

Debating Great Zimbabwe

^{a*}Innocent Pikirayi and ^{b*}Shadreck Chirikure

^a*Department of Anthropology and Archaeology, University of Pretoria, Pretoria, 0002, South Africa;* ^b*Department of Archaeology, University of Cape Town, Rondebosch, 7700, South Africa*

*Email: innocent.pikirayi@up.ac.za; shadreck.chirikure@uct.ac.za

'Iron Age archaeologists in South Africa have been somewhat captive to a structuralist model for the interpretation of settlement space' (S. Hall 1998, 235).

Introduction

We are humbled by Huffman's acknowledgement of our contribution to the archaeology of Great Zimbabwe (Chirikure and Pikirayi 2008) and his spirit of constructive debate (Huffman 2010). Any reasonable scholar cannot deny the positive influence that his research has had on the archaeology of Great Zimbabwe. However, research is not just about reaffirming what has been done; it is more about reinterrogating the data even if it means swinging the pendulum of knowledge violently to push back the frontiers of knowledge. Often, some scholars seem reluctant to accept new thinking that contradicts what they believed earlier. As Martin Hall (1996, 6) once remarked, 'facts like kings are no longer absolute, they cannot sustain themselves forever'. Hall was implying that without a detailed interrogation of both old and new data, there is really no point in doing research. With Great Zimbabwe the extremes would be that cognitive structuralism has explained everything so we do not need to study the site at all or that we should close our minds to new insights that put strong dents in the dominant interpretative reconstructions.

And yet we know very little about the site. It is within this constructive spirit, that we hope our contribution to the debate on the interpretation of Great Zimbabwe will further highlight gaps in existing knowledge, deficiencies in current scholarship and point useful suggestions and future directions in the understanding of this important settlement. In an article published in this journal, Paul Lane (1994/95) cautioned researchers about the 'abuse' of sources such as ethnography through factors such as selective application and even misunderstanding of that ethnography. It seems as if Huffman has not heeded this call for his reaction is no more than a mishmash of incoherent evidence as one critic pointed out a decade ago (Beach *et al.* 1998). The key issue is that his cognitive hypothesis is not supported by material culture in the enclosures. Use and function are correlated, so by implication the material remains in the enclosures should reflect what they were used for. Huffman does not seem to recognise that this is the central thesis in our argument, though we agree that it still needs to be refined with more work and critique, such as the one that he has provided.

It is important right from the onset for readers to remember that this debate comes slightly over a decade following a critique of Huffman's (1996) *Snakes and crocodiles: Power and symbolism in ancient Zimbabwe*, a volume that attempts to interpret what Huffman calls the Zimbabwe Pattern, which, in his view, developed from the Central Cattle Pattern from the beginning of the early second millennium

AD (see Beach *et al.* 1997, 1998). For so long Huffman's preoccupation with the general has held the archaeology of southern Africa captive (M. Hall 1998), frustrating attempts to understand individual sites and their contexts (Anderson 2009).

And yet we know that no matter how significant rules of human behaviour are, individual sites and places experienced different evolutionary trajectories that require critical academic engagement. In a region as poorly archaeologically studied as southern Africa, it is important to interrogate individual sites critically with the aim of contributing to the bigger picture.

It is therefore refreshing to note that Huffman (2009) concedes in his reaction paper that we need more research to understand the Valley Enclosures. This explicit admission suggests that we also need to develop new meanings for all the other enclosures because his model is characterised by the connectedness of events and meanings. This implies that if the meaning of one changes, then that of the others will also change. Although Huffman points to some omissions on our part, for example, the amount of deposit in the Great Enclosure and the issue of metalworking in the valley, we can easily provide supporting evidence in the form of calculations and excavation reports by Collett *et al.* (1992) archived at the Great Zimbabwe Conservation Centre. As such, we limit our response to the key areas below:

- Huffman's misunderstanding of the Portuguese written sources and of Zimbabwean history in his discussion of the Karanga (the inhabitants of the Mutapa state) succession system;
- Huffman's treatment and interpretation of the radiocarbon chronology of the Hill Complex of Great Zimbabwe and its implications for the entire settlement;
- Huffman's interpretation of Great Zimbabwe using models or recent research findings from the middle Limpopo valley, a region that saw the rise and development of the Mapungubwe state (AD 1220-1250), a precursor to the former;
- Huffman's attempts at providing a universal meaning for all Zimbabwe type-sites.

Karanga ethnography and the succession principle

The model of political succession and shifting headquarters that we proposed in our paper was based on Karanga political activity in the Mutapa state since the sixteenth century, where centres of political power moved in accordance with change of dynasty as well as resource availability. To our knowledge, the Mutapa state, which was a direct successor of Great Zimbabwe on the northern Zimbabwe Plateau, continued to exist in that area as well as the adjacent Zambezi Valley lowlands until the late nineteenth century, despite stiff challenges from the Portuguese, the Rozvi and other groups such as the Budya (Pikirayi 1993). In essence, this means that the Zimbabwe Culture pattern continued to exist with remarkable continuity from Great Zimbabwe. So, to assume that some traits only started after the Portuguese had been on the landscape, as Huffman implies, is not logical. In fact post-seventeenth-century developments demonstrate a southward expansion of Zimbabwe Culture settlements,

something captured in Karanga oral traditions (Beach 1980; Pikirayi 2004). Huffman uses seventeenth-century political succession in the Chiteve dynasty to argue that this was the prevailing pattern some two to three centuries earlier at Great Zimbabwe and that, by extension, the Hill Complex was the only palace at the site. We are not concerned about which succession model best suited the kings of Great Zimbabwe, since in explaining the development of the city for one can equally argue that the principle of political succession to which Huffman refers may not have been in operation at the time of Great Zimbabwe. In fact, the issue of succession is irrelevant as we now demonstrate.

Political succession has always been crucial in the survival and stability of Zimbabwe Culture states, e.g. Mutapa (Mudenge 1988). However, this did not mean that rulers got the system perfect and safeguarded themselves against disruption. On the contrary, Mutapa rulers got it wrong most of the time, leading to forceful intervention in order to settle matters. We cannot speak confidently of the succession system prevailing at the time of Great Zimbabwe, but in our article made reference to the system prevailing in the Mutapa state, Great Zimbabwe's immediate and direct successor, supporting this by using the distribution of material culture in Great Zimbabwe's constituent enclosures. According to dos Santos, kings were succeeded by their legitimate sons – their eldest sons from their chief wives - and these too had to be legitimate, or the throne went to the second or third son. Prevailing from the seventeenth to nineteenth centuries, this system is an example of adelphic collateral succession, meaning that the sons of the founding king were, in theory supposed to become the chieftainship houses. In practice, this did not happen for a complex array of reasons (Mudenge 1988, 79-84).

Succession by the brother was possible if he was thought fit to govern and demonstrated the required leadership qualities. There are recorded instances of this in the Mutapa state during the sixteenth and seventeenth centuries. In fact, the recorded history of the Mutapa state shows that there were very few limited cases of father-son succession from the late seventeenth to the beginning of the twentieth centuries! This demonstrates that the system of houses was in operation, with succession having to rotate among the houses of the founder's sons at least in theory, because in practice it became too convoluted and was decided by wars (Mudenge 1988). This is why all those who became Mutapas had claims and links to the first Mutapa, Nyatsimba Mutota, in conformity to the system of houses. As long as your father was not Nyatsimba Mutota, it was not guaranteed that you would be succeeded by your own son; the kingship had to rotate to another house.

We have not denied that fathers may have been succeeded by sons at Great Zimbabwe. Equally, those sons may have lived in their father's residences. Given the succession system outlined above, it is equally possible that some kings were succeeded by their brothers, shifting the power to other parts of the site. The chronology of the site and the development and extension of other enclosures support this thinking.

Furthermore, our point in Chirikure and Pikirayi (2008) was not so much about the succession system as about the materiality of power. We were surprised by the amazing similarity of material culture found in Great Zimbabwe's constituent enclosures. If the spatial organisation of Great Zimbabwe was as suggested by

Huffman, then the material culture ought to reflect this. However, as demonstrated in Chirikure and Pikirayi (2008), the material culture was similar and consistent with self-sufficient units. This invited new interpretations taking this observation into consideration. We therefore put forward the issue of changing centres of power in accord with Karanga systems of succession (Beach 1998) and supported by the distribution of material culture. This places a heavy dent in Huffman's cognitive interpretation, which selectively picks archaeological data and one or two decoration types on enclosure walls, whereas our model is based on the materiality of places and power. If we do more research at the site, as Huffman himself concedes, we will generate more valuable insights that would take the archaeology of Great Zimbabwe forward.

Chronology of Great Zimbabwe

The chronology of Great Zimbabwe is based on very few dated areas of the site, some with an almost complete sequence that may help inform on the development of the settlement. However, there remain teething problems, first on over-reliance on radiocarbon dates at the expense of other methods, and, secondly, the interpretation of the events that archaeologists are dating. Huffman argues on the basis of both relative and radiocarbon dating that stonewalling only appeared at Great Zimbabwe after 1300 AD, an event characterised by intensive settlement of both the Hill and the area occupied by the Great Enclosure. This would tally with his model of Great Zimbabwe structured into a town with a palace on the hill, a ceremonial centre in the Great Enclosure, a wives' area in the Lower Valley and an open space in between being identified as a 'man's area'. This is what he refers to as the Zimbabwe Pattern, the origins of which he traces back to Mapungubwe Hill (Huffman 1996, 103-104, 2007, 46, 379). On both statistical and archaeological grounds we disagree.

Essentially, radiocarbon dates are a measure of statistical dispersion and, as such, when calibrated only provide a time bracket indicative of when a certain event transpired in the past. Huffman uses the available radiocarbon dates for Great Zimbabwe (Huffman and Vogel 1991; Huffman 2007, 2009) to argue for a predominantly fourteenth-century dating of the site, further supporting his model of a structured town. In our paper, we only demonstrated the possibility of coming up with alternative interpretations of the site if one uses calibrated dates based on *2-sigma* rather than *1-sigma*, as Huffman prefers. In seeking a tighter chronology for the site, the problem with the latter is the acceptability of discarding true calendar events falling within the 33-34% of the calibration curve, as Huffman does. How does one interpret dates falling in the upper and lower quartiles or even octiles of the calibration curve (see, for example, Phillipson 1975)? Our dating of Period III and assignment of P-walling to the thirteenth century is based on these fundamental methodological considerations, as well as the reasoning that there is no basis for restricting Period III given that the Great Zimbabwe ceramics remain poorly described. It would be unimaginable to suggest that no stonewalling existed prior to 1300 according to Huffman's (2007, 2009) revised chronology. Stone walling, however rudimentary, was already occurring among Leopard's Kopje/Gumanye communities, some within the site territory of Great Zimbabwe (Sinclair 1987, 100). It is no coincidence that some of the élite were building in stone between 1250 and 1300, and evidence for this comes from the Hill (Chipunza 1994).

We view with considerable scepticism Huffman's revised chronology where *an entire century and a half (AD 1300-1450)* is described as representing the 'first stone walling' at Great Zimbabwe. This negates the dynamism of the period, particularly the developments associated with Q-walling that saw construction of the majority of the Great Enclosure walls, most probably between 1350 and 1400. The monumental nature of the structure suggests the peak of development of the site. We also now notice that the period 1450-1550 is now linked by Huffman to settlement in the Lower Valley, based on evidence from Portuguese records reporting on the Mutapa state that suggest continued settlement at Great Zimbabwe. Huffman interprets the Lower Valley as the residence of Mutapa's wives. This again is a misreading of the Portuguese documents, which Huffman takes too literally. A proper reading of the same document would reveal that Great Zimbabwe was now being controlled by a prince, or one of the sons or vassals of the Mutapa (Pikirayi 2001, 2006). According to João de Barros, Great Zimbabwe was:

...guarded by a nobleman, who has charge of it after the manner of a chief alcaide, and they call this officer Symbacayo, as we should say keeper of the Symbaoe, and there are always some of Benomotapa's wives therein, of whom this Symbacayo takes care" (Theal 1898-1902, vol. 6, 268).

The same document further suggests that, due to civil wars in Toroa and the mining areas guarded by 'vassals' of the Mutapa, Great Zimbabwe had become a pale shadow of its former self. However, royalty was still living there, and not just 'wives' as Huffman would like us to believe. Besides, this document collected in 1538, was possibly making references to events three decades old, which means toward the end of the fifteenth century, and not 1420 or 1550 as Huffman (2010) suggests.

Previously, Huffman and Vogel (1991) used João de Barros in connection with available radiocarbon dates to shorten Great Zimbabwe's chronology and argue for its abandonment during the early-mid fifteenth century. This, they argue, would support the available archaeological evidence, irrespective of the stratigraphic integrity on the terminal phases of settlement at Great Zimbabwe. What this exercise fails to understand is the process of *leaving* Great Zimbabwe and how this is reflected on the site and elsewhere.

The late fifteenth century was characterised by civil wars, which saw the emergence of the Torwa dynasty in the southwestern regions of the Zimbabwe plateau. Oral accounts dating to before the middle of the seventeenth century point to the northward movement of Karanga clans from the area of Great Zimbabwe (Beach 1980). Traditions refer to a region called "Guruhuswa", literally meaning 'the area with tall grass', which geographically may be identified with south central or southwestern Zimbabwe (Garlake 1973) or simply the Zimbabwe highlands (Beach 1980). This Karanga movement coincided with the emergence of the Mutapa state in northern Zimbabwe, identified archaeologically with the expansion of the Zimbabwe tradition northwards (Beach 1980; Pikirayi 1993). Stonewalled settlements architecturally similar to Great Zimbabwe and dating from the fifteenth century onwards have been located in the region. Some of these Mutapa capitals were still being constructed in stone when the Portuguese arrived on the Zimbabwe plateau in the early sixteenth century.

The Hill Complex, the Great Enclosure and palace identification

Our interpretation of the sequence of Great Zimbabwe was only based on evidence from the site itself, and not proxy data from other Zimbabwe type settlements. At this point, Huffman informs us that data from the middle Limpopo Valley, of some of which we were aware when we wrote our original paper, are relevant to the interpretation of Great Zimbabwe (Schoeman 2006a, 2006b). We are now informed that from AD 400/450 to 1250, the sequence on the Hill at Great Zimbabwe is best interpreted in terms of rainmaking activities.

Although the Hill has been used for rainmaking ceremonies in recent times, there is no archaeological basis to suggest that this was a major function on the site during the times of Great Zimbabwe. In fact, it would have been suicidal for a chief or any leader to entrust such functions to himself and his immediate associates, as years of persistent failure to receive rain would be interpreted in terms of failure by the ruler, something that would invite political instability or anarchy. The period 1250-1290 must have been critical for Great Zimbabwe in that regard since it marks the formative stages of the town. We know that the Mutapa state delegated such functions to the 'original inhabitants' of the land, in this case the Dzivaguru cult, or simply did not undertake such ceremonies at its capital (Mudenge 1988). Thus, the ethnography only supports the use of Great Zimbabwe or part of it for such ceremonial functions at a later stage, one when the main town was no longer used for political and economic administrative purposes, i.e. post 1550. There are other references to sacred leaders delegating rainmaking to these cults, for example those based at Khami (Ranger 1999) and we have no evidence that Khami itself was a rainmaking centre. It is thus surely more logical on the basis of cultural continuity to argue that the leaders at Great Zimbabwe would have done the same.

Our model of the settlement of Great Zimbabwe from the Hill to the Lower Valley was not based on *a priori* identification of palace remains of the kind in which Huffman engages in his response; rather, it was based on what best fits available archaeological data rather than ethnographic extrapolations from elsewhere. We do not doubt the existence of a palace on the Hill Complex on the basis of the limited evidence available. However, we contest the argument that the Hill Complex remained the palace for Great Zimbabwe's duration and the suggestion that the sequence of the Great Enclosure can be reconstructed sufficiently from the rubble of early investigators, some of whom pillaged the top levels of the structure. Our references to pillage were for post-1890 excavations (Pikirayi 2001, Chapter 1) particularly the digging by Theodore Bent, who showed total disregard of the material of the 'Kaffirs'.

Huffman (2010) argues that the archaeological context in the Great Enclosure is not secure because Early Iron Age materials have also been found in the same area. Summers (1961), excavated sections which seemed to have escaped the activities of Bent and others. He makes it clear that the earlier material was underneath the Zimbabwe culture occupation. Furthermore, Early Iron Age and Zimbabwe Culture material cultures are so distinct that it is illogical to assume that an archaeologist of Summers' repute could confuse the two. Even if we agree that the deposits are mixed, the chronology and typology of the stone walls provides independent evidence. The wall types show a development from classes P to Q. We interpret this change in

architectural style and the extension of the Great Enclosure to represent the shift of power to this area.

We are also curious as to why the issue of deposits has suddenly become a pertinent one, given that Huffman has used the same “mixed” deposits to support his own arguments. If we throw out the evidence from the deposits, we are left with another line of evidence, but the same cannot be said of Huffman’s viewpoint. Furthermore, there is a glaring inconsistency in the way Huffman interprets excavations by early researchers. On the Hill Complex, he is quick to endorse Robinson’s (1961) section drawing to support his theories, but in the Great Enclosure he doubts Summers’ (1961) conclusions because they contradict what he thinks. It is unlikely that Summers would have been as careless as Huffman implies and instead of finding scapegoats in the deposits, Huffman should accept that the material culture in the Great Enclosure does not support his understanding of the site.

Moreover, the big question is if the Great Enclosure is not a palace complex, then what is it? Its architectural history suggests that it has a multi-component layout and that the earlier walls were not as imposing as the later ones. This, as we argue (Chirikure and Pikirayi 2008), suggests that an important individual such as king resided in it. This interpretation is closer to reality given that the structure is the single most impressive structure in the town, representing the peak of the development of Great Zimbabwe. In its later stages, its layout, architectural execution and finish, and artistic symbolism point to a level of cultural sophistication, multiple functionality and complexity synonymous with a palace anywhere in pre-European Africa and beyond. To relegate such a structure, as Huffman (1996, 146-153) does, to ceremonial functions that do not need to be performed within the precincts of stone walls is simply inappropriate. In fact, Huffman’s version is closer to situations in Europe and elsewhere where temples were the most imposing buildings. However, in southern Zambezia, palaces are also ceremonial centres, as Huffman acknowledges for the Hill Complex. As such, there was no need to build a ceremonial centre *sensu stricto* as implied by Huffman and those before him, including Bent and others.

Problematizing attempts for a universal meaning for all Zimbabwe type-sites

Huffman suggest that we should have included other Great Zimbabwe type-sites, such as Khami, in our interpretation of Great Zimbabwe. The fact is that we are beginning to cast doubt as to whether the state based at Khami was a direct successor of Great Zimbabwe (see also Huffman 2007, 412). However, the whole point of our paper was to link material culture excavated in different enclosures to build meaning for those spaces. Our approach was based on understanding of the site at the local level. A comparative analysis with other sites is the focus on an ongoing research project where we are examining stone architecture and its ideological significance within the Zimbabwe tradition. Our preliminary findings suggest that Great Zimbabwe and Khami type architecture express two competing political ideologies, which at some point were contemporaneous, and which may account for why Khami was, or chose to be, different from the former. Now that we know that the cognitive structuralist approach has been jettisoned by the evidence, we take up Huffman's invitation to use the same approach at other Zimbabwe type-sites.

We cannot generalise much in the absence of detailed studies, something that has been a major criticism of Huffman's own work (see Beach *et al.* 1998). We therefore urge a more cautious approach towards understanding of the broader regional scale since there are so many differences within sites such that a one size fits all approach of the kind keenly adopted by Huffman (1996, 2007) would be inadequate for such an endeavour. For example, there are Zimbabwe type-sites such as Jaunda in the southwestern Zimbabwe area of Gwanda, with conical towers and other elements of Q style walling (Figures 1 and 2). Certainly, this site closely resembles Great Zimbabwe outwardly more than most other sites and yet we know very little about it. It is refreshing that very soon we will have some information that can help us to establish relationships between these Zimbabwe tradition sites. However, given the many outward differences between many sites of the Zimbabwe tradition, we feel strongly that approaches to the study of such settlements as advocated by Huffman bear no analytical merit as they tempt us to refrain from further discovery because 'if you have seen one, you have seen them all'. While patterns across sites did exist, the past was more complex than Huffman's model indicates.

Although we applaud the suggestion that other Zimbabwe tradition sites complement existing knowledge on Great Zimbabwe, we also wish to advise that the state society created in the middle Limpopo Valley and based at Mapungubwe near the Shashe-Limpopo confluence was different from the one that established itself in south-central Zimbabwe around cal. AD 1300. In other words, the inhabitants of Great Zimbabwe deliberately chose to develop a social, political and economic system that was not a carbon copy of the one operating at Mapungubwe a few decades earlier. A closer study of Khami seems to reveal the same.

Conclusion

While new data on Great Zimbabwe are required, we, like other scholars of the Zimbabwe tradition and the public in general, await Huffman's publication of his 1973 excavations of commoner houses next to the perimeter wall, part of which has been released in frugal instalments (see, for example, Beach 1980, 209-210; Huffman 2007, 398). These data have an enormous bearing on the urban layout and, by extension, the interpretation of the town, and it is unfortunate that they still remain unavailable.

We are strongly of the view that a more informative interpretation of Great Zimbabwe must still emerge from evidence obtained from the site itself. It is only when we have gathered sufficient data that we can make comparisons with other sites. Space syntax – a set of techniques and methods used in the analysis of spatial configurations with the objective of mapping the relationships between space and society – is one possibility for exploring Great Zimbabwe. References to the king/royalty, commoners, initiation, rainmaking etc. are key issues of social and political context, special occasions, ritual and ceremony and ownership of space. Patterns exist in the usage of space, but activities and space are not firm. There is a connection between space, gender and age. We must also note the importance of non-built spaces and whatever they were used for, be it refuse disposal, extensions of the built environment or some other purpose. Above all, material culture is important in constructing meaning so we still need a more in depth understanding of material culture from the Zimbabwe sites. Without that, our interpretations are nothing more than hand waving.

In fact, southern African archaeologists must take their cue from Simon Hall (1998, 235), who called on researchers to break away from the 'captivity' imposed by cognitive structuralist models to explore other issues that can breathe fresh air into Iron Age studies and transform them into a theoretically competitive endeavour that is not lagging, but is at the forefront of developments in world archaeology.

Acknowledgments

We thank Munyaradzi Manyanga for sharing with us the photographs and information about Jahunda. His understanding of issues behind Shona succession systems helped us to develop our argument. Simon Hall was always available to discuss the finer details about cultural structure in the Iron Age of Southern Africa.

References

- Anderson, M. 2009. Marothodi – historical archaeology of a Tswana town. London: Aitkkam Publishers.
- Beach, D.N. 1980. The Shona and Zimbabwe, 900-1850: An outline of Shona history. Gweru: Mambo Press.
- Beach, D.N., M.F.C. Bourdillon, J.R. Denbow, M. Hall, P.J. Lane, I. Pikirayi, G. Pwiti and T.N. Huffman. 1997. Review feature on *Snakes and crocodiles: power and symbolism in ancient Zimbabwe* by T.N. Huffman. *South African Archaeological Bulletin* 52: 125-143.
- Beach, D.N., M.F.C. Bourdillon, J.R. Denbow, G. Liesegang, J.H.N. Loubser, I. Pikirayi, D. Schoenbrun, R. Soper and A.B. Stahl. 1998. Cognitive archaeology and imaginary history at Great Zimbabwe. *Current Anthropology* 39: 47-72.
- Chipunza, K.T. 1994. A diachronic analysis of the architecture of the Hill Complex at Great Zimbabwe. Uppsala: Societas Archaeologica Upsaliensis.
- Chirikure, S. and I. Pikirayi. 2008. Inside and outside the drystone walls: revisiting the material culture of Great Zimbabwe. *Antiquity* 82: 976-993.
- Collett, D.P., A. Vines and G. Hughes. 1992. Dating and chronologies of the Valley Enclosures: implications for the interpretation of Great Zimbabwe. *African Archaeology Review* 10: 139-161.
- Garlake, P.S. 1973. *Great Zimbabwe*. London: Thames and Hudson.
- Hall, M. 1996. *Archaeology Africa*. Cape Town: David Philip.
- Hall, S.L. 1998. A consideration of gender relations in the Late Iron Age “Sotho” sequence of the western Highveld, South Africa. In *Gender in African prehistory*, ed. S. Kent, 235-260. Walnut Creek: AltaMira Press.
- Huffman, T.N. and J.C. Vogel. 1991. The chronology of Great Zimbabwe. *South African Archaeological Bulletin* 46: 61-70.

- Huffman, T.N. 1996. *Snakes and crocodiles: power and symbolism in ancient Zimbabwe*. Johannesburg: Witwatersrand University Press.
- Huffman, T.N. 2007. *Handbook to the Iron Age: the archaeology of pre-colonial farming societies in southern Africa*. Pietermaritzburg: University of KwaZulu-Natal Press.
- Huffman, T.N. 2009. Mapungubwe and Great Zimbabwe: the origins and spread of social complexity in southern Africa. *Journal of Anthropological Archaeology* 28: 37-74.
- Lane, P.J. 1994/5. The use and abuse of ethnography in the study of the southern African Iron Age. *Azania* 29/30: 51-64.
- Phillipson, D.W. 1975. The chronology of the Iron Age in Bantu Africa. *Journal of African History* 16: 321-342.
- Pikirayi, I. 1993. *The archaeological identity of the Mutapa state: towards an historical archaeology of northern Zimbabwe*. Uppsala: Societas Archaeologica Upsaliensis.
- Pikirayi, I. 2001. *The Zimbabwe culture: origins and decline in southern Zambezi states*. Walnut Creek: AltaMira Press.
- Pikirayi, I. 2004. Less implicit historical archaeologies: oral traditions and later Karanga settlements in south-central Zimbabwe. In *African Historical Archaeologies*, ed. A.M. Reid and P.J. Lane, 243-267. New York: Kluwer Academic/Plenum Publishers.
- Pikirayi, I. 2006. The demise of Great Zimbabwe, AD 1420-1550: an environmental re-appraisal. In *Cities in the World, 1500-2000*, ed. A. Green and R. Leech, 31-47. Leeds: Maney Publishing.
- Ranger, T.O. 1999. *Voices from the rocks: nature, culture and history in the Matopos Hills of Zimbabwe*. Oxford: James Currey Ltd.
- Schoeman, M.H. 2006a. *Clouding power? Rain control space, landscape and ideology in Shashe-Limpopo state formation*. PhD diss., University of the Witwatersrand.
- Schoeman, M.H. 2006b. Imagining rain places: rain control and changing ritual landscapes in the Shashe-Limpopo confluence area, South Africa. *South African Archaeological Bulletin* 61: 152-165.
- Sinclair, P.J.J. 1987. *Space, time and social formation: a territorial approach to the archaeology and anthropology of Zimbabwe and Mozambique, c. 0-1700 AD*. Uppsala: Societas Archaeologica Upsaliensis.

List of Figures



Figure 1. Photograph showing the rounded entrance at Jahunda.



Figure 2. Rounded entrances at Great Zimbabwe. Note the similarities with Jahunda and the neat coursing that belongs to the Q style of walling.