

Chapter Two

Theory and Method

In remarks that are directly relevant to the central theme of this thesis, Christopher Scarre (1994) suggested that, whilst it is possible to investigate cognitive aspects of burials using archaeological data, it is not possible to get to the content and meaning of grave symbolism. One needs written records to do this. The problem, as Scarre described it, is, in part at least, one of definition: what do we mean by archaeology?

Although there are always multiple ways of dividing and categorizing any subject, a two-way division of the discipline of archaeology is discursively useful. I propose that practitioners of archaeology may be divided into two types: those who conceive of archaeology as the study of material culture, and those who conceive of the discipline as the study of the human past.

Those who believe archaeology to be the study of material culture vary greatly between those who expound complicated theoretical discussions of the concept of material culture and how objects interact with society, and those who study technical aspects of various classes of material items. Proponents of this material culture view follow (often implicitly) the idea that material culture can tell us something about the society that produced the material. I disagree with this view. It depends (again, often implicitly) on flawed ideas of induction. Material culture, on its own, cannot tell us anything about society and beliefs; it can tell us about only material culture. Indeed, studies of material culture often leave us with nothing besides questions that we need to employ other sources of data to answer.

The other view of archaeology, that it is the study of the human past, allows us to overcome the difficulties of naïve empiricism (the belief that explanations can be induced from ‘theory-free’ data). In this view, questions, rather than ‘objectively

collected' data, should inform research. All available sources of data, of whatever sort, should be (and are) drawn into the process of answering these questions. A single class of data, such as material culture, is not privileged simply on the basis of abundance. A practitioner of this approach is what Eugene Kolpakov (1993) and Gavin Lucas (2001:11) (following Walter Taylor [1948] and I. Rouse [1972]) called a 'prehistorian'—someone who collates a number of diverse sources of data relating to prehistory—as distinct from an 'archaeologist' (or 'archaeological technician' as Lucas [2001:11–12] described archaeologists) who collects primary data (see Chippindale 1988; Rowley-Conwy 2006 on the etymology of the term prehistorian).

There is, however, no need to adopt a term such as 'prehistorian'. Archaeologists simply need to focus less on material culture-centric approaches and embrace more diverse sources of data (in practice, many researchers do this, in spite of their 'material culture' rhetoric). This is not to say that we should abandon technical studies of material objects (be they 'material culture' or non-cultural remains). I simply suggest that such studies and approaches should not be a defining characteristic of our discipline. Unless we develop theory and methods (a) to make use of *all* available types of data and (b) to address larger anthropological questions we risk "finding [ourselves] classed with Aldous Huxley's figure who devoted his life to writing a history of the three-pronged fork" (Kluckhohn 1940:43).

If we understand archaeology to be purely the study of ancient material culture, Scarre is correct that researchers cannot get to the content and meaning of symbols. If, however, archaeology is seen as the study of the past, using all available sources of data, such questions may potentially be answered without the generation of spurious distinctions.

To use any source of data to come to an archaeological interpretation requires the use of appropriate theories and methods. It is useful here to distinguish between theories, methods and techniques. All three are difficult to define. Indeed, no less

a figure than Carl Hempel (1966:75) has gone as far as to state that “[t]he distinctive characteristics of a good scientific theory cannot be stated in very precise terms”. I am not going to presume to disagree. Instead, I present ‘working distinctions’ between the three concepts. I do not intend these to be rigorous, all inclusive definitions.

In this context, I consider ‘theory’ as an overarching framework of thought. Structuration is an example of a social theory. ‘Method’, on the other hand, is the form of argument that researchers use to reach a particular conclusion. It is method that the philosophers of science often debate. A deductive argument or an argument by analogy are examples of method. ‘Techniques’ are the things that researchers do. Statistical analyses and radiocarbon dating are examples. In this chapter I do not discuss the techniques I use. The tripartite division I have outlined is not merely pedantic, but is epistemologically valuable in discussions of the sort that follow.

Theory

Wolfgang Iser (2006:5–7) drew an important, and useful, distinction between what he characterized as ‘hard-core theory’ and ‘soft theory’. Hard-core theory, as used in physics for example, is data-free, aims at the generation of predictive laws, is testable and may be discarded if it does not stand up to testing. Soft theory of the sort used in the humanities and social sciences, on the other hand, “pieces together ‘observed’ data, elements drawn from different frameworks, and even combines presuppositions in order to gain access to the domain to be charted. This *bricolage* is adapted to what is scrutinized, and augmented by new viewpoints when required” (Iser 2006:5).

For archaeology, especially of the kind that this thesis addresses, this distinction is crucial. The type of theory used in interpretative archaeology is soft. Yet the concepts used in epistemological discussions of archaeological theory are usually

drawn from philosophical debates about theory in the physical sciences that use hard-core theory. A major disjunction therefore exists between the concept of theory and those theories actually used. The incorporation of explicit theory into archaeology (David Clarke's [1973] 'loss of innocence') was accelerated within the paradigm of New Archaeology, which modelled itself, epistemologically, on the physical sciences. A revised concept of theory has not been introduced into archaeological discussions in tandem with the introduction of soft theory.

The acceptance by many archaeologists of the importance of theory in their work is, of course, a major step forward from the early days of naïve empiricism. Phrases such as the 'theory-laden' nature of data have become commonplace. (This is, of course, not to say that many do not continue with the naïve empiricist data collection of Lucas's archaeological technicians.) There is, however, a danger that the pendulum has swung too far in the opposite direction (see also Dowson 2001:313).

In discussing the nature of archaeology, Ian Hodder (1999:9–15; following Boivin 1997:113, 114) pointed out that the discipline has become fragmented into a number of specialities: lithic analysis, micro-wear analysis, micro morphology specialists, and so forth. Members of each group use a specific jargon and write for others within that specialist area, and in many cases the specialist literature is largely unintelligible to specialists in other fields (Boivin 1997:113, 114; Hodder 1999:12). Along with other specialists, there are also now theory specialists, who write primarily for a specialist 'theory' audience (although some writers have mistakenly suggested that the "era of specialists in Theory with a capital T has probably passed", Pluciennik 2001:24). Whilst the existence of explicit theoretical discussion is valuable, the situation may cause serious problems: many researchers have moved from 'data without theory' to 'theory without data'. This tendency has the corollary that largely theoretical arguments are imposed on bodies of data, with little regard for how well they 'fit'. It is exactly this situation in anthropology that Adam Kuper (Fausto & Neiburg 2002:312) described as "the extreme relativist and anti-empirical kind of anthropology and ideological kind of

anthropology in which the agenda is set by other kinds of ideological issues—feminism, ethnic nationalism, or whatever.” I suggest that some archaeologists have now followed anthropology and are undertaking a kind of ‘ideological archaeology’ in which theoretical and often political concerns not only formulate questions, but also now determine interpretations—often in spite of the data (see Trigger [1984] on political and social influences on the development of nationalist, colonialist and imperialist archaeologies).

In these cases, it is *interpretations* that are applied to the data, not theories. As James Whitley (2002:120) remarked: “When students learn to ‘apply’ theory, what they actually ‘apply’ are . . . interpretations. . . . [S]uch all-purpose interpretations . . . circumvent the tedious business of undertaking contextual analysis, or testing specific models against the available evidence.” Such a tendency must be resisted. Theory is extremely important, but not *supremely* important. Whilst it is easy simply to collect data, it is equally easy to write reams of theoretical assertions that fall down when confronted with data.

Perhaps part of the problem is that method, discussed at such length by the New Archaeologists, has been largely ignored by post-processualists forever in a race to be at the cutting edge of ‘new theory’: theoretical arguments, formulated in abstract terms, may exist, but they cannot be convincingly articulated with data (but see Hodder 1999). As has so frequently been commented, theory becomes productive only when meaningfully articulated with data. To do so requires an appropriate method. I consider method in the following section.

The constant post-processual desire for ‘new theory’ has led to what Christopher Chippindale (1993) characterized as the ‘consumption’ of theory (he gave the word the double meaning of ‘using up’ and of a degenerative, wasting disease). He described the way in which post-processual archaeology adopts new theories, dotes on them for a while, and then discards them in favour of the ‘next big thing’ before they have had a chance to show any worth they may have.

The trend that Chippindale criticized is in line with broader trends in the humanities and social sciences. Because soft theories cannot be ‘tested’ or evaluated in the same rigorous ways as hard-core theories, they also cannot be discarded: a discipline ends up with multiple, competing theories (Iser 2006:7). As Iser (2006:6) suggested, “it is due to changing interests and fashions that certain theories come to dominate their ‘rivals’, while others move out of the orbit . . .”. Chippindale (1993) is right to be concerned that the direction of our discipline may be governed by fashion and the desire by researchers to appear trendy.

There are, however, more positive aspects to theory in archaeology, and they should not be ignored. Writing of archaeology, Matthew Johnson (2006:443) emphasized,

It shies away from narrowly relativist or positivist epistemologies; it subscribes to academic objectivity, but accepts that a political position is unavoidable; it seeks to account for the past, but does so using a series of concepts (materialisation, agency, gender, context) that are not narrowly tied to one particular –ism.

With these cautionary thoughts in mind, I draw on a number of social theories in this thesis. This approach is enabled by the multiple, competing theories that currently exist in archaeology and anthropology. “[E]ach of them starts out from a different presupposition, pursues a specific objective, has a limited scope, and yields something its competitors do not” (Iser 2006:6–7). One of the reasons I make use of these different theoretical perspectives is that many of the arguments I make operate on different scales. Different questions and, inevitably, different theories are appropriate to different scales of interpretation. This study can be roughly divided into three scalar levels:

- Large scale: regional spatial and temporal variations are examined. Culture-history and culture-geography type questions are asked as well as questions about broad-scale social interactions between groups.

- Medium scale: the symbolism of classes of grave goods and grave structures within regions are studied.
- Small scale: detailed interpretations of individual graves and sites are made.

From this brief summary it is clear that the use of many theories adds value to the overall interpretation, although each may be specifically applicable at only certain scales. At one scale, theories of ethnicity and ethnic boundaries are clearly relevant. At another, it is theories of individuality and individual agency that matter. Understandings of the symbolisms of material goods and human bodies are also clearly relevant. Taking cognizance of Whitley's warning, I hope to avoid applying *a priori* interpretations to the data by using various theoretical approaches.

I do not review all these theories here. They are well-known and described in great detail in the anthropological and sociological literature. To summarize them here would be inadequate and of little value. This thesis is not intended to develop or modify theory; rather it draws on well-established existing theory. I discuss relevant points of theory at appropriate places in the text.

Method

Method relates to the mode and structure of argument. Over the years a number of different methods have been used in the construction of archaeological arguments. In recent years, the philosopher of science who has written most extensively about method in archaeology is Alison Wylie. The methods she has promoted in archaeology—which are now well known and widely used—are those exemplified by the metaphors of cabling and tacking.

In championing these methods she followed Richard Bernstein (1983) in searching for methods 'beyond objectivism and relativism' (Wylie 1989, 1993).

At the time when Bernstein was writing, methodological debate had become polarized between positions of extreme objectivism and extreme relativism. He therefore sought for a middle ground in which rationally decisive arguments could still be made.

Bernstein (1983:69, 224) used an amended version of Charles Sanders Peirce's (1934:157) metaphor that scientific arguments are more like cables than chains. He argued that scientific arguments do not, probably cannot, proceed by "a linear movement from premises to conclusions or from individual 'facts' to generalizations" (Bernstein 1983:69). Instead they must exploit "multiple strands and diverse types of evidence, data, hunches, and arguments used to support a scientific hypothesis or theory" (Bernstein 1983:69). Even where there is no single overriding ground for a conclusion "the cumulative weight of evidence, data, reasons, and arguments can be rationally decisive for scientific communities" (Bernstein 1983:74). Wylie (1989, 1993) considered that cable-like arguments are probably the most appropriate to use in archaeological cases (indeed, they were the sort of argument used by many archaeologists already).

Related to cable-like arguments are important ideas about the disunity of science. Wylie (1999, 2000) pointed out that archaeologists exploit the disunity of science in two distinct ways. Both depend on the realization that "evidence is theory-laden but it is not necessarily laden by the theory it is meant to test or support" (Wylie 2000:232). First, archaeologists make use of what she characterized as 'vertical independence'. Here the background assumptions used to formulate a hypothesis or interpretation are theoretically independent of the data that are adduced to support the hypothesis or interpretation.

Second, she described 'horizontal independence', that is independence of different lines of evidence used to support any particular point. The idea here is that

if different types of evidence—different aspects of the archaeological record, interpreted in light of different ranges of background theory—all converge in supporting a particular reconstructive or explanatory claim about the past, the credibility lent these claims by the assembled evidence is enhanced much beyond a simple additive function (Wylie 2000:232).

Wylie (1999:308, 2000:233) warned, however, that three sorts of horizontal independence may be distinguished: causal, inferential (or epistemic) and disciplinary (or institutional) independence. Although the three types may coincide, this is not always the case. Researchers need to be aware of the way or ways in which their lines of evidence are independent of each other. Causal and inferential independence are desirable and meet the conditions necessary to act as independent lines of evidence in cabled arguments. “It is the independence of sources . . . which ensures that the strands of the resulting cables are not just mutually reinforcing but are also, and crucially, mutually constraining” (Wylie 1989:16).

I exploit such independent (to a greater or lesser extent) strands of evidence in cable-like arguments in the interpretative parts of this thesis.

To these ideas of cables, Wylie (1989, 1993) added a second metaphor, that of tacking (once again following Bernstein [1983] who in turn adopted the idea from Clifford Geertz [1979]). Tacking (as the word is used in sailing) is a dialectical process, a movement back and forth between concepts. Wylie (1989) developed ideas of tacking in anthropological and archaeological cases. I do not make explicit use of ideas of tacking in this thesis, although, it is implicit in some of my analyses.

In many ways, one of the methods I use in this thesis is more important than much of the theory. It is also, probably, more contentious in the context of Later Stone Age archaeology. I therefore discuss it in greater detail. Much of the detailed argument in this thesis is analogical (within a broader framework of cabled argument). More specifically, much of it draws on San ethnography in a version of the so-called direct historical approach. San ethnography has been used in

analogical ways in the interpretation of a great deal of Later Stone Age rock art and other aspects of archaeology in southern Africa. But is it appropriate to use it to interpret archaeological material going back to the beginning of the Holocene?

In answering this question I begin with a general discussion of analogy and how it can be used in archaeology. I then provide a brief overview of the sources of San ethnography before proceeding to discuss the ways in which it may be used to interpret early and mid-Holocene archaeological remains.

Analogy

Analogy, specifically ethnographic analogy, has been used in archaeology from the very early days of the discipline (e.g., Ascher 1961; Chang 1967; Orme 1973 for historical overviews). For almost as long as analogy has been used, criticisms have been levelled against its use. The nature and substance of the criticisms have, of course, changed over the years in line with changing paradigms in archaeological thought (see Wylie 1985 for a detailed review of these changing methodological criticisms). Despite this long history of criticism of analogical methods from a number of perspectives, they continue to be used extensively in archaeological argument. As Wylie (1985:64) pointed out, “critics have also failed . . . to establish that analogical inference is radically faulty—that is, categorically unreliable and misleading—as they claim”. She went on to make the important point that “though a candid appreciation of limitations is appropriate where analogical inference is concerned, its use in archaeological contexts is neither dispensable nor radically faulty” (Wylie 1985:64). Following Wylie, I argue that to move forward our understanding of southern Cape burials, analogy is indispensable.

When taking a position such as this, one has, as Wylie stated, to appreciate the limitations of the method (such a caution is undoubtedly true for all methods). Whilst the method may be indispensable, it may still be misleading if used

incorrectly (Wylie 1985:81). The greatest limitation of analogical arguments is that they are ampliative, that is, they are ultimately inductive and therefore incapable of establishing their conclusions with certainty (Wylie 1982:392). This limitation is less serious than it may first appear if we consider that *all* archaeological arguments are inconclusive and that researchers can assess only the relative degree of strength of an argument, not ‘absolute truth’ (Wylie 1982:391). To clarify these limitations further we need to examine what analogy is, and how one can assess the relative strengths of analogies.

In its simplest formulation, one can state that “to draw an analogy between two or more entities is to indicate one or more respects in which they are similar” (Copi & Cohen 1994:454). Analogical inference then “proceeds from the similarity of two or more things in one or more respects to the similarity of those things in some further respect” (Copi & Cohen 1994:455). It is the similarities in these ‘further respects’ in which we are generally interested in archaeological cases.

It is important to note that an analogy is a relationship of *limited* similarity; it is not a relation of equivalence or isomorphism (Wylie 1982:393). Indeed, if all the properties of objects or situations were the same they would be identical rather than merely analogous (Scriven 1976:211). David Fisher (1970:247) described what he called the ‘fallacy of perfect analogy’: “an analogy, by its very nature, is a similarity between two or more things which are in other respects unlike. A ‘perfect analogy’ is a contradiction in terms.” I stress this point because, in archaeological cases, a difference between the source and the subject of an analogy is often pointed to as being fatal to the argument. It is not. I elaborate on this important point in a following section.

The presence of both similarities and differences between the source and subject of an analogy become important when we examine in greater detail how analogical inference works:

[it] consists of the selective transposition of information from source to subject on the basis of a comparison that, fully developed, specifies how the 'terms' compared are similar, different, or of unknown likeness (Wylie 1985:93).

These similarities, differences and unknown values may be described as the positive, negative and neutral components of an analogy (Wylie 1985:94). In an analogical argument, "given the similarities and differences specified in the premises, some specific aspect of the neutral analogy may also be assumed to be similar or, to comprise further points of positive analogy" (Wylie 1985:94). When assuming further similarities in this way, one must always remember that one is making a probabilistic statement which can be assessed using various criteria.

Irwin Copi and Carl Cohen (1994:459–463) listed six criteria by which one may appraise analogical arguments. All six need to be considered when assessing an argument.

1. Number of entities between which the analogies are said to hold. It is important to note, though, that there is not a simple numerical ratio between the number of instances and the probability of the conclusion.
2. Number of respects in which the source and subject are analogous. Once again, simple numerical ratios do not apply.
3. Modesty of conclusions relative to their premises.
4. Number of disanalogies (points of difference) between source and subject. More disanalogies reduce the probability of the conclusion.
5. The more dissimilar the cases mentioned in the source of the analogy, the stronger the argument. Dissimilarity and disanalogy should not be confused. Dissimilarity relates to differences in the source of the analogy; disanalogy relates to differences between source and subject.
6. Relevance of the points of positive analogy. Relevance is the most important point in assessing an analogical argument.

With these points in mind, two forms of analogy may be distinguished, one somewhat stronger than the other. Formal analogy consists of a formal, point-by-point comparison of traits in source and subject. Further points of similarity are

assumed on the basis of those that already exist. The second form, relational analogy, postulates causal relations between particular traits that occur in both the source and subject, thereby strengthening the probability of the argument (Copi & Cohen 1994:462). “At their strongest, relational comparisons involve a demonstration that there are similarities between source and subject with respect to the causal mechanisms, processes, or factors that determine the presence and interrelationships of (at least some of) their manifest properties” (Wylie 1985:95, original parenthesis). Wylie suggested that in many archaeological cases truly causal or determining relationships do not exist (in the sense of A inevitably leads to B through physical mechanism C), but argued that archaeologists should instead consider ‘quasi-causal connections’ where relations of dependence do exist but not in a physical determining way.

One further point needs to be made with respect to analogy in archaeology. I have not discussed most of the criticisms of analogy because they have been so extensively described in the literature (for a review see Wylie 1985). One criticism that does need to be discussed, though, is the persistent claim that the use of ethnographic analogy simply transposes the present into the past. This claim is blatantly untrue, as the points I have discussed show. Wylie has gone further than merely refuting this claim. In contradiction, she has argued that analogy is a *creative* process in which an interpretative model may be built up by appeal to a number of different sources, each bringing its own important aspect not present in the others (Wylie 1985:106). The interpretation of the subject may, therefore, combine attributes in a configuration not known in any one source: “the model may be a conceptualization of a context that is substantially unlike any single, accessible analog” (Wylie 1985:106). There is more to analogy than some of its critics seem to realize.

San ethnography

The single most common analogical source in southern African Later Stone Age archaeological interpretations (based on both excavated and rock art data) is San ethnography. I need not now provide a full review of all available ethnographic sources; a brief sketch will be sufficient for the discussion that follows. The San ethnographies are well-known and have been reviewed in detail many times (e.g., Schapera 1930; Barnard 1992). San ethnographies are often divided into two parts: the older ethnographies and the newer ones.

The older material comes from both northern and southern San groups. It consists of a variety of material including the accounts of travellers, hunters, missionaries and colonial officials collected in the eighteenth and nineteenth centuries, much of it summarized by George Stow (1905) and Isaac Schapera (1930). Although fragmentary and often heavily and obviously biased, these accounts do provide some useful information. In many cases they provide the only information on San groups in some areas. In addition to these accounts, there are two particularly important bodies of early material, both relating to southern San groups. One of these is the relatively small, yet highly significant, set of myths collected by Joseph Orpen (1874) from a San man by the name of Qing in the Maluti-Drakensberg in 1873. Amongst other useful data, Orpen's account contains the only direct comments on rock paintings in the Maluti-Drakensberg by a contemporary San person. The other significant early material is that collected by Wilhelm Bleek and Lucy Lloyd from /Xam speaking people originally from the Northern Cape Province (e.g., Bleek & Lloyd 1911; Bleek 1924; Lewis-Williams 2000; Hollmann 2004; Skotnes 2007; see Hewitt 1986; Lewis-Williams 2000:1–41; Bennum 2004; Bank 2006 for reviews of the collection). This collection contains in excess of twelve thousand pages of verbatim transcripts of /Xam myths and stories in both /Xam and English.

The newer ethnographies come mostly from San groups living in the Kalahari. The large-scale study of the Kalahari San was begun by the Marshall family in the

1950s (e.g., Marshall 1962, 1969, 1976, 1999; Thomas 1959, 2006). (Some ethnographies were collected in the Kalahari before the 1950s.) Their research sparked a wave of studies out of Harvard University (e.g., Lee 1979, 1984, 1993; Lee & DeVore 1976; Howell 1979; Katz 1982; Bieseke 1993; Katz *et al.* 1997) that was to become known as the Harvard School. Researchers based at many other institutions also began work in the Kalahari (e.g., Silberbauer 1965, 1981; Guenther 1986, 1999; Shostak 1982; Valiente Noailles 1993; Keeney 1999; 2003).

The San ethnographies, particularly those from the Kalahari, have not gone unchallenged. Possibly the most important intellectual event in the study of the San is the so-called Kalahari Revisionist Debate. The debate has been reviewed many times, and I do not repeat a similar review here (Schrire 1980; Denbow 1984; Gordon 1984; Wilmsen 1989; Solway & Lee 1990; Wilmsen & Denbow 1990; Kent 1992 are some of the more important contributions to the debate). The crux of the Revisionist position is that the San are not ‘pristine’, untouched hunter-gatherers living in a xeric ‘Garden of Eden’. They have been in contact with herding and farming peoples for up to 2 000 years. From an analogical perspective, this debate has highlighted the importance of ‘source-side’ assessment of San ethnography (e.g., Wylie 1985; Stahl 1993, 1994).

One of the outcomes of the debate has been the realization that San history cannot be discussed without considering the interactions between San and other communities. Following Edwin Wilmsen (1989), much of this research tends to emphasize political economy; the San are viewed as an economic underclass (see Blundell 2004:23–28 for a recent critique of this position). Be this as it may, the Revisionist Debate gave the Kalahari San a ‘history’.

Wilmsen’s treatment of the Kalahari San as an economic underclass led Susan Kent (1992:56) to accuse him of giving the San “history while denying them autonomy”. Her criticism was based on the way in which the San were seen as passive ‘pawns’, acted upon by more dominant societies. Her point may be read in

another way too. The San's history is seen to begin only when they make contact with other cultural groups. They are no longer seen as pristine, unchanging, isolated hunter-gatherers over the last 2 000 years of contact. The unspoken implication, though, is that this is *exactly* what they were before contact. A 'knock' from a more 'advanced', outside society was needed to initiate the sort of history that the Revisionists describe.

Although there are, of course, still extremists on both sides, most researchers have now moved into a state of 'post-Revisionist compromise'. Many now agree that whilst there certainly was contact between San and other groups, the contact was not of such a degree that it obviates the use of the ethnography by researchers. Importantly, there is little archaeological evidence for the incorporation of San as an economic underclass into agropastoralist society over the last two millennia (Sadr 1997).

The idea of change through time, raised by the Revisionists, is an important one for archaeology, particularly when considering the use of ethnography to help interpret the past. Social change in pre-contact hunter-gatherer communities is a major theme of this study. Difference through time is in many ways comparable to differences between contemporary groups. A point that is particularly striking when examining San ethnography is the degree of difference between the groups concerned. Besides the obvious geographical and temporal separations, there are other quite substantial differences between groups. Most San groups speak different, and often mutually unintelligible, languages (Traill 1995). Many groups are also socially different in terms of kinship structure, marriage rules, seasonal fission and fusion and so forth (see Barnard 1992 for a summary).

Despite these obvious, definable differences, there are other substantial cultural and, particularly, religious similarities between these same groups that should not be overlooked. These similarities are not asserted in general terms, but have been demonstrated by detailed, point-by-point comparisons (Lewis-Williams & Bieseke 1978; Lewis-Williams 1981a; Barnard 1992). These cross-group similarities led

researchers, following Daniel McCall (1970), to describe a ‘pan-San cognitive system’.

This is a crucial point. Despite clear geographical, temporal, linguistic and social differences, there are still substantial religious or ideological similarities between San groups. To state the situation another way, there are religious continuities across groups that differ in other ways. From the point of view of this thesis, this is a crucial point.

This brief overview exposes some of the complexities that researchers should not ignore when they use San ethnography in analogical arguments. These complexities underline the need for an *explicit* discussion of the structure of analogical arguments.

Using San ethnography

In southern Africa, San ethnography has been used to interpret both rock art and other Later Stone Age materials. It has been used in different (usually unacknowledged) ways by the different sub-disciplines, and these uses are, of course, determined by the theoretical perspectives and agendas of researchers. Much of the ethnography is expressed in culturally specific idiom and metaphor and therefore itself in need of interpretation (see Whitley 2005:86–88 for a discussion of this issue). Researchers studying rock art have therefore tended to interpret ethnography to understand religion. In contrast, other Later Stone Age researchers have tended to use the ethnography literally to reconstruct subsistence economy, ecological interaction and technology. Subsequent chapters explore the need to bring together these ways of using San ethnography.

Faced with such complexities, researchers naturally enough wish to appear cautious and conservative in their use of analogies. A general comment frequently made in discussions of the use of ethnography in archaeology is that one should

“handle with extreme care” (Yellen 1977:4). Whilst this warning is undoubtedly deserved, it is not all that useful (indeed, who would contest that all our research should be conducted carefully?). Questions such as “what is meant by care?” and “how can one be careful?” come readily to mind. A common way of overcoming these difficulties is to use ethnography to interpret only recent archaeological remains in the hope that this sort of direct analogy will reduce, or even eliminate, the pitfalls of analogical arguments that I have already discussed.

However, as Ann Stahl (1994) has shown in her study in the Banda area of Ghana, recent changes “wrought on so-called ‘traditional’ non-Western societies by the expansion of European and Western capital” (Stahl 1994:182) has in many cases created significant differences between ethnographically recorded societies and their immediate, pre-contact predecessors. Indeed, Stahl (1994) suggested that we should expect substantial (though not total) differences between ethnographically recorded societies and recent, pre-contact archaeological remains because of the huge impact of European colonialism (see also Parkington 1984a).

For the southern African Later Stone Age, Peter Mitchell (2005a:163–164, 2005b:67–68) has made three suggestions as to how we can improve our use of ethnography:

1. “First, develop a closely argued archaeology of ethnographically known San societies” (Mitchell 2005b:67). An example of this sort of study is the work Janette Deacon (1986, 1988, 1996; Deacon & Foster 2005) has undertaken on Northern Cape /Xam-speaking people.
2. “Second, expand the sample of Bushman societies that we use” (Mitchell 2005b:67). Mitchell (2005a, 2005b) made the point that researchers predominantly use /Xam, G/wi and Ju/’hoan ethnography and ignore equally valuable material on other groups (see also Parkington 1984a:172: he argued that archaeologists should ‘de-!Kung’ [!Kung = Ju/’hoansi] their view of the San, a view more recently also taken up by Humphreys [2004/2005]). He suggested that

we should use all (different) available sources. The importance of using diverse sources is well illustrated by the case of the Ju/'hoansi: ethnography on them is frequently used and cited as 'typical San', yet their kinship system, for instance, is atypical of most other San groups recorded (Barnard 1992). The use of multiple, different sources in an analogical argument also has the advantage of strengthening the inferences based on that analogy (Copi & Cohen 1994:459–463).

3. "Third, reach beyond the Kalahari to other parts of the world, where more 'complex' hunter-gatherers may be at least as relevant as the Ju/'hoansi" (Mitchell 2005b:67). The use of other hunter-gatherer ethnographies, from other parts of the world, in a more general analogical way has only infrequently been used in southern Africa in recent years. This approach is, however, being championed by Anthony Humphreys (2004/2005) who is particularly in favour of the use of Australian and North American ethnographies. Karim Sadr (2004) has had some success applying such ethnographies to sites he has excavated on the Western Cape coast. This strategy has the same methodological advantage as using ethnography from multiple San groups.

The points Mitchell made are important and should make a major contribution to debates on the use of San ethnography in southern African archaeology, and, indeed, the principles could be applied to other parts of the world. I add to his points some discussion of the time depths at which San ethnography may be used.

I begin with an example from the study of rock art. Ethnography has been used to interpret Later Stone Age rock art for several decades. The way the ethnography is used has, of course, changed over the years. There has, however, been a persistent mild criticism that goes along the lines of "you don't know how old the art is, so you can't use the ethnography". It is usually countered by researchers who do use ethnography with some sort of statement to the effect that we assume the art is fairly recent, so we can use the ethnography. (There has been a similar criticism relating to geographical distance between the ethnographic source and

archaeological subject.) The implication of both the criticism and the reply is that the *passage of time* somehow renders ethnography unusable.

This is a curious thought. Time alone does not cause change, indeed, cannot *cause* change. It merely permits change to occur. Change is caused by social and ideological factors. Change over time is not inevitable. If we wish to discuss change, we first need to empirically *demonstrate* that it has occurred. Such change cannot simply be assumed as many researchers do; to do so is no different from assuming that change has not occurred—the very thing that many criticize. Neither of these assumptions should be allowed.

Besides simply demonstrating that change has occurred between the ethnographic and archaeological cases, we also need to ask *what* has changed. Was the change social, cultural, technological or so on? Equally importantly, we need to consider whether the change was ‘complete’ or whether there are some remaining similarities between the ethnographic and the archaeological cases. We therefore need to be open to the idea of *differential change*: not all aspects of society change at the same time or rate. “Recognition of change does not negate aspects of continuity. Rather, it points to the need to treat continuity and change as *empirical questions*, not something to be assumed” (Stahl 1993:246, emphasis as in original; see also Huffman 1986:85; Stahl 1994). The idea that if one aspect of a society changes, everything must change comes out of, now discredited, ideas of societies as systems. We may, therefore, find that there is some continuity amongst groups that differ in other ways. The presence of both differences and similarities between contemporary San groups, as I have discussed, illustrates this point. The nature and scale of changes are important aspects of the argument I develop in subsequent chapters: “the interweaving and networking of different types and levels of similarity and difference support interpretation” (Hodder 1986:128).

There is, though, a still more fundamental issue at stake: time is, logically, irrelevant in analogical arguments. It does not in any way impinge on the way an analogical argument is constructed or evaluated. Analogical arguments depend on

a comparison of traits between the sources and subjects of analogies. They are assessed using the criteria I have outlined (Copi & Cohen 1994:459–463), irrespective of geographical or temporal differences between source and subject. Time itself does not come into the evaluation of an analogical argument in any way: the argument stands or falls on other characteristics. (On similar grounds, one should note that a direct historical analogy is logically no stronger than a historically unrelated analogy, hence the importance of Mitchell's [2005b:67] third point.) I do not argue that ethnographies historically linked to archaeological material will necessarily explain that material irrespective of time. They may well not. I simply argue that the utility of the ethnography as an analogical source needs to be assessed on grounds other than time depth.

The following points of significance therefore emerge:

- Time depth is not a relevant consideration.
- Change and continuity needs to be demonstrated rather than assumed.
- The nature of the change needs to be questioned.
- The differential nature of change needs to be taken into consideration.

With these points in mind, I argue that recent ethnographies *can* be used to interpret early and mid-Holocene archaeological remains in a direct historical way, provided that some continuity (i.e., points of positive analogy) can be demonstrated between the archaeological material and the ethnographic evidence. Further, this will still be the case even if other aspects of the two communities can be demonstrated to be different (i.e., disanalogies exist). Analogy, it should be remembered, by definition, consists of both similarities and differences. Archaeological and ethnographic evidence therefore need to be used in recursive, mutually informing, mutually constraining ways. This is the type of process that Wylie (1989, 1993) described as tacking between bodies of evidence.

In taking these propositions forward, I now develop a general model of San cosmology. I do this on the basis that it is cosmology and religion that are similar amongst contemporary San groups. The ubiquity of a trait cannot automatically be

taken as evidence of the antiquity of that trait (Stahl 1993:249–250). I, however, believe that, in the case of the San, it is a reasonable assumption to make that these shared beliefs probably also have great time depth. Although developing the model at this stage, I do not assume its universal applicability. I examine the archaeology and ethnographies in further detail in following chapters to demonstrate continuities and changes, in other words to assess the utility of the analogue.

A model of San cosmology

The model I develop here was first proposed by David Lewis-Williams (1996) in his analyses of San myths (Lewis-Williams 1996, 1997a; a similar concept of San cosmology that preceded the model was presented in Lewis-Williams 1994:278–279) and has subsequently been used and shown to be of value by a number of writers (e.g., Ouzman 1998, 2001, 2002; Hall 2000; Mguni 2002; Pearce 2002). It is an ethnographically-based model derived initially from studying /Xam ethnographies. It is not a position explicitly articulated by any one group. As a *model* it may be useful in understanding cultural remains either by suggesting how different elements interrelate, or by highlighting how particular material culture differs from the constructed ‘norm’.

A model is a way of bringing together diverse types of data in relation to a particular theory to show the relationships between them. In a philosophical sense, models must then be considered to be a type of hypothesis in that they make general empirical statements in relation to a particular theory. It is important, though, not to become too ‘attached’ to a model; it exists as a tool, and may be modified, changed or even discarded as research proceeds. The dogmatic application of an inappropriate model can do nothing but harm. In such cases, they become methodological ‘hammers’, pounding away at problems they cannot solve (Kaplan 1964:28; Moore & Keene 1983:3–5).

Lewis-Williams's implicitly Structuralist original model took the form of a bi-axial division of the cosmos (Lewis-Williams 1996:124, fig. 1). Importantly, he emphasized that “nevertheless, all realms of San cosmology interdigitate; the bi-axial schema is an analytical construct” (Lewis-Williams 1996:124). The extremities of the horizontal axis are the camp and the hunting-ground (Fig. 2.1). The camp was the place shared with one's closest relatives, it was the location of the basic social unit, and it was the place where most important social activities (such as meat sharing and medicine dances) took place; in Structuralist terms, the camp was the ‘culture centre’ of the San world.

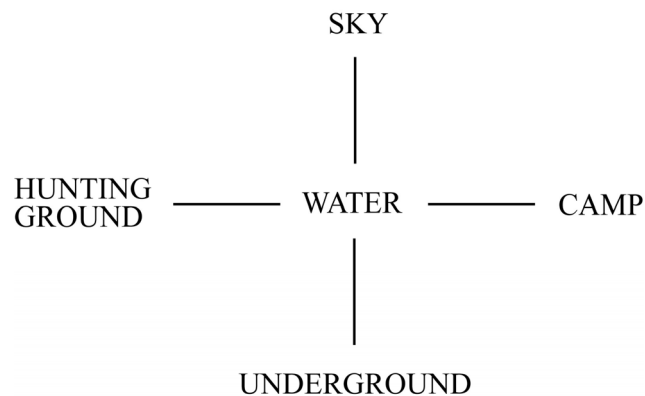


Figure 2.1: Original bi-axial model of the San cosmos (after Lewis-Williams 1996:fig. 1).

Opposed to the camp was the hunting-ground. It was a place populated by strangers, a place of anti-social activities; it was the place of dangerous wild animals and hunting. In Structuralist terms it could be said to be the ‘nature centre’ of the San cosmos.

Between the camp and hunting-ground was the waterhole. This was an ambivalent place where one could find both life-giving water and game animals as well as potentially dangerous strangers and animals. Significantly, the pivotally placed waterhole had a transformative and regenerative role. In /Xam myths, for example, Kwammang-a's shoe grew into an eland in a waterhole (Bleek 1924:2–9), an ostrich feather placed in a waterhole grew into an ostrich (Bleek & Lloyd

1911:137–145) and the Blue Crane’s ‘merrythought’ (wishbone) grew into the Blue Crane in the water (Bleek 1924:26–27).

Such binary or ‘dualist’ concepts of a divide between nature and culture are once again being challenged in anthropology (e.g., Descola 1992, 1994; Descola & Pálsson 1996). These critics argue for a ‘monist’ (Hornborg 1996) position in which there is no firm divide between nature and culture; the relationship between each society and its environment should be separately contextually examined without a preconceived nature–culture divide. Whilst these points of view have serious implications for the horizontal axis of the model, it is the vertical axis with which I am most concerned in this thesis.

The vertical axis of the model is less well defined than the horizontal. The two extremities are the sky and underground (Fig. 2.1). Underground seems to be associated with the dead. The sky seems to be associated with spirits, religious specialists and the ‘Mantis’ (/Kaggen, the southern San trickster-deity). These two extremities are, like the horizontal axis, mediated by water: water wells up from underground, and rain (water) falls from the sky. The transformative role of the waterhole is particularly emphasized along this axis. The vertical axis, then, is a representation of the *axis mundi* (Eliade 1964:259), the means by which religious specialists move between levels of the cosmos.

The model of the San cosmos that Lewis-Williams (1996) originally proposed has ‘water’ as the centre of both axes. The waterhole was the ambivalent place where transformations take place and where beings move from one level of the cosmos to another. The model was formulated to help interpret ethnographically recorded myths (particularly, Lewis-Williams 1996, 1997a) and proved extremely useful for doing so. When the model is taken from its original setting of myths and applied to other bodies of evidence, it becomes apparent that some changes are necessary: contextual considerations are paramount. The additions I suggest relate to the centre point of the model, particularly as it divides the vertical axis (see also Pearce 2002; Lewis-Williams & Pearce 2004a).

In the context of rock paintings Lewis-Williams and Thomas Dowson (1990) argued that the rock face formed a 'veil' between the human world and the spirit world, in essence, another transitional point between levels of the cosmos. Many Later Stone Age rock paintings interact with features of the rock face, often appearing to enter or leave the rock face. What is important here is that creatures from the spirit world were believed to move through the rock face to the human world, and that religious specialists were thought to enter the spirit world through the rock (Lewis-Williams & Dowson 1990). Significantly, it seems that this movement through the rock was sometimes mediated by paint (Lewis-Williams & Dowson 1990; Yates & Manhire 1991; Lewis-Williams 1995a). I therefore suggest that, in the context of rock art, 'rock face' can be added to 'water' in the central mediating position in the model. It performed a similar function to water, acting as both an agent of transformation and as a way of entering the spirit world.

Siyakha Mguni (2002) has argued that, at least amongst some San communities, trees were considered to be *axes mundi*, linking the three tiers of the cosmos. He also showed that depictions of trees, often in association with termitaria, are repeatedly found in the Later Stone Age rock art of Zimbabwe and northern South Africa in contexts related to the spirit world, transformation and movement between levels of the cosmos (Mguni 2001, 2002). In these areas, trees became the vertical axis of the model.

Previous research (Pearce 2002) has suggested a third option for the centre point of the model. I have argued that anthropogenic holes in the ground acted as entrances to the spirit world. As such, the holes fulfilled a comparable role to the water and rock face. I suggest that it is profitable to view the model as having multiple types of 'point of crossover' (Fig. 2.2). It is important to note that, although different in appearance, the three centre points that I suggest are all conceptually similar: all three relate to transformations and entrances to the spirit world.

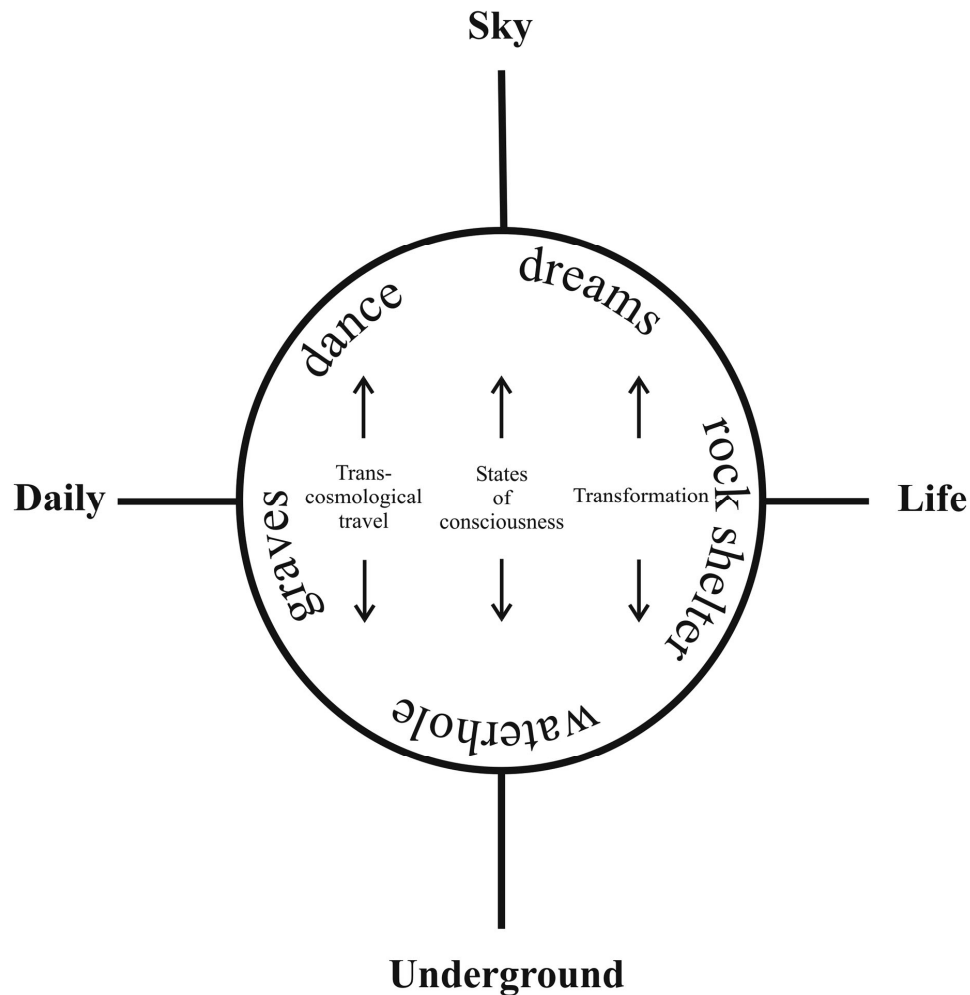


Figure 2.2: Modified bi-axial model of the San cosmos.

The difference in centre points seems to be context specific. Water seems to be particularly emphasized in myth, whereas the rock face is prominent in the practices of painting and engraving. Holes were perhaps highlighted in other ritual activities. This differential emphasis illustrates how the same overriding cosmology and system of beliefs influences all aspects of life.

The model of the San cosmos discussed here may seem ‘static’—the type of position that many workers would now challenge. As I present the model this is (partly) true. Researchers need to remember, though, that in “conception[s] of social action the actor can only pursue such a strategy with reference to some preexisting and ‘external’ ‘structure’ or *habitus* at any given moment in time”

(Johnson 1989:192). The model should be seen in this way, as a sort of '*habitus*' (a term made famous by Bourdieu, e.g., 1977) which, whilst manipulatable, nevertheless serves as a basis for a broad level understanding of Later Stone Age cosmology.

With these methodological thoughts in mind, I move on to an examination of the historical trajectory that has led to the current state of understanding of the southern Cape burials.