Reviews

Visionary Experience and the Archaeology of Rock Art

Shamanism and the Ancient Mind: a Cognitive Approach to Archaeology, by James L. Pearson, 2002. Walnut Creek (CA): AltaMira; ISBN 0-7591-0155-8 hardback, $69; ISBN 0-7591-0156-6 paperback $24.95, x + 195 pp., 16 ills.

Ancient Visions: Petroglyphs and Pictographs of the Wind River and Bighorn Country, Wyoming and Montana, by Julie E. Francis & Lawrence L. Loendorf, 2002. Salt Lake City (UT): University of Utah Press; ISBN 0-87480-692-5 hardback $35, xvi + 239 pp., 97 ills., 6 tables.

Christopher Chippindale

For good reasons most readers of *CAJ* are familiar with, a strong trend in recent archaeological fashion has been to explore how ancient people themselves understood and experienced their worlds. Pearson rightly identifies one impulse to this as a reaction against values inadvertently expressed in 'processual' approaches: the human experience there seems too much to be confined to a passive role, as people are pushed this way or that by the external controlling forces of ecology and economy. In telling that now-standard tale to start his book, Pearson usefully thinks in terms of a continuous range from less towards more 'processual' and 'post-processual' positions, rather than a simple 'battle of the giants' between two extremes. Then, defining his particular version of a 'cognitive archaeology', he stresses the research value of rock art, for these are ancient images which seem directly to express what it was that existed and seemed important in their world as ancient peoples knew it to be. In the last half of the book, he sketches why shamanism is to be considered as the characteristic kind of social knowledge that underlies rock art in the generality and therefore explains the forms rock art commonly takes. This is a remarkably strong claim, given how broadly rock art exists, and of what varied dates and in what varied social contexts. Some rock art is demonstrably not of that nature; much rock art does not seem to have the characteristics of visionary imagery, as some of us begin to think we might reliably recognize it.

Pearson presents his partisan view briefly and well, with verve and conviction. Like most broad surveys, his looks out to the world from a viewpoint at a certain place, in this case California; the key and commended research example is the remarkable work of David Whitley (coincidentally editor of the 'Archaeology of Religion' series to which this book belongs) in exploring far western US rock art in that framework. Critics are occasionally heard in their grumbly voices, and dismissed. Cognitive approaches to archaeology that do not embrace shamanism as the common frame of a broad explanation, or the specific field of rock art, are not part of the book's range.

For a fuller account of this work and first-hand views of the issues, the sympathetic reader might go rapidly on, or jump direct to major publications by the primary researchers; we are lucky that several of them have written at first-rate book length. David Lewis-Williams (2002a) who started this research approach has recently published a set of his selected papers, not just reprinted but revised with typically thoughtful introductions and commentaries as *A cosmos in stone* in the same AltaMira series as Pearson's book; his selection states essentials of his pioneering South African work, and then his application of it beyond that region. For the application of a visionary hypothesis to Palaeolithic art in Europe, there is a new and compelling treatment by Lewis-Williams alone (2002b): *The Mind in the Cave*. It complements Clottes & Lewis-Williams's shorter and superb *Shamans of Prehistory* (1998). For California, there is Whitley's own crisp and very well-illustrated *Art of the Shaman* (2000). No wonder Dr Pearson is such a persuaded enthusiast, and in turn persuasive, as witness Brian Fagan's endorsement in the foreword to his book. Usefully broad and well-balanced is also Neil Price's excellent edited *Archaeology of Shaman-*

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ism (2001), a reliable guide to the pertinent essentials: what is shamanism?; how, where and when may it be archaeologically expressed?; how, where and when may it be archaeologically visible?

Lewis-Williams's breakthrough was to discern clues to the meaning of South African rock art in nineteenth-century ethnohistoric transcripts of the stories of San people who chanced to be prisoners in Cape Town, people who did not come from a country with rock art but who were of the same broad cultural tradition as rock-artists. Theirs are difficult and obscure texts; the transcripts are in their original Khoisan language, a tongue which no one today knows, with the English translations alongside which were made at the time by the transcribers. So any reading has to struggle with old translations, never able to consult a native speaker of the language. Lewis-Williams has recently edited a good new selection of those translations in *Stories that Float from Afar* (2001), and another selection published long ago in specialized journal articles is to be reprinted. Two profound research difficulties follow:

First, how is one reliably to grasp the nature of the painters' visionary experience from texts which do not directly report it? In translation from an extinct language? And when the diligent transcribers and translators did not themselves grasp the stories in visionary terms? This is a specific issue with the San stories, but it may be echoed in how other ethnohistoric records came about and in how we rightly read them now. Reading *Stories that Float from Afar* is a sobering instruction in just how hard this task may be.

The second issue is more general. Although different visionary experiences may have much in common, they are experienced within a certain cultural context and expressed in fitting metaphors. If visionary experience is culturally understood as being like dying or like flying or like being underwater or like being stretched or like being transformed into an animal, will every image of, apparently, death or of a bird or a fruit-bat or a fish or a dolphin or a watery being or an elongated human figure or a human-cum-animal figure in rock art relate to visionary experience? Lewis-Williams & Thomas Dowson in their landmark paper, 'The signs of all times' (1988), reported there existed alongside the potential metaphoric subjects also a suite of distinctive geometric forms characteristic of what is also seen in trance, as the neuropsychologists record it: visionary rock art would have geometric and metaphoric aspects. But Dronfield (1995) has shown cause to worry in a study of Irish megalithic art: some shapes he found there to be diagnostic of visionary experience; just a couple are diagnostic of non-visionary geometries; some again are undiagnostic because not specific to either. And Chippindale *et al*. (2000) provide a worked example from archaic Australia where the researchers discover there seem to be visionary metaphors identifiable in a rock-art tradition which lacks any substantial geometric component of any shape.

How best do we go forward then through these enticing and dangerous rapids? It is not helpful to presume *all* rock art is likely to be visionary with Pearson's enthusiasm, any more than it is to listen to Paul Bahn, ubiquitous commentator on rock-art research, when he derides as a 'shamaniac' the researcher who finds that specifics of any one rock-art tradition suggest it is or may be visionary in whole or part.

The ideal research context to grasp the meaning of ancient rock art will be one which fulfils two conditions. First, there is a rich and reliable ethnohistoric understanding closely tied to the specifics of a rich rock-art corpus: so the materials exist for a good 'informed' knowledge of rock art in its later forms. Second, there is a long and rich preceding rock-art sequence: so the materials exist to survey and trace through more 'formal methods' how the late rock art came to take that form, and what earlier transformations it experienced. Practically nowhere provides those two conditions in a full-hearted way. The southern African San rock art seems to have no clear and sharp chronological resolution at all, despite recent work (e.g. Russell 2000): either it is very stable so the long term shows no perceptive shift or, as I would myself fear, nearly all which survives is very late and it has insubstantial time depth. For Palaeolithic Europe, of course, we have a long chronology (with its difficulties and disputes) but no direct ethnohistory whatever. It will be in those special regions lucky in both ethnohistory *and* chronology that we best can hope to understand series of specific events. From consistencies across those specific series we may then better understand, perhaps, such general pattern as may exist in rock art as a whole, or find a general pattern lacking. If there is a pattern, I anticipate it will show Pearson is wrong to think rock art in a ubiquitous generality is a visionary affair.

One of those special regions is the northwestern Plains of North America, subject of Keyser & Klassen's (2001) first-class regional study (reviewed in this issue of *CAJ*, p. 134). Within the broad area of that large zone is the Bighorn Basin country of Wyoming, subject of Francis & Loendorf's well-informed,

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wonderful and acute study. It is plainly written from the kind of solid knowledge which substantial fieldwork provides, fieldwork in which the contribution of Mike Bies and other colleagues is warmly acknowledged. It is well illustrated with good colour and monochrome photographs, and with exceptionally good line drawings by Linda Olson, and it is well published at a moderate price by the University of Utah Press.

The Bighorn Basin is on the extreme western edge of the northern Plains, where the environmental and cultural worlds of the Great Plains and of the Great Basin meet. It is known for the great pecked anthropomorphs of the 'Dinwoody tradition', huge figures with distorted heads and arms which to the book's authors, and to this reviewer, look more frightening than comic. Along with the Dinwoody anthropomorphs are many images of varied animals, shield-bearing warriors and other human or humanoid forms, and images done in varied techniques of pecking, of incising, of scratching, and of painting in single and multiple colours. The traditional knowledge of Shoshonean and Crow communities provides a strong local ethnographic base. Excavation below panels provides a robust archaeological context. Conventional and experimental dating techniques provide an independent chronology, even the experimental techniques proving, the way Francis & Loendorf see it, as consistent in their results as one might dare to hope for.

This range and combination of disparate material evidence is brought into a convincing synthesis whose broad base can surely be matched at present in very few places; among those, one will be Wardaman country in northern Australia where a similar research strategy chances to have been developed. Francis & Loendorf see the pertinent religious ideologies as complex and diverse; the people of these high arts are no more living lives of simple subsistence through their simple minds than are the indigenous Australians of Wardaman country. And — importantly for interest in shamanic matters — this is a region where the vision quest and certain kinds of relations to a well-understood spirit-world are known to be central religious elements.

After good expositions, of context, of ethnohistory, of image style and classification, and of the dating evidence, the longest chapter — nearly a third of the whole book — gives a persuasive account of the Dinwoody tradition and its singular imagery, relating it to vision quests, to Shoshone knowledge, and to a world structured in a tripartite cosmography. One motif in particular is identified as the Water Ghost Woman, the being who drags the unwary under to drown, and her spirit helper, Turtle.

Dinwoody figures are mostly on the west side of the Bighorn, towards the Great Basin and Shoshonean lands. The following chapter addresses the other and diverse traditions on the east side, which are well-presented in relation to the Plains traditions of that direction, in an analysis having much in common with Keyser & Klassen's broad regional framework.

Francis & Loendorf end with two good chapters of inferences and ruminations on method. Their closing remarks make a quiet statement of an audacious conclusion, a conclusion which if taken note of will make this book the landmark in North American archaeology it deserves to be. Through integrating rock art and the special kind of material evidence it offers, with ethnography and Native American perspectives, this study has enlivened and enriched the conventional archaeological evidence, the thin stones and bones, and equally it has shown how the thin ideas of a functional and materialist determinism can be made into a richer, a livelier, a more human and more persuasive understanding. A notable achievement expressed through a notable book.

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Archaeology, Gender and Death

Gender and the Archaeology of Death, edited by Bettina Arnold & Nancy L. Wicker, 2001. (Gender and Archaeology Series 2.) Lanham (MD): Altamira Press; ISBN 0-7591-0136-1 hardback, US$69.00; ISBN 0-7591-0137-X paperback, US$26.95, xxi + 203 pp., ills.

Sarah Tarlow

Arnold and Wicker's *Gender and the Archaeology of Death* is one of two edited volumes of papers from the fifth Gender and Archaeology Conference held at the University of Wisconsin, Milwaukee, in 1998. The organizers decided that, as the conference papers were too numerous to present in a single volume, and as a substantial number of them were concerned with mortuary evidence, they should be divided between a loosely-themed collection on gender archaeology (Wicker & Arnold 1999) and this volume, specifically about gender and death.

Like many volumes of conference proceedings, the content of *Gender and the Archaeology of Death* is patchy. If they had not been accepted for the conference, it is doubtful that all the papers that appear in this volume would have been accepted for publication elsewhere. Moreover, the editors' claims that this volume bridges the archaeological traditions of the Old World and the New, of historical and prehistoric archaeology suggests a more strategic editorial policy than is evident in the actual selection of papers: the Old World is represented by two Scandinavian, one British and one Chinese contributor; the New by eight contributors from the US.

The editors perceive a 'labour specialization' in the interpretation of gender in mortuary contexts, by which American scholars have provided the theory and Europeans applied this theory to particular contexts. This does not particularly resonate with my experience; there have been significant theoretical developments in this field in Europe, just as Americanist archaeologists have produced first-class contextual applications. This volume does not really succeed in correcting this alleged imbalance; rather, papers by scholars from both sides of the Atlantic suffer from weaknesses of theory and method.

This is not to say that the papers are of uniformly poor quality. There is some interesting and thoughtful material here. Eleanor Scott's analysis of the discourse surrounding infanticide in archaeological contexts concludes that no overarching theory of infanticide, gender and status will have explanatory force, given the variety of cultural meanings attached to the practice. Sandra Hollimon's discussion of the association of third and fourth gender categories with warfare among native North American Plains groups causes her to reflect on previous interpretations (including her own) of traumatic injury and to reassess gendered assumptions about the nature of warfare. Crass' discussion of gender among the Inuit provides fascinating examples of the possible fluidity of gender categories. By examining a society where a child may be known by the name or title of a deceased relative of either sex, where personal pronouns are not gendered and labour roles are open and flexible as far as concerns gender, we are encouraged to confront our own binary models of gender, and to question the universal significance of gender as a structuring principle.

These critiques remain implicit, however, in Crass' paper and are not borne out in most of the other contributions. Reading several of the papers in this volume one gets a general impression of not having moved very far from the approaches to mortuary studies, and to gender, that were prevalent about twenty years ago. The prehistoric, Americanist archaeology of death is still largely preoccupied with

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roles and statuses, and imaginative research is still often choked by an insistence on a hypothetico-deductive method which often means that some of the most interesting aspects of the data are not considered. Additionally, a number of papers suffer from unclear or unnecessary 'statistics'. Judy O'Gorman's paper, claiming to be 'a gendered view of Oneota social organization', illustrates a number of these problems.

O'Gorman cites a number of scholars to justify her founding assumption that 'gender is one of the structuring principles of every aspect of culture', and then undertakes a fairly traditional study of Oneota settlement and graves to look for incipient social inequality. Since she has already decided that gender is significant in everything, she then hypothesizes that women and men both had roles in creating relationships of social inequality, evident in variation between households in terms of, for example, storage facilities; and between burials in terms of number of grave goods and so on. Leaving aside the problematic assumption that status can be 'read off' from grave goods, O'Gorman's contribution is still fundamentally unsatisfying. She concludes that social inequality between households comes about as a result of some households being more successful in acquiring food resources. It is assumed that men did the hunting and women looked after the household's needs and therefore both men and women contributed to the development of social inequality: hypothesis proved. But what have we really learned? It is hard to find evidence here that gender is a significant factor in the process she examines; its centrality has simply been asserted and then 'proved' by the application of assumptions about roles. Even if true, this only tells us something rather bland and ultimately circular: first there was less inequality and then there was more; inequality had an economic base (of course it does if you use material 'wealth' as an indicator of inequality); men and women were involved.

Similarly, the two medieval papers, by Stalsberg and Graslund, have not moved far from the preoccupations of their fields a few decades ago. The former paper looks for relationships between ethnicity and status, basing analysis on grave goods, and simply adds gender to a familiar pattern of analysis whereby grave goods can be regarded as an index of ethnic, gender, status and age identity. The latter looks for the graves of powerful and high-status women with the aim, presumably, of proving that there were some. But surely powerful women have been identified in the Scandinavian Iron Age for some time — and emphasizing the political or economic success of a minority of women may, in any case, do no favours to women's history, a point made forcefully by Ross Samson nearly 15 years ago (Samson 1988).

There is actually plenty of far more challenging gender archaeology out there, just as there is plenty of imaginative yet rigorous archaeology of death. This book does not represent the pick of either crop. One might expect to see, for example, some recent work on the body, which has had an impact on both gender archaeology and the archaeology of death (e.g. Meskell 1999; Hamilakis *et al.* 2002). Another omission is any mention of work with human remains themselves. Of course, there are political reasons why the study of burials now tends to focus on spatial aspects and grave goods, especially in the United States, but the opportunity to conduct research on human remains still exists in many contexts and provides an important starting point for gendered social analysis (e.g. Hastorf 1992; Bell *et al*. 2001).

Themes in archaeological theory seem to date quickly. Symbolic meaning will forever be associated with the 1980s; landscape with the 1990s. Mortuary archaeology, despite much excellent critical work in the last two decades, remains closely associated with the processual approaches of the 1970s, especially in the US. Gender archaeology, on the other hand, and again in spite of significant earlier and later work, is mainly associated in people's minds with the trendy political archaeology of the 1980s. Arnold & Wicker's edited volume captures some of the difficulties of combining these two often incompatible approaches. Not all of the contributors take a processual approach to their mortuary archaeology, but the tenacity of approaches which look for role and status is evident.

In short then, this volume, although it has some useful and thought-provoking papers, also contains too much theoretically naïve and unedifying material. Gender archaeology is sufficiently developed now for us to insist on sophistication in our analyses. This also means that it is not adequate simply to assert the universal relevance of gender as a structuring category. Even when distinctions of gender appear to be made in burial, for example, other categories and statuses may be more significant (Eisner 1991; Meskell 1999). Instead, patterns can make a far more subtle point about gender. Some of the contributors to this volume are aware that sophisticated gender archaeology needs to go beyond reading mortuary practices as straightforward indices of fixed 'identities', but such approaches remain novel and largely unexplored in actual archaeological contexts.

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A Deep Look at the American Northwestern Plains

Plains Indian Rock Art, by James D. Keyser & Michael A. Klassen, 2001. (A Samuel and Althea Stroum Book.) Seattle (WA): University of Washington Press; ISBN 0-295-98094-X paperback, £14.52 & US$24.95, xii + 344 pp., many ills.

Polly Schaafsma

Plains Indian Rock Art is a landmark contribution to rock-art studies in North America. The first large-scale synthesis of the rock art on the Northwestern Plains, this book summarizes images carved and painted on stone over thousands of years from southern Alberta and Saskatchewan through Montana and Wyoming, east to the Dakotas. The text is clearly written, and information is systematically presented, qualities that make the book useful both to the general public and to scholars. Rock-art sites developed for the public, with travel directions, are described in a final chapter.

This is, as well, an attractive and well-designed volume. Clearly-rendered maps show tribal distributions and the geographic patterning of the ten rock-art traditions identified by the authors. Illustrated chronological charts for each tradition supplement the numerous drawings and black-and-white photographs located throughout the text. Drawings are sensitively rendered, accurately documenting nuances of style and differences in technique. The book is printed on a fine grade, bright white, low-gloss paper that further enhances the excellent quality of all of the visual material.

The text is divided into two parts. The first includes introductory material and background observations regarding the dating of rock art, interpretation, and discussion of the Northwestern Plains and its cultural aspects. Among the issues addressed are contextual considerations, distinctions between iconic and narrative modes of representation, and neuropsychological universals. The rock-art traditions to be described are firmly grounded in the archaeological and historical cultures of the Northwestern Plains well summarized in this introductory section. Included is an extremely useful chart of a generalized chronology of Northwestern Plains cultures, their associated artefacts, along with the rock-art traditions proposed.

The second part consists of chapters that delineate the ten rock-art traditions that Keyser & Klassen have identified in the area. Importantly, 'tradition' is the working organizational principle employed for this study. As conceived by the authors, 'tradition' is a rather flexible descriptive unit determined by well-defined criteria, which may incorporate a series of styles and/or temporal and spatial variants. Together, these rock-art traditions span an estimated time-frame of nearly 12,000 years. Chronologically overlapping from oldest to the most recent, they comprise the Early Hunting, Columbian Plateau, Dinwoody, En Toto Pecked, Pecked Abstract, Foothills Abstract, Hoofprint, Ceremonial, Biographic and the Vertical Series traditions.

Treatment of the rock art is careful and systematic. Each tradition is discussed in terms of previous research, a description of the rock art (kinds of figures: animal, human, material culture, compositional

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arrangements), dating and chronology, distribution and regional relationships, cultural affiliations, and interpretations. In some cases, chapter-ends include a two-page in-depth consideration of a particular panel or oral tradition that pertains to the imagery. In the last chapter of the volume, designated traditions are further grouped into 'macro-traditions', along with yet another useful chart illustrating this larger order. This is particularly noteworthy in the case of the Ceremonial-Biographic-Vertical Series macrotradition, distinctive to the Plains, that is, in turn, part of a larger pictorial Plains tradition that includes art made on perishable materials such as robes and in ledger books. Appropriately, they also consign a chapter to art on these other media.

One of the strengths of this volume is the careful and rather thorough consideration of multiple points of view. A diversity of opinions characterizes rock-art studies in general, and there are, as well, numerous unsettled issues. In this vein, it is one of the first rock-art volumes to give full consideration to chronometric dating techniques and their relevance to the rock art at hand. The information provided by chronometric dating is offered, nonetheless, with all the necessary caveats, noting that these techniques have yet to be fully validated. It does seem likely that some of the chronological schemes presented here will ultimately be modified to some degree.

As is commonly the case, one of the issues glossed over by Keyser & Klassen is the inconsistency presented by the absence of Pleistocene megafauna in an art tradition assigned beginning dates of *c.* 9000–10,000 BC, that otherwise features large animals such as bison, elk, mountain sheep and deer. While the validity of really early radiocarbon and cation ratio dates (i.e. *c.* 9000 BC) is questioned by Keyser & Klassen, and all the ambiguities are taken into consideration, they do not deal with the issue of the absence of megafauna and its implications.

It is well-established that rock art expresses the ideology and values of the artists of the authoring group, and Keyser & Klassen are committed to their statement that 'Interpreting (this) rock art requires a detailed familiarity with Plains Indian cultures' (p. 70). Comprehension of the fundamentals of Plains ideologies and values, aids in understanding rock imagery's deeper meanings and contextual significance. It follows that much of the rock art is viewed as documenting the existence of several vision-questing and shamanic traditions throughout the culture history of the Northwestern Plains. This consideration has broad support both from the data in the rock art itself, the situation of the rock art in outstanding landscape settings with oddly-eroded landforms, as well as in the continuity between late prehistoric rock art and the belief systems of historic Plains cultures. Fertility and hunting rites are among the themes present with shamanistic underpinnings. As in robe and ledger art, war records and the importance of personal status are themes in Biographic rock art that document a shift in values and concerns during the nineteenth century, as stress escalated between Indians on the Plains and the European incursion. Of note is the little-understood pictorial communication embedded in the Vertical Series Tradition.

There are a few minor challengeable points such as the spurious interpretation of a single bent figure holding a stick to its mouth as the Southwestern 'Kokopelli'. Also, the repeated idea — currently popular among several scholars — that the large body shield went into disuse soon after equestrian warfare was established is open to debate. This idea is thrown into doubt by the many portrayals on rocks and hides of pedestrian warriors with huge shields confronting horsemen, both in the Plains and in the Southwest. One of the more recent such scenes is dated to 1858 in Canyon de Chelly, Arizona.

A map that names the various physiographic features such as rivers, mountains, basins and so forth — all important contexts for the rock art — is lacking and would have been extremely useful. The absence of certain critical items in the index (the Avonlea complex being one example), the trend against including page numbers in most in-text references, the minimalization of in-text references in general, the absence in figure captions of general locations for numerous rock-art figures (granted, the text picks up some of these omissions), are aspects of 'streamlining' for appeal to a general public that is a bugaboo for scholars who need this information to carry out their research adequately.

These picayune issues do not detract significantly from the high quality of this finely-conceived, well-balanced, richly-textured volume that brings together for the first time an extraordinary amount of information on Northwestern Plains Indian rock art, and will stand as a valued foundation for future work on the Plains and a model for rock-art studies beyond these boundaries.

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Evaluating Evolution

Studying Human Origins: Disciplinary History and Epistemology, edited by Raymond Corbey & Wil Roebroeks, 2001. (Amsterdam Archaeological Series.) Amsterdam: Amsterdam University Press; ISBN 90-5356-464-0, hardback, £27.50, US$46 & EUR39.75, 174 pp., ills.

Terry Hopkinson

For more than twenty years, archaeological theory has explored the relationships between the social, cultural, political and institutional contexts within which archaeology and archaeologists operate, and the nature of archaeological knowledge about the past. Palaeoanthropology (that is, the disciplines of Palaeolithic archaeology and human evolutionary biology that together constitute the study of human origins) has, by contrast, been reluctant to examine itself in this way. And a darned good thing too, some would say. Theoretical discourse in archaeology over the last twenty years has spawned a swollen literature of polemic and dogma-driven interpretation, much of it dominated by a quasi-creationist world view in which the past exists only insofar as it is interpreted into existence by the archaeologist. That path few palaeoanthropologists wish to tread. None but a few doughty feminists have chosen to examine palaeoanthropology's past and present with serious critical intent. One might therefore ask whether, in avoiding philosophical narcissism, palaeoanthropology has in fact suffered a disabling failure of self-awareness. In *Studying Human Origins: Disciplinary History and Epistemology*, Corbey & Roebroeks have assembled a collection of essays that address this question.

The central issue is the value or otherwise of the *history* of human origins studies for palaeoanthropology now and in the future. Several contributors allege that the discipline's lack of critical interest in its own history is responsible for a deep-rooted tendency to produce dogmatic accounts of human origins that owe more to unexamined prejudices than to the real structure of the fossil and archaeological evidence. Dennell argues that before World War II, a combination of biogeographical 'centre of origin' evolutionary models, an *a priori* belief in the primacy of brain enlargement in human evolution and the entrenched racism of authorities like Sir Arthur Keith precluded the acceptance of Africa as a theatre of human evolution, despite the discovery there of early hominid remains in the 1920s and 30s. In similar vein, both Cartmill and Delisle attribute the resurgence in the 1980s of 'discontinuity' theories — in which 'modern humans' are accorded the status of fully-developed humanity, their 'archaic' predecessors are placed squarely in the realm of the animals and the evolutionary space between the two, being traversed rapidly by a small, localized population, is virtually empty — to the unacknowledged impact of cladistic taxonomic methods. These authors therefore question the evidential basis for two key elements in recent accounts of human evolution: the exclusively African origin of the genus *Homo* (a consensus which Dennell presents as no less indebted to scientific prejudice than the pre-war preference for Asia), and the rapid emergence of truly 'modern' humanity.

This perspective is expressed most trenchantly by Murray. He argues that nineteenth-century 'scientific' accounts of human origins were inspired by *a priori* assumptions derived from progressivist social theory and supported by the use of ethnographic analogy, and were in reality immune to scientific scrutiny or testing. Although advances in absolute dating and taphonomic studies have removed the causes of this disjunction between data and interpretation, the epistemological gap has become institutionalized in palaeoanthropological practice and can be closed only if its existence is exposed through a deliberate turn towards disciplinary history.

None of which is of any interest to Bowler or Theunissen. They are professional historians of science for whom the history of human origins studies is a branch of history to be investigated for its own sake. Neither is interested in history as an instrument for the improvement of scientific practice. Indeed, Theunissen thinks that histories conceived with that end in mind do not qualify as 'history' at all since the scientist's aim, even when doing history, is to transcend history in the pursuit of better science. Clearly, he is correct to point out that historians as 'outsiders' and palaeoanthropologists as 'insiders' have different agendas; but either way, it is an impoverished kind of history that denies the present. The point, for those of us interested in human origins, is that palaeoanthropology's past warns us against a complacent confidence in the legitimacy of our ideas and our methods of producing them.

Perhaps the most controversial contribution is that by Stoczkowski. His structuralist analysis of the history of hominization theories identifies a limited number of recurring concepts — brain growth, tool use, environment change and bipedalism, for example

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— that palaeoanthropologists have simply combined and recombined, using unacknowledged rules of structural transformation, to produce origin scenarios. The study of disciplinary history can therefore acquaint researchers better with this traditional matrix of conceptual components and uncover the rules of transformation that govern their recombination. Novel permutations can then be generated, especially if new rules of transformation are deliberately devised and applied. The resulting scenarios can be assessed for consilience with empirical data and those that do not fit discarded, leaving an enlarged pool of explanations that might correspond with reality. New facts are irrelevant; all that is necessary in order to say all that can possibly be said about hominization is a list of recurring concepts and a computer to recombine them *ad nauseam*.

Stoczkowski's is a depressingly pessimistic and profoundly unrealistic view of palaeoanthropology's mission. Fortunately, it does not go unchallenged. Bowler argues that there is in fact little evidence for real connections between structurally similar ideas in the history of palaeoanthropology. Instead, the recurrence of ideas represents no more than the limitations of the data and of logic. If one rejects Neanderthals as ancestors of modern humans then one is compelled to argue that they became extinct without issue. That this has been argued both by Boule and Keith in the early twentieth century and by today's proponents of a recent African origin for living people does not demonstrate any kind of direct historical relationship between them. Bowler's is therefore a much more optimistic perspective. It allows that the refinement of explanatory theory and the amassing of more data can, in principle, enable palaeoanthropology to advance towards more realistic accounts of human origins.

But, welcome as Bowler's optimism is, his denial of any real intellectual linkage across palaeoanthropological generations is hard to swallow. It is difficult to understand how a science founded on the demonstration of humanity's evolutionary continuity with the animals can cling to discontinuity theories of modern human origins unless one locates this in a deep-rooted Western value system that places humanity *as a category* irrevocably and absolutely outside the realm of the animals. In Cartmill's terms, we tend to police, rather than explore, the animal–human boundary. And all too often we do so, as Roebroeks & Corbey point out, through the application of entrenched double standards whereby the material traces of modern and archaic humans are interpreted according to different criteria. A prior, culturally-mediated commitment to the essential separateness of animals and people is thereby institutionalized and perpetuated within palaeoanthropology.

Van Reybrouck approaches this problem through an examination of another recurring theme, namely the use of analogies drawn from the modern world as models for early humans. He compares the late nineteenth-century use of 'primitive' peoples as models with the late twentieth-century preference for chimpanzees and identifies no evidence for any direct influence of the former on the latter; that is, there is no *conceptual* continuity between them. But the shared belief in the validity of modern analogies as representatives of extinct forms of humanity betrays a *discursive* continuity in that both are carried on the same intellectual current.

In the end, *Studying Human Origins* uses historical studies to identify problems rather more effectively than it offers solutions. There is no unanimity between the authors as to a single methodology that could liberate the study of human origins from *a priorism*. Between Clark's spirited but flawed defence of hypothesis-testing, and de Regt's mind-numbing and facile call for a return to strict philosophical empiricism, there is no meaningful common ground. In any case, things are really not as bad as the volume suggests. Neanderthals have been excluded from the ancestry of modern Europeans not only by dogma, but also by new evidence such as the very late Saint-Césaire Neanderthal skeleton and the analysis of DNA extracted from Neanderthal bone. The recent emergence, at least in Britain, of archaeologies of the Palaeolithic in which Pleistocene hominids are recognized as knowledgeable social agents shows that the animal–human dualism discussed above is not impervious to considered challenge. And it is worthy of note that of the contributors, all of whom are male, only Cartmill makes even a passing reference to any of the important historical and epistemological critiques of palaeoanthropology produced by feminist theorists. Nevertheless, *Studying Human Origins* issues a clear and well-founded warning: if palaeoanthropology is to avoid self-referential dogmatism and disinterest in the real past, it must embrace, not reject, a systematic and critical awareness of its practices past and present.

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Landscape and Rock Art

European Landscapes of Rock-art, edited by George Nash & Christopher Chippindale, 2002. London: Routledge; ISBN 0-415-25734-4 hardback £63.63 & US$100.00; ISBN 0-415-25735-2 paperback £19.99 & US$31.95, xvi + 218 pp., ills.

Christopher Tilley

Studies of rock art in relation to the landscape represent something of a new departure in rock-art studies and for this reason alone this book is to be welcomed. Most rock-art studies have previously concentrated on documenting and recording the images and attempting to decode their meanings in various ways. The argument of this book is that the significance of the images has to be understood in relation to place. Considering the images on their own, without reference to place, effectively decontextualizes them. Context and relationality are thus crucial. We need to be concerned with why certain rocks within specific landscape settings were chosen by prehistoric rock artists and others were ignored. What was special about them? From a broadly phenomenological perspective, which the editors claim to adopt, the following kinds of questions would naturally seem to arise:

1. Was it the shape, surface texture, colour, intrinsic characteristics such as cracks or fissure lines of the rock that were of significance?
2. Was it the particular relationship of the rock to others in the surrounding landscape that made it important? Was a carved rock significant, not in isolation, but only in relation to others with their own specific forms and characteristics in the immediate area surrounding it?
3. Was the landscape setting of a carved rock, or series of rocks, intimately related to the fact that it was carved? How might it relate to prominent local topographic features such as prominent hills, features of the coastline, river valleys, waterfalls, cliffs and overhangs, lakes etc.?
4. How does one's experience of the carved rocks change and alter as one approaches them from other carved rocks and different kinds of places, following different paths of movement? Are there specific groupings of designs in relation to different visual fields on a decorated rock or can one see them all at once? Can one see from one carved rock to another? How easy is it to approach a carved rock? What other kinds of rocks (carved or uncarved) does one have to pass, or clamber over, to reach it?
5. Going beyond vision, what do the rocks and the carvings feel like? Might changes in tactile sensations be important? What about the relationship between carved rocks and auditory dimensions of experience that can still be recorded in the surrounding landscape such as the sound of the near or distant sea or the movement of fresh water?
6. How do points 1–5 relate, if at all, to the specific characteristics of the rock art: the form, size, position, orientation and arrangements of designs on specific rocks?
7. Going beyond the decorated rocks themselves, how do these relate to the locations of other known cultural features of the landscape either contemporary with the rock art, or those which are earlier or later such as cairns and barrows, monuments and settlements, votive deposits, distributions of local or exotic artefacts etc.?

These kinds of research questions, some of which I am currently pursuing in my own research on landscape and rock art, are exciting and effectively promise to open up rock-art studies to a new type of inquiry which may permit new and exciting reinterpretations. I found this edited collection rather disappointing in that only three of the nine studies in the book even began to consider some of the types of research questions listed above. Furthermore, the book lacks any proper introduction. That which is provided is slight and very poorly written. Chapter 2 by Baker is an interesting account of grafitti on the Reichstag but quite why it has been included in a book otherwise entirely devoted to prehistoric rock art is not clear. Beckensall provides an overview of the main areas where British prehistoric rock art occurs. There is no interpretation or analysis here. We are, at least, told where it is and are provided with some references. Fossati claims that representations on some boulders and menhirs in Alpine Italy may represent real or mythical maps of the landscape. While describing in some detail the characteristics of the designs on these stones there is no analysis whatsoever of their landscape settings or associations. Frachetti and Chippindale consider the statue-stelae of north Italy considering their motifs and iconography and attempt to interpret them in an interesting manner in relation to concepts of seasonal, cyclical and linear time, but their relationship to the landscapes in which they are found is not considered at all. Ramqvist considers rock art in Fenno-Scandinavia, noting some regional differences in image content and distribution. This is basically a

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dots-on-maps approach to landscape involving no study of the locations of the sites. For northern Sweden he lists 17 painting sites and provides references. For each we are told what is there, e.g. 'Åbosjön, Sidensjö parish, Ångermanland: '1 elk, one unidentified image. On the vertical side of a large boulder situated in Lake Åbosjön' (p. 149). This represents the limits of Ramqvist's landscape analysis apart from some further distribution maps showing different frequencies of image types and different styles of painting. Søgnnes provides a brief literature review of motif types in central Scandinavian rock art but there is no analysis here either. A few broad generalizations are drawn as regards the distributions of sites and types of motifs that occur.

Fortunately, three chapters do make useful and interesting contributions to a study of rock art in the European landscape. Diaz-Andreu discusses Levantine and Schematic art in eastern Spain. She relates the presence of rock art in the Villar del Humo district to the spectacular geology, in particular the red sandstone that outcrops here. All the rock art is confined to this stone which, she suggests, must have been a sacred rock of particular significance. Limestone occurs elsewhere and is never decorated, despite the fact that the local ecology of the limestone and sandstone areas are very similar. The rock-art sites fall into two main groups: those with high visibility and those in more discrete locations. Those sites with high visibility share a common repertoire of motifs. Those less visible have more unique and unusual designs and may have been very special locales in the landscape. Interpretations of this pattern in relation to gender, hunting patterns, shamanism and group identities are discussed.

Nash considers Mesolithic rock paintings from the site of Tumlehed, western Sweden. He argues that a natural fissure cutting through the rock divides it into two ready-made panels relating to a binary opposition between wet (water -based designs, boat, waves and fish) and dry (red deer and net designs). This is claimed to represent a kind of map of the landscape incorporating ideas about the significance of marine and terrestrial resources for the local hunter-gatherer populations who painted it. Quite why the net design is designated as exclusively 'dry' (why not a fishing net?) is not explained. In addition a boat design occurs above the deer on the supposed 'dry' side of the panel. Quite why hunter-fisher-gatherers might need such a 'map' is not considered.

Purcell's chapter, the most detailed and valuable in the book, is about rock art in southwest Ireland. Here there is a detailed consideration of: i) what areas of the landscape are visible from the rock-art sites; ii) how much of the landscape can be seen; and iii) the degree of accessibility of the carvings. The analysis duplicates Bradley's conclusions in relation to British rock-art locations. Purcell argues that there are two main types of rock-art location. One group is found in open accessible areas along routeways, e.g. along river valleys. The other group is in inaccessible and often dangerous locations at viewing points overlooking routeways through the landscape and difficult to find without any prior knowledge. The visual field from such locations is often very specific and restricted. From nearby uncarved rocks, views over the landscape are frequently much wider. Unfortunately this analysis is not related to the form and content of the designs on the carved rocks apart from the observation that 'simple' and 'elaborate' compositions may be found in both types of locations.

In conclusion this is a book with considerable promise that largely fails to deliver a new perspective on the relationship between rock art and landscape. It does, however, point towards the future potential of such studies.

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Hunting for Patterns, Gathering the Data

Constructing Frames of Reference: an Analytical Method for Archaeological Theory Building Using Ethnographic and Environmental Data Sets, by Lewis R. Binford, 2001. Berkeley (CA): University of California Press; ISBN 0-520-22393-4 hardback, £52 & US$75, xx + 563 pp., numerous tables & ills.

Bruno David

In 1968, Lewis Binford announced archaeology's ambition to arrive at 'laws of cultural dynamics' (Binford 1968, 27). This programmatic pronounce-

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ment was to be repeated in archaeology textbooks for decades; university students for years to come would come to memorize this most ambitious of archaeology's potential aims. Yet investigating and finding general laws of human behaviour through archaeological research have remained elusive, and many archaeologists have contested even the wisdom of this aim. More than thirty years on — and despite a change of terminology — Binford himself now shows the feasibility of this most controversial of archaeological aims.

Constructing Frames of Reference is a massive work that has been over 30 years in the making. By 1971 Binford had already drafted an extensive set of observations about hunter-gatherers around the world. Over the ensuing years this list grew monumentally, so that by the beginning of the twenty-first century he had assembled an encyclopaedic compendium of field data on hunter-gatherers, allowing him systematically to enquire into the nature and reasoning behind general behavioural patterns, on a global scale. The result of this investigative vision is this book, self-reflectively presented not so much as an unfolding story as a play of ideas in many acts.

Binford's performance begins with a 'Prologue' that outlines the book's intellectual foundations, its genesis and subsequent history. This is followed by three parts, each with three chapters. Part 1, 'Exploring Prior Knowledge and Belief' concerns the intellectual antecedents of this book, theoretical debates on hunter-gatherer societies and behaviour, and Binford's own methodology for constructing scientific 'frames of reference' by which hunter-gatherer strategies can be systematically investigated (with implications for the interpretability of archaeological data). Part 2 ('Methods for Using Prior Knowledge: Building Frames of Reference and Models') explores the earth's climatic and biogeographic processes and patterns, and hunter-gatherer responses to environmental conditions around the world. Binford's aims here are to establish an environmental foundation upon which hunter-gatherer relationships to environmental variables can be better understood at a global scale. In Part 3 ('Recognizing Patterns and Generalizing about What the World is Like: the Transition from Pattern Recognition to Theory Building') Binford asks 'what is the world of hunter-gatherers like?', and explores hunter-gatherer system-state variability. Part 4 ('Putting Ideas, Second-Order Derivative Patterning, and Generalizations Together: Explorations in Theory Building') analyzes and integrates what has been learned about hunter-gatherer behaviour to better understand why hunter-gatherers behave the way they do. In particular, Binford asks how environmental change differentially affects social scales, adaptive organization and states of demographic packing among hunter-gatherers, and how these lead to the emergence of new, non-hunting and gathering societies. The book then concludes with an 'Epilogue' that summarizes and contextualizes his findings and methodology, and that opens new doors for further investigation.

One of the aims of *Constructing Frames of Reference* is 'to develop a method for productively using ethnographic data to serve archaeological learning goals'; a method that goes beyond direct ethnographic analogy to one that explores and identifies general patterns applicable cross-culturally. Binford is concerned here with behaviour as adaptive response, and with autocorrelations between behavioural modes and environmental conditions, and in so doing models the environment as a platform upon which correspondingly patterned human lifeways unfold. 'Hunter-gatherers' are from the onset identified as an adaptively behavioural category characteristically different from other modes of life. And herein lies the first problem: while there are many peoples who today, in the recent past or in the deeper past hunted, gathered and/or fished, is there really such a thing as a 'hunter-gatherer', a stereotypical and in various senses homogenized category of peoples whose cultures are, in the first instance, able to be understood by the way they obtain their food? At a processual level is the concept of 'hunter-gatherer' at all meaningful? Is the critical system-state that Binford is addressing in the first instance to do with 'hunter-gatherers' or with 'small-scale societies' (given his focus on demographic scale and degree of social integration in particular environmental settings for understanding social change)? These are questions that I do not think have been adequately addressed in this book, despite the central importance of 'hunter-gatherers' as an analytical concept.

A second problem is the effect of contact with non-hunter-gatherers on ethnographically documented hunter-gatherer behaviours — especially such things as disease, resistance and urbanization. How have these historical circumstances affected Binford's data on supposedly effective and biologically viable hunter-gatherer populations?

Third, hunter-gatherers are largely presented as passive extractors of a 'natural' environment, with little discussion of how these same environments have been modified and manipulated through various means such as burning and even planting. Binford writes of habitats, niches and evolution, not

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of history. As a consequence of presenting what appear to be 'normative' hunter-gatherers as organisms or populations filling ecological niches, there is little hint that people may behave very differently in exactly the same environmental settings. Yet there is an increasing (and in my mind at least, welcome) tension as this book progresses between, on the one hand, the modelled environmental conditioning to hunter-gatherer behaviour and, on the other, 'initial conditions' increasingly recognized as a key to understanding cultural variability: 'variability, when examined in greater detail, will also vary with initial founding conditions' (p. 441). These initial founding conditions comprise historically-contingent social practices that lie beyond food-related adaptive explanations.

Fourth, how have historically-related cultural practices contributed to patterning in Binford's modelled system-states? Binford argues that geographical patterns evident in the ethnographic data imply environmentally-conditioned system-states. Yet how do large clusters of historically-related, neighbouring social groups statistically affect geographical patterning? What is the role of history in creating the observed relationships between people and environments in the first place — how do large sets of historically-related social groups over-ride or predetermine geographical patterning in specific behavioural practices among hunter-gatherers? Are Binford's system-states a product of the historical geography of cognate states, rather than of environmental correlates? This is a problem of homology that presently remains unanswered.

Fifth, Binford demonstrates — rather successfully — that, diachronically, human system-states will vary under certain critical environmental circumstances. But it is not environmental conditions as such that will determine or even guide particular human behaviours. Rather, Binford demonstrates that system-states will react to levels of scalar stress, and in doing so result in an intensification of productivity, changes in mobility, and shifts in target resource habitats. This begs a discussion of the role of limiting factors to strategies of food production and social interaction. Unfortunately, the notion of limiting factor is not broached, despite its widespread employment in other cultural-evolutionary writings and its apparent importance to this book.

Last but not least are problems with the presentation of the tabled data. Here there are two difficulties, each of which independently hinder reading and interpretation of the data: a) the individual cases are not labelled on the point plots, making it difficult to follow or assess the plots' meaningfulness; and b) the reasoning behind the patterned shading on the plots is not always, and indeed seldom, clear; rarely do they appear to make statistical sense. Consequently, they distract from a reading of the point plots themselves, a problem compounded by this lack of labelling.

Having said this, I would also like to note that here, as elsewhere, Binford sets a high intellectual standard; it is therefore appropriate to conclude on a positive note. Aspects of cultural variability is what Binford is addressing, and it is this that anthropology needs to explain. Binford himself is most immediately concerned here with a methodology by which to understand better aspects of such variability and its temporal dynamics as found in the archaeological record, in particular the origins of agriculture. Yet it is not *change* that is at stake in this book, but *origins* — in that Binford searches for conditions *necessitating* a movement from one cultural system-state to another. There are many other contexts of change not broached here that could equally account for new cultural traits without venturing into the kinds of adaptive explanations so systematically investigated by Binford. I am thinking here not of the ultimate origins of, say, agriculture, but of its adoption once already in place by neighbouring groups. Such notions of adoption and associated socio-political power and influence are not necessarily problematic for the present thesis, for *Constructing Frames of Reference* does not claim to answer all causes of cultural change, but rather to address critical environmental conditions that demand scalar stress responses in cultural systems.

Despite the above shortfalls, in addressing the above aims *Constructing Frames of Reference* is an immensely important and ambitious book. Rare amongst archaeological writings, it systematically aims to understand *causality*. We are presented with a detailed, systematic demonstration of the effects of demographic packing to resource productivity, target species and habitats, mobility and scales of human interaction (both between people, and between people and their lived environments; although causes of population increase are themselves not systematically addressed). These effects have implications for understanding (necessary) shifts in food procurement and concomitant social strategies. This being so, the question will nevertheless always remain as to why any *particular* observed change in cultural practice has taken place. And it will also remain the archaeologist's role to investigate the nature of these changes, and to model their potential causes. But

armed with Binford's book and methodology, we are now more able to address the environmental constraints that people faced in the past, given the nature of cultural practices immediately preceding the observed changes. This surely will facilitate a more informed address of causes of change in food extraction strategies and in many other social practices.

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Are Memories Made of This?

Archaeologies of the Greek Past: Landscape, Monuments, and Memories, by Susan E. Alcock, 2002. (WB Stanford Memorial Lectures Series.) Cambridge: Cambridge University Press; ISBN 0-521-81355-7 hardback, £37.42 &US$60; ISBN 0-521-89000-4 paperback, £13.72 & US$22, xiii + 222 pp., 48 figs.

Robin Osborne

Hidden away at the end of the title, it is nevertheless 'memories' that lie at the centre of this book. Alcock has little time for those who object that memory is an individual matter, and that societies do not, and cannot, remember. Her response is that 'it is impossible to deny that social groups do share common memories' and that the dispute can be solved by admitting 'the existence of numerous "memory communities"' (p. 15). But since a very great deal rests on this apparently innocuous word 'memory' it is worth pausing longer. We talk most confidently of a group sharing common memories when we know that group to have shared a common (memorable) experience. When we use the term 'collective memory' to refer to shared 'knowledge' of events that were not experienced by the collectivity in question (as one might reckon European youth to have a collective 'memory' of the Second World War), then we are using 'memory' in an extended sense. What the collectivity remembers is not what it has experienced, but something of what it has been told. When we decide to term this 'memory', rather than 'knowledge' or 'belief', we are taking a political decision; we are claiming that the relationship between the collectivity and the past event is not merely academic but that that past event has become part of their shared experience, has ceased to be a set of facts and has become something which involves them morally and emotionally.

Alcock's own moral and emotional commitment to memory comes out very clearly in her discussion of whether the Messenian past was simply invented in the fourth century when the Messenian helots gained their independence from Sparta. Alcock finds such a claim offensive: 'Many historians have taken for granted that nothing links helot memories (if such things, indeed, are even allowed to have existed) with this newly crafted past. I have great misgivings about that supposition and, more particularly, about the grounds on which it is based' (p. 181, compare p. 173). For Alcock, the historian who believes no 'threads of memory' were involved in the re-creation of Messenian history is buying into the liquidation of the Messenians under Spartan rule: to deny the possibility that the villages of Messenian helots maintained some oral history is to be complicit in Spartan non-recognition of helot humanity.

In the face of such a claim it is well to focus on the way in which 'collective memories' are constructed. Like it or not, those of us born since 1945 can never experience the Second World War as those who lived through it experienced it. What we can have, and what we can hand on, is an account of the Second World War, one often even more heavily freighted with moral values than are the memories of those who lived through the war itself. For our 'memories' of what we have never experienced are still more liable to manipulation than our memories of what we have experienced. Calling shared beliefs 'collective memory' invests them with a moral claim, and asserts their centrality to the group identity. Calling those beliefs 'local traditions' or 'corporate mythology' disinvests them of that moral value, and may indeed be heard to suggest that the beliefs are without value or baseless. In the constant replaying of the ancient Greek debate about the respective roles of nature and of culture, 'collective memory' weighs

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in on the side of nature, tradition and mythology, even 'oral history', on the side of culture. Alcock is frightened of those who would take away a group's history; some of us are more frightened of those who would suggest that that any group has a moral right to any particular story.

Alcock's commitment to 'collective memory' is part of her post-colonial inheritance. For the Greece whose archaeology is here explored is not the Greece of Homer, of the tragedians, or of Athenian democracy, but a Greece conquered and oppressed. After a fine introduction to memory studies generally, Alcock provides three case studies in which she works back in time from Roman Greece of the first centuries CE, through the Crete whose warring cities of the Hellenistic period consolidated into fewer dominant urban centres in the Roman period, to the Messenians liberated from Spartan overlordship in the fourth century BC. Alcock's Greece is always responding to overlordship and turning to and from the past as a strategy for maintaining identity and forging resistance. To adapt the wonderful epigram from Whitmer (1993, 267) that heads her second chapter, hers is a world where people think the past perfect because they find the present tense.

What can be said on these same themes of landscape, monuments, and memories if we look forward? Greeks of 700 BC lived in a world much more lightly touched by the past than their successors 1000 years later. The epic tradition of the Homeric 'Catalogue of Ships' may involve some input from 'memories' of past geography, although archaeologists have disputed this. More generally the Homeric epics offer surprisingly little that could be attached to particular places, and the accounts of the genealogy of the gods and of mythic family lines given in poems preserved under the name of Hesiod provide even less that is locally specific. In 700 BC the landscape itself presented relatively few visible remains of past culture, and those that existed were only afforded special recognition in some areas — tomb cult is not universally distributed. The archaeological record yields various finds that have to be assumed to be heirlooms, but these are few in number and rather randomly scattered. For all that Hesiod can talk of past 'Races of Gold and Silver' to compare with the present 'Race of Iron', and Homer can make reference to the much greater strength of men of past generations, most Greeks of *c.* 700 BC did little with even the exiguous memorials of the past available to them.

Upon this base of ignorance about the past later 'memories' were built. The rich mythical geography of Greece that Pausanias records was created over the course of the archaic, classical and hellenistic periods, not handed down from the time of the monuments in question themselves. Notoriously Plutarch, around AD 100, can write a substantial 'life' of the Spartan Lycurgus about whom Herodotus in the fifth century BC could find out almost nothing. But alongside this imaginative reconstruction in their own image, Greeks of the classical and hellenistic periods also carefully orchestrated the memory of their own times. Just as Vibius Salutaris, in second-century AD Ephesus, as Alcock discusses, created a new ritual procession of statues of mythic and historic founders of the city and of the Roman emperor, so Athenians in the sixth century BC had created a processional route for the Panathenaic procession, a route which they then proceeded to line with carefully selected historically-significant monuments.

Looking forward, what one sees is not experiences not to be forgotten but traditions to be handed down, often reinforced by civic ritual. Properly to understand such traditions and rituals it is essential to see what was omitted, what was forgotten. The monumental remains of civic rituals show well what people wanted to shout about, but rarely what that shouting was drowning out. The closest the archaeologist can come to recovering the dynamic element is when priorities change and old monuments are destroyed or selectively allowed to decay. Yet here again the monuments themselves may give only a very selective history. Just as historians of the reformation have increasingly insisted that Catholic beliefs remained popular despite the virtually universal removal of the monumental signs of distinctively Catholic pieties, so we need to take seriously Alcock's observation that it was the élite families of Roman Greece who were behind the 'promulgation of the past' and that it may be the change in the interests of the élite families of Crete between the hellenistic and Roman periods that occasions the decay of many monumentalizings of the local past.

It is here that the claim embedded in 'memory' becomes positively offensive. When we talk of traditions being invented we have a shrewd idea about who they are being invented by. If we substitute 'collective memories' for 'invented traditions' we suggest that somehow we are gaining access to group experiences. The monuments constructed and manipulated to convey the desired view of the past may have been deployed as an act of cultural resistance, but they are deployed by the élite as a way of marshalling general support for their own stand. 'Memories' are a false trail: even when interpreted

as deftly as they are in this book, landscape and monuments do not enable us to get any closer to 'the people without history'. Talk of 'memory communities' does not solve the problem about 'memory', for the suggestion that all can be included contained in the plural is false.

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