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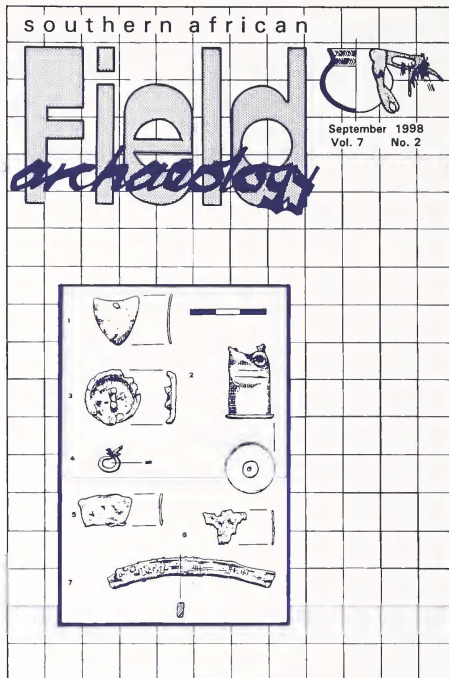
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Cover illustration:
Metal objects found in Late Stone Age contexts in the western and southern Cape, p. 106.

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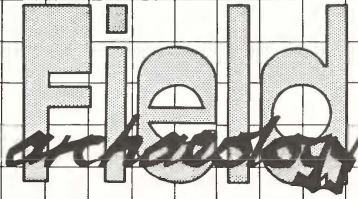
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OPINIONS

SUNSHINE, BRAAIVLEIS, RUGBY, CHEVROLET AND ARCHAEOLOGY: Should children dig and the public record rock art?

About two years ago the Boy Scouts of America introduced an Archaeology merit badge. "The purpose of this badge is to help scouts to understand the archaeological process and to recognize that prehistory and historic resources are fragile remains that need careful study and protection for the future" (Skinner 1977:17).

During the past few years public archaeology and public outreach programmes have become important to advertise archaeology to the wider public. However, different interpretations of what outreach is, and how it must be implemented, may become a major issue in the future. To some it simply means taking children on a dig and members of the public to a rock art site and showing them how to do tracings. After all, it may be argued, what is archaeology other than digging and tracing?

Should children, or untrained people dig? Is it ethically acceptable? Consider the following: A honours degree is

required to be employed as a professional archaeologist. During this period we learn the basic excavation techniques, analysis and publication of data. To do research requiring excavations, we need a permit from, and report to the National Monuments Council (NMC). In certain cases, where the NMC is of the opinion that an applicant is not experienced enough to conduct an excavation on her/his own, it is issued to a supervisor.

Now, after years of study, bombardment and exposure to the importance, ethics and morals of archaeological resources and the responsibilities of heritage management, we allow a group of untrained school kids to dig archaeological sites, because we think that is the way public archaeology is supposed to happen.

Let's return to the Boy Scouts merit badge requirements for a moment.

The requirements will expose boys, their leaders and parents to a wide variety of archaeological subjects without attempting to turn boys into professional archaeologists. Through the influence of scouts and those associated with the scouting programme, a major segment of the general public will gain a new appreciation of archaeology, and many people will become advocates for responsible heritage management.

The badge requirements are a blend of introductory archaeology and cultural resource preservation. Active involvement in a mock dig, an excavation, or in lab work is included, **but not until after the scout understands the archaeological process and how sites are dated, and has collected information about known sites"** (my emphasis) (Skinner 1997:17).

Thus, only after theoretical training the scouts may do a mock dig or an excavation. Notwithstanding, as professional archaeologists are we not violating the conditions of the permit, issued on the researcher's experience, and so become accomplices to the destruction of precious protected archaeological material by untrained people?

Should the public be introduced to tracing rock art?

The arguments are similar to the above, except that no permits are required for rock art tracings. I will return to this point later. Can you imagine how much pressure we are placing on the precious art by encouraging, and showing every Dick, Tom and Mary to spend their Sunday afternoons tracing rock art? Who will take the responsibility when art is damaged by the public who were introduced to tracing by us?

Any professional archaeologist involved in the study and tracing of rock art, will tell you how complicated it is. Special skills are required to 'read' the context of paintings and make accurate tracings. These skills are not acquired on a lazy Sunday afternoon scribbling on plastic with a koki, they come with years of studying paintings and reading the literature to understand what the art means.

The immediate response to the above may be two fold.

Firstly, that the public has the **right** to rock art. BUT, do they have **rights** or only **privileges** to heritage resources? AND if these **rights** and/or **privileges** are violated, should they be taken away? Archaeologists do not have a **right** to excavate sites, it is a **privilege**. Secondly, the concept/idea of **sacrificing** sites - is this an ethically acceptable or a vulgar concept? As so called watch dogs and managers of heritage resources are we allowed even to consider **sacrificing** precious cultural material to be destroyed? By **sacrificing** are we not accomplices to a crime. If we as archaeologists or archaeological resource managers, are incapable of protecting sites and preserving the cultural heritage should we practice public archaeology?

Van Gogh produced several paintings (some 14) of his famous sunflowers in a vase to decorated his living area. Does this mean that we must **sacrifice** one of these precious paintings because there are a couple of these, by placing it in Times Square or Travalgar Square for the public to copy just because they have a **right** to Van Gogh? Each of these paintings are unique, so are each and every individual painting and each rock art panel.

Why do the public need to, or want to trace rock art, for what purpose? What will they do with their precious scribbling on plastic of a 'funny human with an animal body feeding grass to an eland'?

An aspect which needs urgent attention is a management policy regarding the proper protection of rock art - especially now that the public is taught how to trace the art. With the ever increasing pressure on rock art we must consider the following:

1. Tracing of rock art should require a permit, similar to that for excavating. This is the only way NMC and the local Data Recording Centres (DRC's) will be able to monitor the traffic on paintings and the possible damage. In this regard the KwaZulu-Natal Heritage Act of 1997 set an excellent example (see Act No, 10 of 1997).

2. All landowners who exploit rock art for financial benefits (usually to advertise bed and breakfast and/or hiking trails) should register either with NMC or the local DRC or both and comply to certain management policies.

Virtually without exception these entrepreneurs are not even aware that the art is protected by law. These people should be responsible for the protection and management of the art - landowners are only the custodians of the art, which belongs to the nation (see also Ouzman 1996).

Furthermore, these people should contribute a percentage of the financial benefits which they gain from the art to a central fund for the study of rock art.

Remember the TV advert, South Africa is the land of sunshine, rugby, braaivleis and chevrolet, will rock art recording become the fifth most popular weekend past time? Only time will tell.

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LETTERS AND COMMENTS

NDONDONDWANE AND THE ARCHAEOLOGY OF DEEPLY BURIED DEPOSITS CONDUCTED UNDER UNUSUAL CONDITIONS:

A RESPONSE TO VAN SCHALKWYK *ET AL.*

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After reading the most recent report on the 1995 excavations at Ndongondwane by Van Schalkwyk, Greenfield and Jongsma in *Southern African Field Archaeology* (Vol. 6, No. 2, 1997) and in Nyame Akuma (No. 47, 1997), I felt a response was necessary to clarify some ostensible inconsistencies with previously published excavations at the same site. I also believe certain misunderstandings and omissions in their paper cast doubt on my competency as an archaeologist. Some of the responsibility for this situation is clearly mine, as I should have been more explicit in my description of field methodology and personnel, for example. But up until now, one aspect of this project was not widely known; the fact that my excavations occurred under the auspices of the South African Defense Force between 1982 and 1983, in less than ideal research conditions. This letter addresses two main issues; appropriate methodologies to deal with buried archaeological deposit and the role of different research contexts.

Ndongondwane is an incredibly rich site on the northern side of the Thukela River threatened by proposed dam building activities. For this reason it was excavated first by Maggs in 1978 (Maggs 1984) and later by myself in 1982 and 1983 (Loubser 1993). With the benefit of hindsight, funding and a clearly defined research goal, a joint team consisting of Van Schalkwyk, Greenfield and Jongsma excavated substantial areas at Ndongondwane in 1995. With three separate excavations done by different teams under different conditions, it can perhaps be expected that not all results will appear compatible. Add to this mixture the divergent research goals, interests, skills, and methodological approaches of various researchers, it is only to be expected that results would appear incongruent. But over and above the different excavation contexts, each new excavator at a site has more information at his/her disposal as well as the benefit of hindsight. In this letter I propose that it is the responsibility of the most recent excavator to properly

synthesize the results of all previous excavations at that site, and attempt to do this in an even-handed fashion, focusing not only on perceived errors but also use the results of previous work in a positive and productive way.

Cultural Resource Management (CRM) archaeology in the Southeastern United States of America has been designed to build on the results of previous research. It has also been designed to glean the maximum amount of information from buried archaeological deposit, reminiscent of the alluvial/colluvial setting of Ndongondwane. Unlike highly visible Late Iron Age stone-walled settlements, for instance, many archaeological sites in the American Southeast are buried under plough zone deposit and therefore not always visible on the surface. One way to address this situation is a step-like procedure divided into three phases, each consecutive phase involving an additional level of effort. Phase I survey involves site identification and delineation of site boundaries by means of regularly spaced shovel test pits. Based on the results of a Phase I survey, the more promising sites are then subjected to Phase II test excavations. Typically, testing involves a grid of shovel test pits across the entire site to determine artefact concentrations, or hot spots. Test units (ranging from 2 x 2 metre to 5 x 5 metre blocks) are then placed in selected hot spots. If the site shows further promise at this stage of investigation, then entire areas are stripped with heavy machinery during Phase III data recovery excavations.

Due to the competitive bidding process prior to the commencement of a new phase, often each phase is done by a different archaeological company. In this environment, archaeologists constantly confront the problem on how to incorporate the results of earlier work, very often done by different people. This problem is compounded by the tendency that with each new phase, different information is retrieved. For example, Phase II test unit excavations almost invariably capture details missed

during initial Phase I shovel test pit surveys. Horizontal machine stripping of substantial plough zone deposit during final Phase III data recovery excavations in turn expose features and finds not anticipated during Phase II testing. But in spite of the increased data recovered by removal of additional deposit, it is also true that certain information is recovered at the initial survey level which is not found again, even during massive data recovery excavations. The *onus* is therefore on the final Phase III archaeologist(s) to account for all the information recovered at the site, including those recovered during the preceding and less extensive Phase I and II excavations. Partly due to limited time and money, other researchers often consult the Phase III report as the authoritative version, assuming that it accurately accounts for everything which has been done before. In practice, however, a Phase III report can give a misleading picture of previous work, and it often helps researchers to consult the Phase I and Phase II reports for alternative perceptions and presentations.

Similar to many buried sites in the American Southeast, the alluvial/colluvial settings at riverine sites such as Ndongondwane, make it especially difficult to detect what is underneath without extensive excavations. Non-intrusive techniques to detect buried features and artefact concentrations, such as Ground Penetrating Radar, have proved to be a very effective exploratory tools, but often with misleading results, such as falsely representing buried bioturbation as cultural features. Even intrusive techniques, such as shovel testing and test unit excavation, can also be misleading, since artefact voids have been shown to sometimes correspond with buried habitation surfaces which have been kept clean by the prehistoric occupants. In the final analysis, only the stripping of big areas can ground truth for the actual existence of features and artefact concentrations at deeply buried sites, such as Ndongondwane.

Unlike the standardized step-like excavation procedure current in Southeastern America, the Ndongondwane excavations followed a somewhat different trajectory. The level of effort of Maggs's preliminary excavation was like a Phase II test excavation in the United States. Maggs's team excavated the most dense artefact concentration visible on the surface of the ploughed field. No Phase I survey was conducted prior to or during Maggs's excavations, so artefact concentrations other than the one he worked on were not investigated at the time. When I was sent into the field by the South African Defense Force to continue Maggs's excavations, my primary mission was to completely excavate the rich subsurface mound partially excavated by Maggs. This proved to be very fruitful exercise, since in addition to the ceramic mask fragments and baked clay figurines found by Maggs, we found various remains of an iron furnace to the west of Maggs's excavations. Although Maggs found a few fragments of iron ore and calcite, his excavations were east of a slag-lined bowl and the baked clay walls and clay tuyeres of a smelting furnace. Thus, contrary to the claim made in the latest Ndongondwane paper (Van Schalkwyk *et al.* 1997:64), information on the furnace is

not to be found in the Maggs paper but in the one written by Loubser.

It is important that researchers interested in Ndongondwane distinguish the properly excavated and analyzed furnace remains on the northern side of the mound area from the baked clay remains found in the tall grass on the southern side of the ploughed field. As my task was to concentrate on the mound area, I never properly examined this pile of baked clay on the site periphery, apart from mapping it during a theodolite survey on a hot summer afternoon. Since I had reservations about this pile and a similar pile in a neighbouring ploughed field farther to the south, I should have placed a question mark on the map behind at least the one which was shown by Van Schalkwyk's Phase III data recovery excavations to be the remains of a historic pump house. Fortunately, apart from appearing on my map, this erroneously identified "furnace" did not feature in my preliminary interpretation of the site layout. Contrary to a statement made by Van Schalkwyk and his co-authors, I never explicitly interpreted this as a smelting area associated with the main site.

Like Maggs's initial excavation, the level of effort of my excavations resembled Phase II test excavations in America, instead of the full-scale Phase III data recovery excavations and stripping executed by Van Schalkwyk and his Canadian colleagues. Moreover, my excavations were not preceded by a systematic Phase I level survey of subsurface deposit and artefact distribution. However, since I did my stint of National Service in the South African Army at the time and lived next-to the site, I had the opportunity to conduct walk-over surveys of the area during week-ends and noted every artefact exposed on the surface. Each surface artefact occurrence was plotted during the theodolite survey. Of course, additional artefacts appeared after heavy downpours or ploughing, but this did not alter the identification of the main concentrations.

It was actually soon after ploughing of the northern end of the field that we found ceramics on the surface on top of what became known as the dung area. Since I had to find a separate area for a field assistant to excavate, the dung area excavations were initiated. It is important to note that these excavations were conducted over and beyond the initial aims of the investigation. Contrary to claims that Loubser test excavated the dung area (Van Schalkwyk *et al.* 1997:67), I never excavated in this area due to the fact that the field assistant claimed it as his domain. Since his claim had the support of superior officers in the military, it had to be closely followed. The only time I actually worked in the dung area was immediately prior to my removal and confinement in Eshowe by the South African Defense Force. This was late one afternoon when the assistant was away. During this brief window of opportunity I did the plan and profile maps as presented in the 1993 report. Under such bizarre conditions, it was often difficult to conduct proper fieldwork on the site, even though I was the lieutenant supposed to be in charge of excavations.

While confined to the military base in Eshowe - for

reasons I still do not fully comprehend - the South African Defense Force afforded me the opportunity to analyze all the artefacts excavated up until my departure from Ndongondwane. Repeated requests to revisit the site in order to double-check certain field observations were denied by my superior officers. Subsequent to my removal from the field, the field assistant opened additional units within the dung area without keeping proper records. To make a long story short, my descriptions of the dung area were incomplete due to incomplete excavations and record keeping beyond my control. Thanks to subsequent systematic excavations by Van Schalkwyk and his Canadian colleagues, the dung area is now placed within a proper context. It is untrue, however, when Van Schalkwyk and his colleagues refer to any of the actual dung excavations as the work of Loubser. While Van Schalkwyk and his colleagues rightly allude to the unfavourable research context of my Ndongondwane excavations in Southern African Field Archaeology (Van Schalkwyk *et al.* 1997:64), this is omitted from their Nyame Akuma article. I feel this omission casts doubt on my competency as an archaeologist since it creates the misleading impression that it was me who incompletely excavated and described the dung area.

A more systematic survey of the site was not part of my initial goal and any information in terms of artefact concentrations outside the ploughed field and possible site layout was considered an added bonus. It is nevertheless important to note that I never stated "... that the site was enclosed within the modern ploughed field" (Van Schalkwyk *et al.* 1997:64). In fact, both on my map (Loubser 1993:111) and in my site description (Loubser 1993:112), I refer to a surface occurrence of daga and pottery in an area some 40 metres upslope and east of the ploughed field's edge. When my map is compared to that of Van Schalkwyk's team, this occurrence corresponds with their Scrape 2. Moreover, I also show daga occurring in the area north of the dung area, corresponding to their Scrape 4. Where I did make an interpretive error, based on surface artefact scatters, was suggesting that huts may form a line upslope from the dung area. This error was partly due to my failure to detect the deeply buried burnt hut-floor subsequently exposed by Van Schalkwyk's team in the area slightly east and downhill from the dung area and west of the mound area.

The determination and interpretation of the exact settlement layout was not the main concern of my paper in any case, but an interesting aside which obviously needed further investigation. Limited sampling with a small auger during some spare time immediately prior to my removal was exploratory and far from conclusive, as clearly demonstrated by additional excavations at Ndongondwane by Van Schalkwyk's team. Being aware of this limitation, I stated that "... excavation of the daga and pottery line north of the dung area is necessary to verify this reconstruction. The need for further test trenches and the paucity of comparative information from contemporary sites prohibit any definitive statement about

the settlement pattern at Ndongondwane." (Loubser 1993:141). Subsequent publication of work done at other important Natal Early Iron Age sites by Whitelaw in 1994 (Whitelaw 1994) clearly shows circular arrangements of huts around central dung areas. Exactly how Ndongondwane compares to this pattern has not yet been spelt out by the Van Schalkwyk team, but it does not appear to differ significantly.

Ironically, my tentative interpretation that the settlement layout at Ndongondwane is centered on a cattle byre does not appear to differ fundamentally from that of Van Schalkwyk and his colleagues. But apart from this issue, the main thrust and best demonstrated part of my paper - the significance of the mound as an initiation locality and subsequent smelting area - is only indirectly referred to by Van Schalkwyk and his colleagues in their preliminary data recovery report. I trust that in their final synthesis of all the work done at Ndongondwane, Van Schalkwyk, Greenfield, and Jongsma will meaningfully incorporate and do justice to previous work and perspectives at the site, as well as compare their results with other sites in the region, such as those excavated by Whitelaw.

The question if Ndongondwane would have been better understood if it was approached in a phase-like fashion is probably not relevant at this stage of investigation, since the most recent excavators did conduct an overall surface collection of plough zone artefacts, similar to a Phase I survey, as well as machine stripping, reminiscent of Phase III work. In spite of methodological incompatibilities, I personally believe the site has been better served with different archaeologists working on it. Coming from different backgrounds and with divergent research interests, each archaeologist viewed the data from a slightly different angle and so had something unique to contribute. Generally speaking, Maggs had an ecological perspective, while I adopted a more ethnographic stance. At this juncture it may be too early to judge their theoretical position, but Van Schalkwyk, Greenfield, and Jongsma at least seem to depend heavily on systematic research procedures. As a general guide for those researchers who have to reconcile three reports of one site I recommend the following; read Maggs for information on subsistence, read Loubser for information on the mound area, and read Van Schalkwyk for information on the rest, including settlement layout.

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ASPECTS OF RECENT RESEARCH AT NDONDONDWANE: A RESPONSE TO LOUBSER

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This commentary is in response to the preceding comments by Loubser in this issue of *Southern African Field Archaeology*. In his comments on the preliminary reports of our 1995 field season at Ndongondwane (Greenfield *et al.* 1997; van Schalkwyk *et al.* 1997), Loubser makes a valuable contribution to the published knowledge about the site by reviewing some of the more poorly known aspects of history of research at Ndongondwane. This information is a welcome addition to what we have been able to glean out of the poorly archived data base from the previous excavations. It is important to note that Loubser's comment contains very few criticisms of our research. Instead, its focus is to try to put the research at Ndongondwane in a historical perspective. As a result, this commentary is an attempt to respond to only his criticisms (overt and implied) of our research. Other issues will be dealt with in future reports on the site.

First, Loubser criticizes us for not integrating the results of the previous researchers at Ndongondwane with our own studies. In general, this is a specious criticism since the primary intent of the two published reports was to present the preliminary results of only our first season (1995) of field work. Two more subsequent field seasons have taken place and our knowledge of the site has been greatly enhanced (Greenfield & van Schalkwyk *n.d.*; Greenfield 1997, 1998), in addition to a full lab season. During our research at the site, we attempted to collect as much information on previous research and on the background to that research as possible. For purposes of brevity, much of that information was only briefly alluded to in our two preliminary reports on the site (van Schalkwyk *et al.* 1997; Greenfield *et al.* 1997). Nevertheless, a substantial part of these preliminary reports reviewed the history of research at the site. Space considerations imposed by the publishers limited discussion of the implications. It has always been planned

that there will be a fuller consideration of the historical context of the research at Ndongondwane in the final report.

The task of integrating the results of the previous researchers at Ndongondwane with that of our studies has been both rewarding and frustrating experience. The experience has been both rewarding when material was located, and frustrating when it remained beyond our reach. We have tried to locate original field notes and summaries of all previous research at the site, while at the same time tapping into the historical memory of participants. While Maggs' field notes from the original excavations (Maggs 1984a) are archived in the Natal Museum, Loubser's original field notes disappeared from the National Museum in Bloemfontein sometime after his departure to the United States. The field notes have not been located despite the attempts by Loubser, myself and the National Museum staff to relocate them. Copies of some of Loubser's field maps were located in both the Natal Museum and the McGregor Museum, but these are merely graphic summaries of a more detailed data base, most of which were already published by Loubser (1993). After the conclusion of the final field season (1997), we spent three months in 1998-99 tracking down and re-analysing the artifactual remains from the earlier excavations (1976 & 1982-3). All of the well-preserved remains have been re-analyzed using our coding systems and the data will be incorporated into the final reports. Later reports on the site will begin integrating the results gleaned from the reanalysis of the earlier data.

Second, Loubser implicitly criticizes our reports for not developing the historical context of our research with southern African Early Iron Age Archaeology. Research at Ndongondwane has paralleled the development of Early Iron Age research in South Africa. Maggs' (1984a) initial research at the site (conducted in 1976) was clearly

part of his effort to construct a culture historical framework for the region. This was typical of the research effort across much southern Africa during the 1970's. By the early 1980's, archaeologists were beginning to shift focus to a greater understanding of the social and economic organisation of EIA societies (e.g. Hall 1987; Maggs 1984b, 1984c). Loubser's (1993) research clearly falls within this genre. During the 1990's, a number of archaeologists have begun looking at the spatial dynamics of EIA communities (e.g. Huffman 1993; Whitelaw 1993, 1994; van Schalkwyk 1994a, b). Our phase of research at Ndongondwane was to test conflicting models of spatial model of Early Iron Age intra-settlement spatial organisation proposed by Huffman (1993), Loubser (1993), and Maggs (1984b, 1984c). As Stephen Jay Gould has commented in his numerous treatises on the history of evolution, it is important to evaluate the contributions of researchers within the historical context in which the research is conducted. For example, it would be unfair to criticize Maggs' (1984a) original work at the site in the same light as Loubser's or our research. Since Maggs began his research, there has been a pronounced shift away from culture history towards a greater understanding of the spatial dynamics of intra-settlement community organisation. We conducted our research with the benefit of the contributions of earlier scholars and in the context of changing goals for archaeological research.

Third, Loubser uses the step-wise structure of Cultural Resource Management (CRM) in the United States of America to criticize the history of research at Ndongondwane. The history of research at the site cannot be pigeon-holed into a such a structure. To imply that it should have followed the structure of CRM work is to ignore the historical development of archaeology in this region. The history of research at Ndongondwane, in fact, occurred in a step-wise fashion. Maggs' (1984a) research identified the site as an important location for increasing our understanding of the EIA of the region and helped plug holes in the local culture historical sequence. Loubser's research was organized to fill in obvious gaps in Maggs' results and to answer a different set of questions (function of the central midden known as the Mound Area). Our research built upon both Maggs and Loubser's research by shifting the focus away from the central midden to the entire site. As a result, it unfair of Loubser to criticize the history of research at the site as not conforming to the research structure of CRM in the USA. It evolved in a very different culture and historical context, and each step was designed to fill in the gaps in knowledge that were identified by earlier researchers.

Fourth, Loubser criticizes a few specific details of our reports. One of these represents what is clearly recognizable as sloppy bibliographical research on our part (specifically, mis-attributing the excavation of smelting furnaces and associated debris to Maggs' and not Loubser's excavation - this was clearly our error). A second criticism of our report revolves around the question of the historic pump house *cum* furnace at the south end of the site. Our work at Ndongondwane was

designed to try to answer many of the questions raised by Loubser or raised indirectly in his report. One of the major issues unresolved by Loubser's research was the dimensions of the site. Since our research was designed to test conflicting spatial models of Early Iron Age intra-settlement spatial organisation (e.g. Huffman 1993; Loubser 1993; Maggs 1984b, 1984c), it was necessary to determine the dimensions of the site and each activity area. The nature of Loubser's published summary of his survey data (Loubser 1993) was too ambiguous to be able to establish boundaries, a necessary prelude to the investigation of intra-settlement spatial organisation. Therefore, one of our first tasks was to examine the quantitative distribution of remains across the surface of the site. A combination of systematic surface collection, auguring, test excavations, and conductivity survey have been employed to do so. The difference in approaches has been that we have had the luxury of planning a quantitative assessment of the surface material, something that was not part of the original research. The site seems about the same size as Loubser thought, but it is smaller to the south and longer to the north. At the very edge of the ploughed field (south end of the site - Loubser 1993, fig. 1), Loubser identified the presence of a concentration of furnace rubble and pottery. Even though he did not explicitly say that this was a smelting area, the clearly labelled description of the material from his map implied that this was an area of smelting activity. A description of this area was absent from his text. This is a good example of how misleading summary data can be for subsequent researchers. As a potentially significant activity area that might have significance for testing the various spatial models of EIA settlement organisation, we were obligated to investigate this area. It caused us to waste valuable and limited time and resources in order to investigate this area. Each labelled area on a map should be fully described during publication and not left to subsequent scholars to sort out. A similar problem exists with the data from the Dung Area. Loubser's publication implied that he was responsible for the excavation. Therefore, we attributed the results to him. It is both interesting and valuable to learn that he was not responsible for the excavation and was simply trying to report data that would otherwise be lost. We applaud his efforts for presenting these data, but warn other researchers against falling into a similar predicament - attempting to present incomplete data without giving enough background information.

An important part of the history of research at Ndongondwane has been the excavation of the Dung Area. A previous unskilled excavator of limited analytical skills functioned independently of the field director (Jannie Loubser) due to the nature of military control over the excavations. This is not the fault of the Field Director since the excavations were conducted under the auspices of the South Africa Defense Force. As we will demonstrate in our final report (Greenfield & Van Schalkwyk, in prep.), the 1982-3 data from the Dung Area were inadequately excavated, poorly provenanced, and for the most part not curated. Ceramic samples from

the area were curated in paper bags that lacked provenance information, other than that they came from the Dung Area. These have been for the most part discarded. The bones, collected for the most part by Liz Voigt, are better provenanced. These are curated for now in the McGregor Museum. However, there is no linkage between the excavation levels from this area and the natural stratigraphy. As a result, these data were not re-analyzed for the final analysis. We have spent three seasons trying to sort out the stratigraphy and spatial patterning of activity areas in the Dung Area. The data from Loubser's report was important because it summarized his understanding of the area. However, as we have since discovered, it did not conform to the reality of the stratigraphy (which proved to be complex) nor the material remains. Rather than going into this issue in great detail here, we prefer to present our results of the Dung Area excavations separately in our next preliminary report. Nonetheless, we applaud Loubser's attempt to present these data.

In conclusion, it might appear that we were guilty of ignoring or misrepresenting the research of an earlier generation at Ndongondwane. The reality was quite different. Not only were the published and unpublished sources of information consulted, but also one of the previous excavators (Maggs) visited the site during our period of field work. In fact, over 20 South African plus other foreign professionals visited the site during our tenure there. Each was consulted as to their knowledge of the history and significance of the research. Loubser, unfortunately, never had the opportunity to visit the site during our excavations and survey because he had already left the country, and it was only recently that we were able to establish direct contact with him. Since then, we have had a fruitful correspondence which has substantially benefitted our analysis of the material from the site. As with his commentary, we look forward to his continued input in the future.

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