

IDEOLOGY AND HUNTER/HERDER ARCHAEOLOGY IN THE SOUTH WESTERN CAPE*

ROYDEN YATES & ANDREW B. SMITH

Department of Archaeology,
University of Cape Town,
Rondebosch, 7700

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ABSTRACT

The concept of cultural identity has been manipulated to such a degree in twentieth century South Africa that any discussion of the topic relating to the past is easily tainted. Here, in the light of a robust critique of our work, we re-examine some of the evidence for archaeologically visible distinctions between hunter-gatherers and herders in the south-western Cape and examine the critique itself. Among other things, we argue that the contribution of the site of Oudepost to this debate is more ambiguous than the excavator believes.

INTRODUCTION

Schrire's (1992a) critique of Smith *et al.* (1991) is a response to alternative interpretations that we offered of her work at the site of Oudepost as well as an earlier, general model of hunter-gatherer:herder interaction in the south-western Cape (Schrire 1980). Schrire (1980) argued, along with Elphick (1985), that the socio-economic distinctions drawn between indigenous inhabitants by Dutch settlers, and perpetuated in archaeological thinking, were somewhat polarised reifications of a highly fluid cycle of wealth and impoverishment. In terms of this model, those individuals termed Khoikhoen were little different from those termed Soaqua other than their having, at the moment they were seen, quantities of livestock. In the face of misfortune - drought, disease or theft - depriving them of their preferred possessions, the Khoikhoen reverted to a baseline hunting and gathering economy, suffering as a consequence a loss of social status. Many of those referred to as Soaqua could have been individuals or groups in such a predicament, albeit temporarily. In terms of this model the ascription of sites to hunters¹ or to herders, which implied to distinct economic and cultural modes, was unwarranted (Schrire 1980, 1992a).

In contrast we have argued (Smith *et al.* 1991), as has Parkington (1977, 1984), that many of those termed Soaqua were culturally hunter-gatherers and were seen as such by aboriginal herders and subsequently the colonists. Irrespective of precise etymology, the label Soaqua implied stockless people, this connotation

merging pejoratively with that of thieves. Recognizing that herding societies first appeared at the Cape between 1900 and 1600 years ago, Schrire is unwilling to accept that archaeologically distinguishable cultural and economic entities persisted up to the colonial period, this being the thrust of our evidence (Smith *et al.* 1991) and the target of her critique. Schrire holds these views despite the probability that, when first encountered by European travellers, at least some hunters appear to have spoken a language different from that of the herders and, furthermore, were frequently referred to in relation to occupancy of mountainous areas (see Parkington 1984:160 for discussion).

Schrire marshalled evidence in support of her argument from her work at Oudepost, an early Dutch colony frontier redoubt. Indigenous items were recovered from among artefacts of European origin and these former were argued (Schrire & Deacon 1989) to be Khoikhoen, because it was with these people alone that the Dutch documented economic relations. The stone age items recovered from Oudepost are apparently indistinguishable from those generally found on Late Holocene sites from the Cape. It thus follows that arguments for the separation of hunters from herders on the basis of archaeological material is implausible and, furthermore, that the very distinction itself may be spurious. We recognise the importance of this claim and acknowledge the opportunity for addressing the issue potentially provided by the site of Oudepost.

Schrire (1992a) identified three issues in our paper in which we fell short. In the sections below we respond to these purported short comings. We dispute that the evidence marshalled by Schrire, either in her work at Oudepost or her critique, is unambiguous and submit that alternative interpretations remain viable.

1. We follow Elphick and refer to hunters as a shorthand for hunter-gatherers and herders for people herding livestock who also hunted and gathered.

HUNTER AND HERDER MATERIAL CULTURE

We have argued (Smith *et al.* 1991) that hunters and herders do have different, though variable archaeological signatures which can be characterized on the basis of stone tools, ostrich egg-shell beads, pottery frequencies and the overall abundance of domestic stock. To this end we presented the results of a number of excavations in sites from mountains and coastal areas in the south-western Cape. We argued that hunter sites were generally characterised by a high percentage of formal tools, a reasonably frequent use of silcrete raw material, for the most part smallish ostrich eggshell beads, a relatively infrequent use of pottery, and small numbers of sheep, if any. Herder sites generally displayed the inverse of these trends.

The first of Schrire's criticisms which we deal with is that the sites do not fall neatly into one group or the other and that the postulated signatures of hunters and herders are not invariable. Furthermore, she is concerned that we do not know whether the characteristics we isolate in different sites convey cultural identity or matters relating to chronological change or site use. In this respect she challenges us to specify how the archaeological signature of herders out hunting would differ from that of hunters.

Schrire (1992a:63 & table 1) accepted the low incidence of formal tools and silcrete raw material on herder sites, although she fails to see the incongruity of her including the site Drie Susters, with its high silcrete percentages and relatively infrequent ceramics, with sites we regard as herder. This clearly contributed to some of the overlap she claimed as evident (*ibid.*). The problem with Drie Susters, we admit, may be partially due to ambiguity in our presentation and the fact that no formal tools were recovered from the small Drie Susters sample (Smith *et al.* 1991:88); but we think it is also due to her unwillingness to accept the density of ceramics as a distinctive marker.

In criticizing our use of ceramic densities - specifically, implying that the high ceramic densities from the sites of Driebos and Voëlvlei (Table 1) were contradictory to our argument - Schrire presumably had not read Sadr & Smith (1991), published simultaneously with Smith *et al.* (1991). In the former a comparison was offered of the differences in the density of pottery on various sites, including some from interior rock shelter deposits (Sadr & Smith 1991: fig. 7). It was pointed out that on coastal sites the predominant depositional matrix is comprised of shellfish remains, whilst that of interior sites is finer grained sands etc. The deposition of shell results in very high rates of accumulation than is the case of sands, thus diluting the quantity of pottery and other artefacts found per cubic metre of excavated deposit. The high ceramic densities from both Voëlvlei and Driebos reflect the compressed nature of sediments in inland sites relative to those near the coast, and are thus not directly comparable with similar calculations from coastal sites (*ibid.*:113). The sites of Voëlvlei and Driebos are thus not *a priori* contradictory of our argument; we will show that

they are, in fact, entirely consistent.

It is instructive to seek a method of presenting ceramic abundances which avoids the inherent limitation of density values outlined above. To this end we employ an index of ceramic frequency appropriate to the kinds of observations routinely available in the published literature. The index is simply the total number of sherds divided by the total number of pieces of flaked stone. Whilst not ideal - we consider that the weight of each may be more appropriate but such published observations are scarce - the results in Table 1 (which is structured on Schrire (1992a: table 1) clearly show that both Driebos and Voëlvlei have incidences of ceramics relative to flaked stone lower than those of Kasteelberg, and, for that matter, Oudepost, but which are more similar to the coastal sites of Witklip, Vlaeberg Areas 1-3 and Drie Susters reported by Smith *et al.* (1991). At De Hangen, another interior site, the incidence of ceramics is also low (Parkington & Poggenpoel 1971; Sadr & Smith 1991) and formal tools are abundant and of a type similar to those at Voëlvlei (Table 1). The low incidence of silcrete at De Hangen is not surprising given its location in the northern Cedarberg which is distant from known silcrete sources on the coastal plains. What is evident from Table 1 is that, whilst variable, the composition of sites labelled hunter is more distinct from that of those termed herder than Schrire may allow.

This brings us to ostrich eggshell (OES) beads and the usefulness of these artefacts as cultural markers. Schrire has problems with our interpretation of ostrich eggshell bead sizes as distinctive markers. In particular she points to the fact that very large beads are present at Voëlvlei, a site which we think was occupied by hunters on the basis of the silcrete dominated formal stone tool assemblage, ceramic densities and little live-stock. Voëlvlei and De Hangen, similar in many respects, do differ in terms of the sizes of ostrich eggshell beads. But is this a damning argument against our interpretations? We viewed the large beads from Voëlvlei as representing a one-way transfer across a permeable economic and cultural "boundary" from herders to hunters. The big beads from Voëlvlei (which, relative to other sites, are extremely large with a mean of 8.0 ± 1.4 mm, $n=84$) were surprising since, until then, we had not seen such an overall large sized sample in association with a stone artefact assemblage with many scrapers and adzes, relatively few potsherds and an essentially hunted fauna. The beads from De Hangen, for instance, have a mean size of 5.7 mm (± 1.4 , $n=267$).

Beads of the sizes present at Voëlvlei are clearly an innovation of the last two thousand years (Yates in prep.) whereas the stone artefact types common at this site, specifically the adzes and scrapers, originated earlier, both locally and elsewhere in the Cape (Deacon 1976, Schweitzer & Wilson 1982, Deacon 1984, Nackerdien 1989, Manhire 1993). Whilst Voëlvlei has big beads in common with the herder sites of Kasteelberg, the latter do not have formal tools in any appreciable numbers (Table 1). In contrast to Voëlvlei they do, however, have abundant to super-abundant remains of both sheep (Klein

Table 1. Percentages of formal tools, silcrete and ceramic densities and indices of frequency from various sites in the south-western Cape. Modified after (Schrire 1992a: table 1). Please note that the ceramic densities given for WK and VL relate to all the units reflected in the left hand column.

	FT %	Silc. %	n	Ceramics n/m ³	P.I.
Pre-pottery hunters					
WK 4	5.0	29.8	734	0	0
VV*	1.8	11.2	562	0	0
DB	8.5	14.5	59	0	0
KBC/PN	2.0	23.5	5702	0	0
Post-pottery hunters					
WK 1	5.8	28.9	570		0.02
WK 2	6.3	27.8	331	10.8	0.02
WK 3	5.3	24.1	1602		0.01
VL 1	4.9	13.6	41		
VL 2	8.1	61.5	37	15.5	0.2
VL 3	4.7	42.3	256		
VV	1.8	5.4	3777	254.4	0.08
DB	4.5	12.7	1165	354.0	0.2
DS	0	33.9	62	10.7	0.5
DH	3.9	4.6	4668	54	0.07
Post-pottery herders					
DSM	0.4	3.2	236	232.5	1.1
KTB	0	0.8	119	273.8	1.5
KBB	0.2	1.5	22773	735.8	6.1
KBA	0.5	23.1**	4106	225	1.6
Historic					
Oudepost	4.2	46.5	307	1.7	0.9

Key: WK = Witklip; VV = Voëlvelei; DB = Driebos; KBC/PN = Kasteelberg C and Paternoster; VL = Vlaeberg; DS = Drie Susters; DH = De Hangen; DSM = Drie Susters Main; KTB = Kreeftebaai; KBB and KBA = Kasteelberg B and A respectively. Column headings: FT = formal tools; Silc. = silcrete; P.I. = pottery index. Further details can be found in Smith *et al.* (1991).

* site sieved with a fine-mesh screen (1.5mm) which, relative to a 3mm mesh used on other sites, increases the recovery of mostly quartz chips, thus depressing the relative incidence of formal tools and silcrete.

** admixture of MSA tools, many of which are silcrete, from gravels below

& Cruz Uribe 1989) and ceramics (Sadr & Smith 1991). At the very least we are confident that, leaving aside for the moment the issue of activity differentiation, Voëlvelei cannot be regarded as unproblematically a herder site. We prefer, on grounds that the balance of other cultural traits at Voëlvelei fall towards what we regard as the hunting and gathering spectrum, that the site is plausibly seen as such.

To pursue the issue of beads further, we turn now to smaller beads and their variable presence in sites. Our analysis of beads comes from samples collected using a 3 mm sieve, this being the minimum size mesh most widely employed in South African stone age excavations. As has been shown by sieving studies very small beads less than 4.5 mm will variably pass through a 3 mm mesh sieve (Yates in prep.). We cross-checked the effects that this may have had on a number of sites by using a 1.5 mm mesh. At Witklip, a near coastal site we inferred as occupied by hunters (Smith *et al.* 1991), the small mesh beneath the standard 3 mm retained many small beads. We also sieved with a 1.5 mm mesh a representative sample of spoil-heap from the herder site of KBB, as well as a metre square excavation at KBA and all of the excavated deposit at an unreported site

KBE. As we expected, large beads were found in the excavations, but none of the Kasteelberg sites revealed evidence for the presence of large numbers of small beads.

The samples which we illustrated from Witklip and compared to Kasteelberg (Smith *et al.* 1991: fig. 6, Table 2) do not include beads from the 1.5 mm mesh. Had we been able to include them (i.e. if 1.5 mm sieved samples were generally available for comparison) the Witklip mean would decrease (from 4.7 ± 0.8 mm, $n=54$ to 4.2 ± 0.8 mm, $n=61$), thus increasing the distance between the Witklip and Kasteelberg samples and making the distinction between the two even greater.

Furthermore, the very small beads we recognize as common from most hunter deposits are insignificant in all presently known herder inventories. Large, herder-style beads are however, not just present at Voëlvelei but dominant. It thus appears that there was not an exchange of bead styles between the two economic groups; the transfer of bead "style" or beads themselves occurred in one direction, from herders to hunters. We surmise the negative connotations of small hunting style beads may well have rendered such items unattractive to herders.

We next consider whether differences in quantities of

formal tools and ceramics and the nature of faunal assemblages outlined in Table 1 are the result of herders out gathering or - as Schrire suggested - hunting rather than the activities of socially and economically relatively distinct populations. That herders hunted and gathered is beyond doubt. The historical sources make this much clear, as they do the fact that it was a one of a number of recourses in the event of stock loss (Elphick 1985). The implication for Schrire is that herders gathering or hunting employed the material culture found in sites such as Voëlvelei, Driebos, De Hangen, Witklip and others, this explaining among other things the low numbers of domestic animal remains recovered from such sites (Schrire 1980, 1992a; Schrire & Deacon 1989). This is an interesting idea; a number of points however, render this hypothesis less viable.

First, it should be noted that, although heavily dominated by domestic animals, the faunal remains from the Kasteelberg sites do number among them wild species as well (Klein & Cruz Uribe 1989). It is instructive that the shelly deposits from Kasteelberg B (some 1 300 years worth at the most) produced a wild ungulate density of 158 NISP/m³ (observations from own records and Klein & Cruz Uribe 1989), compared with a paltry c. 20 NISP/m³ from c. 4300 to 3500 year old shell middens in Eland's Bay Cave some 60 km to the north (Klein & Cruz Uribe 1987; own observations). The evidence from Kasteelberg thus reveals that wild game was not unavailable to pastoralists near the coast. Indeed, by comparison to pre-pastoral Eland's Bay it may have been locally quite abundant. There is nothing compelling in the Kasteelberg evidence to show that the occupants of the Kasteelberg area were required to remove to the mountains to satisfy a desire or a need to hunt; the evidence, in fact, indicates quite the opposite. The latter areas, anyhow, have a lower nutrient status and thus lower ungulate carrying capacity than the coastal plains (Cowling 1992; Smith 1984). In the light of this, why in the first instance herders, impoverished or otherwise, would penetrate the mountains to hunt is one question Schrire needs to answer.

Short of arguing for an entirely scavenging oriented procurement strategy for wild species, the occupants of Kasteelberg clearly did some hunting. We can surely assume that in the duration of occupation they also scraped some skins, be they of wild or domestic origin, as well as used and maintained wooden implements of some sort. If such activities did take place, the inhabitants of the sites do not appear to have made and/or used and discarded many formal stone artefacts in the process. Thus, from the Kasteelberg sites we have some indication of what herders were not prone to do whilst undertaking some hunting and other activities. We must therefore ask of Schrire's point of view just why herders produced and/or discarded quantities of formally retouched stone artefacts in one context (De Hangen etc.) but did not do so in others (Kasteelberg) when at least some activities were common to both. Furthermore, her functionalist explanation fails to account for the very little silcrete used in sites nearby the sources on the coastal plains and the more intensive use of the material

elsewhere.

Another dilemma is purely a matter of cultural practice. One may ask of Schrire's perspective just why herders visiting De Hangen and other sites besides Voëlvelei should adopt a different size range of ostrich eggshell beads from that widely worn by them at the coast?

All in all we do not think that our evidence is easily accommodated by a simple argument of activity variation within a single cultural system. Schrire's interpretation seems to implicitly agree that the low numbers of domestic animals in mountain contexts is not indicative of intensive herding. If the associated material culture cannot be comfortably accommodated within a model of functional variability as we argue above, it would seem that a cultural explanation is at least worthy of consideration. In this regard, is it really necessary that the cultural signatures of what we regard as hunter sites be shown to be invariable, or for that matter absolutely different from that of sites thought to represent herders? We suggest that to suppose it should, as Schrire implies, is predicated on quite unrealistic expectations of human behaviour. Variability should in fact not be surprising. Human behaviour is known to be fluid, interactive and creative as well as conservative in some of its elements. Hodder (1982), for one, has shown that material culture variably marks and crosscuts a variety of social boundaries in at least one ethnographic context without having to deny the validity of distinctions between social groupings. In that case study at the very least, the implication was that differences in material culture, which may well mark boundaries, are difficult to predict *a priori*. In the south-western Cape we do not have sufficiently detailed ethnographic information to guide us much in this respect. The interpretive challenge, we believe, is to mediate between difference and similarity in the archaeological record and not to regard them *a priori* as epiphenomena of an underlying unitary behavioural system as Schrire seems to do.

We acknowledge certain elements common to both herder and hunter sites - to the presence of sheep and pottery and stone tools we add that of ochre, ostrich eggshell water containers, bone points and tortoise carapace bowls (Schrire & Deacon 1989). The women and men responsible for the creation of the sites on which such items are found were clearly at some level participants in the same historical developments. At deeper levels we think their worlds followed different but interrelated orbits. Evidence presented above demonstrates two points: that some substantial though variable differences in the quantities of certain items exist between a number of sites and that such sites broadly fall into two groups. The extent of conformity of these patterns is better than Schrire believes but clearly awaits further work; presently, the only apparently dissonant instance is the beads from Voëlvelei. However, attributing the presence of large beads as resulting from exchange or stylistic borrowing, as we have done, is not inconsistent with the circumstances of ethnohistorically documented clientship (Elphick 1985). It may well not be a coincidence that, situated at the interface of the

mountains and coastal plains, the beads from Voëlvei reflect a herder type pattern of production. The uppermost levels of the site of Witklip, situated some kilometres distant from Kasteelberg, also contain formal tools, have little pottery and sheep but have beads bigger (mean of $6,3 \pm 2,0$ mm, $n=18$) than those from sites such as De Hangen set back in the mountains.

OUDEPOST AND THE DEBATE

The second of Schrire's concerns was our critique of the Oudepost material and reinterpretation of it. Schrire rejects our suggestion that the indigenous artefacts from Oudepost (Schrire & Deacon 1989) may have entered the site during the 13/14 year hiatus in Dutch occupation which occurred after the massacre of 1673. Previously, she had argued against their being millennia old (Schrire *et al.* 1990). Both of these scenarios imply a lack of historical testimony as to who produced and used the materials, a situation which, if true, seriously undermines the case for unambiguously attested cultural affinities. The site's excavator perceptively recognized the unique possibilities for research provided by Oudepost; it is because of the apparent specificity of the documents as to who of the indigenous peoples were present at the redoubt that we need be sure the archaeological record necessarily reflects this and no other presence.

Schrire is convinced that the indigenous and colonial materials recovered come from the entire period of the redoubt's existence, dated by documentary sources to between AD 1669-1732. This conclusion is based on associational evidence and a sophisticated analysis of the diameter of clay pipe bores. This approach demonstrated evidence for a sequence in a deposit extensively disturbed by dune mole activity (Schrire *et al.* 1990). Schrire has previously stated that there is no documentary or archaeological evidence for indigenous people, be they Khoikhoen or Soaqua, living in the abandoned ruins (Schrire & Deacon 1989:111). As the Dutch were by definition largely absent, the first is perhaps not surprising. And in terms of the second, no depositional trace of the hiatus has yet been reported. We assume this would, at the very least, be necessary to yield the archaeological information needed to substantiate such a claim. Elsewhere, we have argued that the chronological interpretation of Oudepost is not unequivocal (Yates & Smith 1993).

Aside from the problem of the chronology of the archaeological remains at Oudepost, we can take issue with statements that the indigenous residues show a "distribution identical to that of colonial residues" (Schrire & Deacon 1989:111) and that they are integrally related. The evidence for this is neither presented in much detail nor is what is available unequivocal. It is worth noting that the two tables proffered to test "(t)he direct association of indigenous and colonial residues" (*ibid*) nowhere include colonial residues other than the architectural context. Clearly, because the artefacts came from excavations around the buildings they are "directly related to" these structures (*ibid*). Surely the comparison should have been between indigenous and smaller

colonial artefacts as well? The concentration of stone artefacts, pottery and ostrich eggshell beads around the lodge seems good circumstantial evidence of meaningful depositional association, but one can wonder (see Wilson *et al.* 1990), if the lodge excavation has only partly intercepted a wider scatter of stone tools, etc.

A number of pits were excavated to 'test' this proposition. Stone artefacts and the like, we are told, occur repeatedly in association with Dutch residues (Schrire & Deacon 1989:106). We presume this to mean that some test pits were dug where neither were found. This point, if even implied, is presently unclear. If such test pits were not dug, then it is possible that stone artefacts would have been found had the area outside of the distribution of Dutch remains been sampled. Equally important in these key areas would be the choice of volume for the test which would be sufficient to capture the materials in terms of the range of the densities revealed by the systematic excavations. Also necessary is the demonstration that the proportional fall-off in the densities of Dutch and indigenous materials is approximately the same as one moves away from the focus of occupation. If the peripheral tests contained indigenous items at densities which, relative to the average for the excavation, were higher than those of European items then the two would not be identically co-distributed in an exact sense.

For want of information we are not able to evaluate here these spatial propositions, but sufficient data are available for one to scrutinize the distribution of various classes of residues in time. Frequencies of tortoise and mammal bones (Cruz Uribe & Schrire 1991), indigenous items (Schrire & Deacon 1989) as well as pipe stems (Schrire *et al.* 1990) in each of the three major stratigraphic divisions are presented in Table 2. A Chi-square test was conducted of the frequencies in each category of finds through the sequence; each was significantly different from the others ($p < 0.05$, $df=2$). These results suggest a very variable set of distributions in time and hence, potentially interesting differences between temporal units that have yet to be explored. One thing is however, clear: tortoise and indigenous artefacts at their present sample sizes and groupings have in one respect something in common and different from pipes and mammal bones. Table 2 shows that the former two have absolutely highest density values in the older unit II, whereas the latter have absolutely highest values in the middle unit I. Statistically, the indigenous remains at Oudepost are not "identically distributed" with the European materials but have a slight tendency to be most common in the oldest units. The implications for the tortoise remains are not clear, but we wonder whether the fact that tortoise humeri are larger than those from nearby indigenous sites dating within the last millennium BP. (Cruz Uribe & Schrire 1991:101-102, fig. 9) is not in any way significant? One possibility considered by these authors and then dismissed, again on distributional grounds and for the reason that the Dutch clearly ate tortoises (Cruz-Urbe & Schrire 1991:101), is that the tortoises reflect natural die offs. Another view, not addressed but clearly quite out of the question for the

Table 2. Numbers, densities and percentages of various categories of finds from the three principle stratigraphic units from Oudepost.

	Oudepost Units		
	II	I	x
tortoise density	1.7	1.4	0.1
NISP	90	74	2
%	54.2	44.5	1.2
mammal density	52	61	11
NISP	2805	3290	442
%	42.9	50.3	6.8
indigenous artefact density	6.2	5.4	1.1
n	334	290	43
%	50.1	43.4	6.4
pipe density	45	70	15
n	243	3802	619
%	35.5	55.5	9.0

(Note that slight differences in density occur depending on which of the slightly differing volumes that can be derived from the various sources are used. Sources: Schrire *et al.* 1990, Schrire & Deacon 1989, Cruz-Urbe & Schrire 1991.)

site's excavator, is that the tortoises are indeed (considerably?) older than the eighteenth century AD; although not proven by the size data, the strong temporal trend of decreasing mean sizes documented by Klein & Cruz Uribe (1989) for the region as a whole makes it a possibility.

Our final point regarding uncertainty with the Oudepost materials concerns OES beads. If one accepts Kasteelberg (KBB) as an example of a herder site, which Schrire seems to do (Schrire & Deacon 1989:111), the frequency pattern of OES beads there is quite different from Oudepost. Schrire believes her large beads from Oudepost are consistent with those of herders. This is only partly true. The Kasteelberg samples from different levels each generally exhibit a normal distribution with modes between 6,0 and 8,0 mm. Oudepost tends to a bimodal distribution with modes around 5,5 mm and 9,5 mm respectively (Yates in prep.). What may be underrepresented in the present Oudepost sample are very small beads since, whilst mostly sieved with a 3 mm mesh, occasionally only a 6 mm screen was used (Schrire & Deacon 1989:110; note that the exact amounts sieved with the respective mesh sizes are not specified). Furthermore, one result of the wet sieving used at Oudepost was that "the spray sometimes forced tiny beads ... through the 3 mm screen" (Schrire 1990:271). The effects of this are easily demonstrated by comparing the mean sizes of beads from Oudepost 3 mm sieved samples ($5,5 \pm 1,7$ mm, $n=21$) against those from a variable mix of 3 and 6 mm screens from the site as a whole ($6,9 \pm 1,7$ mm, $n=170$). There is thus a probability that the unusual mix of large and relatively small sizes in the present sample of OES beads from Oudepost would before sieving have been even more emphasised. With small and big beads occurring in a mix not documented elsewhere in the south-western Cape the Oudepost assemblage could be a mixture (Wilson *et al.* 1990),

either of different cultural groups or different periods.

The above clearly does not offer a coherent choice among the possible alternative scenarios. That is not the point. The observations presented here lead us to suggest that the bulk of the colonial artefacts from the area excavated could well postdate the massacre. It is possible, therefore, that undocumented occupation of the abandoned fortifications could account for some of the indigenous materials. Equally, doubts that earlier residues have been incorporated are, in our opinion, not entirely assuaged. Both criticisms attack the assumption that Oudepost has fulfilled the undeniable potential that it offered: the apparent identity of those responsible for indigenous material items left on the site. A precise attribution demands, firstly, precision in chronological resolution and, secondly, an understanding of how the objects entered the site. The first appears to be lacking, through no fault of the excavator, in a heavily disturbed depositional environment. To the second we now turn our attention in as far as it concerns Schrire's statements about the political underpinnings of our criticisms and work as a whole.

ON VERWOERDIAN ARCHAEOLOGY AND THE DEBATE

Finally, Schrire accused us of racism, both in the models we use and in our research objectives. We feel a response in this instance is particularly called for. We contend that Schrire's discourse on the Verwoerdian underpinnings of our paper reflects more a polemical tendency to interpolate and dichotomize than it does our actual position.

Our view was that the circumstances prevailing at Oudepost, a military establishment, were not necessarily conducive to the kind of interaction implied by the indigenous items found there. This Schrire (1991:64)

counters by generalizing that "If tension always engendered avoidance, how might we rationalise the vast mulatto populations...that sprawl today across the erstwhile realms of the Dutch East India Company...?" We never said that tension always engendered avoidance; our statements concerned a particular circumstance at Oudepost. The site cannot be simply taken as Cape history writ small without denying that its particular historic moments were of any consequence. In short, we submit that there was more to events in the wider seventeenth and eighteenth century Cape than is represented by Oudepost alone. Thus, our views are right or wrong in terms of that site and period and not the Cape or the former Dutch empire as a whole. To imply, as we believe Schrire has, that the implications of the existence of what she terms "mulatto populations" were lost on us is, with deference to Whitelaw *et al.* (1992), the "cheap shot" of Schrire's response.

Our questioning of Schrire's views on the exact circumstances of social interaction which could have given rise to the residues at Oudepost was based on the fact that conflict was one of the documented interactions. The relationship between Dutch and Khoikhoen at Oudepost was by no means always an easy one, as witnessed by the massacre and indicated by a complaint laid against the post by one of the shepherds who claimed he had been beaten (Cape Archives, Precip and Translations of Letters Received LM 19: 2 April 1726). We would therefore reiterate our contention that after the massacre social relations between the Dutch and local people in the vicinity of Oudepost were strained; Schrire (1990:18) herself has characterised the period as "a guarded truce". Should one therefore easily accept Schrire's contention that the indigenous remains were deposited simultaneously by herders with those of the Dutch?

The records do indicate that local people were trading with and herding sheep for the soldiers at Oudepost. As far as has been published (Schrire 1990, 1991) they say nothing about co-occupation or cohabitation. Here the apparently identical distribution of both male and female indigenous items and the colonial residues is relevant. The documented associations presuppose shared domestic space. Schrire nowhere specifies the exact nature of these interactions beyond noting their intermittence and that they entailed the deposition of both male and female items (Schrire & Deacon 1989:111); nor is there discussion as to whether or not they may have changed through the span of occupation. Perhaps the first is judged as best left to common sense and the second as of no consequence.

It is, however, important to know whether the indigenous artefacts are the cumulative trace of a number of individual visits, intimate or otherwise, or the residues of periodic visits by larger groups, some of whom were trading in livestock. If the latter, why bring both men and women within the confines of a military establishment in the context of the mutual wariness which prevailed? If the former, the indigenous cohabitants have no unequivocal documentary identification, however plausible the inferences (Occam's razor?) one may choose

to make on the basis of those who were recorded as being present on "official business". There are many possible permutations which could have given rise to the residues; more explicit views on this matter would be useful, particularly where so much is made of their presence (Schrire & Deacon 1989).

While Schrire may be "disturbed" (1992a:64) by the underlying political message of our paper, we are surprised at her naivety in assuming that any concern with the possibilities of sociocultural divisions is predicated on the principles of apartheid. Are we to read this to imply, as it would seem to do, that cultural and economic distinctions of any form are entirely fabrications of colonial and postcolonial circumstances and thus, have no part of historical enquiry? We believe not. Concern with broadly termed cultural differences as historically articulated phenomena is not necessarily predicated on racism; in examining this, it is instructive to consider research in a region other than the south-western Cape.

Primarily initiated in the work of Tim Maggs (Maggs & Michael 1976), Patricia Vinnicombe (1976), Lewis-Williams (1981) and Aron Mazel (1989)² archaeological research in Natal has now for over a decade variously focussed on sites yielding evidence of farming or hunting and gathering. There is evidence that important interactions took place between hunters and farmers (summarized by Mazel (1986); see Lewis-Williams & Dowson (1989: 143-145)) and the possibility of structurally similar relations, as opposed to means of production (Hall 1987). Notwithstanding this, it appears commonly accepted that "hunting and gathering and farming persevered as essentially distinct, and archaeologically recognisable, modes of subsistence until relatively recently" (Mazel 1986:442).

The Natal situation also has a particular bearing on a specific comment of Schrire's. Why is the lack of evidence for serial use of rockshelters by hunters and herders in the south-western Cape so strange - does it necessarily presuppose such a wildly improbable settlement strategy that it should be derisively characterized as "very dainty dancing" (Schrire 1992a:63)? The pattern of site juxtapositioning we presented (Smith *et al.* 1991) is analogous to that documented by Mazel in Natal for the last two thousand years (Mazel 1986). There, hunter sites also occur scattered among those of broadly contemporary farmers without evidence for serial usage. Different as the historical moments of Natal and the Cape may have been, commonalties between the two areas reveal that such an arrangement of sites is not at all strange; it may in fact be closer to a productive locational strategy than the whimsy implied by a "dainty dance" as Schrire chooses to characterize it.

Is research such as that in Natal equally characterizable as predicated on racist paradigms? Hall (1984) has pointed out that certain conceptual frames

2. This does not deny other important contributions, both earlier and contemporary, but reflects individuals who played key roles in initiating systematic research.

within which farming communities have been characterized, specifically notions of primordial tribalism, are susceptible to an ideological reading within the broader South African social context. He did not, however, claim as unwarranted any notion of meaningfully articulated social identity such as has been a focus of work in Natal and indeed, elsewhere in the world (see Shennan 1989). We wonder, then, what makes similarly interested research in the south-western Cape so different?

CONCLUSION

Schrire (1992b:132) wonders whether we "might be guided more by ideology than any other frame of reference". One could perhaps ask the same of her work. But no matter. One feature of research into historical indigenous social formations in south-western Cape is the fact that the relevant written records are pretty patchy, sometimes hearsay and certainly not unambiguous. With due respect to Schrire (1980), we do not think that her deconstruction of Dutch accounts of indigenous groupings is conclusive or unequivocal; nor do we dery, in the light of what has come to be called "the revisionist San debate", the significant intellectual challenge and farsightedness of her contribution. We believe the matter requires further investigation, in which archaeological evidence has a crucial, perhaps definitive role to play.

As should be clear, Oudepost in our view most certainly does not unequivocally refute hypotheses of relatively distinct material cultural practices of hunters and herders. Nor are the contrasts we presented earlier (Smith *et al.* 1991) as easily dismissed as Schrire supposes. We do not argue from the archaeological evidence, as Schrire extrapolates, that continuity in some aspects of material culture across the appearance of pottery and sheep presupposes total cultural (behavioural) continuity (Schrire 1992a:63) and thus lack of change. Clearly, many aspects of the settlement and material culture of hunters underwent profound changes coincident with the emergence of pastoralism (Parkington *et al.* 1986, Manhire 1987, Yates *et al.* in press). We assume that change, even profound change, does not ensure convergence and the melding of identities. Ours is not a 'pristine' argument, but one for an appreciable cultural distinction. These respective identities however, were not ineluctable and fossilised; their existence, natures and expressions were historical phenomena. While accepting that individuals can and do cross over such cultural divides, we do not believe that it is common for whole groups to do so at one fell swoop, nor is it necessary that a culturally homogeneous society will result. Equally, the existence of distinguishable social groupings does not necessarily presuppose unremitting mutual hostility and aggression.

In the south-western Cape it is accepted that relations between hunters and herders assumed a variety of forms from raiding through clientship to economic exchange (Wilson 1969; Elphick 1985). It is interesting that some groups historically identified as Soaqua were apparently still called such, despite the fact that they were seen in

possession (momentarily?) of livestock (Elphick 1985:26). It is clear that owning or herding cattle and sheep alone did not effect a transition between sociocultural identities, at least not on the temporal scale of a human lifetime. Thus key components of the evidence for the cyclical model (Schrire 1980; Elphick 1985), which eschews social or cultural factors in favour of narrow economic opportunism, may be susceptible to alternative readings.

Criticism is essential. Debate is properly served by argument around theoretical perspectives and evidence. This, Schrire has offered in part. It is not however, advanced by assertions of racist imperatives (Schrire 1992a), however subsequently modified by Schrire's own tastefully worded dictum (1992b).

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