53) famous declaration that “phylogenetic relationships do not exist between inanimate objects.”

Brew, of course, was correct: tools do not breed. But tool makers do breed, and they do transmit information to other tool makers, irrespective of whether those other tool makers are lineal descendants. Transmission, particularly between parents and offspring of the same sex (Shennan and Steele 1999), creates what archaeologists have long referred to as tool traditions—patterned ways of doing things that exist in identifiable form over extended periods of time. It seems naive, given what we know of the archaeological record, not to believe that tool forms are modeled on preexisting forms. Further, cultural phenomena are parts of human phenotypes in the same way that skin and bones are, and as such they are capable of yielding data relevant to understanding both the process of evolution and the specific evolutionary histories of their possessors.

That, however, is a modern view and not one held throughout much of the twentieth century. Not only was there a wide gulf between such things as pots and bones, there were completely different views on the shape of biological and cultural evolution, the former portrayed as diverging and the latter as being simultaneously diverging and highly reticulate, running through time like braided stream channels. Without clear, unequivocal, and irreversible divergence, so the argument went, how could one hope to trace ancestry except in the most superficial way? Perhaps a trait could be traced back in time, but how did it relate phylogenetically to other traits? What Kroeber ignored—and he subsequently was joined by generations of anthropologists—was over a century of work in historical linguistics, which showed that it was indeed possible to trace the ancestry of languages, despite borrowing and reverse borrowing. Borrowing does not create a “hybrid” culture or language (Goodenough 1997).

With the growing interest in Darwinian evolution that became noticeable in anthropology in the 1960s and accelerated through the 1970s and ’80s (e.g., Campbell 1965, 1970; Dunnell 1980; Durham 1976; Rindos 1980), researchers began to reconsider the relationship between biology and culture, and nowhere was this more evident than in attempts to understand the role of innovation in the evolution of cultural systems. One area of sustained focus not only in anthropology but in the social science in general was cultural transmission (e.g., Boyd and Richerson 1985; Cavalli-Sforza and Feldman 1981; Cloak 1975; Durham 1991; Henrich and Boyd 1998; Lumsden and Wilson 1981; Pulliam and Dunford 1980; Richerson and Boyd 1978, 1992).

One question that arose was: what, exactly, is the unit of cultural transmission? Further, how would we know if we found one (Pocklington 2006)? Various names were proposed for units—menemotype (Blum 1963), sociogene (Swanson 1973), instruction (Cloak 1975),
meme (Aunger 1999, 2002; Blackmore 1999, 2000; Dawkins 1976), and culturgen (Lumsden and Wilson 1981)—but there is still considerable debate over what the units embody (Atran 2001; Sperber 1996, 2000). Although perhaps a bit more sophisticated, these debates are not much different than those decades earlier with respect to what culture traits are.

One area in which there is a difference is whether the units of cultural inheritance have a physical nature similar to genes. No ethnologist or archaeologist of the twentieth century ever assumed that the ideas behind the physical manifestation of culture traits had a physical presence, but some modern researchers in memetics have made that proposal (e.g., Aunger 2002). In one of the most fully developed discussions of cultural transmission, Boyd and Richerson (1985:37–38) indicate that they “do not assume that culture is encoded as discrete ‘particles’” and that “it is possible to construct a cogent, plausible theory of cultural evolution without assuming particulate inheritance.” If Boyd and Richerson are correct, and I believe they are, this is good news for those of us interested in cultural evolution because we can get on with the important issue of figuring out where the units that get culturally transmitted come from in the first place.

Just because the units of cultural inheritance are not particulate in the same way genes are does not mean that biology is incapable of offering helpful analogs when it comes to understanding the production and transmission of novelties (Eerkens and Lipo 2007; Shennan 2002b). The key point is that the “calculated heritabilities for human behavioral traits are as high as or higher than measurements for behavioral and other phenotypic characters in natural populations of non-cultural organisms. . . . Thus it may be that [social learning] is as accurate and stable a mechanism of inheritance as genes” (Boyd and Richerson 1985:55). To be clear, analogs between biological and cultural transmission are just that: analogs, not metaphors. In a recent paper published in Behavioral and Brain Sciences, Mesoudi et al. (2006) argue that we can take advantage of the analogs between cultural and biological evolution in order to model the structure of a science of cultural evolution after the structure of the science of biological evolution (see Mesoudi et al. 2004). In brief, if both cultural and biological change are governed by the same underlying Darwinian processes of variation, differential selection, and the inheritance of selected variants, then the cultural and biological sciences should broadly share the same methodological and conceptual divisions.

One example is evolutionary archaeology (Dunnell 1980; O’Brien and Lyman 2000, 2002), which in numerous key respects is the cultural parallel of palaeobiology. Researchers in both disciplines are interested in identifying past biological/cultural innovations and reconstructing lineages and clades of those forms, thus revealing evolutionary relationships among the forms (O’Brien and Lyman 2002, 2003). In palaeobiology, this has been successfully carried out ever since the Modern Synthesis of the late 1930s and 1940s, with the principles of genetic inheritance providing the mechanism needed to link evolutionary lineages and clades. Indeed, the proper ordering of clades is based solely on our ability to recognize one particular kind of innovation, shared derived characters.

In archaeology, only recently have artifact traditions been seen as being causally connected by inheritance, or cultural transmission (see papers in Lipo et al. 2006 and O’Brien 2008). Lyman and I have referred to this causality as the assumption of heritable continuity, distinguishing it from mere historical continuity, in which units, at whatever scale, are in sequence but not necessarily causally linked by cultural transmission (O’Brien and Lyman 2000, 2003). The assumption of heritable continuity allows a truly Darwinian evolutionary archaeology (O’Brien and Lyman 2000, 2002; Shennan 2002a). Put more forcefully, evolutionary archaeology demands such an assumption.

Innovation, then, becomes a key area of analytical focus in any evolutionary study, especially with respect to the form of the innovation, its composition, and the process that created it. It is one thing to know how and under what conditions an innovation is transmitted, but it is a different matter entirely to understand where it came from. Even more important is understanding that especially with respect to cultural transmission, which is exponentially faster and has less fidelity than biological transmission, the transmission process itself is a continuous creator of innovation. Much more so than is typically the case in biology, tempo, and mode interact in cultural situations to create a new
source of innovation and to create it at scales that are both large and complex. This undoubtedly is what Trigger (1998:364) had in mind when he referred to the “unique, emergent aspects of human behavior.”

One field of analysis complementary to Mesoudi et al.’s (2006) scheme is the emerging field of evolutionary developmental biology, or “evo-devo” (Carroll 2005; Müller and Newman 2003). This approach is gaining increasing influence within biology, and there have been several calls for it to be integrated into the evolutionary synthesis (Kutschera and Niklas 2004). In biology, evo-devo concerns how genetically and environmentally influenced developmental processes interact with longer-term evolutionary change. A “cultural evo-devo” would address the emergent properties of human behavior, focusing attention on the processes by which novelties (tools and other human behavioral products) are generated by culturally transmitted information stored in the brain and how this process interacts with macroevolutionary change (Mesoudi and O’Brien 2008c). In short, the complexity of the information that is transmitted will impact error rates while it is being stored in the mind of the recipient as well as during the time the information is being recalled (Eerkens and Lipo 2005).

Recipes

It might be useful in this context to think of culture traits as “recipes” (Lyman and O’Brien 2003; Neff 1992)—the materials (“ingredients”) required to construct a tool, for example, and the behavioral rules (“instructions”) required to construct and use the tool. Cognitive psychologists (e.g., Weber et al. 1993) have proposed that people represent tools as interlinked, hierarchical knowledge structures, incorporating behavioral scripts governing their construction and use, much like the recipe concept. Biologists, too, use the “recipe” metaphor to describe the development of organisms from genetic information (Dalton 2000; Ridley 2003).

Krause (1985:30–31) was one of the first to employ the concept of “recipe” in a cultural context, defining it as a “list of ingredients and amounts” and a “part that tells you what to do, how to do it, when to do it, and for how long.” Schiffer and Skibo (1987:597) furthered the notion, defining a “recipe for action” as “(1) a list of raw materials, (2) a list of tools and facilities employed, (3) a description of the sequence of specific actions undertaken in the technological process, and (4) the contingent rules used to solve problems that may arise.” Because recipes are often culturally transmitted, they require a “teaching framework [that] consists of a series of practices that can include imitation, verbal instruction, hands-on demonstration, and even self-teaching by trial and error” (Schiffer and Skibo 1987:597).

The concept of recipe is useful for three reasons (Lyman and O’Brien 2003). First, the commonsense meaning of the term captures what anthropologists mean when they use the term “culture trait”—how, when, where, and why to produce something, whether a behavior or an artifact (a behavioral by-product). Second, the recipe concept contains multiple parts of two general kinds—ingredients and rules—that can be reconfigured to form a different recipe. Any change in ingredient acquisition, preparation, type, or amount; change of rules or the order of their implementation; or some combination of each results in a different product. This property was at the root of the problems encountered by early anthropologists when dealing with the scale of a culture trait. Third, the recipe concept highlights the flexibility built into virtually all ways of doing something and still producing a usable product.

Two important points emerge. First, the recipe is an ideational unit, meaning that it has no particulate reality. The product that results from implementing the recipe is an empirical manifestation of that recipe and does have a particulate reality. Second, recipes demonstrate that units of cultural transmission and replication can be of different scales. In biology, we know the scale of the unit of transmission and replication—the gene—but we also know that there often is no one-to-one correspondence between a gene and a somatic character. The same applies to cultural transmission, where conceivably every human behavior is underpinned by a recipe of unique composition, scale, and complexity (Lyman and O’Brien 2003).

Dual-Inheritance Theory

Boyd and Richerson’s collective work (e.g., Bettinger et al. 1996; Boyd and Richerson 1985, 1989; Henrich and Boyd 1998; Richerson and Boyd 1992), often referred to as “dual-inheri-
tance theory” (Richerson and Boyd 1978; Shennan 2002a), is particularly useful here. It posits that genes and culture provide separate, though linked, systems of inheritance, variation, and evolutionary change. One important realization of dual-inheritance theory is that cultural transmission produces significant similarities between those passing on the information and those doing the learning that cannot be accounted for by genetic transmission or continuity of environment (Bentley and Shennan 2003). The spread of cultural information is viewed as being affected by numerous processes, including selection, decision making, and the strength of the transmitters and receivers. But there is much more to Boyd and Richerson’s work than how and why traits spread. Their models also demonstrate that some innovation is produced through the intricacies of the transmission process itself. This calls into question the primacy of selection as the single most important evolutionary process.

I, in no way, want to remove selection from its prominent place at the evolutionary table. Rather, I point out that an overemphasis on selection as the key component of evolution has shifted attention away from adequate consideration of how variation is produced and the effects that production, irrespective of selection, has on evolution. Natural selection is often more a consequence of advantageous qualities that something has than it is the definitive sculptor of organismal histories (Reid 2007), and the same applies for cultural selection. It may turn out on closer examination that selection actually thwarts evolution much more often than it causes it. Certainly the term stabilizing selection suggests as much.

Several studies have made use of models derived from the work of Boyd and Richerson to examine patterns of cultural transmission in archaeological contexts (e.g., Bentley and Shennan 2003; MacDonald 1998; Shennan and Wilkinson 2001). One interesting study of the spread of innovation was Bettinger and Eerkens’s (1997, 1999) analysis of stone projectile points from the Great Basin of the western United States. There the bow and arrow replaced the atlatl (spear thrower) around 300–600—a replacement documented by a reduction in size of projectile points. The weight and length of points manufactured after 600, however, was not uniform across the region. Rosegate points from central Nevada vary little in weight and basal width, whereas specimens from eastern California exhibit significant variation in those two characters. Why are there differences and what, if anything, do they tell us about the production and spread of innovations?

Bettinger and Eerkens proposed that the variation is attributable to differences in how the inhabitants of the two regions obtained and subsequently modified bow-related technology. In eastern California, bow-and-arrow technology was both maintained and perhaps spread initially through what Boyd and Richerson (1985) refer to as guided variation, wherein individuals acquire new behaviors by copying existing behaviors and then modifying them through trial and error to suit their own needs. Conversely, in central Nevada bow-and-arrow technology was maintained and spread initially through indirect bias, in which individuals acquire complex behaviors by opting for a single model on the basis of a particular trait identified as an index of the worth of the behavior. Bettinger and Eerkens proposed that in cases where cultural transmission is through guided variation, human behavior will tend to optimize fitness in accordance with predictions of the genetic model, in which individual fitness is the index of success, with little opportunity for the evolution of behaviors that benefit the group as a whole. In instances where transmission is through indirect bias, which tends to produce behaviorally homogeneous local populations, conditions may be ripe for the evolution and persistence of group-beneficial behaviors.

From the standpoint of innovation, the models present widely differing scenarios. In both, individuals copy existing behaviors wholesale—innovations can suddenly “appear” in a new region as large, complex packages (projectile points, for example)—but in guided variation individuals begin tinkering with certain aspects whereas in indirect bias they do not. Under perhaps extreme conditions, individuals may not even be aware of the underlying principles of how and why something works. All they know is that it does work, and they attempt to reproduce it in toto. Of course, the copying process itself is rarely faithful, thus presenting considerable chance for copying errors, which themselves are novelties (Eerkens and Lipo 2005). Whether or not the errors are reproduced, and at what rates, are separate matters entirely.
A few years ago, Mesoudi and I realized that few experimental studies had attempted to simulate the cultural transmission of innovation. Further, we could find no experimental studies that simulated the transmission of recipes for making objects such as projectile points. Theoretical models such as those presented by Boyd and Richerson (1985) are wonderful things, and applications of the models to actual data, such as that by Bettinger and Eerkens (1997, 1999), are why we do science, but controlled “middle-range” experiments provide the necessary bridge between the two. In that vein we designed an experiment to examine the cultural transmission of projectile-point technology, simulating the two transmission modes—indirect bias and guided variation—that Bettinger and Eerkens suggested were responsible for differences in Nevada and California point-attribute correlations.

In brief, groups of participants designed “virtual projectile points” and tested them in “virtual hunting environments,” with different phases of learning simulating indirectly biased cultural transmission and independent individual learning. As predicted, periods of cultural transmission were associated with significantly stronger attribute correlations than were periods of individual learning. This obviously has ramifications for how we look at innovation. In simplified terms, more “loners,” more innovation; more group-oriented individuals who want packages off the shelf, less innovation. The experiment and subsequent agent-based computer simulations showed that participants who engaged in indirectly biased horizontal cultural transmission outperformed individual-learning controls (individual experimentation), especially in larger groups, when individual learning is costly and the selective environment is multimodal (Mesoudi and O’Brien 2008a, 2008b).

Cultural transmission in a multimodal adaptive landscape, where point-design attributes are governed by bimodal fitness functions, yields multiple locally optimal designs of varying fitness. Mesoudi and I hypothesized that innovations, represented by divergence in point designs resulting from individual experimentation (the individual-learning component of guided variation), were driven in part by this multimodal adaptive landscape, with different individuals converging by chance on different locally optimal peaks. We then argued that indirectly biased horizontal cultural transmission, where individuals copy the design of the most successful person in their environment, allows individuals to escape from these local optima and jump to the globally optimal peak (or at least the highest peak found by people in that group). Experimental results supported this argument, with participants in groups outperforming individual controls when the group participants were permitted to copy each other’s point designs. Computer simulations confirmed that this social-learning strategy of “copy-the-successful” was more adaptive than a number of other social-learning strategies, especially in larger groups of more than fifty people, which have been typical throughout much of human evolution (Dunbar 1995). Simulations also showed that the assumption of a multimodal adaptive landscape was key to this advantage.

This latter finding is potentially important to the production of innovation, as it demonstrates that the nature of the selective environment will significantly affect aspects of cultural transmission. Whereas previous experiments (e.g., Kameda and Nakanishi 2002, 2003; McElreath et al. 2005) have used relatively simple learning tasks requiring a participant to select one of two options (e.g., crops or rabbit locations), Mesoudi and I used a more complex learning task involving multiple continuous and discrete, functional and neutral attributes, some of which have bimodal fitness functions. The resulting multimodal adaptive landscape was instrumental in generating and maintaining diversity in the virtual-point designs.

We also found that the “copy-the-successful” strategy outperformed the “copy-the-majority” strategy. Indeed, the latter performed no better than individual learning because individuals are just as likely to converge on a local optimum as on a global optimum in the absence of information regarding the success of those individuals (unless individuals at the global optimum outcompete individuals at the local optima and become the majority). This finding contrasts with previous models that suggest that conformist transmission is adaptive under a wide range of conditions (Henrich and Boyd 1998), possibly because those models assume that individuals exhibit only one of two behaviors, one of which has a higher payoff.

How realistic is it to assume the presence of a
multimodal adaptive landscape? Boyd and Richerson (1992) argued that multimodal adaptive landscapes are likely to be common in cultural evolution and may significantly affect the historical trajectories of artifact lineages, just as population-genetic models suggest that multimodal adaptive landscapes have been important in biological evolution by guiding historical trajectories of biological lineages (Arnold et al. 2001; Lande 1986; Simpson 1944). Many problems and tasks faced by modern and prehistoric people would have had more than one solution, some better than others, but all better than nothing, and solutions are likely to represent compromises between multiple functions and requirements.

With respect to projectile-point innovation, Cheshier and Kelly (2006) recently summarized experimental evidence for tradeoffs in projectile-point designs among such factors as accuracy, range, killing power, and durability, stating, for example, that “thin, narrow points have greater penetrating power, but wide, thick points create a larger wound that bleeds more easily” (Cheshier and Kelly 2006:353). Such functional tradeoffs would potentially produce multiple locally optimal point designs, with, for example, one optimal design maximizing penetrating power and another maximizing bleeding. Similar studies of tradeoffs in projectile-point design have examined fletching (Hughes 1998), beveling (O’Brien and Wood 1998), and hafting (Beck 1995; Frison and Todd 1986; Howard 1995; Musil 1988).

Understanding design tradeoffs at the micro-level is important, but so too is not losing sight of the fact that it is the intersection of many character states that creates the overall object of interest, the projectile point. At an even more general level is the composite weapon-delivery system, whether a spear and spear thrower or a bow and arrow. Each represents an alternative solution to the same problem (firing projectiles), with the bow and arrow apparently more effective, given that it replaced the spear thrower in most regions (Knecht 1997). This change is likely to have been the result of horizontal cultural transmission, with a jump from a lower peak (spear thrower) to a higher peak (bow and arrow) in the available design space of projectile technology.

**Tempo and Mode**

What about the tempo of the jumps? The ethnological and archaeological records are replete with evidence that the tempo of cultural change is rarely constant, though there are few cases in which it has been measured directly (but see Henrich 2004). Again, how are scale and tempo correlated? Is the apparent rapid emergence of a new form actually sudden or is it an illusion, meaning that the scale at which we are examining something makes it appear as if the object is new when in actuality it is the product of myriad small-scale cumulative modifications that took place over a relatively long period of time?

This same question was asked in paleontology for decades. Darwin’s notion of the evolution of species was based on gradualism—the slow build up of small-scale change over geological time—although his theory did not require that tempo. Simpson (1944) opened the door on the notion of accelerated tempo, and Eldredge and Gould (1972; Gould and Eldredge 1977) opened it wider with their concept punctuated equilibrium. They argued that cladogenesis—the division of a taxon into itself and at least one sister taxon—is the general mode under which evolution operates (as opposed to anagenesis, or the evolution of one taxon into another) and that rapid cladogenesis is orders of magnitude more important than gradualism as a tempo of speciation.

Palaeobiologists have erroneously used punctuated equilibrium to model evolution’s temporal component, despite warnings from Gould and Eldredge that the model is “a specific claim about speciation and its deployment in geological time; it should not be used as a synonym for any theory of rapid evolutionary change at any scale” (Gould 1982:84). They issued such warnings to emphasize the cladogenetic aspect of the punctuation-equilibrium model, thus trying to ensure that it was not confused with saltationism—the belief that evolution depends on the appearance of macromutations that exhibit significant disjunctions with their parents. Rapid anagenetic change—saltation—is the antithesis of cladogenesis, or the diversification of one lineage or species into several.

Figure 1 illustrates the difference between punctuated equilibrium and gradualism. Phyletic gradualism views evolution as continuous, as modeled in the phylogeny on the left of Figure 1, in which the rate of ticking of time’s clock does not vary as one reads up the graph vertically (morphological variation is graphed...
horizontally in uniform units). Punctuated equilibrium is modeled on the right. Notice that in the punctuation diagram some branches emerge from preceding stems quickly with minimal morphological variation and then rapidly become morphologically stable over relatively long spans of time.

Figure 2 illustrates the difference between anagenesis and cladogenesis, without reference to tempo. In fact, assumed change is slow and continuous. In the anagenetic model (top), taxon 134698—maybe a projectile-point class—is the ancestral taxon at time t1. By time t2, the state of character 4 has changed from 6 to 7, and by time t3, the state of character 3 has changed from 4 to 3. The states of characters 1, 2, 5, and 6 never change throughout the sequence. In the cladogenetic model (bottom), taxon 134698 is still the ancestral taxon, but instead of evolving directly into taxon 134798, it splits into two lines—itself, which we can ignore, and another one, 134798. After a time, that line splits, with one leading to taxon 124798 and the other leading off in a different direction in morphospace. After a time the latter line splits, with a line leading to group 133798.

Both models can be found in the organic world (Barnosky 1987) and, I suspect, in the cultural world as well. However, more so than in the organic world, the tempo of innovation in the cultural world can change not only dramatically but quickly. Instead of a rather slow, deliberate rate of innovation, what we might have is cladogenetic change that is so rapid that it produces dramatic, sudden changes in numerous character states simultaneously, which, when viewed over time, appear as packages of new forms. This is the key component of punctuated equilibrium. It is not that brand new forms appear instantaneously but rather that (a) the tempo of change increases and (b) our temporal resolution is such that we can't sort out all the changes in form between time t1 and t2.

As yet we do not have the requisite data to examine in detail the role punctuated equilibrium might play in human evolution, but Lyman and I (O'Brien and Lyman 2000; O'Brien 2005) outlined one example that I think bears closer examination. That example is what was going in North America after roughly 9250 bc with respect to weapon-delivery systems. Specifically, we were interested in the relationship between Folsom and Clovis points in the Southwest and Plains. Ever since the distinction was made in the 1930s between Clovis and Folsom points, archaeologists have proposed that the latter evolved out of the former. This proposition is based on formal similarities between the two point types (Figure 3): slightly later radiocarbon dates for Folsom points (ca. 8950–8500
A comparison of Clovis and Folsom points, and the stratigraphic position of Folsom points relative to Clovis points at Blackwater Draw, New Mexico, the Clovis type site (Cotter 1937).

Because two things are superposed and/or resemble each other does not imply they are connected in a hereditary sense, although this assumption is embedded in American archaeology. Maybe, though, what we are witnessing in the western United States is a case of nonrelated replacement of Clovis points by Folsom points. Bradley and Frison (1996) stated that in their opinion Folsom points evolved not out of Clovis points but out of a point type—Goshen (Figure 3)—that was a contemporary of Clovis. Of considerable interest is a statement by Taylor et al. (1996:523) that the “latest North American Clovis occupation predates the earliest occurrence of Folsom.” In other words, it does not appear that Clovis points and Folsom points overlapped, which one would expect to occur if one evolved out of the other. Unless sampling error is clouding the picture considerably, Bradley and Frison are correct: There was no natural evolution of Clovis into Folsom points. Rather, Clovis split, giving rise to Meserve/Dalton points (Bradley 1997), which spread rapidly over much of the American Midwest and East.

**Discussion**

Tempo and mode are only two of the myriad issues that have as yet been inadequately addressed with respect to the origin and spread of cultural innovation, yet they offer exciting entry points into the discussion (Eerkens and Lipo 2007; O’Brien and Lyman 2000). Certainly they
were central to much of the work undertaken in the field of palaeobiology, which emerged in the 1940s, but they have been less so in anthropology and archaeology. Whether one views punctuated equilibrium as a particularly useful model in understanding the origin and spread of innovation, there should be no denying that it calls attention to the inextricable linkage between tempo and mode. It also highlights the issue of scale and the visibility threshold of innovations. In other words, how visible does something have to be to count as an innovation? Is every tiny change that we identify an innovation?

With respect to scale, we can return to Pocklington’s (2006) question: What is a culturally transmitted unit and why should we care? He points out that many of the criticisms leveled against the study of culture traits (memes) are similar to that made by Atran (2001:356), who claimed that “there [is] no ready way of deciding what counts as a meme. There [is] no set of criteria for determining whether or not the chosen units or ‘chunks’ of information actually cut up culture at its natural joints.” Pocklington (2006:20) agrees: “In many cases, the abstraction of ethnographic or archaeological data into putative culturally transmitted units has been done with little reflection. The assumption that anything that one observes as data in a cultural system is itself reflective of a single unit of cultural transmission that has a distinct history and mode of transmission is a large leap of faith.”

Figure 3. Suspected phylogeny of four projectile-point taxa from the western United States (from O’Brien 2005). Note that the ancestral histories of Goshen and Clovis are unknown.
To by-pass the problem, Pocklington and Best (1997) argued that the appropriate units of selection to use in modeling cultural adaptation will be the largest units that reliably and repeatedly withstand transmission. Why the largest unit? There are two reasons. First, the smallest units we can discern are less likely to be able to accumulate adaptations, in that they have insufficient subcomponents that can vary and thus come under selective control. Second, when multiple units that we can identify follow parallel transmission pathways over long periods of time, they are placed in a situation in which their replication does not leave room for any conflict of interest among the subcomponents (Bull 1994). In Pocklington’s view, the parallel transmission of once-unrelated components is a force that has led to the cooperation behind the generation of larger-level structures such as social groups. When multiple replicators are placed in a situation in which their fate is common, there are benefits to interacting in a synergistic fashion. From an evolutionary perspective, parallel transmission is the force that initiates the process by which multiple isolated elements begin to cooperate with one another and create larger-scale structural integrity. And it is these larger structures in which we have the greatest interest because they are the units that evolve.

Conclusion
The history of the concept of culture trait makes it clear that anthropology has long had an interest in identifying units of cultural transmission and using them to examine the various modes that humans have evolved to transmit information among themselves. That history also reveals not only the roots of modern theoretical difficulties with identifying units of cultural transmission but also some of the properties that such a unit needs to have if it is to be analytically useful to theories of cultural evolution. Given the exponential growth in the literature on both the units of transmission and the processes through which information is transmitted and received, the next decade should witness substantial progress in our understanding of cultural innovation in all its various guises. On a broader plain, evolutionary anthropology has made great strides in developing a body of theory that complements biological evolutionary theory as opposed to borrowing it wholesale and hoping that it contains something of value. There is every reason to suspect that this trend will continue. There is also every reason to suspect that archaeology will be a major contributor.

In summarizing the history of the study of cultural innovation and transmission, I have been forced to mention only briefly what I consider to be one of the most profitable areas for any evolutionary study of innovation, namely, the emergence of rather large-scale novel phenomena that cannot profitably be reduced to a series of subcomponents. What might loosely be referred to as “emergence theory” has made its way into biology (e.g., Holland 1998; Müller and Wagner 1991; Reid 2007) and philosophy (e.g., Wimsatt 1997), although there clearly is a lack of consensus over what emergence entails. In anthropology and archaeology, emergentism has tended to be anchored in a vitalistic, orthogenetic reliance on progress as an explanation of cultural evolution. Put simply, what man needs, man gets in order to make his way up the ladder of cultural complexity. I started this essay by referring to the classical evolutionists Tylor and Morgan, both of whom would have embraced emergentism as a complementary piece to their theories on evolution. But emergentism as I am using the term here has nothing to do with progress, nor is it simply another term for a theory of hierarchical arrangements, whereby innovations can be scaled into components, subcomponents, sub-subcomponents, and the like. Rather, it signifies irreducibility. Whatever it is that is irreducible might not be on the grand scale of Lovejoy’s (1927) “new events,” such as the sudden appearance of eukaryotic organisms, but neither is it at the atomistic level of a subtle change in the angle of a notch on an arrowhead. One method that would appear to be valuable in this regard is the production of clade-diversity diagrams, which have long been used in biology and palaeontology to show the frequency of lower-level taxa within a particular higher-level taxon (e.g., Raup et al. 1973). Generically, clade-diversity diagrams illustrate richness of variants within a series of lineages that are related hereditarily, including artifact lineages. Recent uses of clade-diversity diagrams in cultural studies have shown (a) that the history of increasing diversity in projectile points from the Great Basin is the result of changes in weapon-delivery systems (Lyman and O’Brien 2000);
(b) that the history of increasing diversity in ceramic styles in the Lower Mississippi Valley is the result of changing patterns of social interaction and transmission (Lyman and O’Brien 2000); and (c) that there was an early diversification followed by extinction and relative stasis in bicycle forms between 1800 and 2000 (Lake and Venti 2008). It is such periods of diversification—those sudden spurts in innovation—that are at once marvelously complex and yet so interesting. I don’t think those of us interested in cultural evolution have fully appreciated that complexity nor paid more than analytical lip service to its importance in the evolutionary process. Hopefully that will change.

Acknowledgements

Many of the ideas and points of view presented here couldn’t have come about if not for my collaborations with Lee Lyman and Alex Mesoudi, whose keen insights on cultural transmission have greatly influenced my thinking.

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Commentary
The Late Lower Palaeolithic in Southern Germany.
By Hansjürgen Müller-Beck

Abstract
The main features of this author's dissertation, completed in 1955, are compared here with the current state of scholarship. A few of the conclusions reached originally—and those following up in 1966—turn out to have remained astonishingly up-to-date. At the same time, it is now possible to provide some supplementary discussion which, though touched upon back then, was not developed in detail.

Bearing the same title as this article (Das Obere Altpaläolithikum in Süddeutschland—Ein Versuch zur ältesten Geschichte des Menschen), the full text of my “Inaugural Dissertation for the Purpose of Achieving the Doctorate” from an “Honorable College of Philosophy at the Eberhard Karl University at Tübingen”, was printed by the Hamburg printing and publishing house of Auerdruck—which also produced the newsmagazine Der Spiegel (Müller-Beck 1956). Publication was at that time under the name Hansjürgen Müller1. The catalogue was published on microfilm. The oral examiners were Professors Dr. G. Riek, Dr. W. Kimmig, and Musicology Professor Dr. W. Gerstenberg, the Dean; the oral examination was held on June 30, 1955. Areas of secondary concentration for the orals were geology and ethnology.

At the graduation party on the evening of the examination in the former Restaurant Spitzberg, of Hohentübingen, Gustav Riek—generally so reticent with respect to more general questions—gave an unusual speech of great emotional depth on the future role of our increasingly world-encompassing field, which he called “Diluvial Prehistory” (Diluviale Urgeschichte). In my passport, the required line for the profession listed the somewhat altered term “Pleistocene Archaeologist.” I could not have foreseen that it was this mixture of “oldest archaeology and youngest geology” which attracted attention during all border-crossing checks and, once explained in some detail, was to ease travel considerably, especially behind the Iron Curtain.

Half a century later, this dissertation provides an opportunity for a few comments and reflections intended to gather together some far-flung remarks made over a span of decades. The problems, which remain gratifyingly current, pertain to the transition to Homo sapiens sapiens in southern Germany which is still very scantily represented in terms of skeletal remains (these are, however, overly emphasized). The Mount Carmel studies of the late 1930s show this transition to be clearly coeval with the Eurasian Neanderthals. From the start, this topic has engendered ever new speculative theories (e.g., Rust 1942).

The discussions following such speculation always attract widespread interest. But the interpretive possibilities of the much more numerous, and synchronous, stone implements manufactured at the time as immediate primary products of human action continue to receive far less attention. Nevertheless, during the last decade all of the media (including finally, Der Spiegel) have at last acknowledged that Neanderthals—in addition to other still neglected synchronous variants of the genus Homo in Africa and East Asia—were definitely already very accomplished “other” humans, no matter whether taxonomically classified as Homo sapiens, merely as Homo neanderthalensis, or even just as Homo primigenius. The last is the oldest nomenclature, thus holding the greatest “validity”, and may actually be seen as a highly honorific name. In fact, it might even be advisable to revive this old term 150 years after the eponymous remains were discovered. From the standpoint of universal history, this would not harm my tendency to upgrade the Neanderthals as a sub-species of “modern” humans. They would then stand opposite “us”—whatever that may mean (Ickerodt 2005; Porr 2005)—ranking as a species still capable of interbreeding, judging by the indubitable testimony of identical basic cultural behavior based on the stone artefacts.

In terms of universal history, this frequently disproportionate anthropo-Darwinist “human discussion” is of rather secondary significance. After all, the stone implements speak for themselves, and it is of limited interest what their manufacturers actually looked like, unless there is a desire to cling to the outmoded ideas of fun-
damental qualitative differences. The latter could only be deduced from correlatable archaeological data, never in the past nor in the future from the skull configuration of one individual who might have been responsible for the manufacture of these implements. There is even the unanswered question—as now on Flores or in the case of African Zinjanthropus—whether the discovered individuals were really the manufacturers of the archaeologically assessable finds, although that changes neither the objects nor the story they tell. I therefore ask the reader to forgive my continued failure to be greatly interested in the physical appearance of the people who stand for the Late Lower Palaeolithic.

Beyond the borders of southern Germany, judging by archaeological classifications, the era’s inventories—varying over time and space—surely are accompanied not only by Homo sapiens neanderthalensis (or Homo primigenius) individuals, but also by representatives of late Homo erectus (when Middle Pleistocene Bilzingsleben is included) and, indeed, of early Homo sapiens sapiens in the same strata. Nevertheless, no anthropologist has yet been able to present a Neanderthal found in the same horizon with a “Post-Neanderthal”. This plainly means that they are definitely populations of different morphology, though culturally connected along a comprehensive but regionally quite variable continuum. More detailed assessments will have to await the presentation of new discoveries (Soressi 2004; Stewart 2005; Straus 2005a, b).

Following the custom in archaeology, and the sole determinant for prehistoric interpretation, any such assessment will depend on the discovery context. Anything else remains speculative, especially when also weighted down by attempts at correlation based on statistically uncritical evaluations and complex laboratory techniques. While such attempts may be “interesting”, as is being asserted in the more intellectual press (Lüthi 2006), they can by no means evade archaeological facts. Moreover, in the case of the Late Lower Palaeolithic in southern Germany down to the more recent discoveries at Stuttgart-Cannstatt-Bunker (Wagner 1995; Schatz 2003), i.e., in the full Holstein Interglacial, we actually have a time span of well over 400,000 years—and hence a family tree of more than 40 x 10,000 years with at least 1600 participating generations, provided all partners remained faithful to one another throughout their procreative time and there were no losses due to the woman’s death in childbirth. Assuming the analogy of current and historic hunters’ populations, there were more probably two or three times as many encounters of couples, thus roughly a magnitude of 3000 to 5000, contacts achievable—subject to the rule established by Luther.

In spite of more recent field research, skeletal remains categorized as Neanderthals have remained a rarity in southern Germany. In addition to a confirmed atavistic femur (Hohlenstein-Stadel in the Lone Valley) and a baby tooth which has now vanished (Klausennische near Essing), both of which were known in 1955, there are: two baby teeth and remains of an evidently buried fetus from the Sesselfels Cave (Rathgeber 2006) and a single tooth from the cave at Hunas (Alt et al. 2006). All of these appear in a stratigraphically indubitable context with stone implements of the Late Lower Palaeolithic (in which I expressly included the emergent “Middle Palaeolithic” in 1955, giving detailed reasons). Such a context has not been established for the disarticulated skeleton imbedded in red chalk found in the Mittlere Klaue near Essing, which I recorded. However, it cannot be definitely ruled out without a major series of new datings. More on this later.

The first comment that I would like to make about the dissertation itself regards its title: It retained the then commonly used term “Alt-paläolithikum” (Lower Palaeolithic), while also, for chronostratigraphic reasons, specifying a “Late Lower Palaeolithic”, although the term “Middle Palaeolithic” in a narrower sense had already been introduced by F. Bordes. Even today, there is no unanimity on the question where such a boundary should be placed. Bordes (1954) assigned it geologically to the lower boundary of what was then defined as the Riss-Würm Interglacial after A. Penck. At least this is now accepted internationally as the lower boundary of the Upper Pleistocene in biostratigraphic terms—reduced to the Eem Forest Period and the beginning of Oxygen Isotope Stage (OIS) 5. In contrast with Bordes’ categorization, arguments of artefact morphology are now frequently used to place it in the upper zones of the Middle Pleistocene. In continental sequencing, this frequently does not permit
assignment of find localities to the phases of the Riss or Saale “Ice Age Complexes” and to the preceding “Great Interglacial” (now called Holstein Complex) in a generally reliable, uncomplicated, globally comprehensive, and chronostratigraphically certain manner. Therefore, the term “Oberes Altupaläolithikum—Late Lower Palaeolithic” might still be very viable, being also more open-ended than the denomination “Middle Palaeolithic” with its only vaguely tangible foundation. It was completely clear to me in my research that a dynamically clearer boundary line, such as the subsequent, fully evolved classical Upper Palaeolithic, would have to await better evaluation of its beginnings.

My second direct critical comment regards the expression “of Man” in the subtitle. Regrettably, it represents a linguistic focus upon a too-narrowly-conceived unifying concept not adequately reflected by me at the time; today, we neither would, nor should, so view the genus *Homo* with its initially so clear temporal and regional specifications—even in terms of cultural history. Above all, such a unifying concept prejudices the discussion about the contemporaneity and succession of human types. It was chiefly M. Bloch—whose thoughts were all too soon interrupted by the brutality of the German occupiers and whose reburial in the Pantheon of Paris is probably imminent—who elucidated for all time that we, the palaeohistorians, should always speak of “humans” in their multiplicity. It is only the latter usage that gives cultural space to each individual helping to shape history, and thus his true uniqueness and historic freedom.

Nevertheless, the very brief original preface provides a better introduction to the structure of the dissertation than I had remembered. I was really concerned with all available sources of palaeohistoric significance regarding my topic—in spite of the concomitant firm restriction to the Lower Palaeolithic excavation horizons in the more immediate South German Basin, not including the administrative district of Karlsruhe. This fact also indicates the orientation of archaeological work in those days towards the structures of preservation of antiquities, which controlled access to funding then and is still accountable for it now. Because of the absence of regulations, these various exca-

vated objects had, in the immediate postwar chaos, been at the “private” disposal of the excavators, who had by no means settled their 12-year-long dispute with the unbending life-long proponents of a “Greater Germanic Era”—whose very real, and, until recently well-concealed threat was only exposed by historical research in the last decade. Implicated as well is the fact that the historical preservation agencies in the German states structured the entire organization of prehistory and early history, more rarely protohistory, as a continuation of an old buddy system, since the latter controlled the major part of all available public funding for the profession. In my case, the catalogue could better have been printed in two sections—one in Baden-Württemberg and one in Bavaria—but it also contained—for good reasons that were amply explained—too few illustrations for the expectations of that time.

What had originally triggered my dissertation plan was my intense interest in Oswald Menghin’s 1931 “Palaeolithic World History”. Before my first year at Heidelberg University in 1950 I was asked by a doctoral candidate at Marburg University, H. Mandera, to excerpt that work in order to aid his preparation for his oral doctoral examination to be conducted by G. von Merhart. The latter regretted throughout his life—as he once told me on a field trip to Veringenstadt—that he was never able to do palaeolithic field research after 1914. Added to that was my frequent contact in Ahrensburg after 1952 with A. Rust, the excavator of Jäbrud, during his research in Mauer. Of particular importance, as well, was my inventory, compiled while I was research assistant to H.-G. Bandi in Bern, of the material from the Mount Carmel excavation (cf. Garrod and Bate 1937), which had been given to the Bern Historical Museum by D.E. Garrod (Müller-Beck 1955). This was a work which had to be done in the form of a card catalogue. After some corrections, it was then incorporated into the official inventory ledger of the BHM (Inv. No. BHM 35501-36422) by the person who later became my wife.

Obviously, it then made good sense for me to plan a spatially extensive inventory of material in order to make a scholarly contribution to the earliest prehistory of our own region. Source materials in southern Germany were adequate
to the purpose, and G. Riek—ideally combining expertise in geology, palaeontology, and archaeology—was willing to advise me most liberally in my undertaking. It is true enough that he would occasionally exclaim “Müller, you damned lawyer,” because of my admittedly rather mathematical eagerness to define things, which seemed to me a matter of urgent necessity. Yet, his critical remarks were most helpful in creating definitions. Moreover, he took it for granted that, in addition to artefacts, I was to include any other available objects and conclusions. The roots of this method of operation lay in my basic studies in Heidelberg from 1950 to 1952, my professors there having been palaeontologist L. Rüger, prehistorian E. Wahle, and cultural sociologist A. Rüstow.

In Tübingen, I was fortunate in my contact with H. Graul, the very active geomorphologist (who was simultaneously a farmer implementing modern techniques). Graul would go about southern Swabia (Oberschwaben) on his little motorbike mapping and interpreting the ice advances and terraces with the utmost care; and he would confront his usually rather small number of students with the newest problems of quaternary classification. In the years following my dissertation, in many a discussion, along with K. Brunnacker, whom I first met while taking inventories of materials in Bavaria, Graul would encourage my wider attempts at stratification.

At the time, I inventoried 33 sites in Baden-Wuerttemberg and 35 in Bavaria. They ranged from single objects to stone inventories exceeding 1000 items. I eliminated six sites in Baden-Wuerttemberg and 16 in Bavaria which turned out to be of uncertain Lower Palaeolithic origin (for consistency, I choose not to use the term “Middle Palaeolithic” in these comments). My own recording of archaeological inventories required two steps. The first was an initial general inspection of all discovery horizons accessible to me, during which a heavy motorcycle was of invaluable assistance. Upon development of the system of categorization—based primarily on our collection in Tübingen with its significant sites—there followed stage two, the inventory of all objects considered in the study, i.e., approximately 9000 (not including the occasional flakes of type 46, subsequently more appropriately characterized as category 46). Only about six months were left to do the latter. The procedure proved to be amazingly efficient, not least of all owing to preceding work published by a variety of authors, and most especially to direct contacts with A. Rust, H.-G. Bandi, G. Riek, and F. Bordes, all of whom were interested in these regions. At the recommendation of F. Zeuner, Bordes made a special trip to Tübingen from Paris in order to explain to me his typological system (Bordes 1953), only outlines of which had been published at that time. As a result of this contact with southern Germany, he included in his Mousterian schema the leaf-shaped points defined by A. Bohmers (1951). What I still needed to do, however, was to determine acceptably certain boundaries for distinctive features, metrically defined whenever possible, between the nominal categories (“types”). This was basically a spherical-geometric system, as it might more graphically be called (Müller-Beck 1983a), and it has been developed in a particularly complex fashion for categorizing the numerous variants of hand axes (by López Juquera 1980 in great detail, for example). My purpose was to ensure an equal approach to all of the collections, including the flakes of 1 cm maximum size (type 46) that eluded further characterization but were nevertheless distinct from fragments (type 45) by the presence of recognizable traces of flaking. These were therefore abstract categories which could often not even be clearly classified on the basis of subsequent explanatory illustrations by F. Bordes (1961) or G. Bosinski (1967).

As a result of this abstraction, selective illustrations of major series seemed to me to make little sense. Therefore—with perhaps an excess of consistency—I only documented each type with one original drawing. One thing was certain, as in all biological categorization, hardly any item among the Lower Palaeolithic artefacts was identical in all regards with any other. This was bound to be the case because of the dependence on random shapes arising from the modification of basic shapes—in contrast to the Upper Palaeolithic blade industries. This, indeed, is one of the most important principles distinguishing the Lower Palaeolithic over its hundreds of thousands of years from the entire global Upper Palaeolithic, which encompassed a mere “short” forty thousand years.
This resulted in relatively clear categories, such as the important “atypical scrapers” or the frequent “directional flakes,” in addition to the much more rarely found genuine “blades”. Moreover, the frequency of collected flakes definitely indicated the care taken during each excavation. F. Bordes (1953) included in his system none but all obviously retouched artefacts (i.e., those definitely showing secondary modification). In the rich Murg site (Rogg/Michel brickyard), his system allows only 222 artefacts to be included, omitting the used and unretouched flakes (type 31:156, type 32:289) and fragments (type 44:64) included in my inventory which make up over half of the overall number of items and are of definite importance. The same is true for the 267 flakes (type 46) which clearly reveal the careful fieldwork of all involved, especially Emil and Egon Gersbach as well as G. Kraft, and hence the great value of the site complex.

As the catalogue text reveals, I was in no doubt whatever that I was simply dealing with combinations of morphological-geometrical features—including “scraper edges”, “scrapers” or “denticulates” as well as “retouched flakes”; taken as collective terms, they definitely did not rule out possible “cryo-retouching”—then, as even now, not always clearly distinguishable. It seems that my system—more complex, consciously descriptive, and not intended to be functional, in spite of the terminology which was then, and is today, in common use—was too complex for its day and was not widely accepted. An added factor was that it could not be evaluated with edge-punched cards, as I found out when revising the system for publication about 1965. Meanwhile, thanks to the rapid development of information processing, the situation has changed. It is now quite conceivable that my old approach, due to the morphometric differentiation which it provides, may yet prove to be quite fruitful for the current evaluation of the archaic-tradition stone implement inventories just excavated on Cuba in 2006.

For the time being, then, G. Riek has been proven right: Nobody wants to know that much detail—even though there has been growing awareness that, indeed, these are definitions of shape, only partially determined by function, which reveal the scope of their ever changing application when examined in detail. The hafted items found in the Swiss moist-soil Neolithic and the Arctic Palaeo-Eskimo cultures taught me years ago that it would be better to arrange my system into classes according to production steps, as is now increasingly the case. This would also reveal more clearly that, in addition to the three-dimensionally designed “bifaces” (hand axe variants and leaf shapes), the entire “Late Lower Palaeolithic” abounds in geometrically definable, post-adapted “potential” tool edges occurring in variable but carefully selected flaked shapes and diverging in their modification according to the duration and intensity of their use. This becomes particularly evident during analysis of three-dimensionally designed implements, such as the hand axes, as already performed by G. Albrecht (1994) on the Sehremuz inventory. However, such analysis has not attracted much attention so far.

For each major inventory, the catalogue illustrated the distribution of typological frequencies by means of simple block diagrams by percentage ratio, each broken down into three separate illustrations: a) Overall distribution of ratios by type; b) Overall ratio of tools, flakes, and cores; c) Distribution of tool frequency.

Comparison of these major inventories resulted in a type-ratio-based distinction between four morphological groups which, thanks to observations of collections of Vogelherd (BW 27), Weinberg Caves (B 22), and Sirgenstein (BW 33) lent themselves to relative stratigraphic sequencing: I. Late Lower Palaeolithic with flat cores; II. Late Lower Palaeolithic with hand axe scrapers, clearly subdivided into broad-backed hand axe scrapers, and thin-backed hand axe scrapers and hand axes with thin top edges; III. Late Lower Palaeolithic with leaf-shaped points, tentatively subdivided into leaf-shaped points with variable cross-section, leaf-shaped points with only D-shaped cross-section, and large blades; IV. Late Lower Palaeolithic with scrapers and an increasing ratio of blades.

Above all, what thus became clear beyond a doubt was the tendency for bifacially shaped tools to become thinner, the periodic increase in hand axe scrapers and, along with the disappearance of the late hand axes, the increasing frequency of leaf-shaped points and elongated heavy blades. None of the inventories include...
cores with oval platforms so typical of the Aurignacian. Cores are limited to those with broad surfaces from which blades were removed and a narrow prepared platform on flat cores.

The only firmly identified bone implement found was a point in horizon VI of Vogelherd (BW 27), the stone implements of which still belong in a very late Lower Palaeolithic period. A recent direct dating of this point resulted in an uncalibrated $^{14}C$ age of merely 31,310±240/-230 years (Bolus and Conard 2006). It is therefore more likely to be from the Aurignacian of stratum V in the overburden and had either intruded into horizon VI or had been incorrectly assigned to the stratum during the rapid excavation. Other bone items identified as artefacts are a bone fragment from Sigenstein (BW 33), definitely retouched along one longitudinal edge, and a chip off a horse’s nose that was probably used from stratum VII of Vogelherd (BW 27). It is likely that there were other intentionally modified bone implements or used “auxiliary bone implements” without pronounced preparatory modification, as described by H. Obermaier, which were overlooked. D. and U. Mania (1997) have since shown how great the inventory of such briefly used and often only slightly altered bone implements really is.

For 35 of the sites, the remains of fauna found there had also been classified and published. There was generally little differentiation among them, and it therefore did not permit the definite stratigraphic classification I had hoped for. The represented species ranged from forest elephant (*Elephas antiquus*), deer, and moose to musk oxen and saiga antelope. There were frequent occurrences of mammoth and rhinoceros (*Kahlke 1994*) including the occasionally confirmed snow hare and ice fox. Reindeer occur in 21 sites, frequently in the company of red deer but occasionally found alone. The reindeer’s skin was probably an ideal material even then for winter clothing and tents or as a cover for huts. The red deer is an indicator of more or less extensive forest cover. Relatively large horses are also a frequent occurrence, as is the cave bear, which is found in practically any cave inventory. Cave hyena and Felides are far more rarely encountered.

Beyond a doubt, these discoveries prove that the people who represent the South German Late Lower Palaeolithic hunted a broad spectrum of fauna which varied during the Middle and Upper Pleistocene in the course of climate development. Interglacial fauna can only occasionally be recognized, especially because of the rarity of sedimentation during these phases—almost exclusively travertine; only in horizon IX of Vogelherd (BW 27) was the discovery of a forest elephant’s baby molar accompanied by basal cave debris. The vast majority of hunted fauna is part of the “mammoth/woolly rhinoceros complex” (*Kahlke 1994*), which represents the cooler and cold steppe phases of the Middle and Upper Pleistocene in more northerly Eurasia with its greater sedimentation dynamic. On occasion, elements of more open forest zones increase in frequency, with aurochs, giant stag, and even deer occurring. The steppe zones of grass and herbs are evidenced by horses of the type *Equus* *germanicus* and, later, by *Equus f. przewalskii* in Haldenstein Cave (BW 30). The saiga antelope, which is another quarry, likewise indicates the advance of dry-cool steppes toward the west. The reindeer occurs with particular frequency and is likely to have been a principal prey species, which proves with great certainty that the representatives of the South German Late Lower Palaeolithic were quite capable of surviving in subarctic and arctic conditions. The association of their artefacts with unquestionable examples of musk oxen (skulls) in the Danube Valley near Regensburg (B 31) indicates this even more clearly. Caves used by cave bears are by no means avoided by humans, while artefacts are less frequently found in caves also used by hyenas.

Meanwhile, reevaluation of fauna found in association with Late Lower Palaeolithic inventories in northern Germany under relatively good taphonomic conditions—including evidence of systematically placed slash marks—has furnished unquestionable proof of definitely planned hunting. At the Salzgitter-Lebenstedt tundra site—in the older Würm/Weichsel—reindeer were the principal prey, as they also were very frequently in southern Germany (*Gaudzinski 1998*). In Taubach, a forest site dating from the older Eem or possibly from a preceding warm phase, animals hunted and, most likely, intensively used, were: wood rhinoceros, brown bear, bison/aurochs, stag, and beaver (*B. Bratlund 1999*).
While plant remains are found far more rarely in the context of stone implements in southern Germany—in only five Late Lower Palaeolithic sites—they do confirm the picture of ecosystems gained from the faunal remains. In Murg (BW 15) wood remains of pine and mountain elm were found—definitely boreal forest elements. In Stuttgart-Untertürkheim/Biedermann (BW 29), the artefacts are in association with warm forest flora. In the Peters Cave (B 16) Pinus silvestris and Taxus baccata likewise indicate a still relatively warm forest steppe. The same holds true for the northern oak from the Lower Palaeolithic horizon of the Ofnet (B 17), which may well have survived all of the Middle Pleistocene and possibly even the Upper Pleistocene cold phases in the gallery forests along the south German portion of the Danube. This gallery forest is also documented in the vicinity of Weinberg Caves (B 22), thanks to the pollen profile established in the valley. There, in a relatively narrow horizon, the proportion of warmth-loving trees rises to about 5%, revealing a mixed oak forest with oak, elm, alder, and hazel. In the stratum above, the relative proportion of fir increases temporarily, while pine, a boreal element, maintains its high percentage for a relatively long time. It is only in the overlying loesses that the proportions of tree pollen decrease significantly. What I did not realize at the time is the fact that the last full interglacial is not documented there, but merely an optimal wooded phase above the Lower Würm, as we discovered during follow-up investigations there (von Koenigswald et al. 1974; Brande 1975; von Koenigswald and Müller-Beck 1975).

As mentioned above, human skeletal remains are extremely rare in the context of Late Lower Palaeolithic artefacts. Moreover, the femur found in Hohlenstein-Stadel (BW 1) is not particularly characteristic. On the other hand, the skeleton from Mittlere Klaue in Essing (B 8), unquestionably that of a Homo sapiens sapiens and manipulated with unusual thoroughness in a secondary burial (Gieseler 1953), is of some interest. According to the excavators, it was to be assigned to a sediment horizon in association with artefacts belonging to our morphological group IIb; however, at the time, the latter group was more closely associated with the Solutrean, not least of all because of the very fact that the skeleton was found there. It was expressly ruled out that the skeleton belonged to other strata also present in the cave and containing Upper Palaeolithic items. Meanwhile, a $^{14}$C dating of approximately 18,500 BP has been obtained (Terberger and Street 2002), but it should be re-checked by serial re-measuring, as it postulates a time level that has been archaeologically undocu-mented so far in southern Germany. At the time, I could not definitely rule out the possibility—and cannot do so even today—that typical specimens of Homo sapiens sapiens do already occur as early as the later phase of the Late Lower Palaeolithic. What gives me pause to this day, above all, is that this type of secondary burial is not documented anywhere for the Upper Palaeolithic and could easily be assigned to the Lower Palaeolithic. Nevertheless, I had to state then: “In spite of the above arguments, it has to be emphasized once again that it is no longer possible to make a decision regarding the provenance of the skeleton and that our ability to interpret it has been completely eliminated in all respects. We considered ourselves duty-bound to point out the possibility of such an assignment.”

In overall stratigraphic terms and in accordance with current Quaternary subdivisions, the described inventories of the Late Lower Palaeolithic in southern Germany extend over an imprecisely assessable portion of the Late Middle Pleistocene and the entire Earlier Upper Pleistocene. It remains uncertain how far back they reach. It is likely that the objects found in Stuttgart-Cannstatt-Bunker, with their location in the lower Holstein Complex, have provided clarification in this regard (Wagner 1995; Schatz 2003). Back then, before the first $^{14}$C dating became available for the middle Upper Pleistocene, I did not realize that the gradual temperature increase after the “Lower Würm”—to which the bulk of the finds under consideration belonged, and which I named “Spiezer Fluctuation” in a considerably simplifying manner—actually lasted longer. Shortly afterward, it turned out that this phase, which I then called an Aurignacian Fluctuation in accordance with W. Soergel’s model, comprised a complex and repeatedly fluctuating climate phase of over 10,000 years. We have learned since that the end of the South German Late Lower Palaeolithic does extend into this phase, which is now regarded as part of OIS 3.
As mentioned above, the region under study was limited to the South German Basin proper, as a distinct landscape excised from the wider region of southern Germany so designated by Gradmann in 1931. Our terrain is bounded by the Alps, Black Forest, Neckar hill country in the Kraichgau, Odenwald, Spessart, and Rhone; in the north by the Thüringer Wald and Frankenalb up to the Fichtelgebirge; and in the east by the Böhmerwald and Hausruck. Access was possible as climatic conditions permitted and was provided by the Upper Rhine in the southwest, by the Danube near Passau in the southeast, and—probably of lesser relevance for the era under study—the Kraichgau portion of the Neckar River in a northerly direction. The basin is divided into two nearly equal parts by the Schwäbische Alb and the Fränkische Alb. During warm periods, the dense forests would have been rather difficult for gatherers and hunters to traverse. On the other hand, along the larger rivers broad meadows and old water courses were very productive, providing ample opportunity for fishing and for gathering mollusks. By contrast, decrease in mean annual temperatures improved accessibility by thinning the woods, thus widening the available terrain for gathering and hunting as well as the variety of usable biotopes at different altitudes all the way up to the alpine valleys (Andrist et al. 1964). Only in fully developed cold periods were the level plains south of the Alb accessible—earliest and easiest from the southeast—but surely to climatically adapted groups of people only.

Considering the sources available to me at the time, I decided that the most responsible approach was to attempt to assign the inventories to temporally distinct occupations of humans in the South German core basin analogous to the four morphological groups based on the stone implements. I completely avoided definition of any “cultures”, as the term itself—proposed again and again then, as well as later on—is in itself problematic and yields little in ecological respects. However, I did not justify this decision in detail. Any archaeologist and prehistorian familiar with the way theory is shaped in our discipline can easily realize today why I recoiled from such definitions. (For example, the term “Kulturkreislehre” [the “Doctrine of Cultural Groups”] coined by the Viennese School still had currency then.) My categorization of “cultural behavior” (an open and viable term used to sum up human activity) in the South German Late Lower Palaeolithic was paired with climatological evidence, which could be evaluated by references to multi-faceted sedimentary sequences at the more thoroughly investigated sites. It was often found that the find zone under examination, or even several of them, lay at the base of a subsequent cooling period. This observation can now be readily explained both for loess and for debris and gravel sequences (Welten 1982). Greater quantities of finds are conserved in usually thicker and rather rapidly deposited colluvial wind deposits at the base of colder layers and above accumulation disconformities recognizable as palaeosols or erosive discordances. These greater quantities more frequently include archaeological horizons or their remains, as in Vogelherd (BW 27). The task at hand, was to assign each sequence from these levels of morphological groups and each pertinent assessable climatic period to the most likely South German quaternary classification for the era—a process that can be deduced from my text but which I did not explicitly discuss. I regret my decision in hindsight, especially because it unfortunately is a typical shortcoming of dissertations caused by the fact that the author knows his work in detail but often fails to make the extra effort to facilitate insight into his arguments for readers less familiar with the problems. This becomes an even more critical factor when a reader—as is commonly the case—skips through the text and therefore has a hard time following the arguments.

At the time, my sequences in the Middle Pleistocene were distributed over the phase advancing toward the Maximal Riss and the incipient fluctuation between the Maximal Riss and the Younger Riss (Graul 1952). The latter is now classified as OIS 6, 7, and earlier. In principle, it was then easier to proceed in the Upper Pleistocene, with the Riss-Würm Interglacial at the base, the still poorly understood details of Lower Würm classification (Brunnacker 1954), and the principal fluctuation (table p. 45) with its as yet unknown duration. These terms—which remain difficult for non-specialists even now—turn up in my text but, because of their merely local use and
lack of general acceptance (as, for example, in Büdel 1951) were replaced in the aforementioned table with the more open and more neutral but cumbersome system of F.E. Zeuner (1952). Later, when the first ¹⁴C data became available, comparative work beyond the boundaries of southern Germany resulted in a chronostratigraphic classification for the European and American Upper Pleistocene (see Müller-Beck 1966) which closely resembled today’s classification.

The first human occupation in the South German region seemed to have occurred near the end of the “penultimate warm summer period”, i.e., in the still elusive advance phase of the Riss/Saale Ice Age. Because of its relative proximity to Steinheim (Müller-Beck 1983b), I proposed it was possibly connected with pre-Neanders. As this occupation ended before the culmination of the glacial stage, I designed a brief description of landscape development during this protracted climatic deterioration to suggest possible escape routes. Although this model left open an escape route toward the southeast across the Danube, it seemed to allow for withdrawal in a southwesterly direction for a longer period. As a metaphor for my escape model of the roaming bands, I chose the overlapping wave graph used by Ebbe (p. 47).

I assigned the second occupation, a briefer episode, to an interstadial “of higher order” between the Riss Maximum and the Younger Riss—a climatic phase that is even now quite elusive but evidently does exist during the uppermost Middle Pleistocene prior to the Eem, possibly in the site of Neumark-Nord (Mania et al. 1990). It would, therefore, need to be integrated into the top portion of OIS 6 or be defined as OIS 5“f”.

At the time, my model (as we would say now) showed the third occupation—including morphological groups IIa and parts of IIb—occurring at the end of the Riss glaciation, at the transition point to the last full warm period, with a first advance across the upper Rhine. In addition to minor inventories, which I wanted to assign hypothetically to “individual families”, more complex sites existed which might possibly represent the activities of larger groups. Beyond these rudimentary social concepts, showing activity differences or the existence of primary and secondary camps was out of the question at that time. However, later it was possible to frame a model in the upper Danube (Müller-Beck 1988). At our excavations of the arctic Palaeo-Eskimo site of Umingmak (Müller-Beck 1977), using analogies with the ethnohistoric Neo-Eskimo sites and given the good preservation conditions and complex evaluation of fauna, the classification method proved entirely clear. At best, differences in camp types and activities are revealed by secondary discoveries within larger, well-documented sites.

I reached firmer stratigraphic ground, even for that time, in the study of the beginning of the fourth occupation in the last great warm phase, i.e., the Eem. This was through the means of pollen analysis, though it was poorly assable because of the relatively low density of finds. By comparisons to discoveries in the west and north of southern Germany, palaeoanthropologists concluded that these humans were Neanders, albeit with extremely specialized skull configuration and highly characteristic atavistic features. They were considered even more atavistic than earlier humans, which prompted me to make the following comment (p. 50): “This is a view now generally accepted. But it gives one pause regarding the value of anthropological conclusions in view of previously accepted understanding.” In my opinion, this fourth occupation resulted in continuous habitation until the transition to the last cold period of the lower Würm whose complex classification had not yet been understood. Above all, there was no concept of the massive cold OIS 4, which, however, did not necessarily result in the complete withdrawal of humans from southern Germany, as its maximum cold phase was probably rather brief.

I then correlated the fifth occupation with the protracted climatic deterioration during the as yet inadequately undifferentiated Würm advances. There were other groups of humans who entered from the east the lowland meadows which were still surrounded by forest. I did not comment on the question of whether initial small numbers of these climate-resistant eastern people had reached southern Germany earlier, i.e., during cooler steppe phases. I felt that dramatic clashes with the “native” groups were rather unlikely because of the expanding exploitability of the landscape. It remained
unclear to what degree the first groups in the fourth occupation had participated in the rise of these new developments in the east. Moreover, it remained an unanswered question to what degree these occupations, which entailed the younger leaf-shaped point inventories, also involved populations advancing from the south into the regions east of southern Germany. Today, the warmer phases of OIS 4 and OIS 3 could be claimed for this purpose. Overall, the model is therefore considerably more complex and intertwined. Due to the lack of isolation of groups in our region and the resulting contact, the humans must have experienced accelerated development of “modern” features of their skeletal morphology. This intertwining already corresponded to the migration of fauna types adapted to colder and drier exposed steppes, coming from the east as did, for example, the saiga antelope and musk oxen, during colder climate phases. In warmer phases, on the other hand, forest animals advanced, with the forest elephant (*Elephas antiquus*), for example, reaching Warsaw. I attempted to deduce the apparent greater mobility of these steppe hunters, compared to the native forest hunters who had remained in place ever since the fourth occupation, from their long acclimatization to the colder conditions of the northeast. In terms of today’s chronostratigraphic insights, the forest hunters had actually persisted through at least 60,000 years (from the Eem to the upper segment of OIS 3), as we suspected at the time. That is twice as long as the 30,000-year span of the Upper Palaeolithic. As a consequence, of course, there was a possibility that these new immigrants from the east who came during the cooler steppe periods, recently postulated by W. Weißmüller (1995) in the Sesselfels Grotto, may have been post-Neanderthals. How early they came, and whether by this time, can only be proven by future discoveries. At any rate, the artefacts themselves permit this interpretation, as well as its ecological contingencies. In no instance do they allow the overly dramatic model so often preferred now, according to which a relatively rapid migration by the new type of humans occurred. The latter also cannot necessarily be seen solely in the context of the earliest Upper Palaeolithic. To my mind, even back in 1955, the Late Lower Palaeolithic in southern Germany—assumed for this reason to have lasted longer—encompassed not only the human type of the pre-Neanderthals next to the Neanderthal, but also a “Post-Neanderthal” coming generally from the east as early as the fifth occupation, with a still poorly understood involvement of southern influences (expressly so stated to avoid “needless misunderstanding”).

The terminal phase of the Late Lower Palaeolithic extended into the major fluctuation following the Lower Würm which was indeed significantly underestimated with regard to its length and complexity. While the marine OIS 3 of the Upper Pleistocene is now well understood, it is still difficult to correlate with continent-wide findings. In my opinion, the actual end of the Lower Palaeolithic has remained obscured because of the scarcity of evidence. The breadth of the discontinuity from the well-defined Upper Palaeolithic which followed, therefore has remained uncertain—and remains so to this day.

During the Late Lower Palaeolithic, climatic change in the South German Basin resulted in a variegated, highly diverse, and historically dynamic picture—the latter thanks to the first suggestions of human migration. At the time of my research the mosaic of that era was represented by only a very few discoveries that fitted into the overall technical developments then understood worldwide. The numerous early attempts at mapping (Müller-Beck 1966, 1993) and even more recent drafts by J.K. Kozlowski (2005) cannot really do justice to this dynamism. The distributions shown there indicate only seemingly short-lived stages of development. In reality, they are gigantic summaries of data extending over tens of thousands of years, resulting in dramatic gaps such as those that arise between the maps from the beginning of and the second half of OIS 3 (Kozlowski 2005, fig. 2 and fig. 3). But, moreover, there is an almost complete lack of a critical revision of dates, even though that is now achievable and can be confirmed by at least two approaches. It is overdue as a pan-European project spanning from approximately 420,000 BP—the baseline of the “Holstein Complex” (OIS 11)—to 10,000 BP—the end of the Upper Pleistocene (OIS 2) (Müller-Beck 2005); and it ought to comprise the entire Late Lower Palaeolithic (including the “Middle Palaeolithic”, no matter
how its time span is defined) as well as the Upper Palaeolithic. Such a revision is especially important because these highly incomplete mappings ultimately and obviously still determine the discussion about spatial differentiation and the transition from Neanderthals to post-Neanderthals more than do the discovered human remains themselves.

My material inventory suggested initial signs of a rather complex spiritual world—interpretable only with the utmost restraint (p. 57). They would be even more impressive if the secondary burial from the Mittlere Klause could be shown to belong to the terminal phase of the Late Lower Palaeolithic. Additional dating revisions are urgently needed in this regard, in the context of the proven burials in the entire European and Near Eastern region between 80,000 and 20,000 BP. The same is needed for the direct dating of the South German bone tip fragment and for the accompanying faunal remains from Große Grotte (Wagner 1983) which were found in a secure Lower Palaeolithic context. Test samples taken from this context after 30,000 BP would result in significant critical questions regarding the reliability of early AMS radiocarbon dating.

The past 50 years of research have vastly increased the data from throughout central Europe. Nevertheless, many of the questions raised in 1955 still await an answer. The likelihood of the veracity of my hypothetical model has grown. Evidently, this is chiefly due to its attempt to assess the complexity of the actual historic dynamism in the core area of natural and cultural central European migration routes across more than two hundred thousand years. The Danube Corridor is definitely only one of them. This proves to be the case particularly when denser forests give way to open forest steppes, rendering even secondary routes easier to traverse. Movements during the Upper Palaeolithic clearly demonstrate this fact, compared to the later constraints on early Neolithic cultural dynamics with their growing dependence on limiting climatic factors, which are more readily explained by means of ethnohistoric analogies. Still, for our field there remain some particularly difficult questions (Chrisomalis and Trigger 2004), such as the relatively late advance in America of maize growers far into the northern zone of the hunter/gatherer cultures (Wright and Pilon 2004). They survive there in close proximity to the hunter/gatherer cultures in economic respects but under definitely changed climatic and ecological conditions that determine the evolving cultural behavior.

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Near East

May the Revolution Prosper
By Peter Bellwood


This is a welcome new book on a significant stage in the history of human affairs. Simmons offers a synthesis of the Near Eastern Neolithic using a style of writing that is commendably clear, avoiding sociocultural jargon. The coverage is mainly on the Levant and to a lesser degree Anatolia, rather than Iran and regions to the east. The text unfolds without being dogmatic or self-opinionated, often presenting more than one point of view on controversies: “Nor do I pursue a specific theoretical orientation, although my overall perspective is anthropological and processual” (p. 6). This is a work about evolutionary change, in the sense of a general progression from more simple to more complex over time, albeit with occasional moves in the reverse direction that involved important episodes of putative human impact on regional environments.

In its broad coverage this book is a worthy successor to earlier surveys by Mellaart (1975), Redman (1978), and Cauvin (2000). Simmons clearly sees the Neolithic as enshrining significant changes in human affairs: “Around 10,000 years ago, a dramatic transformation occurred in parts of the Near East that forever affected the human experience…While it is undeniable that the Neolithic was an economic and technological milestone, it also was a dramatic social and symbolic transformation” (pp. 3-4). This is in remarkable contrast to Gamble’s recent opinion (2007) that origins, revolutions, and the Neolithic all represent conceptual blots on the concept of a universal continuity from a transcendent Palaeolithic. Barker (2006), however, would perhaps approve, with his similar title, “The Agricultural Revolution in Prehistory”.

Simmons’ first task is to review past theories on why people “…took a momentous first step in exercising more control over their food, eventually culminating in the domestication of both plants and animals” (p. 10). All the usual players are brought forward, from de Candolle and Childe onwards, via Binford, Flannery, and many more, and Simmons slips in his own view that “…it was sedentism during the early Natufian that created the need for agriculture” (p. 14). I have to agree with this—after all, Simmons quotes me as one source for this view, although I fully acknowledge many predecessors. Population growth, food storage, stress, and social (feasting/brewing) models are also clearly attractive to Simmons, whereas the current crop of postprocessualist models are considered “somewhat fuzzy” (p. 19), a viewpoint with which I must also agree.

A number of specific Near Eastern models are also discussed, including one that sees instability caused by adverse Younger Dryas climatic conditions as playing a role in the development of predomesticatory cultivation during the succeeding Pre-Pottery Neolithic A (hereafter PPNA, followed by PPNB and PPNC). Simmons does not favor any single explanation for agriculture, partly because of the significance of the current “recovery revolution”, the profound flow of new data from palaeobotanical and genetic research, and direct AMS dating. New data can be somewhat devastating for any finely buffed agenda. And to it can be added the flow of data from modern deep foundation and freeway construction projects (Bellwood 2005: 134, 171); the key PPNB site of Ain Ghazal in Jordan was actually discovered during road construction (Simmons p. 194). Something new and hitherto not quite thought of turns up almost every week, so final closure of the question “why agriculture?” is going to be elusive, particularly when extended to other regions apart from just western Asia.

Simmons goes on to consider palaeoclimatic evidence from the Levant, pointing out that climate alone would have been unlikely to determine the course of human action moving into or through the Neolithic. However, determinism apart, the Early Holocene warm, wet, and markedly seasonal climate, apparently with up to twice as much precipitation as occurs in the Levant today, was clearly a significant factor behind the rapid growth of annual cereal and legume agriculture during the PPNA and earlier PPNB. Following this initial growth, increasing PPN population densities and forest clearance in fragile environments ultimately led to considerable environmental instability, wit-
nessed by colluvial slope wash layers of cobbles in many PPNB sites in the southern Levant. Simmons discusses this in the first of several short "case studies" to be presented in the book (pp. 43-45, "Neolithic cobbles layers"). In Simmons' view, admittedly qualified, "The cobbles layers thus represent concrete geoarchaeological evidence for humanly induced environmental degradation in conjunction with heavy summer precipitation. This occurred against a backdrop of deteriorating climatic conditions" (p. 45).

This result, expressed partly also through some rather ambiguous palaeoclimatic evidence for drought towards the end of the PPNB (what came first, the drought or the humans?), could help to explain the disjunction in many settlement histories from PPNB into Pottery Neolithic, bringing home to us the up and down nature of human demographic history in this fragile region (e.g., Kohler-Rollefson 1988; Rosenberg 2003). Simmons discusses this also in his chapter 7, which is focused on the later PPNB megasites in Jordan and their decline, but not total abandonment, and admits that he leans "towards the ecological degradation model as playing a principal role in the dramatic changes witnessed at the end of the PPN (p. 191).

The debate over the intensity of human impact during the PPN in the Levant is of course a very significant one for us to pay attention to today, particularly given Ruddiman's views (2005) on the heavy impact, starting in the Neolithic, of forest clearance and agriculture on atmospheric carbon dioxide and methane levels. Although there is ongoing debate here about the strength of this impact (Olofsson and Hickler in press), my own view is that the southern Levant PPNB presents to us the first documented example of human population "overshoot" in the archaeological record, albeit one that was relatively short lived, and balanced by regrowth in subsequent Pottery Neolithic cultures in the northern Levant and northern Mesopotamia. In this instance, declining productivity, increasing human populations seeking protection in fewer large settlements, and possibly drought (?), led not to permanent cultural and population demise, but to reformulation, a result perhaps aided by increasing investment in pastoralism (Rosenberg 2003). The Sumerian descendants of these early Levant populations four thousand years later in southern Mesopotamia were perhaps not so lucky.

Chapters 4 to 8 of Simmons' book are period-specific (Natufian to Pottery Neolithic), with chapter 9 devoted to Cyprus, the island that has provided one of the most exciting bodies of new information on the Near Eastern Neolithic. Simmons sees the Natufian as having sedentary or semi-sedentary settlements, animal art (with no mother goddesses, similar to Gobekli Tepe), only limited social differentiation, and an economy based mainly on gazelle hunting, wild plant harvesting (including cereals), and possibly some pre-domestication cultivation. Life appears to have been better in the Early Natufian than in the Late, owing to the incursion of Younger Dryas cold and dry conditions, and intensification of cereal exploitation during the latter could have set "the stage for subsequent domestication during the Neolithic" (p. 85).

Simmons' discussion of the PPNA presages the fairly intensive debate during the past year or so about the reality of a long phase of "pre-domestication" cultivation in the Near East (Fuller in press; Willcox et al. in press; Weiss et al. 2007; Zong et al. 2007). It seems now to be agreed that the PPNA had no morphologically domesticated plants or animals, and it is of interest here that Fuller (in press) has defined full morphological domestication in cereals as the acquisition of a non-shattering habit. Many questions arise about just how those early Neolithic populations managed to turn their wild shattering cereals into well-behaved non-shattering and synchronously-ripening domesticated ones. Simmons does not really go into this issue, but many behavioral decisions involving replanting (i.e., cultivation), and especially replanting beyond wild distributions, plus an apparent switch from green towards increasingly riper harvests, all clearly mattered. Non-shattering habits probably required selection imposed through the harvest of ripe grains, so how did Natufian/PPNA collectors balance the considerable loss on shattering, if they attempted to harvest ripe grain, against the greater difficulties of processing if they harvested only green unshattered grain? Did they harvest wild stands on successive occasions in each season, progressively collecting more and more ripe non-shattering grains and favoring the latter for replanting?

The PPNA was "a point of no return" for the Neolithic in Simmons' words (p. 116), quite distinct from the Natufian even if descendant
from it, with some very large settlements, intensive cultivation but not domestication, and a presence of substantial public architecture represented by the Jericho wall and tower and the remarkable stone-lined sunken circular structures at Jerf el-Ahmar and Gobekli Tepe. The PPNB involved more than the PPNA, not just a switch in projectile point styles and house plans (circular to rectangular), but, more importantly perhaps, the full domestication of plants and animals in the archaeological record.

Simmons presents “an exercise in speculation” (p. 124) in which he suggests that the PPNB in the Near East at any one time contained 200 modest-sized villages of 200 people each, and 15 megasites (products especially of the later PPNB and PPNC) with 3500 people each, giving a total population of 92,500, to whom one must add a small number of non-villagers and pastoralists. This, according to Simmons, is rather a low population, one that implies that many regions had only very low numbers of humans in residence. Whether one agrees with this view or not, given the obvious fact that discoveries tend to cluster where archaeologists roam, it can hardly be overlooked that two large regions of the Levant, in northern Syria (south of the Euphrates) and between central and southeastern Anatolia, have an astonishing infrequency of sites, perfectly obvious for instance on Simmons’ fig. 6.1. Is this a real absence, or just the result of a lack of searching? I raise this point because I do not believe that Near Eastern archaeologists have yet systematically explained these gaps, both involving areas with hospitable terrain for early farmers and large modern populations. How complete is the site distribution in the Levant? Could the PPNB in reality have supported many more than 92,500 people?

Whatever the answer, the decline of the megasites in the late PPNB meant a return to smaller settlements in the so-called PPNC (only represented in the southern Levant) and the Pottery Neolithic. Some very large exceptions did continue—at Ain Ghazal in the PPNC and Sha’ar Hagolan in the Pottery Neolithic, for instance, with continuing occupation into the Pottery Neolithic at Abu Hureyra. But with the balkanization of the Levant PPNB interaction sphere that resulted from all the environmental stress, the southern Levant, in Simmons’ view, became marginalized and peripheral during the Pottery Neolithic (p. 226), although he seems rather ambivalent about this and on the next page (p. 227) proposes instead “…an efficient readaptation to new conditions rather than a cultural regression.” True pastoralism in the drier regions now existed side by side with full agropastoralism in the smaller farming settlements. It is around this time, of course, that we see large settlements fully established in central Anatolia (e.g., Catalhoyuk) and eventually southeastern Europe, not to mention Mehrgarh in Pakistan and later on the Nile Valley, although Simmons does not stray far into these regions.

One hundred kilometers or thereabouts across the ocean from the Syrian and Turkish coastlines, the island of Cyprus has given us some of our most thought-provoking information about the abilities of very early Levantine food producing populations. Simmons describes the new results very clearly, having worked on the island himself, especially in the cave of Aetokremnos that seemingly documents an initial phase during the terminal Pleistocene of apparent hunter visitation (scouts?), who perhaps engaged in the extirpation of an insular population of pygmy hippos. Soon after this, the Neolithic settlers arrived in full force, obviously by boat, either in the late PPNA or the early PPNB, and possibly as early as 9000 BC. Housing was established in the PPNA circular mode and continued firmly within it down to the time of Khirokitia, ca. 6500 BC. One of the oldest sites, Shillourokambos, has circular posthole settings, wells up to 5 m deep, a collective burial with 20 skulls, Byblos points, naviform cores, and Cappadocian obsidian, all hinting at a date very early in the PPNB. Most remarkable, however, is the list of introduced animals and plants—pig, dog, cat, fallow deer, sheep, goat, and cattle, together with barley, emmer, and einkorn. Although barley might have grown wild on Cyprus, all of the other food species, both animal and plant, would appear to have been wild in morphological terms, albeit obviously managed, and introduced by bobbing boat or raft across 100 km of rather deep (and doubtless often rough) sea from the Asian mainland. The dogs and cats were perhaps domesticated, or at least tamed, given that the latter were buried with humans.

This evidence for animal translocation across sea is remarkable to people such as myself who frequent islands in the Pacific and Southeast
Asia, regions where it is paralleled only by similar translocations of smaller wild animal species such as wallabies and phalangers (possums) between islands around New Guinea (e.g., Flannery et al. 1998). The cattle and deer on Cyprus seem not to have survived their first introductions, but the other species did, and this of course raises a very fundamental question. Were these morphologically wild species eventually domesticated on Cyprus itself, or were they replaced by domesticated populations introduced from the mainland? Given current debate over how many times agriculture and animal domestication were developed in the Near East, such potential evidence for a semi-independent domestication could be of great interest. Simmons himself (p. 141) suggests that “Domestication of individual species likely was a geographically independent event...” For me, however, the archaeological record of continuing contact with the mainland, for instance with the Anatolian obsidian, would certainly make any suggestion of total independence quite untenable—the first PPN Cypriots presumably arrived with a firm knowledge of the principles of plant and animal food production, otherwise they would not have carried all those species with them. In fact, Cyprus gives us the world’s first evidence for a portmanteau biota (Crosby 1986: 89).

Simmons adds a final chapter in which he states “It is easy to wonder if the Neolithic Revolution has been worth it. Was it an improvement for humanity or a harbinger of the strife that affects so much of the world today?” (p. 278). Many modern archaeologists seem to take the view that the sooner the whole Neolithic Revolution concept is buried the better. I am not one of these, and regard Simmons’ synthesis as balanced, perceptive, and well informed. After reading the book I felt it could perhaps have benefited from a few more illustrations, and it would be nice if archaeologists writing about Cyprus would explain why all those place names have to be in italics! But, as a revolutionary at heart, I have no qualms over giving the Near Eastern Neolithic its due in the worldwide history of human affairs.

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The World

Lord Avebury's Virtual Journey through Time

By Yaroslav V. Kuzmin


This is a book of a quite unusual style for a prehistorian writing about past human societies. It has a description of virtual travel throughout the entire world, from Asia, Africa, Europe, Australia, to the Americas by a prominent archaeologist from the early years of the field. It is almost impossible for a single individual to properly review the whole book, and it seems to be more reasonable to concentrate on areas with which I am more familiar, such as parts of Europe and Asia (constituting mostly the territories of Russia and neighbouring East Asian countries), and to some extent North America. For the rest of the world, I would rely on the author’s information and interpretations.

The volume under review consists of a preface, 53 chapters with extensive endnotes for each chapter where additional and more specific information is given, a comprehensive bibliography, and numerous illustrations and maps. Chapters are not too long but reasonably detailed, and this is a definite advantage for non-academic readers who may, thus, quickly become acquainted with regions and problems under consideration. Maps give a clear understanding where the sites mentioned by the author are located.

The time frame of this volume is from the height of the last glaciation, about 20,000 BC, to the emergence of first civilizations soon after 5000 BC. Throughout these 15,000 years, humanity experienced general climatic amelioration with some cold spells like the Younger Dryas at ca. 10,800-9600 BC (Mithen 2006:12; hereafter only page numbers are given when this source is considered). The emergence of cities, agriculture, animal domestication, and pottery, and the peopling of the New World occurred in this time frame.

As the readers’ guide through time, a figure well-known to archaeologists and anthropologists is chosen: Sir John Lubbock (1834-1913), also known as Lord Avebury (Bahn 2001:43). He was a banker, politician, and scholar in Victorian England. In 1870-1900 he was a member of the British Parliament, and made the first Baron Avebury in 1900 (Merriam-Webster’s Biographical Dictionary 1995:648; Debrett’s Peerage and Baronetage 2003:P93; Mithen mistakenly indicated this as happening in 1890. His best known scientific book is Pre-historic Times (1865, London: Williams & Norgate; seventh edition, 1913). Lubbock is responsible for the introduction of two important archaeological terms, “Palaeolithic” and “Neolithic” (Darvill 2002:235).

The general approach of the book is to characterize the environment, physical anthropology, artifacts, economy, and spiritual life of the inhabitants of different parts of the world at ca. 20,000-5000 BC, with a focus on the most important innovations which took place in early human societies. By doing so, the author points the readers’ attention to the most fascinating features, such as the waterlogged site of Ohalo on the Dead Sea coast with its rich organic remains of the late Upper Palaeolithic (pp. 20-26); the world’s oldest walled city of Jericho (pp. 56-61); one of the most important Mesolithic and Neolithic sites in Europe, Lepenski Vir (pp. 159-163); the mysterious Mesolithic burial ground of Oleny Ostrov [Deer Island] in northwestern Russia (pp. 168-171); one of the oldest sites in the Americas, Monte Verde (pp. 229-235); the valley of Oaxaca in Mexico (pp. 274-285); Niah Cave on equatorial Borneo Island (pp. 348-352); the extremely remote Zhokhov Island in the Arctic Ocean inhabited at ca. 6400 BC (pp. 381-383); Mesopotamian sites that existed just before the emergence of Sumer civilization (pp. 420-440); and the fertile oasis of Wadi Kubbaniya in the Sahara Desert (pp. 444-452).

Beyond the “educational” part of the book briefly mentioned above, there is an “academic” aspect which is the subject of this review. Here the author is saying: “I have tried to write a book that makes the evidence from prehistory accessible to a wide readership while maintaining the highest levels of academic scholarship” (p. xi). As mentioned in the epilogue (p. 506), the most current evidence taken into account
for the compilation of this volume is dated to around mid-2003. Therefore, Mithen should have taken into account anything published before this time, although that is not always the case, as is demonstrated below.

In order to review this book, one should make a decision about its aim, either “academic” or “educational”. In my opinion, Mithen’s volume is an academic-based narrative written for a wide scientific community, particularly for undergraduate and graduate college students (especially those who are not directly involved in archaeology and anthropology programmes), and for the general public. In the following paragraphs, an evaluation of the “academic” part of some chapters is given, with the addition of new relevant information which has come to light since 2003.

In the introductory chapters 1-2 and endnotes to them, I find a statement which appears oversimplified. The accelerator mass spectrometry (hereinafter AMS) radiocarbon (hereinafter 14C) dating method is given precedence over ‘traditional’ liquid scintillation counting (hereinafter LSC). This is the so-called “AMS myth” in which AMS is claimed to be a more advanced and powerful research tool than the ‘routine’ LSC 14C dating. Why myth? Because the AMS method for the detection of the amount of 14C atoms in a given carbon sample is to a major extent only a technical advance. The opinion in the endnotes to chapter 2 that the AMS method allows the “greatest degree of accuracy” and “a much greater degree of precision” (p. 517) in relation to the traditional LSC method is an inaccurate statement. The degrees of precision and accuracy in LSC are generally higher than in AMS, and this is why the LSC method was used for high precision measurements of 14C content in tree rings (e.g., Stuiver 1993:93-104). The greatest advantage of the AMS method is that it requires about 1000 times less carbon than the LSC method, which is especially important in cases of small artifacts (often from museum collections) or tiny fragments of valuable organic matter such as adornments, textiles (the Shroud of Turin is perhaps the best-known example), and human remains. Also, technical details such as the poisonous chemicals required in the LSC method hamper its widespread use due to strict safety regulations in many countries. Indeed, AMS is the fastest growing field in radiocarbon studies, and it is gradually replacing the LSC method (e.g., Jull and Burr 2006).

In chapter 4 Mithen cites Early Natufian communities in the Near East as the cultural complex associated with the oldest domesticated dogs (pp. 34-35). This view is out-of-date, however, because in northern Eurasia the earliest dogs are now known from central and eastern parts of Europe. At the Bonn-Oberkassel site in northern Rhineland (Germany), the direct AMS 14C date of a fully domesticated dog (Canis familiaris) is ca. 12,300 BP (Street and Terberger 2004; see original report of 14C date in Hedges et al. 1998). This find is briefly mentioned in Clutton-Brock (1995:10) as dated to ca. 14,000 BP and the paper by Clutton-Brock (1995) is acknowledged by Mithen (p. 519) although the Bonn-Oberkassel case is ignored. An even older direct AMS 14C value of ca. 13,900 BP for dog is known from the Eliseevichi I site in the central Russian Plain (Sablin and Khlopachev 2002; Sablin and Khlopachev 2003). The modelling of modern dogs’ DNA back to the final Late Pleistocene, ca. 13,000 BP (corresponding to about 15,000 years ago) suggests that East Asia was the place of first domestication of dog (Savolainen et al. 2002). It contradicts to some extent fossil evidence which seems to be common when DNA data are given precedence over ‘traditional’ palaeontological data (e.g., Rohland et al. 2005). Therefore, the Early Natufian “cradle” of domesticated dog as accepted by Mithen, is an outmoded hypothesis by modern standards.

In chapter 7, the ‘agricultural’ trajectory of Neolithisation is described (versus the ‘pottery’ scenario, see below). The oldest fig trees in the world are mentioned as growing in the village near the town of Jericho (p. 56). The latest information from Levant shows that the earliest cultivation of fig may be dated to ca. 9400 BC (Kislev et al. 2006); but see Denham 2007), which is very close to the time of Jericho’s foundation, ca. 9600 BC.

Continuing the survey of the Near East, in chapter 9 Mithen cites the finding of textiles in the town of Beidha to be considered the oldest in the world: “The people of Beidha are dressed elegantly in fabrics made from spun yarn; they are weaving the earliest form of linen, dyed green and turned into tunics and skirts” (p. 74).
This is, in fact, erroneous. The dates of occupation at Beidha are (uncalibrated $^{14}$C values) ca. 8940-8550 BP (level 6), ca. 8640 BP (level 5), and ca. 8810-8730 BP (level 4), leaving the date of ca. 9130 BP as an outlier (Kuijt and Bar-Yosef 1994:240). The volume edited by Bar-Yosef and Kra where these dates are published is known to Mithen (see reference to paper by S.K. Kozlowski on page 592). However, he only mentions in the endnotes (p. 523) that charcoal dates from Nahal Hemar are of ca. 8270-8100 BP, and that organic artifacts (knotted net, twined fabric, and cordage) are much older, ca. 8690-8500 BP Mithen ignores the older $^{14}$C dates on organic remains like linen, 9120 ± 300 BP (BM-2298) and 8850 ± 90 BP (Pta-3625) (Kuijt and Bar-Yosef 1994:240). His wish to maintain “the highest levels of academic scholarship” (p. xi) is not apparent in this case.

Another example of his reference to an out-moded hypothesis may be found in chapter 13 where the Last Glacial Maximum (hereafter LGM) in northwestern Europe is considered (pp. 111-112). Sites with $^{14}$C dates of ca. 20,000-18,000 BP are known in Rhineland (Wiesbaden-Ingstadt), as well as in the Alps (Kastelhöhle-Nord) and its northern piedmonts (Mitterle Klause) (Street and Terberger 1999, 2000; Terberger and Street 2002; see latest data in Verpoorte 2004). Data on all of these sites were published before 2003. Therefore, at the LGM it is clear that people settled not only “southern France and Spain” (p. 111) but also more northern territories of Central Europe.

In chapter 19, the story of the discovery and interpretation of the Mesolithic burial ground at Oleny Ostrov [Oleneostrovski Mogilnik] in northwestern Russia is recounted. I think Mithen is quite sarcastic in saying about the Soviet archaeologist V.I. Ravdonikas that “As he [Ravdonikas] worked in Stalin’s Russia and archaeological observations were made to confirm the pattern of social evolution as laid out by Frederick Engels, Ravdonikas had to justify it, ... His solution—and the one still favoured by so many archaeologists today when lost for an explanation—was ‘ritual’” (p. 169). Perhaps Mithen does not realize that if Ravdonikas had not followed the ideological mainstream of the late 1930s in the Soviet Union, he would probably have continued in his career as chief Gulag archaeologist! It seems to me that Ravdonikas’ step was quite clever for that time.

As for the “ritual” explanation of the nature of the Oleny Ostrov burial assemblages, Mithen, himself, accepted this scenario when he was trying to explain the enigma of the Mesolithic occupation of Oronsay Island in the southern Hebrides (offshore Scotland): “I found myself grasping at the archaeologists’ last resort: Oronsay must have been preferred for some ideological reason that remains quite unknown to us” (p. 205). The inconsistency is obvious: while Ravdonikas’ understanding of the Oleny Ostrov phenomenon is pure ideological, Mithen’s Oronsay case seems to be “scientific”.

Mithen cites a paper by Price and Jacobs (1990) as a primary source of $^{14}$C dates for the Oleny Ostrov cemetery (p. 534). This is incorrect: the first $^{14}$C dates were released in spring 1989 by Mamonova and Sulzerhtsiky (1989:24), and although the article is concerned mainly with the Cis-Baikal region of Siberia, 15 $^{14}$C assays run on human bone collagen from Oleny Ostrov’s burials give dates between ca. 9910 BP and ca. 6790 BP.

Ignorance of the primary literature denies proper attribution of the priority of study of this unique burial ground, and it is necessary to highlight that Mamonova and Sulzerhtsiky’s (1989) results should be properly acknowledged. Sometimes it is not easy to say who actually was first. In the recent case of direct $^{14}$C dating of Chinese Late Pleistocene hominids (Keates et al. 2007; Shang et al. 2007), the first direct $^{14}$C on a presumably early modern human’s femur from Ordos Plateau (Keates et al. 2007) gave recent dates (ca. 250 BP only!) and were to some extent a disappointment; later, an attempt by Shang et al. (2007) was more successful, giving a Late Pleistocene age (ca. 34,400 BP) of the modern human from Tianyuan Cave near Beijing. The Oleny Ostrov case is straightforward, however, with Mamonova and Sulzerhtsiky (1989) clearly the first in the $^{14}$C dating of it. Once again, Mithen’s “academic scholarship” failed to recognize it.

In chapter 25, the issue of the peopling of the New World is considered, with the Monte Verde site in Chile cited as the earliest evidence of humans in the Americas, dated to ca. 12,500 BC. Mithen states, “For the need of this histo-
The time difference between the main cultural component of Monte Verde (MV-II) and other early sites in North America is not so great as to claim Monte Verde as the oldest settlement. Carbon fourteen dates on organic remains from component MV-II are from ca. 11,920 bp (e.g., Bever 2006:597). New mastodon bone 14C dates from component MV-6 are about 12,450 bp (George et al. 2005:770). The variation of organic 14C dates from this single component is about 1650 14C years, and the most reliable of them vary within 500 14C years (Dillehay and Pino 1997).

It is still poorly understood by many scholars that the actual accuracy "[real accuracy] sensu Krenke and Sulerzhitsky (1992); see also Kuzmin and Otrova (1998:24-25)" of the 14C dating method is not less than several hundred years; it is important to keep in mind that real accuracy is unrelated to the precision of 14C dating measurements. Thus, it is impossible to determine with the help of 14C dates which site is older if the age difference between them (in the case of Palaeolithic or Palaeoindian complexes) is less than about 1000 14C years. We should bear in mind that the earliest archaeological sites in Alaska are now securely dated to ca. 11,800-11,600 bp (e.g., Bever 2006); and the oldest Clovis sites are dated to ca. 11,600 bp (e.g., Waters and Stafford 2007). Therefore, the difference between the Monte Verde site in South America and the universally accepted earliest sites in North America (i.e., Clovis and Nenana complexes; see Bever 2001; Waters and Stafford 2007) is about 800-900 14C years and perhaps even less. This is not enough in my opinion to claim that Monte Verde is the oldest site in the New World.

The author proposes the 'birthplace' of Clovis complex is in the forests of eastern North America (p. 245), in spite of the fact that the earliest Clovis sites are known in the western United States on the Great Plains: Aubrey (ca. 11,600 bp) in Texas and Blackwater Draw (ca. 11,300-11,100 bp) in New Mexico (e.g., Holliday 2000; Haynes 2002). None of the Clovis sites east of the Mississippi are older than ca. 11,000 bp (Haynes 2002:12-13; Waters and Stafford 2007). Therefore, the attempt to shift the place of Clovis origin toward the east contradicts primary data.

In chapter 27, in the discussion of Clovis hunting of Late Pleistocene megafaunal species, the author touches on the existence of Holocene mammoths at Wrangel Island in the Siberian Arctic (pp. 252-253). These most recent woolly mammoths were originally thought to be dwarfs (Vartanyan et al. 1993; Lister 1993). However, Garutt, Averianov and Vartanyan (1993) challenged this conclusion a few years later when they published a more extended description of the new sub-species, *Mammuthus primigenius vrangeliensis* (Averianov et al. 1995). It turned out that even though the size of Wrangel's mammoths was smaller than the usual *M. primigenius* individuals, it was still not a dwarf but a small size mammal ("Melky" mammoth in Averianov et al. 1995); "melky" means "small", see Russko-Angliyski Slovar (1998:286). Unfortunately, the reference to the "dwarf" Holocene mammoth from Wrangel Island persists in some publications, including Mithen's book. Latest research on possible dwarfing of mammoth populations revealed that small size mammoths are known not only at Wrangel Island in the Holocene, but also in mainland Siberia in the final late Pleistocene (Tikhonov and Vartanyan 2001; Reumer et al. 2002; see details in Tikhonov et al. 2003). Surprisingly, the most recent finds of Holocene woolly mammoths from the Pribilof Islands in the Bering Sea dated to ca. 5700 bp (Yesner et al. 2005) shows that these latest American mammoths were of normal size! Thus, the suggestion of woolly mammoth dwarfing in the Holocene is now definitely out-of-date.

Chapters 39 to 41 are devoted to China, Japan, and Arctic Russia, and they deserve special attention as the reviewer is familiar with the primary data which were used to describe human existence in East Asia and northeastern Siberia. In the beginning of this section, "Greater Australia and East Asia", the map with archaeological sites, glaciers' margins, and coastlines (pp. 302-303) shows the vast continental ice sheet existed at ca. 20,000 bc (i.e.,
17,000 BP). This is another outmoded theory dated to the 1960s and 1970s (e.g., Arkhipov 1998; Grosswald and Hughes 2002); latest data indicate the absence of large-scale ice sheets in Siberia at the LGM (e.g., Mangerud et al. 1999, 2002; see summary in Svendsen et al. 2004).

In chapter 39, “Down the Yangtze” (pp. 359-369), the origin of rice farming and pottery in China is the main topic. The date of the earliest Chinese pottery as presented by the author seems to be in conflict with existing information. For example, he says that “This slab technique had first been used at Diaotonghuan at around 10,000 BC to produce the oldest known pottery in China” (p. 366). In the endnotes (p. 557), it is added that “Pottery of a similar date has been found elsewhere in China, notably in Yuchan Cave on the southern edge of Hunan Province (Lu, 1999).” The issue of age of the earliest Chinese pottery is quite complex (see review in Kuzmin 2006:364-366); some opinions indicate that it may be dated to ca. 15,200 BP (equal to ca. 16,570 BC; see Reimer et al. 2004:1049). Moreover, the “chronometric hygiene” of 14C records from the earliest pottery sites in China (Kuzmin 2006) allows for the acceptance of the first pottery production at Miaoayan and Yuchanyan [Yuchan Cave] sites at ca. 13,700-13,300 BP (about 14,500-13,800 BC; Reimer et al. 2004:1050-1051).

The Diaotonghuan Cave case is the central one for determination of the timing of pottery manufacture and rice farming in China (pp. 363-364). However, chronometric data for this site are very scanty. Wu and Zhao (2003:18-19) provide only one 14C value from stratum D, ca. 15,090 BP. The rest of the cultural strata with pottery, including ones with rice phytoliths, are still not 14C-dated, to the best of my knowledge. The age estimates for the Diaotonghuan Cave sequence are quite ‘young’, ca. 11,000-8000 BP for zones (or strata in Wu and Zhao 2003) D and E (the latter is below zone D; see review in Kuzmin 2006:365). Zhao (1998) assumed that the age of the earliest domestic rice phytoliths in stratum E is ca. 10,000-9000 BP. Therefore, the age of Diaotonghuan Cave cultural layers, assumed to be ca. 12,000-2000 BC (p. 363), is quite speculative, and more research is needed to get solid data.

The age of rice domestication is one of the hottest topics in modern East Asian archaeology. Recently, Jiang and Liu (2006) found evidence of rice cultivation in southern China at ca. 8900 BC (ca. 9600 BP). Fuller et al. (2007), however, challenged the view that this was completely domesticated rice, and proposed an alternative model in which rice of full domestication occurred only in the fifth millennium BC (ca. 6000 BP). The latest data came from Liu et al. (2007) in which they disagree with Fuller et al. (2007) and offer instead the date of ca. 9000 cal BP (about 8000 BP or 7050 BC) for the initiation of rice cultivation which later led to domestication. Therefore, we can tentatively accept the age of the earliest rice cultivation in China as ca. 7000 BC—much later than the time of pottery emergence in East Asia (see Kuzmin 2006). The assumption that pottery and rice in China appeared at almost the same time—“The near simultaneous invention of pottery and the first cultivation of rice is unlikely to have been coincidental—the vessels were most likely used to steam and boil the grain” (p. 366)—is not supported by primary evidences.

It is now clear that in East Asia pottery preceded agriculture, as opposed to the Near East where ceramics were invented after the emergence of plant cultivation. The author acknowledges different trajectories of Neolithisation: “In Japan and Sahara the invention of pottery preceded the start of farming, whereas it occurred simultaneously with the origin of rice farming in China; its invention in western Asia came about long after farming towns had begun to flourish” (p. 505). However, as it is shown here, the beginning of rice farming and pottery emergence in China are unrelated, and the East Asian ‘way’ of Neolithisation (first pottery, and only afterwards farming) is typical for China, Japan, and neighbouring regions.

The last issue in this chapter is the timing of the separation of Japanese Islands from the mainland after the LGM, when water levels rose due to melting of continental ice sheets. In the end, it is stated that “A detailed chronology for sea-level rise around the coast of Japan and the precise date for when its connection with the mainland was breached are currently unavailable” (p. 558). Here Mithen is quite mistaken because such data have existed for a long time (e.g., Naruse 1981; Korotkii 1985;
Oba et al. 1991; see brief review in Nunn 1999:173-185), and in Japan even the atlas of sea-level changes was published a while ago (Ota et al. 1987)! In one of the most recent summaries (Park et al. 2000) it is shown that the western part of Tsushima (Korea) Strait remained open even at the height of the LGM, ca. 20,000–18,000 bp; I also assumed this when analyzing the bathymetric maps of the Sea of Japan (Kuzmin 1997). Another statement—“This issue [sea-level changes] is complicated by the tectonic activity around the coasts of Japan that has also had a major influence on changing sea levels and interpretation of sedimentary evidence” (p. 558)—is also out-of-date. Detailed investigation of coastal tectonics in Japan has been undertaken since the 1950s, and the results produced were readily summarized (e.g., Berryman et al. 1992; Ota and Yamaguchi 2004). It is clear that tectonic movements during the last 85,000 years or so did not influence severely the coastlines of major straits separating Japan from mainland (e.g., Oba et al. 1991).

In chapter 40, “With the Jomon” (pp. 370-380), various aspects of the earliest pottery cultural complex of Japan are described. Once again, the old cliché appears on page 372: “Jomon pottery is, in fact, the earliest pottery in the world.” Data on Late Glacial pottery (ca. 13,300–10,300 bp) from the Amur River basin, located in the Russian Far East neighbouring Japan and China, were first published in 1997 and in more detail in 2000-2001 (Kuzmin and Jull 1997; Kuzmin et al. 1997; Kuzmin and Orlova 2000; Kuzmin and Keally 2001; see the updates in Kuzmin 2006; Nesterov et al. 2006), and they are now finally accepted by leading Japanese scholars (e.g., Kobayashi 2004:20, 190). The earliest Jomon pottery is now dated to at least 16,000 bc (Nakamura et al. 2001; Kuzmin 2006; Taniguchi 2006) compared with ca. 13,000 bc (p. 372). In fact, the earliest Chinese pottery could be even older than Jomon pottery, but more data is required for confirmation (see Keally et al. 2003; Kuzmin 2006).

As for the possible connection between vegetation changes and the emergence of pottery in Japanese Islands, it is mentioned that C.M. Aikens suggested the “simultaneous spread of broad-leaved woodland and pottery into the northern islands of Japan, both appearing on the northernmost island of Hokkaido at around 7000 bc” (p. 372). However, this is now challenged by new data about much older pottery from Hokkaido dated to ca. 14,400–14,100 bc (Yamahara 2006). It seems that pottery appeared in Hokkaido when environmental conditions were characterized by the dominance of boreal forests with minor (if any) admixture of broad-leaved elements (e.g., Tsukada 1986). The correlation between a sharp increase in pottery quantities at the Jomon sites and the spread of broad-leaved taxa such as oak and chestnut in Japan at ca. 10,000 bp (i.e., around 11,500 bc) is now suggested (Taniguchi 2006).

One of the central points of this chapter is the possible function of Jomon pottery (pp. 372-373). Mithen argues against a utilitarian usage by showing that when pottery appeared in the northern part of Honshu Island (region of Tōhoku) at ca. 14,500 bc at the Odai Yamamoto 1 site (on page 373 it is printed as “Odaiyamamoto”), the environment was “no more than a sparse covering of pine and birch” (p. 373). Some comments are necessary. New data allows for establishing the age of pottery at the Odai Yamamoto 1 site at ca. 16,000 bc (Nakamura et al. 2001). The environmental conditions at that time were not as severe as suggested. The northern part of Tōhoku in the Late Glacial was covered with conifer forests although some broad-leaved taxa (beech) also began to appear (Tsukada 1986:27-34). The statement, “the theory that Japanese pottery was invented to store and cook acorns and other produce of broad-leaved woods cannot be correct” (p. 373) is to some extent incorrect. Even though there were not many nut resources around the Odai Yamamoto 1 site at ca. 16,000 bc, the stable isotope data show clearly that carbonized adhesions (a burnt substance covers the inner and outer parts of potsherds and forms lines indicating the past water level in the pot) represent terrestrial matter which is most probably food remains (Nakamura et al. 2001:1137). Therefore, the utilitarian function of the earliest Japanese pottery for storing and cooking food is very possible, despite the fact that acorns and other nuts were hardly available to the site’s inhabitants.

As for the late spread of rice agriculture to the
northern Honshu and delayed arrival of the Yayoi complex which follows Jomon in the prehistory of Japan (p. 380), Mithen overlooks information on rice cultivation in later Jomon phases in the Tohoku region. D’Andrea et al. (1995) provided evidence of prehistoric cultivation of rice in northeastern Honshu at ca. 2800–2500 BP, using direct AMS $^{14}C$ dates of rice caryopses, which corresponds to calendar ages of ca. 1040–700 BC (median values of calibrated ages with ±sigma; Calib Rev. 5.0.1 software, available at http://www.radiocarbon.org).

It is evident that in Late Jomon people of Tohoku already practiced rice agriculture, i.e. before the arrival of Yayoi populations from the south. Recently, it has been suggested that the boundary between Jomon and Yayoi periods should be shifted from the traditionally accepted ca. 300 BC to ca. 750–400 BC (Imamura 2003). However, even though this is correct, the rice from the Kazahari site in Tohoku (D’Andrea et al. 1995) precedes the Yayoi of Japan.

In chapter 41, “Summer in the Arctic”, one of the major topics is the Dyuktai cultural complex of the Upper Palaeolithic in central Yakutia, northeastern Siberia (pp. 382-385). Mithen traces the origin of this culture to the Transbaikal region south of Yakutia where the Studenoe 2 site with microblade tools and wedge-shaped cores has been excavated. This ‘Transbakan’ connection of the Dyuktai culture looks strange because the Studenoe 2 investigators (Konstantinov 1994; Goebel et al. 2000) do not derive the Dyuktai complex from the late Upper Palaeolithic assemblages of Transbaikal. The $^{14}C$ dating programme of Studenoe 2, which was begun by Konstantinov (1994) and Goebel et al. (2000), has been continued; new results generally confirm the age of the site around 17,900–17,200 BP (Buvit et al. 2003, 2004) and also show that the particular component 4/5, with a total of six hearths, existed until ca. 14,500 BP (Kuzmin et al. 2004).

Generally speaking, the origin of Dyuktai culture is now rarely discussed, and most of the summaries on the Upper Palaeolitic of Siberia lack such discussion (e.g., Morlan 1987; Abramova 1989; Goebel 2004). Based on the presence of bifaces in Dyuktai, the primary investigator of the complex, Mochanov (1977:228-229; 1978:65), derives it from the Middle Palaeolithic assemblages of Europe, Central Asia, and China where bifacial tools are common. Indeed bifaces are found at many Middle Palaeolithic sites in eastern Europe (see Hoffecker 2002:94-95, 104-105) and in the Altai Mountains of southern Siberia (Derevianko and Shunkov 2002, 2004). Derevianko (1996, 1998) considers the Selendzha [Selendga] cultural complex of the Amur River basin in the Russian Far East to be the origin for the Dyuktai complex.

The critical question in the discussion of the Dyuktai complex’s roots is the age of the earliest Dyuktai sites. Yi and Clark (1985) and Abramova (1989:232) assume the lower limit of the Dyuktai assemblages with bifaces and wedge-shaped cores to be ca. 18,000 BP, as opposed to Mochanov’s (1977, 1978, 1984) determination of it as being ca. 35,000 BP, and possibly as late as ca. 26,000-23,500 BP (Vasil’ev et al. 2002:510). Recently, the Yana RHS site with bifacial tools was found in northern Yakutia (Pitulko et al. 2004), dated roughly to ca. 27,000 BP. The assemblage lacks wedge-shaped cores and is not related to the Dyuktai complex according to Pitulko et al. (2004:56). Thus, the origin of Dyuktai culture remains obscure; however, it is clear that emergence of the Dyuktai has nothing to do with the late Upper Palaeolithic microblade assemblages of the Transbaikal.

The issue of frozen mammoth bodies found in Siberia is also discussed in chapter 41. However, there are some factual mistakes. First, “The earliest recorded discovery is a mammoth from Berezovka, located in the far northeast of Siberia” which was made in 1900 (p. 384), is not the earliest one. It is well-documented that the first such find known to scientists was made in 1797 at the mouth of the Lena River; this was the so-called “Adams’ mammoth” finally brought to Imperial Russian Academy of Sciences headquarters in 1808 (Garutt 2001:16); publication followed in a few years (Tilesius von Tilenau 1815). Second, the expedition to get the Berezovka mammoth in 1901 could not depart from Petrograd (p. 384), because at that time the official name of this city was Saint-Petersburg (it was renamed to Petrograd in 1914 due to the beginning of the first World War and the rise of anti-German feelings). As for
Wrangel Island as the place where “the last of the woolly mammoths [were] to walk upon planet earth” (p. 386), a second Holocene mammoth refugium is now known in the Bering Sea at St. Paul Island in the Pribilof Islands (Guthrie 2004; Yesner et al. 2005).

In the book, there are some small factual errors. Paul Martin’s affiliation is the University of Arizona not Arizona State University (p. 213); Bluefish Cave is situated not in Alaska, USA, but in Yukon Territory, Canada (colour photo after p. 240); Japanese scholars listed as “Suago Yamanouchi and Hiroyuki Sato” (p. 372) are in fact Sugao Yamanouchi (see, for example, Aikens and Higuchi 1982:99) and Tatsuo Sato (see Pearson et al. 1986:162, 514; Kobayashi 2004:19; Habu 2004); the paper by Taylor, Haynes, and Stuiver (1996) was published in *Antiquity* but not *American Antiquity* as indicated on page 606; in reference to Vartanyan, Garutt, and Sher (1993) on page 607, the word “dwarf” in the paper’s title is missing. Besides this, there are several typographical mistakes in the book which derive, perhaps, from a not very careful reading of galley proofs. I would mention the most striking ones: “Miginik”, p. 358 (instead of “Mogilnik”); “Aitkins”, pp. 372, 373, and 559 (instead of “Aikens”); “Studenhoe”, pp. 383 and 389 (instead of “Studenoe”); “bc”, endnote 16, p. 533 (instead of “bp”); “Antev”, p. 548 (instead of “Antevs”—see, for example, Waters 1992:8; Bever 2001:128); “late-pleistocene”, p. 555 (instead of “Late Pleistocene”); “Yanoi”, p. 560 (instead of “Yayoi”); “levallois”, p. 566 (instead of “Levallois”); “Biglow”, p. 586 (instead of “Bigelow”); “Cainozoic”, p. 598 (instead of “Cenozoic”). The mistakes, errors, misinterpretations, and other flaws indicated above can easily be fixed if following editions of this quite interesting volume are released. I hope that comments of this review will be helpful to the author in order to make his book better from the standpoint of academic standards.

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Islands All at Sea

By Peter White


Islands are big in academic business these days. The study has its own term, either Nissology or Nesiology depending on your source, but both claiming to derive from the Greek. There are a range of academic journals devoted to them in many fields including archaeology (e.g., Journal of Coastal and Island Archaeology). Special issues in many more are common (e.g., Human Ecology 25(3) 1997). A recently published Reader in Island Studies (Baldacchino 2007) features 16 original articles (one on archaeology) in more than 600 pages. One might therefore think that a slim (200 small pages) volume on the archaeology of islands would be a snap.

But, as Rainbird asks (p.2), “Is there anything special about the archaeology of islands that require a specific set of methodological and interpretational techniques different from that found on continents?”. His answer: ...“a qualified ‘yes’, but for the most part it is a ‘no’”. In his view, this is the wrong question. Rather, our approach should be to decenter dry land as the key geographical element and focus on maritime communities—in other words, an ‘archaeology of the sea’. By page 3 then we are faced with a very different conceptual framework, for maritime communities exist on boats and the edges of continents as well as on islands. What links them is a relationship to the sea. This is not maritime archaeology, with its stress on boats and wrecks, but patterns of behavior in which all maritime communities are similar. In chapter 3, Rainbird uses ethnographic, sociological, and historical insights to understand how an “embedded perception of the sea may be embodied and . . . identified in material culture” (p.46). These societies, often incorporated in larger communities, may be identified by some or all of esoteric language and taboo behavior, house location, diversity of origin, sea-going knowledge, and ritual. Most of these can have material culture correlates, starting with boats and leading eventually to gender relationships (since seamen are often away for long periods, women’s roles are likely to differ from those whose husbands are there all the time—the theme of a number of novels of course).

Setting up this approach, of course, takes us a long way from ‘islands’ in any sense that would be acceptable to regular geographers. But rather than going along the route he had identified, in the next four chapters Rainbird discusses four ‘real’ islands—Malta, Pohnpei, Gotland, and the ‘Atlantic archipelago’ aka Britain, Ireland, and their attachments—in two oceans and two seas. (How are these differentiated? There’s another problem.) In each of these cases he is at pains to argue that it is the maritime aspect which is notably important in driving history. It is here, I suggest, that we encounter the underlying theme song behind Rainbird’s book—and indeed much of his earlier work (e.g., 1999, where the structure of this book is pretty well laid out and, indeed, chapters 1 and 2 are largely replicated in concepts and language). This is that islands are almost never isolates, but are in relatively constant communication with others.

One is tempted to say ‘Leaping lizards, Mr. Scientist!’ The idea that islands were isolates was probably most extremely expressed in McArthur and Wilson’s Theory of Island Biogeography in 1967. Taken up particularly by Pacific archaeologists using the concept of islands as ‘laboratories’ of cultural diversity, it is nonetheless the case that only a few took a view as extreme as the original. By 1986 Kirch was pointing out that while islands were bounded they were not therefore closed, and that the “laboratory analogy can easily be pushed too far” (p.2). The various studies in that volume nearly all realized this. More recently, in the Discussion and Debate on Rainbird’s paper referred to above (1999) all the various commentators recognized that while islands certainly existed (‘islands are habitats surrounded by radical shifts in habitat’ [Terrell]), island societies always extended beyond their basic habitats and insularity is always a cultural construct. The same theme runs through the contributions to Fitzpatrick (2004).
problem from a different discipline, D’Arcy (2006) discusses how closely Pacific Islanders’ lives were integrated with the sea at every scale and there are many other accounts with a similar approach—one thinks of Epeli Hau’ofa’s ‘sea of islands’. In other words, I think that Rainbird’s starting point is a dead horse. However this may be, do the four island studies have themes in common and does their analysis from Rainbird’s viewpoint improve our understanding of their history? I start with each island.

Malta, in the Mediterranean, almost certainly originally settled from Sicily, is probably best known for its Neolithic megalithic architecture, dating roughly 3600–2500 BC. These temples (rather than mausoleums) are mostly oriented with the apex towards the north and northwest. This is the direction of the original homeland as well as of the most obvious exotic material, obsidian from Pantellaria and Lipari, and Rainbird suggests they “ossify a link with travel” (p.72). However, at the time they were built the amount of obsidian had significantly decreased, and the general interpretation of this architecture is that it was the result of isolation and inward-looking attitudes (e.g., Zohar 1996). Rainbird turns this around, suggesting that the architectural similarities between structures and their imposing size were a deliberate attempt to set up a “cohesive identity to impress outsiders” (p.73). Thus the continuing use of obsidian and absence of metal, already in wide use in other parts of the Eastern Mediterranean, become, in his view, devices for maintaining identity. Malta became a “holy island”, to which pilgrims came and where “The deep red wall, sculpted decoration, and statuary would have been illuminated by the flickering flames of fires in hearths, the wood smoke perhaps augmented by a heady mixture of scented herbs” (p.75). The fact, not mentioned here, that most of the statuary was of fat women, suggests a possibly different take on ‘pilgrims’ as followers of the Earth Mother beloved of some modern archaeologists.

But let’s get real for a moment. What is the evidence of such pilgrims, or even contact? The answer is, not much. The fact that many of the carvings in the temples are of grain and domestic animals is used to argue that these might have been brought as offerings. Beyond that, we veer off into analogy with the hajj to Mecca and the holiness of islands in Lake Titicaca. More usefully, some time is spent in discussing the long-continuing and widespread evidence of sea travel in the Mediterranean. From at least the end of the Pleistocene obsidian and domesticated plants and animals were being moved around that sea, parts of which were readily able to function as a ‘sailing nursery’. Maritime travels are a major way, for instance, of making sense of the history of agriculture in the region. So people could and did frequently travel by sea. Pilgrims were possible. But I don’t see that Rainbird gets much further than that. In terms of his general proposition, the idea that it is the maritime aspect of the society which drives the Neolithic history of Malta seems to be based on a very slim dataset.

Oceania is the theme of chapter 5 with a focus on Pohnpei and the Eastern Caroline Islands. This region, of course, Rainbird’s particular specialty, having written the only comprehensive study of the archaeology of Micronesia (2004). Overall, this chapter is a straightforward, though brief, account of island settlement of the entire Pacific and of the known widespread archaeological and ethnographic contacts between islands and archipelagos. He accepts Anderson’s (2000) argument that highly specialized sailing vessels in Polynesia derived from Micronesian technology and were only present from about 1200 AD. He also accepts Forster’s account of Tupaia’s traditional, pre-Cook knowledge of 83 islands in Central Polynesia. Both of these are the subject of a good deal of ongoing debate.

The only detailed discussion in this chapter concerns Nan Madol which, as in the case of Malta, Rainbird argues to have had a role as an ‘attractor’ of visitors to Pohnpei along with its other, internal, island-specific roles. As with Malta, the evidence of such visitors is thin—the likely importation of pearl shell, the use of new species of shell for adzes, and the adoption of a new type of adze, along with the undated evidences of rock art and the arrival of the psychoactive drug kava. That new material or ideas must have arrived with visitors is, he argues, necessary since, at the time of the main building of Nan Madol, Pohnpeians had no ocean-going seacraft. However, it is the earliest European records which are said to document this. We
might thus have only an ‘after this’ date. But how certain are we of the data? In 1595 Quiros was the first European to report on Pohnpei and comments (Markham 1904:114):

Natives came in their canoes from the island under sail, others paddling. As they were unable to cross the reef, they jumped on it and made signs with their hands. In the afternoon one single native in a small canoe came round the end of the reef. He was at a distance to windward...

This doesn’t seem to me to be very strong evidence of absence.

The fact that other Caroline Islanders, all from atolls, were wide-ranging mariners is well documented and this provides some substance to the argument. Rainbird buttresses his case by reference to the sawei system surrounding Yap, but my conclusion is that his argument is more dependent on the model than on the evidence.

The history of Gotland, in the Baltic, seems straightforward, without problematic interpretations of the data. The unusual archaeological features of the island occur in the Late Bronze and Iron Ages, with the occurrence of ‘ship-settings’ (outlines modeled by rocks) and ‘picture stones’, which primarily feature ships. Both are almost exclusive to Gotland and Rainbird sees them as expressing a strong island identity which later culminates in the very large numbers of massive mediaeval churches. He also notes that throughout its history Gotland maintained considerable contacts with various shores of the surrounding mainland and imports on a considerable scale have been found. He suggests that the two aspects of society are in fact related, the one (imports) stimulating the other as Gotlanders faced a wider range of people than more land-based societies. Here it might have been useful to compare Gotland with an archaeology/prehistory of one or more of the societies on the surrounding Baltic shores. If Rainbird’s idea of an ‘archaeology of the sea’ has validity, then we should see things comparable in at least some of them. Restricting himself in this book to oceans and specific islands within them doesn’t allow him to demonstrate that his overall argument is convincing.

Most archaeologists working in the Atlantic Archipelago do not consider themselves working on islands, yet Rainbird is clearly correct in saying this is so. The archipelago is in fact a hierarchy of islands, the Orkneys or Isle of Man offshore to larger islands, Ireland offshore to the ‘mainland’ of Britain, Britain itself almost a peninsula of Europe. He documents the very considerable amount of interaction by sea, not only within the archipelago—notably along the corridor between Britain and Ireland, and beyond to the north—but especially south to Normandy and the Iberian Peninsula. Such traffic has been well described over decades and what Rainbird adds is simply the suggestion that in some ways this could well be considered as a ‘culture area’. Is this really a new suggestion?

In the century of the European Union such a diminution of British isolationism is perhaps not surprising. But what Rainbird does in this context is almost a reversal of his approach to the other islands he has discussed: he doesn’t try to discover any idea of coastal or island identity. The ‘sceptered isle’ is foregone, to become part of a larger entity, part of a ‘sea of islands’. Interestingly, it seems to me that this is the first case where Rainbird’s model is exemplified. While he doesn’t show that people on all of these different islands had a common approach to the sea, or even one distinctive from people inland, he here does really elaborate an archaeology in which the maritime aspect drives the history.

Having said initially that there might be at least something distinctive about the archaeology of islands, we learn in the conclusion what this is. It turns out to be two things. First, there are ‘communities of communication within a maritime context’: communication is what links most people on islands to each other. This communication consists of symbolic markers and shared experience. Second, islands make some things visible. These are the same as expounded by Fitzpatrick recently: environmental change, colonization, migration and demographic change, and inter-regional interaction (2004: 3-18). Rainbird agrees that, in general, each of these processes is likely to be more visible on islands than in more extensive landscapes. I wonder how much more was ever claimed?

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Professor Amilcare Bietti passed away on July 28th 2006, he was 69 years old. Raised archaeologically under the guidance of Mariella Taschini, he was a profound connoisseur of the Typological Method (see Taschini & Bietti 1972 and Taschini, posthumus work by Amilcare, 1979). At the same time, from the beginning he recognised its applicative limits and understood its weakness regarding the interpretation of archaeological evidence. For example, he demonstrated on several occasions the problems linked to the identification of cultural facies through the traditional typological subdivision of the Upper Palaeolithic in central and southern Italy (see Bietti 1976-1977 and particularly the discussion on the term Evolved Epigravettian; Bietti 1990, for a revision, on a “behaviouralist” note, of the main Epigravettian archaeological evidence in Italy).

The “Amilcare approach” was not simply about open criticism (Bietti 1978) but, through the elaboration of mathematical methods applied for probably the first time in a systematic way in Italy (Bietti 1974-1975), it produced alternative models and theories to those already known to the scientific community. The use of multivariate analyses, the T and Chi-square tests and other mathematical instruments allowed Amilcare to propose hypotheses regarding the modalities of site occupation (Bietti 1981, 1985, 1989, 1994) and the management of raw materials (eg. Bietti 1980, Bietti & Grimaldi 1991) clearly following the “procedural” paradigm. Amilcare also proposed discussions and observations of remarkable originality regarding the need to establish a theoretical and methodological framework in which to insert archaeological data, in other words, of defining Palaeoethnology as a scientific discipline (e.g., Bietti & Bietti Sestieri 1985, Bietti 1986, 1991).

In 1980 he became Professor of Ethnology at the former Institute of Anthropology of the “La Sapienza” University in Rome; in 1983 he became Associate Professor of Prehistoric Ecology at the Department of Human and Animal Biology of the same university. In 2004 he became Ordinary Professor. He was president of the IV UISPP Scientific Commission (1991-1996) and as such, also a member of the UISPP Permanent Council and Executive Committee. Furthermore, he had for many years conducted the course “Mathematical and Statistical Methods applied to Archaeology and Palaeoethnology” at the School of Archaeology of the University of Rome. From 1991 to 1999 he was Secretary General of the Italian Institute of Human Palaeoethnology and had recently become President. A complete bibliography of Amilcare’s works will be published by the Italian Institute of Prehistory and Protohistory (Florence).

Notwithstanding his illness, Amilcare continued working, publishing and holding lectures at the University for as long as he was able to. He will remain a scientific figure of continuous reference for future research and a splendid and unforgettable memory for all those that worked with him or simply had the chance to meet him.

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Stanley Ahler was born September 10, 1943, in Florence, Alabama, to Ernest and Ruth Ahler, and passed away on February 3, 2007 in Flagstaff, Arizona at the age of 63. He graduated from the University of Tennessee in Knoxville in 1967 with a major in anthropology and worked on numerous local archaeological projects sponsored by the university. His studies in lithic technology began with his master’s thesis, “Projectile Point Form and Function at Rodgers Shelter, Missouri.” Published in 1971, it became one of the most-cited master’s theses in North American archaeology. One of the first studies to build on the work of the pioneering Russian archaeologist S.A. Semenov, it was seminal in its contribution to microscopic use-wear analysis. His recent study of Folsom points also is especially noteworthy.

In 1970 and 1975 he earned an M.A. and a Ph.D. in anthropology from the University of Missouri-Columbia, where he soon became enmeshed in the prehistory of the Middle Missouri Valley, but with time out to participate in archaeological excavations at Mount Carmel, Israel, and in Alaska. In 1973, he became affiliated with the Illinois State Museum, and in 1975 he moved to the University of North Dakota, where he directed archaeological work at the Knife River Indian Villages National Historic Site for the Midwest Archeological Center, National Park Service from 1976 to 1987, leading to our first real understanding of Hidatsa culture history. In 1990 he moved to Flagstaff and for a time worked as a staff archaeologist with Northern Arizona University. His career expanded significantly when he founded and became director of the PaleoCultural Research Group, a non-profit research and educational organization that has conducted numerous archaeological projects in the midcontinent. Many of these projects were in the Northern Plains in conjunction with the State Historical Society of North Dakota. Major excavations that he led for the society at the On-a-Slant and Double Ditch Mandan villages, and at the prehistoric Beacon Island, Menoken, Scattered Village, and other sites led to a vastly expanded knowledge of the prehistory of the Northern Missouri River valley.

Stan spent a lifetime in the pursuit of archaeology, during which time he made substantial and lasting contributions to Plains prehistory and to archaeological studies in general. Throughout his career he specialized in interdisciplinary studies; studied production systems and the technology, functions, and styles of lithic artifacts and assemblages; and applied quantitative methods to the analysis and interpretation of archaeological assemblages. Stan was one of the first in the Plains to incorporate geophysical techniques as a routine tool in the investigation of sites. His keen mind and deductive capabilities marked him as conceptually innovative, rigorous in approach, and tenacious in work habits. He always conducted himself with integrity and the highest professional standards, and his rigorous excavation procedures have set standards for fieldwork in much of the Plains.

The rich quality and the number of his reports are testimony to the standards to which he held himself and to his pursuit of knowledge. Passing on his knowledge, he has also served as a mentor and later as collaborator to promising students. His career in the Plains has enriched its prehistory and the profession that we pursue. In recognition of his many contributions, the Plains Anthropological Society awarded Stan their Distinguished Service Award in 2006.

W.R.W.
F. Clark Howell 1925-2007

On March 10, 2007, the field of palaeoanthropology lost one of its founders and most renowned practitioners. Early in his career, F. Clark Howell was an advocate of the multidisciplinary approach to human evolutionary studies. He saw that, while fossils are the necessary “hard evidence” documenting evolutionary change, it is important to set these materials in context. Hominids can be studied most effectively when stratigraphic information is considered along with animal and plant remains, behavioral inferences are drawn from stone artifacts, and entire assemblages are dated by radioisotopes and assessed in the light of palaeoecological reconstructions. Along with other pioneers, including Emiliano Aguirre, Camille Arambourg, Glynn Isaac, Louis and Mary Leakey, and Phillip Tobias, Clark Howell helped to integrate the diverse narratives that make up modern palaeoanthropology. More ably than others, he could synthesize vast amounts of information, and he used this talent to enrich our understanding of humans evolving on ancient landscapes.

Howell’s writings range over many topics. In the 1950s, he explored the role of the Neanderthals in human evolution, and these early papers are still cited. With Leslie Freeman of the University of Chicago, he undertook excavations of Middle Pleistocene settlements in Europe. He maintained an interest in the systematics of Australopithecus and Homo, and his much-read chapter on Hominidae (in V.J. Maglio and H.B.S. Cooke, eds., Evolution of African Mammals, 1978) is something of a benchmark for the period. Howell introduced the term palaeo-deme into discussions of the human fossil record. More recently he initiated with Tim White at Berkeley a comprehensive survey of Miocene hominoid localities across the Old World. However, his studies of early humans in Africa serve as particularly solid pillars of a long and distinguished career. As a leader of the International Omo Research Expedition, he directed fieldwork in the northern Turkana (then Lake Rudolf) Basin beginning in 1966. This project was designed to illuminate the geology and dating of a long sequence of Plio-Pleistocene sediments, provide collections of larger mammals, micromammals, reptiles, other animals, and plants useful for tracking changes in palaeoenvironment, and interpret the accumulating evidence for hominid presence securely within a framework informed by the earth sciences and biology. Working in the Omo, Howell’s group found numerous teeth, several jaws, and a few other fossils attributed to Australopithecus and Homo. These finds were soon overshadowed by the more spectacular discoveries of complete skulls and partial skeletons from Koobi Fora, but Howell’s efforts paid off in other ways. With his colleagues, he contributed to several books and many articles detailing the record for this key region of East Africa. Such systematic treatment set the standard for the discipline. Indeed, it is fair to say that Howell defined a path that has been taken by newer generations of researchers, working not only in the Turkana Basin and other parts of Africa, but also in Europe and the Far East. Clark Howell was always generous with advice to his students and associates, and his words of wisdom helped to stabilize a volatile profession. This voice will be missed.

G.P.R.
Thanks to his research, great knowledge, and publications, Jørgen Meldgaard—who died in Copenhagen in the Spring of 2007 just after his 80th birthday—fits admirably into the long series of such leading 20th century Danish Eskimo archaeologists and ethnologists as Kaj Birket-Smith, Erik Holtved, Eigil Knuth, Helge Larsen, Therkel Mathiassen, and others. He knew many of them personally. He received their support, and it was they with whom he worked in the field or on evaluations and publications. The writer was privileged to accompany Jørgen on his first trip to Greenland, and on his last as well. In the context of one of Eigil Knuth’s Pearyland Expeditions, we were entrusted in 1948 with continuing and bringing to a close the excavation of an Eskimo settlement on Clavering O near Dodemandsbugten in northeastern Greenland dating from the time between the 14th and the 18th century, which had been launched a few years earlier by Helge Larsen. In 1996 he showed me archaeological sites in the region of Disko Bay all the way up to Saqqaq. These two Greenland trips bracket almost half a century of close friendship and collaboration highlighted again and again by personal contact in Denmark or Switzerland. It is a token of his impressive and likeable humility that I was unaware of very many of his achievements, successes and awards. Some were only brought to my attention by the 1997 Festschrift in his honor (Gilberg and Gullov 1997) and the necrology written by Danish colleagues (Appelt et. al 2007).

Jørgen Meldgaard was born on Jutland and spent most of his youth—including the war years—on the island of Fyn. In 1945, as soon as the German occupation of Denmark had come to an end, he began his studies in prehistoric archaeology at Copenhagen University. This led to contact with the Department of Ethnography at the Danish National Museum—heeded at the time by Birket-Smith—where his skills and interests were quickly recognized. By 1950, Helge Larsen took him along to Alaska to continue excavations in the Trail Creek Caves in the interior of Seward Peninsula, which provided specimens of the Denbigh Flint Complex, and to investigate an Ipiutak house near Deering. Two years later, he participated, with Helge Larsen and others, in the investigation of the Sermermiut site in the Disko Bay area which provided stratigraphic proof of the sequence of Saqqaq, Dorset and Thule cultures for western Greenland. After a stay at McGill University in Montreal, he completed his studies at Aarhus University in 1953 with a dissertation in the field of European prehistory. Soon thereafter, in 1954, he made significant discoveries in the region of the Igloolik Islands in the eastern part of the Fury and Heela Strait which separates Baffin Island from the Canadian mainland.

In 1959, at the age of 32, Meldgaard was appointed Curator of Eskimo Collections at the Danish National Museum. Until his retirement in 1997, this remained the base from which he continued his varied and successful activity as an archaeologist in Arctic and Subarctic regions. The fact that his research was not exclusively devoted to Eskimo archaeology is indicated, for example, by his initiative to excavate Thjolhild’s Church, the oldest Christian church within the Norman Osterbygd Colony in southwestern Greenland; it had been built by Eric the Red’s wife Thjohild, who converted to Catholicism, and stood near his Brattahild farm. Elsewhere, too, Meldgaard occasionally took an interest in problems of Norman immigrants and their relationship with the Eskimos; he even traced them as far as “Vinland”. Only once did Meldgaard leave the Arctic region: in 1962/63 when directing Danish research in Iraq. Otherwise, he remained faithful to his “primary countries” north of the Arctic Circle all of his life, participating, for example, in the evaluation and publication of the mummies discovered at Qilakitsoq on the Nuussuaq Peninsula in western Greenland and as well in the establishment of a Greenland Museum in Nuuk, Greenland’s capital city (formerly Godthab). In 1979/80 he organized a “Knud Rasmussen Memorial Expedition” to Melville Bay, and in 1992 he saw to it in Thule in northern Greenland that the remains of a small group of Polar Eskimos were returned and buried. They had been taken to New York by Robert E. Peary toward the end of the 19th century and had died there. It should also be mentioned that he had a long history of interest in artifact reaching far back in the Eskimo past, which is now experiencing a new flowering among today’s Inuit.

In closing, it deserves to be noted that Jørgen Meldgaard not only received numerous distinguished invitations to attend conferences and give lectures abroad, but that he also received a number of awards which show how greatly his achievements as a scholar and expert of Inuit Eskimo past and
present were, and still are, appreciated: It began with the Loubed Prize awarded by the Royal Swedish Academy for History; this was followed in 1976 by the Hans Egede Medal from the Royal Danish Geographical Society; by the Greenland Home Rule Decoration in 1997; and, in 2003, by the Erik Westerby Prize of the National Museum in Copenhagen, the highest decoration ever awarded an archaeologist in Denmark.

H.-G.B.

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