Review feature: Snakes and crocodiles: power and symbolism in ancient Zimbabwe, by Thomas N. Huffman

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Snakes and Crocodiles: power and symbolism in ancient Zimbabwe was published by Witwatersrand University Press at the end of 1996 (ISBN 1-86814-254-X Available from Book Promotions, PO Box 5, Plumstead 7801, price R98.95). This book is important in a number of ways: it deals with Great Zimbabwe, the most famous archaeological site in southern Africa (and much else besides). It is the most ambitious attempt yet to understand the architecture of zimbabwe-type sites, and the wider social and cultural context in which they were constructed and occupied. It makes accessible to a general audience more than two decades of research and writing on an important period of southern African history. For all these reasons, Snakes and Crocodiles warrants more than an ordinary book review. Seven researchers with expertise in relevant areas of archaeology, anthropology and history have written reviews. These are followed by a response from Huffman, in which he addresses the reviewers’ criticisms and concerns. This exchange highlights some of the difficulties of interpretation and explanation in archaeology; we hope it will, stimulate readers’ interest in the topic, and in reading the book for themselves.

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Background
I owe Tom Huffman a considerable debt. So does Zimbabwe. When he was working here in 1970-77 he spared me countless precious hours explaining the literature and way of thought of archaeology. Under his guidance, confusing masses of pottery were classed neatly into precise phases and facies, divided or joined by clear breaks or lines. If I know now that things archaeological are not so simple, this was an invaluable introduction to the discipline. Similarly, he did much for Zimbabwean archaeology in difficult times. It is not always remembered that, at the time of the ‘Great Zimbabwe debate’ of 1969-71, those putting forward the scientific view were reduced to arguing that, of the four or five most crucial radiocarbon dates available for the site, two were invalid. This tended to discredit radiocarbon dating in the minds of the uninitiated. His work in resolving the discrepancy and reinforcing the sequence was crucial. Moreover, not so much in guides as in many other publications and lectures to the Rhodesian public and the multiracial school field expeditions, he told the truth where it counted most. Finally, his identification of the unwalled living areas of Great Zimbabwe was the first major step forward in the site’s analysis since 1905. For these and other reasons I take little pleasure in what follows.

Tom Huffman’s departure for South Africa in 1977 put a frontier between him and most of the zimbabwe sites, especially given the strained relations between
Zimbabwe and South Africa in the 1980s. The sites in Mogambique were even more inaccessible. If he was not denied access to sites, he did not excavate, and he appeared to be confined to many whirlwind tours over the years. One way around the problem was to turn to ethnographical and historical material. In the latter case, academic training in the use of oral traditions and documents was needed, as well as a wide knowledge of the sources, and he had neither. Moreover, from 1980 he was influenced by Adam Kuper's work, which does not pretend to cover the Shona, and by the fact that it was possible to work in Venda without crossing a frontier. Unfortunately, the latter was an exceptionally problematic area in terms of all disciplines.

By February 1981 he had produced his first paper on the cognitive archaeology of Great Zimbabwe, and many others followed. From the beginning, in private and in print, I and others warned him that certain of his assumptions or uses of evidence were untenable. These included the ability of oral tradition to retain data over many centuries, the reading of documents and the relevance of data from Venda. Most of these warnings were totally ignored. In 1991, having to assess his whole cognitive archaeological corpus so far, I found not only defects in the use of evidence but also signs of faulty logic, special pleading and a highly improbable picture of "a city that was essentially static in its use of space for at least two centuries" (Beach, forthcoming).

Meanwhile, Huffman was beginning a new book, Snakes and Crocodiles. This represents new developments in his thinking, sometimes - but rarely explicitly - in reaction to earlier criticism.

Intellectual influences and source material

The acknowledgements listed in the book are instructive. Its manuscript was discussed with thirteen academics, none of whom were Zimbabweans and only one of whom - David Collett - had any real field experience in Zimbabwe. Nineteen Zimbabweans get thanks for help and assistance but not, apparently, for any intellectual input. Perhaps this is logical, because one implication of Huffman's work is that Zimbabweans do not fully understand their own greatest archaeological complex, and must thus turn to South Africa for guidance. Yet he does in fact insist (pp. 10-11) that Zimbabweans' do retain, somehow or other, through their culture, data that he can use to interpret Zimbabwe. How is it then, that he does not refer to any of the works by academically trained Zimbabwean archaeologists who, by his logic, should also have acquired such data by the same means?

The foregoing raises the question: at whom is this book aimed? On balance, its intended readership lives outside Zimbabwe and perhaps outside southern Africa. Wherever his readers are, however, they are not being dealt a full hand of cards. Huffman raises their expectations by stating that "this book is my answer" to "criticisms from colleagues I thought should know better" (p. vii) but, with the exception of J. Blacking, he does not name them or respond directly to their criticisms. This is odd, because where he has modified his earlier thinking in reaction to his own criticisms - a necessary part of research - he often does inform his readers of this.
The readers may wonder just where this beautifully produced book stands in terms of technical expertise. After all, it refers in detail to Portuguese documents, oral traditions, the most abstruse points of Shona cosmology and ethnography and to an apparently encyclopaedic knowledge of the precise location of decorated features of a remarkable number of stone zimbabwe. How does it relate to the expertise of others in the field?

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Quite simply, nobody else in the field claims this degree of omniscience, and thus Snakes and Crocodiles will be receiving converging criticisms from entirely different areas of specialisation. Given its reliance on so many kinds of evidence, it must stand or fail on all of them. Here, I concentrate on my own field of history, in which I feel that - some fifteen years after he began Huffman's grasp of the essential methodology is still inadequate.

Oral tradition is essential to Huffman's interpretation of much of the archaeological evidence. Previously, he asserted that it did reach back to the time of Great Zimbabwe, before c. 1500. Now, in what looks like a silent acceptance of what I have been telling him since February 1981, he denies its value for this early phase (p. 10). However, he does use it for an 'ethnographic present' of c. 1450-1830. Previously he named informants whose probable knowledge could be assessed, now he simply refers to 'about ten' informants whose identity is concealed from us; "it does not matter who the informant is as long as he can articulate some of his culture's values and attitudes" (p. 12). This bland statement skips over some very serious questions, but even the readers least familiar with the field may wonder the following: by what process did Huffman select ten informants out of nearly ten million Shona-speakers? To what extent were they influenced by the printed word, radio or television? How many informants' testimonies were rejected by him, and why? Just how did they acquire their knowledge? And crucially - just how did he conduct his interviews?

Alas for his credibility, he gives us a little information about this (pp. 10-11) and displays a textbook example of how not to collect oral tradition. The interviews cited were obviously untaped, and Huffman is not even certain just what his informants said: "In quick succession he said something like the following". It is thus impossible to know just how Huffman phrased his questions, while in the other case he ignores the possibility that his own preconceptions had become known to his informant previously. By holding back from us not only the name of the informant but even the date of the interview he makes it impossible to check his data, but any reader who wonders just how an associate of the Mashayamombe dynasty could relate his 'culture' to the Khami ruins might care to read its history (Beach 1994b: 68-72). Bluntly, on the evidence shown I do not regard Huffman's collection or use of oral tradition as being competent.

Matters are little better where Portuguese documents are concerned. Confined as he is to those available in translation, he argues that those that he cannot read "may be less useful" and that in any case documents in themselves "are incomplete, sometimes mistaken and inevitably biased" (p. 10).
Actually, this is true of all evidence, but historians are trained to avoid these problems as far as possible. One thing that they are supposed to learn is that they cannot simply select from the available body of documentation those sources that happen to support their conclusions: they must also take note of contrary sources. Huffman lays stress on those that indicate that the Shona thought that, at certain times, the ruler must be secluded from the public, and tends to assume that this was the rule (pp. 25-28). This assumption underlies much of his interpretation of the archaeology.

What he does not tell the reader is that there are just as many documents that make it clear that the ruler was not always secluded, but went out to hunt, fight, attend ceremonies in public or even cultivate. I have argued elsewhere (Beach 1994a:142-163) that in many things there was (and is) a considerable difference between what the Shona thought they ought to be doing and what they actually did. This matters because their stone zimbabwe might reflect a much more flexible thought pattern than that to be found in Snakes and Crocodiles or, indeed, in much of Huffman's own work over the last 25 years.

Huffman is primarily an archaeologist, concerned with the physical remains of a very large number of zimbabwe to be found in central and southern Moambique, Zimbabwe, Botswana and the Transvaal. He wishes to explain as much as possible of their layout, architecture and decoration in terms of the culture of the peoples of the region as revealed by modern ethnography - including artefacts where he can - and historical sources such as documents and oral traditions. From the beginning (pp. 5-6) he argues against 'historical particularism' and 'narrow inductivism' in favour of stress on spatial organisation and world-views. This looks very much like a justification for a certain amount of glossing over of awkward questions about evidence and argument in favour of his own complex and rigid model. What he does not tell readers is that this is what he was doing in early 1981, when he argued that it was possible to interpret Great Zimbabwe (c. 1250-1500) by using Portuguese documents, recent oral tradition and Venda culture. Now, he quietly shifts his stance, starting with an 'ethnographic present' of c. 1450-1830 and concentrating on Danangombe, Khami and other sites and using much the same sources to a similar effect. He then traces the origin of his model back through the Great Zimbabwe period to that of Mapungubwe. Whereas he often does show how his thinking on points of detail has changed since the 1980s, he does not show readers quite how and why he has changed his line of attack. In fact, his basic approach has not changed much in fifteen years, and in some important instances - notably the questions of initiation, circumcision and the use of Venda evidence - he is clinging grimly to positions that might have been abandoned by a more flexible writer.

His 'ethnographic present' poses some problems, because his sites nearly all lie far to the south and southwest of the range of knowledge of the documents he cites, while they were nearly all built prior to the 1680s, including the Venda site of Dzata (Loubser pers. comm. 25 June 1993). Thus he more or less has to argue against the 'historical particularist' argument that what was true for the northern and eastern Shona c. 1500-1700 might not have been true for the other areas, and that 'Shona' culture might not have been or be entirely uniform. Moreover, he
does not make it clear to readers that the Changamire Rozvi arose in the north-eastern Shona area no earlier than the 1670s, in a region where the construction of stone zimbabwe had been virtually extinct for 150 years. The Changamire lived in the Torwa zimbabwe of Danangombe up to the 1830s, but they did not build it. Thus traditions about the Changamire or other Rozvi might not be relevant to zimbabwe, although there is no doubt that the Changamire Rozvi did become partly culturally absorbed by the earlier Torwa population that built Khami and Danangombe, having become Kalanga-speakers by the 1830s.

Similarly, the specifically Rozvi-Shona Singo dynasty, whose descendants rule the Venda today and to whom the term 'Venda' is often confined in the literature, migrated south of the Limpopo into the Soutpansberg, where Huffman and others have shown that there were already other Shona-descended people who built zimbabwe. Not only were there Shona and Sotho components in this preSingo Venda culture, but "most linguists agree that it also contained a unique, or 'unknown', component" (Loubser 1991:398). Given this remarkable blend of components of different origins, it is very difficult to tell just which

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element of 'Venda' culture came from where and when.

For example, although Huffman (p. 29) has little enough evidence to link crocodiles with Shona monarchy, and some of that is dubious or irrelevant, there is just enough to construct a tenuous argument that traces this link from the northeastern Shona area through the Rozvi to the Singo Venda. Unfortunately, this link would postdate the stone zimbabwe and their decorations that Huffman is so keen to link to crocodiles! One can see why he doesn't like 'historical particularism', because it raises objections to his mishmash of evidence picked up here, there and everywhere, but he cannot have things both ways: whether he lets his readers know this or not (usually, not) he is trying to make his model work in an historical period that is really very well known by African standards, not an historical vacuum.

Another clash between Huffman's model and history occurs in his assertion that the early Shona practised initiation and circumcision. He came to this conclusion early in the 1980s in connection with Great Zimbabwe, and clings to it here (pp. 85-95, 148-149, 195-206). Although he concedes, in terms strangely similar to those that I made clear to him in 1985 and published in 1987, that the documents and oral sources deny this, it makes little difference to his argument. Yet this forces him to drag in historical arguments in the form of wars to explain why these practices stopped (p. 204). All these demonstrate is his lack of familiarity with Zimbabwean history. Neither the Portuguese nor the Rozvi of the 17th century affected the Zimbabwean region extensively or intensively enough to have had the impact that he invokes. Nor does the evidence from southern Mogambique, which he consistently neglects in his arguments, support him - on the contrary (Smith 1973; Rita-Ferreira 1982). Moreover, the reserve military force that he calls in to eliminate even the memory of initiation and circumcision
consists of the much-maligned Ndebele, cited in obsolete or misunderstood sources. This is palpable nonsense: the evidence for the impact of the Ndebele on the region is excellent, literate observers having arrived within 20 years of the fall of Danangombe, and for 24 years historians have made it clear that it did not have the effect envisaged.

So what is the value of this book? Tom Huffman was not the first to try to explain the zimbabwe in terms of non-archaeological sources, but his is the most ambitious attempt so far. From 1981 I have felt that it is well worth making, but I have had grave doubts about his reasoning and methodology. If younger scholars - I stress the plural wish to follow up the lead set by him and Zimbabwean archaeologists, I would suggest the following trials: i. double-check all of the physical evidence on the zimbabwe themselves, because some of Huffman's depictions of the sites have been questioned; ii. go much more carefully into the nature of Shona society, being ready to consider the possibility of regional variations; iii. be much more careful in the use of oral traditions and documents, and iv. do not leap to conclusions after a few months of study and then spend 15 years trying to substantiate them. Rather, develop tentative hypotheses and be ready to shelve them temporarily or for good if they don't work, being careful to show just how they have changed over time. Obviously, such a course would need co-operation rather than rivalry between scholars in different disciplines, and if Tom Huffman or any other academic researcher should seek help from me I hope that I would be able to match the generosity that he showed me in the 1970s. In the last analysis, research ought to be leading to yet further research, not a final 'answer' to any question, and there is no doubt that the publication of Snakes and Crocodiles will lead to yet other works, not least by its author. That is its main value.

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The author complains that even after improvements to his original structuralist attempt to understand fifteenth century Zimbabwe culture, he still received criticisms from colleagues he thought should know better. This book is his answer. I remain unconvinced.

The book certainly incorporates a wide range of material, painstakingly collected from a variety of sources, and brings this material together into a coherent symbolic system. It provides summary documentation and good illustrations of many important archeological sites in Zimbabwe. It is a work of creative art. I remain sceptical about its relation to life in fifteenth century Zimbabwe. This review will comment on some of the assumptions behind Huffman's structural analysis, and challenge some of the conclusions that arise from these assumptions. Huffman fails adequately to discuss the nature of a 'symbolic system'. We know that many individuals can switch competently from one language to another, depending on the context in which they are speaking and listening. This is possible even when the languages involve very different structures and different sets of symbolic associations. When such switches are made by competent linguists, they are able to leave behind one symbolic system and move into another. We also observe that, in contemporary society, people come into contact
with a variety of symbolic systems and choose between them, depending on the contexts in which they find themselves. An example much discussed in contemporary Africa is the way people switch from traditional healing with its religious associations to formal Christian ways of thinking, depending on the precise personal and social contexts in which they find themselves. Any cultural pool contains a variety of symbols, which groups or individuals may call upon and incorporate into coherent systems or subsystems, depending on particular and immediate needs. There is no reason to assume, as many structuralists appear to assume, that all symbols found in a particular culture cohere in a single system. Huffman states (p. 12, citing de Heusch): "Venda and Shona share the same symbolic system associated with sacred leadership." If this means simply that certain symbols can be found among the two peoples in their legends and ritual practices concerning kingship, I have no problem with the statement. But Huffman implies that the statement allows us to assume similarities in a variety of other areas of social life: here I remain unconvinced. While attending Shona rituals, I have occasionally seen problems arise when members of one group assume that they understand what is happening in the rituals of another and are unaware that certain postures, or certain spirits, have very different connotations in the two groups. While it is understandable that in social life people make broad assumptions from limited observed similarities, such assumptions are not acceptable in academic analysis. Huffman is over-confident that "methodologically .... where there is ... no crucial difference between settlement patterns in the ethnographic present and archeological past, we can infer identity in world-view and social organisation" (p. 8).

Related to his structuralist assumptions is the kind of data Huffman uses to inform us about life in fifteenth century Zimbabwe. On pp. 10-11, he describes how a passing man from near Harare (500 kilometres away), spoke with confidence about the functions of certain stone structures at Khami. For Huffman, such incidents "show that it is not only possible to derive important organisational principles from contemporary sources ..., it is also possible to understand the logic by which these principles applied" (p. 11). An alternative interpretation is that such incidents illustrate the way in which people often confidently assume they understand things about which they know little or nothing.

When I see the variety of sources Huffman has brought to bear on his argument, from contemporary informants to ancient documents, district commissioners and anthropologists with knowledge of Shona peoples and others, I can admire the extent of his research work. I remain unconvinced that it is all relevant to his case. And I regret his failure to discuss the reliability and the nature of his sources: not everything written down can be cited as equally powerful evidence. My problem with Huffman's methodology is his assumptions of excessive continuity between ages and peoples. Strangely, his thesis leads to conclusions of strange discontinuity. Important to the symbolic picture he has created is the Venda initiation ceremony for boys. On this point there is no question of citing
contemporary Shona informants to articulate their culture's values and attitudes: on the contrary, now Huffman has to explain why circumcision has dropped out of Shona living memory (even among the Venda it appears to be a 19th century innovation - p. 203). Also he finds it necessary to play down the role of spirit mediums, so prominent in Shona culture today and mentioned in 16th century documents concerning other parts of Shona country. He produces explanations, more or less coherent with his theory. For me there is a simpler explanation: Huffman's assumptions are wrong and the structure he builds on them is, at least in some respects, wrong.

I do not accept his assumptions. I find parts of his conclusion implausible. I see no reason to work my way through the details of his argument and scrutinise the details of the data he brings to bear on it. I leave that to professional archaeologists. For an anthropologist of symbolic systems, there remains the intriguing question of how such symbolic edifices as that created by Huffman in this volume receive the attention they do in academic circles.

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In this book Tom Huffman weaves ethnographic, historical and archaeological data into an interpretive grid that he then places over Khami, Great Zimbabwe and a number of other sites to explicate their structural symbolism. The book makes extensive use of Shona and Venda ethnography, as well as 16th and 17th century Portuguese descriptions, and is richly illustrated with over 260 photographs, maps and line drawings in addition to 16 color plates. I should like this book, so why does it set my teeth on edge?

I think there are two principal reasons. My first problem is with Huffman's use of decontextualized 'bits' of ethnographic information. His use of the analogy of a "king as a crocodile in his pool" resonates generally with wider Bantu cosmology where a water boundary separates the land of the living from the land of the ancestors.

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Kings, diviners, witches (and even albinos and twins) can see through this water barrier, giving them the ability to mediate between the two realms (see, for instance, Fu Kiau 1969; MacGaffey 1986). Huffman's handling of the relationship between spiritual and worldly power is, however, sometimes naive and results in misleading interpretations. He completely misses the point, for instance, when he argues (p. 43) that rain-making amongst the Shona was cosmologically different from other societies because, for them, it was "caused by powerful ancestors, not powerful medicines". In all of southern Africa, rain-making results from successful mediation with the spiritual or ancestral world, whether or not specific medicines or charms are used. It is not a spiritual matter in one case, and a simple technological problem of medicine preparation in the other. Similarly, the ability to accrue political authority is widely linked with a person's ability to manipulate and control spiritual forces. The distinction is not, therefore, between spiritually endowed 'sacred kings', on the one hand, and 'profane' ones, on the other. Both must be able to manipulate ancestral forces to their benefit while at the same time neutralising the levelling effects of jealousy and witchcraft. Such
interconnections between physical and spiritual forces are overlooked, however, when he argues that the soapstone birds at Great Zimbabwe invoke "the link between Mwari and their [the Mambo's] ancestors", but their absence from later sites is because, by then, "virtually every ruling dynasty was already integrated into a single upper class ... and there was no need to demonstrate their link with Mwari when Great Zimbabwe declined" (p. 190-191). Perhaps this assumption of 'class solidarity' also leads him to argue that the homes of nobles and commoners were arranged around the king's to form "protective circles around the leader, shielding him from political rivals" (p. 110-111). Such an interpretation discounts the equally likely probability that a king's greatest political contenders were also his closest relatives - people whom it would have been in the king's interests to keep close at hand so that a watch could be kept over them.

My second problem is that Huffman's structural arguments are often circular and seldom return, in hermeneutic fashion, to the archaeological record for refinement, modification or elimination. Instead, he repeatedly assures us of the "model's heuristic potential" in a manner that suggests that overarching or totalising explanatory positions have been reached that accommodate all data. Throughout the book, chains of logical dependencies are allowed to grow - not because they are based upon close and contextualised readings of associations between archaeological features and cultural remains, but because they seem to fit his structural models. The following passage illustrates this process: "The main stairway leads up underneath a covering near the top and ends between three huts. The first is prominently sited on the terrace wall ... and can be identified as the messenger's office. Open space next to it may have functioned as the royal court. The second hut is notably smaller and includes a working space behind an exterior fireplace in front. This outside fireplace is in keeping with the diviner's role ... for an interior hearth would destroy the hut's spiritual atmosphere. This exterior fireplace, then, positively associates this small hut with the mambo's diviner" (p. 57).

While all of this may indeed be true, one can perhaps be forgiven for wanting more concrete archaeological evidence to support such claims. The are many other inconsistencies and omissions throughout the book. The legend at the front, for instance, does not include many of

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the abbreviations used on maps in the text. The major problem as I see it, however, is an over-reliance on a structural model, derived from decontextualised ethnographies, that is simply projected upon site plans as an explanation without further recourse to archaeological data. If done at a general enough level, it is possible to make almost anything fit. At Danangombe, for instance, we are told that an "east-queen/west-king division corresponds with the distribution of wall designs" (p. 67). Yet at Great Zimbabwe this east/junior and west/senior status is reversed so that west becomes 'front' and 'public' while east becomes 'back', 'sacred' and, presumably, 'senior'. The enclosures on the south side of the hill Huffman now associates, because of their door notches, with the king's wives, rather than with "spirit mediums as I once thought".
In short, to re-work a phrase from Tilley and Shanks, Huffman seldom presents enough contextualised archaeological data to form even small 'resistances' to his interpretations. Yet at the same time, many of the hypotheses he suggests are intriguing and will undoubtedly be of interest to future researchers. This is a book that will generate much discussion and debate. It deserves a place on the bookshelves of everyone interested in African prehistory.

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Tom Huffman's Snakes and Crocodiles is the pinnacle of some two decades of research and publication, starting with a triad of provocative papers in the early 1980s and focussing on Great Zimbabwe, undoubtedly one of the key places in southern Africa's pre-colonial history, and one of the most written-about archaeological sites in the continent. There can be no doubt that, in Snakes and Crocodiles, Huffman weaves more aspects of Great Zimbabwe's central stone architecture into his interpretation than any previous writer; aspects ranging from the overall design of the stone walling to the smallest detail of decoration are drawn into a schema that, in turn, rests on a 'cognitive structure' that spans the best part of a millennium. Indeed, it is this comprehensiveness that Huffman uses to claim the veracity of his interpretation. What is important, he writes, is "the relevance and success of the overall interpretation compared to other alternatives. The hypothesis based on the least dubious assumptions, covering the most data and with the greatest coherence and predictive potential is superior" (p. 8).

Snakes and Crocodiles ranges across a wide territory, including Venda ethnography, Danangombe, Khami and other stone-walled sites, music, animal slaughtering patterns and ceramic decoration. But the hub of the argument - and the focus of its contribution to southern African historiography - is in Part II, titled "Beyond the Ethnography". Here, Huffman interprets both Mapungubwe and Great Zimbabwe in the style of grand structuralism. Much remains the same, or similar, to the earlier papers, although it is useful to have the work pulled together between two covers, and lavishly illustrated. Other ideas are new, or are significant modifications of Huffman's previous interpretations. Huffman sees Mapungubwe as the fons et origo of the 'Zimbabwe Pattern', a cultural system in which a sacred leader and a small number of officials lived secluded in an elevated enclosure, close by an area reserved for the leader's younger wives and for national rituals. Other wives lived in a separate residential area with an initiation centre, and commoners were housed in a protective circle surrounding this elite area (p. 13). This spatial mapping of social relations originated, Huffman believes, with the incoming 'Leopard's Kopje People', ancestors of the present-day Shona. These migrants settled in the Limpopo river valley, founding a number of villages of which the most important was the so-called 'K2' (an anachronistic name harking back to some extraordinarily bad archaeological fieldwork earlier this century). After a relatively short period, the occupants of K2 relocated to the summit and periphery of nearby Mapungubwe Hill, where the leaders, who had formerly lived at the head of the village, now positioned themselves at the
summit of the Hill, physically distinguishing themselves from their people, and
signifying power and prestige with altitude and separateness. This new spatial
arrangement was expressed in Mapungubwe's architecture. Buildings, the use of
symbols, and settlement design were integrated with social relations in a tightly
welded unity - the Zimbabwe Pattern, the essence of what it was, and is, to be
Shona.

The key transition here was the move from K2 to Mapungubwe Hill. Was this a
pragmatic event that had significant social consequences? Huffman had earlier
argued that it was a result of population increase, stimulated by an expanding
society taking advantage of trade with the East African coast. But in Snakes and
Crocodiles this interpretation has been modified: "when a settlement is first
established, its spatial layout reflects current social relations, but later, because of
the dynamics of a living community, the layout is out of step, and spatial patterns
have to be adjusted. Topographically the K2 area was not appropriately shaped
for the elite pattern. Since the later court was deliberately sited at the bottom of
Mapungubwe Hill and the leader lived on top from the beginning, Mapungubwe
was probably established so that the new social order could be spatially
expressed" (p. 188). In conjunction with this notion of the conscious manipulation
of symbolism for the purposes of power, Huffman offers a new interpretation for
the golden rhinoceros found in a grave on Mapungubwe's summit, and surely one
of South Africa's yet-to-be-recognised national symbols. The rhinoceros,
Huffman believes, was an early Shona symbol of kingship, a metaphor for
solitariness and aggression, representing the fear and awe that the ordinary people
of Mapungubwe town should feel for their sacred leader.

Great Zimbabwe was the direct successor to Mapungubwe, marking the
sacred office of kingship with symbolic birds in deliberate distinction to the angry
rhinoceros of the earlier Shona capital, but in other respects continuing the now-
established traditions of Shona social organisation and settlement layout. Again,
much is familiar from Huffman's earlier work, although there are some
refinements and modifications. The Western Enclosure is still interpreted as the
King's residential compound (contra Peter Garlake and others, who have seen the
entire Hill as the domain of spirit mediums), but the secluded area off the Western
Enclosure becomes the place where the King's younger wives lived. The Eastern
Enclosure is still argued to be a centre for the performance of national rituals, but
some of the soapstone birds, most of which were found in this part of the Hill, are
now interpreted as representing women. The stone ruins in the Valley, separated
from the Hill by a dare, or court, remain the residential area of the King's wives,
under the supervision of the First Wife, or vahozi, whom Huffman places in the
so-called 'Renders Ruin', with its cache of precious artefacts. The valley enclosure
marked, uniquely, by a phallic monolith inserted in a stone groove

is read as the place where the King's wives were secluded for childbirth;
presumably, the younger wives made their way down the precipitous path from
the Hill as parturition approached. The Great Enclosure is still interpreted as an
initiation centre, although no longer for girls alone, as in earlier papers: "the Great
Enclosure was built on the edge of the wives' area because the school belonged to
the domain of women. At the same time, its male/female orientation and internal theme reflected the joint participation of young men and women in this institutionalised preparation for married life” (p. 152).

The trends in the historiography of Huffman's own work seem to be a determination to maintain the principles of classic structuralism, despite the welter of criticism of such an approach both in archaeology and in social and historical studies in general, while allowing greater nuance than his earlier male/female, Hill/Valley binary oppositions; in Snakes and Crocodiles, wives have moved to the Hill, boys are to be found in the Great Enclosure, and both men and women have been carved metaphorically as birds. In general, life at Mapungubwe, Great Zimbabwe and sites that followed later, such as Danangombe and Khaim, has become more dominated by symbolic considerations. People living in the elite areas of these towns must have spent a lot of time ensuring that they were doing the right thing in the right enclosure at the right time.

Making such symbolic interpretations - what Huffman calls 'cognitive archaeology' - is a classic problem of method. Neither the stones of Great Zimbabwe nor the soapstone birds and the Mapungubwe rhinoceros - can speak, and all were made prior to direct written description, and beyond the reach of present-day oral histories. Huffman's approach stands on two pillars - the utility of a normative theory of the human mind, and the reconstruction of a direct ethnography, stretching back from his own informants to the Portuguese in the early sixteenth century, rendering the leap from the known to the unknown a matter of a century or so, rather than half a millennium. As Huffman readily acknowledges, his interpretation will stand or fall on these premises and textual interpretations.

Huffman's early concept of the Zimbabwe Pattern was inspired by Adam Kuper's 1980 paper, "Symbolic dimension of the southern Bantu homestead" - a straightforward exegesis of a wide range of ethnographic evidence resting on Levi-Straussian binary oppositions. However, the form of structuralism in Snakes and Crocodiles is closer to James Deetz's more complex concepts, with intersecting pairs of opposites arranged in the three 'dimensions' of 'life forces', 'status' and 'security'. Thus the Zimbabwe Pattern can be expressed and is indeed represented - as a diagrammatic map of the Shona mind. Secular and sacred, private and public, front and back, and east and west are opposed along the dimension of life forces. First and second, male and female, senior and junior, and older and younger give their paired expression to the intersecting dimension of status. Wives and court, guards and musicians, and king and followers intersect again on the dimension of security. Although this 'cognitive structure' is developed in explanation of the past, the effect is eerily futuristic, conjuring up the image of passing through bone and tissue to a virtual reality inside the cranium, where a three dimensional geometric form rotates like an image on a computer screen.

Huffman uses these "underlying principles of organisation" (p. 113) as the framework for understanding the expression of cognitive structure in Shona ethnography and the architecture and symbolism of settlements. For
example, the ethnographically derived oppositions of judge/messenger, ritual
sister/principal muchinda, and the archaeologically recorded oppositions of Area
A/Area B and passage entrance/rock entrance, are placed in dimensions
considered appropriate. Similarly, geometric designs in the stonework are read as
representing the crocodile, snake and ritual pool of water and, in terms of the
generalised cognitive structure, as symbolising the dimensions of life forces,
status and security. In turn, each of these key designs is seen to have both male
and a female forms, adding a further layer of complexity to the mental template.
Additional substance is given to the ethnography by including the Venda as
Shona, on the grounds that both the rulers of historic Danangombe and of modern
Venda are Rozwi. Huffman is at pains to assert this connection again in Snakes
and Crocodiles - it has proved to be a point of contention in his earlier writing -
because the Venda ethnography provides the institution of the domba, the
initiation school, which is a key element in his interpretation of Great Zimbabwe.
In introducing his method of research in Chapter 1, Huffman is assertive in
rejecting both "historical particularism", which he characterises as the belief that
"day-to-day behaviour - usually political - is the only reality, or at least the only
historically recoverable reality" (p. 5), and "narrow inductivism" - the idea that
"valid conclusions can be reached only by means of some 'correct' method of
analysis, one that leads from the data to the answer" (p. 5). He tells us that, in
contrast, he will move from a general model - "a few well attested premises of
human behaviour" - to the particularities of the case study, in this case "ancient"
Zimbabwe. But, in practice, the argument in Snakes and Crocodiles is not
developed in this way. Instead, Huffman moves backwards and
forwards between the abstract structure of the Shona mind and the detail of the
ethnography and the archaeology, a recursive approach that elaborates the
"premises of human behaviour" while extracting detail and associations from the
mass of the ethnography and stone ruins that will illustrate the empirical
manifestation of cognitive structure.

To achieve this elaboration, and to be able to use the full range of the ethnography
as a single source for interpretation, Huffman must first establish the integrity of
"Shona" as a linguistic and cultural entity, stretching back to the time of Great
Zimbabwe and beyond. This he does remarkably briefly, and by depending on
sixteenth and seventeenth century Portuguese sources and the distribution of
people who call themselves Shona today. "These [Portuguese] eyewitness
accounts", he writes, "make it clear that the Zimbabwe culture was the product of
a Shona-speaking society. Even today the majority of Zimbabwe culture
settlements lie within a Shona-speaking area". Issue can be taken with both of
these statements. The "eyewitnesses" were nothing of the sort, but were rather
compilers of second or third hand traders' stories, and it is anachronistic to read
them as linguists or anthropologists, in a position to identify a 'culture' or a
'society'. Huffman says as much himself, but three pages from the end of the book
and after the Portuguese accounts have been used uncritically as an anchor for the
ethnographic record. He concludes that De Barros, whose 1552 publication Da
Asia has been used without question throughout Snakes and Crocodiles, relied on
sources that were "clearly second hand and garbled", and that he "even confuses a wall design with a characteristic of ancient ruins in Arabia" (p. 211). And it is ironic that Huffman should rely on the argument that the distribution of people today and in the recent past has any bearing on where they may have been living a thousand years ago when his explanation for the inception of the Zimbabwe Pattern depends on the migration of a 'Leopard's Kopje People' into the Shashi-Limpopo area, and when his more general interpretation of the origins and subsequent history of the Iron Age in the whole of sub-Saharan Africa is by far the most migratory model to have been offered in recent years.

On the basis of this argument for Shona "sameness" (p. 6), through four centuries of travellers' tales, ethnographic accounts and recorded oral histories, Huffman matches details of the archaeological sites that fall into the same time period with details from the written record, based on what he describes as a "straightforward premise": "since social and settlement organisations are different aspects of the same thing and products of the same view of the world, a continuity in settlement pattern is evidence for a continuity in social organisation and world-view. Methodologically, therefore, there is identity or at least no crucial difference between settlement patterns in the ethnographical present and archaeological past, we can infer identity in world-view and social organisation" (p. 8). There is an important corollary to this equivalence between social and settlement organisation, latent here but made explicit later in Snakes and Crocodiles. It is not merely that social meaning can be read from the archaeological evidence - the well established practice of ethnographic analogy - but rather that settlement patterns themselves restrict the things that people can do in the places where they live; Huffman sees them as "permitting" and "making possible" a "limited range of behaviour" (p. 103). The Shona are, to turn a cliche, where they live, because their cognitive structures do not allow them to step outside the boundaries of their dimensions of thought and binary-coded reading of the world. For archaeology, this is a very useful premise indeed, because it means that the bare traces of a long-abandoned village can be read with the same confidence as a rich ethnographic monograph. Rather than being subsidiary to historical and ethnographic texts, archaeological evidence becomes equal or superior allowing, where necessary, ethnographic sources to be modified to fit the archaeological evidence: "once general principles are extracted, archaeological data provide the historical context in which to judge ethnographic detail. Thus, there is a reciprocal relationship between archaeology and ethnology" (p. 213).

The key sites in making this connection between social and settlement organisation are Danangombe, capital of the Changamire Rozwi from about 1693 and the early 1800s, and more strongly represented in the historical ethnography than any other Zimbabwe Pattern town, and Khami, the essential bridgehead towards Great Zimbabwe. As Huffman moves between the ethnography and the archaeology, matching figures of authority in Shona society with different parts of the stone architecture of the ruined sites and ritual customs with patterns in the walls, he becomes increasingly confident in his interpretation, and probabilities

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slip into certainties. He reasons, for example that as Danangomb e is the third largest chief's capital after Great Zimbabwe and Khami, it must have had a large court, or dare, and that "the only suitable space" for this is that part of the site lacking residential structures. "Exposed rock near the main entrance probably marks the location of the actual dare, and one of the stone terraces here may have served as the judge's seat" (p. 20, my emphasis). Moving next to the Venda (whom he has associated with the Shona under the common rubric of 'sameness'), Huffman finds a smaller, private court used by royalty. Therefore - and swinging back three centuries - "the same kind of distinction must have existed at Danangombe, the Rozwi capital, and there should have been a royal court in the leader's area" (p. 22, my emphasis). This is despite the fact that such a structure cannot be observed in the archaeological remnants of Danangombe.

Locations for the Palace, women's residences and the initiation school are teased out in similar fashion. Here, the key association between crocodile symbolism and royal residence is introduced. Huffman notes that Venda chiefs are associated with crocodiles metaphorically and, because the continuity between Venda ethnography and Danangombe is by now taken as given, a stone pillar decorated with chevrons from Danangombe can be placed in the royal residence, even though it was discovered out of context: "the original location of this decorated pillar was not recorded, but it was probably found in the Main Building where most early investigators focused their attention" (p. 30, my emphasis). Women's residences are marked by mihombwe, or long vertical grooves in the stonework, which Huffman believes represent furrows in the field and the right of a man to sow his seed. The position of the initiation school (domba) is marked only by an open space with two doorways, and an ambiguous stepped platform. But the 'sameness' between Venda and Shona, and the principle of equivalence between social and settlement patterns, means that Danangombe must have a domba, and the two-doored space is the only possibility left once other parts of the site have been accounted for.

By now, the trap which Huffman has set for himself is apparent. To argue recursively between an ethnography that is based on a dubious assertion of continuity and equivalence, and an archaeology that preserves only the imprint of the richness and variety of the ways in which people lived is to risk circularity: this social custom elucidates an aspect of the archaeology, while that feature of the settlement pattern justifies the relevance of the ethnography. And such circular reasoning is apparent in Huffman's continuing interpretation of Danangombe's domba. The domba, remember, is not actually visible - its presence is inevitable because of the 'sameness' of the Venda ethnography, and there is only one place left for it at the site. But once adumbrated, this shadowy domba is interpreted in very concrete terms. A cord design on nearby walling is read as a python (snake of the water, and womb), a dark line as the pool where the python is to be found, and a check design as the ubiquitous crocodile. This is taken as proving the 'sameness' between the residents of Danangombe and the present-day Venda; a connection which is, of course, inevitable, because it was this assumption of 'sameness' which allowed the interpretation of Danangombe's domba to be made
in the first place. Then, in a second acrobatic loop, the physicality of the Danangombe domba and its associated symbolism is taken as an empirical given - cast in stone, so to speak - and used to interpret other, more ambiguous archaeological sites, which in turn justifies the statement that Danangombe is merely one of a kind: "domba imagery helps us to understand the symbolic code at a number of dzimbahwe, so the domba at Danangombe could hardly be unique" (p. 92).

Huffman moves from Danangombe to Khami, taking us closer to Great Zimbabwe. The ethnohistorical connections are much less secure. However, because of the dictates of ethnographic 'sameness' and the correlation between social organisation and settlement layout, an equivalence in Danangombe and Khami's archaeology will indicate an equivalence between Khami and the ethnography, including present-day Venda. The argument continues to gain rhetorical momentum. At Khami, "the only large open space with rocks lies in the centre between the river and the present-day museum; and this open area must therefore have been the court" (p. 51, my emphasis). Crocodile symbolism is used to deduce the part of the site that is the Palace, and to support the interpretation that "the ruler was secluded in a place complex imbued with crocodile and snake symbolism" (p. 62). Symbols in Khami's stonework, interpreted as marking female areas at Danangombe, are firm evidence for identifying female areas at Khami: the house of the ritual sister (marked by a combination of pool and female crocodile designs), the residence of the king's young wives (pool design and secluded location), and the area where the king's wives were secluded during childbirth (pool design).

Again, though, there is the trap of circularity, this time embracing the theoretical premises of Snakes and Crocodiles as well as the details of interpretation. The key crocodile, snake and pool designs have been interpreted as such, and identified in both male and female forms, by virtue of their physical location within sites, principally Danangombe, and these physical locations have in turn been interpreted through a structuralist reading of the ethnography, principally modem Venda sources. But later, the position of these designs within archaeological sites is used to support the veracity of the structuralist reading of the archaeology and ethnography, and to justify the cognitive structure of the Zimbabwe Pattern, and of the structuralist reading of human behaviour in general. "As a rule", Huffman tells us, "the designs at the back of most buildings emphasise fertility, those at the side designate status and those that encircle afford protection. Decorations on the front wall appear to proclaim the characteristics of sacred leadership and thus involve all three dimensions" (p. 114). Thus, though a process of argumentative slippage, the designs in stone have now become the givens, verifying the general cognitive premise of three dimensions and paired oppositions. This is followed by a more audacious extension of the argument. Because the meaning of the designs has now been 'verified', their reading can survive permutations in a different spatial order: "even when these designs were combined and placed in different locations, they stood
for related themes" (p. 114). In turn again, the presence of snake and crocodile symbols, now presumed to be unambiguously understood, can be used to interpret other stone symbols such as monoliths and towers, stepped platforms, spears, charms and ivory tusks. Where the pattern is violated - as in initiation centres - in which symbols seem to have multiple meanings and appear in unexpected positions - this can be explained against the Rosetta Stone of snakes and crocodiles. "These ambiguities and reversals", Huffman claims, "are due to the liminality associated with rites of passage, and normal rules were probably deliberately flaunted when domba buildings were constructed. Since these reversals are a response to the norms embedded in the cognitive model, they strengthen its validity. The exception proves the rule" (p. 117).

Finally, after one hundred and twenty two pages in which archaeology and ethnography are welded into a robust rendering of the inner structure of the Shona mind, Huffman arrives at Great Zimbabwe. Now, the key site in the Zimbabwe Pattern can be interpreted with assertions of certainty. "The hut terraces of the nobles surround the large Hill Ruin on top - and this was clearly a palace" (p. 128, my emphasis); "the Western Enclosure behind this massive wall contains three to five metres of house Dhaka, the only substantial deposit anywhere in the Hill Ruin, and this must have been the secluded compartment of a sacred leader" (p. 129, my emphasis); "the herring-bone and hombre must surely refer to the ritual sister" (p. 132, my emphasis).

The weight of previous reasoning provides a momentum for understanding the Great Enclosure at Great Zimbabwe, always the weakest and most contentious aspect of Huffman's interpretation of the site. His problem is that the architecture and symbolism of the Great Enclosure is complex, and does not fall into neat, paired oppositions, even with their extension into three intersecting dimensions. But, as the reader by now knows only too well, there must be a domba at Great Zimbabwe, and the Great Enclosure is the only viable candidate. The problem of the detail can be dealt with by reverting again to the argument of reversals in the face of liminality. Thus the location of symbols of status (the large and small stone towers, zebra designs and crocodile patterns), which should be on the side of the Great Enclosure because of the determining effect of the Zimbabwe Pattern cognitive structure, are "misplaced" at the back as an act of "spatial reversal", indicating "multivalence". Case proved.

Is this interpretation - and the reading of Danangombe, Khami and other stone-built settlements between the Zambezi and the Limpopo - part of a "scientific study of cognitive archaeology" (p. 213)? Decidedly not. Whatever formulation is used to define scientific method must include the adjudication of evidence, careful reasoning between alternative points of view and an openness to modifying generalised theory in the light of particular circumstance. Snakes and Crocodiles is written with verve and passion, incorporates an impressive range of ethnography into its interpretation and reflects its author's close knowledge of some of the most significant archaeological sites in southern Africa. But there are repeated violations of historical method. Huffman fails to take into account the context in which any ethnographic source was written, to identify or contextualise
any fieldwork 'informant' or to refer to an archived transcript. This is presumably because he believes that "contrary to the opinions of some critics, it does not matter who the informant is as long as he can articulate some of his culture's values and attitudes" (p. 12); an extraordinary dismissal of shelves of writing by anthropologists and historians. Huffman resurrects the 'savage mind' so favoured by an earlier ethnography: "in common with other Bantu-speaking societies, Shona and Venda do not analytically separate descriptions and interpretations of the social world from descriptions and interpretations of the natural world, as is done in Western society" (p. 118). And he fails to engage with other points of view. Different interpretations of Great Zimbabwe, or previously published criticisms of his approach, find little mention in the text and, often, no place in his bibliography. As an attempt to write history, Snakes and Crocodiles succeeds only in taking history away, consigning 'the Shona' to timeless entrapment in a cognitive structure that has allowed no significant change over almost a millennium, and to the 'dark continent' which has been Europe's prejudice of Africa for generations.

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Ever since David Randall Maclver suggested it as a strategy, archaeologists interested in making sense of the

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As Huffman points out in his introductory chapter, there is sufficient justification for such a strategy, given the historical, cultural and linguistic links between the Mutapa state, nineteenth and early twentieth century Shona societies and the Venda, and the further association of all three with various Zimbabwe-type settlements found throughout Zimbabwe and neighbouring portions of Botswana and South Africa. If one accepts these arguments, then the combination of all
three categories of sources should provide an 'ethnography' of Shona societies of considerable time-depth, spanning almost five hundred years. In theory, this 'ethnography' could be used, first, to assist with the interpretation of the archaeological record for this same (i.e. Khami) period, and then to establish whether the cognitive structures associated with this later period had their antecedents in the more distant past (i.e. during the Great Zimbabwe or Mapungubwe phases). This is precisely what Huffman does, and in some considerable detail, carefully comparing the 'degree of fit' between the spatial layouts and architectural features of thirty-one Khami period and eighteen Zimbabwe-period sites, with his 'ethnographic model' of sacred leadership. In so doing, as he notes, he has adopted a somewhat modified form of the 'direct historical approach' to the interpretation of the archaeological record first developed by Julian Steward (p. 6).

The central premise to Huffman's interpretation of Zimbabwe-type settlements is that the stone-walled portions represent areas occupied by members of a royal elite, who ruled over these communities by means of a system of sacred leadership. By combining ethnographic details concerning this institution as recorded among the Venda and in the Mutapa state, as well as information about Shona and Venda initiation and divination, with an analysis of the relative spatial positioning of various structural, architectural and decorative elements, Huffman also seeks to generate a model of the 'cognitive structures' of these societies. To do this, Huffman begins with an examination of the duties and functions of the male leader (mambo), his messenger, diviner and other court officials, the physical structures associated with each of these and their spatial relationships (Chapter 2). Having established what to expect in terms of buildings, architectural features and spatial organisation, Huffman then proceeds to identify the remains of these different features in various Khami-period capitals such as Danangombe, Naletale and Khami, and at lesser sites of similar date. Next, he uses the same strategy to define and identify the 'female' components of the same sites, especially the residences of the leader's ritual sister, mother, and wives, as well as the dambo, or initiation school (Chapter 3). More controversially, he also offers an interpretation of the meanings of the different geometric designs that occur on some of the walls at these sites, by linking them to the broadly similar symbols which occur on Shona divining dice (pp. 67-68) and various Venda artefacts (p. 89).

In Chapter 4, Huffman combines these different elements to provide a detailed synthesis of the cognitive structures which, he argues, underlay the spatial patterning at these sites. In brief, Huffman suggests that in order to function, each royal settlement had to have five components, "a palace, guards, court, royal wives' area and place for followers" (p. 104), which were arranged in relation to one another "according to three dimensions: (1) life forces, (2) status, and (3) security" (p. 105). From these principles, Huffman derives two sets of diametric opposites which related to, respectively, a front:back division of space according to life forces into public and sacred domains, and a left:right status-based division into junior or female, and senior or male, areas. (Note, however, that in some
cases female status was on a par with male status, and hence not all female areas can be considered structurally subordinate to male areas). In addition, each settlement was also organised into a series of concentric, and hence protective, residential zones around the mambo's palace.

According to Huffman, the meanings of the various stone designs found on some of the walls of these sites, are also explicable in terms of this three-way structure. However, their symbolic import was more complex, as the designs also contain metaphorical referents to 'pools' and 'mountains', and 'snakes' and 'crocodiles'. These latter paired metaphors form much of the imagery associated with the institution of sacred leadership among the Venda, and in Shona oral traditions.

Five basic designs occur, either singly or in different combinations with one another. Their spatial location on individual walls (inside/outside), and within the site as a whole (front/back and left side/right side), can also vary. Using the designs on Shona divining dice as an initial guide, Huffman identifies cord and chevron and herringbone and check designs as the equivalent female and male symbols for, respectively, life forces and status, and dark line designs as the female symbol for security (p. 114 and Table 3, p. 117). He also suggests that there was no need for a 'male' symbol in stone for security, as this was provided by the hill-top location of the palaces themselves (p. 114). Each pair of symbols had additional metaphorical referents such that those concerned with status were associated with crocodiles, those about life forces with snakes, and those regarding security with 'pools' and 'mountains' (Table 2, p. 68). (Note that the metaphoric referents for life force and status symbols are mistakenly transposed in the unnumbered chart on p. 114). However, because these designs can also occur in a number of locations and different combinations, Huffman argues that the specific meaning of individual symbols was much more varied (see below).

Having established a basic model for both the layout of settlements and the meanings of the various decorative elements, Huffman then applies this model to Great Zimbabwe and a number of other contemporary dzimbahwe (Chapter 5), and the earlier site of Mapungubwe (Chapter 6).

Point by point, Huffman highlights what he considers to be similarities between the layout of Great Zimbabwe and later Khami-period sites, and ultimately concludes that each of the five major components of the site "were organised according to the same three dimensions used during the Khami period" (p. 154). In his analysis of other dzimbahwe, however, he notes that the relational order between the four key areas (mambo's, ritual sister's, royal mother's, and young wives') within the palace was not fixed (pp. 162-164). Thus, for instance, at some sites the ritual sister's residence is to the left of the mambo's, while at others it is to the right. The reasons for this are not fully elaborated, however. There seems to have been even more divergence from the model at Mapungubwe, which lacks, for instance, any plausible evidence of a domba (initiation school), or separate enclosures for the ritual sister or royal mother. Consequently, if the basic foundations of the cognitive structures Huffman claims to have identified, did indeed originate at Mapungubwe (or perhaps K2, see p. 188), they underwent considerable elaboration following the shift of economic and political power to
Great Zimbabwe. In the final chapter, Huffman points to a number of other possible differences between the Great Zimbabwe phase and later periods, including what he considers to be evidence for male and female circumcision schools (such as the sites of Kubu and Tshitaba in the Sua Pan area of Botswana), and the subordinate status of spirit mediums.

As I do not have an intimate knowledge of either the 'ethnography' Huffman uses, or the detailed layout and archaeological record of all of the sites he describes, I shall restrict my comments on Snakes and Crocodiles to a consideration of Huffman's theoretical and methodological position. To reiterate, Huffman's model is based on a composite ethnography of 'Shona' peoples spanning approximately five hundred years, and what he describes as the systematic 'testing' of this model against the archaeological record of both this period and that of the preceding Great Zimbabwe and Mapungubwe phases. Huffman describes this approach as a modified version of the direct historical approach, based on the identification of 'sameness' rather than on the use of analogy. Without further discussion, Huffman accepts the evidence for cultural (and probably linguistic) continuities over this seven hundred year period as a sufficient justification of the relevance of his ethnographic model for understanding the archaeological record for the same period. Contrary to Huffman's claims, however, the direct historical approach is no less analogical than other approaches to the use of ethnographic data. Moreover, as Alison Wylie has noted, the existence of cultural continuities between ethnographic source and archaeological subject does not necessarily guarantee that the two situations were similar in other respects. The direct historical approach, in other words, just like other forms of analogical reasoning involving ethnographic data, requires the establishment of the relevance of observed similarities on both source- and subject-sides of the argument (Wylie 1985).

On both accounts, Huffman's method is somewhat open to question. From the perspective of sources, for instance, the use of Shona ethnography is rather cavalier. We are told, for example, that he has "discussed in depth the organisational principles of both stone ruins and recent settlements with about ten adult Shona-speaking men over a wide area of Zimbabwe" (p. 11). Not only does ten seem to be a rather small sample on which to draw conclusions about Shona cosmologies, but also nowhere in the text are we provided with a detailed discussion of what these interviews revealed. Instead, Huffman periodically refers to his 'Shona informants' to substantiate a claim about which no other information is available. Moreover, when his Shona informants cannot help, Huffman turns to ethnographies of other peoples, and in so doing employs the type of ethnographic analogy about which he is so disparaging in his introduction (as, for example, in his use of Pedi, general Sotho-Tswana, Ndebele and Bemba ethnography on pages 148, 197 and 200-202). My point here is not that Huffman's informants did not say these things, but rather that we need a much fuller account of this data so as to be able to assess its relevance to the interpretative problem on hand.
In so far as the subject-side is concerned, a more critical perspective on the 'ethnographic' model needs to be adopted, with attention being given to establishing independent means of establishing its relevance. For instance, there is only limited discussion of either how or why a system of sacred leadership developed at Mapungubwe. Or why other explanations of the changes in settlement layout between K2 and Mapungubwe are less appropriate. Equally, there is no discussion of either why settlement layouts became more complex following the shift in power to Great Zimbabwe, or why, for instance, Khami-period sites tend to be more elaborately decorated with stone symbols.

One of the reasons why Huffman's account fails to provide this kind of information is the limitations of the direct historical approach, which is only suited to tracing continuities between past and present, but not the genesis of forms or their development through time. This is not to say that continuities are irrelevant, but we need to examine developmental sequences as well.

I was also concerned by the lack of discussion about how the same symbol can mean different things, and why it is appropriate in certain contexts that this should occur. For instance, the combination of herringbone above check around the ritual sister's quarters at Naletale is interpreted as signifying 'senior female status', whereas the combination of a dark line above herringbone on walls forming part of the young wives' quarters at the same site is regarded as signifying 'protection of female fertility' (Table 3, p. 117). In the latter case, however, one would expect a cord design (which is associated with snakes and hence life forces, i.e. fertility), rather than herringbone, which is associated with crocodiles, and hence status.

Indeed, the same combination of a line above herringbone, where it occurs on the front wall of Naletale, is interpreted in precisely this way (i.e. as meaning, respectively, protection and senior female status). In other words, one is left wondering why herringbone in one position can connote status, yet in another it is regarded as signalling fertility and life forces. Without a fuller discussion of the polyvalency of symbols, this type of interpretation just seems rather 'convenient', since otherwise there would be a situation where an area identified as being associated with 'young wives' (i.e. a junior social category) is associated with the symbol for status and seniority.

Despite the criticisms raised above, overall, I found many of Tom Huffman's arguments and interpretations quite convincing, and he is to be congratulated for having drawn together a fascinating body of data in his analyses of such a comprehensive range of dzimbahwe. For this reason, among others, Snakes and Crocodiles has undoubtedly advanced our understanding of these sites, and will be an important sources of reference for future scholars interested in the subject. This being said, the text is rather one-sided, and leaves the reader wanting to hear a different opinion. Let us hope that those who disagree with the substantive details of Tom Huffman's argument,

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will respond as extensively and in as much detail as he has provided here.

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Introduction
Snakes and Crocodiles: Power and Symbolism in Ancient Zimbabwe by Tom Huffman is an ethnoarchaeological study of stone buildings found on the Zimbabwe plateau and the adjacent areas in Botswana, Mozambique and South Africa. It aims at understanding the world view (use of space), origins and development of the Zimbabwe culture through settlement organisation and cultural structures. This culture is a product of the Shona and other groups occupying the plateau and adjacent areas. In this review I examine the basis of the book's ethnographic model, followed by its application on Zimbabwe tradition sites, and then consider aspects of cultural continuity and change presented in later chapters.

Defining the cognitive elements and structures
Snakes and Crocodiles is essentially a cognitive archaeological study that examines prehistoric values and beliefs of Zimbabwe tradition society. The theoretical discourse is straightforward, but arguing that "groups of people sharing the same world view organise their settlements according to the same principles wherever they live .... (p. 6) overlooks the contribution of the environment in shaping human lives (but see p. 103). The method calls for the use of an "appropriate" "variation of the direct historical approach" (p. 6) where the relevant ethnography of a prehistoric group's living descendants is sought, and a model built from it. This is then extended back in time and applied to the older archaeological situations. Ethnographic records of the last 500 years as recorded by the Portuguese and other observers are used. In addition, some of the sites of the Zimbabwe culture date to this period, making them contemporaneous with and explainable (in spatial and cognitive terms) by ethnographic data. This model, termed the Zimbabwe Culture Pattern, derives from the Central Cattle Pattern (Huffman 1986; see also Chapter 6, p. 175) first identified at Schrøda and K2 (Bambandyanalo) in the Shashi-Limpopo valley. Briefly presented, the Zimbabwe Culture Pattern is as follows: a sacred leader ruling with the assistance of a sister lived in a decorated palace. This palace was surrounded by guards and inside it were houses for messengers, senior court officials, etc. The back of the palace was reserved for rituals while a private law court was used to settle disputes among royalty. Outside the palace were houses for the king's wives, which also incorporated an initiation centre. The king's mother lived at the edge of the wives' complex. Surrounding the royalty were the houses of commoners, located mostly to the west of the palace. They assembled in a public court located in between their houses and the palace. Completing this pattern were the neatly built enclosures outside the commoner houses, belonging to rivals for political power. Huffman argues that this model can be projected into the prehistoric past where he demonstrates that the spatial pattern generated by the sites is repetitive. Thus the ethnographic present, represented archaeologically by Khami period sites (AD 1450-1800) has a settlement pattern linked to a specific social organisation and this pattern is linked to a similar one from the archaeological past, the Great Zimbabwe period (AD 1290-1450). Thus both Great Zimbabwe and Khami-type sites share the same social organisation and world view. The spatial pattern
reflected in the layout of these sites has an underlying cognitive structure. The different structural components of the settlements together with the men and women who formed an important component of palace/court life, and the ordinary people reflect three dimensions of life forces (e.g., back-front, east-west, sacred-public, etc.), status (e.g., male-female, senior-junior, older-younger) and security (town guards-palace guards, etc.). The wall designs and architectural features signify status on the side, fertility at the back and protection around the perimeter. To construct the model around the archaeological data Huffman uses Portuguese written records, Shona and Venda customs and oral traditions. Venda ethnography is justifiably used because of the Rozvi (Singo) element in the Venda language and the existence of the Great Zimbabwe-type stone enclosures among the Venda in the Zoutpansberg area of South Africa (Loubser 1991). This requires a clearer definition of the elements of Venda ethnography relevant to the interpretation of the Zimbabwe culture. In applying Venda ethnography, Huffman should have considered other groups who were in contact with the Zimbabwe culture peoples in the past, such as the Korekore. Korekore ethnography is relevant to the study of the Zimbabwe culture since some of their dynasties were part of the historical Mutapa state, a northern extension and branch of the Zimbabwe tradition (Pikirayi 1993). Muringaniza & Ruwitah (1996) argue that it is inappropriate to use Venda ethnography on all Great Zimbabwe culture sites, saying 'links with the political and social organisation... is not clearly demonstrable'. While the connection between the Singo movements to the south and the decline of Danangombe is strong, Singo cultural influence on pre-existing groups in the Zoutpansberg is largely undocumented. This is important in isolating the relevant detail necessary in the interpretation of space in Khami period sites. There is danger in applying pre-Singo cultural traits on the earlier Great Zimbabwe phase sites unless one is certain that these had Shona origins. Portuguese written accounts have some inherent limitations but if carefully used they provide data on settlement organisation in the Mutapa state. Apparently Huffman relies more on these than on Venda ethnography. No Shona oral traditions refer to Great Zimbabwe, given the time depth involved. Reliable traditions on the Zimbabwe culture date only from the 17th century and these belong to groups claiming direct descent from the ruling Mutapas in northern Zimbabwe and the Manyika in the east. The oral data gathered from informants (pp. 1011) should have been transcribed for future verification (see Beach 1994b), otherwise it is suspect. This has a bearing on the validity and application of the model.

Applying the model

The model is first applied on Khami period sites. It is recognised that space is a gendered construct, and can be expressed as such using symbols. State power belongs to important men and women in and around the palace. There are various institutions and structures connected with these officials and these can be identified archaeologically. The basis of the arguments presented here is historical: the Rozvi, remembered in Shona traditions for their power and role as king makers, are connected to late Khami period sites such as Danangombe. This provides "a rough contemporaneity between sites and the ethnography" (p.
South African Archaeological Bulletin 18). However when individual sites are examined in relation to the model, there are some discrepancies. The identification of the court and palace at both Danangombe and Khami is fairly clear, as suggested by the elaborate architecture. Both sites have, to the west of and adjacent to the palace, a large open space which Huffman has interpreted as the public court. At Khami the private court for the royalty is located on the Hill, but at the more recent Danangombe it is not identified. Instead we are informed that such courts are found among the Venda (p. 22). Since the Singo are Rozvi one would expect this pattern at Danangombe as well. Another discrepancy lies in the identification of the nefhasi, the 'master of the ground'. There is no Shona equivalent to this muchinda (king's representative). The nearest possibility is nevanji (brother/son/brother's son of the king), or mbokurume (son in law/brother in law of the king) who was the chief confidant (Mudeenge 1988:89, 95). This office is important in Shona court etiquette but it is not discussed, nor are the structures connected with it identified. The audience chamber in the palace should be associated with the Chief Door Keeper, and during the 18th century the Mutapa had six such officers. Could they all have used a single house? Perhaps they occupied several houses in the palace. The identification of Venda court official khotsimunene (chief's brother, also a legal expert) is inadequately supported by the evidence presented. At Naletale, why do the people have first to go through the private court to see the messenger to the king? (Fig. 2.22, p. 37). The house on top of the palace walls probably served functions connected with a subdivision to the south-west, and not for the messenger. There is no evidential basis for the identification of the ritual area in the southern part of the palace. The identification of the messenger's houses at the other sites such as Lotsani B, Chamabvepfa, Rupungubwe, Breslau B and Kongezi is either vague or unsatisfactory. At Dzata, a Venda site in the Zoutpansberg area built in the 18th century by the Singo, the location of the messenger's house is not given. At Nhunguza in northern Zimbabwe the reasons for the identification of the messenger, diviner and ritual sister are thin. Huffman admits that the identification of sacred locations (pp. 42-45) is difficult. Ritual activity such as rainmaking does not necessarily have to be performed at the back of the palace. The 18th century Mutapas, for example, had two prominent cults connected with rain making (Mudenge 1988:126-134).

Every palace has guards, praisers and musicians but if the siting of the structures connected with them is to go strictly by the cognitive rules as laid out in the pattern, then the location of the thondo (Venda word meaning guards' quarters) at Danangombe is problematic. Why built it to the north instead of the west of the palace? Why should the musicians and praisers sit on the exterior terrace platforms at Danangombe, Khami, Naletale and Regina when Portuguese documents locate them "in the first room of the king's palace at the outer door" (p. 50), probably a subdivision of the audience chamber? The determination of the function of the outlying enclosures at Danangombe, Khami and other sites is difficult given the limited excavation data. These enclosures could be identified with some male
chief officers of the king since their duties required their presence at court instead of the provinces (Mudenge 1988:84-93).

Female activity areas on the various Khami period sites are identified using Shona and Venda traditions and symbols such as those expressed through wall decoration. The herringbone design is identified with women. The pattern is related to Shona diving dice (hakata), pairs of which symbolically represent old and young women.

Herringbone designs are, however, not clearly portrayed on the hakata, but instead on the decorative motifs of some Zimbabwe culture ceramics. Ceramics are customarily identified with women (Collett 1993; Ndoro 1996). Huffman (1989:5) sees a positive correlation between group identity and design style, and the latter as repetitive and integrated codes of cultural symbols learned and transmitted within groups of people. This must be translated to wall decoration through a procedure yet to be determined. Besides, there were female palaces in the Mutapa state. Since Nsalansala and Regina (pp. 71-72) are profusely decorated with herringbone, and since Huffman considers the pattern with female symbolism, these enclosures could be female palaces. Historical evidence suggests that some of the chief wives of the Mutapa had their own houses, land and servants and exercised complete jurisdiction over their subjects (Mudenge 1988:108). However these palaces did not function as initiation centres (domba; see below).

A detailed architectural evaluation, supported by extensive excavations and dating would show that the building identified with the female servants at Danangombe was a subsequent appendage to the palace. Evidence from Machemma shows the necessity for a well dated building sequence to understand the changing functions of the site over time. Here, Huffman identifies three building phases, dated from the mid 17th to the 18th century, and he can locate the changing position of the messenger's house. Applying this to other sites such as Makahane, Regina, Danangombe, Khami, etc. provides the necessary caution against a synchronous interpretation of these structures. This problem resurfaces in the second part of the book, 'Beyond the Ethnography' where the ethnographic model is applied to Great Zimbabwe period sites, beginning with Great Zimbabwe itself. Huffman argues the peoples from these two periods were essentially the same culturally and therefore organised their settlements using the same principles wherever they lived. I will focus on the development and growth of the town of Great Zimbabwe. Architectural studies initiated by Whitty (1957-61) (on the Great Enclosure) and refined by Collett et al. (1992) (Valley Enclosures) and Chipunza (1994) (Hill Complex) are important since they give chronology prominence in the diachronic interpretation of the site. This aspect is ignored by Huffman (pp. 153-154) where he argues that the Great Enclosure and the Hill Complex were built around the same time, and that the developmental phases of the former consistently conformed to a domba arrangement. However current studies show that complex architectural changes did occur through time in the social 'meaning' of spatial organisation, and Huffman's model does not adequately account for them. Some Valley Enclosures, for example, were occupied later when most parts of the site were abandoned. The Hill Complex was not a single
building event. A sequence of building and rebuilding began from the west going eastwards. This invalidates any synchronous interpretations of the use of space on the site. Huffman also ignores context (meaning is context-dependent) and change over time. Specific architectural features do not always carry a single meaning. Space is a cultural variable, expressed in symbols meant to convey meaning both to the observer and the user. Venda court language is impregnated with crocodile symbolism and Huffman sees Danangombe and other Khami phase sites expressed in the same way. However this symbolism is not conspicuous among the Shona (Beach 1994:249). The Portuguese did not see it in the Mutapa state. As Huffman points out on p. 29, the association between crocodile and sacred leadership is only

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a general one, probably restricted to the ritual sphere. There should be no reason why wall decorations and features such as the tall vertical ridges (mihombwe) at Danangombe and other Khami phase sites should be interpreted as such. Killing a crocodile among the Tswana was considered taboo (Chirenje 1977:69). Whether crocodile symbolism is important in Tswana court language is unclear. Since Venda has a strong Sotho-Tswana element the cultural importance attached to the crocodile should be traced from there, and not from the Shona as Huffman suggests (p. 206).

Continuity and change

An interesting discussion in the final two chapters centres on the gold rhino and the Zimbabwe birds. They are interpreted as symbols of leadership (pp. 189-191). Huffman uses Hassan’s (1988) ‘political technology’ presumption where such objects could have represented the intention of Mapungubwe and Zimbabwe leaders to enhance their links with supernatural forces before the rise of their respective states. Although the meaning of these objects is largely speculative, they are considered within a framework which views the rise of complex societies from the manipulation of both economic and ritual power. Such models are worth pursuing in attempting to account for the rise of the Zimbabwe state.

Domba, an essential institution in Venda life, has never existed in Shona society. If it had, the Portuguese records would have mentioned it. Huffman admits domba did not exist among the Shona during historical period times, and attributes its disappearance to the Portuguese wars and the mfecane. Initiation into adulthood among the Shona does not require such formal classes as boys and girls are gradually educated into adulthood. Thus the identification of the Great Enclosure at Great Zimbabwe and other sites such as Chiwawa, Zvongombe (N), etc. with the domba is probably incorrect. On the other hand, two roughly walled sites, Bhila and Kubu, which are atypical in plan, and associated with large stone calm fields, may be explained in terms of initiation. However they are probably early 19th century and of Sotho origin. The Sotho, who passed through the area where Bhila is located, are partly responsible for collapse of the Rozvi state (Beach 1984:53) and may have built the structures. Sites such as Black Adder belong to a class of loopholed stone structures described in Pikirayi (1993), and dating to the 17th century. They could not have functioned as domba.
Conclusion

Snakes and Crocodiles attempts an overall interpretation of the symbolism in the stone walls of the Zimbabwe culture sites, despite some reservations that the interpretative context of these sites is yet to be found. It compels the reader to imagine how people operated from within and outside the stone enclosures. While there is room for improvement, especially in the adoption of analytical procedures that take into account architectural developments and chronology, and a more careful scrutiny of the ethnographic and oral sources, any cognitive study that tries to understand the ideology of the occupants of these buildings is difficult. While the reader may find some of the interpretations unconvincing, other aspects of the structuralist exegesis offered are worth considering. "Models of complex phenomena are high-level, nonempirical constructs. They can neither be confirmed nor falsified. Although their 'validity' can be compromised by a disparity between their premises or implications and 'facts' (low level constructs), selection among alternative hypotheses rests on the merit of relative adequacy at any given time with specific epistemic framework not only by correspondence to empirical constructs, but also by consistence with the current knowledge and by the coherence of its internal logic and mode of reasoning. ...The archaeological record is silent on many issues. Without an imaginative analogic framework, we cannot even begin to talk about subsistence or settlements" (Hassan 1988:166).

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Following the pioneering paper Snakes and Birds in 1981, Huffman has produced a series of other works using a structuralist approach now more fashionably called cognitive archaeology to explore the underlying structure and meaning of the organisation of space of the Zimbabwe culture. His ideas have largely relied on ethnographic data which he has used to interpret the rich archaeological record of this important cultural epoch of southern Africa's past. Snakes and Crocodiles seems to bring together the various ideas which Huffman has presented in the past, with some minor revisions. As far as he has pioneered cognitive archaeology in southern Africa, thereby taking us beyond the traditional empiricist level, Huffman deserves applause. Those of us who have followed Huffman's ideas no doubt welcome Snakes and Crocodiles in as far as it is now possible to follow the arguments through in a single book, rather than having to refer to papers published in many different journals. In Snakes and Crocodiles, Huffman presents a detailed examination of a multiplicity of Zimbabwe culture stone buildings, starting with those of the Khami phase and going backwards to the Mapungubwe phase. The examination of the sites particularly focusses on the physical structures, related material culture and the relationship between these and socio-political organisation. Using what he argues to be relevant ethnography as well as Portuguese historical records, we are taken through the world of symbols of the Zimbabwe culture and how this system of symbols and organisation of space relates to social and political organisation. Indeed, Huffman almost succeeds in taking us on a real journey through time and bringing history to life. The reader is almost able to feel
and touch the mambo, his ritual sister and all the other court officials who occupy
the palaces. While this is a refreshing way of writing history and archaeology, in
my view it also constitutes one of the major weaknesses of Huffman's writings since
the Snakes and Birds paper. Huffman is so convinced of the application of his
ethnographic models that what should be offered as interesting hypotheses are
presented to us as almost indisputable fact. I have no problem with Huffman's
clear and detailed knowledge of the archaeology of the Zimbabwe culture, an area
to which he has devoted so much energy, time and academic investigation. I
continue, however, to have reservations about the 'goodness of fit' between the
archaeology and the ethnography, particularly his insistence on the application of
Venda ethnography to what he agrees are principally ancestral Shona sites. Also
of concern has been his continued perception of Great Zimbabwe as having been
constructed according to a preconceived plan, a plan which is then repeated with
some variations at other madzimbahwe through space and time. In all respects,
Huffman has justifiably been criticised in the past by such scholars as Blacking
(1985),

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Soper (1988), Mahachi (1991) and more recently
Chipunza (1994). It appears that in Snakes and Crocodiles Huffman has more or
less ignored all the major criticisms that have been levelled at his work. Where he
has attempted to address these, his reasoning remains unconvincing. I will in
this review focus on only a few of them. I start with the construction sequence at
Great Zimbabwe as it relates to Huffman's overall model of the organisation of
this town and the meanings assigned to the different stone structures. Huffman's
model gives the impression that the town's construction was a 'one off' event
following a set of cultural rules and that it is thus possible to come up with an
overall interpretation. Huffman himself accepts that the town was built and
occupied over a period of at least 150 years. Given such a long period of time, it
is quite clear that new structures were being added all the time and it becomes
difficult to imagine how the meaning of space and function of structures would
have remained constant. It has recently been demonstrated, using a Harris matrix
analysis (Chipunza 1994), that we can trace the construction sequence at Great
Zimbabwe over a long period of time. It then becomes more likely that the
different generations and rulers would have continually added new structures and
assigned different functions and meaning to them. While it is generally accepted,
as Huffman quite rightly argues, that people of the same culture share a basic
symbolic code and world view and that the organisation of space is meaningful,
the fact remains that any interpretation must take cultural dynamism into
consideration. This may well explain why, at the inter-site level, the different
Zimbabwe culture sites do not exactly mirror each other. This being the case, is it
any surprise that some of the structures found at Great Zimbabwe, the Great
Enclosure for example, are not present at other madzimbahwe? (although
Huffman identifies the areas where they should have been).
One of the most controversial aspects of Huffman's interpretations of the
Zimbabwe culture has been his conviction that both female and male initiation
was practised by these Shona ancestors and more specifically, that the Great Enclosure at Great Zimbabwe was used for this purpose. He expands on this in Snakes and Crocodiles. Several scholars (e.g., Blacking 1985, Beach 1991) have questioned the application of Venda ethnography as far as this is concerned. Further, it has been asked why this practice stopped among the Shona and why the earliest Portuguese records failed to mention it. I find Huffman's answers to all these questions rather unconvincing, particularly the suggestion that male initiation was affected by recent unstable military conditions. He suggests that while Shona ethnography is silent on this, appropriate data can be obtained from their neighbours, for example the Sotho. This sounds rather far fetched support for an argument which in any case has failed to stand up both on archaeological and historical grounds. Interestingly, it is not made clear why it stopped for females, if ever it was practised. The argument has also been questioned on the grounds that the investment put into the construction of the Great Enclosure is inconsistent with the importance of initiation rites and even the modern or historical Venda did not invest to that extent in domba. Snakes and Crocodiles does not appear to have a clear answer for this.

Two other aspects of Snakes and Crocodiles beg comment. One is Huffman's interpretation of the Zimbabwe soapstone birds and the other is his brief discussion of 'Royal Burials'. I start with the birds. Towards the end of the book, Huffman argues the birds were part of the initial establishment of the Zimbabwe culture power at Great Zimbabwe and thereafter were no longer a necessary part of the politics of power, and so they have not been found anywhere else. Yet, from his earlier discussion, it would appear that birds continued to be an integral part of the overall symbolism and sacred leadership in the Zimbabwe culture through to the Khami ethnographic present. In the light of this, I find the suggestion that the birds were carved only once in the context of the establishment of power and never again unconvincing. They have to have had other meanings and significance that continued throughout the history of Great Zimbabwe, rather than just at the beginning.

To date, the only Zimbabwe culture burials known are the recent discoveries at Tulamela. We thus do not yet have enough knowledge of the burial practices of this period and culture. In the light of this, the discussion of 'Royal Burials' at the end of Chapter 5 is thin, based on very little archaeology and even ethnography. It could well have been left out as it does little to enhance the general thesis. Despite the above observations, as pointed out earlier, Huffman deserves credit for this thought-provoking book which takes southern African archaeology beyond traditional concerns in the discipline. Its arrangement, starting from the ethnographic present and going back to the origins of the Zimbabwe culture is a useful technique, never mind that there are some problems in the relationship between some of the sources of the ethnography and the archaeology. Snakes and Crocodiles is an important book which has to be read
by everyone who wants to try to understand the Zimbabwe culture stone buildings and their place in the social and political domain of their builders.

REPLY

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Introduction
The Zimbabwe Culture Pattern is a generalised model of settlement organisation at the level of cultural norms. Archaeologists study settlement organisation because it is one approach that can illuminate the values, ideals and beliefs of the past. To understand the complexities of past cultures, we need to consider the relationships between things and not just the things themselves. Settlement organisation is a useful theoretical approach for understanding past cultures because it provides a framework for investigating relationships.

Much of the criticism of Snakes and Crocodiles stems from different approaches to the past. This is probably why the opinions are so divergent. Some colleagues think Snakes and Crocodiles proceeds in logical steps, others that it is circular. Some think the ethnographic data lack context, others think the associations between the ethnographic and archaeological data are well-established. Some question the whole enterprise, others challenge the details. I thank all the reviewers for their contributions. To respond, I begin with philosophical issues.

Scientific method
First, it is simply not possible to derive an interpretation of Great Zimbabwe (or any other archaeological site) from archaeological data alone. There is no inductive method, no series of tried and tested steps that can lead to the right answer. Philosophers of science have been aware of this delusion for almost 100 years. As a result, the emphasis in science is on testing a potential answer - an hypothesis - rather than on its derivation. In a nutshell, we apply competing hypotheses to the data and ascertain which is better. We do not, as Hall erroneously believes, read social meaning from the archaeological data, we apply social meaning to the data.

Valid premises make a study possible. In archaeology and history, however, premises do not form a deductive link to conclusions. As in all empirical sciences, there are hidden premises, and the conclusions must still be tested. Beach seems to think I constructed this approach to suit myself and to avoid awkward questions. On the contrary, I adopted multiple-hypothesis comparison because it is the standard procedure of science. Beach has made a significant contribution in his own work not because his method is right but because it is appropriate to his questions, and he cross checks (i.e. tests) his answers. Part of the testing procedure in science is to examine the premises of competing hypotheses. Of the five premises mentioned in Snakes and Crocodiles (p. 6), two are worth repeating here. First, to create order human societies divide their physical environment into discrete locations where only a limited range of activity
is permitted. True or False? The strength of a rule may vary depending on social and other contexts, but this premise is fundamentally true. Second, spatial locations have social significance and consequences: they provide physical backdrops for social behaviour and in many cases help to shape it. True or False? One may argue about the role spatial organisation plays in social change, but its social significance is indisputable. These valid premises make the study of spatial organisation possible and worthwhile.

To understand social significance, we must understand the other society's world view, otherwise we impose our own. A model of settlement organisation must therefore be derived in part from the relevant ethnography of the descendants of the people who lived there. In the case of the Zimbabwe pattern, some of the relevant ethnography was also contemporaneous with some of the archaeological sites. Thus the model was created through a recursive relationship between ethnographic sources and archaeological data dating to the ethnographic period. Once developed, the model was applied to older archaeological sites.

In one sense this 'direct historical approach' is an argument by analogy - as Lane rightly points out. But in another sense it is an argument about sameness. In the normal argument by analogy there is no reason to expect further similarities when the source and subject are not culturally related. When the model is derived from the descendants, however, we can expect further similarities as long as the similarities are necessarily associated, as long as there has been no significant cultural change and as long as there is no evidence to the contrary. It follows from this last point that each hypothesis has to be tested.

Whether change occurred is an empirical question, and it is the role of archaeology and history to discover the answer. The direct historical approach is one method. Perhaps the name should be restricted to the derivation of the model, which is direct, and not to its application. Whatever the case, the approach has great value.

I adopted the direct historical approach partly in answer to earlier criticisms by colleagues, such as Beach. This approach eliminates the chronological gap between ethnographic data and archaeological sites such as Great Zimbabwe that are older than the ethnographic record. I show later, however, that time is not the real issue.

Incidently, the Portuguese documents from the beginning of the ethnographic period were written largely by people who had been in the country or by people who compiled reports from others who had been there. Contrary to Hall's innuendo, analyses of these sources do show that Shona speakers dominated the area (see Beach 1980). Indeed, the evidence is so overwhelming that no serious scholar questions the Shona identity of the Zimbabwe culture.

Circularity and recursiveness

Another methodological issue needs clarification. Hall and Denbow believe my argument is circular, seldom returning to the archaeological record for refinement, modification or elimination. A circular argument, by definition, assumes the
conclusions and premises are the same, that is, it presupposes the point to be established (Copi 1968). This tautology begs the question of proof. In archaeological circles an argument that uses the same data both to develop and to test an hypothesis is also considered circular in a loose sense because it also begs the question. The table on page 8 of Snakes and Crocodiles outlines the structure of my argument: a model derived from an 'ethnographic present' is applied to an archaeological past. No circularity here of either kind. Presumably, then, the accusations of circularity are levelled at the derivation of the model.

The model, as noted earlier, was built up through a recursive relationship between ethnographic data and archaeological features. I used 31 different sites for this purpose. In some sites an audience chamber could be identified, in others the public court, and so on. For the Khmii period, there were at least 12 excavated or otherwise exposed examples of audience chambers, nine messengers' huts, four diviners' huts, five ritual sisters' huts, seven ritual areas and two eyes. Too many similarities occurred for the pattern to have been a coincidence.

I did not mention a royal court at Danangombe, for example, or a messenger's hut at Dzata because I lacked physical evidence. These omissions were not discrepancies, as Pikirayi calls them: I was building up the model on the basis of recognisable and repeated features.

As another step in the development of the model, I used the Khmii Ruins as a testing ground for the ethnographic present. I chose Khmii because it dates to the beginning of the ethnographic present and because it has been extensively studied. By this point in the development of the model, we already know that there are a limited number of areas and structures in a palace. Among other things, Khmii provides examples of an eye, ritual area, audience chamber and diviner's hut. Denbow admits that the identifications may be right but he wants more archaeological evidence to support the claims. He uses the palace structures at Khmii as an example of decontextualised ethnographic data projected on to site plans. However, the exterior fireplace is a new concrete archaeological feature that supports the identification of the smallest platform as the diviner's hut. Likewise, the central dividing wall inside the enormous hut next to it is a concrete archaeological feature that supports the identification of that structure as an audience chamber. Clearly the argument does return to archaeological data, and the model is modified in the light of those data.

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Venda ethnography

Some reviewers specifically challenge the ethnographic component of the model. For different reasons, Beach, Hall and Pwiti question the use of Venda ethnography. This is an important issue to resolve. Details may be debated, as I discuss in Chapter 7, but is Venda ethnography relevant in principle? The answer is yes. Most of the chieftdoms predating the Singo-Rozwi claim to have come from what is now Zimbabwe; their traditional headquarters included typical Zimbabwe culture palaces by any normal definition; and their material culture was Shona. Linguistic affinities are also conclusive. Several decades ago linguists identified three components of the Venda language: Shona, Sotho-Tswana and
something else. Loubser (1991) showed that the third component was the Shona spoken by the Mapungubwe people. Significantly, the Venda maintained class distinction and sacred leadership well into the 20th century. On these two points, then, recent Venda society is even more relevant than that of the Shona. Beach's unwillingness to accept the relevance of Venda data is a result of his paradigm of extreme historical particularism: the overwhelming connection between Shona and Venda shows that Venda ethnography is a reliable source of evidence for understanding Shona prehistory. As I stated in Snakes and Crocodiles (p. 12), any reconstruction of Shona prehistory that does not include Venda data will be incomplete. I am bewildered by Hall's belief that the success of the ethnography in elucidating the archaeology is not support for the relevance of the ethnography; conclusions most certainly can confirm premises (Copi 1968). If we make a prediction from a premise and it is found to be true, the successful prediction confirms the premise. Furthermore, this success also disproves competing hypotheses if their competing predictions are mutually exclusive. In Hall's own research the ethnography of historical records elucidates the data of historical archaeology, and the close 'fit' shows that the historical record is indeed relevant. The same logic must apply to the Iron Age. Most of Hall's criticisms lack substance because of this elementary mistake about confirmation and falsification.

Stone symbols
The ethnographically derived interpretation of the wall designs caused some comment. The interpretations were first developed from Nettleton's (1984) analyses of Shona divining dice and Venda court art, which I cited. Hall somewhat hastily concludes that the interpretations were derived from the location of the designs. Location formed a later step in the process. It provided the context for the specific application of the core meanings derived from the ethnographic data. This is why herringbone combined with check at the side of Naletale referred to the ritual sister, while herringbone combined with a pool design was appropriate on the wall of the young wives' compartment at the back. In this location, the young wives represented human fertility (not a lower status as Lane supposes), and they were protected by both the pool and ritual sister. The core meanings, independent of location, refer to gender divisions of life forces, status and security. Since the core meanings are independent, they provide support for the spatial pattern. Both were related to sacred leadership.

Sacred leadership
Both Beach and Denbow criticise my emphasis and characterisation of sacred leadership derived from the ethnographic data. As before, Beach is unwilling to accept the general pattern, while Denbow dislikes boundaries and distinctions. In contrast, I believe general patterns, boundaries and distinctions are useful concepts.

By sacred leadership we first mean a mystical association between the chief and the land. In southern Africa this association also involves a link between the chief, his ancestors and God. Venda and Shona have elaborated the concept of God beyond that of other Bantu speaking peoples in southern Africa. To Venda and Shona, God makes it rain, and so on, and it is to God one must turn through the
spirits of dead chiefs. According to Hammond-Tooke (1975), for the southern Nguni at least, mediation through the ancestors was not part of traditional thought. Denbow's statement that "rainmaking results from the successful mediation with the spiritual or ancestral world" glosses over different conceptions of the ancestral world on the one hand and spiritual forces on the other.

Sacred leadership is also an institution that contrasts with other forms such as hereditary leadership and 'big men'. In theory, the ancestors of dead chiefs were supposed to choose a new Venda leader, whereas a special son was supposed to be the next Nguni or Sotho-Tswana chief from the moment he was born. Denbow's statement that "the ability to accrue political authority is widely linked with a person's ability to manipulate and control spiritual forces" applies more to 'big man' leadership than the other forms.

Next, a sacred leader is supposed to be aloof and separated from his people. Beach notes the discrepancy between Shona belief and practice on this point. Nevertheless, settlement patterns are first based on belief: one can sleep in the bathtub, but you are supposed to sleep in the bedroom. Sacred leaders were separated by such things as a special palace language, a raised palace, a divided audience chamber and highly ritualised behaviour. Even public processions (Snakes and Crocodiles p. 47) involved an entourage of guards, praisers and musicians.

All these features show that it is valid, useful and indeed necessary to distinguish between different kinds of leadership.

I emphasise sacred leadership (along with class distinction) because it has been largely overlooked. An understanding of its importance restricts the range of appropriate interpretations of Zimbabwe culture sites. Denbow (pers comm.) for example, once believed that San had lived inside the palace at Toranju (on the edge of Sau Pan in Botswana) in positions of authority. He held this interpretation partly because Toranju is a San name and because local San claim rights over the site. Other than as diviners, however, an association with the palace is most unlikely because they, as San hunter-gatherers anyway, could not have been linked to the right ancestors. What we know constrains what we do not know.

Oral data

I also used oral traditions as part of the ethnographic record, but not as a source for history. From a methodological viewpoint, I was not interested in individual memories of the past. I accepted Beach's point long ago that no Shona person now or even 100 years ago could know where a specific person lived within a 500 year old site, such as Toranju or Great Zimbabwe. Probably, no one knew that detail even 100 years after Great Zimbabwe's abandonment. For this reason I used the oral data to help determine the general rules for the categories of people that should have been in a capital and the rules for where they should have lived. My informants of course interpreted the archaeological sites in terms of their present world view and understanding of how a chief's capital should function. I was therefore more interested in their logic than their...
specific answers. For example, the Great Enclosure at Great Zimbabwe is traditionally associated with an important woman. When informants learned that certain diagnostic objects were excavated from Lower Homestead 1 (formerly the Renders Ruin), they identified that area with the first wife because "she was the only person who could keep such things".

Informants were particularly helpful at the beginning of the study by eliminating unlikely directions. Ultimately though, they were not my major source of data, for much of what they said was recorded in the professional ethnographies. They were an important supplement but not essential.

Several reviewers commented on the informants. To them, 10 seemed a small number. It was small, but then this was not a quantitative study. Victor Turner (1957), one of the most famous symbolic anthropologists, described Ndembu cosmology in great detail on the basis of one principal informant.

Incidentally, I took notes. It is nonsense to believe oral data can only be collected with a tape recorder or prepared questionnaire. Furthermore, I did not name them because I was told by colleagues in Zimbabwe that one was severely victimised for helping me.

Since I wanted informants who held traditional world views, rather than an accurate memory of the past, the demand for an assessment of their 'probable knowledge' is a misguided insistence on a 'right method'. It does not matter who the informants were as long as they could articulate some of their culture's values, beliefs and ideals. The Mashayamombe informant was the head councillor of a chief's court (Snakes and Crocodiles p. 11), the Great Zimbabwe informant was originally afraid to help because the ancestors might be angry (p. 11), and another was a herbalist (p. 12). I am satisfied that their views were authentic and representative.

A decade ago it was fashionable to challenge the notion of traditional society or a traditional value. Now it is clear that some aspects of culture are exceptionally tenacious despite the tremendous pressures of social change. As Hammond-Tooke (1994) notes, most African Christians still believe in their ancestors.

Multiple systems

Bourdillon uses a related example to make the point that people today will switch from one symbolic system to another depending on their personal and social contexts. He emphasises multiple systems to postulate that the Zimbabwe Culture Pattern was not a single coherent system. To address this challenge, one must understand the social contexts that cause people to switch. Significant for our purpose, physical location dominates these contexts. One set of beliefs is appropriate in a Christian church, for example, and another in a spirit medium's hut. In reference to courts, Hilda Kuper, in the title of her 1972 article, referred to locational contexts as 'The language of sites in the politics of space.'

To be able to switch presupposes the existence of two or more separate symbolic systems. In the 16th century there may have been as many as four other symbolic systems in operation: the Central Cattle Pattern, and those associated with Western Bantu, Christian Portuguese and Muslim Africans. All five systems were at a more-or-less
equal level of generalisation. Surely, then, the organisation of a 16th century Zimbabwe capital would have expressed the world view of Zimbabwe royalty, not these competing systems.

If another symbolic system co-existed in a Zimbabwe capital, it could not have been at the same level. This point leads us to consider the nature of normative models.

Normative models

As a normative model, the Zimbabwe Culture Pattern is not designed to investigate the details of daily behaviour. Daily dynamics include code switching as well as the co-existence of conflicting values within the same system. Among the Ndembu, for example, rituals at different times of the year may celebrate contradictory values (Turner 1957). The level of my model is too general at present to recognise a subsystem such as this.

Such dynamic events as these differ in kind from longterm structural changes, such as the transformation from ranked to class-based societies. It is difficult to recognise transformations of this second kind without a datum, or unitary standard. Normative models such as the Zimbabwe Culture Pattern can be used in conjunction with the direct historical approach to recognise structural changes. Indeed, the model and approach were instrumental in recognising the transformation from the ranked society at K2 to the class-based society at Mapungubwe. Normative models and the direct historical approach have theoretical limitations, as Lane notes, but they can be used to discover discontinuities as well as continuities.

The long continuity proposed in Snakes and Crocodiles is a conclusion, rather than a methodological premise as Bourdillon believes. Some reviewers appear reluctant to accept the continuity because of an assumption about the phenomenon of time itself. Other than universals such as night and day and the passage of the seasons, time is a cultural construct (for an African perception of time, see Hallpike 1979). There is no evidence that time per se was a catalyst for change in the African world; and so the passage of time only provided more opportunities for change. We cannot presume that the 14th century was different from the 16th century simply because it was older. Once again it is the role of the archaeologist to discover the answer.

Both Pikirayi and Pwiti challenge the continuity on the strength of Chipunza's (1994) diachronic study of the Hill Ruin at Great Zimbabwe. I did not reference Chipunza's work because it was published after Snakes and Crocodiles had been submitted. Nevertheless, it does not change my interpretation for a number of reasons. First, Chipunza bases his chronology on uncalibrated radiocarbon dates and an out-of-date interpretation of the ceramics. Period III is equivalent to Mapungubwe and dates from approximately AD 1220 to 1290, not 1085. Second, there is no evidence for wailing in Period III. One of the oldest 'P' walls (no. 1A in Chipunza's Figure 14) rests on top of about one metre of Period III deposit. The edge of this deposit formed a sloping bank which supported the inner foundations of the wall. A radiocarbon sample from the top of the bank was dated to AD 1190±50 (Pta-2705) now calibrated to between AD 1270 and 1300. In the Great Enclosure, the earliest P-walling has a calibrated date of between AD 1290
and 1310, and Q-walling appears to have been invented there at about AD 1350 (Huffman & Vogel 1991). According to Chipunza's own study, only Wall 12A and those around the Recess Enclosure were constructed in Q-coursing. Thus the vast majority of the Hill Ruin was built in about 50 and certainly no more than 100 years. Third, even if it had taken longer, the palace hardly changed shape. Shape and contents are also important in determining function, not simply the walls themselves. Not only did the shape remain similar, the contents in the Period IV deposit in the Western Enclosure show that the function remained the same. Huts with raised pot stands, for example, were found stratified in a relatively small area (Snakes and Crocodiles pp 128-129), and huts like this were not found anywhere else. Finally, one must remember that a spatial interpretation is first applied to the upper level of a site. Only afterwards does one consider the stratigraphy and developmental sequence. Obviously, the servant's enclosure at Danangombe was late in the sequence, as Pikirayi notes, but its late appearance does not alter its identification. Likewise, an early entrance at Danangombe (Snakes and Crocodiles p. 73) shows that access to the palace changed over time, but there is no evidence that functions within the palace significantly changed as a result. One chief may switch the location of structures within his palace, but unless he creates new categories, this is not evidence for change at the level of the model. The Zimbabwe Culture Pattern presents normative rules about spatial categories and their relationships: it does not dictate precisely where any category must be located.

As a normative model the Zimbabwe Culture Pattern is also not primarily concerned with regional variations. In fact the model has to subsume such variation in order to extract the common underlying principles. As in any society, however, we expect variation between structures that served the same purpose. We know that a relatively small set of organisational principles can generate a wide range of appearances. The visual difference between Western houses serves as one example: the outsides are often strikingly different yet the insides are divided into similar functional areas. Christian churches provide an example of another point. Large churches in Europe should be constructed in the form of a cross with the back, or chancel, facing east. The chancel is referred to in these terms even if it was not possible to orientate the church that way because of the available space. Zimbabwe settlements were no different. The back of the palace at Danangombe faced south, rather than east, because that was where the large rocks were located. The model is also not equipped to explain why Khami period palaces are more elaborately decorated than palaces during the earlier Zimbabwe period. This question leads from meaning to motivation. I doubt any current model of the past is able to identify prehistoric motivations.

The Zimbabwe Culture Pattern is a cognitive model in the sense that it was created and carried in the minds of Zimbabwe people. Hall unjustifiably turns this point into a statement about the structure of the Shona mind. I presume he derives this surprising characterisation from a failure to recognise the difference between Levi-Straussian structuralism and a structuralist approach. I have no
idea how any brain is structured. I only claim that Zimbabwe people carried a pattern of social and spatial relationships in their minds in the same way we all hold sets of beliefs.

**Initiation**

The most controversial aspect of this cognitive model concerns circumcision and domba. The rejection by Zimbabwean colleagues of these initiation schools appears to rest on two pieces of negative evidence: Portuguese documents do not mention them, and there is no living memory. Frankly, I cannot understand why Beach thinks

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the documents should present complete ethnographies. The documents about the Mozambique coast that Beach alerted me to some years ago (Snakes and Crocodiles p. 203) show only that circumcision was not universal: they do not prove that circumcision never occurred on the plateau. The physical sites suggest that it did.

Pikirayi accepts the physical evidence for circumcision lodges (other than the Black Adder site), but he ascribes them to Sotho-Tswana rather than Shona. A Sotho-Tswana interpretation, however, can hardly apply to the site on top of Gombe mountain in Buhera (Snakes and Crocodiles p. 198; Caton-Thompson 1931:130). There the walling included monoliths and was built in the same P-style as at Matendere. Furthermore, Buhera is home to one of the longest-standing Shona chiefdoms in Zimbabwe (Beach 1980). To be consistent, then, Pikirayi must accept that circumcision occurred at some time among some Shona, or at the least among some Shona subjects.

The argument against domba is much the same. In this case readers should be aware that the Great Enclosure is not unique in terms of function. Snakes and Crocodiles presents five other examples from the Khami period and seven from the Zimbabwe period. Therefore one-off explanations will not suffice. Despite its rejection by Zimbabwean colleagues, no other interpretation covers the locations, contents and shapes of these twelve examples better than domba. The validity of this interpretation is not diminished by a poor understanding of why the practice ceased.

Furthermore, the lack of traditions about circumcision and domba is irrelevant. The following example demonstrates this point. Life in Zimbabwe was unquestionably different before the 18th century. Before then, sacred leaders ruled from secluded palaces inside relatively large capitals. Yet today, there is no memory of large traditional towns. This is why local people at Great Zimbabwe believe there was one village on the central hill and another in the valley around the Great Enclosure. Regardless of how one calculates the population, extensive residential complexes below the hill had to have been the result of large numbers of people living together. Local beliefs can be wrong.

**Concluding comments**

Once again we return to scientific method. Arguments that the past equates with the present because one cannot imagine otherwise are logically invalid. They take the deductive form of a single premise (the present situation) leading to a single conclusion (the same situation in the past). But cause must precede effect, and so
the present cannot affect what has already happened, only its interpretation. Valid interpretations of the past must be supported by data contemporaneous with the event in question. Otherwise, they beg the question of proof. The evidence suggests to me that anthropologists, archaeologists and historians in Zimbabwe need to rethink their interpretations of the past. Snakes and Crocodiles advances our understanding of the Zimbabwean past by presenting a coherent model of settlement organisation and symbols in stone. Too many similarities occur in too many different realms to be a coincidence. To advance the study further, critics need to propose alternatives that fit the data better.

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