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Review Articles

Just Like Us?

Village Life in Ancient Egypt: Laundry Lists and Love Songs, by A.G. McDowell, 1999. Oxford & New York (NY): Oxford University Press; ISBN 0-19-814998-0 hardback, £40, xvii+279 pp., 26 figs.

Barry Kemp

The village of Deir el-Medina is the best known and most intensively studied community from ancient Egypt. In its heyday (the 19th and 20th Dynasties, *c.* 1290–1070 BC) it consisted of around sixty-eight houses and families, tucked away in a side valley at western Thebes, within walking distance of the Valley of Kings. The heads of household were employed to quarry and decorate the rock-tombs of Egypt's kings and thus approximated to state employees. Two factors have ensured their immortality. One is the degree of preservation of normally perishable substances, unusual even for Egypt, which has led to the survival of a mass of papyri. The other is the habit that the community developed of supplementing the supply of papyrus with flakes of limestone and sherds of pots to an extent which, it is now clear, was eccentric for its time. The result is a uniquely large archive which documents the lives of the villagers in remarkable detail.

One must be careful in situating Deir el-Medina within Egyptian society. It had the size and physical separation of a village, yet by employment and wider contacts the inhabitants belonged to the conurbation that was ancient Thebes. A constant theme of Deir el-Medina studies is the difficulty in evaluating how far one can extend conclusions reached to the rest of New Kingdom society. Nonetheless, Deir el-Medina takes one as close as one can get to what it was like to live and think as an Egyptian. Whilst the scope for meaningful quantification is very restricted, and no one can seek by observation or cross-questioning information beyond what was written down, the sources predate anthropological accounts of pre-modern societies by three thousand years and are, of course, untainted by the presence of outside observers. It is by no means the earliest inner record of ancient Egyptian life, but it is by far the richest and most coherent. It is thus a good place from which to consider an intriguing question which evolutionary theorists are asking: is humanity still evolving? If it is the case that 'our evolution — particularly the evolution of our minds — is actually proceeding at an accelerating pace' (Wills 1998, 1–2), should we not be using the literate cultures of the past to investigate this, either by way of challenge or of clarification? A simple reading of the evolutionary claim is that the ancient Egyptians might have scored poorly in modern IQ tests.

McDowell's book is essentially a source book, an anthology of translated extracts, 197 in all, each accompanied by a brief explanation and bibliography. They are grouped by topic — Adultery, Production for Trade, Personal Piety, and so on — and chosen to illustrate the many-sided character of the texts and the community they portray. The author made her name with an authoritative study of the legal system at Deir el-Medina (1990), and has written other shorter contributions, all of which explore a particular theme. They are likely to be found, however, only in specialist libraries. *Village Life* is a bid to reach a wider audience. She has, all the same, held to a fairly uncompromising policy on translations, resisting the option of forcing them into easily readable modern idiom. Sometimes the texts are incomplete, or the meaning is elusive either because we do not know enough of the background or because the full range of nuances of their language is still not understood. By preserving some of these hesitations the reader is thereby introduced to some of the limits of inference.

The introductions to each text, and a general introductory chapter, provide straightforward background details. They assume that the sources, by and large, speak for themselves; that we have sufficient humanity in common with the people of Deir el-Medina not to need a dense anthropological text to mediate between us. The reader will draw upon a natural understanding to fill in, by intuition, the gaps in the documentation and so complete the story lines. The apparent transparency of many of the texts is on a par with many Bible stories which have, over

the centuries, been converted into local narratives by people from extraordinarily diverse cultures, mostly without the aid of modern scholarship. The account of the lady Naunakhte (McDowell's no. 14), in her will favouring her good children and not favouring her ungrateful children, reads like the raw material for a parable itself. But are we deluding ourselves as to the extent of the common ground?

My guess, and it is clearly the publisher's too, is that, up to a point, this direct engagement of the reader is successful. In their dealings with one another and with outside authority the villagers seem to be as intelligible as many communities from the pre-modern world who are closer to us and whose lives we can read about (the classic example being *Montaillou*: Le Roy Ladurie 1980), or, for that matter, communities that are closer still. 'If we want to capture something of the essence of working-class life in such a phrase, we must say that it is the "dense and concrete life", a life whose main stress is on the intimate, the sensory, the detailed and the personal' (Hoggart 1957, 88). Although written of working-class life in the north of England in the first half of the twentieth century, that and many similar observations echo lives from far earlier periods. The potential of universal experience was exploited in 1984 by John Romer, through a television series and a book based on the same Deir el-Medina sources, aptly titled *Ancient Lives*.

Recognition is helped by the fact that the villagers seem to have been generally unencumbered by elaborately constructed cultural constraints, such as lineage, family honour, the role of ancestors, or whom one married. Whilst archives and funerary monuments provided the families with links with their own past, in one of her other studies McDowell has concluded that 'on a day to day level, most workmen seem to have had a rather surprising lack of interest in their family backgrounds' (1992, 106). We meet the familiar contrast between conflict amongst individuals (much of Chapter 5, 'Law'), sometimes over property that can at times seem tiny in real value, and group solidarity against an outside threat (or at least irritant), illustrated by the demonstrations against failings in food distribution (nos. 187–90). The villagers seem often to have lived in reassuringly small family units. Extract no. 24 is from a village census which seems to list the occupants of each house, in which only one household had as many as five members, seven of the houses being occupied only by single men. This probably points less to low birthrate (the lady Naunakhte had eight children as heirs) than to the rapidity with which small independent households were established, almost certainly leading a proportion of the offspring to set up home elsewhere. The impression that the villagers lived according to a pragmatic, negotiated way of life which kept formal rules in the background is strengthened by the ethical teachings by which ancient Egyptians set great store and which were copied out and taught in Deir el-Medina (nos. 101–5). Religion did not lay down a path to be followed. In its place, scribes from the past or living heads of families promulgated a kind of godless morality, illustrated constantly by wordy figurative language which urged imitation of the man of good character: diligent, patient and reticent. (This might strike one as a contrast to a generalized impression one can form of a Deir el-Medina villager from many of the texts, as having been vocal and litigious.)

Most Deir el-Medina documents come from the relatively superficial level of day-to-day encounters with fellow villagers. They reveal little or nothing of the inner life of the writer. Buying a donkey, and disputing the quality of the donkey supplied (no. 47), does not take one far into the minds of the disputants, nor do more complex narratives of conflict and wrongdoing (nos. 146, 150–52), brought on by what appear to be easily comprehensible greed and jealousy. The documents shield us from their creators on account of their conciseness and reticent style. This is the case even when they are letters or personal reports on significant events (nos. 1–5, 187). A strict focus on matters in hand and a reluctance to comment on the broader significance of events and on the character and motives of individuals is also to be found in the archive of letters, only slightly earlier in date, which were exchanged between the courts of Egypt and of other rulers in the Near East, the Amarna Letters. This has prompted discussion of how complete a picture they provide of the writers' world. A recent collection of papers (Cohen & Westbrook 2000) summarizes the debate as being between 'primitivists' and 'modernists'. The former prefer to see the mental horizons of the writers defined by the letters themselves, so that international relations really were a matter of maintaining a 'brotherhood' among great rulers, largely through gift exchanges. The absence of analysis of the quite complex political situation which we can see was present implies that their mental connectedness was less than one would expect now. They and their advisers really did have the minds of archetypal villagers. The 'modernist' position is to accept that, despite a lack of verbal exploration, kings saw that there was also an underlying balance of power to be maintained by

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a variety of means, and thus that the letters do not reveal the full agenda. In the end the discussions are inconclusive for lack of other suitable historical sources by which comparisons could be made.

The same problem can be identified at Deir el-Medina. The periodic protests directed at local representatives of authority had as their spoken cause the late or incomplete payment of rations. If this were a modern situation, however, a thoughtful commentator would try to see if the protests, which sometimes seem disproportionately vigorous and could involve torchlight processions (no. 189), had deeper causes and if rations were being used to articulate other grievances or to achieve other ends. The fact that the reported speech, in the brief accounts that we have, is only about payments could be telling us that this, indeed, was the only issue; or it could be that the scribes who wrote the reports and belonged to the village, either did not perceive the background clearly enough, or did not see that report-writing need embrace a more discursive and speculative style. The 'primitivist' case then passes by default, but is not necessarily correct.

Although the uses of writing had come a long way since the Early Dynastic Period, it was still not a flexible and fluid means of expressing ideas and sensations. 'Love Songs', which make their first appearance in our sources at this time, manage sensuous imagery which translation still conveys (no. 110), but still it was the case that certain genres had developed and it evidently seemed natural to adhere to them. Each served a particular end and boundaries were crossed to only a limited extent. Letters and memoranda report speech, and are particularly valuable in this, but philosophize little and make literary reference very rarely. A father's letter to his son (no. 107), which is a piece of ethical instruction with quotes from some of the classics, is about as adventurous a piece of genre-mixing as one will find in the New Kingdom.

The limitations of using the sources as snapshots of ancient life are, to me at least, also illustrated by the procedures for resolving personal disputes at Deir el-Medina, by a village council whose proceedings sometimes included a kind of street theatre centred on a portable image of the village saint, the deified founder King Amenophis I. The written sources provide numerous accounts of individual instances, all written in a matter-of-fact style. Ancient pictures, drawn in the serene Egyptian manner, show the statue being carried sedately on the shoulders of the village elders who doubled as its priests. Modern scholars follow the style of the sources in presenting it as a calmly used judicial adjunct, calling it an 'oracle', and wondering how it could have been manipulated.

Some, perhaps many, of the occasions on which it was used seem not to have involved particularly contentious matters and so perhaps proceeded undemonstrably. But a dramatist might view some of the cases differently. When there were prospects of heavy thrashings with a stick, even of mutilations, of denunciations, of sanctioned raids on one's house, and of being handed over to higher authority for further examination outside the village (all meted out to women as well as to men) a far more charged and emotional atmosphere is suggested, bringing out a harsher, crueller side of life in a small and bounded community which lacked privacy. In this intoxicating combination of fear, possibilities of personal triumph and satisfaction at observing the utter humiliation of third parties, even of blood-letting, the scene was set for clamour from the crowd towards particular and perhaps rival verdicts. The participation of the saint's statue offered a route to release and resolution. Its inhibiting weight on the shoulders of the village elders perhaps gave them a sense of the weight of their responsibility, helped to concentrate their mental energy towards articulating a verdict, and bestowed on them a greater degree of self-control by which they could moderate the proceedings. All the same, in the end even the saint's decisions were challengeable (no. 129). When it was over the scribe salvaged the outlines of what had happened, wrote them as a verdict and obtained supporting signatures to verify it. At this moment the noise and the confusion vanished. If some of the participants had screamed in semi-coherence this was lost, and no scribe wrote down the actual injurious or even crippling effects of receiving a hundred severe blows with a stick (no. 142) to which branding and hard labour might be added (no. 149/182). We can suspect that our sources present us with a cleaned-up version of what happened.

McDowell includes a brief account (no. 98) of something which ought to feature in books on Great Archaeological Discoveries, the 'library' of Kenherkhepshef. Judged for intellectual content this counts as one of the 'wonderful things' from ancient Egypt. If it consistently fails the coffee-table test it is because it lacks arresting pictures, and perhaps the shameful circumstances of discovery have something to do with it, too. It was discovered by the Bruyère excavations in the cemetery at Deir el-Medina in 1928. 'During the night after their discovery, however, a substantial number of the papyri were stolen,

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to appear on the antiquities market soon afterwards' (p. 134). This fact was only admitted in print fifty years later (Černý & Posener 1978, viii), and led to a painstaking reconstruction of a list of what the library probably contained, which might not, of course, be complete (Pestman 1982). The list comprises around forty papyri, including the group bought at that time by Mr Chester Beatty and shortly afterwards, with one exception, presented by him and his wife to the British Museum (and, one should add, then published in a complete and sumptuous edition: Gardiner 1935; also 1931 for the remaining roll). Amongst the forty are to be found representatives of virtually all of the genres of ancient Egyptian literature, including temple ritual and mythology, as well as letters and accounts.

The library had been gathered piecemeal and treated to a mixture of careful and casual handling. Internal evidence takes the formation back at least to one of the Deir el-Medina scribes, Kenher-khepshef, who was born in the reign of Rameses II. More than a century later a stepson, Maanakhtuef, seems to have been the one who buried the collection. Males in the family were literate craftsmen, Maanakhtuef calling himself, for example, both a carpenter and a 'scribe' (literate person). At a crucial moment in its history the library must have been in the house of Kenher-khepshef's widow (now remarried), the lady Naunakhte, who has left the detailed instructions for the distribution to her children of her property (no. 14). It is noteworthy that the library is not mentioned, even though Maanakhtuef was one of her sons (by the second husband): perhaps it was already in the charge of his elder brother Amen-nakhte from whom he, in turn, would inherit them.

The variety and ordinariness of the means by which the collection had been gathered and subsequently used speaks for it not having been exceptional. Papyrus appears not to have been an expensive substance (a roll having the approximate value of a pair of sandals: Janssen 1975, 447–8). One could purchase scrolls already copied out, including illustrated versions of the Book of the Dead, although these could be significantly more expensive (Janssen 1975, 245–6). The stepson of Kenher-khepshef, Amen-nakhte, was given the beautifully written P. Chester Beatty I by a scribe from Thebes, Nakht-sebek. There is reference in a letter (no. 99) to another papyrus archive (which had been rained on!) which the descendants of the village scribe Amen-nakhte (a different man to Kenher-khepshef's stepson) kept stored in the village and its cemetery, even after the families had moved out. It is also likely that one or more similar libraries or deposits of papyri had been found by treasure-hunters in the nineteenth century, a good part of the contents finding its way to the Turin Egyptian Museum.

From these and from observations on other sources it has been estimated that around forty per cent of the workmen were literate, and so also was a proportion of the women (Janssen 1992, which McDowell cites with approval: p. 4). This is much higher than estimates of general literacy within ancient Egypt based on other and very flimsy evidence, which has given rise to the view that in this respect, as in others (the level of wealth, for example), Deir el-Medina was not typical, was even exceptional. I do not see how this can withstand critical scrutiny. Throughout New Kingdom Egypt there were long-running constructional projects which required levels of skill and artistic excellence equal to those needed for the tombs in the Valley of Kings, and sometimes on a larger scale, implying larger communities. Abydos is an example, which saw the development of its temple of Osiris and, more particularly, the construction of temples for the cult of kings, one of which, for Seti I, was one of that king's principal architectural undertakings. But the community of workmen and artists who must have been maintained here and presumably buried here has vanished virtually without trace. A single document tells us that, somewhere in the area, there was an oracle of Ahmose I, the predecessor of the Amenophis I who acted as Deir el-Medina's saint, but whether it served the workers' community or the adjacent town we cannot say. How can we possibly claim that Deir el-Medina was more literate or better-off than this community or any of the others of this kind which must have existed throughout the country? Deir el-Medina (in the Ramesside Period) is unique in its state of preservation into the modern era, and in its common substitution of flakes of stone and sherds for papyrus, but whether in other respects as well we simply cannot tell.

The alternative position is to be grateful for Deir el-Medina having provided us with a window on to the lives of a distinctive category of persons in ancient Egyptian society, those who were established within the bountiful embrace of the state, either directly or through family ties (principally marriage). The passport was the confident sense of belonging there and of having an attachment which brought some measure of paid responsibility and status. It was natural to want a degree of literacy, and fathers strongly urged their sons to acquire it. As a category it went beyond, perhaps quite a long way beyond,

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those whom we can identify as 'officials' properly speaking, for it embraced workers and craftsmen. What proportion of the country's population this represented in the New Kingdom again we cannot tell, but in the towns and cities, which seem themselves to have been agglomerations of villages, what good reason is there for denying a level of literacy which matched that at Deir el-Medina?

One obstacle which stands in the way of evaluating the library and other literary material found at Deir el-Medina is the Egyptians' own explicit view of the value of literacy. There was a whole genre of texts which drove home the message that being a scribe put one in control. Learning to read and write had the utilitarian purpose of securing the best jobs. One text from the library (no. 101A) celebrates famous authors from the past, but even here the purpose is to show that scribes have an advantage over the builders of monuments in obtaining immortality. These famous writers of old also provided the literate with figures of authority drawn from their own ranks. The practical work of scribes required the mastery of a limited range of set styles: letters to subordinates and to one's seniors, praises of the king, diaries and reports of events. These were not the places to display knowledge of literature or to make philosophical points. This is the genre adherence I have mentioned above. Yet the material used in scribal 'schools' included complex literary texts, some in the older form of language (Middle Egyptian), and Kenher-khepshef's library likewise embraces very diverse material. One has to conclude that, whilst perhaps for many a functionalist attitude to being literate was sufficient, a proportion of people at Deir el-Medina, and elsewhere took an interest in the literature of the present and of the past for its own sake, and that this was for personal edification since the uses of literacy allowed almost no scope for written commentaries and criticism.

The extent and variety of the literature of the time is impossible now to estimate. Anthologies of the principal literary texts written in Late Egyptian and thus of the Ramesside Period include around nine. It is alarming to realize that all of them bar one (Khonsu-em-heb and the Ghost, no. 109) are preserved only as a single copy, often incomplete. Two of them (The Contendings of Horus and Seth, and The Blinding of Truth, no. 108) come from Kenher-khepshef's library. If this had not survived, we would not have either of them. If one family of only medium means at Deir el-Medina could accumulate a diverse library, what were the libraries like which developed at court and other metropolitan centres? One does well to remember that Thebes (including Deir el-Medina), in supplying the bulk of the papyri that have survived from the New Kingdom, greatly biases our knowledge in favour of southern Egypt which, by this time, was increasingly a provincial part of the country (despite the status of Thebes as a ceremonial centre), a long way from from the northern region. It was in the north that the principal royal residence lay and where contact with the Mediterranean and the Near East was regular, and the population is likely to have been, by this time, heterogeneous in origin. The Amarna Letters themselves — another chance survival — in containing a number of Akkadian literary texts and evidence of an Akkadian-language school, point to one possible source of greater breadth (Izre'el 1997).

The mixture of sacred and secular texts in Kenher-khepshef's library also challenges the view that, in ancient Egypt, sacred knowledge was restricted either to a class of priests or to a narrow scribal élite. It was part of educated culture. P. Chester Beatty IX is the manual for the dawn, midday and evening rituals to be performed daily at the shrine of the village's principal god, the same deified king Amenophis I. It was not a special composition, but an adapted version of the standard daily temple ritual as used, for example, in the temple of Karnak. The ritual was, to the Egyptians, given logical backing by linking the ritual acts to the Horus-Seth-Osiris myth. It is not hard to find why a document which we might consider to be so sacred was in Kenher-khepshef's library: he would have been a part-time priest in the cult of Amenophis I. As if to emphasize the legitimacy of such material in the village, another archive found in the nineteenth century also contained a copy (no. 63). Evidently the scope was there in New Kingdom Egypt for people at the level of the urban craftsman and the not particularly influential scribe to acquire, read and work with the full range of available literature, no matter how sacred (in our eyes) it might have been.

There was another equally pressing reason to be able to read, namely, it gave you a degree of control over the supernatural forces which permeated the world and from which one could not insulate oneself. Egyptians found themselves born into a world where a pantheon of gods and goddesses were identified for them, who were responsible for the turbulence that was visible in wider upheavals and in personal misfortunes, including sickness, whose powers therefore had to be taken seriously but who did not need to be loved or even respected. At its heart lay the myth of Horus and Seth and, as sup-

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porting deities, Osiris and Isis. This provided a framework for positioning the dark side of the world: destruction, chaos, sickness and falsehood. The myth did not give it a proper origin, but took the optimistic view that its victories were only temporary. In the end, Horus triumphs. The myth is inextricably woven into the world of invisible power and fears: into the Ritual of Amenophis I, into spells against enemies, nightmares, and scorpion stings (nos. 84–7).

The gods acted with a degree of capriciousness or at least unpredictability yet could also be directed or constrained through knowledge and utterance. Any individual could grapple with them (though a knowledge of reading clearly helped). One could confer with others, as in no. 87, where one scribe from outside the Deir el-Medina community gives to the Deir el-Medina scribe a spell against poison. One could also consult a specialist, one group of whom were 'wise women'. In no. 83 Kenher-khepshef, himself the owner of magical texts, writes to a woman urging her to consult a Wise Woman about the death of two boys in her charge. He asks: 'was it their fate or was it their lot? And consult (about) them for me, and also see about my own life and the life of their mother. As for any god who will be (mentioned) to you afterwards, you will write to me about their name . . .'.

Particularly revealing is the Chester Beatty Dream Book (no. 81; Gardiner 1935, 9–23), which, despite its lamentably fragmentary condition, can be seen to have attempted a schemata of the human condition and to have tried to answer the question why people act in the way they do. The dream interpretations were in two groups, corresponding to whether individuals were counterparts ('followers') of Horus or Seth. Only fragments of the section which characterized the latter have survived, and this described a series of variants of the general type, identifiable by bundles of observable characteristics with estimates of their expected life-spans (sixty and eight-four years in the two preserved instances). Their marks (literally 'brands') included red hair (presumably paler pigmentation is meant), drunkenness, sexual prowess, coarseness of behaviour and general aggressiveness. They were, nonetheless, entitled to good dreams as well as bad although only a token portion of this part of the text has survived. It was presumably against this sense of predetermination of character that the scribal author of no. 101B gave the advice: 'Beware lest you say, "Every man is according to his character, ignorant and wise alike; destiny and fortune are graven on the character in the writing of god himself".' The sense of a world in the grip of polarities was also projected through calendars of lucky and unlucky days (no. 82), which ascribed a good or bad character to each of three daytime divisions of time.

As Hoggart (1957, 29) said of his northern community:

> The world of experience is mapped at every point, particularly closely at the great nodes, in two colours, into those things which 'mean good luck' and those which 'mean bad luck'. These divisions are invoked daily and automatically . . . Dreams are not to be ignored, not because they help to explain something in the past or indicate some hidden worry, but because they *fore*tell: and they 'go by contraries'; if you cry in a dream that means something pleasant.

It is right to point out that people in the twentieth century can hold superstitious beliefs in addition to a whole range of others.

> They do not intellectually examine them (superstitions); yet on certain occasions they laugh readily at them as 'old wives' tales'. But usually they take care to obey their directions . . . Change is very slow, and people are not troubled by inconsistencies; they believe and do not believe (Hoggart 1957, 30).

Lacking rationalist philosophies our Deir el-Medina people had no other direction in which to look. Or did they? There was the self-sufficient possibility of doubt and thus of relying upon intuitive values.

Another of the treasures of Kenher-khepshef's library is what has become known as 'The Contendings of Horus and Seth' (P. Chester Beatty I; not included by McDowell in her selection). This is the earliest version of the myth to have survived written out at any length (there are small fragments of a Middle Kingdom version: Parkinson 1991, 120–21). It presents it as a burlesque, in which the gods are quarrelsome, vulgar and open to ridicule. At one point the great company of Nine Gods (the Ennead), together with the sun-god Ra-Horakhty, reply to a letter from Osiris with pure sarcasm: 'If you had never existed, if you had never been born, barley and emmer wheat would still exist' (recto 15.2–3). 'It is amusing', wrote the original editor and translator of the text (Gardiner 1931, 9), 'to see how the Egyptian deities look when bereft of their hieratic poses'. Amusing it might be, but it also carries a far more profound message. The Contendings scoffs at the whole epic struggle, reducing it to a comic story. But Kenher-khepshef's library had something to put in its place. The story of the Blinding of Truth (no. 108) on another papyrus is basically a secular version of

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the same myth, but now, divested of its ridiculous deities, capable of being told in a serious and respectful tone.

Gardiner visualized the Contendings being recited 'by a village story-teller before a squatting circle of guffawing fellaheen' (1931, 11). By this somewhat demeaning statement he evidently sought to relegate the story to the periphery of Egyptian culture but this does not really fit what we know of its background: given as a beautifully written scroll from one scribe to another and preserved in a village which was not occupied by people who could properly be classed as fellahin. It is more appropriate to accept the Contendings as belonging to mainstream educated culture, the complexity of which is only occasionally revealed in the rarely preserved non-adminstrative papyri, but is implicit in a great deal of recorded behaviour.

By innumerable studies of the past and the present, historians and anthropologists have succeeded in mapping the diversity of human belief. What is often less apparent from their researches is the strength and consistency of those beliefs amongst the individuals who make up the communities in question. Is it perhaps normal to exist on the cusp of belief and disbelief, to have only partial engagement with the describable structure of one's own society and its beliefs? At Deir el-Medina some individuals responded, on occasions, very positively to the divine, developing a sense of personal piety (nos. 70–72). One man, another of Kenher-khepshef's stepsons who also bore the same name, claims a true devotee's attachment to sacred places (no. 72). But the spectrum of belief also embraced a sceptical, distanced view of the gods and their mythology. The 'oracle' which, on occasions, could deliver authoritative judgements could, on others, be seen as a fallible voice. The language of sycophantic subservience, which we find in most sources from ancient Egypt, was the price to be paid for the survival of a popular culture of disrespect.

The modern mind seems built around two characteristics: a capacity to doubt sometimes held with relish, and a mapping ability that can find pathways to link utterly diverse bodies of knowledge. Within the Egyptian mind doubt was present, although the opportunities for expressing it were very limited. But that seamless web of pathways criss-crossing through the mind, enabling each part to be reached from another and then compared, was still in a preliminary stage of formation. Would an educated ancient Egyptian, if brought back to life now, rejoin us after a short period of cultural rehabilitation and progress to passable IQ scores? There is reason to be sceptical but the debate has only just begun. *Village Life* is a good way to make a first acquaintance of a key source population. I wish it had been longer, in respect both of the number of extracts and in the commentaries, but this might have made it seem intimidating to those outside Egyptology, and informed outside comment is what the subject needs.

Barry Kemp
McDonald Institute for Archaeological Research
University of Cambridge
Downing Street
Cambridge
CB2 3ER

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Thinking Places

An Archaeology of Natural Places, by Richard Bradley, 2000. London: Routledge. ISBN 0-415-22150-1 paperback, £15.99 & US$24, xii+ 177 pp.

Christopher Tilley

In this book Richard Bradley sets out to examine the significance of 'natural places' in European prehistory. Natural places such as caves, rivers, springs, mountain peaks, rock formations, bogs, islands, waterfalls, coastal cliffs and islands are defined as being features of the landscape unaltered by human agency, part of nature rather than culture. Nevertheless, as his book so well demonstrates, they are of fundamental significance, and indeed may in many cases have been of much greater cultural salience than the 'non-natural places' (monuments and settlements) that archaeologists usually study. Here, of course, is the paradox: these natural places are simultaneously cultural monuments. The book immediately raises a fundamental theoretical issue which unfortunately lies dormant throughout: what is 'nature' and what is 'culture'? Is the division a useful one? Or does it block our understanding? There is a considerable body of literature demonstrating that the very concept of nature is a historically and culturally variable construction. An archaeology of natural places is therefore entirely anachronistic insofar as it is a product of modernist epistemologies. To us these places may be 'natural'; to the prehistoric populations they were *supernatural* places. What Bradley's book demonstrates is that 'natural places' create cultural landscapes and his work is really a study of the latter rather than the former: a continuum of relationships between unaltered topographic features of the landscape, imposed structural forms, spreads and depositions of artefacts, bodily dispositions and movements, perception and reception: people in places, and places in mind.

The book is a broad synthetic study discussing Saami offering places, Cretan peak sanctuaries and caves, rock carvings in Spain and Scandinavia, votive deposits and stone axe production quarries in Norway and England, and the relationships of monuments to the landscape. Overall it is slight on detail and rich in generalization. This is both the great strength and the weakness of the book, making it both a highly stimulating read and somewhat exasperating at the same time. Promised studies of the significance of natural places in the landscape all too often turn out to be bare outlines: notes and ideas which could, and should, be followed up further. Because the book is so wide-ranging and covers so many diverse landscapes and sets of archaeological materials in different parts of Europe, it represents more of a programme for future detailed study than a substantive discussion in itself. Ideas for about twenty research monographs are to be found in the book, and it is no mean feat that Bradley has provided them for us.

The principal groups of questions Bradley addresses are:

1. How were natural places used, and how significant were they in the past? How does their conceptualization alter interpretations of the landscape?
2. How did the activities at them compare with those associated with monuments, and how did monument building alter the significance of these places?

He rightly points out that virtually all discussions of the significance of unaltered places in the landscape have been inferred from tracing their relationships to monuments, and he shows how a much wider study of these places can be undertaken. The places where votive deposits, hoards, quarries and production sites occur in the landscape may indicate the significance of natural places to prehistoric populations, as may the locations of rock art. The problem in the literature has been that analysis has almost solely been undertaken in relation to the form and content of the remains (what kinds of things are deposited together in the bogs, what kinds of signs occur in the rock art, how axes were produced at the quarries) but their relationship to the landscape has been overlooked. Here, Bradley can claim to be opening out a whole new field of research that has many exciting possibilities. Of course, the potential significance of these places can be recognized archaeologically be-

cause of the presence of recognizable deposits or marks. Bradley's approach invites another even wider question: can we identify the significance of natural places where there are no artefacts, structures or carved or painted surfaces? Might absences be as significant as presences? This would seem to be a further area for research.

Another very significant point that Bradley makes throughout the book is that the origins of raw materials used to construct artefacts and monuments may be of vital importance. The meaning of an artefact or a monument may have a relational significance, referring to the specific places in the landscape from which these things were obtained. So, for example, part of the meaning and significance of the stone axes from the Langdale Fells derives from the characteristics of the quarry site itself. The Stonehenge bluestones derive part of their meaning from the rock outcrops of the Preseli hills where they were obtained. A megalithic tomb may 'draw together' significant elements in the landscape by incorporating different materials from different places. I have made similar points in relation to specific cases (Tilley 1996; 1999a, ch. 6). Bradley's excellent metaphorical generalization is: artefacts can be places, and monuments can be landscapes.

As regards the manner in which monument construction alters an understanding of the landscape, Bradley points out that, through time, it may be much more nuanced than I have suggested (Tilley 1994). Monument construction did not irrevocably alter the cultural significance of all 'natural places' from the Neolithic onwards. Processes by which natural features were culturally appropriated continued well into medieval times and, through time, old monuments may have been regarded in a similar way to 'natural' or (a term which I would prefer) ancestral places. Bradley makes these points particularly well. From them I want to make a further and more sweeping generalization. Throughout prehistory, the primary relationship of people to the landscape as expressed through architectural forms, or artefacts, was of a *mimetic* character. In other words, structures (either monumental or domestic) and artefacts imitated, or referenced the landscape in various ways. Such relationships are also documented world-wide through the ethnographic record. The other relationship to landscape, in which the role of architecture is to contrast with it, first arises in classical Greek architecture and the form of the Doric temple, and has come to dominate architecture in the West ever since. Here arises for the first time a separation between 'nature' and 'culture' that has become increasingly hardened and sedimented in our modernist thought. The rocks, bogs, rivers, artefacts and rock carvings, for us, are no longer living, they have lost their souls — and this is why we have such difficulty in trying to comprehend artefacts as places, monuments as landscapes, things as persons. In effect, Bradley invites us to escape from the conceptual limits imposed by our modernity.

I believe that both an archaeology of landscape, and of 'natural' places demands not only that we think in new ways (metaphorically: Tilley 1999a,b) but that we conduct research in new ways. I have termed this a phenomenological perspective (Tilley 1994). In essence what this demands is an embodied form of understanding and interpretation. Much of Bradley's book, in common with most landscape studies, represents what I would critically term 'paper landscapes'. In these paper landscapes knowledge is primarily acquired through maps and discourses, to varying degrees abstracted from knowledge acquired through being in place, understanding and interpreting things in place. Let me explain. Bradley discusses votive deposits as indicating the significance of 'natural' places (i.e. bogs) in the landscape, but these places become an abstracted category. What of their shapes, contours, configurations in relation to the surrounding landscape (even if these places are drained and destroyed, old maps could be put to good use in combination with field visits)? Could it be that the character of different bogs relates to what is found in them and where? Investigation of this might produce an entirely new kind of understanding of this category of 'natural' places. In relation to production sites, Bradley makes a series of excellent points about their often dramatic and inaccessible character. But if he had spent several weeks, or months, on the narrow ledges of the face of Pike o'Stickle, describing and thinking about the rocks and comparing and contrasting them with other production sites, I think he would be able to provide us with a much more nuanced and powerful understanding of these places and how they might relate to different styles and forms of artefacts. Similar points can be made about the other studies in the book. Here I think it is important to emphasize that this criticism is not a demand that Bradley be more 'subjective', i.e. convey a purely personalized response to place, but that in-depth subjective experience is the only way in which a more objective, i.e. rooted and grounded in what is 'out there', understanding can be reached. I am not, contrary to what Bradley appears to suggest (p. 42), particularly interested in conveying to others my

own personal experiences. The subjective and the personal should not be conflated, and are very different things. The former may lead to new observations and understandings, the latter manifests itself in the purple prose of narcissism.

Finally, I very much hope that this book becomes required reading for all archaeologists. We all owe a debt to Richard Bradley for having taken the time and trouble to write it.

Christopher Tilley
Department of Anthropology
University College London
Gower Street
London
WC1E 6BT

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A War of Words

Ancient Warfare: Archaeological Perspectives, edited by John Carman & Anthony Harding, 1999. Stroud: Sutton; ISBN 0-7509-1795-4 hardback, £25.00, US$44.95, viii + 279 pp., 68 figs.

Nick Thorpe

Warfare, and more generally conflict, is clearly a hot topic in both archaeology and anthropology, as the number of recent overviews and volumes of case studies attests (e.g. Keegan 1993; Redmond 1994; Reyna & Downs 1994; Keeley 1996; Martin & Frayer 1997; LeBlanc 1999). Not only is it a subject of great importance in its own right, but it touches on several other major issues, such as the attempt by evolutionary psychology to co-opt archaeological evidence, the history and biases of archaeology as a discipline, and the nature of archaeological evidence.

Ancient Warfare, a significant review of the current evidence, is dominated by specific studies, primarily of regions or periods of prehistoric Europe, with two papers concentrating their gaze even further on Neolithic enclosures in Macedonia and Britain. Consequently, the general rejection of sociobiological or evolutionary psychology as a fruitful approach to ancient conflict is largely implicit. Only the editors and Brothwell raise the popular notion that warfare is an ineradicable part of human nature — a view based on analogies with primate behaviour in which male-centred competition over access to females takes violent form (e.g. Wrangham & Peterson 1996). Unfortunately, this theory, based primarily on simplistic analogies with chimpanzee behaviour, produces an equally simplistic model which its proponents attempt to apply to all societies, regardless of context (Robarchek 1989). *Ancient Warfare* acts as a valuable corrective, with many of the papers seeking to situate past conflict within its social context.

Recent interest in ancient warfare has been stimulated by Keeley's polemical *War Before Civilization: the Myth of the Peaceful Savage* (1996). As the title states, he seeks to demolish the 'myth' which he believes has been peddled by archaeologists and anthropologists who have attempted to pacify the past. In a sense, *Ancient Warfare* can be seen as a response to Keeley. Although the conference from which it originates was held at the same time as his book appeared, the published version refers frequently to his work. References range from the approbatory, with Kristiansen describing it as 'inspirational', to the more dubious tone adopted by the editors in their introduction, who refer the reader to Ferguson's critical review (1997). Unfortunately, none of the contributors refer to the lengthier critique by Otterbein (1997). From an American perspective, with the dramatic change in view of the Maya from peace-loving to continually at war (Culbert 1988), Keeley's critique makes sense, but for a European audience, which has always seen the Bronze Age as a time of warriors, his overall case is far less persuasive.

The difficulty with Keeley's argument is that, although he rightly points to the high incidence of ethnographically recorded conflict and more formal inter-group warfare and makes a good case that this is not 'ritual' but real war, with a high level of casualties, this does not in itself imply much about ancient warfare. For here we are in the realm of interpretation. As Chapman notes in his thoughtful and substantial contribution to *Ancient Warfare*, Keeley's examples amount to a mere dozen cases over 300,000 years. Ferguson (1997) takes the weakness of the evidence to imply that the ethno-

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graphically recorded recent past is very different to the ancient past, and that the difference is due to the expanding imperialist frontier. A partial answer to this difficulty is to carry out a close assessment of the evidence for warfare during the period of initial contact, as Redmond (1994) has done so successfully for South America, and LeBlanc (1999) on a smaller scale for the Southwest United States of America.

As Carman & Harding note in their introduction, there are three primary lines of evidence for ancient warfare, each of which has its own particular difficulties. These are skeletal evidence of trauma, defended sites, and weapons. The most clearcut, at least initially, is skeletal evidence, as this does demonstrate that harm was actually done to one or more individuals. Even here, however, there are grey areas of interpretation. Thus Brothwell suggests that there may have been Neanderthal conflict on the basis of the frequency of traumatic injury, and suggests that these cannot simply be accidents. Other analysts, however, have concluded that Neanderthals suffered a higher level of trauma than other hominids because of their practice of short-range hunting with spears (Berger & Trinkaus 1995).

From later periods there are many examples of individuals with traumatic injuries, and rather fewer with injuries produced by projectiles. Of course, we must recognize the possibility that some of these are the product of hunting accidents. On the other hand, there is in the ethnographic literature a clear correlation between hunting and higher levels of conflict (Otterbein 1997), so the two clearly go together, with hunting presumably providing an invaluable training in aspects of small-scale warfare. It is certain that large numbers of deaths could be brought about by human conflict as early as the Mesolithic, most clearly in the case of Ofnet Cave in Bavaria, as discussed briefly by Vencl. (More substantial examinations of the evidence can be found in Frayer 1997 and Orschiedt 1998.) As in a minority of the papers, Vencl does not consider the wider social implications of the evidence, in this case the killing of some thirty-eight gatherer-hunters, which may have been the vast majority of the group. Instead he is more concerned to make a general case for warfare being pervasive during the Mesolithic and Neolithic.

A number of papers consider fortifications at length, most obviously Kokkinidou and Nikolaidou for Neolithic Macedonia, generally concluding that their primary role was defensive. In the case of the Macedonian sites, no supporting evidence is presented for the fortification interpretation; in particular there is no sign of skeletal injuries. This is a clear contrast to the *Linearbandkeramik* enclosures discussed by Vencl, where occasional sites do seem to produce war casualties from the ditch. However, Vencl, like Keeley (1996), goes too far in claiming that all Early Neolithic enclosures were essentially defensive. In his careful and persuasive paper, Mercer documents the evidence of violent death through arrowhead injury and the destruction of causewayed enclosures in Britain, most convincingly at Hambledon Hill in Dorset, where around 3500 BC this high-status settlement was successfully attacked and destroyed, leaving the bodies of two young adult males killed by arrowheads in the ditch. This is a rare case where multiple lines of evidence are brought to bear on a single instance of conflict. Significantly, Mercer refuses to generalize from Hambledon Hill to other enclosures, and argues that even at Hambledon Hill there are elements of the complex which were clearly not defensive.

As Chapman suggests, we need to move beyond debates over whether early enclosures were either symbolic boundaries or defended sites and instead revel in the ambiguity presented by particular sites. Certainly, any attempt to argue that enclosures are always defensive must founder on the rock of historical archaeology, for there are numerous periods in which enclosed settlement is the norm, but there is no case for them fulfilling a defensive role (e.g. Roman Britain: Hingley 1989).

Chapman also takes an interesting approach to the vexed question of weapons, proposing categories of Tool-Weapons and Weapon-Tools as well as weapons. Tool-Weapons would include the shoe-last axe, clearly used as a weapon at sites such as Talheim in Germany, where a mass grave was discovered (Wahl & König 1987), but from use-wear analysis known to be used as a wood-working tool on other occasions.

Only with the Bronze Age do we see the emergence of items such as swords which can only be 'used' as weapons; but they also held considerable symbolic significance. As Harding notes, taking a rather more conservative approach than some of the contributors, we cannot assume that weapons equal warfare. As Kristiansen documents at length, however, we seem to see the emergence of distinct warrior aristocracies marked by a panoply of offensive and defensive weaponry during the Bronze Age. It is good to note that they have both expanded on their contributions elsewhere (Kristiansen 1998; Harding 2000).

Neither Harding nor Kristiansen consider skeletal evidence at any length, and indeed there seems to be little to discuss. This brings us back to Britain,

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where the clearest evidence for warfare occurs in the Early Neolithic. The Early Bronze Age, which should see the emergence of warrior aristocrats, fails to provide examples either of skeletal trauma or of defended settlements. So did warrior aristocracies maintain their authority without fighting? As many of the contributors to *Ancient Warfare* argue, the next step in the study of early conflict is to situate warfare as part of society. Simply accumulating evidence to demonstrate that people have always been in conflict is no longer sufficient. Instead, we have to move to a more nuanced approach which recognizes that early war had its ritual and real aspects, and that it was not something set aside from the rest of social action, but an integral part of it.

Nick Thorpe
Department of Archaeology
School of Humanities
King Alfred's College
Winchester
SO22 4NR

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Of Matters Material

Matter, Materiality and Modern Culture, edited by Paul Graves-Brown, 2000. London: Routledge; ISBN 0415167051, paperback, £16.99 & US$27.99; ISBN 0415167043 hardback, £50 & US$85, 171 pp., ills.

Matthew Johnson

This collection of papers continues the concern with modern material culture studies evident in much of the literature of recent years; it sets itself the goal of reinscribing 'materiality' at the heart of this literature. I don't think it is fully successful in defining and rigorously addressing the question of materiality, but I do think it succeeds as an exciting collection of papers.

Recent work in modern material culture, the editor Paul Graves-Brown argues, has bypassed or ignored questions of materiality 'where the emphasis has been on post-modern word play and the culture of consumption' (p. 1). I feel this is a valid point. Archaeological and historical studies, of consumption in particular have tended to use the term 'consumption' as a catch-all without really investigating its multiple meanings; thus we know that we must look at much more than the common-sense production of artefacts, and that we must consider the social context of their use, but the analysis tends to stop at that point.

The major exception to this criticism is the work of Danny Miller, who has done most to examine the concept of consumption critically and to delve into its complex and problematic nature. The debt to Miller is acknowledged by Graves-Brown, who also

cites Judith Butler on the relationship between materiality and the feminist project. Yet limitations with both Miller and Butler are identified without being explored in any depth; I would have liked to see critiques of both these figures developed and deepened. As it is, the introduction is rather short, and the volume lacks a concluding discussion.

Inevitably, then, the central goal of the volume is only partly followed through in the contributed case studies. These case studies are mostly exciting and well thought-out pieces of work, but their relevance to the central theme of materiality is not always brought out as clearly as it might be. In one case (Latour) the paper is a translation of a 1993 article and in another (Schiffer) it is clear that the author cannot be in sympathy with the stated editorial position. As the inclusion of Latour and Schiffer indicates, the authors of the papers are from an eclectic and interdisciplinary group. The institutional affiliations given suggest four archaeologists, two sociologists/anthropologists, two psychologists and a philosopher among the contributors.

The papers, nevertheless, do form a coherent and stimulating collection. They fall into two broad groups: case studies, and more philosophical meditations. Inevitably these two groups blur into one another. For example, in the former group, Bruno Latour discusses 'the Berlin key', an unusual and counter-intuitive form of lock and key found in the former West Berlin; his paper is a witty and elegant demonstration of the difficulties of separating the technical from the social.

Michael Schiffer discusses how we might best explain the decline and disappearance of the electric car in the 1910s and 1920s. I found his argument, centred around the car's failure to reach a middle-class market, utterly convincing, and his discussion and rejection of 'indigenous theories' given retrospectively by informants, instructive. Schiffer introduced a more sensitive and analytical awareness of the importance of class and gender in attitudes to commodities; this careful differentiation should be noted by historical archaeologists of consumption in particular. I did not see, however, how he justified his claim that this analysis was 'grounded in the theories of behavioural archaeology' (p. 92). The article seemed merely excellent scholarship to me, scholarship that (aside from the name on the article) had little to do with the rather sterile writings emanating from Arizona. Three more case studies (Nash's analysis of Victorian cemeteries, Schofield's of popular culture and Graves-Brown on Princess Diana and the crash) seemed less satisfying than these two; though all raised pressing issues of modern material culture and its interpretation, in all three cases it was unclear how the studies said much that was new beyond the work of Tarlow, Hebdidge and Baudrillard specifically and 'cultural studies' in general.

In more philosophical vein, Beth Preston discusses the relationship between form and function, highlighting how archaeologists of all theoretical stripes have been quite unclear on this topic in the past. Tim Ingold looks at the definition and production of artefacts, pointing out that their definition is highly problematic (he uses the instructive question of how we might program a robot sent to Earth to recognize 'artefacts' and differentiate these from other objects). For Ingold, 'weaving the world' is not an activity readily divided into material and non-material categories. In the cases of both Preston and Ingold, there are implications that archaeologists need to consider very carefully. What are the implications, for example, for the ready division we make into 'ritual' and 'non-ritual' contexts? Breaking down the divide between material and non-material is also the concern of Williams & Costall, who use the example of autism to show how this divide has limited understanding of child psychology.

I came away from reading this volume not convinced that the central theme of materiality had been clearly and coherently argued through in all papers. At the same time, however, I came away excited and stimulated about the possibilities for more theoretically informed studies of modern material culture that explored themes of matter and materiality in ways that told us about contexts and topics in both modern present and historic and prehistoric past. Graves-Brown writes that 'the purpose of this enterprise . . . is to make the familiar unfamiliar, to lead the reader to take their day-to-day experience and re-examine how the things around them shape practice, and are used to shape practice' (p. 1). The volume succeeds in this aim. It is a stimulating collection of papers that archaeologists of all areas and periods will benefit from reading. They may not emerge with a coherent understanding of 'materiality', but they will be more sensitive to the interpretive possibilities and pitfalls embedded in any group of artefacts.

Matthew Johnson
Department of Archaeology
University of Durham
South Rd
Durham
DH1 4RJ

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Grahame Clarke in Context

World Prehistory: Studies in Memory of Grahame Clark, edited by John Coles, Robert Bewley & Paul Mellars, 1999. (Proceedings of the British Academy 99.) Oxford: Oxford University Press for the British Academy; ISBN 0-19-726196-5, hardback, £29.99, xiv + 246 pp., 49 ills., 8 colour plates.

Peter Bogucki

World Prehistory contains papers delivered at a conference 'Grahame Clark and World Prehistory' that took place at the British Academy in November 1997, two years after Clark's death. The purpose of this symposium was to celebrate Clark's contributions to the theme of world prehistory. Although no specific definition of 'world prehistory' is offered in this volume, let me attempt one for the uninitiated: 'the integrated global synthesis of human biocultural evolution'. Clark was one of the first professional archaeologists to move beyond national and regional archaeology as practised during the first half of the twentieth century and to attempt an authoritative prehistory of all inhabited continents. This was made possible by two major archaeological advances: the emergence of radiocarbon dating to provide a chronological framework unconstrained by regional typological sequences, and the coming of age of archaeological research in far-flung areas such as Australia, Africa, and the Americas.

In the 40 years since the appearance of the first of Clark's *World Prehistory* 'outline' books, others (including, recently, myself) have tried their hand at such an exercise and realize quickly how daunting a task it is. John Coles in this volume writes that 'world prehistory is now beyond the capabilities of any one person, and . . . any synthesis that tries to cover the world will, like a British Rail timetable, inevitably have gaps and missed connections'. I disagree with the first part of his statement, but I can say from experience that the second part is always true. To attempt such a global synthesis painfully exposes gaps in personal knowledge that were easy to suppress when eagerly signing a publishing contract. One also realizes that even if the data for any particular region are extensive, dozens of small civil wars rage in regional archaeological communities about the proper way to interpret them. The global synthesizer, inevitably an outsider to any such community other than his or her own, risks choosing sides in such disputes on very shaky grounds. A global synthesis is inevitably selective, so one always leaves out the site that someone somewhere considers to be critical to understanding human biocultural evolution, yet which, thanks to the author's professional judgement (or ignorance), does not appear even as a dot on a map. It must be done, nonetheless, both to integrate knowledge for professionals and to provide a coherent picture of past human experience for the public.

In addition to this global perspective, Clark had many other interests, and in the course of discussing his impact on world prehistory, the papers in this volume pick up other themes that were dear to Clark's heart. I have identified six main themes that the papers in the volume discuss, alongside biographical essays that assess Clark's impact on world prehistory. These include economic prehistory; environment; dating; Scandinavia, Antipodes/Africa/Asia; and 'psychic needs'. Let me explain each:

1. *Economic prehistory* — Clark's major contribution to archaeological interpretive method and theory, the idea that prehistoric society could be understood through integrated examination of subsistence, settlement, technology, and trade;
2. *Quaternary research* — a precondition for economic prehistory, in that the relationship between humans and their environment must be understood before one can understand subsistence, settlement, and technology;
3. *Absolute dating* — the techniques that permitted Clark's global syntheses of prehistory and a topic in which he had an intense interest;
4. *Scandinavia* — the area in which the rich archaeological record and the work of indigenous archaeologists provided such inspiration to Clark throughout his career;
5. *Antipodes, Africa, Asia* — areas in which Clark's travels and the placement of his students broadened his archaeological horizons from northern Europe;
6. *Psychic needs* — a term used by Clark in his 1975 *The Earlier Stone Age Settlement of Scandinavia* to describe the symbolic and spiritual aspects of human society that complement subsistence and technology.

The attention that the papers in this volume pay to these topics can be summarized in a chart (Table 1), which reflects only the content of the papers in this volume, not necessarily the research on which they are based. Several of the contributions are primarily biographical memoirs with particular emphases, and the paper by Hodder presents a discussion of a novel approach to archaeological method and interpretation, also one of Clark's major interests. In general, the papers cover Clark's interests very well, although

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it is surprising that none discusses the British Mesolithic, for it was the study of these materials that launched his career and where he subsequently undertook his landmark research at Star Carr. Despite the fact that this work was done in his own country, thus not 'global', it is important to understand how Clark's first decades of scholarly activity shaped his view of world prehistory later on.

A more interesting aspect of these papers is that so many of them earned an 'x' under 'psychic needs'. Parkington describes how the painted rock shelters of the western Cape of southern Africa constitute 'the symbolic marking of the landscape with highly socially charged paintings'; Higham discusses the rich burials at Khok Phanom Di in their regional context; Larsson considers the social significance of the mortuary rite at Skateholm and other Mesolithic cemeteries; Clottes argues that the paintings at Chauvet cave and elsewhere represent 'a common frame of beliefs that passed from generation to generation'; and Hodder notes a connection between different forms of art at Çatalhöyük and different social rhythms. Clark was not commonly known for his consideration of these human qualities, but Hodder points out that this impression is misleading in that Clark 'argued that social factors are both a consequence and a cause of economic change' in his 1939 book *Archaeology and Society*. Coles notes that at the time of his death Clark was 'planning another book, to be called *Man the Spiritual Primate*' which, to judge from the title, would presumably have addressed 'psychic needs' in greater detail.

The papers by Rowley-Conwy and Larsson focus specifically on the Scandinavian Stone Age, for Clark was one of the few non-Scandinavian archaeologists to become closely familiar with the archaeological record of Norway, Sweden, Denmark, and Finland. This region, an immense part of Europe, is generally absent from most recent surveys. When it does appear, Denmark and neighbouring Scania are often taken as representative of the much larger area, obscuring a remarkable amount of archaeological heterogeneity. For example, while southern Sweden was being inundated by rising sea levels, central and northern Sweden were rebounding from the relief of the ice burden, with important implications for prehistoric settlement. The fact that they are now often overlooked in the English-language literature is not lost on the archaeological communities in these countries, and it is time to recapture the appreciation that Clark had for the prehistoric societies of northern Europe.

I was especially pleased to see the paper by Louwe Kooijmans that begins, 'What would our picture of the North European prehistoric past be without the wonders of the wetlands, without the miracles of the mires, without the beauty of the bog bodies?' The research carried out over the last 25 years in the Rhine-Meuse delta, perhaps to a greater and more sustained degree than most other wetland projects in Europe, could be considered an intellectual heir to the Fenland Research Committee in its interdisciplinary study of an active Quaternary landscape, with the Leiden University mirroring the role of Cambridge in this story. Remarkable traces of late Mesolithic settlement and burials 6–10 metres below sea level at Hardinxveld hold great promise for refining our understanding of the latest foragers of this area.

The biographical memoirs each emphasize a

Table 1. *Topical coverage in* World Prehistory.

Author(s)	Economic prehistory	Quaternary research	Absolute dating	Scandinavia	Antipodes, Africa, Asia	'Psychic needs'
J.D. Clark	biographical memoir (global synthesizer)					
Wood & Collard			x		x	
Parkington	x	x			x	x
Jones		x	x		x	
Fagan	biographical memoir (Americas)					
Higham	x				x	x
Larsson	x	x	x	x		x
Louwe Kooijmans	x	x				
Rowley-Conwy	x	x	x	x		
Clottes			x			x
Hodder	archaeological method					x
Mulvaney	biographical memoir (including Australia)					
Coles	biographical memoir (career summary)					

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different aspect of Clark's contribution to archaeology. Fagan's essay on Clark's impact on American archaeology is particularly interesting, for he notes that Clark's emphasis on economy and settlement anticipated the so-called 'new archaeology' by about a decade. A crucial difference must be noted, however. Whereas the early 'new archaeology' tended to dehumanize its subjects into impersonal models and systems, Clark's emphasis on individual behaviour and social choice really anticipated the directions in which archaeology would evolve during the 1970s and beyond. Perhaps for that reason, so many of the contributors to this volume are able to place 'psychic needs' alongside subsistence and settlement so comfortably.

Permit me to close on a personal memoir, for Grahame Clark was part of my archaeological education, despite the fact that it occurred completely in the United States, with fieldwork in Poland, and without an opportunity to meet him. My Cambridge-educated undergraduate mentor at the University of Pennsylvania, Bernard Wailes, taught us to admire *Prehistoric Europe: the Economic Basis* as an example of looking at the past without regard for national boundaries or rigid chronological units. When I arrived at Harvard to do my Ph.D. in 1975, one of our first case studies in the seminar for novice archaeology graduate students involved an examination of the Fenland Research Committee and its impact. We started with the story of the trawler *Colinda* dredging up the moorlog from the bottom of the North Sea and the antler harpoon tumbling out, before going on to the collaboration of Godwin and Clark, and eventually to Star Carr. In that room, under the portraits of giants of American archaeology like Tozzer and Bowditch, we were made to understand the importance of environmental archaeology as practised in northern Europe in the 1930s and 1950s. Steve Williams, the Peabody Professor of American Archaeology, was a particular fan of Clark. A few years later, when taking down the name of a British colleague at a conference, I naturally wrote it down 'G-r-a-h-a-m-e', assuming that to be the only possible spelling, only to be corrected politely by *Graeme* Barker. In Poland, *Europa Przedhistoryczna — Podstawy Gospodarcze,* a translation of Clark's 1952 book, was widely cited and imitated. For example, Tadeusz Wiślański's 1969 *Podstawy Gospodarcze Plemion Neolitycznych w Polsce Południowo-Zachodniej* (*Economic Basis of Neolithic Tribes in NW Poland*) was a masterful synthesis of subsistence and settlement data that provided an important starting point for my dissertation. Only now in the hindsight of several decades do I realize how much personal intellectual debt I owe to Clark's work, and by evoking such personal reflection, this volume has clearly served its purpose in honouring his memory.

Peter Bogucki
School of Engineering and Applied Science
Princeton University
Princeton NJ 08544-5263
USA

The Sky is Not the Limit

Landscapes of Neolithic Ireland, by Gabriel Cooney, 2000. London: Routledge; ISBN 0-4151-6977-1, paperback, £17.99 & US$27.99; ISBN 0-4151-69776-3, hardback, £55 & US$85, xv+276 pp., many ills.

Cornelius Holtorf

Gabriel Cooney provides us with an innovative, interpretive approach to the Neolithic period in Ireland. After an introductory overview of both his theoretical perspective and the Irish Neolithic, subsequent chapters are devoted to symbolic and everyday landscapes; to houses, households and daily life; to the dead, the ancestors and monumental tombs; to the locations and meanings of these monuments in the landscape; to the 'life-histories' and meanings of Neolithic pottery and stone axes; and finally to stories and histories at the scales of a single monument of the Irish regions and of Europe as an entirety respectively. The book is well written, beautifully illustrated and has a particularly wonderful front cover design (to which I will come back). It is laudable that Cooney engages mostly with themes and issues rather than sites and objects for themselves. His text contributes to many recent debates among prehistorians of the British Isles and leaves culture-historical agendas of past generations of archaeologists far behind. Post-processualism has become 'normal science'. But this is not an excessively theoretical or esoteric book. *Landscapes of Neolithic Ireland* provides students not only with stimulating ideas, but also with a useful synthesis of the archaeology of Neolithic Ireland.

So persuasive is this book that I did not find a single point in the argument with which I would like to take issue, or significantly expand upon in this commentary. Instead, I would like to draw attention to a concern which struck me as in desperate need of both wider discussion and some creative solutions. I

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should say at the outset that although Cooney's book brought this problem forcefully back to my attention, it is in no way a problem peculiar to this book but rather one that concerns most, if not all, contemporary prehistoric archaeologists. The problem is one of two elementary dichotomies.

The first dichotomy is that of the meaningful landscape. Throughout the book, Cooney separates cultural from natural, and conceptual from physical landscapes as well as the sacred from the profane; the mundane from the spiritual; the ceremonial from the everyday; the ritual from the domestic; death and the ancestors from the living; the social from the economic aspects of life, and so on. Although Cooney clearly recognizes that 'the sacred is also there in every aspect of life, just as profane concerns may intrude on sacred activity' (p. 21) and tries to move continuously from one sphere to the other, the use of these oppositional terms means ultimately to ignore the manifold interconnections between both 'sides'. This is unfortunate as these very connections and overlaps are not only more significant for human societies, both in the past and in the present, but also more interesting than the dichotomous oppositions which may be long-established in Western thought but today appear more and more artificial and surreal. Every attempt to discuss the interconnections is half-hearted and ultimately unconvincing when the continued use of these oppositional terms stresses precisely the same dichotomies that the discussion aims to overcome. Terms such as sacred, profane, ritual and domestic are far too contaminated now to allow us to fine-tune their precise meaning. We may therefore, be in need of a new vocabulary and a way of thinking prehistory that transcends the old dichotomies (see Edmonds 1999!).

The second and (even) more fundamental dichotomy is that between past and present. Cooney assumes that, as archaeologists, our ambition is 'to regain' the 'Neolithic world' (pp. 90–91) and by trying to experience the archaeological features 'in the way that Neolithic people would have' (p. 91) 'to get as close as possible to the lived, complex reality' of prehistory (p. 5). In his view (and that of many others) we have to 'recover' lost meanings and 'reconstruct' what is lost in a three-step process, focusing in turn on (1) the data contained in the 'archaeological record', (2) its recovery, ordering and analysis, and (3) its ideally 'objective' interpretation. But this process is flawed with difficulties, as everybody realizes. Even in 'ideal' circumstances of good preservation, the successful application of sophisticated methodologies and available explanatory models of considerable plausibility, our language appears often to give statements more factual power than we think we can master. We feel uncomfortable about drawing conclusions that are all too meaningful and understandable to us today in order to make sense of something that ultimately is not. Many times, therefore, Cooney uses phrases like 'may have', 'would have', 'would appear', 'seems to', 'appears to', 'quite likely', 'seems likely', 'seems probable', 'possibility', and 'suggest'. Such phrases mediate between his felt uncertainties in the present and his felt respect for a past reality that exists in our imagination alone. Ultimately, however, they distract from our own visions and diminish the power of our interpretive understanding on which all our thinking and writing relies. I have used such phrases many times myself, and so have others, but I am increasingly dissatisfied with the way they discredit our perspectives and insights and keep pulling us back into the constraints of positivistic science, representational epistemologies, and cautionary scholarship. It has been argued that all the processes that make up archaeology are in fact based on particular interpretive processes in the present. Prehistory is therefore anything but 'very remote' (p. 212) and does not challenge us 'to radically adjust our perceptions and try to tune in with those people living in a very different world' (p. 21). We can only ever hope to understand the past in our own terms: is this not indeed the reason why we will never have finally accomplished our job? Cooney's book and its existing market as well as the sites and objects he discusses, the collections and records on which he draws, the associations in his mind and the terms he chooses for his written account, are proofs of the fact that the Neolithic is very much a part of our own present and a result of contemporary ways of perceiving and acting in the world. I agree with Cooney when he states that 'the archaeological record is the material expression of the behaviour and ideas of the people who created it' (p. 3) but surely the people referred to are the archaeologists themselves? The difficulty archaeologists have, in coping with archaeological understanding as clear and present interpretation, has nothing to do with inadequacies of the archaeological record and its analysis, or with flaws in existing archaeological approaches and in our faculties of imagination. It arises, instead, from the mistake of playing-down, rather than celebrating, the present-day perspective that our views and our writing necessarily express.

I have raised two concerns which both derive from insufficiencies of the language we tend to use

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for discussing the past. I see no obvious ways of avoiding the problems mentioned, other than avoiding this language altogether. As archaeologists, with an interest in the workings of material culture, we should know better than many that conveying meaning does not necessarily require words. Maybe we need to explore new modes of expression beyond conventional academic writing and find some confidence and sophistication in them before we can, perhaps, return to writing in a new, more adequate and more self-assured way. What is needed, I suggest, is an account of prehistory that does justice to the cover design of Cooney's book: the sky is *not* the limit. I do not know if and how this could function in our archaeological realities of the here and now. But I do feel strongly that right now our language is holding us back from going where archaeologists have not gone before and where, I suggest, the landscapes of Neolithic Ireland only just begin.

Cornelius Holtorf
Department of Archaeology
University of Cambridge
Downing Street
Cambridge
CB2 3DZ

Reference

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Social Worlds of Technology

The Social Dynamics of Technology: Practice, Politics and World Views, by Marcia-Anne Dobres & Christopher R. Hoffman, 1999. Washington (DC): Smithsonian Institution Press; ISBN 1-56098-909-2 hardback, £26.95 & US$45, 240pp., 28 ills.

Neil Brodie

Gordon Childe once argued that, from a purely instrumental perspective, many of the actions carried out during the manufacture of an artefact were unnecessary, and could be described as ideological delusions (1956, 171). Nevertheless, he continued, these 'delusions' may have had a real historical function, and it is this function, or functions, which the present volume explores.

The editors set out their agenda in the introduction. They adopt a broad definition of technology, as 'a web of social and material dynamics that together contribute to the making and remaking of society', thereby avoiding any conceptual distinction between the social and the technical while at the same time retaining an essential material aspect. One of their primary objectives is to introduce concepts of practice and agency into the study of technology, although they admit that not all contributors accept or agree with this objective. This makes for a confusing read and, if any of the contributors felt that theories of agency or practice were inappropriate for the task at hand, the book would have benefited from having their objections spelt out.

Indeed, the editors themselves might also have been more critical of their chosen approach. Agency theory can be construed as a description of present-day reality as viewed through a middle-class (?academic) lens, where knowledgeable agents proceed in conditions of reasonable (if imperfectly understood) opportunity. Less-privileged groups might choose to foreground a different aspect of their own reality where, in the absence of opportunity, knowledgeable practice counts for very little. This possibility leaves me suspicious of the entire theoretical project, unless it is recognized at the outset and clearly confronted.

Contextual objections aside, several of the book's authors resolve the dichotomy between structure and agency by arguing that 'social structures' may be given substance, or materialized, through the organization of a production process, and that these structures are then available to be transformed or reaffirmed by those involved. Thus the apparent 'structures' do not exist independently of technological practice. It follows from this that social identities are created, maintained and transformed through acts of production, which situate a person within the 'technological' and, thus, social web. Social worth and self-esteem are fused in the act of skilful production so that people are encouraged or coerced to act in the interest of individuals or institutions other than themselves, and what Pfaffenburger calls a moral conflict between the perceived needs of individuals or groups is resolved. Nevertheless, despite this role of technological practice in constructing a false consciousness, it also provides a field of engagement in which established social norms are opened to question, and by the performance of a technique can be accepted or challenged.

The concept of technical performance appears

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to form a bridge between these (and similar) practice-oriented studies of technology, and the 1980s discussions of artefact style. There is a shift in focus though, from artefact to artisan, and in his Foreword Ingold stresses the generative rather than expressive nature of technological practice, and argues that technical performance cannot be reduced to a social epiphenomenon — a communication of pre-existing information through the medium of non-functional variation of artefact design. Other authors, too, seek to distance themselves from this conceptualization of style as a medium of communication, but it oversimplifies the evolving views of those who wrote about style in the 1980s — the debate over the location and meaning of style was a productive one and is still relevant. It is easy to see how the distinction between active and passive style is pertinent to an agent-centred analysis of technology. Even if it is accepted that all technical acts are imbued with intention (albeit with unintended consequences), some may (with apologies to Orwell) be more expressive of intentionality than others. If this is the case, how can material axes of intentionality be recognized or described?

The attractions of agency theory for providing an account of everyday technological practice are clear, but it is less obvious that the same theory has much to say about episodes of abrupt or radical change. It is sobering to reflect that, during the time of Bourdieu's study in Kabylie, the potting style of one lowland Kabyle village disappeared when its potters adopted the novel style of a mountain village forcibly relocated into their vicinity (Balfet 1984, 293), and that an archaeologist, today, attempting to explain this change solely in terms of Bourdieu's (1977) theory of practice, would get it horribly wrong. Thus Larick, in his study of colonial architecture of New England, is right to insist that historical contingency cannot be overlooked. Similarly, it remains to be seen what shape an explicit 'agency of innovation' would take, if indeed it would differ from existing studies of innovation, and how it would explain the movement of innovative technological practices through space and time. A start is made here by Wake & Ridington, who describe the reception of foreign artefacts into the traditional technological repertoires of two native American groups, but the larger problems posed by the acceptance or rejection of new technological practices wait to be confronted and thus remain unresolved.

The passage from social theory or anthropological interpretation to archaeological practice is still not clearly charted, and this book offers no clear guidance. The two substantial archaeological contributions (Lechtman, Roux & Matarasso) are the outcome of long-established projects of archaeological or materials science research which owe little to agency theory. Roux & Matarasso describe a systems-oriented analysis of Harappan bead manufacture in which they utilize a hierarchical method to investigate what they term a technosystem, in this case of bead production, which allows them to develop hypotheses about the organization of craft production and even to speculate about the origins of the Harappan state.

Lechtman suggests that non-linguistic categories, what she terms ethnocategories, can be given form through technological practice, and argues convincingly that in the Pre-Columbian Andes the surface appearance of an artefact was a realization of its internal structure or essence which, she argues, is indicative of a more general conceptual structure, or world-view. (She also demonstrates in passing that archaeological investigations of technology are expensive in both time and resources — her conclusions are based on more than twenty years of archaeological, metallurgical and ethnohistoric research.) If Lechtman is correct, and she is certainly persuasive, her work presents a challenge to those who would read material culture as text. Non-linguistic categories have been given little attention in the archaeological (or anthropological) literature and their structure or expression are not understood and thus their interpretation and even recognition remain problematical.

From an archaeological perspective, each of the editors' own individual chapters is disappointing. After close study of harpoon barbs from three Palaeolithic Magdalenian sites Dobres suggests that the variation shown in the techniques of manufacture are evidence of individual agency, and that there was no single, normative 'Magdalenian' cognitive template to which all routinely adhered. Hoffman describes three examples of intentional breakage of metalwork, which he interprets as technical and thus social acts, and therefore inherently meaningful. But although both may have located evidence of technological agency it is not clear (to me at any rate) what new insights are gained.

This disappointment is caused perhaps by their mode of presentation. Many archaeologists, myself included, respond better to paradigmatic than to programmatic arguments. Lechtman, Roux & Matarasso present paradigms which might be followed by others wishing to emulate their work, while Dobres & Hoffman prefer to expand upon the context of their

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work, and thus leave little room for more detailed and substantive exposition. A more sustained paradigmatic explication of the use of agency theory for the study of technological practice, and particularly for technological change, might win over those who, again like myself, remain to be convinced of its utility.

Reading this book in Europe one is uneasily aware that it has been written, or at least collated, in North America. The editors collect together archaeological and anthropological chapters which serve well to illustrate the concepts and arguments under review, but fail — nor indeed set out — to offer a convincing archaeological methodology (or methodologies) with which to proceed. Nevertheless, the message presented for the archaeology is an optimistic one: that technological practice materializes what are otherwise ideational structures and concepts, and in so doing makes the social process available for archaeological investigation.

Neil Brodie
McDonald Institute for Archaeological Research
Downing Street
Cambridge
CB2 3ER

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Chaco Reconstructed

The Chaco Meridian: Centers of Political Power in the Ancient Southwest, by Stephen H. Lekson, 1999. Walnut Creek (CA): Altamira Press; ISBN 0-7619-9180-8 hardback, $62; ISBN: 0-7619-9181-6 paperback $23.95, 235 pp., 50 figs.

R. Gwinn Vivian

Stephen Lekson has a reputation as a *provocateur* in Southwestern archaeology, and *The Chaco Meridian* provides an opportunity to voice his concern with what he believes is the failure of many Southwestern archaeologists to stop 'endless fine-tuning' of the regional record. His proposed political history of a significantly expanded ancient Pueblo world (most of the southwestern United States, excluding the Hohokam, from *c.* AD 900–1450) is intended to make us think globally and escape the confines of 'feeble provincialism'. In this reconstruction, he traces the dynastic enterprises of a small (*c.* 1000) élite group whose three historically related and shifting capitals of Chaco, Aztec and Paquime spanned five centuries and a distance of 720 km (Aztec to Paquime).

Each capital was a 'near-urban' cluster of buildings, a 'ceremonial city' for a large surrounding region. The operational mechanics of these cities were initiated in Chaco Canyon near the centre of the San Juan Basin in northwestern New Mexico around AD 900 and characterized subsequent capitals in varying forms. These included the construction of 'Great Houses' as storage facilities, ritual public architecture and residences for a small élite and their retainers. Élite support came from farmers and craftspersons living in nearby small hamlets whose surpluses were stored in Great Houses for redistribution within the community and region. Agriculture was based on increasingly more dependable and technologically manipulated water sources at each capital. As the functional zone around each centre expanded, communities within the region were integrated through participation in a political-prestige economy based on West Mexican exotics including macaws and parrots, copper bells and shell.

The capital at Chaco climaxed in the early twelfth century and was followed by a new centre at Aztec on the Animas River in the northern San Juan Basin. Aztec functioned from approximately AD 1110 to 1275 when, like most Puebloan settlements in the northern San Juan Basin, it was abandoned. The earlier, relatively short (85 km) move of capital and power from Chaco to Aztec was dwarfed by the shift to Paquime (Casas Grandes) in northern Chihuahua, a distance of 720 km. The ceremonial precinct was smaller than both Chaco and Aztec, and the architecture was of puddled adobe — a transition presaged at Aztec where Aztec North is believed to be of adobe. Paquime began in the mid-thirteenth century and effective government ended by 1450. In an appendix, Lekson notes the meridianal alignment and historical position of Viejo Culiacan in Sinaloa with Paquime, Aztec and Chaco and concludes 'more research is necessary'.

But what of the research to support the historical reconstruction? Lekson advises that he has taken data developed by others, combined the arguments 'in a novel way' (p. 151), and supported his reinterpretation through 'circumstantial evidence' (p. 150).

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This evidence consists of five architectural elements including room-wide platforms (log shelves or beds at the end of, and across the short axis of some rooms), sandstone post-support discs, masonry or adobe colonnades, platform mounds, and triwalled or biwalled structures. None of these features is present in large numbers at any of the sites, and only room-wide platforms and sandstone discs are present at all three cities. Though architectural parallels are important, Lekson argues that the cardinal alignment of the three capitals is more critical because it demonstrates intentional legitimation of Aztec and Paquime by referencing their symbolic position *vis-à-vis* Chaco.

Lekson's historical reconstruction will generate controversy. Persons working in northern Chihuahua, southern New Mexico and Arizona will question the removal of Paquime from a late desert Mogollon tradition and its assignment to the Ancestral Puebloan world. My concerns are somewhat narrower and run the risk of being labelled 'fine-tuning'. Lekson tells us that he has borrowed from past research, but there are gaps in that borrowing as well as alternative interpretations of the empirical record.

Central to the premise of the Chaco Meridian is the concept of a small élite class residing in essentially non-residential Great Houses. To promote managerial efficiency, one would expect the élite to be concentrated — as at Paquime — in a central building. There are fifteen Great Houses in and near Chaco Canyon, though only eight are in Lekson's 'downtown Chaco'. All were occupied at the height of Chacoan power and contain more than 3000 rooms. Though all rooms are not fully contemporaneous, this represents an enormous expenditure of labour in an agriculturally marginal area for a handful of managers in each Great House. Lekson tends not to notice this problem.

He also believes that Great Houses were primarily non-residential. Why? They have all of the features characteristic of 'normal' Southwestern pueblos — contiguous room-blocks composed of door-connected room suites, plazas, kivas, and refuse mounds. Lekson, like others, points to the lack of fire pits in room floors (up to 80 per cent of rooms in excavated Great Houses lack this feature) as evidence of non-domestic use. The tightly controlled excavation of Pueblo Alto (Windes 1987) revealed 7 fire pits in the 15 rooms excavated, but 126 heating pits in the same rooms. Heating pits average half the volume of fire pits (10 vs. 20 litres) and may be plastered, but show less preparation than fire pits. Heating pits replace fire pits in wood-poor and winter-chilled Chaco Canyon, where special measures to extract the greatest benefit from a scarce resource would have been taken. Though of smaller volume, several heating pits could raise room temperatures equal to a fire pit, and their distribution in several places in a room was more efficient. Great Houses may well have been residential structures and not vacant ceremonial precincts.

Aztec, the second ceremonial city, is crucial to Lekson's thesis, because the Chacoan élite had to spend time some place before moving to Paquime in the late 1200s. Few Southwestern archaeologists deny that groups from Chaco Canyon relocated to the San Juan and Animas River valleys in the late 1000s and early 1100s. What is far less certain is Chacoan occupation of the Aztec complex after AD 1150. Based on his extensive excavations in the Aztec West Ruin, Earl Morris (1919; 1928) concluded that the building was constructed by Chacoans in the early 1100s, abandoned in the late 1100s, and then reoccupied in the mid-1200s by a population using classic Mesa Verde ceramics.

Morris' documented break in occupation of the West Ruin has not been confirmed at other Aztec Great Houses because they have not been excavated, but Lekson cites McKenna & Toll (1992) to support 'a continuous architectural history' (p. 75) from 1110–1275 at Aztec. Based on ceramic assemblages and a few non-cutting tree-ring dates, McKenna & Toll do postulate continued use of some buildings at Aztec by 'smaller populations' using various building styles. But the identity of the builders and the magnitude of construction is unclear. Lekson handles the problem of 'Mesa Verde replacement' by downgrading the importance of the Mesa Verde region ('a bit of a backwater', p. 103) and incorporating it within a larger and more prominent (powerful) 'Aztec region'. Thus, Mesa Verde becomes Aztec, and the identification of persons remodelling Great Houses is no longer a problem. Determining the magnitude of construction requires more refined dating of structures in the Aztec complex, but at this point data to confirm a second Chacoan capital at Aztec are extremely tenuous.

Lekson concludes his revised Puebloan history with brief notes on its implications for emergent order, cognitive evolution and archaeological methods. He compares the Chaco-Aztec-Paquime political structure with Anderson's (1994) concept of Mississipian political 'cycling' and concludes that Chaco was a 'remarkably clean example of "cycling . . ."' (p. 165) operating on greater geographic scales and longer schedules compared to Southeastern chiefdoms. Lekson attributes the long-term success of his Puebloan dynasty to the invention of abstract space

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that involved a shift from a human-scale, 'effective' concept of space to a 'created' spatial world that could enhance the prestige of persons using 'space and distance for political power' (p. 167).

Lekson notes his frustration with colleagues who ask for 'proof' to support his revised Puebloan history. I am frustrated by his admonition that 'we allow ourselves to see' (p. 173) this 'extraordinarily visible, knowable example of political continuity across time and tide' (p. 173) using only 'circumstantial, anecdotal, (and) juristic' evidence (p. 171). In 1994 Lekson, Linda Cordell and George Gumerman urged the building of 'a comparative archaeology of polities and their residual landscapes' that would include 'archaeologically-knowable empirical patterns of architecture, settlement, and region . . .' (p. 172). In 1999 Lekson observes that such an archaeology does not exist. Why not use the Chaco-Aztec-Paquime data to initiate just such an archaeology? *The Chaco Meridian* presents some of the data; it is time to develop a methodology worthy of testing those data.

R. Gwinn Vivian
Arizona State Museum
University of Arizona
Tucson, AZ 85721
USA

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